

ELEMENTS
OF
MECHANICAL PHILOSOPHY,
BEING THE SUBSTANCE OF
A COURSE OF LECTURES
ON THAT
SCIENCE.

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THE HISTORY OF THE
CITY OF EDINBURGH

The history of the city of Edinburgh, from its first foundation to the present time, is a subject of great interest and importance. It is a city of great antiquity, and has been the seat of government and commerce for many centuries. The city is situated on a rocky island, and is surrounded by water on three sides. It is a city of great beauty, and is one of the most interesting cities in the world. The city is a city of great antiquity, and has been the seat of government and commerce for many centuries. The city is situated on a rocky island, and is surrounded by water on three sides. It is a city of great beauty, and is one of the most interesting cities in the world.

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ADVERTISEMENT.

THE following pages contain the substance of a Course of Lectures, which have been read by me during the annual sessions of the Colleges, ever since the year 1774. Any person, well acquainted with Natural Philosophy must be sensible that, in the short space of a six months session, justice cannot be done to the various branches of this extensive science. I found that I must either treat in a loose manner subjects which require and admit of strict reasoning, or must omit some articles usually taught in this class; and I was induced to prefer the latter method, because I was of opinion that a looser manner of proceeding is neither suitable to the Institution in this University, nor calculated to convey useful knowledge. In one session I omitted the consideration of Magnetism and Electricity, and in the next session these were treated of, and Optics was omitted.

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But

But this plan was not always acceptable. I was therefore induced to print these Elements, in the hopes of being able to shorten the lecture, and thus to include all the articles of the course. I shall now think myself at liberty to lecture in a more popular manner, as the student, by consulting the text-book, will find the demonstration of what was only sketched in the lecture of the day.

Such being the intention in this publication, the reader will see in what respects, and for what reasons, it may differ a little from a formal system of Natural Philosophy. It is intended that it shall contain a system. But all the articles will not be treated with the same minuteness. The experience of thirty years has enabled me to judge what articles are more abstruse or intricate, and require a more detailed discussion.

The general doctrines of Dynamics are the basis of Mechanical Philosophy, distinguishing it from every other department of science. They are nearly abstract truths, containing the laws of human judgement concerning all those phenomena which we call mechanical. We shall find these laws nearly as simple and precise as the propositions in geometry, and that they carry with them

a similar accuracy, wherever they can be properly applied. We shall have the pleasure of seeing the complete success of this application, to very extensive and important articles of the science.

These doctrines being so important, and so susceptible of accurate treatment, nothing is omitted here that is necessary for their full establishment; and hence this occasions the first part of the course to be very minute and particular. But, afterwards, a more familiar mode of discussion may be admitted. If the student make himself familiarly acquainted with the principles of Dynamics, it is hoped that he will find little difficulty afterwards, in the application of these abstract doctrines to the investigation of the laws of mechanical nature, or to the explanation of subordinate phenomena. For this reason, it is not intended to annex the mathematical demonstration to every proposition in the subsequent parts of the course. This will not be omitted, however, when either the difficulty or importance of the subject seems to require it.

The student must be mindful that this book will not supersede the necessity of carefully attending to the lecture. Many things, illustrative and interesting, will be heard in the class, which

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have

have no place here. It will also contribute to his improvement, if he accustom himself to take notes in the class; and he is advised to take particular notice of such formulæ, or other symbols of mathematical reasoning, as occur in the lecture. These will frequently give a compendious expression of a process of reasoning which he may otherwise find very difficult to remember with distinctness.

In applying the abstract doctrine of Dynamics to the mechanical history of nature, some arrangement must be adopted which may facilitate the task. It is proposed, in this course of lectures, to arrange the mechanical appearances as much as possible in the order of their generality or extent. It will be found that this is, in fact, arranging them by the great distinguishing powers of natural substances, by which this generality of event is effected.

All the mechanical phenomena that we observe are effected,

1. By gravity.
2. By cohesion.
3. By magnetism.
4. By electricity.
5. By the affections of light.

Hence

Hence is suggested the following arrangement of the articles which will be treated of in this course of lectures.

I. GRAVITY.

1. As it is seen in the celestial motions—its law of action discovered by Sir Isaac Newton—applied by him, with great success, to the explanation of all the phenomena—universal gravitation.
2. As it is observed on this globe—motion of falling bodies—of projectiles—theory of gunnery.

II. COHESION.

Corpuscular forces—Theory of Bosovich.

Mechanical qualities of tangible matter—bodies are solid—or fluid—and these differ exceedingly in their mechanism.

Mechanism of Solid Bodies.

Laws of the excitement of corpuscular forces.

1. Motion in free space—impulsion—direct—oblique—precession of the equinoxes—force of moving bodies.
2. Motion in constrained paths.

3. Rotation—centrifugal force.
4. Solidity combined with gravity—stability—theory of arches and domes.
5. Motion on inclined planes.
6. Motion of pendulums—measure of gravity—measure of time.
7. Theory of machines—or MECHANICS commonly so called—mechanic powers—compound machines—maxims of construction.
Of friction.
Of the action of springs.

Mechanism of Fluid Bodies.

1. Coherent fluids—HYDROSTATICS, treating of the pressure and equilibrium of fluids—HYDRAULICS, treating of the motion, impulse, and resistance of fluids.
Hydraulic machines.
Construction and working of ships.
2. Expansive fluids—PNEUMATICS, treating of the pressure of the air—its elasticity—its motion, impulse, and resistance—Pneumatic machines—sound—theory of music—action of gunpowder—theory of artillery, and of mines—account of the steam engine.

III. MAGNETISM.

General laws of the phenomena—theory of Æpinus—Gilbert's terrestrial magnetism—mariner's compass—variation—dip of the needle—artificial magnetism.

IV. ELECTRICITY.

General laws.

Theory of Æpinus.

Thunder—aurora borealis, &c.

Galvanic phenomena.

V. OPTICS.

Mathematical laws—catoptrics—dioptrics.

Vision—optical instruments.

Newtonian discoveries concerning colours.

Physical optics—further discoveries of Newton—mechanical nature of light—mutual action of bodies and light.

Province, and history of natural philosophy.

THE READER IS REQUESTED TO CORRECT THE FOLLOWING
ERRORS OF THE PRESS.

Page.	Line.		Page.	Line.	
6	12	c'	231	1	() (345)
	13	c'	239	19	calculation calculation
9	8	AFBaFb	326	10	$p n o$ $p v n$
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64	17	AK		16	PF'—PG' PH'—PI'
73	23	ultimate	452	26	Fig 64. Fig. 64. N ^o 1.
75	2	<i>deflecting forces</i>	458	28	after P (Fig. 64. N ^o 2.)
79	12	parallel	471	18	$L e Q q$ $E e Q q$
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CORRECTIONS FOR THE FIGURES.

- Figure.
- 9 Draw EQ.
- 37 Draw ED. The line ES was drawn (295.) perpendicular to IC. In sect. 298, it is supposed to be perpendicular to i C. The two perpendiculars would not be distinguishable.
- 44 e should be d
- 46 Produce BS to M
- 52 Draw SN perpendicular to PN
- 64 D should be in the crossing of $I i$ and $e q$
- 65 Draw $A p$
- 71 The upper S should be s
- 73 Infer G at the crossing of EQ and $N d S$
- 76 Write f to the left of F, on the outside of all.

THE BOOKBINDER IS DESIRED TO PLACE THE PLATES
AS FOLLOWS :

- Plate 1. to face page 48.
2. to face page 72.
3. to face page 80.
4. to face page 144.
5. to face page 160.
6. to face page 16.

- Plate 7. to face page 182.
8. to face page 224.
9. and succeeding ones to be placed agreeably to the engraved reference at the top of each.

EXPLANATION OF SYMBOLS

USED IN THE FOLLOWING PAGES.

(a) THE symbol $a:b$ expresses the ratio or proportion of a magnitude a to another magnitude b of the same kind, such as two lines, two surfaces, two weights, velocities, times, &c.

(b) $a:b = c:d$. The ratio of a to b is equal to, or is the same with, that of c to d .—This is usually read, *a is to b as c to d*.

(c) ab is the product of two numbers, or the rectangle of two lines, a and b .

(d) $a \dot{=} b$ is a symbol made up of the symbol $:$ of proportion, and the symbol $=$ of equality. It means that *a increases or decreases at the same rate with b*, so that if b become double or triple, &c. of its primitive value, the contemporaneous a is also double, triple, &c. of its first value.

This is a short way of writing $A : a = B : b$, in which A and a are successive values of one changeable magnitude, and B, b , the corresponding or simultaneous values of the other. In this symbol, a and b may be magnitudes of different kinds, which cannot hold with respect to the symbol $a : b$, because there is no proportion between magnitudes of different kinds, as between a yard and a pound, an hour and a force, &c. This may be called the symbol of a PROPORTIONAL EQUATION.

(e) $ab : cd$ expresses the ratio compounded of the ratio of a to c and that of b to d . It therefore expresses the ratio of the product of the numbers a and b to that of the numbers c and d . In like manner, it represents the proportion of two rectangles, a and b being the sides of the first, and c and d the sides of the second. In the same manner $abc : def$ is the ratio compounded of those of a to d , of b to e , and of c to f ; and so on, of any number of ratios compounded together. (See Euclid, VI. 23.)

(f) $a : b = \frac{1}{c} : \frac{1}{d}$ means that a is to b in the inverse proportion of c to d , or, that $a : b = d : c$. It is plain that if c be doubled or trebled, the fraction $\frac{1}{c}$ is reduced to one half or one third, &c. so that $\frac{1}{c}$ or $\frac{1}{d}$ are increased in the same proportion that c or d are diminished.

(g)

(g) $a : b = \frac{c}{e} : \frac{d}{f}$ means that the ratio of a to b is the same with that of the fraction $\frac{c}{e}$ to the fraction $\frac{d}{f}$, or that the ratio of a to b is compounded of the direct ratio of c to d and the inverse or reciprocal ratio of e to f . It is the same with $a : b = cf : de$.

(h) $x \doteq \frac{x}{y}$ means that x increases at the same ratio that y diminishes, and is equivalent to $X : x = \frac{1}{Y} : \frac{1}{y}$, or equivalent to $X : x = y : Y$.

(i) $x \doteq \frac{y}{z}$ means that x varies in the ratio compounded of the direct ratio of y and the inverse ratio of z .

(k) $x' : y'$ expresses the proportion between the *difference* of two successive values of x and the *difference* of the two corresponding values of y . It is equivalent to the ratio of $X - x$ to $Y - y$.

(l) Suppose that, in the continual variation of x and y , these simultaneous and corresponding differences are always in the same ratio; then $x' : y'$ is a constant ratio. Thus, Let AD and AF (fig. A) be two right lines diverging from A, and let BC, Bc, BD, be successive values of x , and the parallel ordinates CE, ce, DF be corresponding values of y . Draw EG and eg parallel to AD, and consequently equal to CD and cD, then CD and GF are corresponding differences of the successive

cessive values of x and y . So are cD and gF . Now it is plain that $CD : GF = cD : gF$, and $x' : y'$ is a constant ratio.

(*m*) But it more frequently happens that the ratio $x' : y'$ is not constant. Thus, if the line EeF (fig. B) be an arch of a curve, such as a hyperbola, of which A is the centre, we know that CD has not the same ratio to GF that cD has to gF , and that the ratio of x' to y' continually increases as the point C or c approaches to D . We know that while C is above D , the ratio of CD to GF , or cD to gF is less than that of the subtangent TD to the ordinate DF . But when c' gets below D , the ratio of $E'G'$, or $c'D$, to $G'F$ is greater than that of TD to DF ; and the difference of these ratios increases, as c separates from D on either side. The ratio of x' to y' , therefore, approximates to that of TD to DF as c approaches to D from either side. For this reason, the ratio of TD to DF has been called the *ultimate* ratio of the *evanescent* magnitudes x' and y' , as the magnitudes x' and y' are continually diminished, till both *vanish* together, when c coalesces with D . If, again, we conceive the point C to set out, either upward or downward, from D , the ratio $TD : DF$ is called the *prime* ratio of the *nascent* magnitudes x' and y' .

We know also that the ratio of the subtangent tc to the ordinate ce is less than that of TD to DF , and that the ratio of the subtangent to the ordinate increases continually, as D is taken further from the vertex V of the
the

the hyperbola. But we know also that it never is so great as the ratio of AD to Df (the ordinate produced to the asymptote) but approaches nearer to it than by any difference that can be assigned. For this reason, $AD : Df$ has been called the *ultimate* ratio of the subtangent and ordinate—in the same manner, the ultimate ratio of DF to Df has been said to be the ratio of equality.

(*n*) But, in these two cases, the employment of the term *ultimate* is rather improper, because this ratio is never attained. Perhaps the term *limiting* ratio, also given it by Sir Isaac Newton, is more proper in both these cases. $TD : DF$ is the limiting ratio of $x' : y'$, or the limit, to which the variable ratio of the nascent, or, evanescent magnitudes x' and y' continually approaches.

(*o*) Sir Isaac Newton, the author of this way of considering the variations of magnitude, has expressed by a particular symbol this limiting ratio of the variations x' and y' . He expresses it by $x : y$. It is not the ratio of any x' to any y' , however small, but the limit to which their ratio continually approaches. When we chance to employ the terms *ultimate* or *prime*, we desire to be understood always to mean this limiting ratio. The foreign mathematicians employ the symbol $dx : dy$, in which d means the infinitely or incomparably small difference between two succeeding values of x or y .

We have been thus particular in describing this view of the variations of quantity, because without a knowledge of some of those limiting ratios, it is scarcely possible to advance in mechanical philosophy.

(*p*) The case already mentioned, namely $TD : DF = x' : y'$, occurs very frequently in our investigations.

And, in like manner, if the arch BF be represented by the symbol z , we have $x' : z = TD : TF$, and $y' : z = DF : TF$.

Also, if $E\varepsilon$ be drawn parallel to the tangent te , we have Ee to $E\varepsilon$ ultimately in the ratio of equality. For, because the triangles tce and $Ed\varepsilon$ are similar, we have $Ed : E\varepsilon = tc : te$, that is, $= x' : z$, that is, $= Cc : Ee$, or $Ed : Ee$, and therefore, ultimately, $E\varepsilon = Ee$.

(*q*) Such limiting ratios may also be obtained in curves that are referred to a pole or focus, instead of an abscissa. Thus, let $BF G$ (fig. C) be an ellipse, whose centre is C , and focus D . Let $F e$ be a very small arch of the curve. Draw DF and $D e$, and about the pole D , with the distance $D e$, describe the circular arch $E e g$, cutting FD in g . Draw the tangent FT , and DT perpendicular to DF . Now, representing FD by x , FB by z , and the circular arch eE by y , it is plain that $x' : z = FD : FT$, and $x' : y = FD : DT$. All this is very evident, being demonstrated by the same reasoning as in the case of the hyperbola referred to its axis or abscissa (*m*).

(*r*)

(r) Another limiting ratio, of very frequent occurrence, is the following. Suppose two curves AB and ab (fig. D) round the same pole F , from which are drawn two right lines FA , FB , cutting both lines in A , a , B , and b . Let FB , by revolving round F , continually approach to FA . Let it come, for example, into the situation FcC very near to FAa . Let S and s represent the mixtilineal spaces AFB and Fb . Then S' and s' may express the spaces AFC and aFc . It is plain that the limiting ratio of AFC to aFc is that of FA^2 to Fa^2 , and we may say that $S : s = FA^2 : Fa^2$.

(s) The last example which shall be mentioned is of almost continual occurrence in our investigations.—Let FHK and fbk (fig. E) be two curves, having the abscissæ AE and ae . Let these abscissæ be divided into an equal number of small equal parts, such as AB , BC , DE and ab , bc , de ; and let ordinates be drawn through the points of division. And on these ordinates, as bases, let parallelograms, such as $ABLF$, $BCNG$, &c. and $ablf$, $bcng$, &c. be inscribed, and others, such as $ABGM$, $ACHO$, and $abgm$, $acho$, &c. be circumscribed.—It is affirmed, 1st, that if the subdivision be carried on without end, the mixtilineal areas $AEKF$ and $aekef$ are, ultimately, in the ratio of equality to the sum of all the inscribed, or of all the circumscribed parallelograms; and, 2^{dly}, that the ratio of the space $AEKF$ to the space $aekef$ is the limiting ratio of the

B

sum

sum of all the parallelograms (inscribed or circumscribed) in $A E K F$ to the sum of those in $a e k f$.

1st, Make DS and ds equal to AF and af , and draw SR , sr , parallel to AE , ae . It is evident that the parallelogram $SRKQ$ is equal to the excess of all the circumscribed over all the inscribed parallelograms. Therefore, by continuing the subdivision of AE without end, this parallelogram may be made smaller than any space that can be assigned. Therefore the inscribed and circumscribed parallelograms are ultimately in the ratio of equality—or equality is their limiting ratio. The space $A E K F$ is greater than all the inscribed, and less than all the circumscribed parallelograms, and is nearly the half sum of both. Therefore, much more accurately is equality the limiting or ultimate ratio of $A E K F$ to either sum. The same must be true of the other figure.

2^{dly}, Since each mixtilineal figure is ultimately equal to its parallelograms, it is plain that both have the same ratio with the sums of the parallelograms.

(*t*) *Cor.* If the ordinates which are drawn through similarly situated points of the two abscissæ, be in a constant ratio, the areas are in the ratio compounded of the ratio of AE to ae , and that of AF to af , or are as $AE \times AF$ to $ae \times af$. This is evident. For, by the supposition, $CN : cn = AF : af$. And, since the number of parallelograms is the same in both figures, BC and bc are similar parts of AE and ae ; that is, $BC : bc = AE : ae$. Therefore $BCNG : bcng = AE \times AF : ae$
 $\times af$.

$\times af$. Since this is true of every corresponding pair of parallelograms, it is true of their sums, and of the mixtilineal spaces $AEKF$ and $aekf$, which are ultimately equal to those sums.

(u) It may be thought that in these cases where the limiting ratio is not an ultimate ratio actually attained, there remains some small error. The foreign mathematicians seem to acquiesce in this, and content themselves with assuming dx or dy as infinitely small; inferring from thence that the remaining error is infinitely small, so that it will not amount to a sensible quantity, though multiplied by any number, however great. But this concession leads them *necessarily* into the supposition of quantities infinitely smaller than quantities already assumed as infinitely small; a supposition plainly absurd or unintelligible. But no error whatever lurks in this method of limiting ratios. For it is all founded on the following unquestionable axiom.

(v) If the ratio of a to b be greater than *any ratio whatever* that is less than the ratio of c to d , but less than *any ratio whatever* which is greater than that of c to d , then a is to b as c is to d .

For if a be not to b as c to d , let a be to b as m to n . Then if $m:n$ be greater than $c:d$, $a:b$ is less than $m:n$. If $m:n$ be less than $c:d$, then $a:b$ is greater than $m:n$, both which consequences are contrary to the conditions assumed. Therefore $a:b$ must be $c:d$.

The proposition (*s*) may be demonstrated in this way. The space $\Delta E K F$ is to $a e k f$ in a greater ratio than that of the parallelograms inscribed in the first to those circumscribed on the second, but in a less ratio than that of the parallelograms circumscribed on the first to those inscribed in the second. We perceive, by continuing the subdivision of the two abscissæ, that this holds true with regard to every ratio that is either greater or less than that of $\Delta E K F$ to $a e k f$. Thus, the proposition is demonstrated without the smallest room for error.

(*w*) This doctrine of limiting ratios is of the greatest service in the physico-mathematical sciences. Nature presents magnitudes in a continual change. The velocity of a falling body, and the line of its fall, are increasing together.—As a piece of iron approaches a magnet, its distance, its velocity, and the force by which it is urged, all vary together, and there is an indissoluble relation between their respective simultaneous variations. These variations also are the immediate measures of their rates of variation. Hence it is plain that, by knowing these rates, we can learn the whole change, and by observing the whole change we can infer the rate of variation; just as the navigator learns his day's progress by heaving the log every hour, in order to discover the ship's rate of sailing, and conversely.

(*x*) The letters $F, V, T,$ &c. will be used to express Force, Velocity, Time, and other magnitudes. Thus,

F, Δ

F, A expresses the force acting in the point A . F, AB is the force acting along the line AB .

(*y*) A proper notation, and arrangement of the symbols, greatly assist our conceptions in mathematical reasoning. When ratios are compounded (a thing perpetually occurring in our disquisitions) it is extremely convenient to recollect that the ratio, which is compounded of many numerical ratios, is the same with that of the product of all the antecedents to the product of all the consequents.

Thus, if $a : b = c : d$
 and $e : f = g : h$
 and $i : k = l : m$
 and $n : o = p : q$
 then $a e i n : b f k o = c g l p : d h m q$.

If we use lines, we can go no farther without substitutions than three such compositions, because space has but three dimensions. All our practical uses of the doctrines must be prosecuted by means of arithmetical calculations, although some linear ratios, such as that of the diameter of a circle to its circumference, or that of the diagonal of a square to its side, cannot be accurately expressed by numbers. But, as we know perfectly what substitutions may be made in every case where more than three ratios are compounded, so as to obtain accurate ratios, no mathematician objects to this method of merely expressing the composition.

MECHANICAL PHILOSOPHY.

INTRODUCTION.

1. **M**AN is induced by an instinctive principle, implanted in his mind by the Author of Nature, to consider every change observed in the condition of things as an **EFFECT**, indicating the agency, characterising the kind, and measuring the degree of its **CAUSE**.

2. The kind and degree of the cause are, therefore, *inferred* from the *observed* kind and degree of the change which we consider as its effect.

3. The appearances in the material world, exhibited in the *changes of motion* which we observe, are called **MECHANICAL APPEARANCES**, or **PHENONEMA**, and the causes, to the agency of which we ascribe them, are called **MECHANICAL CAUSES**.

4. **MECHANICAL PHILOSOPHY** is the study of the mechanical phenomena of the universe, in order to discover their causes, and by their means to explain subordinate

dinate phenomena, and to improve arts, and thus increase man's power over nature.

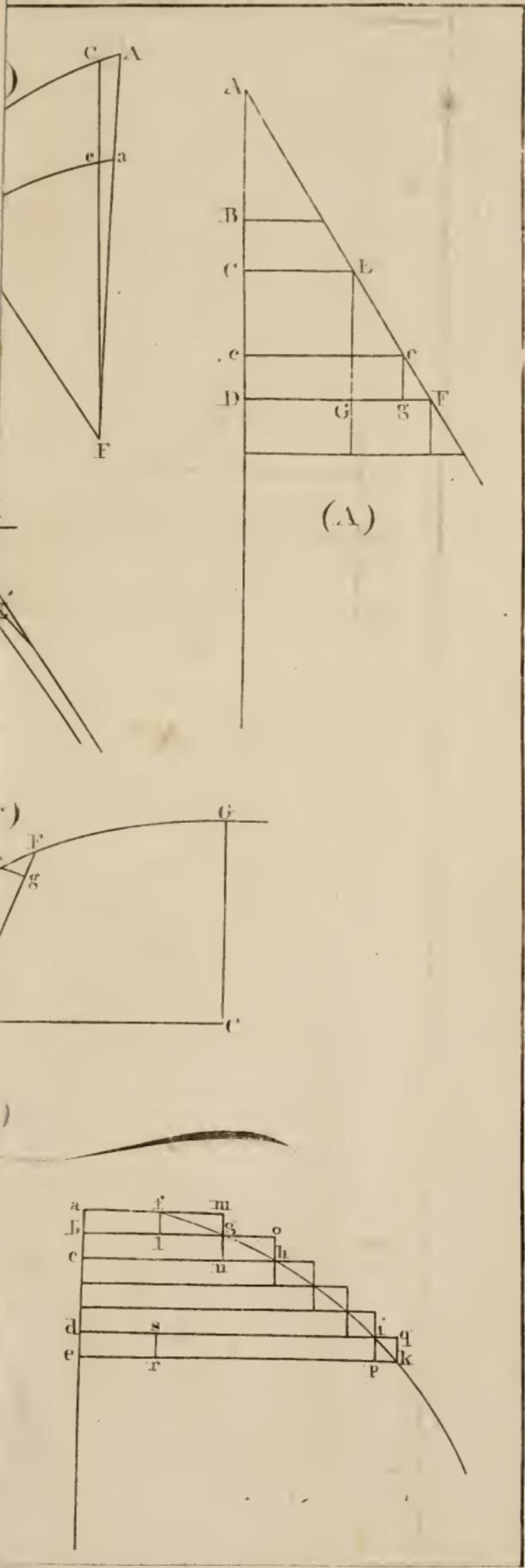
This definition of the study points out Motion, with all its affections and varieties; as the objects of our first attention, a knowledge of these being indispensably necessary for perceiving and appreciating its changes, from which alone we are to derive all our knowledge of their causes, the mechanical powers of nature.

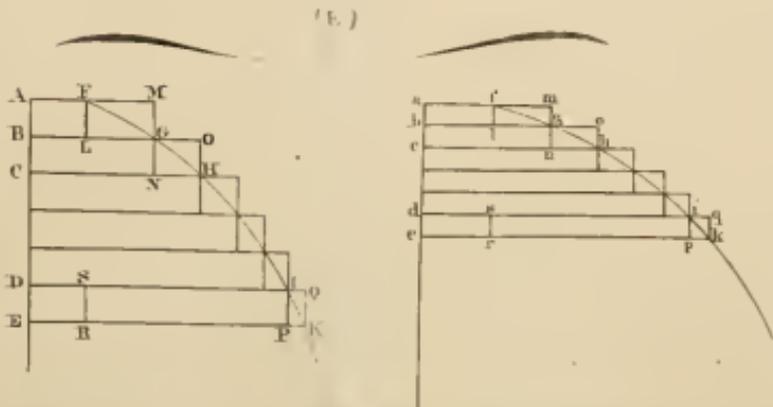
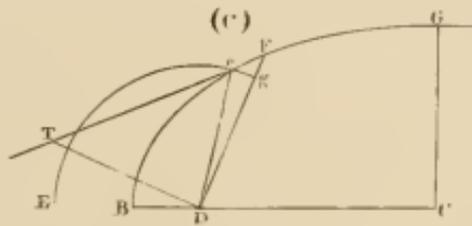
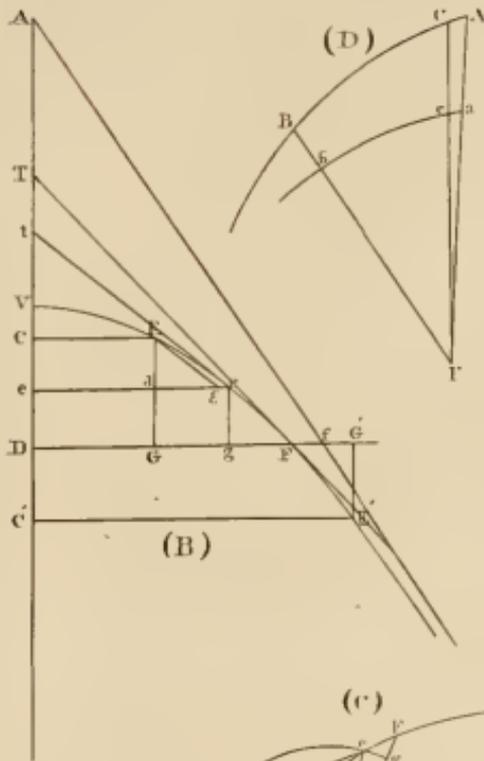
OF MOTION.

5. In motion we observe the *successive* appearance of the thing moved in *different* parts of space. Therefore, in our idea of Motion are involved the ideas or conceptions of SPACE and of TIME.

6. Space is conceived by us as a quantity, that is, it may be conceived as great or little. It is one of that small class of quantities of which we have the clearest and most distinct conceptions. We conceive them as magnitudes made up of their own distinguishable parts, and measurable by one of these as a unit. We cannot conceive so clearly of heat, or pressure, or many other things which are magnitudes, capable of increase and diminution, but not distinguishable into separate parts.

7. In our simplest conception of space, it is mere extension; we think of nothing but a distance between two places. This is the most usual conception of it in
mechanical





mechanical disquisitions—the path along which a thing moves; and we say, figuratively, that the thing *describes* this path.

But the geometer considers space as having not only length, but also breadth, and he then calls it a *surface*; and, in order to have a complete notion of the capaciousness of a portion of space, he considers not only its length and breadth, but also its thickness—and such space he calls a *solid space*. But, by solid, he means nothing but the susceptibility of measure in three ways. He calls it extension of three dimensions.

But, in pure mechanics, we seldom have occasion to consider more than one dimension of space.—In our investigations, however, we make use of geometrical reasonings, which include both surfaces and solids—but our reasoning always terminates in a mechanical theorem, of which distance alone is the subject.

8. The adjoining parts or portions of space are distinguished or separated from one another by their mutual boundaries. Contiguous portions of a line are separated by points—contiguous portions of a surface are separated by lines—and contiguous portions of a solid are separated by surfaces.

9. These boundaries are not parts of the contiguous portions of space, but are common to both. They are the places where the one portion of space ends, and the other begins. It is of importance to have very clear

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9. These boundaries are not parts of the contiguous portions of space, but are common to both. They are the places where the one portion of space ends, and the other begins. It is of importance to have very clear

notions of this distinction, for great mistakes have arisen in mechanical discussions by not attending to it.

10. We cannot conceive space as having any bounds, and it is therefore said to be infinite, or unbounded.

11. A portion of space may be considered in relation to its situation among other portions. This may be called the RELATIVE PLACE of the Body which occupies this portion of space. It may also be called its SITUATION.

Or it may be considered as a determinate portion of infinite space, the individuality or identity of which consists entirely in its being *there*. This is called the ABSOLUTE PLACE of the body which occupies this portion of infinite space.—It is plain that in this sense, space is immoveable—that is, we cannot conceive this identical portion of space as removed from where it is, to another place—for whatever be taken from thence, space remains. Yet we always proceed on the contrary supposition in our actual measurements. If we find that three applications of a foot rule to one line completely exhaust it, and that six applications are required for another line, we affirm that the last is double of the first. But this really proceeds on another supposition, viz. *that the rule, though it do not always occupy the same space, yet, in every situation, it occupies an equal space*. Granting this, the conclusion is just. It will afterwards appear that this remark on the immobility of space is of importance in mechanical discussions.

12. We do not perceive the absolute place of any object.—A person in the cabin of a ship does not consider the table as changing its place while it remains fastened to the same plank of the deck. Few persons think that a mountain changes its place while it is observed to retain the same situation among other objects. On the other hand, most men think that the stars are continually changing their places, although we have no proof of it, and the contrary is almost certain.

13. We acquire our notions of time by our faculty of memory, in observing the successions of events.

14. Time is conceived by us as unbounded, continuous, homogeneous, unchangeable in the order of its parts, and divisible without end.

15. The boundaries between successive portions of time may be called INSTANTS, and minute portions of it may be called *moments*.

16. Time is conceived as a proper quantity, made up of, and measured by, its own parts. In our actual measurements, we employ some event, which we imagine always to require an equal time for its accomplishment; and this time is employed as a unit of time or duration, in the same manner as we employ a foot rule as a unit of extension. As often as this event is accomplished during some observed operation, so often do we imagine

that the time of the operation contains this unit. It is thus that we affirm that the time of a heavy body falling 144 feet, is thrice as great as the time of falling 16 feet; because a pendulum $39\frac{1}{8}$ inches long makes three vibrations in the first case, and one in the last.

17. There is an analogy between the affections of space and time so obvious, that, in most languages, the same words are used to express the affections of both.—Hence it is that time may be represented by lines, and measured by motion; for uniform motion is the simplest succession of events that can be conceived.

18. All things are placed in space, in the order of situation.—All events happen in time, in the order of succession.

19. No motion can be conceived as instantaneous. For, since a moveable, in passing from the beginning to the end of its path, passes through the intermediate points; to suppose the motion along the most minute portion of the path instantaneous, is to suppose the moveable in every intervening point at the same instant.—This is inconceivable, or absurd.

20. ABSOLUTE MOTION is the change of absolute place. RELATIVE MOTION is the change of situation among other objects. These may be different, and even contrary

21. The relative motions of things are the differences of their absolute motions, and cannot, of themselves, tell us what the absolute motions are. The detection and determination of the absolute motions, by means of observations of the relative motions, are often tasks of great difficulty.

22. Mathematical knowledge is indispensably requisite for the successful study of mechanical philosophy. On the other hand, the consideration of motion, in all its varieties of space, direction, and time, is purely mathematical, and carries with it, into all subjects, the most incontrovertible evidence.

23. Motion is susceptible of varieties in respect of *quantity* and of *direction*.

24. That affection of motion which determines its quantity, is called VELOCITY. Its most proper measure is the length of the line uniformly described during some given unit of time. Thus, the velocity of a ship is ascertained, when we say that she sails at the rate of six miles per hour.

25. The DIRECTION of a motion is the position of the straight line along which it is performed. A motion is said to be in the direction A B (fig. 1.) when the thing moved passes along that line *from A towards B*. In common discourse we frequently express the direction otherwise.

wind. Thus we say a westerly wind, although it moves eastward.

26. In rectilinear motion, the direction remains the same, during the whole time of the motion.

27. But if the motion be performed along two contiguous straight lines AB , BC (fig. 2.) in succession, the direction is changed in the point B . From Bc , the prolongation of AB , it is changed to BC .

This change may be called DEFLECTION; and this deflection may be measured, either by the angle cBC , or by a line cC drawn from the point c , to which the moveable would have arrived, had its motion remained unchanged, to the point C , at which it actually arrives in the same time.

When a moveable describes the sides of a polygon, there are repeated deflections, with undeflected motions intervening.

28. But if the motion be performed along a curve line, such as $ADBEC$ (fig. 3.) the direction is *continually* changing. The direction in the point B is that of the tangent BT , that direction alone lying between any pair of polygonal directions, such as BC and Bc , or BD and BE , however near we take the points A and C , or D and E , to the point B .

29. A curvilinear motion supposes the deviation and deflection

deflection to be continual, and a continual deflection constitutes a curvilinear motion.

1. *Of Uniform Motions.*

30. In our general conceptions of motion, in which we do not attend to its alterations, the motion is supposed to be equable and rectilinear; and it is only by the deviations from such motion that we are to obtain the marks and measures of all changes, and therefore of all changing causes, that is, of the mechanical powers of nature. Let us therefore fix the characters of uniform or unchanged motion.

31. *In uniform motions, the velocities are in the proportions of the spaces described in the same, or in equal times.*

For these spaces are the measures of the velocities, and things are in the proportion of their measures.

Let S and s represent the spaces described in the time T , and let V and v represent the velocities. We have the analogy $V : v = S : s$. This may be expressed by the proportional equation $v \doteq s$.

32. *In uniform motions with equal velocities, the times are in the proportion of the spaces described during their currency.*

For, in uniform motions, equal spaces are described in equal times. Therefore the successive portions of time are

are equal, in which equal spaces are successively described, and the sums of the equal times must have the same proportion as the corresponding sums of equal spaces. Therefore, in all cases that can be represented by numbers, the proposition is evident. This may be extended to all other cases, in the same way that Euclid demonstrates that triangles of equal altitude are in the proportion of their bases.

33. These propositions are often expressed thus: “*The velocities are proportional to the spaces described in equal times.—The times are proportional to the spaces described with equal velocities.*” Proportion subsists only between quantities of the same kind.—But nothing more is meant by these inaccurate expressions, than that the proportions of the velocities and times are the same with the proportions of the spaces.

34. It is on this authority that uniform motion is universally employed as a measure of time.—But it is not easy to discover whether a motion which may be proposed for the measure is really uniform—sundial—clepsydra—sundial—clock—revolution of the starry heavens.

35. *In uniform motions, the spaces described are in the ratio compounded of the ratio of the velocities and the ratio of the times.*

Let the space S be described with the velocity V , in the time T , and let the space s be described with the velocity

locity v , in the time t . Let another space Z be described in the time T with the velocity v .

Then, by art. 31, we have $S : Z = V : v$

And, by art. 32, $Z : s = T : t$

Therefore, by composition of ratios (or by VI. 23. Eucl.) we have $= V \times T : v \times t = S \times Z : s \times Z$; that is, $= S : s$.

36. This is frequently expressed thus: “*The spaces described with a uniform motion are proportional to the products of the times and the velocities.*”—Or thus:

37. “*The spaces described with a uniform motion are proportional to the rectangles of the times and the velocities.*”

These are all equivalent expressions, demonstrated by the same composition of ratios. By products or rectangles of the times and velocities, is meant the products of numbers, which are as the times, multiplied by numbers, which are as the velocities; or the rectangle, whose bases are as the times, and whose heights are as the velocities.—There are several other modes of expressing these propositions,

58. Cor. 1. *If the spaces described in two uniform motions be equal, the velocities are in the reciprocal proportion of the times.*

For, in this case, the products $V T$ and $v t$ are equal, and therefore $V : v = t : T$, or $V : v = \frac{1}{T} : \frac{1}{t}$. Or, be-

D

cause

cause the rectangles AC, DF (fig. 4.) are in this case equal, we have (by Eucl. VI. 14.) $AB : BF = BD : BC$, that is $V : v = t : T$.

39. *In uniform motions, the times are as the spaces, directly, and as the velocities, inversely.*

For, by art. 35, $S : s = VT : vt$

therefore $Svt = sVT$

and $T : t = Sv : sV$

or $T : t = \frac{S}{V} : \frac{s}{v}$

and $t \doteq \frac{s}{v}$

40. *In uniform motions, the velocities are as the spaces, directly, and as the times, inversely.*

For, as before, $Svt = sVT$

therefore $V : v = St : sT$

or $V : v = \frac{S}{T} : \frac{s}{t}$

and $v \doteq \frac{s}{t}$

41. It is evident that the absolute magnitudes of the space and time do not change the values of the results of these propositions, provided both are changed in the same ratio. The value of $\frac{20 \text{ feet}}{40''}$, or of $\frac{6 \text{ feet}}{12''}$, is the same with $\frac{1}{2}$ of a foot per second. Therefore, if s' be taken to express an extremely minute portion of space described with this velocity in the minute portion of time

t' , we still have the velocity v accurately expressed by $\frac{s'}{t'}$. Also $\frac{s'}{v}$ is the accurate expression of the time t' .

-2. *Of Variable Motions.*

42. It rarely happens that the phenomena of nature present to our observation motions perfectly uniform. Yet we distinctly conceive them, with all their properties; and the deviations from these are the only marks and measures of the variations, and, therefore, of the changing causes. Therefore it is plain, that it is of the first importance that all these deviations be thoroughly understood.

43. If a body continue to move uniformly in the same direction, its motion, or condition in respect to motion, is unchanged. Its condition, therefore, must be allowed to be the same in any two portions of its path, however distant they may be. The difference of place does not imply any change, because a change of place is involved in the very conception of motion. If, therefore, two bodies be moving with the same velocity in this path, or in two lines parallel to it, their condition in respect of motion must be allowed to be the same. They have the same direction, and move at the same rate. No circumstance, therefore, seems to enter into our conception of the state of a body, in respect of motion, except its velocity and its direction. Changes in one

or both of these circumstances constitute all the changes of which this condition is susceptible. We shall first consider changes of velocity.

Of Accelerated and Retarded Motions.

44. Every one is sensible that a falling stone is carried downward with greater rapidity in every successive moment of its fall. During the first second of its fall, we know that it falls 16 feet; during the next, it falls 48; during the third, it falls 80; during the fourth, 112; and so on: falling, during every second, 32 feet more than during the preceding.

Such a motion is, with propriety, called an ACCELERATED MOTION. On the contrary, an arrow shot perpendicularly upward is observed to rise with a motion continually RETARDED. These bodies are therefore conceived to be in *different* states of motion in every succeeding instant. The velocity of the falling body is conceived to be greater in a certain instant than in any preceding instant. Mechanicians say that when it has fallen 144 feet, its velocity is thrice as great as when it has fallen only 16 feet. But it is plain that this inference cannot be made *directly*, from a comparison of the spaces described in the following moments; for in these, it falls 112 and 48 feet: nor from the spaces described in the moments immediately preceding; for in these, the body fell 80 and 16 feet. The assertion however supposes that this variable condition, called Velocity, is susceptible

ceptible of an accurate measure in every instant, although in no moment, however short, does the body describe uniformly a space which may be taken as the measure of its velocity at the beginning of that moment. The space described in any moment is too great for measuring the velocity at the beginning of the moment, and too small for the measure of the velocity at the end of it. Yet its mechanical condition is not known till we obtain such a measure.

In a motion, like this, *continually* accelerated, there can be no such measure. In an instant, no space is described, for this requires time. But the body has, in that instant, what may be called a **POTENTIAL VELOCITY**, a certain **DETERMINATION**, however imperfectly conceived by us, which, if not changed, would cause it to describe, and would be indicated by its actually describing, a certain space uniformly, during a certain assignable portion of time. At another instant, it has another determination, by which, if not changed, another space would be uniformly described in the same, or an equal portion of time. It is in the difference of those two determinations that its difference of mechanical state consists. The spaces which would thus be uniformly described, are the marks and measures of those determinations, and must therefore be sought for with the most scrupulous care, as the measures of those velocities; and the proportions of those spaces must be taken as the proportions of the velocities. This research is effected by the following proposition.

45. Let the straight line ABD (fig. 5.) be described with a motion any how *continually* varied, and let it be required to determine the proportion of the velocity in the point A to the velocity in any other point C .

Let the right line abd represent the time of this motion along the path AD , so that the points a, b, c, d , may mark the instants of the moveable's being in A, B, C, D , and the portions ab, bc, cd , may express the times of describing AB, BC, CD , that is, may be in the proportion of those times. Moreover, let ae , perpendicular to ad , express the velocity of the moveable at the instant a , or in the point A .

Let egh be a line, so related to the axis ad , that the areas $abfe, bcfg, cdhg$, comprehended between the ordinates ae, bf, cg, dh , all perpendicular to ad , may be proportional to the spaces AB, BC, CD , described in the times ab, bc, cd , and let this relation obtain in every part of the figure.

It is then affirmed that the velocity in A is to the velocity in B , or C , or D , as ae to bf , or cg , or dh , &c. In other words,

If the abscissa ad of a curve egh be proportional to the time of any motion, and the areas interrupted by parallel ordinates be proportional to the spaces described, the velocities are proportional to those ordinates.

Make bc and cd equal, so as to represent very small and equal moments of time, and make pa equal to one of them, and complete the rectangle $paeg$. This will represent the space uniformly described in the moment

pa ,

$p a$, with the velocity $a e$ (35.) Let $P A$ be the portion of the space thus uniformly described in the moment $p a$. Let the lines $i m, k n$, parallel to $a d$, make the rectangles $b c m i$ and $c d n k$, respectively equal to the areas $b c g f$ and $c d h g$.

If the motions along the spaces $P A$ and $B C$ had been uniform, their velocities would have been proportional to the spaces described (31.), because the times $p a$ and $b c$ are equal. That is, the velocity in A would be to the velocity in C , as the rectangle $p a e q$ to the area $b c g f$, that is, as $p a e q$ to $b c m i$, that is, as the base $a e$ to the base $c m$, because the altitudes $p a$ and $b c$ are equal.

But the motion along $B C$ is not represented here as uniform. For the line $f g b$ diverges from the axis $b d$, the ordinate $c g$ being greater than $b f$. Therefore the spaces, which are measured by those areas, increase faster than the times, and the figure represents an accelerated motion. Therefore the velocity with which $B C$ would be uniformly described during the moment $b c$, is less than the velocity at the end of that moment, that is, at the instant c , or in the point C of the path. It must therefore be represented and measured by a line greater than $c m$.

We prove, in the same manner that $c k$ represents and measures the velocity with which $C D$ would be uniformly described during the moment $c d$. Therefore, since the motion along $C D$ is also accelerated, the velocity at the beginning of that moment is less than the velocity with which it would be uniformly described in the same

same time, and must be represented by a line less than ck .

Therefore the velocity in A is to that in C in a less ratio than that of ae to cm , but in a greater ratio than that of ae to ck . But, in this example, as long as the instant b is prior and d posterior, to the instant c , cm is less, and ck is greater, than cg . Therefore the velocity in A is to that in C in a ratio that is greater than any ratio less than that of ae to cg , but less than any ratio greater than that of ae to cg . And, consequently, the velocity in A is to that in C as ae to cg . (Symb. (v))

Since this can be proved in the same manner with respect to the velocity in any other point D, the proposition is demonstrated.

It is plain that the reasoning would have been precisely the same, had the motion along BCD been retarded.

46. Cor. 1. *The velocities in different points of the path AD are in the ultimate ratio of the spaces described in equal small moments of time.* For, drawing go parallel to ad , the velocity in the instant a is to that in the instant c as ae to cg , that is, as the rectangle pe to the rectangle co , that is, as $paeg$ to $cdhg$ very nearly. As the moments are diminished, the difference gob between the rectangle $cgod$ and $cgbd$, diminishes, nearly in the duplicate ratio of the moment; so that if the moment be taken $\frac{1}{2}$, $\frac{1}{3}$, or $\frac{1}{4}$ of cd , the error gob is reduced

duced to $\frac{1}{4}$, or $\frac{1}{9}$, or $\frac{1}{16}$. The ultimate ratio of $c g o d$ to $c g h d$ is plainly the ratio of equality, and the corollary is manifest. That is, the velocity in A is to that in C in the ultimate ratio of PA to BC described in equal small moments.

47. It often happens that we cannot ascertain this ultimate ratio, although we can measure the spaces described in very small moments. We are then obliged to take these as measures of the velocity. The error is reduced almost to nothing, if we take the half sum of the spaces BC and CD for the measure of the velocity in the point C; or, which is the same thing, if we take BC for the measure of the velocity in the middle of the moment $b c$. For the spaces BC and CD are measured by the areas $b f g c$ and $c g h d$, which is very nearly equal to the rectangle $b t o d$. Now $b c g t$, or $c d o g$, is the half of it; and it is evident by this proposition, that the velocity in A is to that in C, as the rectangle $p a e q$ to the rectangle $b c g t$, or $c d o g$.

48. Cor. 2. *The momentary increments of the spaces described are in the ratio compounded of the ratio of the velocities and the ultimate ratio of the moments.*

For the increments PA, CD, are as the rectangles $p e$ and $c o$ ultimately (35.); and these are in the ratio compounded of the ratio of the base $a e$ to the base $d o$, and the ultimate ratio of the altitude $p a$ to the altitude $c d$. This may be expressed by the proportional equation; $\dot{p} a \dot{c} d :: v a v c$.

49. Consequently $v \doteq \frac{\dot{s}}{t}$, and $\dot{t} \doteq \frac{\dot{s}}{v}$. The equation $\dot{s} \doteq v \dot{t}$, $v \doteq \frac{\dot{s}}{t}$, and $\dot{t} \doteq \frac{\dot{s}}{v}$ seem to be the same with those in art. 41. But, in art. 41, the small space s' was described uniformly, and the equations were absolute. In the articles 48. and 49. \dot{s} does not represent α space uniformly described. But $\dot{s} : \dot{s}$ expresses the ultimate ratio of S' to s' , when they are diminished continually, and vanish together. The meaning of the equation $\dot{s} \doteq v \dot{t}$ therefore is, that the ultimate ratio of S' to s' is the same with that of $V T'$ to $v t'$.

50. The converse of this proposition may be thus expressed :

If the abscissa a d of the line e f h represent the time of a motion along the line A B D, and if the ordinates a e, b f, c g, &c. be as the velocities in the points A, B, C, &c. then the areas are as the spaces described. This is most expeditiously demonstrated, indirectly, thus :

If the spaces A B, A D be not proportional to the areas $a b f e$, $a d h e$, they must be proportional to some other areas $a b f' e$, $a d h' e$, of another line $e f' h'$, passing through e . But, if so, then, by art. 45, the velocity in A is to that in B as $a e$ to $b f'$. But the velocity in A was stated to that in B as $a e$ to $b f$. Therefore $a e : b f = a e : b f'$, which is absurd. Therefore, &c.

51. The only immediate observation that we can make on these variable motions' is the relation between
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the space described and the time which elapses. The preceding propositions teach us how to infer from this relation the mechanical condition of the body, to which condition we have given the name Velocity, which, however, more properly denominates the effect and measure of this condition or determination.

The same inference may be made in another way. Instead of taking the uniform motion along a line to represent the uniform lapse of time, Sir Isaac Newton often represents it by the uniform increase of an area during the motion along the line taken for the abscissa. The velocities, or determinations to motion in the different points of this line, will be found inversely proportional to the ordinates of the curve which bounds this area.

Thus, let a point move along the straight line AD (fig. 6.) with a motion any how continually changed, and let the curve $LKI H$ be so related to AD that the area $KICB$ is to the area $KHDB$ as the time of moving along BC to that of moving along BD ; and let this be true in every point of the line AD . Let Cc, Dd be two very small spaces described in equal times, draw the ordinates ic, bd , and draw ik, bl perpendicular to KC, HD .

It is evident that the areas $ICci$ and $HDdb$ are equal, because they represent equal moments of time. It is also plain that as the spaces Cc and Dd are continually diminished, the ratio of $ICci$ and $HDdb$ to the rectangles $kCci$ and $lDdb$ continually approaches to that of equality, and that the ratio of equality is the limiting or

rect. area, velocity

ultimate ratio. Therefore, since the areas $ICci$ and $HDdb$ are equal, the rectangles $kCci$ and $lDdb$ are ultimately in the ratio of equality. Therefore their bases ic and bd are inverfely as their altitudes Cc and Dd , that is, $ic:bd = Dd:Cc$. But Cc and Dd being described in equal times, are ultimately as the velocities in c and d (46). Therefore ic and bd are inverfely as the velocities in c and d . Because this may be fimilarly demonstrated in refpect of every point of the abfciffa, the proposition is demonstrated.

52. It now appears that in all cafes in which we can difcover by obfervation the relation between the fpaces described and the times elapsed during the defcription, we difcover the velocities and the mechanical condition of the moveable. To make any practical application of our conclufions, we muft always have recourfe to arithmetical calculations. Thefe are indicated by the algebraic fymbols of our geometrical reasonings. We represent any ordinate cg of fig. 5. by v , and the portion cd of the abfciffa by i , and the area $cdhg$, or rather, its equal, the rectangle $cdog$, by vi . And fince this rectangle is as the corresponding portion CD of the line of motion, and CD is represented by i , we have the equation $i = vi$.

We may now affume as true, all the mathematical confequences of thefe representations. Therefore $i = \frac{v}{v}$, as in art. 41. For the algebraic fymbols are the representations of arithmetical operations, and they represent
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the operations of geometry more remotely, and only because the area of a rectangle is analogous to the product of numbers which are proportioned to its sides. If we use the symbol $\int_v i$ to represent the sum of all these rectangles, it will express the whole area $adbe$, and will also express the whole line of motion AD , and we may state the equation $s = \int_v i$. In like manner $\int \frac{i}{v}$ will be equivalent to $\int i$, that is, to t , and will express the whole time ad . It is also easy to see that $\frac{i}{v}$ represents the ordinate DH of the line $LKI H$ of fig. 6, because any portion Dd of its abscissa is properly represented by i , and the ordinates are reciprocally proportional to the velocities, that is, are proportional to the quotients of some constant number divided by the velocities, and therefore, to $\frac{1}{v}$. Now i being represented by the rectangle $kCci$, which is also represented by $i \times \frac{1}{v}$, we have $i = \frac{s}{v}$, and $t = \int \frac{s}{v}$, as before.

Such symbolical representations will frequently be employed in our future discussions, and will enable us greatly to shorten our manner of proceeding.

53. There is one case of varied motion, which has very particular and useful characters, namely, when the line $efgb$ of fig. 5. is a straight line. Let fig. 7. represent this case of motion along the line AD , and let pa , bc , cd represent equal moments of time, in which

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the moveable describes PA, BC, CD ; draw fm, gn, es parallel to the absciss ad .

It is evident that mg and nh are equal, or that equal increments of velocity are acquired in equal times. Also eq, er, es are proportional to qf, rg, sh , and therefore the increments qf, rg, sh , of velocity, are proportional to the times ab, ac, ad , in which they are acquired.

This motion, may with great propriety be called **UNIFORMLY ACCELERATED**, in which the velocity increases at the same rate with the times, and equal increments are gained in equal times.

If the line eb cut the abscissa in some point v , it will represent a motion uniformly accelerated from rest, during the time vd , and will give us the relations between the spaces, velocities and times in such motions.

From this manner of expressing these relations, it follows that, *in motions uniformly accelerated from a state of rest, the acquired velocities are proportional to the times from the beginning of the motion.* For ae, bf, cg, dh , represent the velocities acquired during the times va, vb, vc, vd , and are in the same proportion with those lines.

54. Also, *the momentary increments of velocity are as the moments in which they are acquired; or the increments of velocity are as the increments of time.*

55. Also, *the spaces described from the beginning of the motion are as the squares of the times.* For the spaces are represented

represented by the triangles $v a e$, $v b f$, $v c g$, &c. and
 $v a e : v b f = v a^2 : v b^2$ &c.

REMARK.

This gives us the ostensible character of an uniformly accelerated motion. For all that we can immediately observe in a motion, is a space described, and a time elapsed. Velocity is not an observation, but the name of an observed relation between the increase of the space and that of the time. The space described in the time $v b$ is observed to be to that in the time $v d$, as $v b^2$ to $v d^2$. We can represent the proportion of $v b^2$ and $v d^2$ by the triangles $v b f$ and $v d h$, which have the same proportion. We then see that the points v, f, h are in a straight line, and therefore $b f$ and $d h$ are as $v b$ and $v d$, that is, when we observe a motion such that the spaces described are proportional to the squares of the times, we are certain that the velocities are as the times from the beginning of the motion, and that the increments of velocity are as the increments of the times, and therefore the motion is uniformly accelerated.

56. Also, *the increments of the spaces are as the increments of the squares of the times* (counted from the beginning of the motion), that is, $v b f - v a e : v d h - v c g = v b^2 - v a^2 : v d^2 - v c^2$.

57. Also, *the spaces described from the beginning of the motion are as the squares of the acquired velocities*. For $v a e : v b f = a e^2 : b f^2$.

58. Also, *the momentary increments of the spaces are as the momentary increments of the squares of the velocities.* For $bcgf : cdhg = cg^2 - bf^2 : dh^2 - cg^2$ &c. This last is a corollary of frequent use, as it often happens that we can only observe momentary changes.

59. Also, *the space described during any portion of time, by a motion uniformly accelerated from rest, is one half of the space uniformly described in the same time with the final velocity of the accelerated motion.* For the triangle vdb measures the space described in the time vd by the accelerated motion, and the rectangle $vdhH$ measures the space uniformly described in the time vd with the velocity dh .

Here it is to be remarked, that $cgbd$ is only half of the difference between the rectangles $vdhH$ and $vcgG$. If we make $dh = vd$, then $vdhH$ and $vcgG$ will be the squares of the velocities dh and cg . In this case, nb , the increment of velocity, is also equal to gn , and $dn \times nb$ is $= cg \times nb$. Employing v and \dot{v} to express velocity and its momentary increment, $v\dot{v}$ will be the expression of the rectangle $cg \times nb$. Now $2v\dot{v}$ is the usual expression of the increment of the square of velocity. As halves are proportional to their wholes, $v\dot{v}$ is always proportional to $2v\dot{v}$, and is generally used to express the variation of v^2 . But we must keep in mind that it is only the half of it.

60. *And the space described during any portion of the time of the accelerated motion, is equal to that which would*

be described in the same time with the mean between the velocities at the beginning and end of this portion of time.

For $b d h f = b d \times c g$.

These properties of uniformly accelerated motion will be found of very great service in the investigation of all other varied motions, particularly in cases where an approximation is all that can be effected without very tedious and complicated processes.

61. Acceleration may be considered as a measureable quantity. A stone falling in the vertical line, much sooner acquires a great velocity, than when rolling down a slope, and all are sensible that the acceleration is less as the declivity is more gentle.

If we suppose the acceleration to be always the same, the conception that we have of this constancy is, surely, that in equal times equal increments of velocity are acquired; and, consequently, that the augmentations of velocity are proportional to the times of acquiring them. This being supposed, that acceleration must surely be accounted double or triple, &c. in which a double or triple velocity is acquired; and, in general, the augmentation of velocity uniformly acquired in a given time, must be taken for the measure of the acceleration.

62. Cor. Therefore accelerations are proportional to the spaces described in equal times with motions uniformly accelerated from a state of rest, (in which the velocities gradually increase from nothing). For, in this case the spaces are the halves of what would be uniformly described in

the same time with the acquired final velocities, and are therefore proportional to these velocities (31), that is, to the accelerations, seeing that these velocities were uniformly acquired in equal times.

On the other hand, that acceleration must be reckoned double or triple of another, in which a given augmentation of velocity is uniformly acquired in one half or one third of the time. For, if a given augmentation of velocity be acquired in half of the time, then, if the same acceleration be continued during the remaining half of the given time, another equal augmentation will be acquired, the acceleration being constant. The whole augmentation acquired in the same time will be double, and therefore the acceleration is double. The same thing must be granted for any other proportion.

63. Therefore, we must say that *accelerations are proportional to the increments of velocity uniformly acquired, directly, and to the times in which they are acquired, inversely.*

$$A : a = \frac{V}{T} : \frac{v}{t}.$$

Or, we may express it by the proportional equation

$$a \doteq \frac{v}{t}.$$

It is to be remarked here, that this relation between the Acceleration, Velocity, and Time, is not confined to the case of a motion passing through all degrees of velocity from nothing to the final magnitude v , but is equally true (in uniformly accelerated motions) with respect to

any momentary change of velocity. For, since the velocity increases at the same rate with the time, we have $v : v' = t : t'$ (v' and t' expressing the simultaneous increments of velocity and time). Therefore the symbols $\frac{v}{t}$ and $\frac{v'}{t'}$ have the same value, and therefore $a \doteq \frac{v'}{t'}$.

64. On the other hand, since the augmentation of velocity is the measure of the acceleration, and is therefore proportional to it, and since in uniformly accelerated motions, the velocity increases at the same rate with the times, it follows that the augmentations of velocity are as the accelerations and as the times, jointly. This gives the proportional equation $v \doteq a t$,
and $v' \doteq a t'$.

65. Since all that we can observe in a motion is a space described, and a time elapsed during the description, it is desirable to have a measure of acceleration expressed in these terms only.

This is easily obtained. We have seen in art. 62. that, when the velocity has uniformly increased from nothing, the spaces described in equal times are very proper measures of acceleration. And, in uniformly accelerated motions, the spaces are as the squares of the times (56). Therefore, when the acceleration remains the same, the fraction $\frac{s}{t^2}$ must remain of the same value, and a is proportional to $\frac{s}{t^2}$.

F 2

Therefore,

Therefore, *accelerations are proportional to the spaces described with a motion uniformly accelerated from rest, directly, and to the squares of the times, inversely.*

66. Farther, since $a \doteq \frac{v}{t}$ (64) we have $a \doteq \frac{v}{vt}$; but $vt \doteq s$, therefore $a \doteq \frac{v^2}{s}$. This gives us another measure of acceleration, *viz. Accelerations are directly as the squares of the velocities, and inversely as the spaces along which the velocities are uniformly augmented.*

67. On the other hand, since, when the spaces are equal, we have $a \doteq v^2$; and, in uniformly accelerated motions, that is, when a remains constant, if the space is increased in any proportion, v^2 increases in the same proportion; it follows that \bar{v}^2 increases in the proportion, both of the acceleration and of the space. Therefore we have, in general, $v^2 \doteq a s$.

Again (as in art. 64, 65) we shall have $v^2 \doteq a S$, and $V^2 - v^2 \doteq a S - a s$, or $\doteq S - s$, which we may express in this manner $\overline{v v'} \doteq a s'$. That is, *the momentary change of the square of the velocity, in a motion uniformly accelerated, is proportional to the acceleration and to the space, jointly.* This will be found a most important theorem.

Thus we see that the acceleration continued during a given time t , or t' , produces a certain augmentation of the simple velocity; but the acceleration continued along a given space s , or S , produces a certain augmentation

of

of the square of the velocity. This observation will be found of very great importance in mechanical philosophy.

68. Hitherto the acceleration has been considered as constant—that is, we have been considering only such motions as are *uniformly* accelerated; but these are very rare in the phenomena of nature. Accelerations are as variable as velocities, so that it is equally difficult to find an actual measure of them.

Yet it is only by changes of velocity that we get any information of the changing cause, or the mechanical power of nature. It is only from the continual acceleration of a falling body, that we learn that the power which makes it press on our hand, also presses the body downward, while it is falling through the air; and it is from our observing that it acquires equal increments of velocity in equal times, that we learn that the downward pressure of gravity on it is the same, whatever be the rapidity of its descent. No rapidity withdraws it in the smallest degree from the action of its gravity or weight. This is valuable information; for it is very unlike all our more familiar notions of pressures. We feel that all such pressures as we employ, have their accelerating power diminished as the body yields to them. A stream of water or of wind becomes less and less effective as the impelled bodies move more rapidly away, and, although they are still in the stream, there is a limiting velocity which they cannot pass, nor ever fully attain. It is of the greatest consequence therefore to obtain

tain accurate measures of acceleration, even when continually varying.

We may obtain this in the very same way that we get measures of a velocity which varies continually. We can conceive a line to increase along with our velocity, and to increase precisely at the same rate. It is evident that this rate of increase of the velocity is the very thing that we call Acceleration, just as the rate at which the line now mentioned increases is the very thing that we call Velocity. We have only therefore to consider the areas of fig. 5. or the line AD of that figure, as representing a velocity; then it is plain that the ordinates to the line egb , which we demonstrated to be proportional to the rate of variation of this area, will represent, or be proportional to the variation of this velocity, that is, to the acceleration. Hence the following proposition.

69. *If the abscissa ad of a curve line egh represent the time of a motion, and if the areas $abfe$, $acge$, $adh e$, &c. are proportioned to the velocities at the instants b, c, d , &c. then the ordinates ae, bf, cg, dh , &c. are proportional to the accelerations at the instants a, b, c, d , &c.*

This is demonstrated precisely in the same manner as in art. 45. and we need not repeat the process. We have only to substitute the word *acceleration* for the word *velocity*.

From this proposition, we may deduce some corollaries which are of continual use in every mechanical discussion.

70. *The momentary increments of velocity are as the accelerations, and as the moments, jointly.*

For, the increment of velocity in the moment cd (for example) is accurately represented by the area $cdhg$, or by the rectangle $cdnk$; and cd accurately represents the moment. Also, the ultimate ratio of ck to such another ordinate bi , is the ratio of cg to bf (45); that is, the ratio of the acceleration in the instant c to the acceleration in the instant b . Therefore the increment of velocity during the moment pa is to that during the moment cd as $pa \times ae$ to $cd \times dg$.—We may express this by the proportional equation $v \doteq a t$.

71. *Conversely.* The acceleration a is proportional to $\frac{\dot{v}}{t}$, agreeably to what was shown when the motion is uniformly accelerated (63).

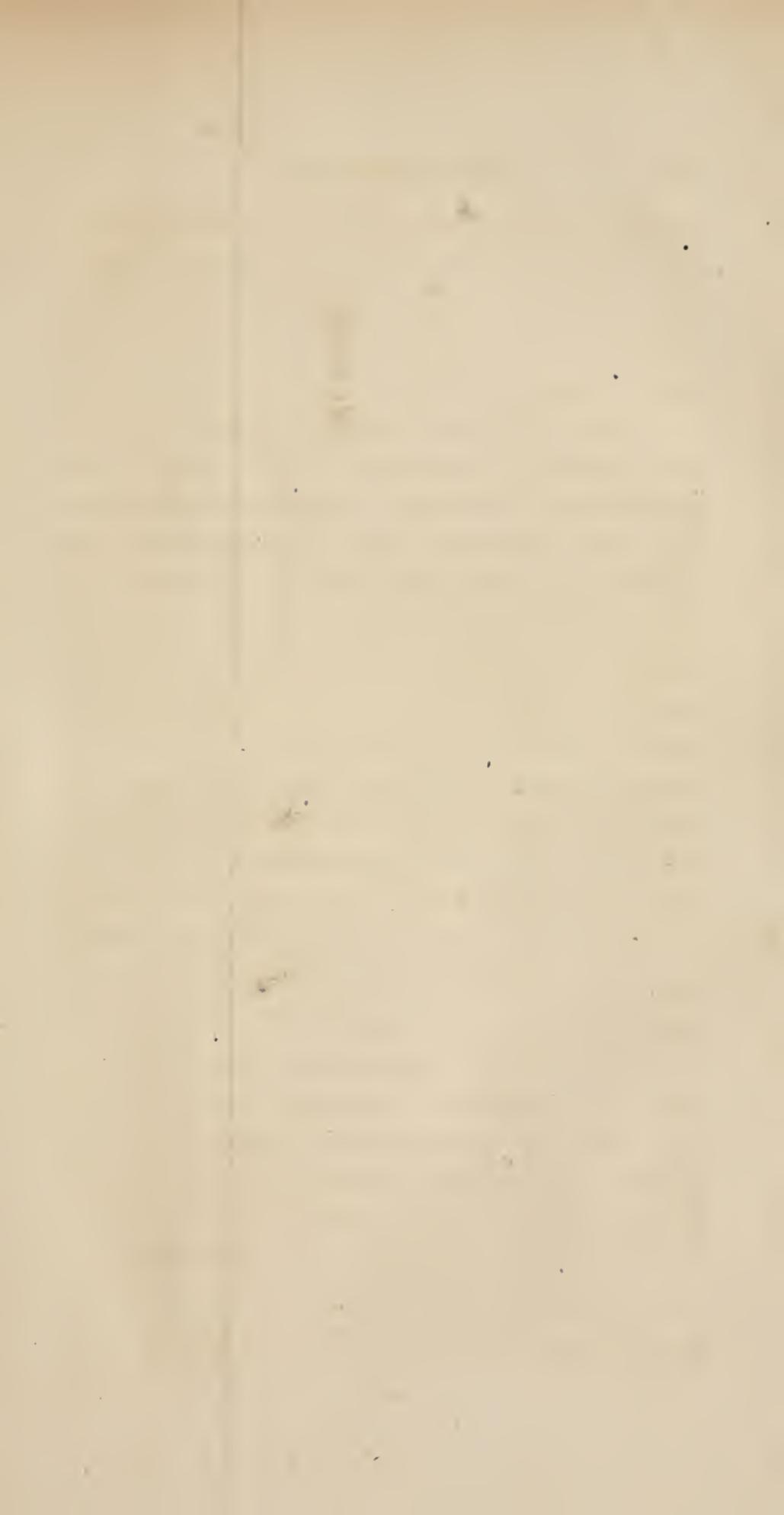
When, from the circumstances of the case, we can measure the area of this figure, as it is analogous to the sum of all the inscribed rectangles, we may express it by $\int a t$; and thus we obtain the whole velocity acquired during the time AP , and we say $v \doteq \int a t$.

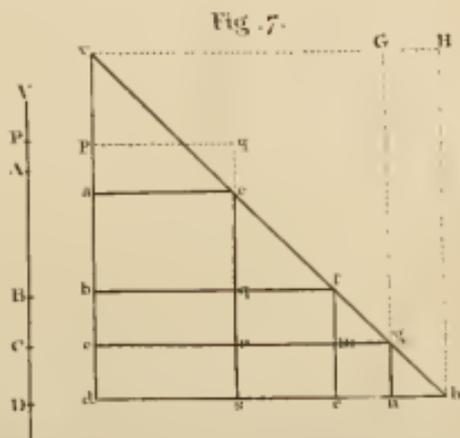
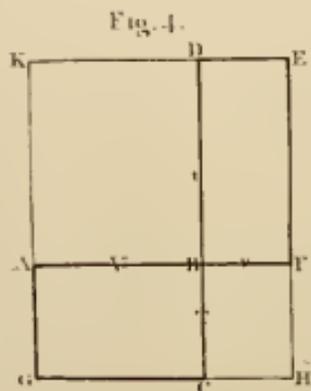
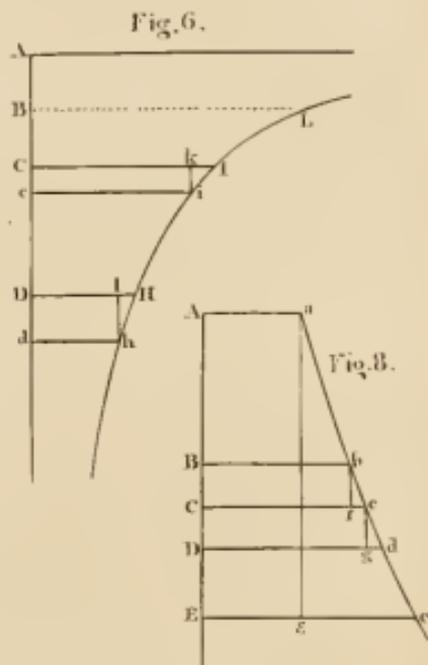
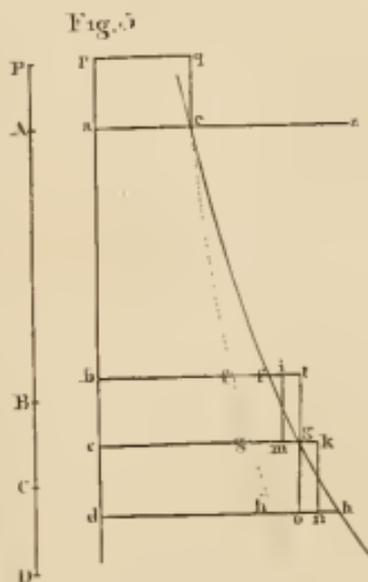
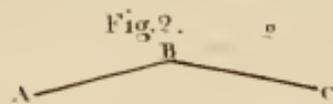
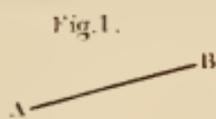
It frequently happens that we know the intensities (or at least their proportions) of the accelerating powers of nature in the different points of the path, and we want to learn the velocities in those points. This is obtained by means of the following proposition:

72. *If the abscissa ΛE of a line acc (fig. 8.) be the space along which a body is moving with a motion continually varied,*

varied, and if the ordinates Aa , Bb , Cc , &c. be proportional to the accelerations in the points A , B , C , &c. then, the areas $ABba$, $ADda$, $AEea$, &c. are proportional to the augmentations of the square of the velocity in A at the points B , D , E , &c.

Let BC , CD , be two very small portions of the line AE , and draw bf , cg , parallel to AE . Then, if we suppose that the acceleration Bb continues through the space BC , the rectangle $BbfC$ will express the augmentation made on the square of the velocity in B (67). In like manner, $CcgD$ will express the increment of the square of the velocity in C ; and, in like manner, the rectangles inscribed in the remainder of the figure will severally express the increments of the squares of the velocity acquired in moving over the corresponding portions of the abscissa. The whole augmentation therefore of the square of the velocity in A (if there be any velocity in that point) during the passage from A to B , is the aggregate of these partial augmentations. The same must be affirmed of the motion from B to E . Now, when the subdivision of AE is carried on without end, it is evident that the ultimate ratio of the area $AEea$ to the aggregate of inscribed rectangles, is that of equality; that is, when the acceleration varies, not by starts, but continually, the area $ABba$ will express the augmentation made on the square of the initial velocity in A , during the motion along AB . The same must be affirmed of the motion along BE .—Therefore the intercepted areas $ABba$, $BDdb$, $DEed$, are proportional





portional to the changes made on the squares of the velocity in A, B, and D.

73. *Cor. 1.* If the moveable had no velocity in A, the areas $ABba$, $ADda$, &c. are proportional to the squares of velocity acquired in B, D, &c.

74. *Cor. 2.* The momentary change on the square of the velocity is as the acceleration and increment of the space jointly, or, we have $v\dot{v} = a\dot{s}$; and thus we find that what we demonstrated strictly in uniformly accelerated motions (67) is equally true when the acceleration continually changes.

75. *Cor. 3.* Since we found $v\dot{v}$ equal to half the increment of the square of the velocity (59), it follows that the area $AEea$, or the fluent $\int a\dot{s}$ is only equal to $\frac{V^2 - v^2}{2}$, supposing v and V to be the velocities in A and E.

76. All that has been said of the acceleration of motion is equally applicable to motions that are retarded, whether uniformly or unequally; the momentary variations being decrements of velocity instead of increments. A moveable, uniformly retarded till it is brought to rest, will continue in motion during a time proportional to the initial velocity; and it will describe a space proportional to the square of this velocity; and the space so described

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is one half of what it would have described in the same time with the initial velocity undiminished, &c. &c. &c.

Having now obtained proper marks and measures of all variations of velocity, it remains to obtain the same for all changes of direction. Thus we shall obtain a knowledge of the greatest part of those motions which the spontaneous phenomena of nature exhibit to our view. It is very doubtful whether we have ever seen a motion strictly rectilinear.

3. *Of Compound Motions.*

79. In our endeavours to obtain a general mark or characteristic of any change of motion, it is evident that when the change is supposed to be the same in any two or more instances, the ostensible marks must be the same, whatever have been the previous conditions of the two moveables. There must be observed, in all the cases of change, some circumstance in the difference between the former motions and the new motions, which is precisely the same, both in respect of kind and of quantity, that is, in respect of direction and of velocity. We may therefore suppose one of the bodies to have been previously at rest. In this case, the whole change produced on it is unquestionably the very motion which we see it acquire, or the determination to this motion.

Therefore, in the first place, *a change of motion is, itself, a motion, or determination to motion.* In the case now mentioned, it is the new motion, and that only,

But

But it is by no means the new motion in every other case. For, if the *previous* condition of the body has been different from that of a body at rest, and if the *same* change has been produced on it, the *new* condition must also be different from the new condition of the other, and therefore the *new condition* cannot be the *change*, because this is supposed to be the same in both cases. But, farther, when the same change is made in any previous motion, we must see, in the difference between the former motion and the new motion, something that is equivalent to, or the same with, this motion produced in the body that was previously at rest, and which has received the same change. And also, the difference between the new motions of these two bodies must be such as shall indicate the difference between these previous conditions of each.

Assuming therefore as a principle, that the change of motion is itself a motion, let us endeavour to find out a motion, which alone shall produce that difference from the former motion which is really observed in the new motion, in all cases whatever. This, undoubtedly, is the proper mark and measure of the change.

Something very analogous to these indispensable conditions may be observed in the following motions. Suppose the straight line EI (fig. 9.) lying east and west, crossed by the line EK from north to south. Let the line EK (which we suppose to be material, such as a rod or wire) be carried along the line EI in a minute, keeping always parallel to its first position, that is, al-

ways lying north and south. At the end of 20'' it will have the position $Gg\gamma$, its end E having moved uniformly along $\frac{1}{3}$ of EI ; at the end of 40'' it will have the position $Hb\chi$, E having described $\frac{2}{3}$ of EI ; and at the end of the minute it will have the position Ii .

In the mean time, let the line EI (also supposed material) move uniformly from north to south, keeping always parallel to its first position EI . At the end of 20'' it will have the position $mg\eta$, its end E having moved along $\frac{1}{3}$ of EK . At the end of 40'' it has the position $ob\phi$, and Eo is $\frac{2}{3}$ of EK . And at the end of the minute, it has the position $K\gamma\chi i$.

It is plain that the common intersection of these two lines will always be found in the diagonal Ei of the parallelogram $EKiI$; for $EmgG$ is a parallelogram similar to $EKiI$, because $EG:EI = Em:EK$. In like manner $EobH$ is a parallelogram similar to $EKiI$. These parallelograms are therefore about a common diameter Ei .

Further, the motion of the point of intersection of these lines is uniform; because $EG:EI = Eg:Ei$, and $EH:EI = Eb:Ei$, &c.; and therefore the spaces Eg , Eb , Ei are proportional to the times.

And thus it appears that the intersection of two lines, each of which moves uniformly in the direction of the other, moves uniformly in the direction of the diagonal of the parallelogram formed by the lines in their first or last position, and that the velocity of the intersection is to the velocity of each of the motions of the lines as the diagonal

diagonal is to the side in whose direction the motions are performed.

This motion of the intersection may, with great propriety of language, be said to be constituted by, or *compounded* of, the two motions in the direction of the sides. For the point g of the line $G\gamma$ is, at the instant, moving eastward, and the same point g of the line $mg n$ is moving southward. Therefore, if the point g be considered as a point of both lines (as if it were a ring embracing both) it partakes, in every instant, of both motions.

It is also evident that the point g separates from G in the same direction, and with the same velocity, as if $E K$ had remained at rest, and the ring had moved to m . Also it separates from the point o at the same rate, and in the same direction as if it had moved from E to G . The motion along $E i$ therefore contains both of the motions along $E I$ and along $E K$, and is really identical with a motion compounded of those motions, plainly indicating both, or the determination to both. Accordingly, we say that in every situation of the point of intersection, its velocity is compounded of the velocity $E I$ and the velocity $E K$. If therefore $E I$ has been a previous motion, that is, if a body was moving so that, had its motion continued unchanged, it would have described $E I$ uniformly in a minute, but we observe that after coming to E , it turns aside, and describes $E i$ uniformly in a minute, we should say that the change which it sustains in the point E , is a motion $E K$. For, if the
body

body had been previously at rest in E, and we observe it describe EK in a minute, then the motion EK is, unquestionably, the change which it has sustained. The motion E*i* is not the change; for had EL been the primitive motion, the same motion E*i* would have resulted from compounding the motion EM with it. Now, since EL is different from EI, it is impossible that the *same* change can make the new conditions the same.

Moreover, there is no other motion, which, by compounding it with EI, will produce the motion E*i*.

And lastly, the motion EK is the only circumstance of sameness between changing the motion EI into the motion E*i*, and giving the motion EK to a body previously at rest.

After a mature consideration of all these conditions, we may say, that

A change of motion is that motion which, by composition with the former state of motion, produces the new motion.

80. This composition of motion is usually presented to the mind in a way somewhat different. A body is supposed to move uniformly in the direction EI, while the space in which this motion is performed is carried uniformly in the direction EK. But we *cannot conceive* a portion of space to be moved out of its place. We can conceive the composition very distinctly by supposing a man walking along a line EI drawn on a field of ice, while the ice is floating in the direction EK. This will produce the very motion E*i*, and affords the clearest
 notion

motion of the composition. If one man stands still, and another walks in the direction and with the velocity $E I$, and a third in the direction and with the velocity $E L$, while the ice floats in the direction and with the velocity $E K$, then the new condition of the first man will be the motion $E K$, that of the second will be $E i$, and that of the third will be $E Q$. There can be no doubt of these three men having sustained the very same change of motion. Now, the only circumstance of sameness in these three new conditions is the composition of their former condition with a motion $E K$.

The reflecting reader will perceive, however, that this way of illustrating the subject, by the motion on moving ice, is not precisely a composition of two *determinations to motion*. This is completed in the first instant. As soon as the motion in the direction and with the velocity $E i$ begins, there is no need of further exertion; the motion will continue, and $E i$ will be described. But it serves very well to exhibit to the mind the *mathematical* composition of *two motions*, which is all we want at present. We have shewn that, in the result of this combination, all the characteristics of the two determinations are to be found, because the point of intersection, whether we consider it as a material existence, or as a mere mathematical conception, partakes of both motions. There is a physical question which will come under consideration afterwards, that is very different from the present, namely, Whether two natural powers, which are known to be productive, separately,

of

of two determinations of a body to two distinct motions, will, by their joint action, produce a determination to that motion which is compounded of those which they would produce separately?—This is a question of very difficult solution; but we trust that the notions already acquired will enable us to give an answer with confidence.

81. Thus then have we obtained a general mark or characteristic of a change of motion, perfectly consonant with our mark and measure of every moving cause, namely, the very motion which we conceive it to produce. Nay, perhaps what we have just now established is the foundation even of our former measures. For every acceleration, or retardation, or deflection, may be considered as a new motion, compounded with the former. This is not a mere substitution, to aid the imagination; for it is, almost always, the very fact. For what we take for the beginning of motion, in all our actions on bodies, and all our observations of the bodies which surround us, is in fact only a change induced on a motion already existing, and exceedingly rapid. This results from the motion of rotation, by which we are carried round the axis of the earth, and even this is compounded with the motion of revolution round the sun. What we consider as changes of motion, and therefore as the proper measures and marks of the changing causes, the powers of mechanical nature, are indeed changes, and the very changes that we imagine. But they are by no means changes of the motions that we imagine.

We

We shall soon learn, that if we measure or estimate the changes of motion in the way now proposed, all our deductions will be perfectly conformable to the appearances of nature, and the inferences of their causes perfectly consistent and legitimate, giving us accurate knowledge of those causes. And we shall find that *no other way of estimating and measuring the changes of motion* will have these qualities. Thus we demonstrate the justness of our principle, and that it gives a sufficient ground for mechanical science.

82. Since the actual composition of motion is so general in the phenomena of the universe, that it obtains in every motion and change of motion that we can produce or observe, and since the characteristic which we have assumed of a change of motion is the same, whatever the previous motion may have been, and therefore is equally applicable to motions which are really simple, and such as we observe them, it is plain that a knowledge of the *general* results of this composition of motion must greatly promote our knowledge of mechanical nature. We shall therefore consider them in order.

83. The general theorem, to which all others may be reduced, is the following.

Two uniform motions, having the directions and velocities represented by the sides EI, EK, of a parallelogram, compose a uniform motion in the diagonal. This is already demonstrated. For the motion of the point of intersec-

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tion

tion of these two lines, while each moves uniformly in all its points, in the direction of the other, is, in every instant, composed of these two motions, and is the same as if a point described EI uniformly, while EI is uniformly carried in the direction EK . And this motion is along the diagonal Ei , and is uniform, as has been already shewn. Also, because EI and Ei are described in the same time, the velocities of the motions along EI , EK , and Ei , are proportional to those lines.

84. *Cor. 1.* The COMPOUND MOTION Ei is in the same plane with the two CONSTITUENT or SIMPLE MOTIONS EI and EK . For a parallelogram lies all in one plane.

85. *Cor. 2.* The motion Ei may arise from the composition of any two uniform motions, which have the direction and velocities represented by the sides of any parallelogram $ELiM$, or EiK , of which Ei is the diagonal.

Cases frequently occur, where we know the directions of the two simple motions which compose an observed motion, but do not know the proportion of their velocities. The velocity is ascertained by this proposition, because the direction of the three motions, viz. the two simple and the compound motions, determines the species of parallelogram, and the ratio of the sides.

Sometimes we have the direction and the velocity of one of the simple motions, and therefore its proportion

to that of the observed compound motion. The direction and velocity of the other is also found by this proposition, because these data also determine the parallelogram.

The motion in the diagonal is evidently equivalent to the motions in the sides combined. Thus, if the moveable first describe EI , and then Ii (or EK), it will be in the same point i as if it had described Ei . Therefore Ei is frequently called the EQUIVALENT motion, the RESULTING motion.

It frequently gives great assistance in our investigations, if we substitute for an observed motion such motions as will produce it by composition. This is called the RESOLUTION OF MOTIONS. It is in this way that the navigator generally computes the ship's change of situation at the end of a day, in which she has perhaps sailed in many different courses. He considers how much he has gone to the eastward, or westward, and how much to the northward or southward, on each course; and he then adds together all his eastings, and all his southings, and then supposes that the ship has sailed for the whole day on that unvaried course which would be produced by the same easting and southing combined.

In like manner, it is very useful for the mechanician to consider how much his observed motion has advanced the body in some particular direction, EF , for example. (fig. 10). To do this, he considers the motion AB as composed of a motion AC parallel to the given line EF ,

and another motion AD perpendicular to EF , AB forming the diagonal of a parallelogram $ACBD$, of which one side AC is parallel, and the other AD is perpendicular to EF . It is plain that the motion AD neither promotes nor obstructs the progress in the direction EF , and that the body has advanced in the direction of EF , just as much as if it had moved from a to b , instead of moving from A to B .

This proceeding is called ESTIMATING a motion in a given direction, or REDUCING it to that direction.

In like manner, the mechanician is said to estimate a motion AB (fig. 11.) in a given plane $EFGH$, when he considers it as composed of a motion AD perpendicular to that plane, and AC parallel to it. The lines DA , BC being drawn perpendicular to the plane, cut it in two points a and b , and AC is parallel to ab .

86. Any number of motions AB , AC , AD , AE (fig. 12.) may be thus compounded, forming a motion AF . The method for ascertaining the motion resulting from this composition is as follows. AB , compounded with AC , produces the motion AG . This, compounded with AD , produces AH ; and this, compounded with AE , produces AF .

The same final situation F will be found by supposing all the motions AB , AC , AD , AE , to be performed in succession. Thus the moveable describes AB ; then BG , equal and parallel to AC ; then GH , equal and parallel to AD ; and then, HF , equal and parallel to AE .

NOTE.

NOTE.—It is not necessary that all these motions be in one plane.

87. Three motions AB , AC , AD (fig. 13.) which have the direction and proportions of the sides of a parallelopiped, compose a motion in the diagonal AF of that parallelopiped; for AB and AC compose AE , and AE and AD compose AF .

The mine-surveyor proceeds in this way. Like the navigator, he sets down any gallery of the mine, not directly by its real position, but enters his table with its easting or westing, and with its northing or southing. But he also keeps an account of its rise or dip. He refers all his measures to three lines, one running east and west, one running north and south, and one running perpendicularly up and down. These three lines are evidently like the three angular boundaries AB , AC and AD of a rectangular box.

This is now the constant procedure of the mechanic, in his more elaborate investigations. It was first practised (we think) by M'Laurin, in the excellent physico-mathematical speculations which are to be found in his Treatise on Fluxions. The mechanic refers all motions to three *co-ordinate* lines AB , AC , AD , which are perpendicular to each other, and his ultimate result is the diagonal AF of some parallelopiped.

88. Hitherto we have considered the composition of uniform motions only. But *any* motions may be compounded,

pounded, as we may easily conceive, by supposing a man to walk on a field of ice along any crooked path, while the ice floats down a crooked stream.

Thus, a uniform motion in the direction AB (fig. 14.) may be compounded with a uniformly accelerated motion in the direction AC . Such a motion is observed when we see a stone fall from the mast-head of a ship sailing steadily forward in the direction AB ; for this stone will be observed to fall down parallel to a plummet hung from the mast-head. The real motion of the stone will therefore be a parabolic arch $A b f g$, which AB touches in A ; for while the mast-head describes the equal lines AB, BF, FG , the stone has fallen to β and ϕ and γ , and the line ACA' has got into the positions BB', FF', GG' , so that $A\phi$ is four times $A\beta$; and $A\gamma$ is nine times $A\beta$. Therefore $A\beta, A\phi, A\gamma$, are as the squares of $\beta b, \phi f$, and γg , and the line $A b f g$ is a parabola.

It is in this way that a nail in the sole of a cart-wheel describes a cycloid, while the cart moves along a smooth plane. This is the composition of a progressive motion with an equal circular motion. The geometrical lectures of Dr Barrow contain many beautiful examples of such compositions of motion; and it was by introducing this process into mathematical reasoning, that this celebrated geometer gave a new department to the science, which quickly extended it far beyond the pale of the ancient geometry of the Greeks, and suggested to Sir Isaac Newton his doctrine of Fluxions.

89. When two motions, however variable, are compounded, we discover the direction and velocity of the compound motions in any instant, if we know the direction and velocities of each of the simple motions *at that instant*. For we may suppose, that, at that instant, each motion proceeds unchanged. Then we construct a parallelogram, the sides of which have the directions and proportions of the velocities of the simple motions. The diagonal of this parallelogram will express the direction and velocity of the compound motion.

90. On the other hand, knowing the direction and velocity of the compound motion, and the directions of each of the simple motions, we discover their velocities.

91. When a curvilinear motion ADV (fig. 15.) results from the composition of two motions, whose directions we know to be AC and AF , we learn the velocities of the three motions in any point D , by drawing the tangent DI , and the ordinate Db parallel to one of the simple motions, and from any point L in that ordinate, drawing LI parallel to the other motion, cutting the tangent in I . The three velocities are in the proportion of the three lines IL , LD , and ID . This is of very frequent use.

Since the phenomena are our only marks and measures of their supposed causes, it is plain that every mistake with respect to a change of motion, is accompanied by a mistake in our inference of its cause. Such mistakes

takes

takes are avoided with great difficulty, because the motions which we observe are, at all times, extremely different from what we take them to be. A book lying on the table seems to be at rest; but it is really moving with a prodigious speed, and is describing a figure very like the figure described by a nail in the nave of a coach-wheel while the carriage is going over the summit of a gentle rising. We imagine that we are at rest, and we judge of the motion of another body merely by its change of distance and direction from ourselves.

Thus, if a ship is becalmed at B (fig. 16.) in a part of the ocean where there is an unknown current in the direction BD; and if the light of another ship is seen at A, and if A really sails to C while B floats to D, A will not appear to have sailed along AC, but along AK; for when B is at D, and A at C, A appears at C, having the bearing and distance DC. Therefore, if AK be made equal and parallel to DC, it will have the same bearing by the compass, and the same distance from B that C has from D; and therefore the spectator in B, not knowing that he has moved from B to D, but believing himself still at B, must form this opinion of the motion of A.—In the same manner it must follow, that our notions of the planetary motions must be extremely different from the motions themselves, if it be true that this earth is moving to the eastward at the rate of nearly twenty miles in every second. It would seem a desperate attempt therefore for us to speculate concerning the powers of nature by which these motions are regulated.

And,

And, accordingly, nothing can be conceived more fantastical and incongruous than the opinions formerly entertained on this subject. But Mathematics affords a clue by which we are conducted through this labyrinth.

92. *The motion of a body A relative to, or as seen from, another body B, which is also in motion, is compounded of the real motion of A, and the opposite to the real motion of B. (Fig. 16.)*

Join AB , and draw AE equal and parallel to BD , and complete the parallelogram $ACFE$, and join ED and DC . Also produce EA till AL is equal to AE or BD , and complete the parallelogram $LACK$, and draw AK and BK . Had A moved along AE while B moves along BD , they would have been at E and D at the same time, and would have the same bearing and distance as before. If the spectator in B is insensible of his own motion, A will appear not to have changed its place. It is well known that two ships, becalmed in an unknown current, appear to the crews to remain at rest. It is plain, therefore, that the real position and distance DC are the same with BK , and that if the spectator in B imagines himself at rest, the line AK will be considered as the motion of A . This is evidently composed of the motion AC , which is the real motion of A , and the motion AL , which is equal and opposite to the motion BD .

93. In like manner, if BH be drawn equal and opposite to AC , and the parallelogram $BHGD$ be completed,

pleted, and BG and AK be drawn, the diagonal BG will be the motion of B relative to A . (92.) Now, it is plain that $KAGB$ is a parallelogram. The relative position and distances of A and B at the end of the motion are the same as in the former case. B appears to have moved along BG , which is equal and opposite to AK . Therefore, *the apparent or relative motions of two bodies are equal and opposite, whatever the real motions of both may be, and therefore give no immediate information concerning the real motions,*

94. It needs no farther discussion to prove the same propositions concerning every *change* of motion, viz. that the relative *change* of motion in A is composed of the real change in A , and of the opposite to the motion, or change of motion in B .

Suppose the motion BD to be changed into $B\delta$. This has arisen from a composition of the motion BD with another $D\delta$; draw $C\alpha$ equal and opposite to $D\delta$, and complete the parallelogram $EC\alpha\epsilon$. The diagonal $E\alpha$ is the apparent or relative change of motion. For the bearing and distance δC is evidently the same with $D\alpha$, because the lines δC and $D\alpha$ which join equal and parallel lines are equal and parallel.

95. Therefore, if no change happen to A , but if the motion of B be changed, the motion of A will *appear* to be equally changed in the opposite direction.

Hence we draw a very fortunate conclusion, that the observed or relative changes of motion are equal to the
real

real changes. But we remain ignorant of its direction, because we may not know in which body the change has happened. $E\varepsilon$ is the apparent *change* of motion of the body A, because EC was the apparent motion before the change into Ez . Complete the parallelogram $ACx\alpha$. The diagonal Az would have been the motion of A, had its motion AC sustained the composition or change $A\alpha$. It is plain that either the motion $D\delta$, compounded with BD, or the motion Az compounded with AC, will produce the same apparent or relative change of motion. Still, however, it is important to learn that the apparent and real changes are the same in magnitude; because they give the same indication of the magnitude of the changing cause.

96. It is evident that if we know the real motion of B, we can discover the real motion of A, by considering its apparent motion EC as the diagonal of a parallelogram of which one side EA is equal and opposite to the known motion BD. It must therefore be AC.

97. In like manner, if any other circumstances have assured the spectator in B, that AC is the true motion of A, which had appeared to him to move along AK, he must consider AK as the diagonal of a parallelogram ALKC, and then he learns that B has moved over a line BD, equal and opposite to AL. It was in this manner that Kepler, by observations on the planet Mars, discovered the true form of the earth's orbit round the Sun.

98. If equal and parallel motions be compounded with all and each of the motions of any number of bodies, moving in any manner of way, their relative motions are not changed by this superinduction. For, by compounding it with the motion of any one of the bodies, which we may call A, the *real* motion of A is indeed changed. But its motion relative to another body B, or its apparent motion as seen from B, is compounded of the real change (94.), and of the opposite to the real change in B, that is, opposite to the real change in A, and therefore destroys that change, and the relative motion of A remains the same as before.—In this manner, the motions and evolutions of a fleet of ships in a current which equally affects them all, are not changed, or are the same as if made in still water. The motions in the cabin of a ship are not affected by the ship's progressive motion; nor are the relative motions on the surface of this globe sensibly affected by its revolution round the sun. We should remain for ever ignorant of all such common motions, if we did not see other bodies which are not affected by them. To these we refer, as to so many fixed points.

4. *Of Motions continually Deflected.*

99. A curvilinear motion is a case of continual deflection. It is susceptible of infinite varieties, and its modifications and chief properties are of difficult investigation.

The simplest case of curvilinear motion is that of uniform motion in a circular arch. Here, the deflections in equal times from rectilinear motion are equal. But, should the velocity be augmented, it is plain that the momentary deflection is also augmented, because a greater arch will be described, and the end of this greater arch deviates farther from the tangent; but it is not easy to ascertain in what proportion it is increased. When one uniform rectilinear motion AB (fig. 17.) is deflected into another BC , we ascertain the linear deflection by drawing a line from the point c , at which the body would have arrived without deflection, to the point C , to which it really does arrive. And it is the same thing whether we draw dD , or cC , in this manner, because these lines, being proportional to Bd , Bc , will always give the same measure of the velocities (41.), and the lines of deflection are all parallel, and therefore assure us of the direction of the deflection in the point B . But it is otherwise in any curvilinear motion. We never have $dD : cC = Bd : Bc$; moreover, it is very rarely that dD , cC , &c. are parallel. We know not therefore which of these lines to select for an indication of the direction of the deflection at B , or for a measure of its magnitude.

Not only does a greater velocity in the same curve cause a greater deflection, but also, if the path be more incurvated, an arch of the same length described with the same velocity, deviates farther from the tangent. Therefore, if a body move uniformly in a curve of variable curvature, the deflection will be greater where the curvature is greater.

We may learn from these general remarks, that the directions and the measures of the deflections by which a body deviates *continually* into a curvilinear path, can be ascertained, only by investigating the ultimate positions and ratios of the lines which join the points of the curve with the simultaneous points of the tangent, as the points δ and C are taken nearer and nearer to B. Some rare, but important cases occur, in which the lines joining the simultaneous points c and C, d and δ , &c. are parallel. In such cases, the deflection in B is certainly parallel to them, and they are cases of the composition of a motion in the direction of the tangent with a motion in the direction of the lines c C, d δ , &c. But, in most cases, we must discover the direction of the deflection in B, by observing what direction the lines d δ , c C, &c. taken on both sides of B, continually approximate to. The following general proposition, discovered by the illustrious Newton, will greatly facilitate this research.

100. *If a body describe a curve line ABCDEF (fig. 18.) which is all in one plane, and if there be a point S in this plane, so situated, that the lines SA, SB, SC, &c. drawn to the curve, cut off areas ASB, ASC, ASD, &c. proportional to the times of describing the arches AB, AC, AD, &c. then are the deflections always directed to this point S.*

Let us first suppose that the body describes the polygon ABCDEF, formed of the chords of this curve, and that it describes each chord uniformly, and is deflected

flexed only in the angles $B, C, D,$ &c. Let us also (for the greater simplicity of argument) suppose that the sides of this polygon are described in equal times, so that (by the hypothesis) the triangles $ASB, BSC, CSD,$ &c. are all equal.

Continue the chords $AB, BC,$ &c. beyond the arches, making Bc equal to AB , and Cd equal to BC , and so on. Join $cC, dD,$ &c. and draw $cS, dS,$ &c.; also draw Cb parallel to cB or BA , cutting BS in b , and join bA , and draw CA , cutting Bb in o . Lastly, make a similar construction at E .

Then, because cB is equal to BA , the triangles ASB and BSc , are equal, and therefore BSc is equal to BSC ; but they are on the same base SB . Therefore they are between the same parallels; that is, cC is parallel to BS , and BC is the diagonal of a parallelogram $BbCc$. The motion BC therefore is compounded of the motions Bc and Bb , and Bb is the deflection, by which the motion Bc is changed into the motion BC ; therefore the deflection in B is directed to S .—By similar reasoning fF , or Ei , is the deflection at E , and is likewise directed to S ; and the same may be proved concerning every angle of the polygon.

Let the sides of this polygon be diminished, and their number increased without end. The demonstration remains the same, and continues, when the polygon exhausts or coalesces with the curvilinear area, and its sides with the curvilinear arch.

Now, when the whole areas are proportional to the times, equal areas are described in equal times; and
therefore,

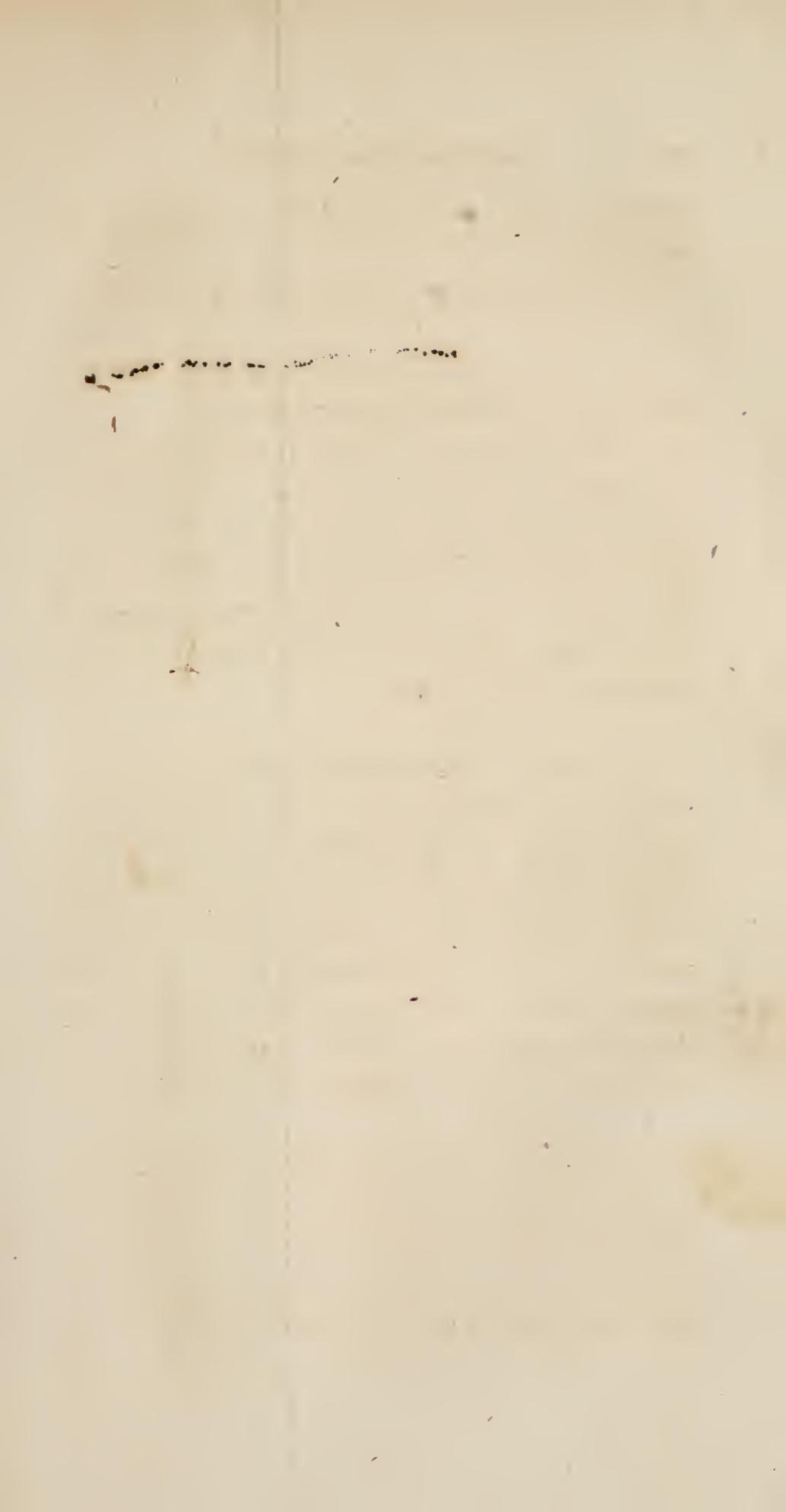
therefore, in such motion, the deflections are always directed to S.

This point S may be called the *centre of deflection*.

101. *If the deflection by which a curve line ADF is described, be continually directed to a fixed point, the figure will be in one plane, and areas will be described round that point proportional to the times.* For B C is the diagonal of a parallelogram, and is in the plane of S B and B c (84.); and c C is parallel to B S, and the triangles S B C, S B c, and S B A, are equal. Equal areas are described in equal times; and therefore areas are described proportional to the times, &c. &c.

102. *Cor. 1. The velocities in different points of the curve are inversely proportional to the perpendiculars S r and S t (fig. 19.) drawn from S on the tangents A r, E t in those points of the curve.* For, because the elementary triangles A S B, E S F, are equal, their bases A B, E F, are inversely as their altitudes S r, S t. These bases, being described in equal times, are as the velocities, and they ultimately coincide with the tangents at A and E. Therefore the velocity in A is to that in E as S t to S r.

103. *Cor. 2. The angular velocities round S are inversely as the squares of the distances.* For, if we describe round the centre S the small arches B a, F b, they may be considered as perpendiculars on S A and S E; also with the distance S F describe the arch g b. It is evident that



that $g b$ is to $F \delta$ as the angle $A S B$ to the angle $E S F$. Now, since the areas $A S B$, $E S F$, are equal, we have $B \alpha : F \delta = S E : S A$.

But $g b : B \alpha = S E : S A$
 therefore $g b : F \delta = S E^2 : S A^2$
 and $A S B : E S F = S E^2 : S A^2$

104. We now proceed to determine the magnitude of the deflection, or, at least, to compare its magnitude in B , for example, with its magnitude in E . In the polygonal motion (fig. 18.) the deflection in B is to that in E as the line $B b$ to the line $E i$; for $B b$ and $E i$ are the motions, which, by composition with the motions $B c$ and $E f$, make the body describe BC and EF . Therefore, when the sides of the polygon are diminished without end, the ultimate ratio of $B b$ to $E i$ is the ratio of the deflection at B to the deflection at E .

In order to obtain a convenient expression of this ultimate ratio, let $A B C X Z Y$ be a circle passing through the points A, B, C , and draw $B S Z$ through the point S , and draw $C Z, A Z$.

The triangles $B C b$ and $A Z C$ are similar; for $C b$ was drawn parallel to $c B$ or $B A$. Therefore the angle $C b B$ is equal to the ultimate angle $b B A$ or $Z B A$, which is equal to the angle $Z C A$, being subtended by the same chord $Z A$; also $C B b$, or $C B Z$, is equal to $C A Z$, standing on the same chord $C Z$. Therefore, the remaining angles $b C B$ and $C Z A$ are equal, and the triangles are similar; therefore $B b : C A = B C : A Z$.

K

Now,

that $g b$ is to $F \delta$ as the angle ASB to the angle ESF . Now, since the areas ASB , ESF , are equal, we have $B \alpha : F \delta = SE : SA$.

$$\begin{aligned} \text{But} & \quad g b : B \alpha = SE : SA \\ \text{therefore} & \quad g b : F \delta = SE^2 : SA^2 \\ \text{and} & \quad ASB : ESF = SE^2 : SA^2 \end{aligned}$$

104. We now proceed to determine the magnitude of the deflection, or, at least, to compare its magnitude in B , for example, with its magnitude in E . In the polygonal motion (fig. 18.) the deflection in B is to that in E as the line Bb to the line Ei ; for Bb and Ei are the motions, which, by composition with the motions Bc and Ef , make the body describe BC and EF . Therefore, when the sides of the polygon are diminished without end, the ultimate ratio of Bb to Ei is the ratio of the deflection at B to the deflection at E .

In order to obtain a convenient expression of this ultimate ratio, let $ABCXYZ$ be a circle passing through the points A, B, C , and draw BSZ through the point S , and draw CZ, AZ .

The triangles BCb and AZC are similar; for Cb was drawn parallel to cB or BA . Therefore the angle CbB is equal to the ultimate angle bBA or ZBA , which is equal to the angle ZCA , being subtended by the same chord ZA ; also CBb , or CBZ , is equal to CAZ , standing on the same chord CZ . Therefore, the remaining angles bCB and CZA are equal, and the triangles are similar; therefore $Bb : CA = BC : AZ$.

K

Now,

Now, since, by continually diminishing the sides of the polygon, the points A and C continually approach to B, and CA continually approaches to cA or to $2cB$, or $2CB$, and is ultimately equal to it; also AZ is ultimately equal to BZ. Therefore, ultimately, $Bb : 2BC = BC : BZ$, and $Bb \times BZ = 2BC^2$, and $Bb = \frac{2BC^2}{BZ}$.

In like manner, at the point E, we shall have Ei ultimately equal to $\frac{2EF^2}{Ez}$, Ez being that chord of the circle through D, E, and F, which passes through i .

$$\text{Therefore } Bb : Ei = \frac{2BC^2}{BZ} : \frac{2EF^2}{Ez}.$$

The ultimate circle, when the three points A, B, C, coalesce, is called the CIRCLE OF EQUAL CURVATURE, or the EQUICURVE CIRCLE, coalescing with the curve in B in the most close manner. The chord BZ of this circle, which has the direction of the deflection in B, may be called its DEFLECTIVE CHORD.

Since BC and EF are described in equal times, they are proportional to the velocities in B and E. Therefore, we may express this proposition in the following words:

In curvilinear motions, the deflections in different points of the curve are proportional to the square of the velocities in those points, directly, and to the deflective chords of the equicurve circles in those points, inversely.

It must be here remarked, that this theorem is not limited to curvilinear motions, in which the deflections are always directed to one fixed point, but extends to all

all curvilinear motions whatever. For it may evidently be expressed in this manner; *The deflecting forces are ultimately proportional to the squares of the arches described in equal times, directly, and to the deflective chords of the equicurve circle, inversely.*

The equable description of areas only enabled us to see that the lines BC and EF were described in equal times, and therefore are as the velocities.

It will be convenient to have a symbolical expression of this theorem. Therefore, let the deflective chord of the equicurve circle be represented by c , and the deflection by d , the theorem may be expressed by

$$d \doteq \frac{v^2}{c}, \text{ or } d = \frac{2 \text{ arch}^2}{c}$$

105. REMARK.—The line Bb is the linear deflection, by which the uniform motion in the chord AB is changed into a uniform motion in the chord BC , or it is the deviation cC from the point where the moveable would have arrived, had it not been deflected at B . But, in the present case of curvilinear motion, the lines Bb and Bc express the measures of the velocities of these motions, or the measures of the determinations to them. Bc is to Bb as the velocity of the progressive motion is to the velocity of the deflection, generated during the description of the arch BC . But, because the deflection in the arch has been continual, and because it is to be measured, like acceleration, by the velocity which is generated uniformly during a given moment of time, it

may be measured by the velocity generated during the description of the arch BC . Its measure therefore will be double of the space through which the body is *actually* deflected in that time from the tangent in B . The space described will be only one half of Bb , or it will be BO . Now, this is really the case; for the tangent is ultimately parallel to OC , and bisects cC ; so that although the deflection from the tangent to the curve is only half of the deflection from the produced chord to the curve; yet the velocity gradually generated is that which will produce the deflection from the produced chord, or is that which constitutes the polygonal motion in the chords.

It is perfectly legitimate, therefore, to reason from the subsultory deflections of a polygonal motion to the continual deflections in a curvilinear motion; for the deflections in the angles of the polygon have the same ratio to one another with the deflections in the same points of the curve. But we must be careful not to confound the deflections from the tangent with those from the chords. This has been done by eminent mathematicians. For the employment of algebraical expressions of the increments of the abscissæ and ordinates of curves, always gives the true expression of the deflections in a polygonal motion. But, when we turn our thoughts to the figures, and to the curvilinear motions themselves, we naturally think of the deflections (such as we see them) from the tangent to the curve. We then make geometrical inferences, which are true only when affirmed of the curvilinear

vilineal motions. We are apt to mix and confound these inferences with the results of the fluxionary calculus, which always refer to the polygon. By thus mixing quantities that are incongruous, some celebrated mathematicians have committed very gross mistakes.

It is, in general, most convenient, and surely most natural, to use the ultimate ratio of the actual deflections from the tangent, or $\frac{BC^2}{BZ}$; and this even gives us its measure in feet or inches, when we know the dimensions of the figure described. Thus we know that, in one minute, the Moon, when at her mean distance, deflects 193 inches from the tangent to her orbit round the Earth, and that the earth deviates 424 inches in the same time from the tangent of her orbit round the Sun.

106. The velocity in any point of a curvilinear motion is that which would be generated by the deflection in that point, if continued through $\frac{1}{4}$ of the deflective chord of the equicurve circle. Let x be the space along which the body must be accelerated in order to acquire the velocity BC .

We have Bb^2 , or $4BO^2 : BC^2 = BO : x$ (57) and therefore $x = \frac{BC^2 \times BO}{4BO^2}$, $= \frac{BC^2}{4BO}$, and $4x = \frac{BC^2}{BO}$, or $BO : BC = BC : 4x$. But $BO : BC = BC : BZ$. Therefore $x = \frac{1}{4} BZ$.

RECAPITULATION.

Thus have we obtained marks and measures of all the principal affections of motion.

The acceleration a is $\frac{\dot{v}}{t}$ (71) or $\frac{v \dot{v}}{s}$ (72) or $\frac{\dot{s}}{t^2}$ (65)

The momentary variation of velocity $\dot{v} = a t$ (71)

The momentary variation of the square of velocity

$$2 v \dot{v} = 2 a s \quad (72)$$

The momentary deflection $d = \frac{\text{arc.}^2}{\text{chord}}$ (105)

The deflective velocity $= \frac{2 v^2}{c}$ (104)

But, in order to apply the doctrines already established with the accuracy of which physico-mathematical subjects are susceptible, it is necessary to select some point in any body of sensible magnitude, or in any system of bodies, by the position or motion of which we may form a just notion of the position and motion of the body or system. It is evident that the condition which ascertains the propriety of our choice, is, that *the position, distance, or motion of this point shall be a medium or average of the positions, distances, and motions of every particle of matter in the assemblage.*

107. This will be the case, if the point be so situated that, if a plane be made to pass through it in any direction *whatever*, and if perpendiculars be drawn to this plane from every particle of matter in this assemblage, the sum of all the perpendiculars on one side of this
plane

plane is equal to the sum of all the perpendiculars on the other.

That there may be found in every body such a point, is demonstrated (after Boscovich) in the *Encycl. Britan.* Art. *Position (Centre of)*.

Let P (fig. 20.) be a point so situated, and let QR be a plane (or rather the section of a plane, perpendicular to the plane of the paper) at any distance from the body. The distance Pp of P from this plane, is the average of all the distances of each particle. For, let the plane APB pass through this point, parallel to the plane QR. The distance CS of a parallel C from this plane is DS — DC, or Pp — DC; and the distance GT of a particle G is HT + GH, or Pp + GH. Let n be the number of particles between QR and AP; and let o be the number on the other side of AP; and let m be the number of particles in the whole body, that is, let $m = n + o$. It is evident that the sum of all the distances, such as CS is $n \times Pp$ minus the sum of all the distances, such as CD. Also $o \times Pp$, plus the sum of the distances GH, is the sum of all the distances GT. Now, the sum of the lines CD is equal to that of all the lines GH, and therefore $\overline{n + o} \times Pp$, or $m \times Pp$, is equal to the sum of all the lines CS and GT, and Pp is the m^{th} part of this sum, or the average distance.

Now, suppose the body to have approached to the plane QR (fig. 21), and that P is now at π . It is plain that the distance πp is again the average distance, and $m \times \pi p$ is the sum of all the new distances. The difference from
the

the former sum is $m \times P \pi$, and consequently $m \times P \pi$ is the sum of the approaches of every particle; and $P \pi$ is the m^{th} part of this sum, or is the average of them all. The distance, position, and motion of this point is therefore the average position, distance, and motion of the whole body. The same demonstration will apply to any system of bodies. The point P is therefore properly chosen.

108. Since the point P is the same, in whatever direction the plane APB is made to pass through it, it follows that the last proposition is true, although the body may have turned round some centre or axis, or though the bodies of which the system consists may have changed their mutual positions.

109. The point P , thus selected, may, with great propriety, be called the CENTRE OF POSITION of the body or system.

110. If A and B (fig. 22.) be the centres of position of two bodies A and B , and if a and b express the numbers of equal particles in A and B , or their quantities of matter, the common centre C of this system of two bodies lies in the straight line AB joining their respective centres, and $AC : CB = b : a$. This is evident.

111. If a third body D , whose quantity of matter is d , be added, the common centre of position of this system.

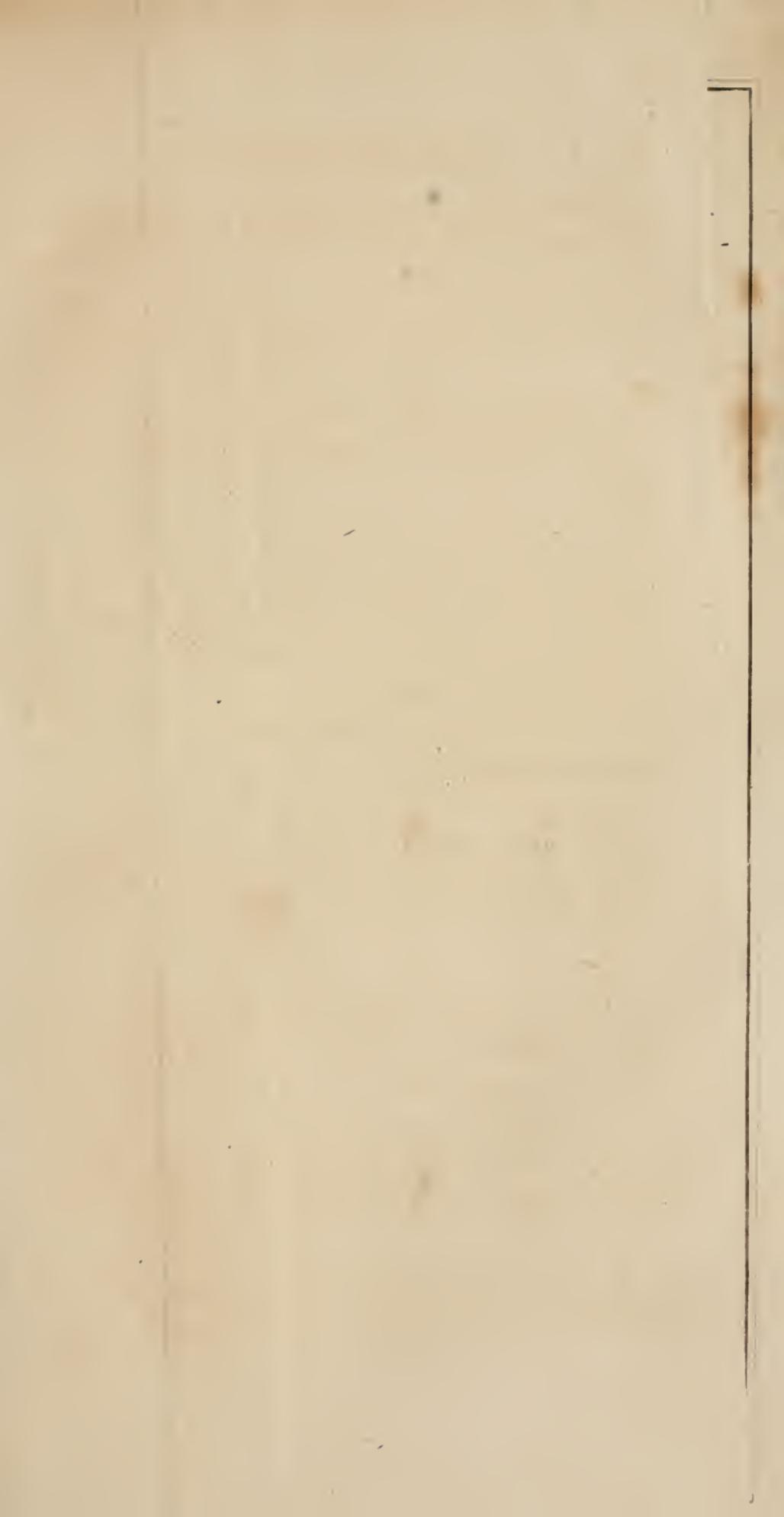


Fig. 19

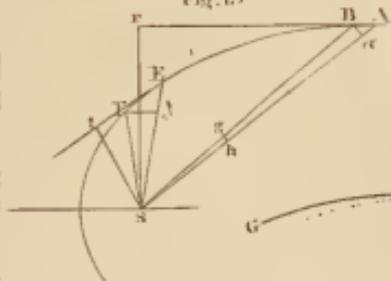


Fig. 17

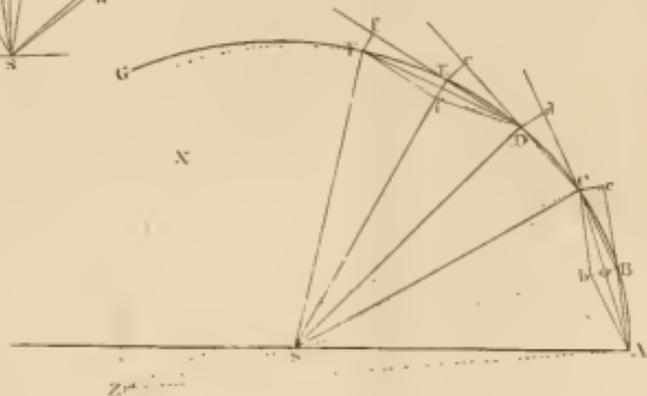
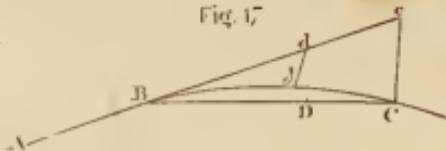


Fig. 18.



Fig. 21.

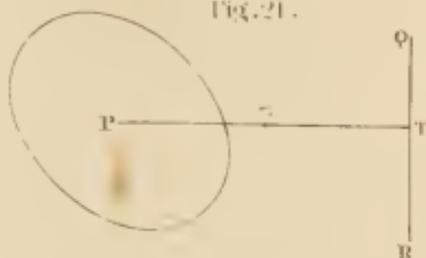
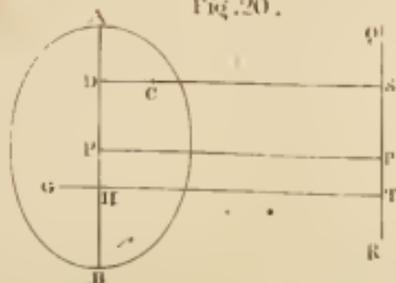


Fig. 20.



system of these three bodies lies in the straight line DC , joining D with the centre of the other two, and $DE : EC = a + b : d$.

In like manner, if a fourth body be added, the common centre of position is in the line joining it with the centre of the other three, and the distance of the fourth from this common centre, is to the distance of that from the common centre of the three, as the matter of all the three to the matter of the fourth—And the same thing is true for every addition.

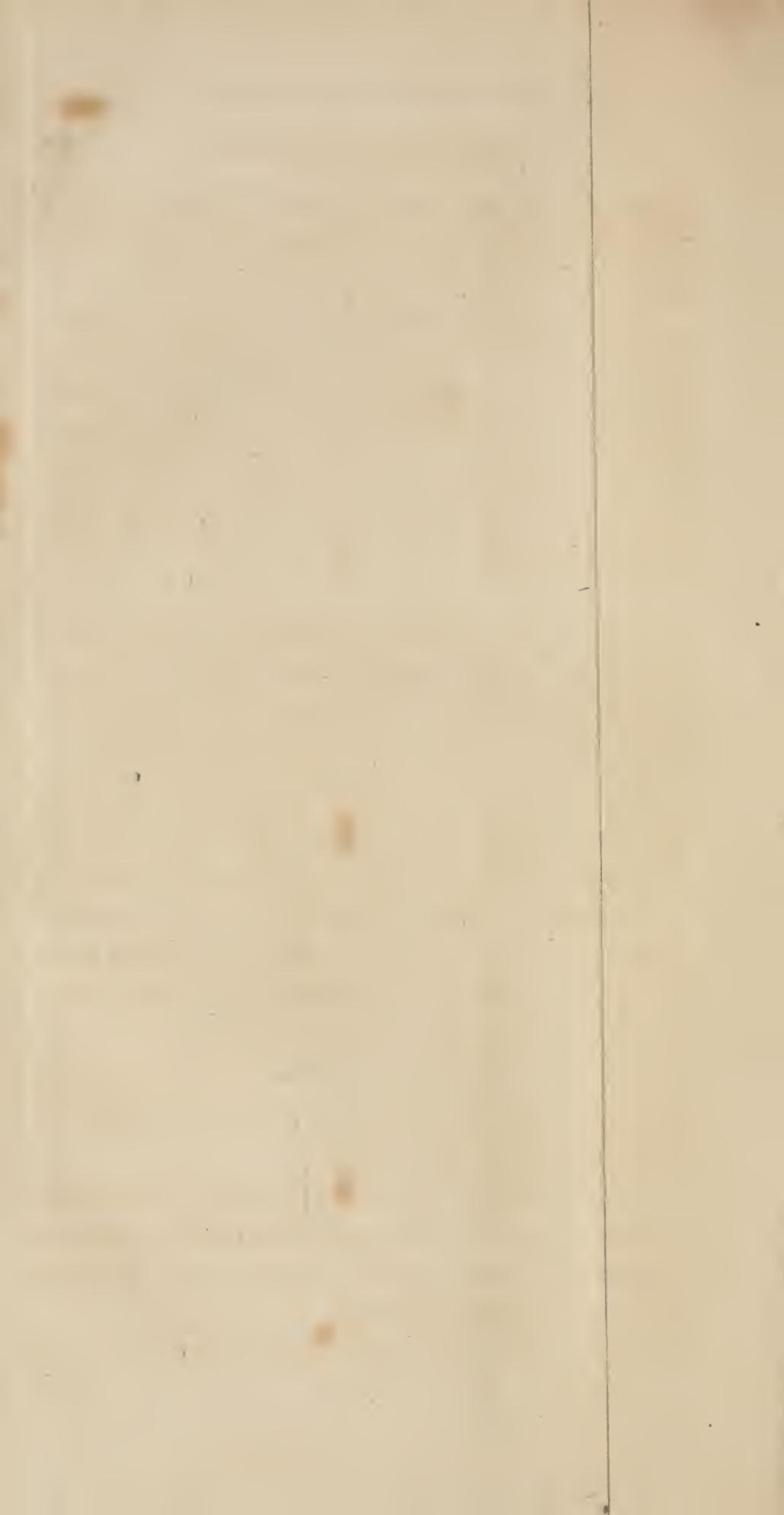
112. If the particles or bodies of any system be moving uniformly in straight lines, with any velocities and directions whatever, the centre of the system is either at rest, or it moves uniformly in a straight line.

For, let one of the bodies D move uniformly from D to F . Join F with the centre C of the remaining bodies, and make Cf to Ff as the matter in F is to that in the remaining bodies. It is plain that $E f$ is parallel to DF , and that $DF : E f = A + B : D$. In like manner, may the motion of the centre be found that is produced by that of each of the other bodies.

But these motions of the centre F are all uniform and rectilinear. Therefore, the motion compounded of them all is uniform and rectilinear.

It may happen that the motion resulting from this composition may be nothing, by reason of the contrariety of some individual motions. In this case, the centre will remain in the same point.

This obtains also, if the centres of any number of
L
bodies



system of these three bodies lies in the straight line DC , joining D with the centre of the other two, and $DE : EC = a + b : d$.

In like manner, if a fourth body be added, the common centre of position is in the line joining it with the centre of the other three, and the distance of the fourth from this common centre, is to the distance of that from the common centre of the three, as the matter of all the three to the matter of the fourth—And the same thing is true for every addition.

112. If the particles or bodies of any system be moving uniformly in straight lines, with any velocities and directions whatever, the centre of the system is either at rest, or it moves uniformly in a straight line.

For, let one of the bodies D move uniformly from D to F . Join F with the centre C of the remaining bodies, and make Cf to Ff as the matter in F is to that in the remaining bodies. It is plain that $E f$ is parallel to DF , and that $DF : E f = A + B : D$. In like manner, may the motion of the centre be found that is produced by that of each of the other bodies.

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This obtains also, if the centres of any number of
L
bodies

bodies move uniformly in right lines, whatever may have been the motion of each body, by rotation or otherwise. The motion of the common centre will still be uniform and rectilinear.

113. *Cor. 1.* The quantity of motion of such a system, is the sum of the quantities of motion of each body reduced (85.) to the direction of the centre's motion, and it is had by multiplying the quantity of matter in the whole system by the velocity of the centre.

114. *Cor. 2.* This velocity of the centre is had by reducing the motion of each particle to the direction of the centre motion, and divesting the sum of the reduced motions by the quantity of matter in the system.

115. If equal and opposite quantities of motion be any how impressed on any two bodies of such an assemblage, the motion of the centre of the whole is not affected by it. For the motion of the centre, arising from the motion of one of the bodies, being compounded with the equal and opposite motion of the other, the diagonal of the parallelogram becomes a point, or these motions destroy one another, and no change is induced thereby in the motion of the centre. The same thing must be said of equal and opposite quantities of motion being impressed on any other pair of the bodies, and, in short, on every pair that can be formed in the assemblage. Therefore the proposition is still true.

MECHANICAL PHILOSOPHY.

PART I. SECTION I.

OF MATTER.

116. THE term MATTER expresses that substance of which all things which we perceive by means of our senses are conceived to consist. It is almost synonymous, in our language, with BODY. MATERIAL and CORPORAL seem also synonymous epithets.

117. Sensible bodies are usually conceived as consisting of a number of equal PARTICLES or ATOMS of this substance. These atoms may also be supposed similar in all their qualities, each possessing such qualities as distinguish them from every thing not material.

118. But we are entirely ignorant of the essential qualities of matter, and cannot affirm any thing concerning it, except what we have learned from observation. To us, matter is a mere phenomenon. But we must as-

certain with precision the properties which we select as distinctive of matter from all other things.

119. All men seem agreed in calling that alone matter, which excludes all other substances of the same kind, or prevents them from occupying the same place, and which requires the exertion of what we call force to remove it from its place, or anyhow change its motion. These two properties have been generally called SOLIDITY OR IMPENETRABILITY, and INERTIA OR MOBILITY. Mere mobility, however, is not, perhaps, peculiar to matter; for the mind accompanies the body in all its changes of situation. When mobility is ascribed to matter, as a distinguishing quality, we always conceive force to be required. We are conscious of exerting force in moving even our own limbs. In like manner, extension, and figure, and divisibility, although primary qualities of matter, are common to it with empty space.

120. Mobility in consequence of the exertion of force may be used as a characteristic of matter, or of an atom of matter. All possess it—and probably all possess it alike, their sensible differences being the consequence of a difference in the combinations of atoms to form a particle.

121. A particle of matter under the influence of a moving force, is the object of purely mechanical contemplation, and the consideration of the changes of motion

tion which result from its condition as thus described may be called the MECHANISM of the phenomenon.

122. Perhaps all changes of material nature are cases of local motion (though unperceived by us) by the influence of moving forces. Perhaps they cannot be said to be *completely* understood, till it can be shewn how the atoms of matter have changed their situations. Perhaps the solution of a bit of silver in aqua fortis is not *completely* explained, till we shew, as the mechanician can shew with respect to the satellites of Jupiter, how an individual atom of silver is made to quit its connexion with the rest, and by what path, and with what velocity in every instant of its motion, it gets to its final state of rest, in a distant part of the vessel. But these motions are not considered by the judicious chemist. He considers the phenomenon as fully explained, when he has discovered all the cases in which the solution takes place, and has described, with accurate fidelity, all the circumstances of the operation.

123. We have derived our notions of SOLIDITY or IMPENETRABILITY chiefly from our sense of touch. The sensations got in this way seem to have induced all men to ascribe this property of tangible matter to the mutual contact of the particles—and to suppose that no distance is interposed between them.

124. But the compressibility and elasticity of all known bodies, their contraction by cold, and many ex-
amples

amples of chemical union, in which the ingredients occupy less room when mixed, than one of them did before mixture, seem incompatible with this constitution of tangible matter. Did air consist of particles, elastic in the same manner that blown bladders are, it would not be fluid when compressed into half of its usual bulk, because, in this case, each spherule would be compressed into a cube, touching the adjoining six particles in the whole of its surfaces. No liquid, in a state of sensible compression, could be fluid; yet the water at the bottom of the deepest sea is as fluid as at the surface. Some optical phenomena also shew incontrovertibly that very strong pressure may be exerted by two bodies in *physical* or *sensible* contact, although a measurable distance is still interposed between them. On the whole, it seems more probable that the ultimate atoms of tangible matter are not in mathematical contact.

125. Bodies are penetrated by other matter in consequence of their porosity. Therefore the same bulk may contain different quantities of matter.

126. DENSITY is a term, which, in strict language, expresses vicinity of particles. But, when used by the mechanician as a term of comparison, it expresses the proportion of the number of equal particles, or the quantity of matter, in one body, to the number of *equal* particles in the same bulk of another body.

127. Therefore the quantity of matter (frequently called the MASS) is properly expressed by the product of numbers expressing the bulk B and the density D. If M be the quantity of matter,

then

$$M \doteq B D$$

$$B \doteq \frac{M}{D}$$

$$D \doteq \frac{M}{B}$$

MECHANICAL PHILOSOPHY.

PART I. SECTION II.

DYNAMICS.

128. **D**YNAMICS is that department of physico-mathematical science which contains the *abstract* doctrines of moving forces; that is, the necessary results of the relations of *our* thoughts concerning motion and the causes of its production and changes.

129. Changes of motion are the only indications of the agency, the only marks of the kind, and the only measures of the intensity of those causes.

130. We cannot think of motion, *in abstracto*, as a *thing*, properly so called, that can subsist separately, but as a *quality*, or rather as a *condition*, of some other thing. Therefore we consider this condition as permanent, like the situation, figure or colour of the thing, unless some cause of change exert its influence on it.

131. Looking round us, we cannot fail of *observing* that the changes in the state or condition of a body in respect of motion, have a distinct and constant relation to the situation and distance of some other bodies. Thus, the motions of the Moon, or of a stone projected through the air, have an evident and invariable relation to the Earth. A magnet has the same to iron—an electrified body to any body near it—a billiard ball to another billiard ball, &c. &c. Such seeming dependences may be called the *mechanical relations* of bodies. They are, unquestionably, indications of *properties*, that is, of distinguishing qualities. These accompany the bodies wherever they are, and are commonly conceived as *inherent* in them; and they certainly ascertain and determine what we call their mechanical nature. The mechanician will describe a magnet, by saying that it attracts iron. The chemist will describe it, by saying that it contains the martial oxyd in a particular proportion of metal and oxygen.

132. Philosophers are not uniform, however, in their reference of the qualities indicated by those observed relations. Magnetism is a term expressing a certain class of phenomena, which are relations subsisting between magnets and iron; but many reckon it a property of the magnet, by which it attracts iron; others imagine it a property of the iron, by which it tends to the magnet. This difference generally arises from the interest we take in the phenomenon; both bodies are probably affected alike, and the property is distinctive of both: For, in all

cases that have yet been observed, we find that the indicating phenomenon is observed in both bodies;—the magnet approaches the iron, and the electrified body approaches the other. The property therefore is equally inherent in both, or perhaps in neither; for there are some philofophers, who maintain that there are no such mutual tendencies, and that the observed approaches, or, in many cases, mutual separations, are effected by the extraneous impulsion of an æthereal fluid, or of certain ministering spirits, intrinsic or extrinsic.

133. These mechanical affections of matter have been very generally called POWERS or FORCES; and the body conceived to possess them is said to ACT on the related body. This is figurative or metaphorical language. Power, and force, and action, cannot be predicated, in their original strict sense, of any thing but the exertions of animated beings; nay, it is perhaps only the exerted influence of the mind on the body which we ought to call action. But language began among simple men; they gave these denominations to their own exertions with the utmost propriety. To move a body, they found themselves obliged to exert their *strength*, or *force*, or *power*, and to *act*. When speculative men afterwards attended to the changes of motion observed in the meetings or vicinity of bodies, and remarked that the phenomena very much resembled the results of exerting their own strength or force; and when they would express this occurrence of nature, it was easier to make use

of an old term, than to make a new one for things which so much resembled; because there are always such differences in other circumstances of the case, that there is little danger of confounding them. We are not to imagine that they thought that inanimate bodies exerted strength, as they themselves did. This was reserved for much later times of refinement.—In the progress of this refinement, the word power or force was employed to express any *efficiency whatever*; and we now say, the power of aqua fortis to dissolve silver—the force of argument—the action of motives, &c. &c.

To this notion of conveniency we must ascribe, not only the employment of the words *power* and *force*, to express *efficiency* in general, but also of the terms *attraction*, *repulsion*, *impulsion*, *pressure*, &c. all of which are metaphorical, unless when applied to the actions of animals. But they are used as terms of distinction, on account of the resemblance between the phenomena and those which we observe when we pull a thing toward us, push it from us, kick it away, or forcibly compress it.

134. Much confusion has arisen from the unguarded use of this figurative language. Very slight analogies have made some animate all matter with a sort of mind, a *ὁμοίη ψυχή*, while other resemblances have made other speculatists materialize intellect itself.

The very names which we give to those powers which we fancy to be inherent in bodies, shew that we

know nothing about them. These names either, like magnetism, express a relation to the particular substances which we imagine possess the power, or they express something of the effect which suggested their existence. Of this last kind are *cohesion*, *gravity*, &c. They are almost all verbal derivatives, and should be considered by us merely as abbreviated descriptions or hints of the phenomena, or as abbreviated references to certain bodies, but by no means as any explanation of their nature. The terms are the worse by having some meaning. For this has frequently misled us into false notions of the manner of acting. Perhaps the only strict application of the term ACTION is to the effect produced by our exertions in moving our own limbs. But we think that we move other bodies, because our own body, which is the immediate instrument of the mind, is overlooked, like the plane in the hand of the carpenter, attending to the plank which he dresses.

135. Forces have been divided into IMPULSIONS and PRESSURES. Impulsions are those which produce the changes of motion by the collision of moving bodies. Pressure is a very familiar idea, and perhaps enters into every clear conception that we can form of a moving force, when we endeavour to fix our attention on it. We know that pressure is a moving force; for, by pressing round the handle of a kitchen jack, we can urge the fly into any rapidity of motion. Even when one ball puts another in motion by hitting it, we think that some-
 thing

thing precisely like our own pressure is the *immediate* producer of the motion; for if the ball is compressible, we see it dimpled by the blow. Gravity, or elasticity, and the like, are called pressing powers; because a ball, lying on a mass of soft clay, makes a pit in it, and, if lying on our hand, it excites the same feeling that another man would do by pressing on our hand. There are some indeed who call such powers, as gravity, magnetism, and electricity, SOLICITATIONS to motion. We shall soon see that this classification of forces is of no use.

136. Pressure and impulsion are thought to be essentially distinguished by this circumstance, that, in order to produce a finite velocity in a body by pressure, it must be continued for some time—as when we urge the fly of a jack into swift motion by pressing on the handle; whereas impulsion produces it in an instant.—They are also distinguished by another circumstance. The impelling body loses as much motion as the impelled body has gained; so that there seems something like the transfusion of motion from the one to the other. Accordingly, it is called the COMMUNICATION of motion. But we shall find that neither the instantaneous production of motion by impulse, nor the transfusion of it into the body, are true.

137. Some again think that impulsion is the only cause of motion, saying, ‘*Nihil movetur nisi a contigua*

'*et moto*;' and they have supposed streams of æther, which urge heavy bodies downward—which impel the iron and magnet toward each other, &c.

138. But a third sect of mechanicians say that forces, acting at a distance, as we see in the phenomena of gravitation, magnetism, and electricity, are the sole causes of motion; and they assert that such forces are exhibited; even in the phenomena of sensible contact, pressure; and impulsion.

139. The only safe procedure is to consider all the forces which we observe in action as mere phenomena. The constitution of our mind makes us infer the agency of a cause, whenever we observe a change. But, whether the exertion of force shall produce motion or heat, we know not, except by experience, that is, by observation of the phenomena. Nor will speculations about the intimate nature of these forces, and their manner of acting, contribute much to our *useful* knowledge of mechanical nature. We gain all that is possible concerning the nature of those faculties which accompany matter, or are supposed to be its inherent properties, by noticing the LAWS according to which their exertions proceed. Without a knowledge of these laws, the other knowledge is of no value.

140. It is also from the change of motion alone that we learn the *direction* of any force. Thus, by observing that

that an arrow is retarded during its ascent through the air, but accelerated during its fall, we infer, or learn, that the force of gravity acts downwards.

141. When a force is known to be in action, and yet its characteristic motion does not follow, we suppose that it is opposed by a force acting in the opposite direction. Thus the agency of that other force is detected, and its intensity may be measured. Thus, the force with which the parts of a string cohere, or with which a springy body unbends, are detected by their supporting a weight—and the magnitude of the weight is the measure of the cohesion of the string, or of the elasticity of the spring.

142. But the body in which this opposing force is thus detected, is also said to *resist* the force to which it is opposed. This is figurative language, and, as used in mechanical philosophy, it is generally improper. In wrestling, when my antagonist exerts his strength, to prevent his being thrown down, I am sensible of his exertion, and I thus learn that he resists. But should I feel no more exertion necessary than if he were a mass of lifeless matter, I should not think that he resisted. In the mechanical operations of nature, a force of any kind always produces its full effect, agreeably to the circumstances of the case, and can do no more. The force is indeed expended in producing its effect, because matter is not moved without force. The weight lying on a
spring,

spring, and keeping it in a state of tension, is as completely measured by the degree of tension which supports it, as this tension is measured by the supported weight;—neither can, with propriety, be said to resist. Silver is said to resist the dissolving power of aqua regia, but not that of aqua fortis; yet the dissolving power of aqua fortis is expended, and that of aqua regia is not. All this is very inaccurate employment of words, and this inaccuracy has done much harm in natural philosophy. The word INERTIA, which had been employed by Kepler and Newton, to express the indifference of matter as to motion or rest, or its tendency to retain its present state, has got other notions annexed to it by subsequent writers, and has been called a force, *vis inertiae*. Mr Rutherford, in his System of Natural Philosophy, lectures which he read in the University of Cambridge with great applause, is at pains to shew that matter is not merely indifferent, but RESISTS every change of motion, by exerting what he calls the *force of inactivity*, by which it preserves its condition unchanged. But, surely, this is as incongruous as to speak of a square circle. Yet is inertia considered as a real existence, and is said to be proportional to the quantity of matter in a body. When we find that we must employ twice as much force to move A with a certain velocity as to move B, we say that A contains twice as much matter, because we see that it has twice as much inertia. Is it not enough to say that we judge A to have twice as much matter, because all matter requires force to move it?—this is its characteristic. Should

we find that we can move a thing by a very wish, or a command, we should not think it matter. Inertia, taken in this sense, as expressing the necessity of what we call force, in order to change the motion of matter, is just one of those general phenomena by which it is known to us. Whether this force be, in every case, external to the material atom, or whether some of the observed powers of body may not be inherent in it, is a question of Metaphysics, and is probably beyond the reach of our faculties. But naturalists have generally supposed that the atom is purely passive and indifferent, and that all its powers are superadded to the mere material atom.

These doubts and difficulties in the study have all arisen from the introduction of the notion of *resistance*, or force exerted by matter, in order to remain as it is. It would have been infinitely better to have employed the term REACTION, because this is the expression of the very fact; for, in all the phenomena of changed motion, there is observed an equal change in opposite directions in the two acting bodies. Iron approaches to the magnet—the magnet to the iron. In the collision of bodies, the impelling and impelled are observed to sustain equal and opposite changes. But in most, and probably in all, we discover that those changes are brought about by forces familiarly known to us in other ways; and no method has been discovered, by which we may learn whether the *whole* of the change is owing to those mutual forces, or whether some part is to be ascribed to inertia.

143. When the body B is always observed to approach to A, and no intermediate cause can be assigned, A is said to attract B. Thus a magnet is said to attract a piece of common iron. But if B is always observed to shun A, or to separate from it, A is said to repel B. Thus one electrified body repels another.

144. Mechanical forces are considered as measurable magnitudes. But, since they are not objects of our perception, but only inferences from the phenomena, it is plain that we can neither measure nor compare their magnitudes directly. Having no knowledge of their agency, nor any mark of their kind, except the change of motion which we consider as their effect, it is only in this change of motion that we must look for any measure of their magnitude or intensity;—this is also the only mean of comparison. Now, change of motion, involving no ideas but of space and time, affords the most perfect measurement. We cannot find a better measure; nay, it is improper to employ any other; and the most eminent philosophers, by employing other measures, founded on their fancied knowledge of the intimate nature of mechanical force, have advanced most incongruous opinions, which have spoiled the beauty of the science. We shall therefore adhere strictly to the measure suggested by this reasoning, and shall call that a double or triple force, which, by its similar action, during the same time, produces a double or triple change of motion, whether it accelerates, or retards, or deflects a
 motion

motion already going on. We express this notion in the most simple manner by saying, that we consider force merely as something that is proportional to the change of velocity.

Of the Laws of Motion.

145. Such being our notions of motion, and of the causes of its production and changes, there are certain results, which, by the constitution of our minds, necessarily arise from the relations of these ideas. These are laws of human judgement, independent of all experience of external nature, just as it results from the laws of judgement that the three angles of a right lined triangle are equal to two right angles, although there should not be a triangle in the universe.

Some of these laws may be intuitive, and may be called axioms; others, equally necessary truths, may not be so obvious, and may require steps of argument.

There are three such laws, first proposed in precise terms by Sir Isaac Newton, which seem to give a sufficient foundation for all the doctrines of Dynamics, and to which, as to first principles, we may appeal for the explanation of every mechanical phenomenon of nature.

First Law of Motion.

146. *Every body continues at rest, or in uniform rectilinear motion, unless affected by some mechanical force.*

If we adhere to our inference of the agency of force only from an observed change of motion, and to this inference from every such change, and if we grant that we have no notion of a force independent of a change of motion, this law seems little more than a tautological proposition. For, unless we suppose the agency of a mechanical force, we do not suppose a change of motion, that is, the absence of mechanical agency is the absence of a change of motion, and the body continues in its former state of rest or motion. But philosophers have attempted to demonstrate this law in various ways.

147. Some consider it as a necessary truth, in the nature of the thing. A body, they say, can neither accelerate, nor retard, nor deflect, because the event is but one, and there is no cause of determination whether it shall accelerate, or retard, or deflect, nor whether to the right or to the left, or which determines any one degree of any of those changes. This sort of proof is obscure and unsatisfactory.

148. Others choose to consider it as a physical law, as an universal fact, for which, perhaps, we can give no reason. They offer numerous proofs by induction. Thus, a coach being suddenly accelerated, or checked in its progress, or turned out of its course, the fitters are thrown towards the back, or the front of the coach, or to one side, shewing, in all cases, a tendency to continue in their former condition in respect of motion. Number-
less

less examples may be given of the same marks of this tendency to continue in the former state.

149. But it may be objected, that it is very far from being a matter of universal experience. Whoever grants the truth of the Copernican description of the planetary motions, will also grant that we perhaps never saw one instance, either of rest or of uniform rectilinear motion. Our most familiar observations shew an evident tendency to rest, a sort of sluggishness in all matter. For it is a fact, that all motions gradually diminish, and, in a short time, terminate in rest. No force seems necessary for maintaining a state of rest. But motion, they say, is a violent state, the continual production of an effect, and therefore requiring a continuation of the cause. Motion therefore requires the continual exertion of the cause. They say that a body in motion continues in it, only by the continual agency of a force infused into it in giving it the motion, and inherent in it while in motion. They call it the *inherent force*—*vis insita corpori moto*.

150. But this is contrary to our clearest experience, and to any distinct notions that we can form of motion as an effect of force. We are not conscious of any exertion, in order to continue our motion in sliding or skating on smooth ice; and when any obstruction comes in our way, we feel *distinctly* our natural tendency to continue our speed undiminished—we feel that we must
resist

resist a tendency to fall forwards—we feel all obstructions as checks on our speed, and think that if the ice were *perfectly* smooth, we should go on for ever. It is equally contrary to our notions of a moving force. By its instantaneous action, it produces motion, that is, a successive change of place, otherwise it produces nothing. Or if, in any instant of its action, it do not produce a continuing motion, it cannot produce it by continuing to act. Continuation of motion is implied in our very idea of motion. In any instant, the body does not move over any space; but it is in a certain condition (however imperfectly understood by us) or has a certain determination, which we call velocity, by which, if not hindered, a certain length of path is passed over in a second. This must be effected by the instantaneous action of the moving cause, otherwise it is not a cause of motion. In short, motion is a *state or condition*, into which a body may be put, by various causes, but by no means a *thing* which can be infused into a body, or taken out of it.

Should it be said that we have full evidence of a force residing in a moving body, by observing its impulsive power, which is not to be found in the same body at rest, we may answer, that there *are* forces residing in moving bodies, but that they are equally inherent in them when at rest, but that motion is necessary, in order that these forces may be able to exert their action on the other body long enough to produce a sensible effect. Motion in the impelling body is not the *cause* of
that

that of the body impelled by it, but only an *occasion* or *opportunity* for the forces to act effectually, and without which the other body would withdraw itself from the action. The bow-string must continue pressing the arrow forwards—the hammer must follow the nail, that it may drive it to the head by one blow. This will be clearly shewn as we proceed.

The gradual diminution and final cessation of all motions mentioned above is granted, but is easily explained by stating the obstructions. The diminution is observed to be precisely what should arise from those obstructions, on the supposition that if there were no obstruction, there would be no diminution. For example, where we can shew that the obstruction is only half, the diminution of motion is only one half. This would not be, if there were any diminution where there is no obstruction. A pendulum is soon brought to rest when vibrating in water; it vibrates much longer in air; and still longer in the exhausted receiver of an air-pump. The planets have continued for many ages without the smallest perceptible diminution of their motions.

151. Another sect of philosophers deny this law altogether, and affirm that matter is essentially prone to motion. Every body, when at liberty, begins to move, and continually accelerates this spontaneous motion. Bodies are so far from being sluggish, that they are perpetually active.

152. All these differences of opinion may be completely settled, by adhering to the principle, that '*every change is an effect.*' It is a matter of fact, that the human mind always considers it as such. Therefore, the law is strictly deduced from our ideas of motion and its causes; for, even if it were essential to matter gradually to diminish its motion, and, at last, come to rest, this would not invalidate the law, because our understanding would consider this diminution as the indication of an essential, or, at least, a universal 'property' of matter. We should ascribe it to a natural retarding force, in the same way that we give this name to the weight of an arrow discharged straight upwards. The nature of existing matter would be considered as the cause, and we should estimate the law of its action as we have done in the case of gravity; and, as in that case, we should still suppose that were it not for this particular property, the material atom would continue its motion for ever undiminished.

This is quite sufficient for all the purposes of mechanical philosophy. Nay, if we assumed any thing else in this case, we should be led into continual blunders. Should we say that a body maintains its motion undiminished solely by the action of an inherent force, we should be obliged to adopt the opinion, that when one body in motion impels another, part of this force is transfused from the impelling into the impelled body, and all the absurdities which are necessarily attached to this opinion.

Therefore, to conclude on this subject, let us consider motion merely as a state or condition, into which
matter

matter may be brought by various causes, and which, like its whiteness or roundness, will remain, till some efficient cause shall change it. This we have called the *mechanical condition of the body*, and have settled the meaning of the term with sufficient precision. It consists in its velocity and direction, and in no other circumstance.

In the next place, let us consider the change which may be induced on it as consisting solely in a change in these circumstances, and this change as the only indication, the only mark, and the only proper measure of the changing cause, that is, of the force (for we are considering mechanical causes only). It is evident that, as far as this procedure will carry us, we acquire certain knowledge, susceptible of mathematical treatment. In order to make our task useful, we must endeavour to learn whether the deviations from uniform motion follow regular laws—what the laws are—and to what bodies they refer.

154. The deviations from uniform motion are discoverable only by a comparison with uniform motions. But we cannot tell whether a proposed motion be uniform, unless we have an accurate measure of time. For it is to be learned only by observing the proportions of the spaces, and those of the times, and by observing that those proportions are the same. To obtain a measure of time, various contrivances have been employed. They are all to this purpose—An event is selected, in which

we have no reason to think that any variation occurs in the operation of those causes which effectuate its accomplishment. It is then presumed that it will always be accomplished in equal times. The rotation of the heavens, in twenty-three hours and fifty-six minutes and four seconds, has been agreed on as the standard to which all other contrivances are referred or compared, and their accuracy is estimated by their agreement with this standard.

Second Law of Motion.

155. *Every change of motion is proportional to the force impressed, and is made in the direction of that force.*

This also is little more than a tautological proposition. If a force is to be measured only by the change which it makes in the motion of a body, the proposition is only a repetition of this measure in different terms; for, surely, quantities are proportional to their accurate measures. Indeed, this would have been a sufficient demonstration, had not philosophers attempted it in another way, which has given rise to a great schism in the estimation of forces. They have attempted to demonstrate it as an application of the undoubted maxim, that *effects are proportional to their causes*. But it is easy to see that this application *cannot* be made; for it presupposes that we know the proportion of the forces, and that of their causes, and that we perceive those proportions to be the same.—Now, in most cases, this is impossible; for the
forces

forces are not objects of our observation. We know nothing of their proportions. When Newton says that gravity at the surface of the earth is 3600 times greater than at the moon, he proves it by shewing that the deflection caused by it in a second, at the earth's surface, is 3600 times greater than that of the moon. But this is begging the question, or assuming this proposition as true, unless this law of motion be admitted as an axiom. There are very few cases indeed, where we can shew that forces are proportional to the changes of motion produced by them; yet such cases are not altogether wanting. Thus, a spring stilyard can be made, the rod of which is divided by hanging on, in succession, a number of perfectly equal weights. The elasticity of the spring, in its different states of tension, is proportional to the pressures of gravity which it balances.—Should we find that, at Quito in Peru, a lump of lead draws out the rod to the mark 312, and that, at Spitzbergen, it draws it to 313, we seem entitled to say that the pressure of gravity at Quito is to its pressure at Spitzbergen as 312 to 313, on the authority of effects being proportional to their causes.

But such cases are extremely rare, because it is seldom that a natural power, accurately measured in some other way, is *wholly* employed in producing the observed motion. Part of it is generally expended in some other way, and therefore we frequently see that the motions are not in the same proportion with the supposed forces. But even though this could be strictly done, this would

only be the proof of a general law or fact, whereas the pretensions of the philosophers aim at a proof of it *a priori*, of an abstract truth.

156. Sir Isaac Newton seems to consider it only as a physical law. In this sense, we are not without very good arguments.

I. A ball moving with a double, triple, or quadruple velocity, generates in another, by impulse, a double, or triple, or quadruple velocity, or the same velocity in a double, &c. quantity of matter, and the ball loses the same proportions of its own velocity.

II. Two bodies, meeting with equal quantities of motion, mutually stop each other.

III. Two forces, which, by acting similarly during equal times, would produce equal velocities in some third body, will, by acting together during the *same time*, produce a double velocity.

IV. If any pressure, acting for a second, produce a certain velocity, a double pressure, acting during a second, will produce a double velocity in the same body.

V. A force, which we know to act equably, produces equal increments of velocity in equal times, whatever these velocities may be.

In all these examples, we see the forces in the same proportion with the change of motion similarly produced by them.

157. But, about the middle of the 17th century, Dr Robert Hooke, Fellow of the Royal Society of London,

don, discovered a vast collection of facts, in which the forces seemed to be in a very different proportion.

1. In the production of motion. Four springs, equal in strength, and bent to the same degree, generated only a double velocity in the ball which they impelled; nine springs produced only a triple velocity, &c.

2. In the extinction of motion. A ball moving with a double velocity will penetrate four times as deep into a uniformly resisting mass; a triple velocity will make it penetrate nine times as far, &c.

These are but two instances of an immense collection of facts to the same purpose, and they are closely connected with the most important applications of dynamical science.

158. Mr Leibnitz eagerly availed himself of these facts, as authority for declaring himself the discoverer of the real nature and measure of mechanical action and force, which he said had hitherto been totally mistaken by philosophers; and he affirmed that the inherent force of a body in motion was in the proportion, not of the velocity, but of the square of the velocity. John Bernoulli, his zealous champion, warmly supported him in this argument, adducing a variety of the most simple facts, all confirming this relation between the *inherent force* of a body in motion and its velocity. They farther supported it by many metaphysical considerations, relating to the procedure of nature in generating this force and velocity, and the way in which it may be extinguished. The most cogent argument offered by Leibnitz

nitz is, that the force inherent in a moving body is to be estimated by all that it is able to do before its motion is completely extinguished. When, therefore, it penetrates four times as far, it should be considered as having produced a quadruple effect. The mechanicians of Europe were divided in their opinions; the Germans adhering to that of Leibnitz, and the British and French to that of Des Cartes, who first affirmed the relation which we have adopted as a second law of motion. We shall see presently, that, in the Leibnitzian measure, many things are gratuitously assumed, many contradictions are incurred, and, finally, that *it is only because forces are assumed as proportional to the velocities which they generate, that the facts observed by Hooke, and employed by Leibnitz, come to be proportional to the squares of the same velocities.* It shall only be noticed at present, that when Leibnitz assumes the quadruple penetration as the proof of the quadruple force of a body having twice the velocity, he does not consider that a double time is employed in this penetration. Now, a double force acting equably during a double time, should produce a quadruple effect. This circumstance is neglected in one and all of the facts adduced by Mr Leibnitz. It may be added, that his followers, as well as himself, agree with us in every consequence which we draw from the measure adopted by us. They grant that a force which produces a uniformly accelerated motion is a constant force, and they agree with the Cartesians in all the valuations of accelerating and deflecting forces, and have been among the most assidu-

ous and successful cultivators of the Newtonian philosophy, which proceeds entirely on the measure of moving forces by the velocity which they generate.

159. We must here observe that we are considering nothing but *moving forces*. When a ball has had a certain velocity given it, whether impelled by the air in a pop-gun, or by a spring, or struck off by a blow, or urged forward by a stream of wind or water, or has acquired it by falling, we conceive that in all these cases it has sustained the same action of moving force. Perhaps pressure is the only distinct notion we can form of force; but it is experience only that has informed us that pressure produces motion, but does not produce heat or sweetness. Production of motion is a circumstance in which all mechanical forces may agree, while they may differ in many others. By, or in, this circumstance of resemblance, they may be compared, and get a name expressing this comparison; namely, *moving force*. Therefore *this particular faculty* of pressure, elasticity, &c. may be measured by the change of motion which pressure produces. And whatever may be the proportions of pressure on the quiescent body, we may take it for granted that the pressure *actually exerted* in the production of motion may be measured by the magnitude of the change of motion. This is really the only change of mechanical condition effected by the pressure in the body moved by it; therefore it may be measured by the velocity. Accordingly, we find that when the same change of velocity

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city is produced by pressure on a soft clay ball, the same pressure has *really* been exerted, whether the velocity has been augmented from 99 to 100, or diminished from 4 to 3. For the same dimple will be observed in both cases. Nay, all our actions on the surface of this globe are proofs of this. A ball sustains the same dimple whether we impel it, at noon-day, to the westward or to the eastward, north or south, or though this should be done at midnight; yet the real velocities at noon and midnight differ by nearly twice the velocity of a cannon ball battering in breach. This could not be, if the changes of motion were not proportional to the exerted pressures.

160. The same conclusion may be deduced from our notions of a constant or invariable force. It is surely a force which produces equal effects, or changes of motion, in equal times. Now, equal augmentations of motion are surely equal augmentations of velocity. We find this notion of an invariable accelerating force confirmed by what we observe in the case of a falling body. This receives equal additions of velocity in equal times; and we have no reason to think that this force is variable. We should therefore infer, that whatever force it imparts in one second, it will impart four times as much in four seconds. So it does, if we allow a quadruple velocity to indicate a quadruple force; but in no other estimation of force.

To all this may be added, that although four springs, applied to an ounce ball, impel it only twice as fast as one

one spring will do, yet they will give the same velocity to a four ounce ball which one spring gives to an ounce ball. And we can demonstrate, to the satisfaction of Mr Leibnitz, that, in this last case, the four springs act during the same time with the single spring.

161. *Therefore, finally, a change of motion, in all its circumstances of velocity and direction, is the proper measure of a changing force.*

But it is also the proper measure of a moving force. For bodies in different states of motion may sustain one and the same change of motion. Now, suppose one of these bodies to be previously at rest, the change which it sustains is the same thing with the motion which it acquires. Therefore the force which produces any change of motion in a body already moving, is the same with the force which produces a motion equivalent to this change, in a body previously at rest, in which case it is, simply, a moving force.

It seemed necessary to be thus particular in the account of this contest about the measure of forces, because Mr Leibnitz's opinion has influenced the sentiments of many writers of reputation; and some of them, particularly Gravesande and Muschenbroek, have mixed it a good deal with their practical deductions. There could not have been any dispute, had not philosophers allowed themselves to consider force as something existing in body, whereas the term is never used to express any reality except the phenomenon which they conceived to be

its full effect and adequate measure. It is quite allowable to measure *ascensional*, or *penetrating force*, by the *ascension* and the *penetration*, and to remark that these are as the square of the velocity. But this must not be considered as the general, or the best, measure of force, and particularly of *moving force*. This *must* be measured by the simple change of motion which is produced by it. And this measure has the advantage of being equally applicable to the phenomena of ascension and penetration, as we shall see very soon. We may now enounce it in a different form, adapted to the characteristic and measure of a change of motion, which was shown in art. 79. to be the most proper.

Law of the Changes of Motion.

162. *In every change of a motion from AB (fig. 23.) to AD, the new motion AD is compounded of the former motion AB, and of the motion AC, which the changing force produces in a body at rest.*

For it was shewn in art. 79. that the change in any motion is that motion which, when compounded with the former motion, produces the new motion; and, in art. 81, that the new motion is that compounded of the former motion and the changing motion. Now, since the change of motion is the characteristic and the measure of the changing force (161.), determining both its direction and its intensity, or the velocity produced by it, the proposition follows of course.

163. It was remarked in art. 80, that the composition of motions, and the similar composition of forces, are two very different things. The first is a truth, purely mathematical, and as certain as any theorem in geometry. The second is a physical question entirely, depending on the nature of the mechanical forces which exist in the universe. We do not clearly see that two forces, each of which will separately produce motions having the directions and velocities expressed by the sides of a parallelogram, will, by their joint action, produce a motion in the diagonal. The demonstrations given of this proposition by almost all the writers of Elements are altogether inconclusive, being all similar to the case of a man walking on a field of ice, while the ice floats down a stream. This is only the composition of *motions*. Other writers, endeavouring to accommodate their reasonings to physical principles, have assumed postulates that appear gratuitous. The first legitimate demonstration was given by Dan. Bernoulli, in the *Comment. Petropol. Vol. I.* But it employs a series of many propositions, some of which are very abstruse. Mr D'Alembert greatly simplified and improved this demonstration, in a Memoire of the Acad. des Sciences 1769. But this also requires many propositions. Fontenex and Riccati, in vol. III. of the Memoires of the Academy of Turin, have given another very ingenious one. D'Alembert has also improved this demonstration, and has given another, in the same Memoires, and one in his *Dynamique*. The first is very refined and obscure, and the second does not seem

very conclusive. An attempt is made in the *Encyclop. Britan. Suppl.* § DYNAMICS, to combine Bernoulli's, D'Alembert's, and one by F. Frisi, which is more expeditious than either of the two first, and appears legitimate. The demonstration given in this place is undoubtedly complete, if the reasoning be complete that is employed in art. 79, to prove that the motion which, when compounded with the former motion, produces the new motion, is the true change of motion. We apprehend it to be so.

164. We have most abundant proof of this law of motion, if we consider it merely as a physical law, or universal fact.

1. Nothing is more familiar than the joint action of different forces. Thus, we frequently see a lighter dragged in different directions by two track-ropes, on different sides of the canal, and the lighter moves in an intermediate direction, in the same manner as if it were dragged by one rope in that direction.

In like manner, we may observe that if a ball, moving in a particular direction, receive a stroke athwart this direction, it takes a direction which lies between that of the primitive motion and that of the transverse stroke.

165. 2. If a point or particle of matter A (fig. 23.) be urged at once by two pressures, in the directions AB and AC, and if AB and AC are proportional to the intensities of those pressures, the joint action of these

two pressures is equivalent to the action of a third pressure, in the direction of the diagonal AD , having its intensity in the proportion of AD . This is completely proved, by observing that the point A will be withheld from moving, by a pressure AE , equal and opposite to AD . Now, we know that pressures are moving forces, and produce velocities (when acting similarly during equal times) proportional to their intensities. Therefore, the proposition is true with respect to pressures considered merely as pressures, and also with respect to the motions produceable by their composition,

166. 3. A ball suspended by a thread, and drawn aside from its quiescent position, is urged downwards by its weight, and is supported obliquely by the thread. We can say precisely what are the directions and intensities of the forces which incite it to motion in any position, and what velocities will result from them, upon the supposition of the truth of this proposition. And we can tell what number of oscillations it will make in a day. It is a fact, that, when every thing is executed with care, the number of vibrations will not differ from our computation by one unit in a hundred thousand.

4. Lastly, the planetary motions, computed on the same principles of the composition of forces, exhibit no sensible deviation from our calculations, after thousands of years.

There is nothing therefore that we can rest on with greater confidence, than the perfect agreement between
the

the composition of motions and the composition of the forces which would, separately, produce those motions, and are measured by the velocities which they generate.

It particularly deserves remark, that if we measure moving forces by the squares of the velocities which they generate, the composition is impossible; that is, two forces represented by the sides of a parallelogram made proportional to the squares of the velocities, will not compose a force which can be represented by the diagonal. Yet nature shews the exact composition of forces, on the supposition that they are as the velocities.

Therefore, finally, whether we consider this proposition as an abstract truth, or as a physical law, it may be considered as fully established. Its converse is the following.

167. *The force which changes the motion AB into AD, is that which would produce in a quiescent body the motion AC, which, when compounded with AB, produces the motion observed AD.*

168. *A force which will produce in a quiescent body a motion having the direction and velocity represented by AC, if applied to a body moving with the velocity and in the direction AB, will change its motion into the motion AD, the diagonal of the parallelogram ABDC. For the new motion must be that compounded of AB and AC (80.), that is, must be AD (83.)*

From

From these two propositions combined arises a third, which is the most general; viz.

169. *If a body A be urged at once by two forces, which would, separately, cause it to describe AB and AC, sides of a parallelogram ABCD, the body will, by their joint action, describe the diagonal AD in the same time.* For, had the body been already moving with the velocity and in the direction AB, and had it been acted on in A by the force AC, it would describe AD in the same time (168.). Now, it is immaterial at what time it got the determination by which it would describe AB. Let it therefore be at the instant that the force AC is applied to it. It must describe AD, because its mechanical condition in A, having the determination to the motion AB, is the same as in any other point of that line.

170. *Cor.* Two forces, acting on a body in the same, or in opposite directions, will cause it to move with a velocity equal to the sum, or to the difference, of the velocities which it would have received from the forces separately. For, if AC approach continually to AB, by diminishing the angle BAC, the points C and D will at last fall on *c* and *d*, and then AD is equal to the sum of AB and AC. But if the angle BAC increase continually, the points C and D will, at last, fall on *x* and *y*, and then A *y* becomes equal to the difference of AB and AC.

In the last case, it is evident that if AC be equal to AB, the point D or *y* will coincide with A, and there
will

will be no motion, the two forces being equal, and acting in opposite directions.

171. In such a case, the equal and opposite forces AC and AB are said to BALANCE each other, and, in general, those forces which, by their joint operation, produce no change of motion, are said, in like manner, to balance each other; and they are accounted equal and opposite, because each produces on the body a change of motion equal to what it would produce on a body at rest, and at the same time equal to the motion produced by the other force on a body at rest. These two motions are therefore equal and opposite, and therefore the forces are so.

172. We may now apply to the motions produced by the combined action of forces all that was demonstrated concerning the affections of compound motions, in the articles 83, 84, 85, 86, 87, 88, 89, & 90.

But, in making this transference, we must carefully attend to the essential difference between the composition of motions and the composition of forces. In this last, the composition is complete, as soon as the body has gotten the determination to move in the diagonal with the proper velocity, and after this there is no more composition. The body then moves uniformly, till some force change its condition. But, in the composition of two or more motions, the two constituent motions are supposed to continue, and *by their continuance only*, does the compound

pound motion exist. If any force can generate a finite velocity by its instantaneous action (which does not appear possible), two such forces generate the determination in the diagonal in an instant. But if the action must continue for some time, in order to generate the velocities AB or AC , the *joint* action must continue during the same time, in order to produce the velocity AD . Also, it is necessary that, during the whole time of their joint action, the moving powers of the two forces must retain the same proportion to each other, although they may perhaps vary in their intensity during that time. From not attending to this circumstance, many experiments, which have been made in order to compare this doctrine with the phenomena, have exhibited results which deviate greatly from it. The experiments made by the combination of pressures, such as weights pulling a body by means of threads, agree with this doctrine with the utmost precision, it being always found that two weights pulling in the directions AB , AC , and proportional to those lines, are exactly balanced by a third weight in the proportion of AD , and pulling in the direction AE . By these, the composition of *pressures* is most unexceptionably proved; and, seeing that we have scarcely any other clear conception of a moving force, these experiments may be considered as sufficient. But we need not stop here; for we have the most distinct proof, by experiment, that pressures produce motions in proportion to their intensities by their similar action during equal times. The planetary motions, in which the

directions and intensities of the compounded forces are accurately known as moving forces, complete the proof of the physical law, by their exquisite agreement with the calculations proceeding on the principles of this doctrine. This perfect agreement must be received as a full proof of the propriety of the measure of a moving force which we have assumed. Any other measure would give results widely different from the phenomena.

173. The force which singly produces the motion in the diagonal, may be said to be EQUIVALENT to the forces which produce the motions in the sides of the parallelogram. It may also be called the COMPOUND FORCE, and the RESULTING FORCE; and the forces which act in the direction of the sides, may be called the SIMPLE FORCES, or the CONSTITUENT FORCES.

174. *The two constituent forces and their resulting force act in one plane; and they are proportional to the three sides of a triangle having their directions, or of any similar triangle (84).*

175. *Each force is proportional to the sine of the angle contained by the directions of the other two. For the sides of any triangle are as the sines of the opposite angles.*

176. A force acting in the direction parallel to any line BD does not affect the approach toward that line, or its recess from it, occasioned by the action of another
force

force A C. For, because the motion A D is uniform, the points δ and ϵ , to which the body would have gone by the force A B, are at the same distance from B D with the points d and e to which it really goes in the same time, by the joint action of the forces A B and A C.

177. A body under the influence of any number of forces A B, A C, A D, A E, (fig. 12.) will describe the line A F, determined as in article 86. ; and A F will express the equivalent or resulting force, both in respect of direction and intensity.

178. Any force A B may be conceived as resulting from the joint action of two or more forces having any directions whatever, and their intensities may be compared as in art. 85.

179. Forces may be *estimated* in the direction of a given line or plane, or may be *reduced* to that direction, as in art. 35.

180. Any number of forces, acting on a particle of matter, will be balanced by a force equal and opposite to their resulting or equivalent force.

181. If any number of forces are in equilibrio, and are estimated in, or reduced to, any one direction, or in one plane, the reduced forces are in equilibrio.

To these two laws of motion, which we have attempted to shew to be necessary consequences of the relations of those conceptions which we form of motion and of mechanical force, and also to be universal facts or physical laws, Sir Isaac Newton has added another, or

Third Law of Motion.

182. *The actions of bodies on one another are always mutual, equal, and in contrary directions. It is usually expressed thus—Reaction is always equal and contrary to action.*

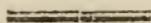
This is indeed a fact, observed without exception, in all the cases which we can examine with accuracy. Sir Isaac Newton, in the general scholium or remark on the laws of motion, seems to consider this equality of action and reaction as an axiom deduced from the relations of ideas. But this seems doubtful. Because a magnet causes the iron to approach towards it, it does not appear that we necessarily suppose that iron also attracts the magnet. The fact is, that although many observations are to be found in the writings of the ancients concerning the attractive power of the magnet, not one of them has mentioned the attractive power of the iron. It is a modern discovery, and Dr Gilbert is, I think, the earliest writer, in whose works we meet with it. He affirms that this *mutual* attraction is observed between the magnet and iron, and between all electrical substances and the light bodies attracted by them. Kepler
noticed

noticed this mutual influence between the Earth and the Moon. Wallis, Wren, and Huyghens, first distinctly affirmed the mutual, equal, and contrary action of solid bodies in their collisions; and it has been confirmed by innumerable observations. Nay, since that time, Sir Isaac Newton himself only *presumed* that, because the Sun attracted the planets, these also attracted the Sun; and he is at much pains to point out phenomena to astronomers, by which this may be proved, when the art of observation shall be sufficiently improved. These must be put on the same footing with the phenomena by which the mutual actions of the planets are proved. Now, this last action was *altogether* a presumption, although the proof was by far the most easy. The discovery and complete demonstration of this, as a physical law, is certainly the most illustrious specimen of Newton's genius and nice judgment.

We must receive it therefore as a law of motion, with respect to all bodies on which we can make experiment, or observation fit for deciding the question.

183. As it is an universal law, we cannot rid ourselves of the persuasion that it depends on some general principle, which influences all the matter in the universe. It powerfully induces us to believe that the ultimate atoms of matter are all perfectly alike—that a certain collection of properties belong, in the same degree, to every atom—and that all the sensible differences of substance which we observe arise from a different combination of
primary

primary atoms in the formation of a particle of those substances. A very slight consideration may shew us that this is perfectly possible. Now, if such be the constitution of every primary atom, there can be no action of any kind of particle, or collection of particles, on another, which will not be accompanied by an equal reaction in the opposite direction. Nothing can be clearer than this. This therefore is, in all probability, the origin of this Third Law of Motion.



184. The aim of the Newtonian philosophy, which we profess to follow, is to investigate the laws observed in the production of natural effects, and to comprehend any proposed phenomenon in one or other of those laws. We then account it as explained.

These general, but still subordinate, laws are to be established only by observation and experiment; but when so established as far as observation extends, it is only by means of some observed analogy that we can use them as explanations of many other phenomena. With this we must rest satisfied, because it seems impossible for our faculties to discover the efficient causes of those general laws, so as to be able to demonstrate that ^{they} *must* be such as we observe. But in the establishment of them as mere matters of fact, we may observe them to be of various extent, and that some are subdivisions of others. In this subordination, we can discern

cern much order, harmony and beauty, and our minds are left deeply impressed with admiration of the wisdom and skill of the contrivance, by which this magnificent fabric is fitted for the accomplishment of a great and beneficent purpose.

185. The three axioms, and, indeed, the two first, seem to include the whole first principles of Dynamics, and enable us, without other help, to accomplish every purpose of the science. Some authors of eminence have thought that there were other principles, which influenced every natural operation, and that these operations could not be fully understood, nor an explanation properly deduced, without employing those principles. Of this kind is the principle of OECONOMY OF ACTION, or SMALLEST ACTION, affirmed by Mr Maupertuis to be pursued in all the operations of nature. This philosopher says, that the perfect wisdom of Deity must cause him to accomplish every change by the smallest possible expenditure of power of every kind; and he gives a theorem which he says expresses this œconomy in all cases of mechanical action. He then asserts that, in order to shew in what manner such and such bodies, so and so situated, shall change each other's condition, we must find what change in each will agree with this value of the smallest action. He applies this to the solution of many problems, some of which are intricate, and gives solutions perfectly agreeable to the phenomena.

But the fact is, that the theorem was suggested by the phenomena, and is only an induction of particulars.

It is a law, of a certain extent, but by no means a first principle; for the law is comprehended in, and is subordinate, by many degrees, to the three laws of motion now established. It is no just expression of a minimum of action; and he has obtained solutions, by its means, of problems, in which its elements are altogether supposititious, which is proof sufficient of its nullity and impropriety.

186. Mr D'Alembert and Mr De la Grange have also given general theorems, which they call first principles, and which they think highly necessary in dynamical disquisitions. These, too, are nothing but general, but very subordinate laws, most ingeniously employed by their authors in the solution of intricate problems, where they are really of immense service. But still they are not principles; and a person may understand the *mechanique analytique* of De la Grange, by studying it with care, and yet be very ignorant of the real natural principles of mechanism. All these theorems are only ingenious combinations of the second and third Newtonian Laws of Motion.

187. The application or employment of these laws is to a twofold purpose.

1. To discover those mechanical powers of natural substances which fit them for being parts of a permanent universe. We accomplish this by observing what changes of motion among the neighbouring bodies always accom-
pany

pany those substances, wherever they are. These changes are the only characteristics of the powers. It is thus that we discover and describe the power of magnetism, gravity, &c.

2. Having obtained the mechanical character of any substance, we ascertain what will be the result of its being in the vicinity of the bodies mechanically allied to it, or we ascertain what change will be induced on the condition of the neighbouring bodies.

To save us a great labour, which must be repeated for every question, if we make *immediate* application of the laws of motion to the phenomenon, it will be extremely convenient to have in readiness a few general rules, accommodated to the more frequent cases of natural operations. The mechanical powers of bodies occasionally accelerate, retard, and deflect the motions of other bodies. Therefore it is proper to premise the principal theorems relating to the action of accelerating, retarding, or deflecting forces. They have got these names, because we know nothing of their nature, or of the manner in which they are effective, and therefore name them, as we measure them, by the phenomena which we consider as their effects.

Of Accelerating and Retarding Forces.

188. Since we have adopted the changes of motion as the marks and measures of the forces, it is evident that every thing already said of accelerations and retarda-

tions is equally descriptive of the effects of accelerating and retarding forces. Therefore,

If the abscissa a d (fig. 5.) represent the time of any motion, and if the areas a b f e, a c g e, &c. are as the velocities at the instants b, c, &c. the ordinates a e, b f, c g, &c. are as the accelerating forces at those instants (69).

189. *Cor. 1.* The momentary change of velocity is as the force f and the time t jointly, which may be thus expressed (71.)

$$\dot{v}, \text{ or } -\dot{v}, \doteq f t.$$

Also, the accelerating or retarding force is proportional to the momentary variation of the velocity, directly, and to the moment of time in which it is generated, inversely (71.)

$$f \doteq \frac{\dot{v}}{t}, \text{ or } \doteq -\frac{\dot{v}}{t}.$$

Indeed, all that we know of force is that it is something which is always proportional to $\frac{\dot{v}}{t}$.

190. *Cor. 2.* *Uniformly accelerated or retarded motion is the indication of a constant or invariable accelerating force.* For, in this case, the areas $a b f e$, $a c g e$, &c. increase at the same rate with the times $a b$, $a c$, &c. and therefore the ordinates $a e$, $b f$, $c g$, &c. must all be equal; therefore the forces represented by them are the same, or the accelerating force does not change its intensity, or, it is constant. If, therefore, the circumstances

stances mentioned in articles 54, 55, 56, 57, 58, 59, 60, 61, are observed in any motion, the force is constant. And if the force is known to be constant, those propositions are true respecting the motions.

191. *Cor. 3.* No finite change of velocity is generated in an instant by any accelerating or retarding force. For the increment or decrement of velocity is always expressed by an area, or by a product ft , one side or factor of which is a portion of time. As no finite space can be described in an instant, and the moveable must pass in succession through every point of the path, so it must acquire all the intermediate degrees of velocity. It must be continually accelerated or retarded.

192. *Cor. 4.* The change of velocity produced in a body in any time, by a force varying in any manner, is the proper measure of the accumulated or whole action of the force during this time. For, since the momentary change of velocity is expressed by $f\dot{t}$, the aggregate of all these momentary changes, that is, the whole change of velocity, must be expressed by the sum of all the quantities $f\dot{t}$. This is equivalent to the area of the figure employed in art. 188, and may be expressed by $\int f\dot{t}$.

193. If the abscissa A E (fig. 8.) of the line a c e be the path along which a body is urged by the action of a force,

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varying in any manner, and if the ordinates $A a$, $B b$, $C c$, &c. be proportional to the intensities of the force in the different points of the path, the intercepted areas will be proportional to the changes made on the square of the velocity during the motion along the corresponding portions of the path.

For, by art. 72, the areas are in this proportion when the ordinates are as the accelerations. But the accelerations are the measures of, and are therefore proportional to, the accelerating forces. Therefore the proposition is manifest.

194. *Cor. 1.* The momentary change on the square of the velocity is as the force, and as the small portion of space along which it acts, jointly;

$$v \dot{v} \doteq f \dot{s}$$

and

$$f \doteq \frac{v \dot{v}}{\dot{s}}.$$

195. It deserves remark here, that as the momentary change of the simple velocity by any force f depends only on the time of its action, it being $= f t$ (189.), so the change on the square of the velocity depends on the space, it being $= f s$. It is the same, whatever is the velocity thus changed, or even though the body be at rest when the force begins to act on it. Thus, in every second of the falling of a heavy body, the velocity is augmented 32 feet per second, and in every foot of the fall, the square of the velocity increases by 64.

196. The whole area $A E e a$, expressed by $\int f s$, expresses the whole change made on the square of the velocity which the body had in A , whatever this velocity may have been. We may therefore suppose the body to have been at rest in A . The area then measures the square of the velocity which the body has acquired in the point E of its path. It is plain that the change on v^2 is quite independent on the time of action, and therefore a body, in passing through the space $A E$ with any initial velocity whatever, sustains the same change of the square of that velocity, if under the influence of the same force.

197. This proposition is the same with the 39th of the First Book of Newton's Principia, and is perhaps the most generally useful of all the theorems in Dynamics, in the solution of practical questions. It is to be found, without demonstration, in his earliest writings, the Optical Lectures, which he delivered in 1669 and following years.

198. One important use may be made of it at present. It gives a complete solution of all the facts which were observed by Dr Hooke, and adduced by Leibnitz with such pertinacity in support of his measure of the force of moving bodies. All of them are of precisely the same nature with the one mentioned in art. 157, or with the fact, "that a ball projected directly upwards
" with a double velocity, will rise to a quadruple height,
" and

“ and that a body, moving twice as fast, will penetrate
 “ four times as far into a uniformly tenacious mass.”
 The uniform force of gravity, or the uniform tenacity
 of the penetrated body, makes a uniform opposition to
 the motion, and may therefore be considered as a uni-
 form retarding force. It will therefore be represented,
 in fig. 8, by an ordinate always of the same length, and
 the areas which measure the square of the velocity lost
 will be portions of a rectangle $A E \varepsilon a$. If therefore $A E$
 be the penetration necessary for extinguishing the velo-
 city 2, the space $A B$, necessary for extinguishing the
 velocity 1, must be $\frac{1}{4}$ of $A E$, because the square of 1
 is $\frac{1}{4}$ of the square of 2.

199. What particularly deserves remark here, is,
 that this proposition is true, *only on the supposition that
 forces are proportional to the velocities generated by them in
 equal times*. For the demonstration of this proposition
 proceeds entirely on the previously established measure
 of acceleration. We had $\dot{v} \doteq f \dot{t}$; therefore $v \dot{v} \doteq f \dot{t} v$.
 But $\dot{t} v \doteq \dot{s}$; therefore $v \dot{v} \doteq f \dot{s}$, which is precisely
 this proposition.

200. Those may be called *similar* points of space,
 and *similar* instants of time, which divide given portions
 of space or time in the same ratio. Thus, the beginning
 of the 5th inch, and of the 2d foot, are similar points
 of a foot, and of a yard. The beginning of the 21st
 minute,

minute, and of the 9th hour, are similar instants of an hour, and of a day.

Forces may be said to act *similarly* when, in similar instants of time, or similar points of the path, their intensities are in a constant ratio.

201. *Lemma.* If two bodies be similarly accelerated during given times ac and bk (fig. 24.), they are also similarly accelerated along their respective paths AC and HK .

Let a, b, c be instants of the time ac , similar to the instants b, i, k of the time bk . Then, by the similar accelerations, we have the force $ae : bl = bf : im$. This being the case throughout, the area af is to the area bm as the area ag to the area bn (Symbols (t)). These areas are as the velocities in the two motions (71.) Therefore the velocities in similar instants are in a constant ratio, that is, the velocity in the instant b is to that in the instant i , as the velocity in the instant c to that in the instant k .

The figures may now be taken to represent the times of the motion by their abscissæ, and the velocities by their ordinates, as in art. 45. The spaces described are now represented by the areas. These being in a constant ratio, as already shewn, we have A, B, C , and H, I, K , similar points of the paths. And therefore, in similar instants of time, the bodies are in similar points of the paths. But in these instants, they are similarly accelerated, that is, the accelerations and the forces are
in

in a constant ratio. They are therefore in a constant ratio in similar points of the paths, and the bodies are similarly accelerated along their respective paths (200.).

202. *If two particles of matter are similarly urged by accelerating or retarding forces during given times, the whole changes of velocity are as the forces and times jointly; or $v \doteq f t$.*

For the abscissæ ac and bk will represent the times, and the ordinates ae and bl will represent the forces, and then the areas will represent the changes of velocity, by art. 70. And these areas are as $ac \times ae$ to $bk \times bl$, (by Symbols (*s. Cor.*))

$$\text{Hence } t \doteq \frac{v}{f}, \text{ and } f \doteq \frac{v}{t}.$$

203. *If two particles of matter are similarly impelled or opposed through given spaces, the changes in the squares of velocity are as the forces and spaces jointly; or $\doteq f s$.*

This follows, by similar reasoning, from art. 72.

It is evident that this proposition applies directly to the argument so confidently urged for the propriety of the Leibnitzian measure of forces, namely, that four springs of equal strength, and bent to the same degree, generate, or extinguish, only a double velocity.

204. *If two particles of matter are similarly impelled through given spaces, the spaces are as the forces and the squares of the times jointly.*

For

For the moveables are similarly urged during the times of their motion (converse of 201.) Therefore $v \doteq ft$, and $v^2 \doteq f^2 t^2$; but (203.) $v^2 \doteq fs$. Therefore $fs \doteq f^2 t^2$, and $s \doteq ft^2$.

Cor. $t^2 \doteq \frac{s}{f}$ and $f \doteq \frac{s}{t^2}$. That is, the squares of the times are as the spaces, directly, and as the forces, inverfely; and the forces are as the spaces, directly, and as the squares of the times, inverfely.

205. The quantity of motion in a body is the fum of the motions of all its particles. Therefore, if all are moving in one direction, and with one velocity v , and if m be the number of particles, or quantity of matter, mv will exprefs the quantity of motion q , or $q \doteq mv$.

206. In like manner, we may conceive the accelerating forces f , which have produced this velocity v in each particle, as added into one fum, or as combined on one particle, by article 170. They will thus compose a force, which, for distinction's fake, it is convenient to mark by a particular name. We fhall call it the MOTIVE FORCE, and exprefs it by the symbol p . It will then be confidered as the aggregate of the number m of equal accelerating forces f , each of which produces the velocity v on one particle. It will produce the velocity mv , and the fame quantity of motion q .

207. Let there be another body, confifting of n particles, moving with one velocity u . Let the moving

S

force

force be represented by π . It is measured in like manner by nu . Therefore we have, $p : \pi = m v : n u$, and $v : u = \frac{p}{m} : \frac{\pi}{n}$; that is,

The velocities which may be produced by the similar action of different motive forces, in the same time, are directly as those forces, and inversely as the quantities of matter to which they are applied.

$$\text{In general,} \quad \dot{v} \doteq \frac{\dot{p}}{m}.$$

$$\text{And } f \text{ being } = \frac{\dot{v}}{t}, \quad f \doteq \frac{\dot{p}}{m t}.$$

REMARK.

208. In the application of the theorems concerning accelerating or retarding forces, it is necessary to attend carefully to the distinction between an accelerative and a motive force. The caution necessary here has been generally overlooked by the writers of Elements, and this has given occasion to very inadequate and erroneous notions of the action of accelerating powers. Thus, if a leaden ball hangs by a thread, which passes over a pulley, and is attached to an equal ball, moveable along a horizontal plane, without *the smallest* obstruction, it is known that, in one second, it will descend 8 feet, dragging the other 8 feet along the plane, with a uniformly accelerated motion, and will generate in it the velocity 16 feet per second. Let the thread be attached to three such balls. We know that it will descend 4 feet in a second, and
generate

generate the velocity 8 feet per second. Most readers are disposed to think that it should generate no greater velocity than $5\frac{1}{3}$ feet per second, or $\frac{1}{3}$ of 16, because it is applied to three times as much matter (207.) The error lies in considering the motive force as the same in both cases, and in not attending to the quantity of matter to which it is applied. Neither of these conjectures is right. The motive force changes as the motion accelerates, and in the first case, it moves two balls, and in the second it moves four. The motive force decreases similarly in both motions. When these things are considered, we learn by articles 202 and 207, that the motions will be precisely what we observe.

Of Deflecting Forces, in general.

209. It was observed, in art. 99, that a curvilinear motion is a case of *continual* deflection. Therefore, when such motions are observed, we know that the body is under the *continual* influence of some natural force, acting in a direction which crosses that of the motion in every point. We must infer the magnitude and direction of this deflecting force by the magnitude and direction of the observed deflection. Therefore, all that is affirmed concerning deflections in the 99th and subsequent articles of the Introduction, may be affirmed concerning deflecting forces. It follows, from what has been established concerning the action of accelerating forces, that no force can produce a finite change of velocity in an instant.

Now, a deflection is a composition of a motion already existing with a motion accelerated from rest by insensible degrees. Supposing the deflecting force of invariable direction and intensity, the deflection is the composition of a motion having a finite velocity with a motion uniformly accelerated from rest. Therefore the linear deflection from the rectilinear motion must increase by insensible degrees. The curvilinear path, therefore, must have the line of undeflected motion for its tangent. To suppose any finite angle contained between them would be to suppose a polygonal motion, and a subfultory deflection.

Therefore *no finite change of direction can be produced by a deflecting force in an instant.*

210. The most general and useful proposition on this subject is the following, founded on art. 104.

The forces by which bodies are deflected from the tangents in the different points of their curvilinear paths are proportional to the squares of the velocities in those points, directly, and inversely to the deflective chords of the equicurve circles in the same points. We may still express the proposition by the same symbol

$$f \doteq \frac{v^2}{c},$$

where f means the intensity of the deflecting force.

211. We may also retain the meaning of the proposition expressed in article 105, where it is shewn that the actual

tual linear deflection from the tangent is the third proportional to the deflective chord and the arch described in a very small moment. For it was demonstrated in that article (see fig. 18.) that $BZ : BC = BC : BO$.

We see also that Bb , the double of BO , is the measure of the velocity, generated by the uniform action of the deflecting force, during the motion in the arch BC of the curve.

212. The art. 106. also furnishes a proposition of frequent and important use, viz.

The velocity in any point of a curvilinear motion is that which the deflecting force in that point would generate in the body by uniformly impelling it along the fourth part of the deflective chord of the equicurve circle.

REMARK.

213. The propositions now given proceed on the supposition that, when the points A and C of fig. 18, after continually approaching to B , at last coalesce with it, the last circle which is described through these three points has the same curvature which the path has in B . It is proper to render this mode of solving these questions more plain and palpable.

If $ABCD$ (fig. 25.) be a material curve or mould, and a thread be made fast to it at D , this thread may be lapped on the convexity of this curve, till its extremity meets it in A . Let the thread be now unlapped or **EVOLVED** from the curve, keeping it always tight. It is plain

plain that its extremity A will describe another curve line $A b c$. All curves, in which the curvature is neither infinitely great nor infinitely small, may be thus described by a thread evolved from a proper curve. The properties of the curve $A b c$ being known, Mr Huyghens (the author of this way of generating curve lines) has shewn how to construct the evolved curve $A B C$ which will produce it.

From this genesis of curves we may infer, *1st*, that the detached portion of the thread is always a tangent to the curve $A B C$; *2dly*, that when this is in any situation $B b$, it is perpendicular to the tangent of the curve $A b c$ in the point b , and that it is, at the same time, describing an element of that curve, and an element of a circle $\alpha b \varkappa$, whose momentary centre is B , and which has $B b$ for its radius. *3dly*, That the part $b A$ of the curve, being described with radii growing continually shorter, is *more* incurvated than the circle $b \alpha$, which has $B b$ for its constant radius. For similar reasons the arch $b c$ of the curve $A b c$ is *less* incurvated than the circle $\alpha b \varkappa$. *4thly*, That the circle $\alpha b \varkappa$ has the same curvature that the curve has in b , or is an equicurve circle. $B b$ is the radius, and B the centre of curvature in the point b .

$A B C$ is the CURVA EVOLUTA or the EVOLUTE. $A b c$ is sometimes called the INVOLUTE of $A B C$, and sometimes its EVOLUTRIX.

214. By this way of describing curve lines, we see clearly that a body, when passing through the point b of the

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the curve $A b c$ may be considered as in the same state, in that instant, as in passing through the same point b of the circle $a b x$; and the ultimate ratio of the deflections in both is that of equality, and they may be used indiscriminately.

The chief difficulty in the application of the preceding theorems to the curvilinear motions which are observed in the spontaneous phenomena of nature, is in ascertaining the direction of the deflection in every point of a curvilinear motion. Fortunately, however, the most important cases, namely those motions, where the deflecting forces are always directed to a fixed point, afford a very accurate method. Such forces are called by the general name of

Central Forces.

215. *If bodies describe circles with a uniform motion, the deflecting forces are always directed to the centres of the circles, and are proportional to the square of the velocities, directly, and to their distances from the centre, inversely.*

For, since their motion in the circumference is uniform, the areas formed by lines drawn from the centre are as the times, and therefore (100) the deflections, and the deflecting forces (209) are directed to the centre. Therefore, the deflective chord is, in this case, the diameter of the circle, or twice the distance of the body from the centre. Therefore, if we call the distance from the centre d , we have $f \doteq \frac{v^2}{d}$.

216. *These forces are also as the distances, directly, and as the square of the time of a revolution, inversely.*

For the time of a revolution (which may be called the PERIODIC TIME) is as the circumference, and therefore as the distance, directly, and as the velocity, inversely. Therefore $t \doteq \frac{d}{v}$, and $v \doteq \frac{d}{t}$, and $v^2 \doteq \frac{d^2}{t^2}$, and $\frac{v^2}{d} \doteq \frac{d}{t^2}$.

217. *These forces are also as the distances, and the square of the angular velocity, jointly.*

For, in every uniform circular motion, the angular velocity is inversely as the periodic time. Therefore, calling the angular velocity a , $a^2 \doteq \frac{1}{t^2}$, and $\frac{d}{t^2} \doteq d a^2$, and therefore $f \doteq d a^2$.

218. *The periodic time is to the time of falling along half the radius by the uniform action of the centripetal force in the circumference, as the circumference of a circle is to the radius.*

For, in the time of falling through half the radius, the body would describe an arch equal to the radius (59), because the velocity acquired by this fall is equal to the velocity in the circumference (212.) The periodic time is to the time of describing that arch as the circumference to the arch, that is, as the circumference is to the radius.

219. *When a body describes a curve which is all in one plane, and a point is so situated in that plane, that a line drawn*



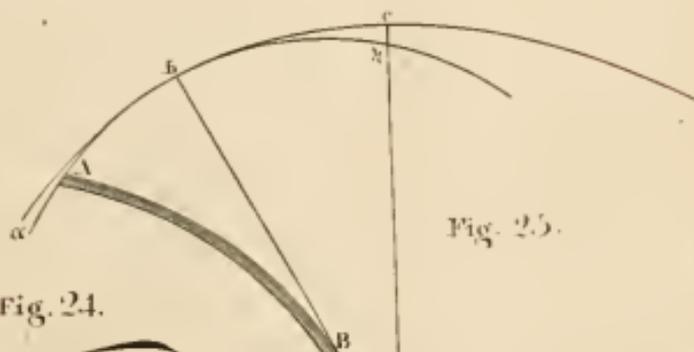
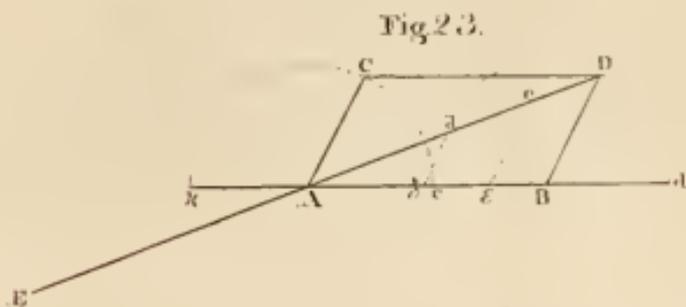
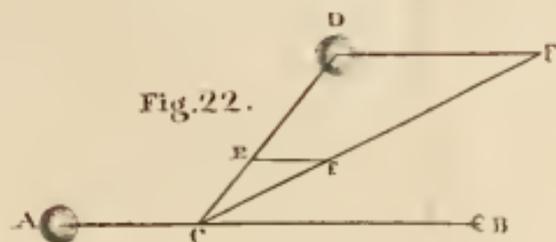
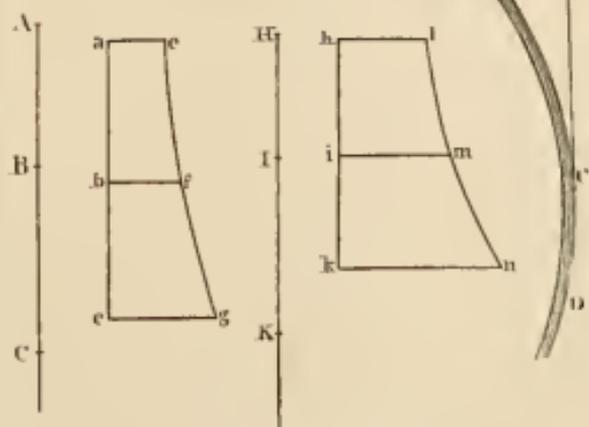


Fig. 25.

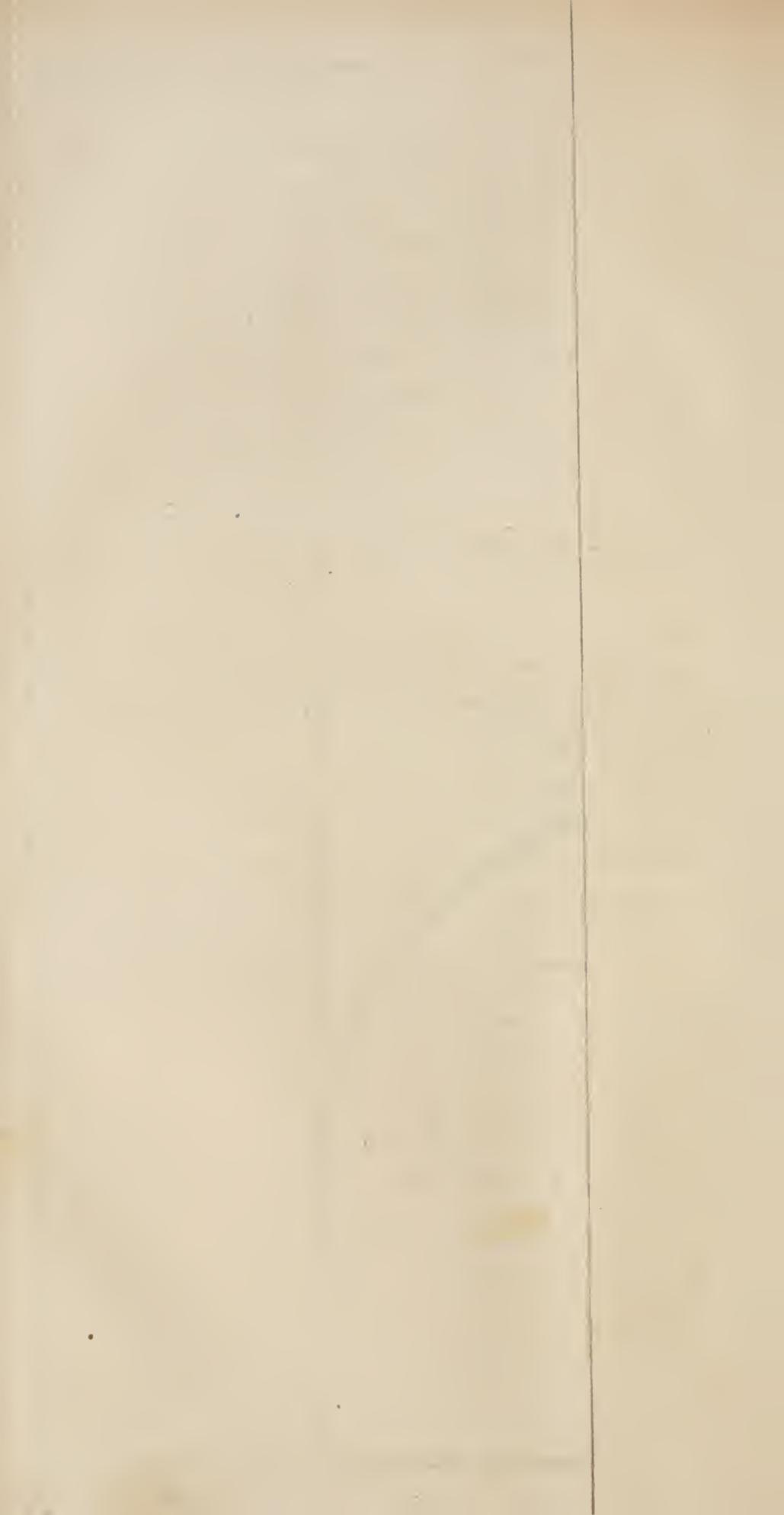


drawn from it to the body describes round that point areas proportional to the times, the deflecting force is always directed to that point (100.)

220. Conversely. If a body is deflected by a force always directed to a fixed point, it will describe a curve line lying in one plane which passes through that point, and the line joining it with the centre of forces will describe areas proportional to the times (101.)

The line joining the body with the centre is called the RADIUS VECTOR. The deflecting force is called CENTRIPETAL, or ATTRACTIVE, if its direction be always toward that centre. It is called REPULSIVE, or CENTRIFUGAL, if it be directed outwards from the centre. In the first case, the curve will have its concavity toward the centre, but, in the second case, it will be convex toward the centre. The force which urges a piece of iron towards a magnet is centripetal, and that which causes two electrical bodies to separate is centrifugal.

221. The force by which a body may be made to describe circles round the centre of forces, with the angular velocities which it has in the different points of its curvilinear path, are inversely as the cubes of its distances from the centre of forces. For the centripetal force in circular motions is proportional to $d a^2$ (217.) But when the deflections (and consequently the forces) are directed to a centre, we have $a \div \frac{1}{d^2}$ (103.) and $a^2 \div \frac{1}{d^4}$, therefore $d a^2 \div d \times \frac{1}{d^4}$, $\div \frac{1}{d^3}$, therefore $f \div \frac{1}{d^3}$.



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This force is often called centrifugal, *the centrifugal force of circular motion*, and it is conceived as always acting in every case of curvilinear motion, and to act in opposition to the centripetal force which produces that motion. But this is inaccurate. We suppose this force, merely because we must employ a centripetal force, just as we suppose a *resisting vis inertię*, because we must employ force to move a body.

222. *If a body describe a curve line ABC by means of a centripetal (fig. 26.) force directed to S, and varying according to some proportion of the distances from it, and if another body be impelled toward S in the straight line a b S by the same force, and if the two bodies have the same velocity in any points A and a which are equidistant from S, they will have equal velocities in any other two points C and c, which are also equidistant from S.*

Describe round S, with the distance SA, the circular arch Aa, which will pass through the equidistant point a. Describe another arch Bb, cutting off a small arc AB of the curve, and also cutting AS in D. Draw DE perpendicular to the curve.

The distances AS and aS being equal, the centripetal forces are also equal, and may be represented by the equal lines AD and ab. The velocities at A and a being equal, the times of describing AB and ab will be as the spaces (31). The force ab is wholly employed in accelerating the rectilinear motion along aS. But the force AD, being transverse or oblique to the motion
along

along AB , is not wholly employed in thus accelerating the motion. It is equivalent (173) to the two forces AE and ED , of which ED , being perpendicular to AB , neither promotes nor opposes it, but incurvates the motion. The accelerating force in A therefore is AE . It was shewn, in art. 71, that the change of velocity is as the force and as the time jointly, and therefore it is as $AE \times AB$. For the same reason, the change of the velocity at a is as $ab \times ab$, or ab^2 . But, as the angle ADB is a right angle, as also AED , we have $AE : AD = AD : AB$, and $AE \times AB = AD^2 = ab^2$. Therefore, the increments of velocity acquired along AB and ab are equal. But the velocities at A and a were equal. Therefore the velocities at B and b are also equal. The same thing may be said of every subsequent increase of velocity, while moving along BC and bc ; and therefore the velocities at C and c are equal.

The same thing holds, when the deflecting force is directed in lines parallel to aS , as if to a point S' infinitely distant, the one body describing the curve line $VA'B'$, while the other describes the straight line VS .

223. The propositions in art 102. and 103. are also true in curvilinear motions by means of central forces.

When the path of the motion is a line returning into itself, like a circle or oval, it is called an ORBIT; otherwise it is called a TRAJECTORY.

The time of a complete revolution round an orbit is called the PERIODIC TIME.

224. The formula $f \doteq \frac{v^3}{c}$ serves for discovering the law of variation of the central force by which a body describes the different portions of its curvilinear path; and the formula $f \doteq \frac{d}{r^2}$ serves for comparing the forces by which different bodies describe their respective orbits.

225. It must always be remembered, in conformity to art. 105, that $f = \frac{v^2}{c}$ or $f = \frac{\text{arc}^2}{c}$ expresses the linear deflection from the tangent, which may be taken for a measure of the deflecting force, and that $f = \frac{2v^2}{c}$ or $f = \frac{2 \text{arc}^2}{c}$ expresses the velocity generated by this force, during the description of the arc, or the velocity which may be compared directly with the velocity of the motion in the arc. The last is the most accurate, because the velocity generated is the real change of condition.

226. *A body may describe, by the action of a centripetal force, the direction of which passes through C (fig. 27.) a figure VPS, which figure revolves (in its own plane) round the centre of forces C, in the same manner as it describes the quiescent figure, provided that the angular motion of the body in the orbit be to that of the orbit itself in any constant ratio, such as that of m to n.*

For, if the direction of the orbit's motion be the same with that of the body moving in it, the angular motion
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of the body in every point of its motion is increased in the ratio of m to $n + m$, and it will be in the same ratio in the different parts of the orbit as before, that is, it will be inversely as the square of the distance from S (103). Moreover, as the distances from the centre in the simultaneous positions of the body, in the quiescent and in the revolving orbit, are the same, the momentary increments of the area are as the momentary increments of the angle at the centre; and therefore, in both motions, the areas increase in the constant ratio of m to $m + n$ (103). Therefore the areas of the absolute path, produced by the composition of the two motions, will still be proportional to the times; and therefore (101) the deflecting force must be directed to the centre S ; or, a force so directed will produce this compound motion.

227. *The differences between the forces by which a body may be made to move in the quiescent and in the moveable orbit are in the inverse triplicate ratio of the distances from the centre of forces.*

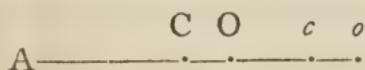
Let $VKS BV$ (fig. 27.) be the fixed orbit, and $upkb u$ the same orbit moved into another position; and let $VpnNoNtQV$ be the orbit described by the body in absolute space by the composition of its motion in the orbit with the motion of the orbit itself. If the body be supposed to describe the arch VP of the fixed orbit while the axis VC moves into the situation $u C$, and if the arch up be made equal to VP , then p will be the place of
the

the body in the moveable orbit, and in the compound path Vp . If the angular motion in the fixed orbit be to the motion of the moving orbit as m to n , it is plain that the angle VCP is to VCp as m to $m+n$. Let PK and pk be two equal and very small arches of the fixed and moving orbits. PC and pc are equal, as are also KC and kC , and a circle described round C with the radius CK will pass through k . If we now make VCK to VCn as m to $m+n$: the point n of the circle Kkn will be the point of the compound path, at which the body in the moving orbit arrives when the body in the fixed orbit arrives at K , and pn is the arch of the absolute path described while PK is described in the fixed path.

In order to judge of the difference between the force which produces the motion PK in the fixed orbit and that which produces pn in the absolute path, it must be observed that, in both cases, the body is made to approach the centre by the difference between CP and CK . This happens, because the centripetal forces, in both cases, are greater than what would enable the body to describe circles round C , at the distance CP , and with the same angular velocities that obtain in the two paths, viz. the fixed orbit and the absolute path. We shall call the one pair of forces the *circular forces*, and the other the *orbital*. Let C and c represent the forces which would produce circles, with the angular velocities which obtain in the fixed and moving orbits, and let O and o be the forces which produce the orbital motions in these two paths.

These

These things being premised, it is plain that $o - c$ is equal to $O - C$, because the bodies are equally brought toward the centre by the difference between O and C and by that between o and c . Therefore $o - O$ is equal to $c - C$. * The difference, therefore, of the forces which produce the motions in the fixed and moving orbits is always equal to the difference of the forces which would produce a circular motion at the same distances, and with the same angular velocity. But the forces which produce circular motions, with the angular motion that obtains in an orbit at different distances from the centre of forces, are as the cubes of the distances inversely (221). And the two angular motions at the same distance are in the constant ratio of m to $m + n$. Therefore the forces are in a constant ratio to each other, and their differences are in a constant ratio to either of the forces. But the circular force at different distances is inversely as the cube of the distance (221). Therefore the difference of them in the fixed and moveable orbits is in the same proportion. But the difference of the orbital forces



* For let $A o$, $A O$, $A c$, $A C$ represent the four forces o , O , c , and C . By what has been said, we find that $o c = O C$. To each of these add $O c$, and then it is plain that $o O = c C$, that is, that the difference of the circular forces c and C is equal to that of the orbital forces o and O .

forces is equal to that of the circular. Therefore, finally, the difference of the centripetal forces by which a body may be retained in a fixed orbit, and in the same orbit moving as determined in article 226, is always in the inverse triplicate ratio of the distances from the centre of forces.

In this example, the motion of the body in the orbit is in the same direction with that of the orbit, and the force to be joined with that in the fixed orbit is always additive. Had the orbit moved in the opposite direction, the force to be joined would have been subtractive, unless the retrograde motion of the orbit exceeded twice the angular motion of the body. But in all cases, the reasoning is similar.

228. Thus we have considered the motions of bodies influenced by forces directed to a fixed point. But we cannot conceive a mere mathematical point of space as the cause or occasion of any such exertion of forces. Such relations are observed only between existing bodies or masses of matter. The propositions which have been demonstrated may be true in relation to bodies placed in those fixed points. That continual tendency towards a centre, which produces an equable description of areas round it, becomes intelligible, if we suppose some body placed in the centre of forces, attracting the revolving body. Accordingly, we see very remarkable examples of such tendencies towards a central body in the motions of the planets round the Sun, and of the satellites round the primary planet.

But,

But, since it is a universal fact that all the relations between bodies are mutual, we are obliged to suppose that whatever force inclines the revolving body towards the body placed in the centre of forces, an equal force (from whatever source it is derived) inclines the central body toward the revolving body, and therefore it cannot remain at rest, but must move towards it. The notion of a fixed centre of forces is thus taken away again, and we seem to have demonstrated propositions inapplicable to any thing in nature. But more attentive consideration will shew us that our propositions are most strictly applicable to the phenomena of nature.

229. For, in the first place, the motion of the common centre of position of two, or of any number of bodies, is not affected by their mutual actions. These, being equal and opposite, produce equal and opposite motions, or changes of motion. In this case, it follows from art. 115. that the state of the common centre is not affected by them.

230. Now, suppose two bodies S and P, situated at the extremities of the line SP (fig. 28.) Their centre of position is in a point C, dividing their distance in such a manner that SC is to CP as the number of material atoms in P to the number in S (110.) or $SC : PC = P : S$. Suppose the mutual forces to be centripetal. Then, being equal, exerted between every atom of the one, and every particle of the other, the vis motrix may be expressed

U

pressed

pressed by $P \times S$. This must produce equal quantities of motion in each of the bodies, and therefore must produce velocities inversely as the quantities of matter (127). In any given portion of time, therefore, the bodies will move towards each other, to s and p , and Ss will be to Pp as P to S , that is as SC to PC . Therefore we shall still have $sC : pC = SC : PC$. Their distances from C will always be in the same proportion. Also we shall have $SC : SP = P : S + P$, and $sC : pC = P : S + P$; and therefore $SC : SP = sC : sP$. Consequently, in whatever manner the mutual forces vary by a variation of distance from each other, they will vary in the same manner by the same variation of distance from C . And, conversely, in whatever manner the forces vary by a change of distance from C , they vary in the same manner by the same change of distance from each other.

Let us now suppose that when the bodies are at S and P , equal moving forces are applied to each in the opposite directions SA and PB . Did they not attract each other at all, they would, at the end of some small portion of time, be found in the points A and B of a straight line drawn through C , because they will move with equal quantities of motion, or with velocities SA and PB inversely as their quantities of matter. Therefore $SA : PB = SC : PC$, and A , C , and B are in a straight line. But let them now attract, when impelled from S and P . Being equally attracted toward each other, they will describe curve lines Sa and Pb , so that their deflections Aa and Bb are as SC and PC ; and we shall

have

have $aC : bC = SC : PC$. As this is true of every part of the curve, it follows that they describe similar curves round C, which remains in its original place.

Lastly, If the motion of P be considered by an observer placed in S, unconscious of its motion, since he judges of the motion of P only by its change of direction and of distance, we may make a figure which will perfectly represent this motion. Draw the line EF equal and parallel to PS, and EG equal and parallel to ab . Do this for every point of the curve Sa and Pb. We shall then form a curve FG similar to the curves Sa and Pb, having the homologous lines equal to the sum of the homologous lines of these two curves. Thus the bodies will describe round each other curve lines which are similar and equal (lineally) to the lines which they describe round their common centre by the same forces. They may appear to describe areas proportional to the times round each other; and they really describe areas proportional to the times round their common centre of position, and the forces, which really relate to the body which is *supposed* to be central, have the same mathematical relation to their common centre.

Thus it appears that the mechanical inferences, drawn from a supposed relation to a mere point of space, are true in the real relations to the supposed central body, although it is not fixed in one place.

231. The time of describing any arch FG of the curve described round the other body at rest in a centre

of forces (where we may suppose it forcibly withheld from moving) is to the time of describing the similar arch $P\theta$ round the common centre of position in the subduplicate ratio of $S + P$ to S , that is, in the ratio of $\sqrt{S + P}$ to \sqrt{S} . For the forces being the same in both motions, the spaces described by their similar actions, that is, their deflections from the tangent are as the squares of the times T and t (204). That is, $HG : Bb = T^2 : t^2$, and $T : t = \sqrt{HG} : \sqrt{Bb} = \sqrt{S + P} : \sqrt{S}$.

Hence it follows that the two bodies S and P are moved in the same way as if they did not act on each other, but were both acted upon by a third body, placed in their common centre C , and acting with the same forces on each; and the Law of variation of the forces by a change of distance from each other, and from this third body, is the same.

232. If a body P (fig. 29.) revolve around another body S , by the action of a central force, while S moves in any path ASB , P will continue to describe areas proportional to the times round S , if every particle in P be affected by the same accelerating force that acts, in that instant, on every particle in S . For, such action will compound the same motions Pp and Ss with the motions of S and P , whatever they are; and it was shown in art. (98.) that such composition does not affect their relative motions. This is another way of making a body describe the same orbit in motion which it describes while the orbit is fixed (226).

MECHANICAL PHILOSOPHY.

PART II.

THE MECHANICAL HISTORY OF NATURE.

INTRODUCTION.

233. WE have now considered in sufficient detail those general Consequences which result from the relations of the Ideas that we have of Matter and Motion, and of the Causes of its changes. These consequences are the metaphysical or abstract doctrines of Mechanical Philosophy. They are, in reality, descriptions, not of external nature, but of the proceedings of the human mind in contemplating or studying it. Being independent of all experience of any thing beyond our own thoughts, they form a body of demonstrative truths. If this has been made sufficiently complete, that is, if all the possible mechanical changes are comprehended in the three propositions which we called the Laws of Motion, we should now be in a condition to consider every change of motion, and every changing cause, which nature presents to our view, whether in order to investigate and discover

discover natural Forces hitherto unknown, and to give an account of the Laws by which their action is regulated, or to explain complicated phenomena, by referring them to the operation of some known forces.

234. Both of these purposes are to be attained by a careful observation of the phenomena. All circumstances of coincidence or resemblance among them are to be taken notice of, and considered as indications of a similarity in their Causes. The more extensive the observed coincidence of appearances is, the more general must the affection of matter be which is the cause of this resemblance. If any similarity is universally observed, it must be considered as the indication of a mechanical quality that is competent to all matter.

235. This consideration points out to us a principle for arranging the mechanical phenomena of the universe. Those should be first considered that are most general. Thus are we made acquainted with the most general mechanical properties of Bodies, which extend their influence to phenomena in all the subordinate classes, and modify even that circumstance which forms the particular class. Our previous acquaintance with those general properties will enable us to free the more particular phenomena from part of that complication which makes the study of them more difficult; and then to consider apart those circumstances of the phenomena which are indications of qualities less general.

236. The most general phenomenon that we observe is the curvilinear motion of bodies in free space. The Globe which we inhabit, the Sun, and all his attending Planets and Comets, are continually moving in curve-lined paths. And these curvilinear motions are compounded with all the other motions that are performed on the surface of this Globe. When a cannon bullet is discharged in a southerly direction with the velocity of 1500 feet in a second, it is at the same time carried eastward, nearly at the same rate, by the rotation of the Earth; and by its revolution in a year round the Sun, it is moving eastward, more than sixty times as fast. Such being the condition of the visible universe, it appears that the deflecting forces, by which all these bodies are kept in their curvilinear paths, must be acknowledged to have the most extensive influence. The phenomena which are the indications of these forces, claim the first place in the Mechanical history of Nature. These are observed in the celestial motions, and Astronomy is therefore the first department of that history to which we shall turn our attention,

237. This order of study has other advantages besides this scientific propriety. It is that part of the study of material nature in which the understanding of man has been most successful. It is perhaps owing to the unexceptionable proofs, which Astronomy alone affords of the perfect conformity of our abstract doctrines with the real state of the world, that those doctrines have
been

been admitted as a just exposition of the elements of Universal Mechanics, and thus have given us a groundwork, on which we can proceed with confidence in explaining the mechanical phenomena of this sublunary world.

Astronomy is also the department of natural science that is the most easily comprehended with the distinctness and accuracy that deserve the name of science. Here we have a clear and adequate idea of the subject, and a distinct feeling of the validity of the evidence by which any proposition is supported. In the simplest proposition of common Mechanics, or Hydraulics, the subject under consideration has a degree of complication not to be found in the most abstruse proposition in Astronomy. Accordingly, the knowledge which we can acquire in Astronomy approaches near to the certainty of first principles; while in those other departments it is only a superficial knowledge of some very general property that we are able to acquire.

Astronomy is therefore recommended to our first notice, by the universality of the powers of nature that are indicated by the planetary motions,—by the successfulness of the investigation,—and by the easy access which it gives us to the elementary principles of all Mechanical science.

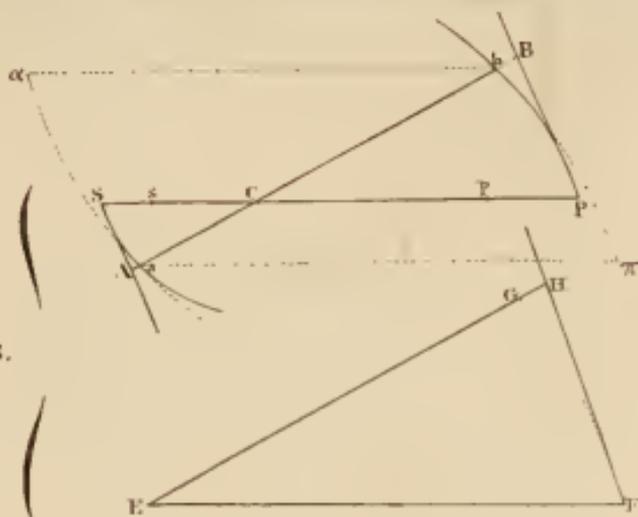


Fig. 28.

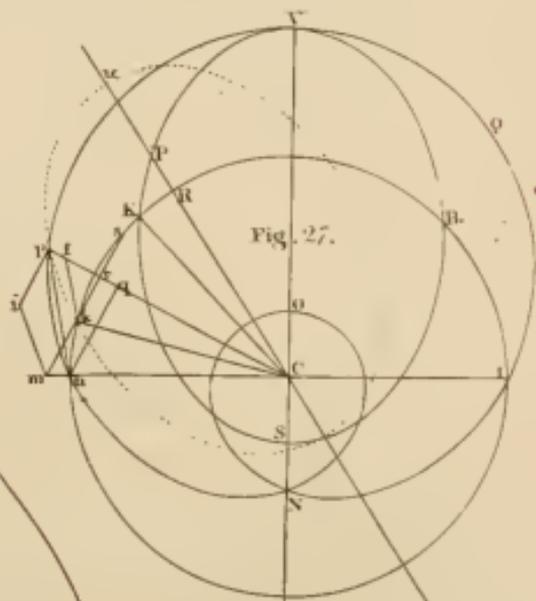


Fig. 27.

Fig. 26.

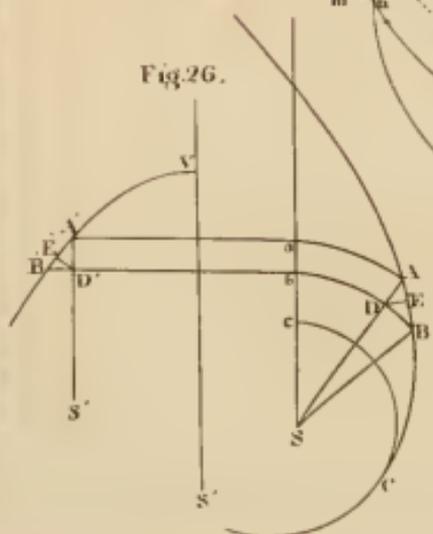
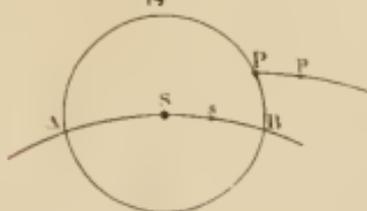


Fig. 29



MECHANICAL HISTORY

OF

NATURE.

SECT. I. ASTRONOMY.

238. **A**STRONOMY was first studied as an art, subservient to the purposes of social life. Some knowledge of the celestial motions was necessary, in every state of society, that we might mark the progress of the seasons, which regulate the labours of the cultivator, and the migrations of the shepherd. It is necessary for the record of past events, and for the appointment of national meetings.

While the motions of the heavenly bodies afford us the means of attaining these useful ends, they also present to the curious philosopher a series of magnificent phenomena, the operation of the greatest powers of material nature; and thus they powerfully excite his curiosity with respect to their causes. This circumstance alone makes the celestial motions the proper objects of attention to a student of Mechanical Philosophy, and he has less concern in the beautiful regularity and subordination which have made them so subservient to the purposes of Navigation, of Chronology, and the occupations of rural life.

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But the purposes of the Mechanical philosopher cannot be attained without attending to that beauty, regularity, and subordination. These features are exhibited in every circumstance of the celestial motions that renders them susceptible of scientific arrangement and investigation; and a philosophical view cannot be taken, without the same accurate knowledge of the motions that is wanted for the arts of life. It must be added, that society never would have derived the benefits which it has received from Astronomy, without the labours of the philosopher: For, had not Newton, or some such exalted genius as Newton, speculated about the deflecting forces which regulate the motions of the Solar system, we never should have acquired that exquisite knowledge of the mere phenomena that is absolutely necessary for some of the most important applications of them to the arts. It was these speculations alone that have enabled our navigators to proceed with boldness through untried seas, and in a few years have almost completed the survey of this globe. And thus do we experience the most beneficial alliance of Philosophy and Art.

Since the motions of bodies are the only indications, characteristics, and measures of moving forces, it is plain that the celestial motions must be accurately ascertained, that we may obtain the data wanted for the purpose of philosophical inference. To ascertain these is a task of great difficulty; and it has required the continual efforts of many ages to acquire just notions of the
motions

motions exhibited to our view in the heavens. For the same general appearances may be exhibited, and the same perceptions obtained, and the same opinions will be formed, by means of motions very different; and it is frequently very difficult to select those motions which alone can exhibit *every* observed appearance. If a person who is in motion, imagines that he is at rest, and assumes this principle in his reasonings about the effects of the motions which he perceives, he mistakes the conclusions which he draws for real perceptions; and calls that a deception of sense, which is really an error in judgement. Errors in our opinions concerning the motions of the heavenly bodies, are necessarily accompanied by false judgements concerning their causes. Therefore, an accurate examination of the motions which really obtain in the heavens, must precede every attempt to investigate their causes.

The most probable plan for acquiring a just and satisfactory knowledge of these particulars, is to follow the steps of our predecessors in this study, and first to consider the more general and obvious phenomena. From these we must deduce the opinions which most obviously suggest themselves, to be corrected afterwards, by comparing them with other phenomena, which may happen to be irreconcilable with them.

Astronomical Phenomena.

239. To an observer, whose view on all sides is bounded only by the sea, the heavens appear a concave sphere, of which the eye is the centre, studded with a great number of luminous bodies, of which the Sun and Moon are the most remarkable. This sphere is called the SPHERE OF THE STARRY HEAVENS.

The only distances in the heavens which are the immediate objects of our observation, are arches of great circles passing through the different points of the starry heavens. Therefore, all astronomical computations and measurements are performed by the rules of spherical trigonometry.

240. We see only the half of the heavens at a time, the other half being hid by the earth, on which we are placed. The great circle $HBOD$ (fig. 30.), which separates the visible hemisphere HZO from the invisible hemisphere HNO , is called the HORIZON. This is marked out on the starry heavens by the farthest edge of the sea. The point Z immediately over the head of the observer is called the ZENITH; and the point N , diametrically opposite to it, is called the NADIR.

241. The zenith and nadir are poles of the horizon.

242. If an observer looks at the heavens, while a plummet is suspended before his eye, the plumb line will mark out on the heavens a quadrant of a circle, whose plane is perpendicular to the horizon, and which therefore passes through the zenith and nadir, and through two opposite points of the horizon. $ZONH$ and $ZBND$ are such circles. They are called **VERTICAL CIRCLES** and **AZIMUTH CIRCLES**.

243. The **ALTITUDE** of any celestial phenomenon such as a star A , is the angle ACB , formed in the plane of the vertical circle ZAN , by the horizontal line CB and the line CA . This name is also given to the arch AB of the vertical circle which measures this angle. The arch ZA is called **ZENITH DISTANCE** of the phenomenon.

244. The **AZIMUTH** of the phenomenon is the angle OCB , or OZB , formed between the plane of the vertical circle ZAB passing through the phenomenon, and the plane of some other noted vertical ZON . The arch CB of the horizon, which measures this angle, is also frequently called the **Azimuth**.

245. The starry heavens appear to turn round the earth, which seems pendulous in the centre of the sphere; and by this motion, the heavenly bodies come into view in the east, or **RISE**; they attain the greatest altitude, or **CULMINATE**, and disappear in the west, or **SET**. This is called the **FIRST MOTION**.

246. This motion is performed round an axis NS (fig. 31.), passing through two points N, S , called the poles of the world. In consequence of this motion, a celestial object A describes a circle $ADBF$, through the centre C of which the axis NS passes, perpendicularly to its plane. This motion may be very distinctly perceived as follows. Let a point, or sight, be fixed in the inside of a sky-light fronting the north, and inclined southwards from the perpendicular at an angle equal to the latitude of the place. An eye placed at this point will see the stars through the glass of the window. Let the points of the glass, through which a star appears from time to time be marked. The marks will be found to lie in the circumference of a circle, the centre of which will mark the place of the pole in the heavens.

247. Those stars which are farthest from the poles will describe the greatest circles; and those will describe the largest possible circles which are in the circumference of the circle ÆWQE , which is equidistant from both poles. This circle is called the EQUATOR, and, being a great circle, it cuts the horizon in two points, E, W , diametrically opposite to each other. They are the east and west points of the horizon.

248. If a great circle $ANQS\text{Æ}$ passes through the poles perpendicularly to the horizon $HWOE$, it will cut it in the north and south points; and any star A will acquire its greatest elevation when it comes to the
femicircle

femicircle NAS , and its greatest depression when it comes to the femicircle NBS ; and the arch DAF of its apparition will be bisected in A .

249. If the circle $ADBF$ of revolution be between the equator and that pole N which is above the horizon, the greatest portion of it will be visible; but if it be on the other side of the equator, the smallest portion will be visible. One half of the equator is visible. Some circles of revolution are wholly above the horizon, and some are wholly below it. A star in one of the first is always seen, and one in the last is never seen.

250. The distance $A\mathcal{E}$ of any point A from the equator is called its **DECLINATION**, and the circle $ADBF$, being parallel to the equator, is called a **PARALLEL OF DECLINATION**.

251. The angle $\mathcal{E}CH$, contained by the planes of the equator and horizon, is the complement of the angle NCO , which is the elevation of the pole.

252. The revolution of the starry heavens is performed in $23^h 56' 4''$. It is called the **DIURNAL REVOLUTION**. No appearance of inequality has been observed in it; and it is therefore assumed as the most perfect measure of time.

253. The time of the diurnal apparition or disappearance of a point of the starry heavens is bisected in the
instant

instant of its culmination or greatest depression. The sun, therefore, is in the circle $N A S Q$ at noon. For this reason the circle $N A S Q$ is called the MERIDIAN.

254. A phenomenon whose circle of diurnal revolution $A D B F$ is on the same side of the equator with the elevated pole, is longer visible than it is invisible. The contrary obtains if it be on the other side of the equator.

255. Any great circle $N A \mathcal{A} E S$, or $N B L S$ (fig. 32.), passing through the poles of the world, is called an HOUR CIRCLE.

256. The angle $\mathcal{A} C L$, or $\mathcal{A} E N L$, contained between the plane of the hour-circle $N B L S$, passing through any phenomenon B , and the plane of the hour circle $N \mathcal{A} E S$, passing through a certain noted point \mathcal{A} of the equator, is called the RIGHT ASCENSION of the phenomenon. The intercepted arch $\mathcal{A} L$ of the equator, which measures this angle, is called by the same name.

257. In assigning the place of any celestial phenomenon, we cannot use any points of the earth as points of reference. The starry heavens afford a very convenient means for this purpose. Most of the stars retain their relative situations, and may therefore be used as so many points of reference. The application of this to our purpose requires

requires a knowledge of the positions of the stars. This may be acquired. The difference between the meridional altitude of a star B, and of the equator, gives the arch A \mathcal{A} E, intercepted between the equator and the parallel of declination, or circle of diurnal revolution A B D, described by the star. And the time which elapses between the passage of this star over the meridian, and the passage of that point \mathcal{A} E of the equator from which the right ascensions are computed, gives the arch \mathcal{A} E L of the equator which has passed during this interval. Therefore, an hour circle N L S being drawn through the point L of the equator, and a circle of revolution A B D being drawn at the observed distance A \mathcal{A} E from the equator, the place of the star will be found in their intersection B.

258. Globes and maps have been made, on which the representations of the stars have been placed, in positions similar to their real positions; and catalogues of the stars have been composed, in which every star is set down with its declination and right ascension, this being the most convenient arrangement for the practical astronomer. Their longitudes and latitudes (to be explained afterwards) are also set down, in separate columns. The most noted of all these is the BRITANNIC CATALOGUE, constructed by Dr Flamsteed, from his own observations in the Royal Observatory at Greenwich. This catalogue contains the places of 3030 stars. It is accompanied by a collection of maps, known to all astronomers by the

title of *ATLAS CELESTIS*. An useful abridgement of both has been published by *Bode* in *Berlin*, and by *Fortin* at *Paris*, in small quarto. Two planispheres have also been published by *Senex*, in *London*, constructed from the same observations, and executed with uncommon elegance; as also a particular map of that zone of the heavens to which all the planetary motions are limited. This is also executed with superior elegance and accuracy. The place of any phenomenon may be ascertained in it within 5' of the truth, by mere inspection, without calculation, scale, or compasses. No astronomer should be unprovided with it.

259. All these representations and descriptions of the starry heavens become obsolete, in some measure, in consequence of a gradual change in the declination and right ascension of the stars. But, as this may be accurately computed, the maps and catalogues retain their original value, requiring only a little trouble in accommodating them to the present state of the heavens. The *Britannic Catalogue* and *Atlas* are adjusted to the state of the heavens in 1690; and the planispheres, &c. by *Senex* are the same. The editions of *Paris* and *Berlin* are for 1750.

260. In these maps and catalogues, it has been found convenient to distribute the stars into groups, called *CONSTELLATIONS*; and figures are drawn, which comprehend all the stars of a group, and give them a sort of connexion

connexion and a name. Each star is distinguished by its number in the constellation, and also by a letter of the alphabet. Thus, the most brilliant star in the heavens, the Dog star, or Sirius, is known to all astronomers as N^o 9., or as α , *canis majoris*. The numbers always refer to the Britannic catalogue, it being considered as classical.

261. Since the publication of that work, however, great additions have been made to our knowledge of the starry heavens, and several Catalogues and Atlases have been published in different parts of Europe. Of the catalogues, the most esteemed are, 1. a small catalogue of 389 stars, the places of which have been determined with the utmost care by Dr Bradley, at the Greenwich Observatory; 2. a catalogue of the southern stars by Abbé de la Caille; 3. a catalogue of the zodiacal stars by Tobias Mayer at Gottingen; and, *lastly*, a new atlas celestis, consisting of a catalogue and maps of the whole heavens, and containing above 15,000 stars, by Mr Bode of Berlin. The Rev. Mr Fr. Wollaston published, in 1780, a specimen of a general astronomical catalogue of the fixed stars, arranged according to their declinations, folio, London, 1780. This is a most valuable work, containing the places of many thousand stars, according to the catalogues of Flamsteed, La Caille, Bradley, and Mayer. These being arranged in parallel columns, we see the differences between the determinations of those astronomers, and are advertised of any changes which have occurred in the heavens. The catalogue is accompanied

by directions for prosecuting this method of obtaining a minute survey of the whole stary heavens.

In the valuable astronomical tables published in 1776 by the academy of Berlin, Mr Bode has given a similar synopsis of the catalogues of Flamsteed, La Caille, Bradley and Mayer, not indeed so extensive, nor so minute, as Wollaston's, but of great use.

262. Having thus obtained maps of the heavens, the place of a celestial phenomenon is ascertained in a variety of ways. 1. By its observed distance from two known stars. 2. By its altitude and azimuth. 3. Most accurately, by its right ascension and declination.

263. This last being the most accurate method of ascertaining the place of any celestial phenomenon, observations of meridional altitude, and of TRANSITS over the meridian, are the most important. For an account of the manner of conducting these observations, and a description of the instruments, we may consult Smith's Optics, Vol. II. ; Mr Vince's Treatise of Practical Astronomy ; La Lande's Astronomy, &c. The MURAL QUADRANT, TRANSIT INSTRUMENT, and CLOCK, are therefore the capital furniture of an observatory ; to which, however, should be added an EQUATOREAL INSTRUMENT for observing phenomena out of the meridian. Other instruments, such as the EQUAL ALTITUDE INSTRUMENT, the RHOMBOIDAL RETICULA, the ZENITH SECTOR, and one or two more, are fitted for astronomers on a voyage.

264. The position of the meridian, and the latitude of the observatory, must be accurately determined. Various methods of determining the meridian. The most accurate is to view a circumpolar star through a telescope which has an accurate motion in a vertical plane, and to change the position of the telescope till the times which elapse between the successive upper and lower transits of the star are precisely equal. The instrument is then in the plane of the meridian (fig. 33.)

265. In order to find the declination of a phenomenon more readily, it is convenient to know the inclination of the axis of diurnal revolution NS (fig. 31.) to the horizon, or the elevation of the pole N. The best method for this purpose is to observe the greatest elevation IO, and the least elevation KO, of some circumpolar star. The elevation of the pole N is half the sum of those elevations.

266. The elevation of the pole is different in different places. An observer, situated $69\frac{1}{2}$ statute miles due north of another, will find the pole elevated about a degree more above his horizon. From observations of this kind, the bulk and shape of the earth are determined. For it is plain that 360 times $69\frac{1}{2}$ miles must be the circumference of the globe. It is found to be nearly an elliptical spheroid, of which the axis is 7904 miles, and the greatest diameter $7940\frac{2}{3}$ miles. This deviation from perfect sphericity has been discovered by measuring,
in

in the way now mentioned, a degree of the meridian in different latitudes. One was measured in Lapland, in latitude $66^{\circ} 20'$, and it measured 122,457 yards, exceeding $69\frac{1}{2}$ miles by 137 yards. Another was measured at Peru, crossing the very equator. It contained 121,027 yards, falling short of $69\frac{1}{2}$ miles by 1293 yards, and wanting 1430 yards, or almost a mile, of the other. Other degrees have been measured in intermediate latitudes; and it is clearly established, that the degrees gradually increase, as we go from the equator towards either pole.

267. The length of a degree is the distance between two places where the tangents to the surface are inclined to one another one degree, or where two plumb lines, which are perpendicular to the surface of standing water, will, when produced downwards, meet one another, intercepting an angle of one degree. The surface of the still ocean is therefore less incurvated as we approach the poles, or it requires a longer arch to have the same curvature. It is a degree of a larger circle, and has a longer radius. Persons who do not consider the thing attentively, are apt to imagine, from this, that the earth is shaped like an egg; because, if we draw from its centre lines CN (fig. 33. N^o 2.) CO , CP , CQ , equally inclined to one another, the arches NO , OP , PQ , will gradually increase from N towards Q . If these lines make angles of one degree with one another, they will meet the surface in points that are farther and farther
afunder,

afunder, and the degree will appear to increase as we approach the points E and Q , which we suppose, at present, to be the poles. But let such persons reflect, that if these lines from the centre are produced beyond the surface, they cannot be plumb lines, perpendicular to the surface of standing water. But if an ellipse $NESQ$ (fig. 33. $N^{\circ} 2.$) be made to turn round its shorter axis NS , it will generate a figure flatter round N and S than at E or Q . If we draw two lines aD and bB perpendicular to the curve in a and b , and exceedingly near one another, they will be tangents to a curve $ABDF$, by the evolution of which the elliptic quadrant EaN is described. AE is the radius of curvature of the equatorial degree of the meridian EaN . NF is the radius of the polar degree, and aD is the radius of curvature at the intermediate latitude of a , &c. All these radii are plumb lines, perpendicular to the elliptical curve of the ocean.

These plumb lines therefore do not meet in the centre of the earth, as is commonly imagined, but meet, in succession, in the circumference of the evolute $ABDF$. The earth is not a *prolate* spheroid like an egg, but an *oblate* spheroid, like a turnip or bias bowl.

268. Since the axis of diurnal revolution passes through the centre of the earth, it marks on its surface two points, which are the poles of the earth. These are in the extremities of the axis of the terrestrial spheroid. In like manner, the plane of the celestial equator
passing

passing through the centre of the earth, divides it into two hemispheres, the northern and southern, separated by the *terrestrial* equator. Also the hour circles, passing through the earth's centre, mark on its surface the terrestrial meridians.

269. The position of a place on the surface of the earth is determined by its LATITUDE, or distance from the terrestrial equator, and its LONGITUDE, or the angular distance of its meridian, from some noted meridian.

270. Astronomical observations are made from a point on the surface of the earth, but, for the purposes of computation, are supposed to be made from the centre. The angular distance between the observed place A (fig. 34.) of a phenomenon S in the heavens, as seen from a place D on the Earth's surface, and its place B, as viewed from the centre, is called the PARALLAX of the phenomenon.

271. Besides the motion of diurnal revolution, common to all the heavenly bodies, there are other motions, which are peculiar to some of them, and are observed by us by means of their change of place in the starry heavens. Thus, while the starry heavens turn round the Earth from east to west in $23^{\text{h}} 56' 4''$, the Sun turns round it in 24^{h} . He *must*, therefore, change his place to the eastward in the starry heavens. The Moon has an evident motion eastward among the stars, moving her

own

own breadth in about an hour. There are five stars which are observed to change their places remarkably in the heavens, and are therefore called PLANETS, or wanderers; while those which do not change their relative places are called FIXED STARS. The planets are MERCURY, VENUS, MARS, JUPITER, and SATURN. To these we must now add the planet discovered in 1781 by Dr Herschel, which he called the Georgian Planet, in honour of his Sovereign, the distinguished patron of Astronomy. Astronomers on the continent have not adopted this denomination, and seem generally agreed to call it by the name of the discoverer. M. Piazzi, at Palermo, has discovered another, and M. Olbers, at Bremen, a third, which they have named Ceres and Pallas. None of the three are visible to the naked eye.

272. Planets are distinguishable from the fixed stars by the steadiness of their light, while all the fixed stars are observed to twinkle. The following symbols are frequently used:

For the Sun	-	-	-	-	☉
the Moon	-	-	-	-	☾
Mercury	-	-	-	-	♿
Venus	-	-	-	-	♀
the Earth	-	-	-	-	♁
Mars	-	-	-	-	♂
Jupiter	-	-	-	-	♃
Saturn	-	-	-	-	♄
Herschel	-	-	-	-	HL

The motions of these bodies have become interesting on various accounts. In order to acquire a knowledge of their motions more easily, it is convenient to abstract our attention from the diurnal motion, common to all, and attend only to their proper motions among the fixed stars.

Of the proper Motions of the Sun.

273. We cannot observe the motion of the Sun among the fixed stars immediately, on account of his great splendour, which hinders us from perceiving the stars in his neighbourhood. But we can observe the instant of his coming to the meridian, and his meridional altitude (257.) The Sun must be in that point of the heavens which passes the meridian at that instant, and with that altitude. Or we can observe the point of the heavens which comes to the meridian at midnight, with a declination as far on one side of the equator as the Sun's observed declination is on the other side of it. The Sun must be in the point of the heavens which is diametrically opposite to this point. By taking either of these methods, but particularly the first, we can ascertain a series of points of the heavens through which the Sun passes. These are found to be in the circumference of a great circle of the sphere *ASVW* (fig. 35.), which cuts the celestial equator in two opposite points *A, V*, and is inclined to it at an angle of $23^{\circ} 28' 10''$ nearly. This circle, or Sun's path, is called the *ECLIP TIC*.

274. In consequence of the obliquity of the ecliptic, the Sun's motion in it is accompanied by a change in the Sun's declination and right ascension, by a change in the length of the natural day, and by a change of the seasons. Therefore, the revolution of the Sun in the ecliptic is performed in a year.

275. The points V, A, are called EQUINOCTIAL POINTS; because, when the sun is in these points, his circle of diurnal revolution is the celestial equator, and therefore the day and night are equal. The point V, through which he passes in the month of March, is called the VERNAL EQUINOX, and the point A is called the AUTUMNAL EQUINOX. The points S and W, where he is farthest from the equator, are called the SOLSTITIAL POINTS, S being the summer, and W the winter solstice. The parallels of declination passing through the solstitial points are called TROPICS.

276. Right ascension is always computed eastward on the equator, from the vernal equinox.

277. The ecliptic passes through the constellations

Aries, distinguished by the symbol		♈
Taurus	- - - - -	♉
Gemini	- - - - -	♊
Cancer	- - - - -	♋
Leo	- - - - -	♌
Virgo	- - - - -	♍

Z 2

Libra,

Libra, distinguished by the symbol					♎
Scorpio	-	-	-	-	♏
Sagittarius	-	-	-	-	♐
Capricornus	-	-	-	-	♑
Aquarius	-	-	-	-	♒
Pisces	-	-	-	-	♓

These constellations are called the SIGNS of the ZODIAC; and a motion from west to east is said to be DIRECT, or IN CONSEQUENTIA SIGNORUM, while a contrary motion is called RETROGRADE, IN ANTECEDENTIA SIGNORUM.

278. The changes of the seasons were attributed by the ancients to the influence of the stars which were seen in the different seasons of the year.

279. The position of the ecliptic is invariable, and a complete revolution is performed in 365 days, 6 hours, 9 minutes, and 11 seconds.

280. If successive observations be made of the Sun's crossing the equator, it will be found that the equinoctial points are not fixed, but move to the westward about 50" in a year, so that they would make a complete revolution in about 25,972 years. This is called the PRECESSION of the EQUINOXES.

281. Sir Isaac Newton made a very ingenious and important inference from this astronomical fact. If we know

know the situation of the equinoctial points at the time of any historical event, the date of the event may be discovered. He thinks that this position at the time of the Argonautic expedition may be inferred from the description given by Aratus of the starry heavens. The poet describes a celestial sphere by which Chiron, one of the heroes, directed their motions; and from this he deduces data for a chronology of the heroic or fabulous ages. But, since the equinoctial points shift only at the rate of a degree in 72 years, and the Greeks were so ignorant, for ages after that epoch, that they did not know that the positions of the stars were changeable, it does not appear that much reliance can be had on this datum. We cannot, from the description by Aratus, be certain of the position of the vernal equinox within five or six degrees. This makes a difference of 400 years in the epochs.

282. The axis of diurnal revolution is not always the same, and the poles of the heavens describe (in 25,972 years) a circle round the pole of the ecliptic, distant from it $23^{\circ} 28' 10''$ nearly.

283. On account of the westerly motion of the equinoctial points, the return of the seasons must be accomplished in less time than that of the Sun's revolution round the heavens. The seasons return after an interval of $365^{\text{d}} 5^{\text{h}} 48' 45''$. This is called a TROPICAL year, to distinguish it from the interval $365^{\text{d}} 6^{\text{h}} 9' 11''$, called a SYDEREAL year.

284. Astronomers have chosen to refer the places of the heavenly bodies to the ecliptic, on account of its stability, rather than to the equator. For this purpose, great circles, such as PVp , PAp , (fig. 36.) are drawn through the poles P, p , of the ecliptic. These are called ECLIPTIC MERIDIANS. The arch AB of one of these circles, intercepted between a phenomenon A and the ecliptic, is called the LATITUDE of the phenomenon; and the arch VB , intercepted between the point V of the vernal equinox and the point B , is called the LONGITUDE of the phenomenon. This is sometimes expressed in degrees and minutes, and sometimes in signs, (each = 30° .)

285. The motion of the Sun in the ecliptic is not uniform. On the first of January his daily motion is nearly $1^\circ 1' 13''$. But on the first of July, his daily motion is $57' 13''$. The mean daily motion is $59' 08''$. The Sun's place in the ecliptic, calculated on the supposition of a daily motion of $59' 08''$, will be behind his observed place, from the beginning of January to the beginning of July, and will be before it, from the beginning of July to the beginning of January. The greatest difference is about $1^\circ 55' 32''$, which is observed about the beginning of April and October; at which times, the observed daily motion is $59' 08''$.

286. This unequable motion of the Sun appeared to the ancient astronomers to require some explanation.

It

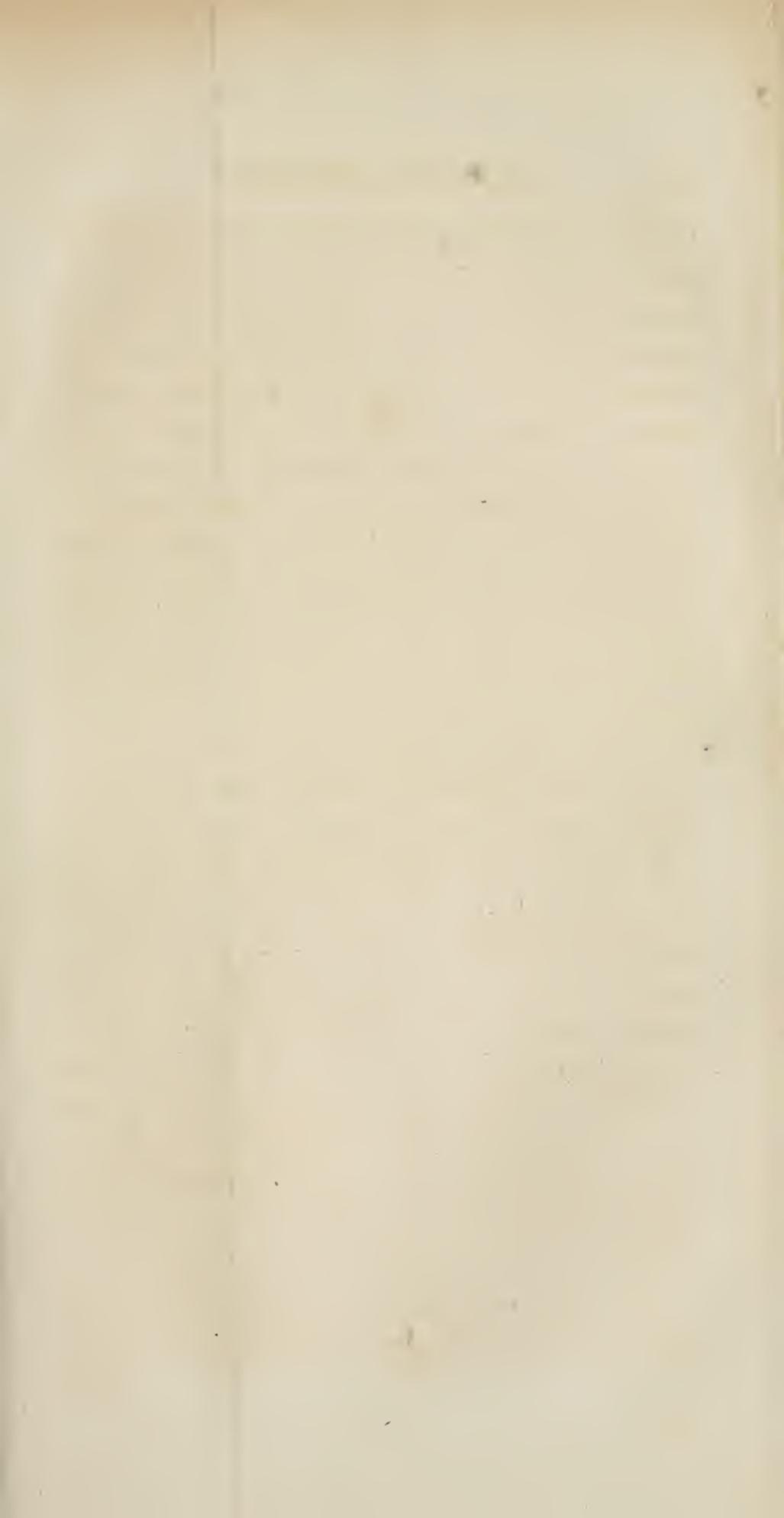


Fig. 30.

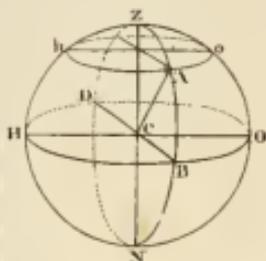


Fig. 31.

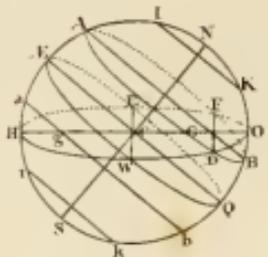


Fig. 32.

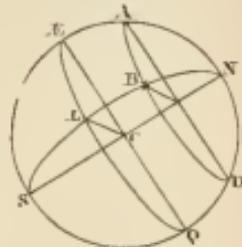


Fig. 33.

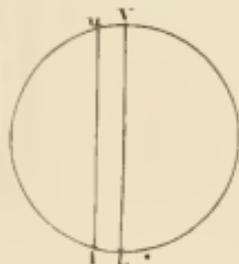


Fig. 33. N^o 2.

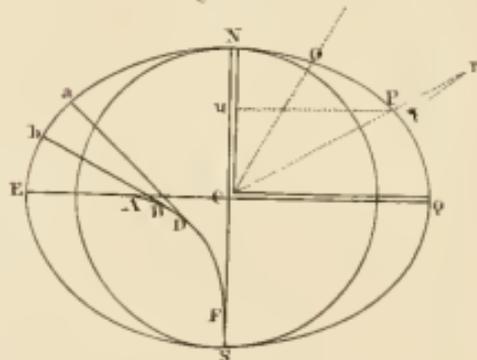


Fig. 35.

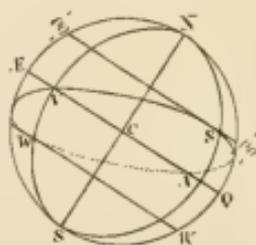


Fig. 34.

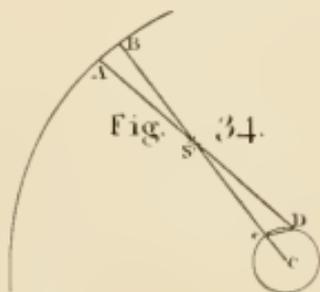
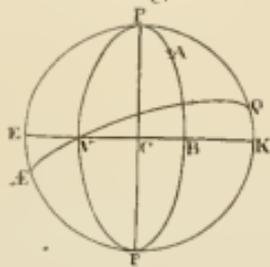
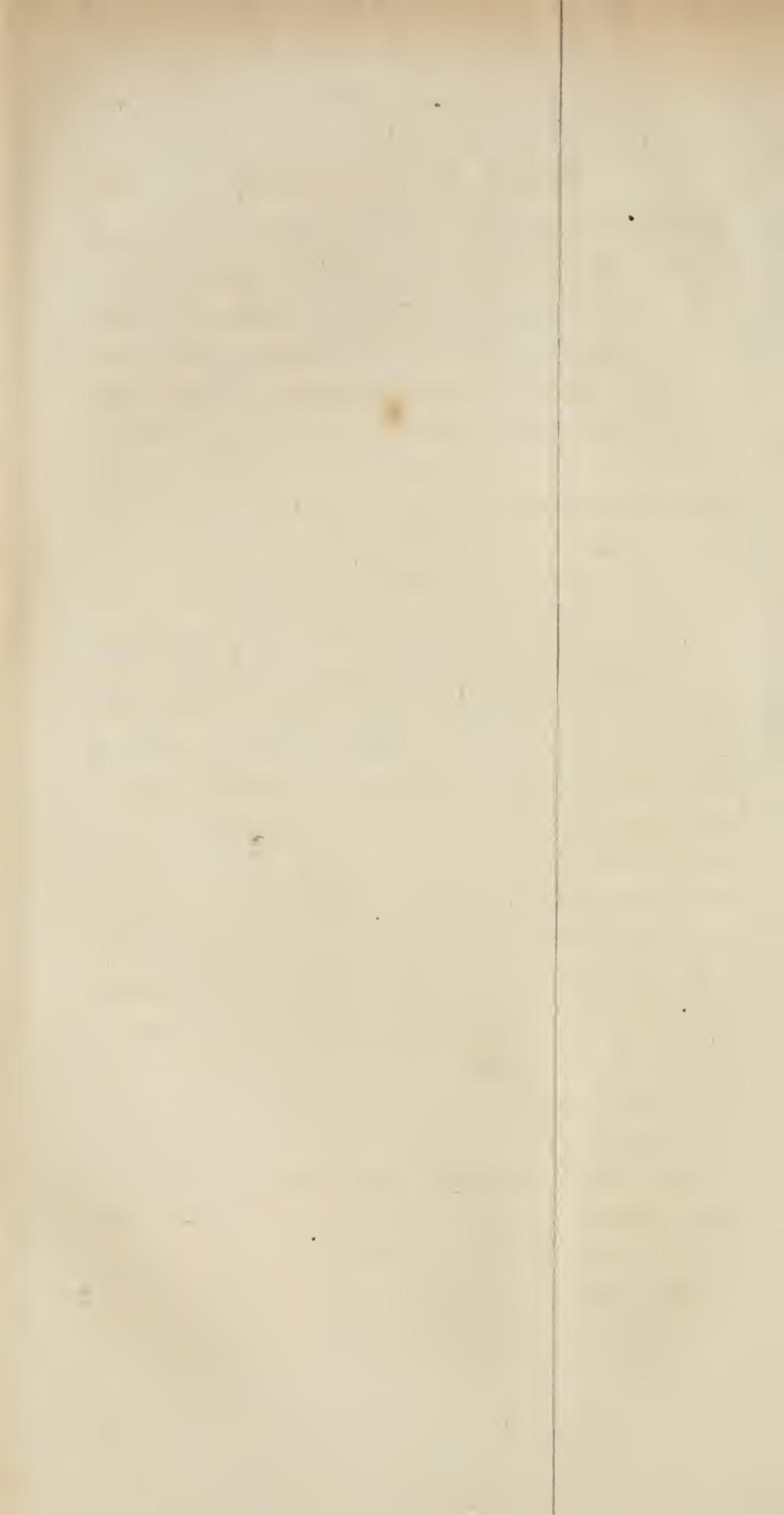


Fig. 36.



It had been received as a first principle, that the celestial motions were of the most perfect kind—and this perfection was thought to require invariable sameness. Therefore the Sun must be carried uniformly in the circumference of a figure perfectly uniform in every part. He must therefore move uniformly in the circumference of a circle. The astronomers therefore supposed that the Earth is not in the centre of this circle. Let $AbPd$ (fig. 37.) represent the Sun's orbit, having the Earth in E , at some distance from the centre C . It is plain that if the Sun's motion be uniform in the circumference, describing every day $59' 08''$, his angular motion, as seen from the Earth, must be slower when he is at A , his greatest distance, than when nearest to the Earth, at P . It is also evident that the point E may be so chosen, that an arch of $59' 08''$ at A shall subtend an angle at E that is only $57' 13''$, and that an arch of $59' 08''$ at P shall subtend an angle of $61' 13''$. This will be accomplished, if we make EP to EA as $57' 13''$ to $61' 13''$, or nearly as 14 to 15. This was accordingly done; and this method of solving the appearances was called the *eccentric hypothesis*. EC is the **ECCENTRICITY**, and PE is to PC nearly as 28 to 29.

287. But although this hypothesis agreed very well with observation in those points of the orbit where the Sun is most remote from the Earth, or nearest to it, it was found to differ greatly in other parts of the orbit, and particularly about half way between A and P . Astronomers,



It had been received as a first principle, that the celestial motions were of the most perfect kind—and this perfection was thought to require invariable sameness. Therefore the Sun must be carried uniformly in the circumference of a figure perfectly uniform in every part. He must therefore move uniformly in the circumference of a circle. The astronomers therefore supposed that the Earth is not in the centre of this circle. Let $AbPd$ (fig. 37.) represent the Sun's orbit, having the Earth in E , at some distance from the centre C . It is plain that if the Sun's motion be uniform in the circumference, describing every day $59' 08''$, his angular motion, as seen from the Earth, must be slower when he is at A , his greatest distance, than when nearest to the Earth, at P . It is also evident that the point E may be so chosen, that an arch of $59' 08''$ at A shall subtend an angle at E that is only $57' 13''$, and that an arch of $59' 08''$ at P shall subtend an angle of $61' 13''$. This will be accomplished, if we make EP to EA as $57' 13''$ to $61' 13''$, or nearly as 14 to 15. This was accordingly done; and this method of solving the appearances was called the *eccentric hypothesis*. EC is the **ECCENTRICITY**, and PE is to PC nearly as 28 to 29.

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astronomers, after trying various other hypotheses, were obliged to content themselves with reducing the eccentricity considerably, and also to suppose that the angular motion of $59' 08''$ per day was performed round a point e on the other side of the centre, at the same distance with E . This, however, was giving up the principle of perfect motion, if its perfection consisted in uniformity; for, in this case, the Sun cannot have an uniform motion in the circumference, and also an uniform angular motion round e . Besides, even this amendment of the eccentric hypothesis by no means agreed with the observations in the months of April and October: but they could not make it any better.

288. Astronomical computations are made on the supposition of uniform angular motion. The angle proportional to the time is called the MEAN MOTION, and the place thus computed is called the MEAN PLACE. The differences between the mean places and the observed, or TRUE PLACES, are called EQUATIONS. They are always greatest when the mean and true motions are equal, and they are nothing when the mean and true motions differ most. For, while the true daily angular motion is less than the mean daily motion, the observed place falls more and more behind the calculated place every day; and although, by gradually quickening, it loses less every day, it still loses, and falls still more behind; and when the true daily motion has at last become equal to the mean, it loses no more indeed, but it is now the farthest.

farthest behind that can be. Next day it gains a little of the lost ground, but is still behind. Gaining more and more every day, by its increase of angular motion, it at last comes up with the calculated place; but now, its angular motion is the greatest possible, and differs most from the equable mean motion:

289. These computations are begun from that point of the orbit where the motion is slowest, and the mean angular distance from this point is called the MEAN ANOMALY. A table is made of the equations corresponding to each degree of the mean anomaly. The true anomaly is found by adding to, or subtracting from the computed mean anomaly, the equations corresponding to it.

In this manner may the sun's longitude, or place in the ecliptic, be found for any time.

290. In consequence of the obliquity of the ecliptic, and the sun's unequal motion in it, the natural days, or the interval between two successive passages of the sun over the meridian, are unequal; and if a clock, which measures $365^d 5^h 48' 45''$ in a tropical year, be compared from day to day with an exact sun dial, they will be found to differ, and will agree only four times in the year. This difference is called the EQUATION OF TIME, and sometimes amounts to 16 minutes. The time shewn by the clock is called MEAN SOLAR TIME, and that shewn by the dial is called TRUE TIME and APPARENT TIME.

291. The change in the sun's motion is accompanied by a change in his apparent diameter, which, at the beginning of January, is about $32' 39''$, and at the beginning of July is about $31' 34''$, $\frac{1}{10}$ less. This must be ascribed to a change of distance, which must always be supposed inversely proportional to the apparent diameter.

292. By combining the observations of the sun's place in the ecliptic with those of his distance, inferred from the apparent diameter, and by other more decisive, but less obvious observations, Kepler, a German astronomer, found that his apparent path round the earth is an ellipse, having the earth in one focus, and having the longer axis to the shorter axis as 200,000 to 199,972.

The extremities A and P of the longer axis of the sun's orbit A B P D (fig. 37.) are called the APSIDES. The point A, where the sun is farthest from the earth (placed in E), is called the higher apsis, or APOGEE. P is the lower apsis, or PERIGEE. The distance E C between the focus and centre is called the ECCENTRICITY, and is 1680 parts of a scale, of which the mean distance E D is 100,000.

293. Kepler *observed*, that the sun's angular motion in this orbit was inversely proportional to the square of his distance from the earth; for he observed the sun's daily change of place to be as the square of his apparent diameter. Hence, he inferred that the radius vector E B described areas proportional to the times (103.)

294. From this he deduced a method of calculating the sun's place for any given time. Draw a line EF from the focus of the ellipse, which shall cut off a sector AEF , having the same proportion to the whole surface of the ellipse, which the interval of time between the sun's last passage through his apogee, and the time for which the computation is made, has to a syderal year; F will be the sun's true place for that time. This is called KEPLER'S PROBLEM.

This problem, the most interesting to astronomers, has not yet been solved otherwise than by approximation, or by geometrical constructions which do not admit of accurate computation.

295. Let $ABPD$ (fig. 37.) be the elliptical orbit, having the earth in the focus E . A and P , the extremities of the transverse axis, are the apogee and perigee of the revolving body. BD is the conjugate axis, and C the centre. It is required to draw a line ET which shall cut off a sector AET , which has to the whole ellipse the proportion of m to n ; m being taken to n in the proportion of the time elapsed since the body was in A to the time of a complete revolution.

Kepler, who was an excellent geometer, saw that this would be effected, if he could draw a line EI , which should cut off from the circumscribed circle $AbPd$ the area EAI , which is to the whole circle in the same proportion of m to n . For, then, drawing the perpendicular ordinate IR , cutting the ellipse in T , he knew that the area AET has the same proportion to the el-

lipse that AEI has to the circle. The proof of this is easy, and it seems greatly to simplify the problem. Draw IC through the centre, and make ES perpendicular to ICS . The area AEI consists of the circular sector ACI , and the triangle CIE . The sector is equal to half the rectangle of the radius CI and the arch AI , that is, to $\frac{CA \times IA}{2}$. The triangle CIE is equal to $\frac{CI \times ES}{2}$, or $\frac{CA \times ES}{2}$. Therefore it is evident that, if we make the arch IM equal to the straight line ES , the sector ACM will be equal to the circular area ACI , and the angle ACM will be to 360 degrees, as m to n .

296. Hence we see that it will be easy to find the time when the revolving body is in any point T . To find this, draw the ordinate RTI ; draw ICS and ES , and make $IM = ES$. Then, 360° is to the arch AM as the time of a revolution to the time in which the body moves over AT . This is (in the astronomical language) finding the mean anomaly when the true anomaly is given. The angle ACM , proportional to the time, is called the MEAN ANOMALY, and the angle AET is the TRUE ANOMALY. The angle ACI is called the ANOMALY OF THE ECCENTRIC, or the ECCENTRIC ANOMALY.

297. But the astronomer wants the true anomaly corresponding to a given mean anomaly. The process here given cannot be reversed. We cannot tell how
much

much to cut off from the given mean anomaly AM , so as to leave AI of a proper magnitude, because the indispensable measure of MI , namely ES , cannot be had till ICS be drawn. Kepler saw this, and said that his problem could not be solved geometrically. Since the invention of fluxions, however, and of converging series, very accurate solutions have been obtained. That given by *Frisius* in his *Cosmographia* is the same in principle with all the most approved methods, and the form in which it is presented is peculiarly simple and neat. But, except for the construction of original tables, these methods are rarely employed, on account of the laborious calculation which they require. Of all the direct approximate solutions, that given by Dr Matthew Stewart at the end of his *Traëts, Physical and Mathematical*, published in 1761, seems the most accurate and elegant; and the calculations founded on it are even shorter than the indirect method generally employed. His construction is as follows.

298. Let the angle AEM be the mean anomaly, join EM , and draw CI parallel to it, and MO perpendicular to CI . If the orbit is not more eccentric than that of Mars, make the arch iI equal to the excess of the arch Mi above its sine MO . Then AI is the eccentric anomaly corresponding to the mean anomaly AM , and the ordinate IR will cut the ellipse in T , so that AET will be the true anomaly required. The error will not amount to two seconds in any part of such orbits.

bits. But, for orbits of greater eccentricity, another step is necessary. Join Ei , and draw CQ parallel to Ei , meeting the tangent iQ in Q . Let D represent the excess of the arch Mi above its sine MO^* , and institute the following analogy, $\text{fin. } MCi : \tan. iCQ = D : iI$, taking iI from i towards M . The point, I , will be so situated that the sector AEI is very nearly equal to the sector ACM , or AI is the eccentric anomaly corresponding to the mean anomaly AM . The error will not amount to one second, even in the orbit of Mercury.

The demonstration of this construction is by no means abstruse or difficult. Draw IS , and MI . The triangles iCE and iCM are evidently equal, being on one base iC , and between the parallels iC and ME . For similar reasons, the triangles iSI and iEI are equal. Therefore the triangle iCE , together with the segment included between the arch Mbi and the chord Mi , will be equal to the circular sector iCM .

Now it is plain, from the construction, that $Si : Ci = SE : iQ, = MO : iQ, = \overline{Mbi - MO} : iI$. Therefore $Si \times iI = Ci \times Mbi - Ci \times MO$. But $Ci \times$
 Mbi

* This excess must be expressed in degrees, minutes, or seconds. The radius of a circle is equal to an arch of 206,265 seconds. The logarithm of this number is 5.3144251. Therefore we shall obtain ES , or the seconds in ES , by adding this logarithm to the logarithms of EC (AC being unity), and the logarithm of the sine of ACI . The sum is the logarithm of the seconds in ES .

Mbi is equal to twice the sector MCi , and $Ci \times MO$ is equal to twice the triangle MCi . Therefore $Si \times iI$ is equal to twice the segment contained between Mbi and the chord Mi . Therefore this segment is equal to the triangle iSI , or to the triangle iEI . Therefore the space $CiIEC$ is equal to the sector iCM , and the sector AEI to the sector ACM .

The calculation founded on this construction is extremely simple. In the triangle MCE , the sides MC and CE are given, with the included angle MCE ; and the angles CEM , CME are sought. Moreover, AE is the sum of the given sides, and PE is their difference, and ACM is the sum of the angles M and E . Therefore $AE : EP = \tan. \frac{E + M}{2} : \tan. \frac{E - M}{2}$; and thus E and M , or their equals, ACi and MCi , are obtained. In the next place, in the triangle iCE , the sides iC , CE , and the included angle iCE , are given, and the angle EiC is sought. We have, in the same manner as before, ACi equal to the sum of the angles E and i , and therefore $AE : EP = \tan. \frac{E + i}{2} : \tan. \frac{E - i}{2}$. Thus the angle EiC , or its equal, iCQ , is obtained, and then, the arch $iI = D \times \frac{iQ}{MO}, = D \times \frac{\tan. iCQ}{\sin. MCi}$.

In the very eccentric orbits of the comets, this brings us vastly nearer to the truth than any of the indirect methods we know does by the first step. So near indeed, that the common method, by the *rule of false position*, may now be safely employed. If the point, I , has been accurately found,

found, it is plain that if to the arch AI we add ES , that is, $EC \times \sin. ACI$, we obtain the arch AM with which we began. But if I has not been accurately determined, AM will differ from the primitive AM . Therefore, make some small change on AI , and again compute AM . This will probably be again erroneous. Then apply the rule of false position as usual. The error remaining after the first step of Dr Stewart's process is always so moderate that the variations of AM are very nearly proportional to the variations of AI ; so that two steps of the rule will generally bring the calculation within two or three seconds of the truth. The astronomical student will find many beautiful and important propositions in these mathematical tracts. The proposition just now employed is in page 398, &c.

299. Astronomers have discovered, that the line AP moves slowly round E to the eastward, changing its place about $25' 56''$ in a century. This makes the time of a complete revolution in the orbit to be $365^d 6^h 15' 20''$. This time is called the ANOMALISTIC YEAR.

Of the proper Motions of the Moon.

300. Of all the celestial motions, the most obvious are those of the Moon. We see her shift her situation among the stars about her own breadth to the eastward in an hour, and in somewhat less than a month she makes a complete tour of the heavens. The gentle beauty of her

her

her appearance during the quiet hours of a serene night, has attracted the notice, and we may say the affections of all mankind; and she is justly styled the Queen of Heaven. The remarkable and distinct changes of her appearance have afforded to all simple nations a most convenient index and measure of time, both for recording past events, and for making any future appointments for business. Accordingly, we find, in the first histories of all nations, that the lunar motions were the first studied, and, in some degree, understood. It seems to have been in subserviency to this study alone that the other appearances of the starry heavens were attended to; and the relative positions of the stars seem to have interested us, merely as the means of ascertaining the motions of the Moon. For we find all the zodiacs of the ancient oriental nations divided, not into 12 equal portions, corresponding to the Sun's progress during the period of seasons, but into 27 parts, corresponding to the Moon's daily progress, and these are expressly called the HOUSES OR MANSIONS of the Moon. This is the distribution of the zodiac of the ancient Hindoos, the Persians, the Chinese, and even the Chaldeans. Some have no division into 12, and those who have, do not give *names* to 12 groups of stars, but to 27. They first describe the situation of a planet in one of these mansions by name, noting its distance from some stars in that group, and thence infer in what part of which twelfth of the circumference it is placed. The division into 12 parts is merely mathematical, for the purpose of calculation. In all probability, therefore, this

was an after-thought, the contrivance of a more cultivated age, well acquainted with the heavens as an object of sight, and beginning to extend the attention to speculations beyond the first conveniences of life.

301. When the Moon's path through this series of mansions is carefully observed, it is found to be (very nearly) a great circle of the heavens, and therefore in a plane passing through the centre of the earth.

302. She makes a complete revolution of the heavens in $27^{\text{d}} 7^{\text{h}} 43' 12''$, but with some variations. Her mean daily motion is therefore $13^{\circ} 10' 25''$, and her horary motion is $32' 56''$.

303. Her orbit is inclined to the plane of the ecliptic in an angle of $5^{\circ} 8' 45''$, nearly, cutting it in two points called her NODES, diametrically opposite to each other; and that node through which she passes in coming from the south to the north side of the ecliptic, is called the ASCENDING NODE.

304. The nodes have a motion which is generally westward, but with considerable irregularities, making a complete revolution in about $6803^{\text{d}} 2^{\text{h}} 55' 18''$, nearly $18\frac{1}{3}$ years.

305. If we mark on a celestial globe a series of points where the Moon was observed during three or
four

four revolutions, and then lap a tape round the globe, covering those points, we shall see that the tape crosses the ecliptic more westerly every turn, and then crosses the last round very obliquely; and we see that by continuing this operation, we shall completely cover with the tape a zone of the heavens, about ten or eleven degrees broad, having the ecliptic running along its middle.

306. The Moon moves unequally in this orbit, her hourly motion increasing from $29' 34''$ to $36' 48''$, and the equation of the orbit sometimes amounts to $6^{\circ} 18' 32''$; so that if, setting out from the point where her horary motion is slowest, we calculate her place, for the eighth day thereafter, at the rate of $32' 56''$ per hour, we shall find her observed place short of our calculation more than half a day's motion. And we should have found her as much before it, had we begun our calculation from the opposite point of her orbit.

307. Her apparent diameter changes from $29' 26''$ to $33' 47''$, and therefore her distance from the Earth changes. This distance may be discovered in miles by means of her parallax.

She was observed, in her passage over the meridian, by two astronomers, one of whom was at Berlin, and the other at the Cape of Good Hope. These two places are distant from one another above 5000 miles; so that the observer at Berlin saw the Moon every day considerably more to the south than the person at the Cape. This

difference of apparent declination is the measure of the angle $DS C$ (fig. 34.) subtended at the Moon by the line cD of 5443 miles, between the observers. The angles SDc and ScD are given by means of the Moon's observed altitudes. Therefore any of the sides SD or Sc may be computed. It is found to be nearly 60 semidiameters of the earth.

308. By combining the observations of the Moon's place in the heavens with those of her apparent diameter, we discover that her orbit is nearly an ellipse, having the Earth in one focus, and having the longer axis to the shorter axis nearly as 91 to 89. The greatest and least distances are nearly in the proportion of 21 to 19.

309. Her motion in this ellipse is such, that the line joining the Earth and Moon describes areas which are nearly proportional to the times. For her angular hourly motion is observed to be as the square of her apparent diameter.

310. The line of the apfides has a slow motion eastward, completing a revolution in about $3232^d 11^h 14' 30''$, nearly 9 years.

311. While the Moon is thus making a revolution round the heavens, her appearance undergoes great changes. She is sometimes on our meridian at midnight, and, therefore, in the part of the heavens which is opposite

posite to the Sun. In this situation, she is a complete luminous circle, and is said to be FULL. As she moves eastward, she becomes deficient on the west side, and, after about $7\frac{1}{3}$ days, comes to the meridian about six in the morning, having the appearance of a semicircle, with the convex side next the Sun. In this state, her appearance is called HALF MOON. Moving still eastward, she becomes more deficient on the west side, and has now the form of a crescent, with the convex side turned towards the Sun. This crescent becomes continually more slender, till, about 14 days after being full, she is so near the Sun that she cannot be seen, on account of his great splendour. About four days after this disappearance in the morning before sunrise, she is seen in the evening, a little to the eastward of the Sun, in the form of a fine crescent, with the convex side turned toward the Sun. Moving still to the eastward, the crescent becomes more full, and when the Moon comes to the meridian about six in the evening, she has again the appearance of a bright semicircle. Advancing still to the eastward, she becomes fuller on the east side, and, at last, after about $29\frac{1}{2}$ days, she is again opposite to the Sun, and again full.

312. It frequently happens that the Moon is ECLIPSED when full; and that the Sun is eclipsed some time between the disappearance of the Moon in the morning on the west side of the Sun, and her reappearance in the evening on the east side of the Sun. This eclipse of the
Sun

Sun happens at the very time that the Moon, in the course of her revolution, passes that part of the heavens where the Sun is.

313. From these observations, we conclude, 1. That the Moon is an opaque body, visible only by means of the Sun's light illuminating her surface; 2. That her orbit round the Earth is nearer than the Sun's.

314. From these principles all her PHASES, or appearances, may be explained (fig. 39.)

315. When the Moon comes to the meridian at mid-day, she is said to be NEW, and to be in CONJUNCTION with the Sun. When she comes to the meridian at midnight, she is said to be in OPPOSITION. The line joining these two situations is called the line of the SYZIGIES. The points where she is half illuminated are called the QUADRATURES; and that is called the first quadrature which happens after new moon.

316. When the Moon is half illuminated, the line EM (fig. 39.) joining the Earth and Moon, is perpendicular to the line MS, joining the Moon and Sun. By observing the angle SEM, the proportion of the distance of the Sun to the distance of the Moon may be ascertained.

This method of ascertaining the Sun's distance was proposed by Aristarchus of Samos, about 264 years before the Christian æra. The thought was extremely ingenious,

genious, and strictly just; and this was the first observation that gave the astronomers any confident guesses at the very great distance of the Sun. But it is impossible to judge of the half illumination of the Moon's disk with sufficient accuracy for obtaining any tolerable measure. Even now, when assisted by telescopes, we cannot tell to a few minutes when the boundary between light and darkness in the Moon is exactly a straight line. When this really happens, the elongation SEM wants but $9'$ of a right angle, and when it is altogether a right angle, there is no sensible change in the appearance of the Moon. All that the ancient astronomers could infer from their best estimation of the bisection of the Moon was, that the Sun was, for certain, at a much greater distance than any person had supposed before that time. Aristarchus said, that the angle SEM was not less than 87 degrees, and therefore the Sun was at least twenty times farther off than the Moon. But astronomers of the Alexandrian school said, that the angle SEM exceeded 89° , and the Sun was sixty times more remote than the Moon. Modern observations shew him to be near four hundred times more remote.

317. This succession of phases is completed in a period of $29^d 12^h 44' 3''$, called a SYNODICAL MONTH and a LUNATION.

It may be asked here, how the period of a lunation comes to differ from that of the Moon's revolution round the Earth, which is accomplished in $27^d 7^h 43' 12''$? This

is owing to the Sun's change of place during a revolution of the Moon. Suppose it new Moon, and therefore the Sun and Moon appearing in the same place of the heavens. At the end of the lunar period, the Moon is again in that point of the heavens. But the Sun, in the mean time, has advanced above 27 degrees; and somewhat more than two days must elapse before the Moon can overtake the Sun, so as to be seen by us as new moon.

318. The period of this succession of phases may be found within a few hours of the truth in a very short time. We can tell, within four or five hours, the time of the Moon being half illuminated. Suppose this observed in the morning of her last quarter. We shall see this twice repeated in 59 days, which gives $29\frac{1}{2}$ for a lunation, wanting about three fourths of an hour of the truth. About 433 years before the Christian æra, Meton, a Greek astronomer, reported to the states assembled at the Olympic games, that in nineteen years there happened exactly 235 lunations.

319. The lunar motions are subject to several irregularities, of which the following are the chief:

320. 1. The periodic month is greater when the Sun is in perigee than when in apogee, the greatest difference being about 24 minutes. Tycho Brahé first remarked this anomaly of the lunar motions, and called
the

the correction, (depending on the Sun's place in his orbit), the ANNUAL EQUATION of the Moon.

321. 2. The mean period is less than it was in ancient times.

322. 3. The orbit is larger when the Sun is in perigee than when he is in apogee.

323. 4. The orbit is more eccentric when the Sun is in the line of the lunar apses; and the equation of the orbit is then increased nearly $1^{\circ} 20' 34''$. This change is called the EJECTION. It was discovered by Ptolemy.

324. 5. The inclination of the orbit changes.

325. 6. The moon's motion is retarded in the first and third quarters, and accelerated in the second and last. This anomaly was discovered by Tycho Brahe, who calls it the VARIATION.

326. 7. The motion of the nodes is very unequal.

Of the Calendar.

327. Astronomy, like all other sciences, was first practised as an art. The chief object of this art was to know the seasons, which, as we have seen, depend either

immediately, or more remotely, on the Sun's motion in the ecliptic. A ready method for knowing the season seems, in all ages, to have been the chief incitement to the study of astronomy. This must direct the labours of the field, the migrations of the shepherd, and the journies of the traveller. It is equally necessary for appointing all public meetings, and for recording events.

Were the stars visible in the day time, it would be easy to mark all the portions of the year by the Sun's place among them. When he is on the foot of Castor, it is midsummer; and midwinter, when he is on the bow of Sagittarius. But this cannot be done, because his splendour eclipses them all.

328. The best approximation which a rude people can make to this, is to mark the days in which the stars of the zodiac come first in sight in the morning, in the eastern horizon, immediately before the Sun rise. As he gradually travels eastward along the ecliptic, the brighter stars which rise about three quarters of an hour before the Sun, may be seen in succession. The husbandman and the shepherd were thus warned of the succeeding tasks by the appearance of certain stars before the Sun. Thus, in Egypt, the day was proclaimed in which the Dogstar was first seen by those set to watch. The inhabitants immediately began to gather home their wandering flocks and herds, and prepare themselves for the inundation of the Nile in twelve or fourteen days. Hence that star was called the *Watch-dog*, ΤΗΟΤΗ, the Guardian of Egypt.

This

This was therefore a natural commencement of the period of seasons in Egypt; and the interval between the successive apparitions of Thoth, has been called the NATURAL year of that country, to distinguish it from the civil or artificial year, by which all records were kept, but which had little or no alliance with the seasons. It has also been called the *Canicular* year. It evidently depends on the Sun's situation and distance from the Dog-star, and must therefore have the same period with the Sun's revolution from a star to the same star again. This requires $365^{\text{d}} 6^{\text{h}} 9' 11''$, and differs from our period of seasons. Hence we must conclude that the rising of the Dog-star is not an infallible presage of the inundation, but will be found faulty after a long course of ages. At present it happens about the 12th or 11th of July.

This observation of a star's first appearance in the year, by getting out of the dazzling blaze of the Sun, is called the *heliacal rising* of the star. The ancient almanacks for directing the rural labours were obliged to give the detail of these in succession, and of the corresponding labours. Hesiod, the oldest poet of the Greeks, has given a very minute detail of those heliacal risings, ornamented by a pleasing description of the successive occupations of rural life. This evidently required a very considerable knowledge of the starry heavens, and of the chief circumstances of diurnal motion, and particularly the number of days intervening between the first appearance of the different constellations.

Such an almanack, however, cannot be expected, except among a somewhat cultivated people, as it requires a long continued observation of the revolution of the heavens in order to form it; and it must, even among such people, be uncertain. Cloudy, or even hazy weather, may prevent us for a fortnight from seeing the stars we want.

329. The Moon comes most opportunely to the aid of simple nations, for giving the inhabitants an easy division and measure of time. The changes in her appearance are so remarkable, and so distinct, that they cannot be confounded. Accordingly, we find that all nations have made use of the lunar phases to reckon by, and for appointing all public meetings. The festivals and sacred ceremonies of simple nations were not all dictated by superstition; but they served to fix those divisions of time in the memory, and thus gave a comprehensive notion of the year. All these festivals were celebrated at particular phases of the Moon—generally at new and full Moon. Men were appointed to watch her first appearance in the evening, after having been seen in the morning, rising a few minutes before the Sun. This was done in consecrated groves, and in high places; and her appearance was *proclaimed*. Fourteen days after, the festival was generally held *during full Moon*. Hence it is that the first day of a Roman month was named *KALENDÆ*, the day *to be proclaimed*. They said *pridie, tertio, quarto, &c. ante calendas neomenias Martias*; the third, fourth, &c. before

before proclaiming the new Moon of March. And the assemblage of months, with the arrangement of all the festivals and sacrifices, was called a KALENDARIVM.

As superstition overran all rude nations, no meeting was held without sacrifices and other religious ceremonies—the watching and proclaiming was naturally committed to the priests—the kalendar became a sacred thing, connected with the worship of the gods—and, long before any moderate knowledge of the celestial motions had been acquired, every day of every Moon had its particular sanctity, and its appropriated ceremonies, which could not be transferred to any other.

330. But as yet there seemed no precise distinction of months, nor of what number of months should be assembled into one group. Most nations seem to have observed that, after 12 Moons were completed, the season was pretty much the same as at the beginning. This was probably thought exact enough. Accordingly, in most ancient nations, we find a year of 354 days. But a few returns of the winter's cold, when they expected heat, would shew that this conjecture was far from being correct; and now began the embarrassment. There was no difficulty in determining the period of the seasons exactly enough, by means of very obvious observations. Almost any cottager has observed that, on the approach of winter, the Sun rises more to the right hand, and sets more to the left every day, the places of his rising and setting coming continually nearer to each other; and that,

that, after rising for two or three days from behind the same object, the places of rising and setting again gradually separate from each other. By such familiar observations, the experience of an ordinary life is sufficient for determining the period of the seasons with abundant accuracy. The difficulty was to accomplish the reconciliation of this period with the sacred cycle of months, each day of which was consecrated to a particular deity, jealous of his honours. Thus the Hierophantic science, and the whole art of kalendar-making, were necessarily entrusted to the priests. We see this in the history of all nations, Jews, Pagans, and Christians.

331. Various have been the contrivances of different nations. The Egyptians, and some of the neighbouring Orientals, seem early to have known that the period of seasons considerably exceeded 12 months, and contained 365 days. They made the civil year consist of 12 months of 30 days, and added 5 complementary days without ceremonies; and when more experience convinced them that the year contained a fraction of a day more, they made no change, but made the people believe that it was an improvement on their kalendar that their great day, the first of *Thoth*, by falling back one day in four periods of seasons, would thus occupy in succession every day of the year, and thus sanctify the whole in 1461 years, as they imagined, but really in 1425 of their civil years. We have but a very imperfect knowledge of the arrangement of their festivals. Indeed they were totally different in almost every city.

It is important to the astronomer to know this method of reckoning; because all the observations of Hipparchus and Ptolemy, and all those which they have quoted from the Chaldeans, Persians, &c. are recorded by it. In An. Dom. 940, the first day of Thoth fell on the first of January, and another Egyptian year commenced on the 31st of December of that year. From this datum it is easy to reckon back by years of 365 days, and to say on what day of what month of any of our years the 1st day of Thoth falls, and this wandering year commences.

332. The Greeks have been much more puzzled with the formation of a lunifolar year than the Egyptians. Solon got an oracle to direct his Athenians (594 years before our æra), *θεῖν κατα τρια, κατα Ἴλιον, κατα Σεληνην, και κατα ἡμερας*. The meaning of which seems to be, to regulate their year by the Sun, or seasons, their months by the Moon, and their festivals by the days. Observing that 59 days made two months, he made these alternately of 30 and of 29 days, *πλειαι*, and *κοιλαι*, full, and deficient; and the 30th day of a month, the *τριακτις*, was called *ἐνη και νεα, νεομηνια*, as it belonged to both months.

But this was not sufficiently accurate; and the Olympic games, celebrated on every fourth year, during the full Moon nearest to midsummer day, had gone into great confusion. The Hierophants, whose proclamation to all the states assembled the chiefs together, had not
skill

skill enough to keep them from gradually falling into the autumn months. Injudicious corrections were made from time to time, by rules for inserting months to bring things to rights again. It deserves to be remarked here, that this is the way in which the ancient astronomy improved, before the establishment of the Alexandrian school. It was not by a more accurate observation of the motions, as in modern times, but by discovering the errors, when they amounted to an unit of the scale on which they were measured. The astronomers then improved their future computations by repeatedly cutting off this unit of accumulated error.

333. All these contrivances were publicly proposed at the meeting of the States for the Olympic Games. This was an occasion peculiarly proper, and here the scheme of Meton was received with just applause. For Meton not only gave his countrymen a very exact determination of the lunar month, but accompanied it with a scheme of intercalation, by which all their festivals, religious and civil, were arranged so as to have very small dislocations from the days of new and full Moon. As this had hitherto been a matter of insuperable difficulty, Meton was declared victor in the first department, a statue was decreed him, and his arrangement of the festivals was inscribed on a pillar of marble, in letters of gold. This has occasioned the number expressing the current year of the cycle of 19 years (called the Metonic cycle) to be called the Golden Number. This scheme of Meton's

ton's was indeed very judicious, though intricate, because he arranged the interpolation of a month so as never to remove the first day of the month two days from the time of new Moon, whereas it had often been a week.

The Metonic cycle commenced on 16. July, 433 years before the beginning of the Christian æra, at 43 minutes past 7 in the morning, that being the time of new Moon. The first year of each cycle is that in which the full Moon of its first month is the nearest to the summer solstice.

334. The Roman kalendar was in a much worse condition than the rudest of the Greeks. The superstitious veneration for their ceremonies, or their passion for public sports, had diverted the attention of the Romans (who never were cultivators or graziers) from the seasons altogether. They were contented with a year of ten months for several centuries, and had the most absurd contrivances for producing some conformity with the seasons. At last, that accomplished general, Julius Cæsar, having attained the height of his vast ambition, resolved to reform the Roman kalendar. He was profoundly skilled in astronomy, and had written some dissertations on different branches of the science, which had great reputation, but are now lost. He had no superstitious or religious qualms to disturb him, and was determined to make every thing yield to the great purpose of a kalendar, its use in directing the occupations of the people, and for recording the events of history. He took the

help of Sosigenes, an astronomer of the Alexandrian school, a man perfectly acquainted with all the discoveries of Hipparchus and others of that celebrated academy.

These eminent scholars, knowing that the period of seasons occupied 365 days and a quarter very nearly, made a short cycle of 4 years, containing three years of 365, and one of 366 days; thus cutting off, in the Grecian manner, the error, when it amounted to a whole day. Cæsar resolved also to change the beginning of the year from March, where Romulus had placed it in honour of his patron Mars, to the winter solstice. This is certainly the most natural way of estimating the commencement of the year of seasons. What we are most anxious to ascertain is the precise day when the Sun, after having withdrawn his cheering beams, and exposed us to the uncomfortable cold and storms of winter, begins to turn toward us, and to bring back the pleasures of spring, and by his genial warmth to give us the hopes of another season of productive fertility.* Cæsar therefore chose

* In almost all nations this season is distinguished by festivities of various kinds. Many of these were incorporated with the religious ceremonies of the Christian Church by our ecclesiastics, because they saw that the people were too much wedded to them, to relinquish them with good humour. Among ourselves, there are pretty evident traces of druidical superstition. We know that, in ancient times, the chief druid, attended by crowds of the people, went into the woods in the night of the winter solstice, and with a golden sickle cut a branch

chose for the beginning of his kalendar, a year in which there was a new Moon following close upon the winter solstice. This opportunity was afforded him in the second year of his dictatorship, and the 707th year from the foundation of Rome. He found that there would be a new Moon 6 days after the winter solstice. He made this new Moon the 1st of January of his first year. But, to do this, he was obliged to keep the preceding year dragging on 90 days longer than usual, containing 444

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days,

branch of the miselto of the oak, called Ghiah in Celtic, and carried it in triumph to the sacred grove. The people cut for themselves, and carried home their prize, consecrated by the druid. At present, the pews of our churches, and even the chambers of our cottages, are ornamented with this plant at Christmas. In France, till within these 150 years, there were still more perceptible traces. A man personating a prince (*Roi follet*) set out from the village into the woods, bawling out, *Au Gui menez—le Roi le veut*. The monks followed in the rear with their begging-boxes called *tire-liri*. They rattled them, crying *tire-liri*; and the people put money into them, under the fiction that it was for a lady in labour. People in disguise (*Guifards*) forced into the houses, playing antic tricks, and bullied the indwellers for money, and for choice victuals, crying *tire-liri—tire-liri—maint du blanc, et point du bis*. They made such riots, that the Bishop of Soissons represented the enormities to Louis XIV., and the practice was forbidden. May not the guifearths of Edinburgh, with their cry of “Hog menay, troll lollay; gie’s your white bread, none of your gray,” be derived from this?

days, instead of the old number 354. As all these days were unprovided with solemnities, the year preceding Cæsar's kalendar was called *the year of confusion*. Cæsar also, for a particular reason, chose to make his first year consist of 366 days, and he inserted the intercalary day between the 23d and 24th of February, choosing that particular day, as a separation of the lustrations and other piaculums to the infernal deities, which ended with the 23d, from the worship of the celestial deities, which took place on the 24th of February. The 24th was the *sextus ante kalendas neomenias Martias*. His inserted day, answering civil purposes alone, had no ceremonies, nor any name appropriated to it, and was to be considered merely as a supernumerary *sextus ante kalendas*. Hence the year which had this intercalation was styled an *annus bissextilis*, a bissextile year. With respect to the rest of the year, Cæsar being also Pontifex Maximus (an office of vast political importance), or rather, having all the power of the state in his own person, ordered that attention should be given to the days of the month only, and that the religious festivals alone should be regulated by the sacred college. He assigned to each month the number of days which has been continued in them ever since.

335. Such is the simple kalendar of Julius Cæsar. Simple however as it was, his instructions were misunderstood, or not attended to, during the horrors of the civil wars. Instead of intercalating every fourth year, the intercalation was thrice made on every succeeding
third

third year. The mistake was discovered by Augustus, and corrected in the best manner possible, by omitting three intercalations during the next twelve years. Since that time, the kalendar has been continued without interruption over all Europe till 1582. The years, consisting of $365\frac{1}{4}$ days, were called *Julian years*; and it was ordered, by an edict of Augustus, that this kalendar shall be used through the whole empire, and that the years shall be reckoned by the reigns of the different emperors. This edict was but imperfectly executed in the distant provinces, where the native princes were allowed to hold a vassal sovereignty. In Egypt particularly, although the court obeyed the edict, the people followed their former kalendars and epochs. Ptolemy the astronomer retains the reckoning of Hipparchus, by Egyptian years, reckoned from the death of Alexander the Great. We must understand all these modes of computation, in order to make use of the ancient astronomical observations. A comparison of the different epochs will be given as we finish the subject.

336. The æra adopted by the Roman Empire when Christianity became the religion of the state, was not finally settled till a good while after Constantine. Dionysius Exiguus, a French monk, after consulting all proper documents, considers the 25th of December of the forty-fifth year of Julius Cæsar as the day of our Saviour's nativity. The 1st of January of the forty-sixth year of Cæsar is therefore the beginning of the æra now used

used by the Christian world. Any event happening in this year is dated *anno Domini primo*. As Cæsar had made his first year a bissextile, the year of the nativity was also bissextile; and the first year of our æra begins the short cycle of four years, so that the fourth year of our æra is bissextile.

That we may connect this æra with all the others employed by astronomers or historians, it will be enough to know that this first year of the Christian æra is the 4714th of the Julian period.

It coincides with the fourth year of the 194th Olympiad till midsummer.

It coincides with the 753d *ab urbe condita*, till April 21st.

It coincides with the 748th of Nabonassar till August 23d.

It coincides with the 324th civil year of Egypt, reckoned from the death of Alexander the Great.

In the arrangement of epochs in the astronomical tables, the years before the Christian æra are counted backwards, calling the year of the nativity 0, the preceding year 1, &c. But chronologists more frequently reckon the year of the nativity the first before Christ. Thus,

Years of Cæsar . . .	41,	42,	43,	44,	45,	46,	47,	48,	49
Astronomers	4,	3,	2,	1,	0,	1,	2,	3,	4
Chronologists	5,	4,	3,	2,	1,	1,	2,	3,	4

This kalendar of Julius Cæsar has manifest advantages in respect of simplicity, and in a short time sup-
planted

planted all others among the western nations. Many other nations had perceived that the year of seasons contained more than 365 days, but had not fallen on easy methods of making the correction. It is a very remarkable fact, that the Mexicans, when discovered by the Spaniards, employed a cycle which supposed that the year contained $365\frac{1}{4}$ days. For, at the end of fifty-two years, they add thirteen days, which is equivalent to adding one every fourth year. In their hieroglyphical annals, their years are grouped into parcels of four, each of which has a particular mark.

337. But although the Julian construction of the civil year greatly excelled all that had gone before, it was not perfect, because it contained $11' 14\frac{1}{2}''$ more than the period of seasons. This, in 128 years, amounts exactly to a day. In 1582, it amounted to $12^d 7^h$. The equinoxes and solstices no longer happened on those days of the month that were intended for them. The celebration of the church festivals was altogether deranged. For it must now be remarked, that there occurred the same embarrassment on account of the lunar months, as formerly in the Pagan world.

The Council of Nice had decreed that the great festival, Easter, should be celebrated in conformity with the Jewish passover, which was regulated by the new moon following the vernal equinox. All the principal festivals are regulated by Easter Sunday. But by the deviation of the Julian kalendar from the seasons, and the words
of

of the decree of the Nicene Council, the celebration of Easter lost all connexion with the Passover. For the decree did not say, 'The first Sunday after the full moon following the vernal equinox, but the first Sunday after the full moon following the 21st of March.' It frequently happened that Easter and the Passover were six weeks apart. This was corrected by Pope Gregory the XIII. in 1582, by bringing the 21st of March to the equinox again. He first cut off the ten days which had accumulated since the Council of Nice; and, to prevent this accumulation, he directed the intercalation of a bissextile to be omitted on every centurial year. But the error of a Julian century containing 36525 days, is not a whole day, but $18^h 40'$. Therefore the correction introduces an error of $5^h 20'$. To prevent this from accumulating, the omission of the centurial intercalation is limited to the centuries not divisible by four. Therefore 1600, 2000, 2400, &c. are still bissextile years; but 1700, 1800, 1900, 2100, 2200, &c. are common years. There still remains an error, amounting to a day in 144 centuries.

The kalendar is now sufficiently accurate for all purposes of history and record—and even for astronomy, because the tropical year of seasons is subject to a periodical inequality.

338. A correction, much more accurate than the Gregorian, occurred to Omar, a Persian astronomer at the court of Prince Gelala Eddin Melek Schah. Omar
proposed

propofed always to delay to the thirty-third year the intercalation which fhould have been made in the thirty-second. This is equivalent to omitting the Julian intercalation altogether on the 128th year. This method is extremely fimple, and fcrupuloufly accurate. For the error of 11' 15" of the Julian year amounts precifely to a day in 128 years. It differs from the truth only one minute in 120 years. This correction took place in A° Di 1079, at the fame time that the Arab Alhazen was reforming the fcience of aftronomy in Spain.

The Gregorian kalendar, however, has lefs chance of being forgotten or miftaken. Centurial years are remarkable, and call the attention, even by the unufual found of the words. The thirty-second year has nothing remarkable, and may be overlooked.

339. It now appears that certain attentions are neceffary for avoiding miftakes, when we would appeal to very diftant obfervations. We muft know the accurate interval, however large. Although one hundred Julian years contain 36525 days, we muft keep in mind that between 1500 and 1600 ten days are wanting; and that each of the centuries 1700 and 1800 alfo want a day. The interval from the beginning of our æra and A. D. 1582 needs no attention; but that between 1505 and 1805 wants twelve days of three Julian centuries.

340. We muft alfo be careful, in ufing the ancient obfervations, to connect the years of our Lord with the

years before Christ in a proper manner. An eclipse mentioned by an astronomer as having happened on the 1st of February anno 3tio A. C. must be considered as happening in the forty-second year of Julius Cæsar. But if the same thing is mentioned by a historian or chronologist, it is much more probable that it was in the forty-third year of Cæsar. It was chiefly to prevent all ambiguities of this kind that Scaliger contrived what he called the *Julian period*. This is a number made by multiplying together the numbers called the *Lunar or Metonic cycle*, the *solar cycle*, and the *indiction*. The lunar cycle is 19, and the first year of our Lord was the second of this cycle. The solar cycle is 28, being the number of years in which the days of the month return to the same days of the week. As the year contains fifty-two weeks and one day, the first day of the year (or any day of any month) falls back in the week one day every year, till interrupted by the intercalation in a bissextile year. This makes it fall back two days in that year; and therefore it will not return to the same day till after four times seven, or twenty-eight years. The first year of our Lord was the tenth of this cycle. The **INDICTION** is a cycle of fifteen years, at the beginning of which a tax was levied over the Roman Empire. It took place A. D. 312; and if reckoned backward, it would have begun three years before the Christian æra. The year of this cycle for any year of the Christian æra, will therefore be had by adding three to the year, and dividing by fifteen. The product of these three numbers

bers is 7980; and it is plain that this number of years must elapse before a year can have the same place in all the three cycles. If therefore we know the place of these cycles belonging to any year, we can tell what year it is of the Julian period.

The first year of our æra was the second of the lunar cycle, the tenth of the solar, and the fourth of indication, and the 4714th of the Julian period. By this we may arrange all the remarkable æras as follows.

	J. P.	☉	☾	I.	A. C.
Æra of the Olympiads . . .	3938	18	5	8	775,776
Foundation of Rome . . .	3961	13	9	1	752,753
Nabonassar	3967	19	15	7	746,747
Death of Alexander . . .	4390				323,324
First of Julius Cæsar . . .	4669	21	14	4	44, 45
A. Dom. 1.	4714	10	2	4	

341. Did the Metonic cycle of the Moon correspond exactly with our year, it would mark for any year the number of years which have elapsed since it was new moon on the 1st of January. But its want of perfect accuracy, the vicinity of an intercalation, and the lunar equations, sometimes cause an error of two days. It is much used, however, for ordinary calculations for the Church holidays. To find the golden number, add one to the year of our Lord, divide the sum by 19, the remainder is the golden number. If there be no remainder, the golden number is 19.

342. Another number, called Epact, is also used for facilitating the calculation of new and full moon in a gross way. The epact is nearly the moon's age on the 1st of January. To find it, multiply the golden number by 11, add 19 to the product, and divide by 30. The remainder is the epact.

Knowing, by the epact, the Moon's age on the 1st of January, and the day of the year corresponding to any day of a month, it is easy to find the Moon's age on that day, by dividing the double of the sum of this number and the epact by 59. The half remainder is nearly the Moon's age.

Although these rude computations do not correspond with the motions of the two luminaries, they deserve notice, being the methods employed by the rules of the Church for settling the moveable Church festivals.

Of the proper Motions of the Planets.

343. The planets are observed to change their situations in the starry heavens, and move among the signs of the zodiac, never receding far from the ecliptic.

Their motions are exceedingly irregular, as may be seen by fig. 65. A, which represents the motion of the planet Jupiter, from the beginning of 1708 to the beginning of 1716. EK represents the ecliptic, and the initial letters of the months are put to those points of the apparent path where the planet was seen on the first day of each month.

It

It appears that, on the 1st of January 1708, the planet was moving slowly eastward, and became stationary about the middle of the month, in the second degree of Libra. It then turned westward, gradually increasing its westerly motion, till about the middle of March, when it was in opposition to the Sun, at R, all the while deviating farther from the ecliptic toward the north. It now slackened its westerly motion every day, and was again stationary about the 20th of May, in the twenty-second degree of Virgo, and had come nearer to the ecliptic. Jupiter now moved eastward, nearly parallel to the ecliptic, gradually accelerating in his motion, till the beginning of October, when he was in conjunction with the Sun at D, about the eleventh degree of Libra. He now slackened his progressive motion every day, till he was again stationary, in the second degree of Scorpio, on the 12th or 13th of February 1709. He then moved westward, was again in opposition, in the twenty-seventh degree of Libra, about the middle of April. He became stationary, about the end of June, in the twenty-first degree of Libra; and from this place he again proceeded eastward; was in conjunction about the beginning of November, very near the star in the southern scale of Libra; and, on the 1st of January 1710, he was in the twenty-fourth degree of Scorpio.

This figure will very nearly correspond with the apparent motions of the planet in the same months of 1803 and 1804. Jupiter will go on in this manner, forming a loop in his path in every thirteenth month; and he is

in

in opposition to the Sun, when in the middle of each loop. His regress in each loop is about 10 degrees, and his progressive motion is continued about 40° . He gradually approaches the ecliptic, crosses it, deviates to the southward, then returns towards it; crosses it, about six years after his former crossing, and in about twelve years comes to where he was at the beginning of these observations.

344. The other planets, and particularly Venus and Mercury, are still more irregular in their apparent motions, and have but few circumstances of general resemblance.

The first *general* remark which can be made on these intricate motions is, that a planet always appears largest when in the points R, R, R, which are in the middle of its retrograde motions. Its diameter gradually diminishes, and becomes the least of all when in the points D', D', D', which are in the middle of its direct motions. Hence we infer that the planet is nearest to the Earth when in the middle of its retrograde motion, and farthest from it when in the middle of its direct motion.

It may also be remarked, that a planet is always in conjunction with the Sun, or comes to our meridian at noon, when in the middle of its direct motions. The planets Venus and Mercury are also in conjunction with the Sun when in the middle of their retrograde motions. But the planets Mars, Jupiter, and Saturn, are always in opposition to the Sun, or come to our meridian at
midnight,

midnight, when in the middle of their retrograde motions. Their situations also, when stationary, are always similar, relative to the Sun. These appearances in all the planetary motions have therefore an evident relation to the Sun's place.

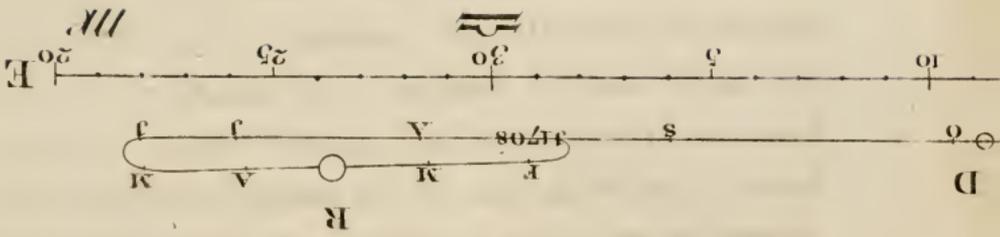
345. The ancient astronomers were of opinion that the perfection of nature required all motions to be uniform, as far as the purpose in view would permit. The planetary motions must therefore be uniform, in a figure that is uniform; and the astronomers maintained that the observed irregularities were only apparent. Their method for reconciling these with their principle of perfection is very obviously suggested by the representation here given of the motion of Jupiter. They taught that the planet moves uniformly in the circumference of a circle qrs (fig. 40.) in a year, while the centre Q of this circle is carried uniformly round the Earth T , in the circumference of another circle QAL . The circle QAL is called the DEFERENT CIRCLE, and qrs is called the EPICYCLE. They explained the deviation from the ecliptic, by saying that the deferent and the epicycle were in planes different from that of the ecliptic. By various trials of different proportions of the deferent and the epicycle, they hit on such dimensions as produced the quantity of retrograde motion that was observed to be combined with the general progress in the order of the signs of the zodiac.—But another inequality was observed. The arch of the heavens intercepted between two successive

ſucceſſive oppoſitions of Jupiter, (for example), was obſerved to be variable, being always leſs in a certain part of the zodiac, and gradually increaſing to a maximum ſtate in the oppoſite part of the zodiac.

In order to correſpond with this SECOND INEQUALITY, as it was called, and yet not to imply any inequality of the motion of the epicycle in the circumference of the deferent circle, the aſtronomers placed the Earth not in, but at a certain diſtance from, the centre of the deferent; ſo that an equal arch between two ſucceeding oppoſitions ſhould ſubtend a ſmaller angle, when it is on the other ſide of that centre. Thus, the unequal motion of the epicycle was explained in the ſame way as the Sun's unequal motion in his annual orbit. The line drawn through the Earth and the centre of the deferent is called the line of the planet's APSIDES, and its extremities are called the *apogee* and *perigee* of the deferent as in the caſe of the Sun's orbit (292.) In this manner, they at laſt compoſed a ſet of motions which agreed tolerably well with obſervation.

The celebrated geometer Apollonius gave very judicious directions how to proportion the epicycle to the deferent circle. But they ſeem not to have been attended to, even by Ptolemy; and the aſtronomers remained very ignorant of any method of conſtruction which agreed ſufficiently with the phenomena, till about the thirteenth century, when the doctrine of epicycles was cultivated with more care and ſkill.

A very full and diſtinct account is given of all the ingenious contrivances of the ancient aſtronomers for
explaining



explaining the irregularities of the celestial motions, in the first part of Dr Small's History of the Discoveries of Kepler, published in 1803.

Of the Motions of Venus and Mercury.

346. Venus has been sometimes seen moving across the Sun's disk from east to west, in the form of a round black spot, with an apparent diameter of about 59". A few days after this has been observed, Venus is seen in the morning, rising a little before the Sun, in the form of a fine crescent, with the convexity turned toward the Sun. She moves gradually westward, separating from the Sun, with a retarded motion, and the crescent becomes more full. In about ten weeks, she has moved 46° west of the Sun, and is now a semicircle, and her diameter is 26". She now separates no farther from the Sun, but moves eastward, with a motion gradually accelerated, and she gradually diminishes in apparent diameter. She overtakes the Sun, about 9½ months after having been seen on his disk. Some time after, Venus is seen in the evening, east of the Sun, round, but very small. She moves eastward, and increases in apparent diameter, but loses of her roundness, till she gets about 46° east of the Sun, when she is again a semicircle, having the convexity toward the Sun. She now moves westward, increasing in diameter, but becoming a crescent, like the waning Moon; and, at last, after a period of nearly 584 days, comes again into conjunction with the Sun, with an apparent diameter of 59".

347. From these phenomena we conclude that the Sun is included within the orbit of Venus, and is not far from its centre, while the Earth is without this orbit. Therefore, while the Sun revolves round the Earth, Venus revolves round the Sun.

The time of the revolution of Venus round the Sun may be deduced from the interval which elapses between two or more conjunctions, by help of the following theorem :

348. Let two bodies A and B revolve uniformly in the same direction, and let a and b be their respective periods, of which b is the least, and t the interval between two successive conjunctions or oppositions.

$$\text{Then } b = \frac{at}{a+t}, \text{ and } a = \frac{bt}{t-b}.$$

For the angular motions are inversely proportional to the periodic times. Therefore the angular motions of A and B are as $\frac{1}{a}$ and $\frac{1}{b}$. And, since they move in the same direction, the synodical or relative motion is the difference of their angular motions. Therefore the fundamental equation is $\frac{1}{b} - \frac{1}{a} = \frac{1}{t}$. Hence $\frac{1}{b} = \frac{1}{t} + \frac{1}{a}$, $= \frac{a+t}{at}$, and $b = \frac{at}{a+t}$. Also $\frac{1}{a} = \frac{1}{b} - \frac{1}{t}$, $= \frac{t-b}{tb}$, and $a = \frac{bt}{t-b}$.

We may also calculate the synodical period t , when we know the real periods of each. For $\frac{1}{t} = \frac{1}{b} - \frac{1}{a} = \frac{a-b}{ab}$, and $t = \frac{ab}{a-b}$.

This

This gives for the periodic time of Venus round the Sun $224^d 16^h 49' 13''$.

349. But it is evident that if this angular motion is not uniform, the interval between two successive conjunctions may chance to give a false measure of the period. But, by observing many conjunctions, in various parts of the heavens, and by dividing the interval between the first and last by the number of intervals between each (taking care that the first and last shall be nearly in the same part of the heavens), it is evident that the inequalities being distributed among them all, the quotient may be taken as nearly an exact medium. Hence arises the great value of ancient observations. In eight years we have five conjunctions of Venus, and she is only $1^\circ 32'$ short of the place of the first conjunction. The period deduced from the conjunctions in 1761 and 1769, scarcely differs from that deduced from the conjunctions in 1639 and 1761. But the other planets require more distant observations.

350. Venus does not move uniformly in her orbit. For, if the place of Venus in the heavens be observed in a great number of successive conjunctions with the Sun (at which time her place in the ecliptic, as seen from the Sun, is either the Sun's place, as seen from the Earth, or the opposite to it), we find that her changes of place are not proportional to the elapsed times. By observations of this kind, we learn the inequality of the angular

motion of Venus round the Sun, and hence can find the equations for every point of the orbit of Venus, and can thence deduce the position of Venus, as seen from the Sun, for any given instant.

This however requires more observations of this kind than we are yet possessed of, because her conjunctions happen so nearly in the same points of her orbit, that great part of it is left without observations of this kind. But we have other observations of almost equal value, namely, those of her greatest elongations from the Sun. There is none of the planets, therefore, of which the equations (which indeed are very small) are more accurately determined.

351. We can now determine the form and position of the orbit. For we can *observe* the place of the Sun, or the position of the line ES (fig. 41.), joining the Earth and Sun. We know the length of this line (291.) We can *observe* the GEOCENTRIC place of Venus, or the position of the line ED joining the Earth and Venus. And we can compute (350.) the HELIOCENTRIC place of Venus, or the position of the line SC joining Venus and the Sun. Venus must be in V, the intersection of these two lines; and therefore that point of her orbit is determined.

352. By such observations Kepler discovered that the orbit of Venus is an ellipse, having the Sun in one focus, the semitransverse axis being 72333, and the eccentricity

city 510, measured on a scale of which the Sun's mean distance from the Earth is 100000.

353. The upper apsis of the orbit is called the APHELION, and the lower apsis is called the PERIHELION of Venus.

354. The line of the apsides has a slow motion eastward, at the rate of $2^{\circ} 44' 46''$ in a century.

355. The orbit of Venus is inclined to the ecliptic at an angle of $3^{\circ} 20'$, and the nodes move westward about $31''$ in a year.

356. Venus moves in this orbit so as to describe round the Sun areas proportional to the times.

357. The planet Mercury resembles Venus in all the circumstances of her apparent motion; and we make similar inferences with respect to the real motions. His orbit is discovered to be an ellipse, having the Sun in one focus. The semitransverse axis is 38710, and the eccentricity 7960. The apsides move eastward $1^{\circ} 57' 20''$ in a century. The orbit is inclined to the ecliptic 7° . The nodes move westward $45''$ in a year. The periodic time is $87^{\text{d}} 23^{\text{h}} 15' 37''$; and areas are described proportional to the times.

Of the proper Motions of the Superior Planets.

358. Mars, Jupiter, and Saturn, exhibit phenomena considerably different from those exhibited by Mercury and Venus.

1. They come to our meridian both at noon and at midnight. When they come to our meridian at noon, and are in the ecliptic, they are never seen crossing the Sun's disk. Hence we infer, that their orbits include both the Sun and the Earth.

2. They are always retrograde when in opposition, and direct when in conjunction.

The planet Jupiter may serve as an example of the way in which their real motions may be investigated.

359. Jupiter is an opaque body, visible by means of the reflected light of the Sun. For the shadows of some of the heavenly bodies are sometimes observed on his disk, and his shadow frequently falls on them.

360. His apparent diameter, when in opposition, is about 46", and, when in conjunction, it is about 31", and his disk is always round. Hence we infer, that he is nearest when in opposition, and that his least and greatest distance are nearly as two to three. The Earth is, therefore, far removed from the centre of his motion; and, if we endeavour to explain his motion by means of a deferent circle

circle and an epicycle (), the radius of the deferent must be about five times the radius of the epicycle.

361. Since Jupiter is always retrograde when in opposition, and direct when in conjunction, his position, with respect to the centre of his epicycle, must be similar to the position of the Sun with respect to the Earth. His motion, therefore, in the epicycle, has a dependence on the motion of the Sun; and his motion, as seen from the Sun, must be simpler than as seen from the Earth.

His position, as seen from the Sun, may be accurately *observed* in every opposition and conjunction.

It was very natural for the ancient astronomers of Greece to infer, from what has been said just now, that the position of Jupiter, in respect of the centre of his epicycle, was the same as that of the Sun in respect of the Earth, not only in opposition and conjunction, but in every other situation. For, in twelve years, we see it to be so in the oppositions observed in 12 parts of the heavens, and in 83 years we see it in 76 parts. It is very improbable, therefore, that it should be otherwise in the intervals.

The motion of a superior planet may be explained upon these principles in the following manner :

Let T (fig. 40.) be the Earth, and $\alpha\beta\kappa\delta\varepsilon\varphi\gamma\chi\alpha$ be the Sun's orbit. Also, let A, B, C, D, E, F, G, H, I, be the places of the centre of the epicycle in the circumference of the deferent when the sun is in $\alpha, \beta, \kappa, \delta, \varepsilon, \varphi, \gamma, \chi, \alpha$, make Aa parallel to $T\alpha$, and Bb parallel to $T\beta$,

T β , and C c parallel to T α , &c., and make these lines of a length that is duly proportioned (by the Apollonian rule) to the radius T A of the deferent circle.

When the Sun is in α , β , γ , &c. the centre of the epicycle is in A, B, C, &c. and the planet is in a , b , c , &c.; and the dotted curve $a b c d e f g h a k$ is its path in absolute space between two succeeding oppositions to the Sun, viz. in a , and in k .

362. If we make the radius of Jupiter's deferent circle to that of the epicycle, as 52 to 10, the epicyclical motion arising from this construction will very nearly agree with the observation. Only we may observe that the oppositions which succeed each other near the constellation Virgo, are less distant from one another than those observed in the opposite part of the heavens; so that the centre of the epicycle seems to move slower in the first case than in the last. To reconcile this with the perfect uniformity of the motion of that centre in the circumference of the deferent circle, the ancient astronomers said that the earth was not exactly in the centre of the deferent, but so placed that the equable motion of the centre of the epicycle appeared slower, because it is then more remote; and after various trials, they fixed on a degree of eccentricity for the deferent, which accorded better than any other with the observations, and really differed very little from them. Copernicus shews that their hypothesis for Jupiter never deviates more than half a degree from observation, if it be properly employed. They found that the epicycle moved round the deferent

ferent in $4332\frac{1}{2}$ days, with an equation gradually increasing to near 6 degrees; so that if the place of the epicycle be calculated for a quarter of a revolution from the apogee, at the mean rate of 5' per day, it will be found too far advanced by near ten weeks motion.

363. But the ancient astronomers had no such data for determining the absolute magnitude of the deferent circles and epicycles for the superior planets, as Mercury and Venus afforded them. The rules given them by Apollonius only taught them what proportion the epicycle of each planet must have to its deferent circle, but gave no information as to the absolute magnitude of either, or the proportion between the deferent circles of any two superior planets. Accordingly, no two ancient astronomers agree in their measures, farther than in saying that Saturn is farther off than Jupiter, and Jupiter than Mars. This they inferred from their longer periods. All that they had to take care of was to make their sizes sufficiently different, so that the epicycles of two neighbouring planets should not cross and jumble each other. Yet they might easily have come very near the truth, by a small and very allowable addition to their hypothesis of epicyclical motion, namely, by supposing that the epicycle of each planet is equal to the Sun's orbit. This was quite allowable.

364. If we do this, we shall deduce consequences that are very remarkable, and which would have put the

ancient astronomy on a footing very near to perfection. For, if Cc (fig. 40.) be not only parallel to $T\alpha$, but also equal to it, then $CT\alpha c$ is a parallelogram, and αc is equal and parallel to TC . The bearing (to express it as a mariner) and distance of Jupiter from the Sun, is at all times the same with the bearing and distance of the centre of his epicycle from the Earth; and Jupiter is always found in an orbit round the Sun, equal and similar to the deferent orbit round the Earth. Thus, αa is equal to TA ; βb to TB ; αc to TC , &c. with respect to all the points of the looped curve. If the Earth be in the centre of the deferent, the distance of Jupiter from the Sun is always the same, and he may be said to describe a circle round the Sun, while the Sun moves round the Earth. Nay, it results from the equality of Aa to TA , of Bb to TB , &c., that whatever eccentricity, or whatever form it has been thought necessary to assign to the deferent, the distances αa , βb , αc , &c. will still be respectively equal to TA , TB , TC , &c. The circle which the astronomers called the deferent, because it is supposed to carry Jupiter's epicycle round the Earth, may be supposed to accompany the Sun, being carried round by him in a year, the line of its apsidæ (362.) keeping parallel to itself, that is, in our figure, to TA . And thus, the motion of Jupiter round the Sun will be incomparably more simple than the looped curve round the Earth; for it will be precisely the motion which was given by the astronomers to the centre of Jupiter's epicycle. The motion of Jupiter in
absolute

absolute space is indeed the same looped curve in both cases; but the way of conceiving it is much more simple.

365. This supposition of the equality of Jupiter's epicycle to the Sun's orbit, and the parallelism of Cc to Tx in every position of Jupiter, are fully verified by the modern discoveries of his satellites. These little planets revolve round him with perfect regularity, and their shadows frequently fall on his disk, and they are often obscured by his shadow. This shews the position of Jupiter's shadow at all times, and, consequently, Jupiter's position in respect of the Sun. This we find at all times to be parallel to the supposed position of the centre of his epicycle. Thus xc is found parallel to TC .

366. We now can tell the precise point in which Jupiter is found in any moment of time. Having made the radius Tx to the radius TA in the due proportion of 10 to 52, and having placed the Earth at the proper distance from the centre of the deferent QAL , we can calculate (298.) the position and length of the line Tx joining the Earth with the Sun. We can draw the line TC to the supposed centre of Jupiter's epicycle, having learned the law or equation of the supposed motion of that centre by our observation of his oppositions in all quarters of the ecliptic (362.), and we then draw xV parallel to it. This must pass through Jupiter, or Jupiter must be somewhere in this line. We observe Jupiter, however, in the direction TZ . Jupiter must there-

fore be in the intersection c of the lines ∞V and TZ . And then we can measure cx , Jupiter's distance from the Sun.

367. Kepler, by taking this method with a series of observations made by Tycho Brahé, discovered that Jupiter was always found in the circumference of an ellipse, having the Sun in its focus. Its semitransverse axis is 520098, the mean distance of the Earth from the Sun being supposed 100000. Its eccentricity is 25277. Its inclination to the ecliptic is $1^\circ 20'$, and the nodes move eastward about $1'$ in a year.

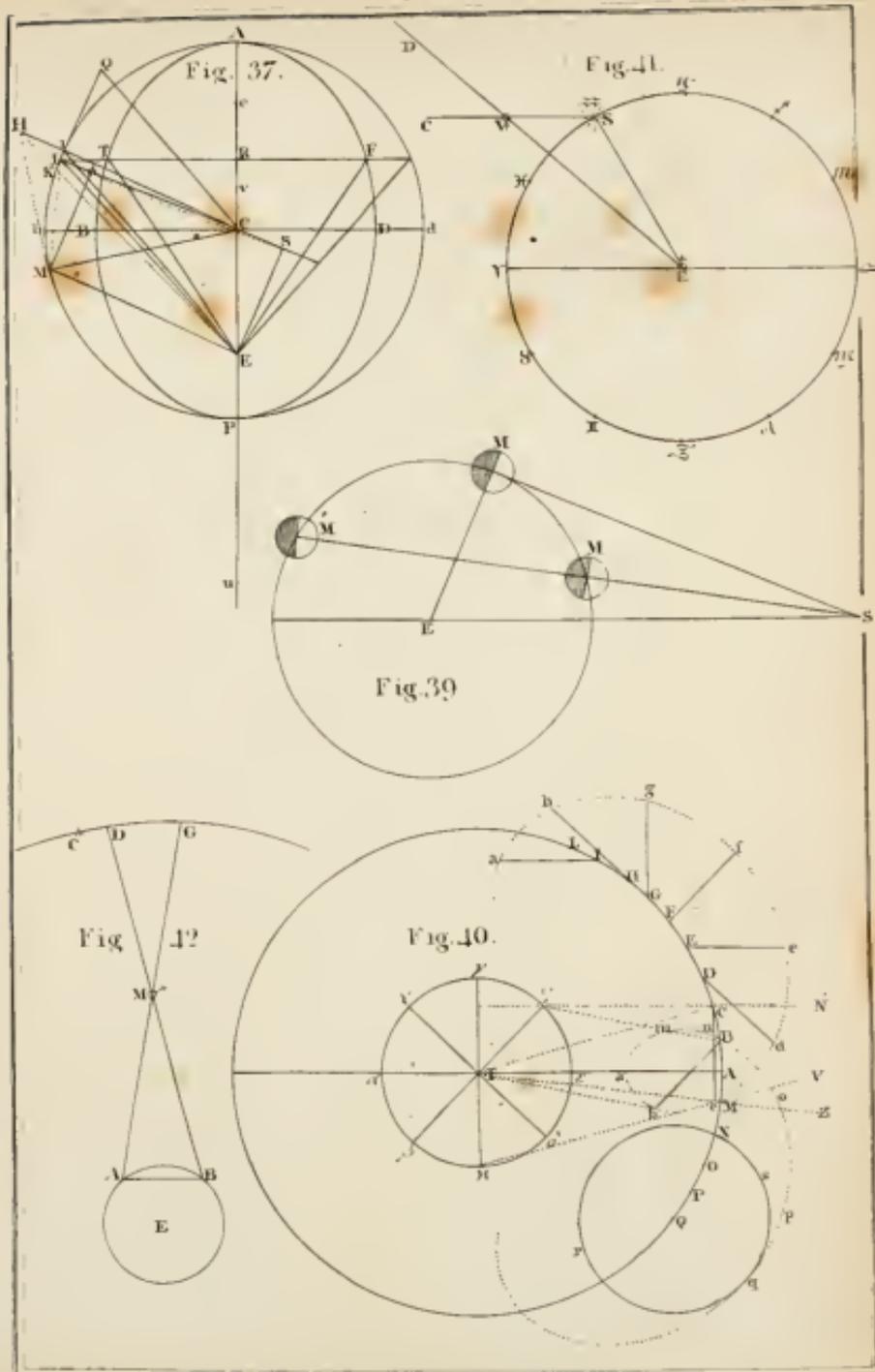
368. The revolution in this orbit is completed in $4332\frac{1}{3}$ days, and areas are described proportional to the times.

369. Proceeding in the same manner, we discover that the planets Mars, Saturn, and the one discovered by Dr Herschel in 1781, are always found in the circumference of ellipses, with the Sun in one focus, and describe round him areas proportional to the times.

The chief circumstances of their motions are stated as follows :

	<i>Mean Distance.</i>	<i>Eccentricity.</i>	<i>Period in Days.</i>
Georgian planet	1908584	90738	30456,07
Saturn - - - -	953941	53210	10759,27
Mars - - - -	152369	14218	686,98

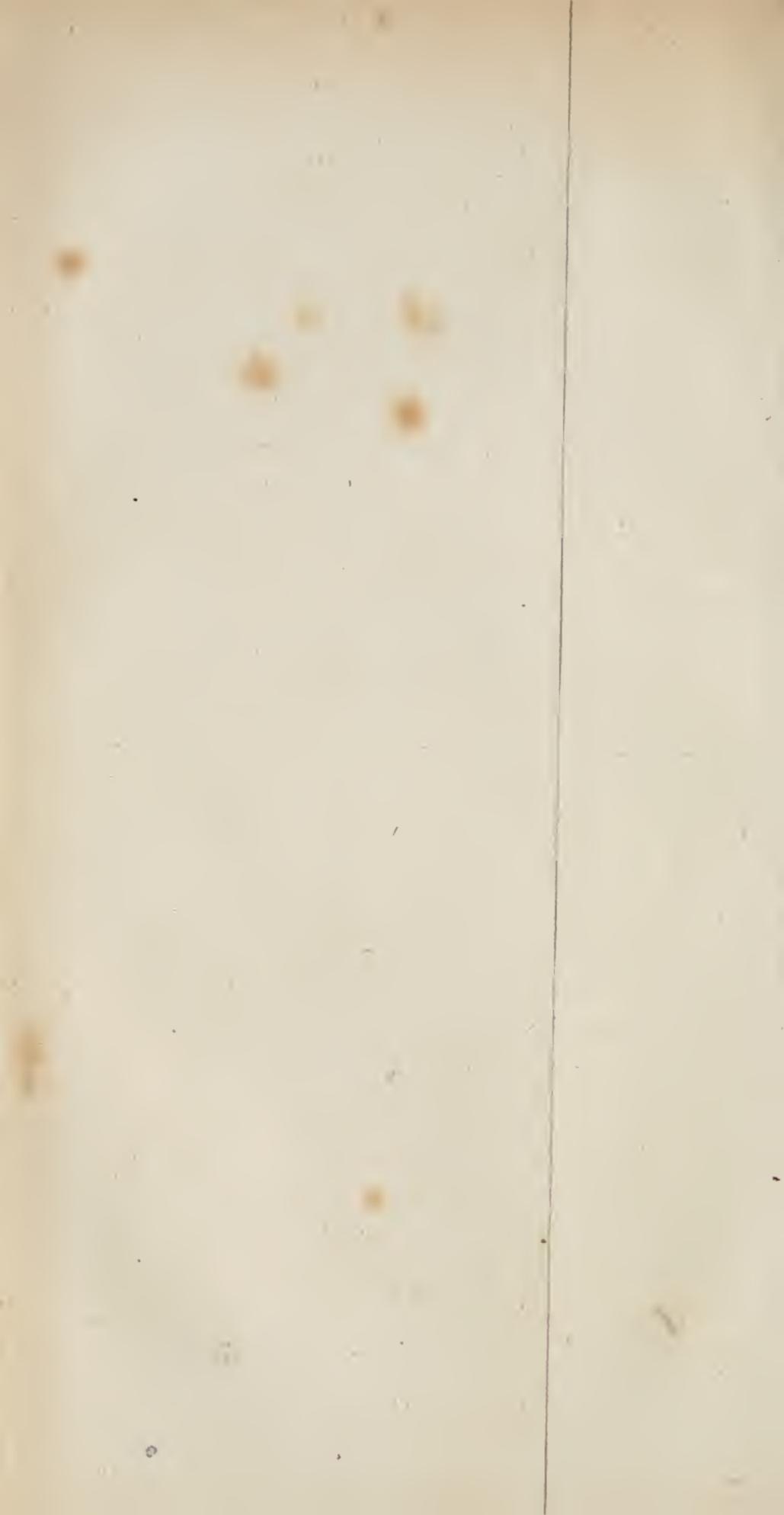




370. Two other bodies have been lately detected in the planetary regions, revolving round the Sun in orbits which do not seem very eccentric, and seem placed between those of Mars and Jupiter. The first was observed in 1801 by Mr Piazzi of Palermo, and by him named Ceres. The other was discovered in 1802 by Mr Olberg of Bremen, who has called it Pallas. They are exceedingly small, and we have seen too little of their motions as yet to enable us to state their elements with any precision.

371. Thus it has been discovered, that, while the Sun revolves round the Earth, the six planets now mentioned are always found in the circumferences of ellipses, having the Sun in one focus; and that they describe round the Sun areas proportional to the times.

372. But now, instead of supposing that the centre of a small epicycle is carried round the circumference of a greater deferent circle, different for each planet, we may rather consider the Sun's orbit round the Earth as the only deferent circle, and suppose that the planets describe their great elliptical epicycles round him with different periods, while he moves round the Earth in a year. The real motions of the planets are still the same looped curves in both cases. For, in either case, the motion of a planet is compounded of the same motions. But the latter supposition is much more probable. We can scarcely conceive the motion of Jupiter in the epicycle qrs as
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 having

having any physical relation to its centre, a mere mathematical point of space. We cannot consider this point as having any physical properties that shall influence the motions of the planet. This point also is supposed to be in motion, carrying with it the influence by which the planet is retained in the circumference of the epicycle. This is another inconceivable circumstance. This combination of circles, therefore, cannot be considered as any thing but a mere mathematical hypothesis, to furnish some means of calculation, or for the delineation of the looped path of the planet. Accordingly, the first proposers of these epicycles, sensible of the mere nothingness of their centre, and the impossibility of a nothing moving in the circumference of a circle, and drawing a planet along with it, farther supposed that the epicycles were vast solid transparent globes, and that the planet was a luminous point or star, sticking in the surface of this globe. And, to complete the hypothesis, they supposed that the globe turned round its centre, carrying the planet round with it, and thus produced the direct and retrograde motions that we observe. Aristotle taught that this motion was effected by the genius of the planet residing in the globe, and directing it, as the mind of man directs his motions. But, further, to account for the motion of this globe in the circumference of the deferent, the ancient philosophers supposed that the deferent was also a vast crystalline, or, at least, transparent material spherical shell, turning round the earth, and that this shell was of sufficient thickness to receive the epicycle

cyclic globe within its solid substance, not adhering, but at liberty to turn round its own centre. This hypothesis, though more like the dream of a feverish man than the thoughts of one in his senses, was received as unquestionable, from the time of Aristotle till that of Copernicus. It is scarcely credible that thinking men should admit its truth for a minute, even in its most admissible form. But, as the art of observing improved, it was found necessary to add another epicycle to the one already admitted, in order to account for an annual inequality in the epicyclical motion. This was a small transparent globe, placed where Aristotle placed the planet, and the planet was stuck on *its* surface. Even this was found insufficient, and another set of epicycles were added, till, in short, the heavens were filled with solid matter. It is needless to say any more of this epicyclical doctrine and machinery.

373. But the other mode of conceiving the planetary motions, while it equally furnishes the means of calculation or graphical operation, has much more the appearance of reality. The Sun's motion is round the Earth, which we are naturally disposed to think the centre of the world; and the planets revolve, not round a mathematical point, a nothing, but round the Sun, a real, and very remarkable substance.

374. Kepler, to whom we are indebted for this discovery of the elliptical motions, and the equable description

tion of areas, also observed that the squares of the periodic times in these ellipses are proportional to the cubes of the mean distances from the Sun. He also observed the same analogy with respect to the Sun's period and distance from the Earth.

375. The distances here alluded to, are all taken from a scale of equal parts, of which the Sun's mean distance from the Earth, contains 100000. But astronomers wish to know the absolute quantity of those distances in some known measures. This may be learned by means of the parallax of any one of the planets. Thus, let Mars be in M (fig. 42.), and let his distance from some fixed star C be observed by two persons on the surface of the Earth at A and B . The difference GD of the observed distances CG , CD , will give the angle DMG , or its equal AMB . The angles MAB and MBA are given by observation, and the line AB is given; and therefore AM , and consequently EM , may be computed in miles.

The transit of Venus across the Sun's disk affords much better observations for this purpose. For, at the time, Venus is much nearer to the Earth than Mars is when in opposition, their distances from us being nearly as 28 to 52. Therefore the distance between the observers will subtend a larger angle at Venus. This may be measured by the distance between the apparent tracks of Venus across the Sun's disk. A spectator in Lapland, for example, sees Venus move in the line CD (fig.

(fig. 43.), while one at the Cape of Good Hope sees her move in the line A B. Also, as C D is a shorter chord than A B, the transit will occupy less time. This difference in time, amounting, in some fortunate cases, to many minutes, will give a very exact measure of the interval between those two chords:

376. The transits in 1761 and 1769 were employed for this purpose, at the earnest recommendation of Dr Edmund Halley. From those observations, combined with the proportions deduced from Kepler's third law, we may assume the following distances from the Sun in English statute miles, as pretty near the truth.

The Earth	93,726,900
Mercury	36,281,700
Venus	67,795,500
Mars	142,818,000
Jupiter	487,472,000
Saturn	894,162,000
Georgian Planet	1,789,982,000

Of the Secondary Planets.

377. Jupiter is observed to be always accompanied by four small planets called *SATELLITES*, which revolve round him, while he revolves round the Sun.

Their distances from Jupiter are measured by means of their greatest elongations, and their periods are discovered by their eclipses, when they come into his shadow,

dow, and by other methods. They are observed to describe ellipses, having Jupiter in one focus; and they describe areas round Jupiter, which are proportional to the times. Also the squares of their periods are in the proportion of the cubes of their mean distances from Jupiter.

378. It has been discovered by means of the eclipses of Jupiter's satellites, that light is propagated in time, and employs about $8' 11''$ in moving along a line equal to the mean distance of the Earth from the Sun.

The times of the revolutions of these little bodies had been studied with the greatest care, on account of the easy and accurate means which their frequent eclipses gave us for ascertaining the longitudes of places. But it was found that, after having calculated the time of an eclipse in conformity to the periods, which had been most accurately determined, the eclipse happened later than the calculation, in proportion as Jupiter was farther from the Earth. If an eclipse, when Jupiter is in opposition, be observed to happen precisely at the time calculated; an eclipse three months before, or after, when Jupiter is in quadrature, will be observed to happen about eight minutes later than the calculated time. An eclipse happening about six weeks before or after opposition, will be about four minutes later than the calculation, when those about the time of Jupiter's opposition happen at the exact time. In general, this *retardation* of the eclipses is observed to be exactly proportional to the increase of
Jupiter's

Jupiter's distance from the Earth. It is the same with respect to all the satellites. This error greatly perplexed the astronomers, till the connexion of it with Jupiter's change of distance was remarked by Mr Roemer, a Danish astronomer, in 1674. As soon as this gentleman took notice of this connexion, he concluded that the retardation of the eclipse was owing to the time employed by the light in coming to us. The satellite, now eclipsed, continued to be seen, till the *last* reflected light reached us, and, when the stream of light ceased, the satellite disappeared, or was eclipsed. When it has passed through the shadow, and is again illuminated, it is not seen at that instant by a spectator almost four hundred millions of miles off—it does not reappear to him, till the *first* reflected light reaches him. It is not till about forty minutes after being reilluminated by the Sun, that the first reflected light from the satellite reaches the Earth when Jupiter is in quadrature, and about thirty-two minutes when he is in opposition.

This ingenious inference of Mr Roemer was doubted for some time; but most of the eminent philosophers agreed with him. It became more probable, as the motions of the satellites were more accurately defined; and it received complete confirmation by Dr Bradley discovering another, and very different consequence of the progressive motion of light from the fixed stars and planets. This will be considered afterwards; and, in the mean time, it is evinced that light, or the cause of vision, is propagated in time, and requires about $16\frac{1}{2}$ mi-

minutes to move along the diameter of the Sun's orbit, or about $8' 11''$ to come from the Sun to us, moving about 200,000 miles in a second. Some imagine vision to be produced by the undulation of an elastic medium, as sound is produced by the undulation of air. Others imagine light to be emitted from the luminous body, as a stream of water from the disperse of a watering-pan. Whichever of these be the case, Light now becomes a proper subject of Mechanical discussion; and we may now speculate about its motions, and the forces which produce and regulate them.

379. Saturn is also observed to be accompanied by seven satellites, which circulate round him in ellipses, having Saturn in the focus. They describe areas proportional to the times, and the squares of the periodic times are proportional to the cubes of their mean distances.

380. Besides this numerous band of satellites, Saturn is also accompanied by a vast arch or ring of coherent matter, which surrounds him, at a great distance. Its diameter is about 208,000 miles, and its breadth about 40,000. It is flat, and extremely thin; and as it shines only by reflecting the Sun's light, we do not see it when its edge is turned towards us. Late observation has shewn it to be two rings, in the same plane, and almost united. But that they are separated, is demonstrated by a star being seen through the interval between them.

them. Its plane makes an angle of 29° or 30° with that of Saturn's orbit; and when Saturn is in $11^{\circ} 20'$, or $5^{\circ} 20'$, the plane of the ring passes through the Sun, and reflects no light to us.

381. In 1787, Dr Herschel discovered two satellites attending the Georgian planet; and in 1798, he discovered four more. Their distances and their periodic times observe the laws of Kepler; but the position of their orbits is peculiarly interesting. Instead of revolving in the order of the signs, in planes not deviating far from the ecliptic, their orbits are almost, if not precisely perpendicular to it; so that it cannot be said that they move either in the order of the signs, or in the opposite.

382. Thus do they present a new problem in Physical Astronomy, in order to ascertain the Sun's influence on their motions—the intersection of their nodes, and the other disturbances of their motions round the planet.

383. They also shew the mistake of the Cosmognomists, who would willingly ascribe the general tendency of the planetary motions from west to east along the ecliptic to the influence of some general mechanical impulsion, instructing us how the world may be made as we see it. These perpendicular orbits are incompatible with the supposed influence.

Of the Rotation of the Heavenly Bodies.

384. In 1611, Scheiner, professor at Ingolstadt, observed spots on the disk of the Sun, which come into view on the eastern limb, move across his disk in parallel circles, disappear on the western limb, and, after some time, again appear on the eastern limb, and repeat the same motions. Hence it is inferred that the Sun revolves from west to east in the space of $25^{\text{d}} 14^{\text{h}} 12'$, round an axis inclined to the plane of the ecliptic $7\frac{1}{2}$ degrees, and having the ascending node of his equator in longitude $2^{\text{s}} 10^{\circ}$.

Philosophers have formed various opinions concerning the nature of these spots. The most probable is, that the Sun consists of a dark nucleus, surrounded by a luminous covering, and that the nucleus is sometimes laid bare in particular places. For the general appearance of a spot during its revolution is like fig. 43.

385. A series of most interesting observations has been lately made by Dr Herschel, by the help of his great telescopes. These observations are recorded in the Philosophical Transactions for the years 1801 and 1802. They lead to very curious conclusions respecting the peculiar constitution of the Sun. It would seem that the Sun is immediately surrounded by an atmosphere, heavy and transparent, like our air. This reaches to the height of several thousand miles. On this atmosphere seems to float

float a stratum of shining clouds, also some thousands of miles in thickness. It is not clear however that this cloudy stratum shines by its native light. There is above it, at some distance, another stratum of matter, of most dazzling splendor. It would seem that it is this alone which illuminates the whole planetary system, and also the clouds below it. This resplendent stratum is not equally so, but most luminous in irregular lines or ridges, which cover the whole disk like a very close brilliant network. Something of this appearance was noticed by Mr James Short in 1748, while observing a total eclipse of the Sun, and is mentioned in the Philosophical Transactions. Some operation of nature in this solar atmosphere seems to produce an upward motion in it, like a blast, which causes both the clouds and the dazzling stratum to remove from the spot, making a sort of hole in the luminous strata, so that we can see through them, down to the dark nucleus of the Sun. Dr Herschel has observed that this change, and this denudation of the nucleus, is much more frequent in some particular places of the Sun's disk. He has also observed a small bit of shining cloud come in at one side of an opening, and, in a short time, move across it, and disappear on the other side of the opening; and he thinks that these moving clouds are considerably below the great cloudy stratum.

386. Dr Herschel is disposed to think that the upper resplendent stratum never shines on the nucleus; not
even

even when an opening has been made in the stratum of clouds. For he remarks that the upper stratum is always much more driven aside by what produces the opening than the clouds are; so that even the most oblique rays from the splendid stratum do not go through, being intercepted by the border of clouds which immediately surround the opening.

387. From Dr Herschel's description of this wonderful object, we are almost led to believe that the surface of the Sun may not be scorched with intolerable and destructive heat. It not unfrequently happens that we have very cold weather in summer, when the sky is overcast with thick clouds, impenetrable by the direct rays of the Sun. The curious observations of Count Rumford of the manner in which heat is most copiously communicated through fluid substances, concur with what we knew before, to shew us that even an intense heat, communicated by radiation to the upper surface of the shining clouds by the dazzling stratum above them, may never reach far down through their thickness. With much more confidence may we affirm that it would never warm the transparent atmosphere below those clouds, nor scorch the firm surface of the Sun. It is far from being improbable therefore, that the surface may not be uninhabitable, even by creatures like ourselves. If so, there is presented to our view a scene of habitation 13,000 times bigger than the surface of this Earth, and about 50 times greater than those of all the planets added together.

388. Similar observations, first made by Dr Hooke in 1664, on spots in the disk of Jupiter, show that he revolves from west to east in $9^{\text{h}} 56'$, round an axis inclined to the plane of his orbit $2\frac{1}{2}^{\circ}$. It is also observed that his equatoreal diameter is to his axis nearly as 14 to 13.

389. There are some remarkable circumstances in the rotation of this planet. The spots, by whose change of place on the disk we judge of the rotation, are not permanent, any more than those observed on the Sun's disk. We must therefore conclude that, either the surface of the planet is subject to very considerable variations of brightness, or that Jupiter is surrounded by a cloudy atmosphere. The last is, of itself, the most probable; and it becomes still more so from another circumstance. There is a certain part of the planet that is sensibly brighter than the rest, and sometimes remarkably so. It is known to be one and the same part by its situation. This spot turns round in somewhat less time than the rest. That is, if a dark spot remains during several revolutions, it is found to have separated a little from this bright spot, to the left hand, that is, to the westward. There is a minute or two of difference between the rotation of Jupiter, as deduced from the successive appearances of the bright spot, and that deduced from observations made on the others.

390. These circumstances lead us to imagine that Jupiter is really covered with a cloudy atmosphere, and

that this has a slow motion from east to west relative to the surface of the planet. The striped appearances, called Belts or Zones, are undoubtedly the effect of a difference of climate. They are disposed with a certain regularity, generally occupying a complete round of his surface. Mr Schroeter, who has minutely studied their appearances for a long tract of time, and with excellent glasses, says that the changes in the atmosphere are very anomalous, and often very sudden and extensive; in short, there seems almost the same unsettled weather as on this globe. He does not imagine that we ever see the real surface of Jupiter; and even the bright spot which so firmly maintains its situation, is thought by Schroeter to be in the atmosphere. The general current of the clouds is from east to west, like our trade winds, but they often move in other directions. The motion is also frequently too rapid to be thought the transference of an individual substance; it more resembles the rapid propagation of some short-lived change in the state of the atmosphere, as we often observe in a thunder storm. The axis of rotation is almost perpendicular to the plane of the orbit, so that the days and nights are always equal.

391. The rotation of Mars, first observed by Hooke and Cassini in 1666, is still more remarkable than that of Jupiter. The surface of the planet is generally of unequal brightness, and something like a permanent figure may be observed in it, by which we guess at the
time

time of the rotation. But the figure is so ill defined, and so subject to considerable changes, that it was long before astronomers could be certain of a rotation, so as to ascertain the time. Dr Herschel has been at much pains to do this with accuracy, and, by comparing many successive apparitions of the same objects, he has found that the time of a revolution is 24 hours and 40 minutes, round an axis inclined to the plane of the ecliptic in an angle of nearly 60 degrees, but making an angle of $61^{\circ} 18'$ with his own orbit.

392. It is midsummer-day in Mars when he is in long. $11^{\circ} 19'$ from our vernal equinox. As the planet is of a very oblate form, and probably hollow, there may be a considerable precession of his equinoctial points, by a change in the direction of his axis.

393. Being so much inclined to the ecliptic, the poles of Mars come into sight in the course of a revolution. When either pole comes first into view, it is observed to be remarkably brighter than the rest of the disk. This brightness gradually diminishes, and is generally altogether gone, before this pole goes out of sight by the change of the planet's position. The other pole now comes into view, and exhibits similar appearances.

394. This appearance of Mars greatly resembles what our own globe will exhibit to a spectator placed on Venus or Mercury. The snows in the colder cli-

mates diminish during summer, and are renewed in the ensuing winter. The appearances in Mars may either be owing to snows, or to dense clouds, which condense on his circumpolar regions during his winter, and are dissipated in summer. Dr Herschel remarks that the atmosphere of Mars extends to a very sensible distance from his disk.

395. Observers are not agreed as to the time of the rotation of Venus. Some think that she turns round her axis in 23^h , and others make it 23 days and 8 hours. The uncertainty is owing to the very small time allowed for observation, Venus never being seen for more than three hours at a time, so that the change of appearance that we observe day after day may either be a *part* of a slow rotation, or more than a complete rotation made in a short time. Indeed no distinct spots have been observed in her disk since the time of the elder Cassini, about the middle of the seventeenth century. Dr Herschel has always observed her covered with an impenetrable cloud, as white as snow, and without any variety of appearance.

396. The Moon turns round her axis in the course of a periodic month, so that one face is always presented to our view. There is indeed a very small LIBRATION, as it is called, by which we occasionally see a little variation, so that the spot which occupies the very centre of the disk, when the Moon is in apogee and in
perigee,

perigee, shifts a little to one side and a little up or down. This arises from the perfect uniformity of her rotation, and the unequal motion in her orbit. As the greatest equation of her orbital motion amounts to little more than 5° , this causes the central spot to shift about $\frac{1}{4}$ of her diameter to one side, and, returning again to the centre, to shift as far to the other side. She turns always the same face to the other focus of her elliptical orbit round the Earth, because her angular motion round that point is almost perfectly equable.

397. It has been discovered by Dr Herschel that Saturn turns round his axis in $10^h 16'$, and that his ring turns round the same axis in $10^h 32\frac{1}{4}'$. This axis is inclined to the ecliptic in an angle of 60° nearly, and the intersection of the ring and ecliptic is in the line passing through long. $5^s 20'$ and $11^s 20'$. We see it very open when Saturn is in long. $2^s 20'$, or $8^s 20'$; and its length is then double of its apparent breadth. It is then midsummer and midwinter on Saturn. When Saturn is in the line of its nodes, it disappears, because its plane passes through the Sun, and its edge is too thin to be visible. It shines only by reflecting the Sun's light. For we sometimes see the shadow of Saturn on it, and sometimes its shadow on Saturn. It will be very open in 1811. Just now (1803) it is extremely slender, and it disappeared for a while in the month of June. Its diameter is above 200,000 miles, almost half of that of the Moon's orbit round the Earth.

398. No rotation can be observed in Mercury, on account of his apparent minuteness; nor is any observed in the Georgian planet for the same reason.

399. Many philosophers have imagined that the Earth revolves round its axis in $23^{\text{h}} 56' 4''$ from west to east: and that this is the cause of the observed diurnal motion of the heavens, which is therefore only an appearance. It must be acknowledged that the appearances will be the same, and that we must be insensible of the motion. There are also many circumstances which render this rotation very probable.

400. 1. All the celestial motions will be rendered incomparably more moderate and simple. If the heavens really turn round the Earth in $23^{\text{h}} 56' 4''$, the motion of the Sun, or of any of the planets, is swifter than any motion of which we have any measure; and this to a degree almost beyond conception. The motion of the Sun would be 20,000 times swifter than that of a cannon ball. That of the Georgian planet will be twenty times greater than this. If the Earth turns round its axis, the swiftest motion necessary for the appearances is that of the Earth's equator, which does not exceed that of a cannon ball.

The motions also become incomparably simpler. For the combination of diurnal motion with the proper motion of the planets makes it vastly more complex, and impossible to account for on any mechanical principles.

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This diurnal motion must vary, in all the planets, by their change of declination, being about $\frac{1}{7}$ slower when they are near the tropics. Yet we cannot conceive that any physical relation can subsist between the orbital motion of a planet and the position of the Earth's equator, sufficient for producing such a change in the planet's motion. Besides, the axis of diurnal revolution is far from being the same just now and in the time of Hipparchus. Just now, it passes near the star in the extremity of the tail of the little Bear. When Hipparchus observed the heavens, it passed near the snout of the Camelopard. It is to the last degree improbable that every object in the universe has changed its motion in this manner. It must be supposed that all have changed their motions in different degrees, yet all in a certain precise order, without any connexion or mutual dependence that we can conceive.

401. 2. There is no withholding the belief that the Sun was intended to be a source of light and genial warmth to the organized beings which occupy the surface of our globe. How much more simply, easily, and beautifully, this is effected by the Earth's rotation, and how much more agreeably to the known œconomy of nature!

402. 3. This rotation would be analogous to what is observed in the Sun and most of the planets.

403. 4. We observe phenomena on our globe that are necessary consequences of rotation, but cannot be accounted for without it. We know that the equatorial regions are about twenty miles higher than the circum-polar; yet the waters of the ocean do not quit this elevation, and retire and inundate the poles. This may be prevented by a proper degree of rotation. It may be so swift, that the waters would all flow toward the equator, and inundate the torrid zone; nay, so swift, that every thing loose would be thrown off, as we see the water dispersed from a twirled mop. Now, a very simple calculation will shew us that a rotation in $23^{\text{h}} 56'$ is precisely what will balance the tendency of the waters to flow from the elevated equator towards the poles, and will keep it uniformly spread over the whole spheroid. We also observe that a lump of matter of any kind weighs more (by a spring steelyard) at Spitzbergen than at Quito, and that the diminution of gravity is precisely what would arise from the supposed rotation, viz. $\frac{1}{25}$.

There are arguments which give the most convincing demonstration of the Earth's rotation.

404. 1. Did the heavens turn round the Earth, as has long been believed, it is almost certain that no zodiacal fixed star could be seen by us. For it is highly probable that light is an emission of matter from the luminous body. If this be the case, such is the distance of any fixed star A (fig. 44.) that, when its velocity AC is compounded with the velocity of light emitted in

any

any direction AB , or $A b$, it would produce a motion in a direction AD , or $A d$, which would never reach the Earth, or which might chance to reach it, but with a velocity infinitely below the known velocity of light; and, in *any* hypothesis concerning the nature of light, the velocity of the light by which we see the circumpolar stars must greatly exceed that by which we see the equatorial stars. All this is contrary to observation.

2. The shadow of Jupiter also should deviate greatly from the line drawn from the Sun to Jupiter, just as we see a ship's vane deviate from the direction of the wind, when she is sailing briskly across that direction. If the diurnal revolution is a real motion, when Jupiter is in opposition, his first satellite must be seen to come from behind his disk, and, after appearing for about $1^h 10'$, must be eclipsed. This is also contrary to observation; for the satellites are eclipsed precisely when they come into that line, whereas it should happen more than an hour after.

405. We must therefore conclude that the Earth revolves round its axis from west to east in $23^h 56' 4''$. We must further conclude, from the agreement of the ancient and modern latitudes of places, that the axis of the Earth is the same as formerly; but that it changes its position, as we observe in a top whose motion is nearly spent. This change of position is seen by the shifting of the equinoctial points. As these make a tour of the ecliptic in 25972 years, the pole of the equator, keeping

always perpendicular to its plane, must describe a circle round the pole of the ecliptic, distant from it $23^{\circ} 28' 10''$, the inclination of the equator to the ecliptic. It will be seen, in due time, that this motion of the Earth's axis, which appeared a mystery even to Copernicus, Tycho Brahé and Kepler, is a necessary consequence of the general power of nature by which the whole assemblage is held together; and the detection of this consequence is the most illustrious specimen of the sagacity of the discoverer, Sir Isaac Newton.

Of the Solar System.

406. We have seen (372.) that the planets are always found in the circumferences of ellipses, which have the Sun in their common focus, while the Sun moves in an ellipse round the Earth. The motion of any planet is compounded of any motion which it has in respect of the Sun, and any motion which the Sun has in respect of the Earth. Therefore (92. 93.) the appearances of the planetary motions will be the same as we have described, if we suppose the Sun to be at rest, and give the Earth a motion round the Sun, equal and opposite to what the Sun has been thought to have round the Earth.

In the second part of that article concerning relative motion, it was shewn that the relative motion, or change of motion, of the body B, as seen from A, is equal and opposite to that of A seen from B. In the present case, the distance of the Sun from the Earth is equal to that

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of the Earth from the Sun. The position or bearing is the opposite. When the Earth is in Aries or Taurus, the Sun will be seen in Libra or Scorpio. When the Earth is in the tropic of Capricorn, the Sun will appear in that of Cancer, and her north pole will be turned toward the Sun; so that the northern hemisphere will have longer days than nights. In short, the gradual variation of the seasons will be the same in both cases, if the Earth's axis keeps the same position during its revolution round the Sun. It must do so, if there be no force to change its position; and we see that the axes of the other planets retain their position.

407. Then, with respect to the planets, the appearances of direct and retrograde motion, with points of station, will also be the same as if the Sun revolved round the Earth. That this may be more evident, it must be observed that our judgement of a planet's situation is precisely similar to that of a mariner who sees a ship's light in a dark night. He sets it by the compass. If he sees it due north, and a few minutes after, sees it a little to the westward of north, he imagines that the ship has really gone a little westward. Yet this might have happened, had both been sailing due east, provided that the ship of the spectator had been sailing faster. It is just the same in the planetary motions. If we give the Earth the motion that was ascribed to the Sun, the real velocity of the Earth will be more than double of the velocity of Jupiter. Now suppose, according to the

old hypothesis, the Earth at T (fig. 40.) and the Sun at α . Suppose Jupiter in opposition. Then we must place the centre of his epicycle in A, and make $A\alpha$ equal to $T\alpha$. Jupiter is in a , and his bearing and distance from the Earth is Ta , nearly $\frac{4}{7}$ of TA . Six weeks after, the Sun is in β ; the centre of Jupiter's epicycle is in B. Draw Bb equal and parallel to $T\beta$, and b is now the place of Jupiter, and Tb is now his bearing and distance. He has changed his bearing to the right hand, or westward on the ecliptic; and his change of position is had by measuring the angle aTb . His longitude on the ecliptic is diminished by this number of degrees.

408. Now let the Sun be at T, according to the new hypothesis, and let ABEL be Jupiter's orbit round the Sun. Let Jupiter be in opposition to the Sun. We must place Jupiter in A, and the Earth in ϵ , so as to have the Sun and Jupiter in opposition. It is evident that Jupiter's bearing and distance from the Earth are the same as in the former hypothesis. For $A\alpha$ being equal to ϵT , we have ϵA , the distance of Jupiter from the Earth, equal to Ta of the former hypothesis. Six weeks after, the Earth is at ϕ , and Jupiter at B. Join ϕB , and draw ϕN parallel to TA . It is evident that the distance ϕB of Jupiter from the Earth, is equal to the distance Tb of the former construction. Also the angle $N\phi B$, which is Jupiter's change of bearing, (by the astronomer's compass, the ecliptic), is equal to the angle

aTb

aTb of the former construction. Jupiter therefore, instead of moving to the left hand, has moved to the right, or westward, and has diminished his ecliptical bearing or longitude by the degrees in the angle $N' \phi B$.

409. In the same manner may the apparent motion of Jupiter be ascertained for every situation of the Earth and Jupiter; and it will be found that, in every case, the line corresponding to ϕB is equal and parallel to the line corresponding to Tb ; thus γC is equal and parallel to Tc ; χD is equal and parallel to Td , &c.

The apparent motions of the planets are therefore precisely the same in either hypothesis, so that we are left to follow either opinion, as it appears best supported by other arguments.

410. Accordingly, it has been the opinion of some philosophers, both in ancient and modern times, that the Earth is a planet, revolving round the Sun placed in the focus of its elliptical orbit, and that it is accompanied by the Moon, in the same manner as Jupiter and Saturn are by their satellites.

The following are the reasons for preferring this opinion to that contained in the 371st and 373d articles, which equally explains all the phenomena hitherto mentioned, and is more consistent with our first judgments.

411. 1. The celestial motions become incomparably more simple, and free of those looped contortions which
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must be supposed in the other case, and which are extremely improbable, and incompatible with what we know of the laws of motion.

412. 2. This opinion is also more reasonable, on account of the extreme minuteness of the Earth, when compared with the immense bulk of the Sun, Jupiter, and Saturn; and because the Sun is the source of light and heat to all the planets.

The reasons adduced in this and the preceding article were all that could offer themselves to the philosophers of antiquity. They had not the telescope, and the satellites were therefore unknown. They had no knowledge of the powers of nature by which the planetary motions are produced and regulated; their knowledge of dynamical science was extremely scanty. Yet Pythagoras, Philolaus, Apollonius, Anaxagoras, and others, maintained this opinion. But they had few followers in an opinion so different from our habitual thoughts, and for which they could only offer some reasons founded on certain notions of propriety or suitableness. But, as men became more conversant, in modern times, with the mechanical arts, every thing connected with the motion of bodies became more familiar, and was better understood, and we had less hesitation in adopting sentiments unlike the first and most familiar suggestions of sense. Other arguments now offered themselves.

413. 3. If the Earth turns round the Sun, then the analogy between the squares of the periodic times and the

the

the cubes of the distances, will obtain in all the bodies which circulate round a common centre; whereas this will not be the case with respect to the Sun and Moon, if both turn round the Earth.

414. 4. It is thought that the motion of the Sun round the Earth is inconsistent with the discoveries which have been made concerning the forces which operate in the planetary motions.

We have seen, by article 230, combined with the third law of motion, that neither can the Sun revolve round the Earth at rest, nor the Earth round the Sun at rest, but that both must revolve round their common centre of position. It is discovered that the quantity of matter in the Sun is more than 300,000 times that of the matter in the Earth. Therefore the centre of position of these two bodies must be almost in the centre of the Sun. Nay, if all the planets were on one side of the Sun, the common centre would be very near his centre.

415. But, perhaps, this argument is not of the great weight that is supposed. The discovery of the proportion of these quantities of matter seems to depend on its being previously established that the Sun is in, or near, the centre of position of the whole assemblage. It must be owned, however, that the perfect harmony of all the comparative measures of the quantities of matter of the Sun and planets, deduced from sources

sources independent of each other, renders their accuracy almost unquestionable.

416. 5. It is incontestably proved by observation. A motion has been discovered in all the fixed stars, which arises from a combination of the motion of light with the motion of the Earth in its orbit.

Suppose a shower of hail falling during a perfect calm, and therefore falling perpendicularly. Were it required to hold a long tube in such a position that a hailstone shall fall through it without touching either side, it is plain that the tube must be held perpendicular. Suppose now that the tube is fastened to the arm of a gin, such as those employed in raising coals from the pit, and that it is carried round, with a velocity that is equal to that of the falling hail. It is now evident that a perpendicular tube will not do. The hailstones will all strike on the hindmost side of the tube. The tube must be put into the direction of the *relative* motion of the hailstones. Now, it was demonstrated in § 92, that this is the diagonal of a parallelogram, one side of which is the real motion of the hail, and the other is equal, but opposite, to the motion of the tube. Therefore if the tube be inclined *forward*, at an angle of 45° , the experiment will succeed, because the tangent of this angle is equal to the radius; and, while the hailstone falls two feet, the tube advances two, and the hailstone will pass along the tube without touching it.

In the very same manner, if the Earth be at rest,
and

and we would view a star near the pole of the ecliptic, the telescope must be pointed directly at the star. But if the Earth be in motion round the Sun, the telescope must be pointed a little forward, that the light may come along the axis of the tube. The proportion of the velocity of light to the supposed velocity of the Earth in its orbit is nearly that of 10,000 to 1. Therefore the telescope must lean about 20" forward.

Half a year after this, let the same star be viewed again. The telescope must again be pointed 20" a-head of the true position of the star: but this is in the opposite direction to the former deviation of the telescope, because the Earth, being now in the opposite part of its orbit, is moving the other way. Therefore the position of the star must appear to have changed 40" in the six months.

It is easy to shew that the consequence of this is, that every star must appear to have 40" more longitude when it is on our meridian at midnight, than when it is on the meridian at mid-day. The effect of this composition of motions which is most susceptible of accurate examination is the following. Let the declination of some star near the pole of the ecliptic be observed at the time of the equinoxes. It will be found to have 40" more declination in the autumnal than in the vernal equinox, if the observer be in latitude $66^{\circ} 30'$; and not much less if he be in the latitude of London. Also every star in the heavens should appear to describe a little ellipse, whose longer axis is 40".

417. Now this is actually observed, and was discovered by Dr Bradley about the year 1726. It is called the **ABERRATION OF THE FIXED STARS**, and is one of the most curious, and most important discoveries of the eighteenth century. It is important, by furnishing an incontrovertible proof that the Earth is a planet, revolving, like the others, round the Sun. It is also important, by shewing that the light of the fixed stars moves with the same velocity with the light of the Sun, which illuminates our system.

418. This arrangement of the planets is called the **COPERNICAN SYSTEM**, having been revived and established by Copernicus, represented in fig. A. The other opinion, mentioned (371.), which equally explains the general phenomena, was maintained by Longomontanus.

419. Account of the **PTOLEMAIC, EGYPTIAN, and TYCHONIC** systems (fig. B, C, D.) *

420. The Copernican system is now universally admitted; and it is fully established, 1. That the planets
and

* In the preceding pages, no notice has been taken of the latitude of the planets, and the observations by which it may be ascertained. What is delivered here is not to be considered as a treatise of the celestial motions; nothing was inserted but what was necessary for enabling the reader to judge of the evidences for the progressive and other motions of the heavenly
bodies,

and the comets describe round the Sun areas proportional to the times; and that the Moon, and the satellites of Jupiter and Saturn, describe round the Earth, Jupiter, and Saturn, areas proportional to the times. 2. That the orbits described by those bodies are ellipses, having the Sun, or the primary planet, in one focus. 3. That the squares of the periodic times of those bodies which revolve round a common centre are proportional to the cubes of their mean distances from that centre. These three propositions are called the LAWS OF KEPLER.

421. There is however an objection to this account of the planetary motions, which has been thought formidable. Suppose a telescope pointed in a direction perpendicular to the plane of the Earth's orbit, and carried round the Sun in this position. Its axis, produced to the starry firmament, should trace out a figure precisely equal and similar to the orbit, and we should be able to mark it among the stars round the pole of the ecliptic. But, if this be tried, we find that we are always looking at the same point, which always remains the centre of the little ellipse which is the effect of the aberration of light.

This objection was made, even in the schools of Greece, to Aristarchus of Samos, when he used his ut-

bodies, from which we are to infer the nature of those forces by which they are continually regulated. The motion of revolution, from which the inference is made, is in one plane, and is elliptical. This suffices for the purpose of philosophy.

most endeavours to bring into credit the later opinion of Pythagoras, placing the Sun in the centre of the system. And the answer given by Aristarchus is the only one that we can give at the present day.

422. The only answer that can be given to this is, that the distance of the fixed stars is so great, that a figure of near 200 millions of miles diameter is not a sensible object. This, incredible as it may seem, has nothing in it of absurdity. We know that their distance is immense. The comet of 1680 goes 150 times farther from the Sun than we are, and we must suppose it much farther from the nearest star, that it may not be affected by it in its motion round our Sun. Suppose it only twice as far, the Earth's orbit traced among the stars would appear only half the diameter of the Sun. We have telescopes which magnify the diameter of objects 1200 times. Yet a fixed star is not magnified by them in the smallest degree. That is, though we were only at the 1200th part of our present distance from it, it would appear no bigger. The more perfect the telescope is, the stars appear the smaller. We need not be surpris'd therefore that observation shews no parallax of the fixed stars, not even 1". Yet a parallax of 1" puts the object 206,000 times farther off than the Sun. But space is without bounds, and we have no reason to think that our view comprehends the whole creation. On the contrary, it is more probable that we see but an inconsiderable part of the scene on which the perfections of the Creator and Governor of the universe are displayed.

Of the Comets.

423. There are sometimes seen in the heavens certain bodies, accompanied by a train of faint light, which has occasioned them to be called comets. Their appearance and motions are extremely various; and the only general remarks that can be made on them are, that the train, or tail, is generally small on the first appearance of a comet, gradually lengthens as the comet comes into the neighbourhood of the Sun, and again diminishes as it retires to a distance. Also the tail is always extended in a direction nearly opposite to the Sun.

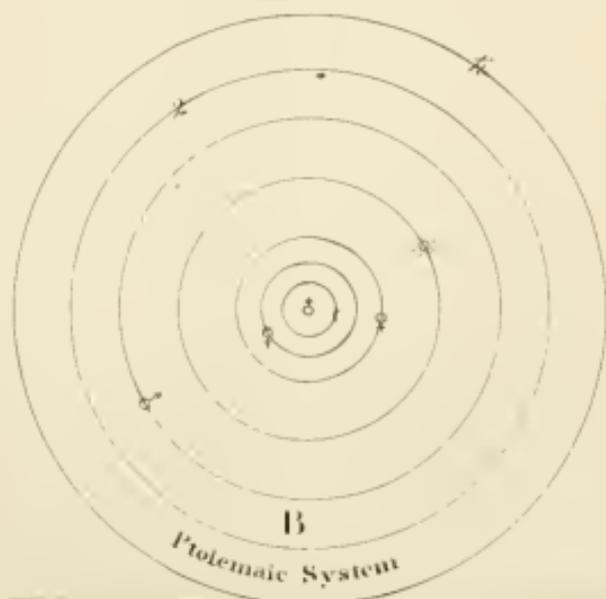
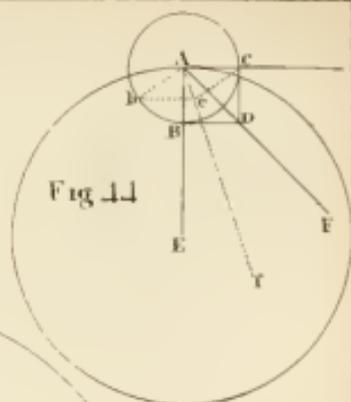
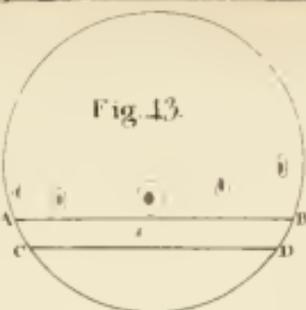
424. The opinions of philosophers concerning comets have been very different. Sir Isaac Newton first showed that they are a part of the solar system, revolving round the Sun in trajectories, nearly parabolical, having the Sun in the focus. Dr Halley computed the motions of several comets, and, among them, found some which had precisely the same trajectory. He therefore concluded, that these were different appearances of one comet, and that the path of a comet is a very eccentric ellipse, having the Sun in one focus. The apparition of the comet of 1682 in 1759, which was predicted by Halley, has given his opinion the most complete confirmation.

425. Comets are therefore planets, resembling the others in the laws of their motion, revolving round the Sun in ellipses, describing areas proportional to the times,
and

and having the squares of their periodic times proportional to the cubes of their mean distances from the Sun. They differ from the planets in the great variety in the position of their orbits, and in this, that many of them have their course *in antecedentia signorum*.

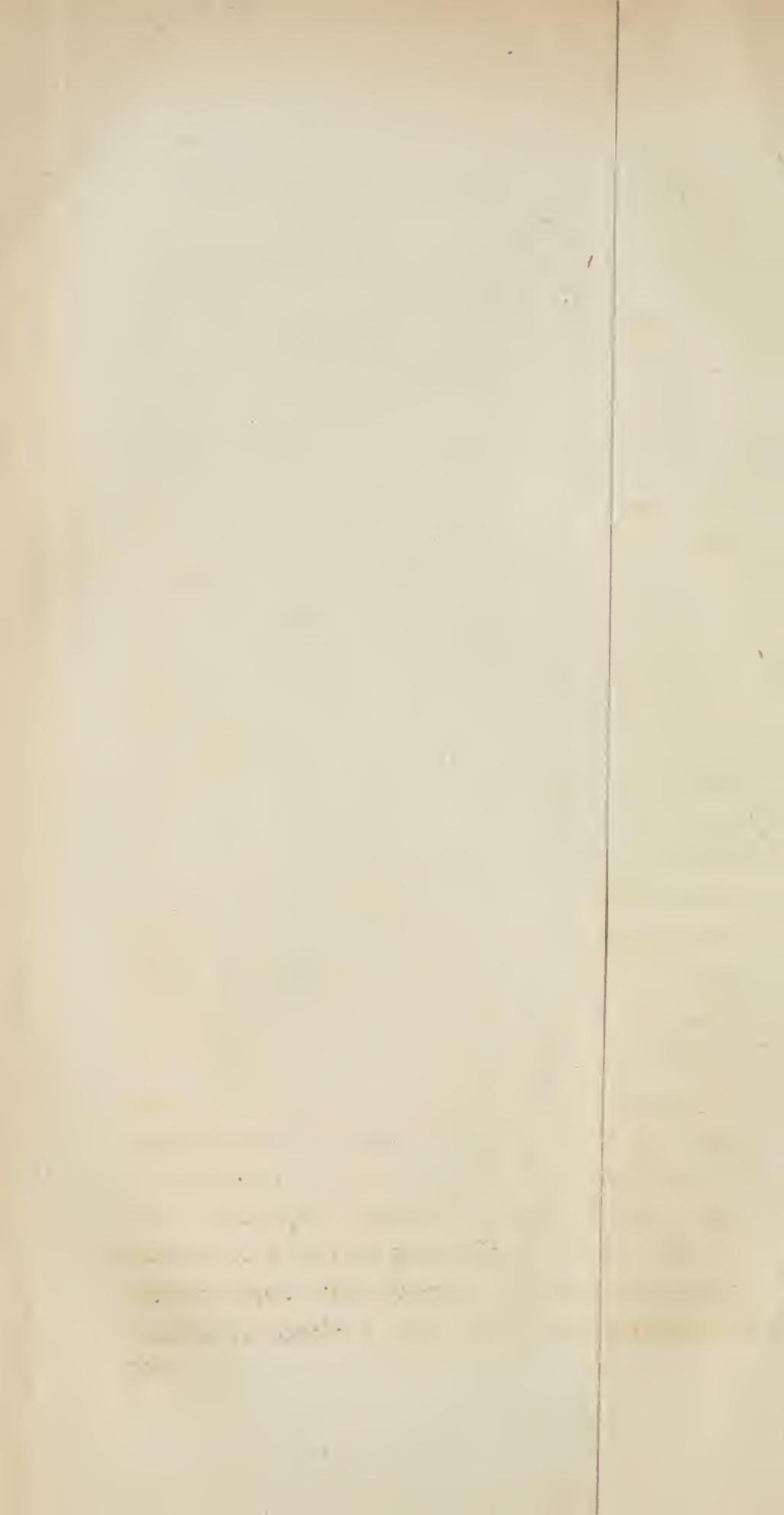
426. Their number is very great; but there are but few with the elements of whose motions we are well acquainted. The comet of 1680 came very near to the Sun on the 11th of December, its distance not exceeding his semidiameter. When in its aphelion, it will be almost 150 times farther from the Sun than the Earth is. Our ideas of the extent of the solar system are thus greatly enlarged.

427. No satisfactory knowledge has been acquired concerning the cause of that train of light which accompanies the comets. Some philosophers imagine that it is the rarer atmosphere of the comet, impelled by the Sun's rays. Others imagine, that it is the atmosphere of the comet, rising in the solar atmosphere by its specific levity. Others imagine, that it is a phenomenon of the same kind with the aurora borealis, and that this Earth would appear like a comet to a spectator placed on another planet. Consult Newton's Principia;—a Dissertation, by Professor Hamilton of Trinity College, Dublin; —a Dissertation, by Mr Winthorpe of New Jersey, &c.; both in the Philosophical Transactions.



PHYSICAL ASTRONOMY.

428. **I**T is hoped that the preceding account of the celestial phenomena has given the attentive student a distinct conception of the nature of that evidence which Kepler had for the truth of the three general facts discovered by him in all the motions, and for the truth of those seeming deviations from Kepler's laws which were so happily reconciled with them by Sir Isaac Newton, by shewing that these deviations are examples of mutual deflections of the celestial bodies towards one another. Several phenomena were occasionally noticed, although not immediately subservient to this purpose. These are the chief objects of our subsequent attempts to explain. The account given of the kind of observation by which the different motions were proved to be what has been affirmed of them, has been exceedingly short and slight, on the presumption that the young astronomer will study the celestial phenomenology in the detail, as delivered by Gregory, Keill, and other authors of reputation. This study will terminate in the fullest conviction of the validity of the evidence for the truth of the Copernican system of the Sun and planets; and in a minute acquaintance with



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with all those peculiarities of motion that distinguish the individuals of the magnificent assemblage.

We are now in a condition to investigate the particular characters of those extensive powers of nature, those mechanical affections of matter, which cause the observed deviations from that uniform rectilinear motion which would have been observed in every body, had it been under no mechanical influence. And we shall also be able to explain or account for the distinguishing peculiarities of motion which characterise the individuals of the system, if we shall so far succeed in our first investigation as to shew that no other force operates in the system, and that these peculiarities are only particular and accurately narrated cases of the three general laws, precisely conformable to their legitimate consequences. *

In

* I think it necessary here to forewarn the well-informed mathematician, if any such shall honour these pages with a perusal, that he will be disappointed if he look for any thing profound, or curious, or new, in what follows. My sole aim is to assist the ignorant in the elements of physical astronomy; and I mean to insert nothing but what seems to me to be elementary in the Newtonian philosophy. This study requires (I think) a few more steps than are usually given in the elementary publications of this country. These performances generally leave the student too scantily prepared for reading the valuable works on this subject, unless by a very obstinate and fatiguing study. They are deterred by the great difficul-

ties

In our first investigation, we must affirm the forces to be such as are indicated by the motions, in the manner agreed on in the general doctrines of Dynamics. That is, the kind and the intensity of the force must be inferred from the direction and the magnitude of the change which we consider as its effect.

In all this process, it is plain that we consider the heavenly bodies as consisting of matter that has the same mechanical properties with the bodies which are daily in our hands. We are not at liberty to imagine that the celestial matter has any other properties than what is indicated by the motions, otherwise we have no explanation,

ties thus occasioned in the beginning; and, proceeding no further, they never taste the great pleasure afforded by this noble science. I wish to render it accessible to all who have learned Euclid's Elements, and the leading properties of the three conic sections. I have preferred the geometrical to the algebraical manner of expressing the quantities under consideration. Frequently both methods are symbolical; but, even in this case, the geometrical symbol, by presenting a picture of the thing, gives an object of easier recollection, and more expressive of its nature, than an algebraical formula; and in physical astronomy, the geometrical figure is often not a symbol, but the very quantity under examination. It is from the experience of my own studies that I am induced to prefer this method, fully aware, however, that its advantages are restricted to mere elementary instruction, and that no very great progress will be made in the more recondite parts of

tion, and may as well rest contented with the simple narration of the facts. The constant practice, in all attempts to explain a natural appearance, is to try to find a class of familiar phenomena which resemble it; and if we succeed, we account it to be one of the number, and we rest satisfied with this as a sufficient explanation. Accordingly, this is the way that philosophers, both in ancient and modern times, have proceeded in their attempt to discover the causes of the planetary motions.

429. 1. Nothing is more familiar to our experience than bodies carried round fixed centres by means of solid matter connecting the bodies with the centre, in one way or another. This was the first attempt to explain the
planetary

physical astronomy without employing the algebraic along with the geometrical analysis.

I fear that I shall frequently be thought prolix and inelegant. But I beg that it may be remembered for whom these pages are written—for mere beginners in the study. I wish to leave no difficulty in the way that I can remove. If I have failed in this—*operam perdidit et oleum*. But I hope that I may enable an attentive student to read Newton's lunar theory with some relish, and a perception of its beauty. If so, my favourite point is gained,—the student will go forward.

The two articles which occupy so much at the close of this subject, are not so far pursued in our elementary books; yet what is here inserted are only the elements of the subject; and without this instruction, we can have no conception of them that is of any use.

planetary motions of which we have any account. Eudoxus and Callippus, many ages before our æra, taught that all the stars in the firmament are so many lucid points or bodies, adhering to the inside of a vast material concave sphere, which turned round the Earth placed in the centre in twenty-four hours. It was called the CRYSTALLINE ORB or Sphere.

But this will not explain the easterly motion of the Sun and Moon, unless we suppose them endowed with some self-moving power, by which they can creep slowly eastward along the surface of the crystalline orb; far less will it account for the Moon sometimes hiding the Sun from us. These philosophers were therefore obliged to say that there were other spheres, or rather spherical shells, transparent, like vast glass globes, one within another, and all having a common centre. The Sun and the Moon were supposed to be attached to the surface of those globes. The sphere which carried the Moon was the smallest, immediately surrounding the Earth. The sphere of the Sun was much larger, but still left a vast space between it and the sphere of the fixed stars, which contained all.

This machinery may make a shift to carry round the Moon, the Sun, and the stars, in a way somewhat like what we behold. But the planets gave the philosophers much trouble, in order to explain their retrograde and direct motions, and stationary points, &c. To move Jupiter in a way resembling what we behold, they supposed the shell of his sphere to be of vast thickness, and in its

solid matter they lodged a small transparent sphere, in the surface of which Jupiter was fixed. This sphere turned round in the hollow made for it in the thick shell of the deferent sphere, and, as all was transparent, exhibited Jupiter moving to the westward, when his episphere brought him toward us, and to the east, when it carried him round toward the outer surface of the deferent shell. Meanwhile, the great deferent globe was moving slowly eastward, or rather was turning more slowly westward than the sphere of the stars.

No doubt, this mechanism will produce round-about motions, and stations and retrogradations, &c. This, however, is only a very gross outline of the planetary motions. But the Sun's unequable motion could not be represented without supposing the Earth out of the centre of rotation of his sphere. This was accordingly supposed—and it was an easy supposition. But the motion of Jupiter in relation to the centre of his epicycle must be similar to the Sun's motion in relation to the Earth (361.); but a solid sphere, turning in a hollow which exactly fits it, can only turn round its centre. This is evident. Therefore the inequality of Jupiter's epicyclical motion cannot be represented by this mechanism. The deferent sphere may be eccentric, but the epicycle cannot. This obliged those engineers to give Jupiter a secondary epicycle much smaller than the epicycle which produced his retrogradations and stations. It moved in a hollow lodgement made for it in the solid matter of the epicycle, just as this moved in a hollow in the solid matter of the deferent globe.

Ever

Even this would not correspond with tolerable exactness with the observed tenor of Jupiter's motion; other epicycles were added, to tally with every improvement made on the equation of the apparent motion, till the whole space was almost crammed full of solid matter; and after all these efforts, some mathematicians affirmed that there are motions in the heavens that are neither uniform nor circular, nor can be compounded of such motions. If so, this spherical machinery is impossible. In modern times, Tycho Brahé proved beyond all contradiction that the comet of 1574 passed through all those spheres, and therefore their existence was a mere fiction.

One should think the whole of this contrivance so artless and rude, that we wonder that it ever obtained the least credit; yet was it adopted by the prince of ancient philosophers,—by Aristotle; and his authority gave it possession of all the schools till modern times.

But where, all this while, is the mover of all this machinery? Aristotle taught that each globe was conducted, or turned round its axis, by a peculiar genius or dæmon. This was worthy of the rest; and when such assertions are called *explanations*, nothing in nature need remain unexplained. We must however do Hipparchus and Ptolemy the justice to say that they never adopted this hypothesis of Eudoxus and Callippus; they did not speculate about the causes, but only endeavoured to ascertain the motions; and their epicycle and deferent circles are given by them merely as steps of mathematical contemplation, and in order to have some principle

to direct their calculation, just as we demonstrate the parabolic path of a cannon ball by compounding a uniform motion in the line of direction with a uniformly accelerated motion in the vertical line. There is no such composition, but the motion of the ball is the same as if there were.

430. 2. A much more feasible attempt was made by Cleanthes, another philosopher of Greece, to assign the causes of the planetary motions. He observed that bodies are easily carried round in whirlpools or vortices of water. He taught that the celestial spaces are filled with an ethereal fluid, which is in continual motion round the Earth, and that it carried the Sun and planets round with it. But a slight examination of this specious hypothesis shewed that it was much more difficult to form a notion of the vortices, so as to correspond with the observed motions, than to study the motions themselves. It therefore gave no explanation. Yet this very hypothesis was revived in modern times, and was maintained by two of the most eminent mathematicians and philosophers of Europe, namely, by Des Cartes and Leibnitz; and, for a long while, it was acquiesced in by all.

We must constantly keep in mind that an explanation always means to shew that the subject in question is an example of something that we clearly understand. Whatever is the avowed property of that more familiar subject, must therefore be admitted in the use made of it for explanation. We explain the splitting of glass by
heat,

heat, by shewing that the known and avowed effects of heat make the glass swell on one side to a certain degree, with a certain known force; and we shew that the tenacity of the other side of the glass, which is not swelled by the heat, is not able to resist this force which is pulling it asunder; it must therefore give way. In short, we shew the splitting to be one of the ordinary effects of heat, which operates here as it operates in all other cases.

Now, if we take this method, we find that the effects of a vortex or whirl in a fluid are totally unlike the planetary motions, and that we cannot ascribe them to the vortical motion of the æther, without giving it laws of motion unlike every thing observed in all the fluids that we know; nay, in contradiction of all those laws of mechanics which are admitted by the very patrons of the hypothesis. To give this fluid properties unknown in all others, is absurd; we had better give those properties to the planets themselves. The fact is, that these two philosophers had not taken the trouble to think about the matter, or to inquire what motions of a vortex of fluid are possible, and what are not, or what effects will be produced by such vortices as are possible. They had not thought of any means of moving the fluid itself, or for preserving it in motion; they contented themselves (at least this was the case with Des Cartes) with merely throwing out the general fact, that bodies *may* be carried round by a vortex. It is to Sir Isaac Newton that we are indebted for all that we know of vortical motion.

motion. In examining this hypothesis of Des Cartes, which had supreme authority among the philosophers at that time, he found it necessary to inquire into the manner in which a vortex may be produced, and the constitution of the vortex which results from the mode of its production. This led him, by necessary steps, to discover what forms of vortical motion are possible, what are permanent, and the variations to which the others are subject. In the second book of his *Mathematical Principles of Natural Philosophy*, he has given the result of this examination; and it contains a beautiful system of mechanical doctrine, concerning the mutual action of the filaments of fluid matter, by which they modify each other's motion. The result of the whole was a complete refutation of this hypothesis as an explanation of the planetary motions, shewing that the legitimate consequences of a vortical motion are altogether unlike the planetary motions, nay, are incompatible with them. It is quite enough, in this place, for proving the insufficiency of the hypothesis, to observe that it must explain the motion of the comets as well as that of the planets. If Mars be carried round the Sun by a fluid vortex, so is the comet which appeared in 1682 and 1759. This comet came from an immense distance, in the northern quarter of the heavens, into our neighbourhood, passing through the vortices of all the planets, describing its very eccentric ellipse with the most perfect regularity. Now, it is absolutely impossible that, in one and the same place, there can be passing a stream of the vortex
of

of a planet, and a stream of the cometary vortex, having a direction and a velocity so very different. It is inconceivable that these two streams of fluid shall have force enough, one of them to drag a planet along with it, and the other to drag a comet, and yet that the particles of the one stream shall not disturb the motion of those of the other in the smallest degree: even the infinitely rare vapour which formed the tail of the comet was not in the least deranged by the motion of the planetary vortices through which it passed. All this is inconceivable and absurd.

It is a pity that the account given by Newton of vortical motions appeared on such an occasion; for this limited the attention of his readers to this particular employment of it, which purpose being completely answered in another way, this argument became unnecessary, and was not looked into. But it contains much valuable information, of great service in all problems of hydraulics. Many consequences of the mutual action of the fluid filaments produce important changes on the motion of the whole; so that till these are understood and taken into the account, we cannot give an answer to very simple, yet important questions. This is the cause why this branch of mechanical philosophy is in so imperfect a state, although it is one of the most important.

431. 3. Many of the ancient philosophers, struck with the order, regularity, and harmonious cooperation

of the planetary motions, imagined that they were conducted by intelligent minds. Aristotle's way of conceiving this has been already mentioned. The same doctrine has been revived, in some respect, in modern times. Leibnitz animates every particle of matter, when he gives his *Monads* a perception of their situation with respect to every other monad, and a motion in consequence of this perception. This, and the elemental mind ascribed by Lord Monboddo to every thing that begins motion, do not seem to differ much from the *ὄσπηρ ψυχή* of Aristotle; nor do they differ from what all the world distinguishes by the name of *force*.

This doctrine cannot be called a hypothesis; it is rather a definition, or a misnomer, giving the name Mind to what exhibits none of those phenomena by which we distinguish mind. No end beneficial to the agent is gained by the motion of the planet. It may be beneficial to its inhabitants—But should we think more highly of the mind of an animal when it is covered with vermin?—Nor does this doctrine give the smallest explanation of the planetary motions. We must explain the motions by studying them, in order to discover the laws by which the action of their cause is regulated: this is just the way that we learn the nature of any mechanical force. Accordingly,

432. 4. Many philosophers, both in ancient and modern times, imagined that the planets were deflected from uniform rectilineal motion by forces similar to what

we observe in the motions of magnetical and electrical bodies, or in the motion of common heavy bodies, where one body seems to influence the motion of another at a distance from it, without any intervening impulsion. It is thus that a stone is bent continually from the line of its direction towards the Earth. In the same manner, an iron ball, rolling along a level table, will be turned aside toward a magnet, and, by properly adjusting the distance and the velocity, the ball may be made to revolve round the pole of the magnet. Many of the ancients said that the curvilinear motions of the planets were produced by *tendencies* to one another, or to a common centre. Among the moderns, Fermat is the first who said in precise terms that the weight of a body is the sum of the tendencies of each particle to every particle of the Earth. Kepler said still more expressly, that if there be supposed two bodies, placed out of the reach of all external forces, and at perfect liberty to move, they would approach each other, with velocities inversely proportional to their quantities of matter. The Moon (says he) and the Earth mutually attract each other, and are prevented from meeting by their revolution round their common centre of attraction. And he says that the tides of the ocean are the effects of the Moon's attraction, heaping up the waters immediately under her. Then, adopting the opinion of our countryman, Dr Gilbert of Colchester, that the Earth is a great magnet, he explains how this mutual attraction will produce a deflection into a curvilinear path, and adds, *Veritatis in me fit amor an gloria, loquantur dogmata mea, quæ ple-*

raque ab aliis accepta fero. Totam astronomiam Copernici hypothefibus de mundo, Tychonis vero Braheii observationibus, denique Gulielmi Gilberti Angli philosophiæ magneticæ inædifico. EPIT. ASTR. COPERN.

433. The most express fuffice to this purpose is that of Dr Robert Hooke, one of the most ardent and ingenious students of nature in that busy period. At a meeting of the Royal Society, on May 3. 1666, he expressed himself in the following manner.

“ I will explain a system of the world very different from any yet received; and it is founded on the three following positions.

“ 1. That all the heavenly bodies have not only a gravitation of their parts to their own proper centre, but that they also mutually attract each other within their spheres of action.

“ 2. That all bodies having a simple motion, will continue to move in a straight line, unless continually deflected from it by some extraneous force, causing them to describe a circle, an ellipse, or some other curve.

“ 3. That this attraction is so much the greater as the bodies are nearer. As to the proportion in which those forces diminish by an increase of distance, I own (says he) I have not discovered it, although I have made some experiments to this purpose. I leave this to others, who have time and knowledge sufficient for the task.”

This is a very precise enunciation of a proper philosophical theory. The phenomenon, the change of motion,

is considered as the mark and measure of a changing force, and his audience is referred to experience for the nature of this force. He had before this exhibited to the Society a very pretty experiment contrived on these principles. A ball suspended by a long thread from the ceiling, was made to swing round another ball laid on a table immediately below the point of suspension. When the push given to the pendulum was nicely adjusted to its deviation from the perpendicular, it described a perfect circle round the ball on the table. But when the push was very great, or very small, it described an ellipse, having the other ball in its centre. Hooke shewed that this was the operation of a deflecting force proportional to the distance from the other ball. He added, that although this illustrated the planetary motions in some degree, yet it was not suitable to their cause. For the planets describe ellipses having the Sun, not in the centre, but in the focus. Therefore they are not retained by a force proportional to their distance from the Sun. This was strict reasoning, from good principles. It is worthy of remark, that in this clear, and candid, and modest exposition of a rational theory, he anticipated the discoveries of Newton, as he anticipated, with equal distinctness and precision, the discoveries of Lavoisier, a philosopher inferior perhaps only to Newton.

Thus we see that many had noticed certain points of resemblance between the celestial motions and the motions of magnets and heavy bodies. But these observers let the remark remain barren in their hands, because they
had

had neither examined with sufficient attention the celestial motions, which they attempted to explain, nor had they formed to themselves any precise notions of the motions from which they hoped to derive an explanation.

434. At last a genius arose, fully qualified both by talents and disposition, for those arduous tasks. I speak of Sir Isaac Newton. This ornament, this boast of our nature, had a most acute and penetrating mind, accompanied by the soundest judgment, with a modest and proper diffidence in his own understanding. He had a patience in investigation, which I believe is yet without an equal, and was convinced that this was the only compensation attainable for the imperfection of human understanding, and that when exercised in prosecuting the conjectures of a curious mind, it would not fail of giving him all the information that we are warranted to hope for. Although only 24 years of age, Mr Newton had already given the most illustrious specimen of his ability to promote the knowledge of nature, in his curious discoveries concerning light and colours. These were the result of the most unwearied patience, in making experiments of the most delicate kind, and the most acute penetration in separating the resulting phenomena from each other, and the clearest and most precise logic in reasoning from them; and they terminated in forming a body of science which gave a total change to all the notions of philosophers on this subject. Yet this body of optical science was nothing but a fair narration of the facts presented

presented to his view. Not a single supposition or conjecture is to be found in it, nor reasoning on any thing not immediately before the eye; and all its science consisted in the judicious classification. This had brought to light certain general laws, which comprehended all the rest. Young Newton saw that this was sure ground, and that a theory, so founded, could never be shaken. He was determined therefore to proceed in no other way in all his future speculations, well knowing that the fair exhibition of a law of nature is a discovery, and all the discovery to which our limited powers will ever admit us. For he felt in its full force the importance of that maxim so warmly inculcated by Lord Bacon, that nothing is to be received as proved in the study of nature that is not logically inferred from an observed fact; that accurate observation of phenomena must precede all theory; and that the only admissible theory is a proof that the phenomenon under consideration is included in some general fact, or law of nature.

435. Retired to his country house, to escape the plague which then raged at Cambridge where he studied, and one day walking in his garden, his thoughts were turned to the causes of the planetary motions. A conjecture to this purpose occurred to him. Adhering to the Baconian maxim, he immediately compared it with the phenomena by calculation. But he was misled by a false estimation he had made of the bulk of the Earth. His calculation shewed him that his conjecture did not agree

agree with the phenomenon. Newton gave it up without hesitation; yet the difference was only about a sixth or seventh part; and the conjecture, had it been confirmed by the calculation, was such as would have acquired him great celebrity. What youth but Newton could have resisted such a temptation? But he thought no more of it.

As he admired Des Cartes as the first mathematician of Europe, and as his desire of understanding the planetary motions never quitted his mind, he set himself to examine, in his own strict manner, the Cartesian theory, which at this time was supreme in the universities of Europe. He discovered its nullity, but would never have published a refutation, hating controversy above all things, and being already made unhappy by the contests to which his optical discoveries had given occasion. His optical discoveries had recommended him to the Royal Society, and he was now a member. There he learned the accurate measurement of the Earth by Picard, differing very much from the estimation by which he had made his calculation in 1666; and he thought his conjecture now more likely to be just. He went home, took out his old papers, and resumed his calculations. As they drew to a close, he was so much agitated, that he was obliged to desire a friend to finish them. His former conjecture was now found to agree with the phenomena with the utmost precision. No wonder then that his mind was agitated. He saw the revolution he was to make

make in the opinions of men, and that he was to stand at the head of philosophers.

436. Newton now saw a grand scene laid open before him; and he was prepared for exploring it in the completest manner; for, ere this time, he had invented a species of geometry that seemed precisely made for this research. Dr Hooke's discourse to the Society, and his shewing that the pendulum was not a proper representation of the planetary forces, was a sort of challenge to him to find out that law of deflection which Hooke owned himself unable to discover. He therefore set himself seriously to work on the great problem, to "determine the motion of a body under the continual influence of a deflecting force." There were found among his papers many experiments on the force of magnets; but this does not seem to have detained him long. He began to consider the motions of terrestrial bodies with an attention that never had been bestowed on them before; and in a short time composed twelve propositions, which contained the leading points of celestial mechanism. Some years after, viz. in 1683, he communicated them to the Royal Society, and they were entered on record. But so little was Newton disposed to court fame, that he never thought of publishing, till Dr Edmund Halley, the most eminent mathematician and philosopher in the kingdom, went to visit him at Cambridge, and never ceased importuning and entreating him, till he was prevailed on to bring his whole thoughts on the subject together, digested into a

regular system of universal mechanics. Dr Halley was even obliged to correct the manuscript, to get the figures engraved, and, finally, to take charge of the printing and publication. Newton employed but eighteen months to compose this immortal work. It was published at last, in 1687, under the title of *Mathematical Principles of Natural Philosophy*, and will be accounted the sacred oracles of natural philosophy as long as any knowledge remains in Europe.

437. It is plain, that in this process of investigation, in order to explain the planetary motions by means of our knowledge of motions that are more familiar, Newton was obliged to suppose that the planets consist of common matter, in which we infer the nature of the moving cause from the motions that we observe. Newton's first step, therefore, was a scrupulous observation of the celestial motions, knowing that any mistake with regard to these must bring with it a similar mistake with regard to the natural power inferred from it. Every force, and every degree of it, is merely a philosophical interpretation of some change of motion according to the Copernican system. The Earth is said to gravitate toward the Sun, because, and only because she describes a curve line concave toward the Sun, and areas proportional to the times. If this be not true, it is not true that the Earth gravitates to the Sun. For this reason, a doubt was expressed (415.), whether the Newtonian discoveries were used with propriety as arguments for the truth of the Copernican system.

Most fortunately for science, the real motions of the heavenly bodies had been at last detected; and the sagacious Kepler had reduced them all to three general facts, known by the name of the laws of Kepler.

438. The first of those laws is, that *all the planets move round the Sun in such a manner that the line drawn from a planet to the Sun passes over or describes (verrit, sweeps) areas proportional to the times of the motion.*

Hence Newton made his first and great inference, *that the deflection of each planet is the action of a force always directed toward the Sun (219.),* that is, such, that if the planet were stopped, and then let go, it would move toward the Sun in a straight line, with a motion continually accelerated, just as we observe a stone fall toward the Earth. Subsequent observation has shewn this observation to be much more extensive than Kepler had any notion of; for it comprehends above ninety comets, which have been accurately observed. A similar action or force is observed to connect the Moon with this Earth, four satellites with Jupiter, seven with Saturn, and six with Herschel's planet, all of which describe round the central body areas proportional to the times. Newton ascribed all these deflections to the action of a mechanical force, on the very same authority with which we ascribe the deflection of a bombshell, or of a stone, from the line of projection to its *weight*, which all mankind consider as a *force*. He therefore said that *the primary planets are retained in their paths round the Sun,*

and the satellites in their paths round their respective primaries, by a force tending toward the central body. But it must be noticed that this expression ascertains nothing but the direction of this force, but gives no hint as to its manner of acting. It may be the impulse of a stream of fluid moving toward that centre; or it may be the attraction of the central body. It may be a tendency inherent in the planet—it may be the influence of some ministring spirit—but, whatever it is, this is the direction of its effect.

439. Having made this great step, by which the relation of the planets to the Sun is established, and the Sun proved to be the great regulator of their motions, Newton proceeded to inquire farther into the nature of this deflecting force, of which nature he had discovered only one circumstance. He now endeavoured to discover what variation is made in this deflection by a change of distance. If this follow any regular law, it will be a material point ascertained. This can be discovered only by comparing the momentary deflections of a planet in its different distances from the Sun. The magnitude or intensity of the force must be conceived as precisely proportional to the magnitude of the deflection which it produces in the same time, just as we measure the force of terrestrial gravity by the deflection of sixteen feet in a second, which we observe, whether it be a bombshell flying three miles, or a pebble thrown to the distance of a few yards, or a stone simply dropped from
the

the hand. Hence we infer that gravity is every where the same. We must reason in the same way concerning the planetary deflections in the different parts of their orbits.

Kepler's second law, with the assistance of the first, enabled Newton to make this comparison. This second general fact is, that *each planet describes an ellipse, having the Sun in one focus*. Therefore, to learn the proportion of the momentary deflections in different points of the ellipse, we have only to know the proportion of the arches described in equal small moments of time. This we may learn by drawing a pair of lines from the Sun to different parts of the ellipse, so that each pair of lines shall comprehend equal areas. The arches on which these areas stand must be described in equal times; and the proportion of their linear deflections from the tangents must be taken for the proportion of the deflecting forces which produced them. To make those equal areas, we must know the precise form of the ellipse, and we must know the geometrical properties of this figure, that we may know the proportion of those linear deflections.*

440.

* Some of those properties are not to be found among the elementary propositions. For this reason, a few propositions, containing the properties frequently appealed to in astronomical discussions, are put into the hands of the students, and they are requested to read them with care. Without this information,

440. *The force by which a planet describes areas proportional to the times round the focus of its elliptical orbit is as the square of its distance from the focus, inversely.*

Let F be the deflecting force in the aphelion A (fig. 45.) and f the force in any intermediate point P . Let V and v be the velocities in A and P , and C and c be the deflective chords of the equicurve circles in those points.

Then, by the dynamical proposition in art. 210, we have $F : f = \frac{V^2}{C} : \frac{v^2}{c}$, or $= V^2 c : v^2 C$. But, when areas are described proportional to the times, the velocity in A is to that in P inversely as the perpendiculars drawn from F to the tangents in A and P (102.) FA is perpendicular to the tangent in A , and FN is perpendicular to the tangent PN . Therefore $F : f = \frac{c}{FA^2} : \frac{C}{FN^2} = FN^2 \times c : FA^2 \times C$.

But it is shewn (Ellipse, § 4.) that C , the deflective chord at A , is equal to L the principal parameter of the ellipse. It was also shewn (Ellipse, § 9.) that PO is half the deflective chord at P , and (§ 8.) that PR is half the principal parameter L . Moreover, the triangles FNP and PQO and PQR are similar, and therefore $FN : FP = PQ : PO$. But $PO : PQ = PQ : PR$. Therefore $PO : PR = PO^2 : PQ^2$. Therefore $FN^2 : FP^2 = PR : PO$,
and

formation, no confident knowledge can be acquired of that noble collection of demonstrative truths taught by our illustrious countryman.

and $FN^2 \times PO = FP^2 \times PR$, and $FN^2 \times 2PO = FP^2 \times 2PR$, that is, $FN^2 \times c = FP^2 \times L$.

Therefore $F : f = FP^2 \times L : FA^2 \times L, = FP^2 : FA^2$, that is, inverfely as the fquare of the diftance from F.

441. This propofition may be demonftrated more briefly, and perhaps more palpably, as follows :

It was fhewn (Ellipfe, § 10. Cor.) that if Pp be a very minute arch, and pr be perpendicular to the radius vector PF , then qp , the linear deflection from the tangent is, ultimately, in the proportion of pr^2 . But, becaufe equal areas are defcribed in equal times, the elementary triangle PFp is a conftant quantity, when the moments are fupposed equal, and therefore pr is inverfely as PF , and pr^2 inverfely as PF^2 . Therefore qp is inverfely as PF^2 , or the momentary deflection from the tangent is inverfely as the fquare of PF , the diftance from the focus. Now, the momentary deflection is the meafure of the deflecting force, and the force is inverfely as the fquare of the diftance from the focus.

Here then is exhibited all that we know of that property or mechanical affection of the mafles of matter which compofe the folar fyftem. Each is under the continual influence of a force directed toward the Sun, urging the planet in that direktion; and this force is variable in its intensity, being more intense as the planet comes nearer to the Sun; and this change is in the inverfe duplicate ratio of its diftance from the Sun. It will free us entirely from many metaphyfical objections which

which have been made to this inference, if, instead of saying that the planets manifest such a variable tendency toward the Sun, we content ourselves with simply affirming the fact, that the planets are continually deflected toward the Sun, and that the momentary deflections are in the inverse duplicate ratio of the distances from him.

442. We must affirm the same thing of the forces which retain the satellites in their elliptical orbits round their primary planets. For they also describe ellipses having the primary planet in the focus; and we must also include the Halleyan comet, which shewed, by its reappearance in 1759, that it describes an ellipse having the Sun in the focus. If the other comets be also carried round in eccentric ellipses, we must draw the same conclusion. Nay, should they describe parabolas or hyperbolas having the Sun in the focus, we should still find that they are retained by a force inversely proportional to the square of the distance. This is demonstrated in precisely the same manner as in the case of elliptical motion, namely, by comparing the linear deflections corresponding to equal elementary sectors of the parabola or hyperbola. These are described in equal times, and the linear deflections are proper measures of the deflecting forces. We shall find in both of those curves qp proportional to $p r^2$. It is the common property of the conic sections referred to a focus.

It is most probable that the comets describe very eccentric

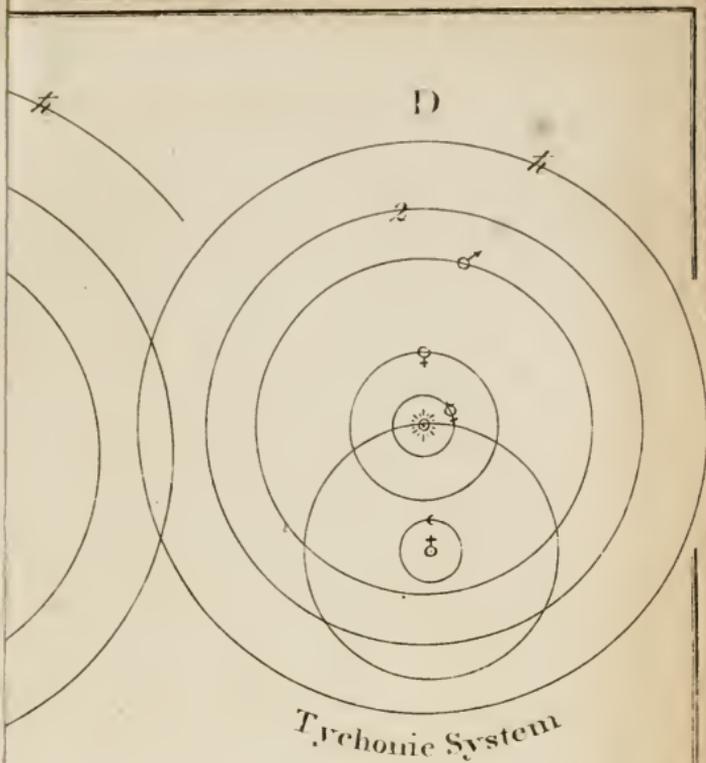
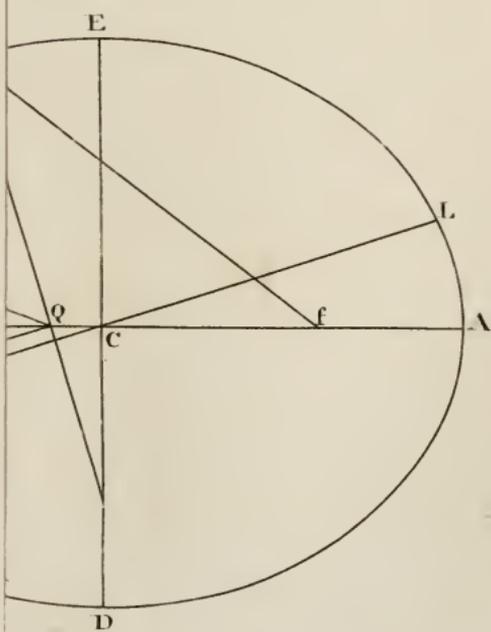


Fig. 45.



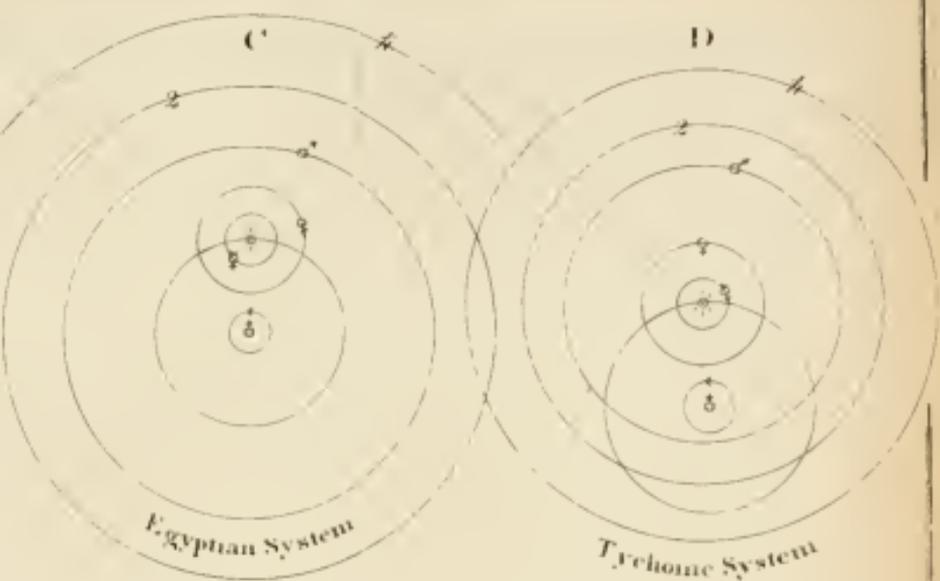
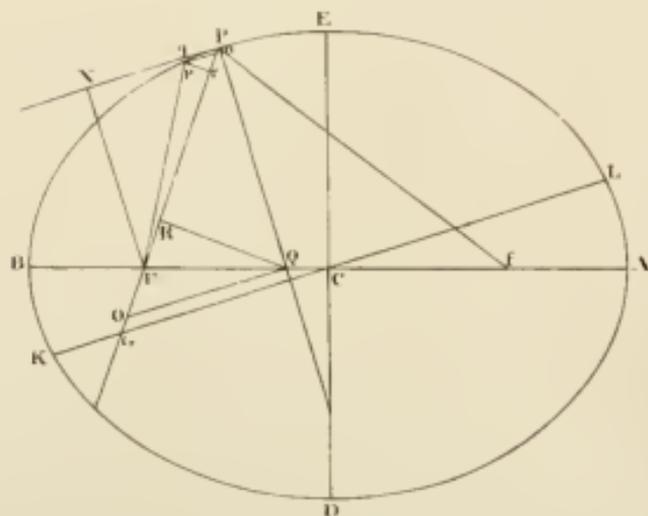
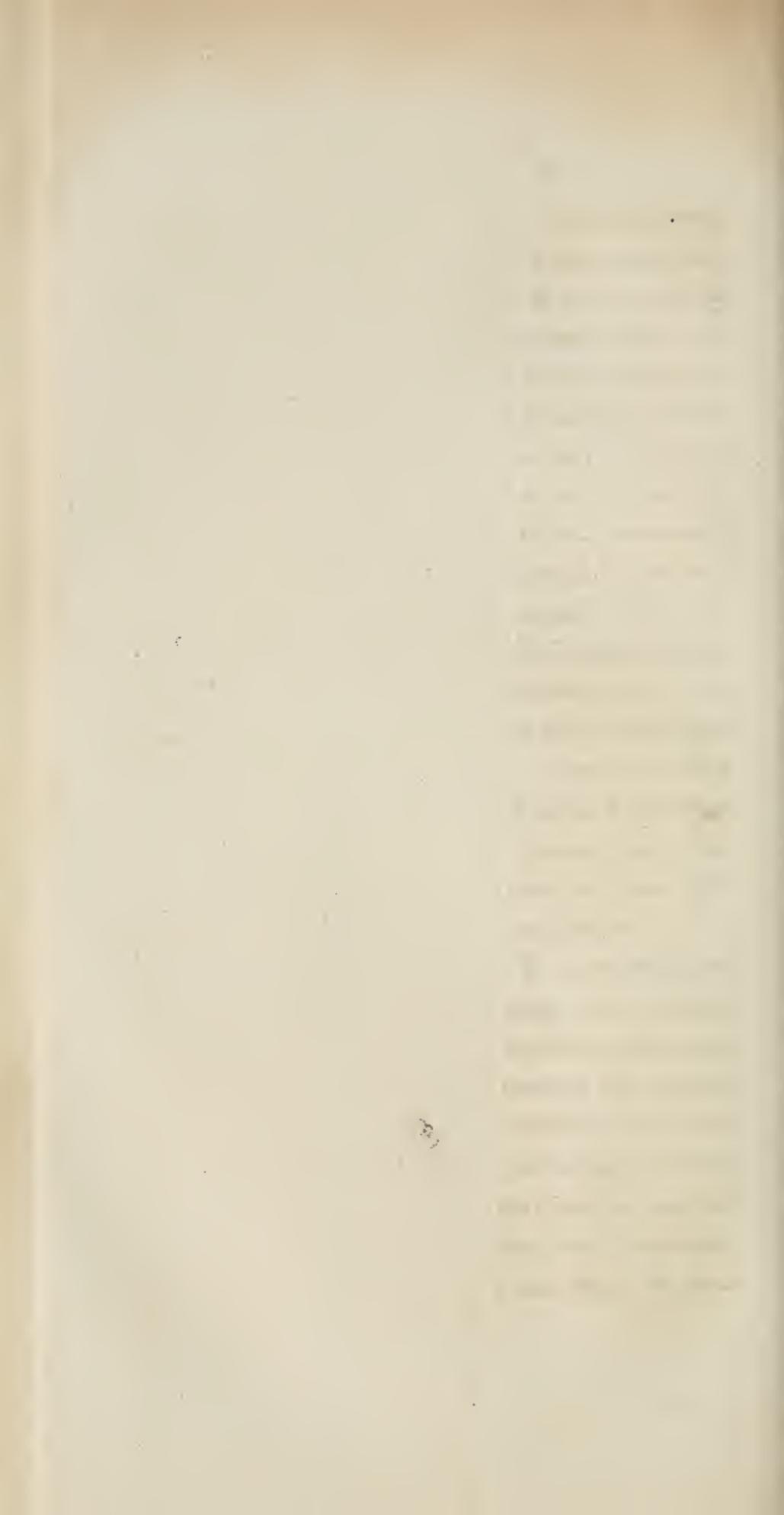


Fig. 45.



centric ellipses. But we get sight of them only when they come near to the Sun, within the orbit of Saturn. None has yet been observed as far off as that planet. The visible portion of their orbits sensibly coincides with a parabola or hyperbola having the same focus; and their motion, computed on this supposition, agrees with observation. The computation in the parabola is very easy, and can then be transferred to an ellipse by an ingenious theorem of Dr Halley's in his *Astronomy of Comets*. M. Lambert of Berlin has greatly simplified the whole process. The student will find much valuable information on this subject in M'Laurin's *Treatise of Fluxions*. The chapters on curvature and its variations, are scarcely distinguishable from propositions on curvilinear motion and deflecting forces. Indeed, since all that we know of a deflecting force is the deflection which we ascribe to it, the employment of the word *force* in such discussions is little more than an abbreviation of language.

This proposition being, by its services in explaining the phenomena of nature, the most valuable mechanical theorem ever given to the world, we may believe that much attention has been given to it, and that many methods of demonstrating it have been offered to the choice of mathematicians, the authors claiming some merit in facilitating or improving the investigation. Newton's demonstration is very short, but is a good deal incumbered with composition of ratios, and an arithmetical or algebraical turn of expression frequently mixed



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with ideas purely geometrical. Newton was obliged to compress into it some properties of the conic sections which were not very familiar at that time, because not of frequent use: they are now familiar to every student, making part of the treatises of conic sections. By referring to these, the succeeding authors gave their demonstrations the appearance of greater simplicity and elegance. But Newton gives another demonstration in the second and third editions of the *Principia*, employing the deflective chord of the equicurve circle precisely in the way employed in our text. This mode of demonstration has been varied a little, by employing the radius of curvature, instead of the chord passing through the centre of forces. The theorems given by M. De Moivre were the first in this way, and are very general, and very elegant. Those of Jo. Bernoulli, Hermann, and Keill, scarcely differ from them, and none of them all is preferable to Newton's now mentioned, either for generality, simplicity, or elegance.

443. It remains now to inquire whether there be any analogy between the forces which retain the different planets in their respective orbits. It is highly probable that there is, seeing they all respect the Sun. But it is by no means certain. Different bodies exhibit very different laws of action. Those of magnetism, electricity, and cohesion, are extremely different; and the chemical affinities, considered as the effects of attractive and
repulsive

repulsive forces, are as various as the substances themselves. As we know nothing of the constitution of the heavenly bodies, we cannot, *a priori*, say that it is not so here. Perhaps the planets are deflected by the impulsion of a fluid in motion, or are thrust toward the Sun by an elastic æther, denser and more elastic as we recede from the Sun. The Sun may be a magnet, and at the same time electrical. The Sun so constituted would act on a magnetical planet both by magnetical and electrical attraction, while another planet is affected only by his electricity. A thousand such suppositions may be formed, all very possible. Newton therefore could not leave this question undecided.

Various means of deciding it are offered to us by the phenomena. The motion of the comets, and particularly of the Halleyan comet, seems to decide it at once. This comet came from a distance, far beyond the remotest of the known planets, and came nearer to the Sun than Venus. Therefore we are entitled to say, that a force inversely as the square of the distance from the Sun, extends without interruption through the whole planetary spaces. But farther, if we calculate the deflection actually observed in the Halleyan comet, when it was at the same distance from the Sun as any of the planets, we shall find it to be precisely the same with the deflection of that planet. There can remain no doubt therefore that it is one and the same force which deflects both the comet and the planet.

But Newton could not employ this argument. The motions of the comets were altogether unknown, and probably would have remained so, had he not discovered the sameness of the planetary force through its whole scene of influence. The fact is, that Newton's first conjectures about the law of the solar force were founded on much easier observations.

Kepler's third law is, that *the squares of the periodic times of the planets are in the same proportion with the cubes of their mean distances from the Sun.* Thus, Mars is nearly four times as far from the Sun as Mercury, and his period is nearly eight times that of Mercury—Now $4^3 = 64, = 8^2$.

The planets describe figures which differ very little from circles, whose radii are those mean distances. If they described circles, it would have been very easy to ascertain the proportion of the centripetal forces. For, by art. 216, we had $f \doteq \frac{d}{t^2}$. Now, in the planetary motions, we have $t^2 \doteq d^3$. Therefore, in this case, $f \doteq \frac{d}{d^3}$, or $\doteq \frac{1}{d^2}$, that is, the forces which regulate the motions of the planets at their mean distances are inversely as the squares of those distances.

It was this notion (by no means precise) of the planetary force, which had first occupied the thoughts of young Newton, while yet a student at college—and, on no better authority than this, had he supposed that a similar analogy would be observed between the deflection

of the Moon and that of a cannon ball. His disappointment, occasioned by his erroneous estimation of the bulk of this Earth, and his horror at the thoughts of any such controversies as his optical discoveries had engaged him in, seem to have made him resolve to keep these thoughts to himself. But when Picard's measure of the Earth had removed his cause of mistake, and he saw that the analogy did really hold with respect to the force reaching from the Earth to the Moon; he then thought it worth his while to study the subject seriously, and to investigate the deflection in the arch of an ellipse. His study terminated in the proposition demonstrated above,—doubtless, to his great delight. He was no longer contented with the vague guesses which he had made as to the proportion of the forces which deflected the different planets. The orbit of Mars, and still more, the orbit of Mercury, is too eccentric to be considered as a circle. Besides, at the mean distances, the radius vector is not perpendicular to the curve, as it is in a circle. He was now in a condition to compare the simultaneous deflections of any two planets, in any part of their orbits. This he has done. In the fifteenth proposition of the first book of the *Principia*, he demonstrates that if the forces actuating the different planets are in the inverse duplicate ratio of the distances from the Sun, then the squares of the periodic times must be as the cubes of the mean distances.—This being a matter of observation, it follows, conversely, that the forces are in this inverse duplicate ratio of the distances.

Thus

Thus was his darling object attained. But, as this fifteenth proposition has some intricacy, it is not so clear as we should wish in an elementary course like ours. The same truth may be easily made appear in the following manner.

444. *If a planet, when at its mean distance from the Sun, be projected in a direction perpendicular to the radius vector, with the same velocity which it has in that point of its orbit, it will describe a circle round the Sun in the same time that it describes the ellipse.*

Let $ABPD$ (fig. 46.) be the elliptical orbit, having the Sun in the focus S . Let AP, BD , be the two axes, C the centre, A the aphelion, P the perihelion, and B, D , the two situations of mean distance. About S describe the circle BDM . Let BK and BN be very small equal arches of the circle and the ellipse, and let BE be one half of BS .

BM , the double of BS , is the deflective chord of the circle of curvature in the point B of the orbit (ellipse, § 9.), and BE is $\frac{1}{4}$ of that chord. Therefore (212.) the velocity in B is that which the force in B would generate by uniformly impelling the planet along BE . But a body projected with this velocity in the direction BK will describe the circle $BKMD$. (106—212.)

The arches BK and BN , being equal, and described with equal velocities, will be described in equal times. The triangles BKS, BNS , having equal bases BK and BN ,

BN , are proportional to their altitudes BS and BC (for the elementary arch BN may be considered as coinciding with the tangent in B , and BC is perpendicular to this tangent). But, because BS is equal to CA , the area of the circle BMD is to that of the ellipse $ABPD$ as AC to BC , that is, as BS to BC , that is, as the triangle BKS to the triangle BNS . These triangles are therefore similar portions of the whole areas, and therefore, since they are described in equal times, the circle BMD and the ellipse $ABPD$ will also be described in equal times.

Thus it appears that Newton's first conjecture was perfectly just. For if the planets, instead of describing their elliptical orbits, were describing circles at the same distances, and in the same times, they would do it by the influence of the same forces. Therefore since, in this case, we should have $t^2 \doteq d^3$, the forces will be proportional to d^2 inversely.

445. We now see that the forces which retain the different planets in their orbits are not different forces, but that all are under the influence of one force, which extends from the Sun in every direction, and decreases in intensity as the square of the distance from the Sun increases. The intensity at any particular distance is the same, in whatever direction the distance is taken. Although the planetary courses do not depart far from our ecliptic, the influence of the regulating force is by no means confined

fined to that neighbourhood. Comets have been seen which rise almost perpendicular to the ecliptic; and their orbits or trajectories occupy all quarters of the heavens.

This relation, in which they all stand to the Sun, may justly be called a cosmical relation, depending on their mutual constitution, which appears to be the same in them all. As this force respects the Sun, it may be called a SOLAR FORCE, in the same sense as we use the term magnetical force. All persons unaffected by peculiar philosophical notions, conceive magnetism distinctly enough by calling it *Attraction*. For, whatever it is, its effects resemble those of attraction. If we conceive the magnetical phenomena as effects of a tendency toward the magnet, inherent in the iron, we may conceive the planetary deflections as produced in the same way; but this also indicates a sameness in the constitution of all the planets. Or we may ascribe the deflections to the impulses or pressure of an æther; but this also indicates a sameness of constitution over the whole system.

Thus, whatever notion we entertain of what we have called a solar or a planetary force (and the observed law of action limits us to no exclusive manner of conceiving it), we see a power of nature, whether extrinsic, like the action of a fluid, or intrinsic, like tendencies or attractions, which fit the Sun and planets for a particular purpose, giving them a cosmical relation, and laws of action.

- ‘ ———quas dum primordia rerum
 ‘ Pangeret, omniparens leges violare Creator
 ‘ Noluit, eternique operis fundamina fixit.
 ‘ Sol folio residens ad se jubet omnia prono
 ‘ Tendere descensu, nec recto tramite currus
 ‘ Sidereos patitur vastum per inane moveri,
 ‘ Sed rapit; immotis, se centro, singula gyris.’

HALLEY.

446. It is still more interesting to remark that the satellites observe the same law of action. For, in the little systems of a planet and its satellites, we observe the same analogy between the distances and periodic times. In short, a centripetal force in the inverse duplicate ratio of the distance seems to be the bond by which all is held together.

447. As the analogy observed by Kepler between the distances of the revolving bodies and the periods of their revolutions, led Newton to the discovery of the law of planetary deflection; so, this law being established, we are led to the second and third fact observed by Kepler as its necessary consequences. It appears that the periodic time of a planet under the influence of a force inversely as the square of the distance, depends on its mean distance alone, and will be the same, whether the planet describe a circle or an ellipse having any degree whatever of eccentricity. This, as was already observed, is the fifteenth proposition of the first book of Newton's

Principia. Suppose the shorter axis BD of the ellipse $ABPD$ (fig. 47.) to diminish continually, the longer axis AP remaining the same. As the extremity B of the invariable line BS moves from B toward C , the extremity S will move toward P , and when B coincides with C , S will coincide with P , and the ellipse is changed into a straight line PA , whose length is twice the mean distance SB .

In all the successive ellipses produced by this gradual diminution of CB , the periodic time remains unchanged. Just before the perfect coincidence of B with C , the ellipse may be conceived as undistinguishable from the line PA . The revolution in this ellipse is undistinguishable from the ascent of the body from the perihelion P to the aphelion A ; and the subsequent descent from A to P . Therefore a body under the influence of the central force will descend from A to P in half the time of the revolution in the ellipse $ADPBA$. Therefore the time of descending from any distance BS is half the period of a body revolving at half that distance from the Sun. By such means we can tell the time in which any planet would fall to the Sun. Multiply the half of the time of a revolution by the square root of the cube of $\frac{1}{2}$, that is, by the square root of $\frac{1}{8}$; the product is the time of descent. Or divide the time of half a revolution by the square root of the cube of 2, that is, by the square root of 8, that is, by 2,82847; or, which is the shortest process, multiply the time of a revolution by the decimal 0,176776;

Mercury

	d.	h.
Mercury will fall to the Sun in - - -	15	13
Venus - - - - -	39	17
The Earth - - - - -	64	10
Mars - - - - -	121	0
Jupiter - - - - -	290	0
Saturn - - - - -	798	0
Georgian planet - - - - -	5406	0
The Moon to this Earth - - - - -	4	21

Cor. The squares of the times of falling to the Sun are as the cubes of the distances from him.

448. So far did Newton proceed in his reasonings from the observations of Kepler. But there remained many important questions to be decided, in which those observations offered no direct help.

It appeared improbable that the solar force should not affect the secondary planets. It has been demonstrated (252.) that if a body P (fig. 29.) revolve round another body S, describing areas proportional to the times, while S revolves round some other body, or is affected by some external force, P is not only acted on by a central force directed to S, but is also affected by every accelerating force which acts on S.

While, therefore, the Moon describes areas proportional to the times round the Earth, it is not only deflected toward the Earth, but it is also deflected as much as the Earth is toward the Sun. For the Moon accom-

panies the Earth in all its motions. The same thing must be affirmed concerning the satellites attending the other planets.

And thus has Newton established a fourth proposition, namely,

The force by which a secondary planet is made to accompany the primary in its orbit round the Sun is continually directed to the Sun, and is inversely as the square of the distance from him. For, as the primary changes its distance from the Sun, the force by which it is retained in its orbit varies in this inverse duplicate ratio of the distance. Therefore the force which causes the secondary planet to accompany its primary *must* vary in the same proportion, in order to produce the same change in its motion that is produced in that of the primary. And, further, since the force which retains Jupiter in his orbit is to that which retains the Earth as the square of the Earth's distance is to that of Jupiter's distance, the forces by which their respective satellites are made to accompany them must vary in the same proportion.

Thus, all the bodies of the solar system are continually urged by a force directed to the Sun, and decreasing as the square of the distance from him increases.

449. Newton remarked, that in all the changes of motion observable in our sublunary world, the changes in the acting bodies are equal and opposite. In all impulsions, one body is observed to lose as much motion as the other gains. All magnetical and electrical

cal attractions and repulsions are mutual. Every action seems to be accompanied by an equal reaction in the opposite direction. He even imagined that it may be proved, from abstract principles, that it must be so. He therefore affirmed that this law obtained also in the celestial motions, and that not only were the planets continually impelled toward the Sun, but also that the Sun was impelled toward the planets. The doubts which may be entertained concerning the authority of this law of motion have been noticed already. At present, we are to notice the facts which the celestial motions furnish in support of Sir Isaac Newton's assertion.

450. Directions have been given (294.) how to calculate the Sun's place for any given moment. When the astronomers had obtained instruments of nice construction, and had improved the art of observing, there was found an irregularity in this calculation, which had an evident relation to the Moon. At new Moon, the observations corresponded exactly with the Sun's calculated place; but seven or eight days after, the Sun is observed to be about 8" or 10" to the eastward of his calculated place, when the Moon is in her first quadrature, and he is observed as much to the westward when she is in the last quadrature. In intermediate situations, the error is observed to increase in the proportion of the sine of the Moon's distance from conjunction or opposition.

Things must be so, if it be true that the deflection of
the

the Moon toward the Earth is accompanied with an equal deflection of the Earth toward the Moon. For (230.) the Moon will not revolve round the Earth, but the Earth and Moon will revolve round their common centre of position. When the Moon is in her first quadrature, her position may be represented by *M* (fig. 48.) while the Earth is at *E*, and their common centre is at *A*. A spectator in *A* will see the Sun *S* in his calculated place *B*. But the spectator in the Earth *E* sees the Sun in *C*, to the left hand, or eastward of *B*. The interval *BC* measures the angle *BSC*, or *ASE*, subtended at the Sun by the distance *EA* of the common centre of the Earth and Moon from the centre of the Earth. At new Moon, *A*, *E*, and *S*, are in a straight line, so that *B* and *C* coincide. At the last quadrature, the Moon is at *m*, the Earth at *e*, and the common centre at *a*. Now the Sun is seen at *c*, 8" or 10" to the westward of his calculated place. This correction has been pointed out by Newton, but it was not observed at the first, owing to its being blended with the Sun's horizontal parallax which had not been taken into account. But it was soon recognised, and it now makes an article among the various equations used in calculating the Sun's place.

Here, then, is a plain proof of a mutual action and reaction of the Earth and Moon. For, since they revolve round a common centre, the Earth is unquestionably deflected into the curve line by the action of a force directed towards the Moon. But we have a much better proof.

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proof. The waters of the ocean are observed every day to heap up on that part of our globe which is under the Moon. In this situation, the weight of the water is diminished by the attraction of the Moon, and it requires a greater elevation, or a greater quantity, to compensate for the diminished weight. On the other hand, we see the waters abstracted from all those parts which have the Moon in the horizon. Kepler, after asserting, in very positive terms, that the Earth and Moon would run together, and are prevented by a mutual circulation round their common centre, adduces the tides as a proof.

451. As the art of observation continued to improve, astronomers were able to remark abundant proofs of the tendency of the Sun toward the planets. When the great planets Jupiter and Saturn are in quadrature with the Earth, to the right hand of the line drawn from the Earth to the Sun's calculated place, the Sun is then observed to shift to the left of that line, keeping always on the opposite side of the common centre of position. These deviations are indeed very minute, because the Sun is vastly more massive than all the planets collected into one lump. But in favourable situations of these planets, they are perfectly sensible, and have been calculated; and they *must* be taken into account in every calculation of the Sun's place, in order to have it with the accuracy that is now attainable. It must be granted that this accuracy, actually attained by means of those corrections, and unattainable without them, is a positive proof of
this

1848
The first of the year
was a very dry one
and the crops were
very poor. The
winter was also
very cold and
the snow lay on
the ground for
many weeks.

The second of the year
was a very wet one
and the crops were
very good. The
winter was also
very mild and
the snow lay on
the ground for
very few weeks.
The third of the year
was a very dry one
and the crops were
very poor. The
winter was also
very cold and
the snow lay on
the ground for
many weeks.

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this

this mutual deflection of the Sun toward the planets. The quantity corresponding to one planet is too small, of itself, to be very distinctly observed; but, by occasionally combining with others of the same kind, the sum becomes very sensible, and susceptible of measure. It sometimes amounts to 38 seconds, and must never be omitted in the calculations subservient to the finding the longitude of a ship at sea. Philosophy, in this instance, is greatly indebted to the arts. And she has liberally repaid the service.

452. Here it is worthy of remark, that had the Sun been much smaller than he is, so that he would have moved much further from the common centre, and would have been much more agitated by the tendencies to the different planets, it is probable that we never should have acquired any distinct or useful knowledge of the system. For we now see that Kepler's laws cannot be strictly true; yet it was those laws alone that suggested the thought, and furnished to young Newton the means of investigation. The analogy of the periodic times and distances is accurate, only with respect to the common centre, but not with respect to the Sun. But the great mass of the Sun occasions this common centre to be generally within his surface, and it is never distant from it $\frac{1}{4}$ of his diameter. Therefore this third law of Kepler is so nearly exact in respect of the Sun, that the art of observation, in Newton's lifetime, could not have found any errors. The penetrating eye of Newton however immediately

immediately perceived his own good fortune, and his error in supposing Kepler's laws accurately true. But this was not enough for his philosophy; he was determined that it should narrate nothing but truth. With great ingenuity, and elegance of method, he demonstrates that his mechanical inferences from Kepler's laws are still strictly true, and that his own law of planetary force is exact, although the centre of revolution is not the centre of the Sun. All the difference respects the absolute magnitude of the periodic times in relation to the magnitude of the force. This he demonstrates in a series of propositions, of which our § 231. is the chief.

453. Newton proceeds still further in his investigation of the extent of the influence of this planetary force, and says that *all the planets mutually tend toward each other*. It does not appear how this opinion arose in his mind. There are abundance of phenomena, however, of easy observation, which make it very evident. It was probably a conjecture, suggested by observing this reciprocal action between the Earth and Moon. But he immediately followed it into its consequences, and pointed them out to the astronomers. They are very important, and explain many phenomena which had hitherto greatly perplexed the astronomers.

Suppose Jupiter and Mars to be in conjunction, lying in the same line from the Sun. As Mars revolves much quicker than Jupiter, he gets before him, but, being attracted by Jupiter, his motion is retarded—and Jupiter,

being attracted by Mars, is accelerated. On the contrary, before Mars arrives at conjunction with Jupiter, Mars is accelerated, and Jupiter is retarded. Further, the attraction of Mars by Jupiter must diminish the tendency of Mars to the Sun, or must act in opposition to the attraction of the Sun; therefore the curvature of Mars's orbit in that place must be diminished. On the contrary, the tendency of Jupiter to Mars, acting in the same direction as his tendency to the Sun, must increase the curvature of that part of Jupiter's orbit. If Jupiter be at this time advancing to his aphelion, this increase of curvature will sooner bend the line of his motion from an obtuse into a right angle with the radius vector. Therefore his aphelion will be sooner attained, and it will appear to have shifted to the westward. For the opposite reasons, the apses of Mars will seem to shift to the eastward. There are other situations of these planets where the contrary effects will happen. In each revolution, each planet will be alternately accelerated twice, and twice retarded, and the apses of the exterior planet will continually recede, and that of the interior will advance. It is obvious that this disturbance of the motion of a planet by its deflection to another, though probably very minute, yet being continued for a tract of time, its accumulated result may become very sensible. These changes are all susceptible of accurate calculation, as we shall afterwards explain particularly.

This must be considered as a convincing proof of the mutual action of the heavenly bodies, and it adds fresh

lustre to the penetration and genius of Newton, who made these assertions independent of observation, pointing out to astronomers the sure means of perfecting their knowledge of the celestial motions.

454. Here therefore we have established a fifth proposition in physical astronomy, namely, that *all the bodies in the solar system tend mutually toward one another, with forces which vary in the inverse duplicate ratio of the distances.*

It did not satisfy Newton that he merely pointed out the gross effect of this mutual tendency. He gave astronomers the means of investigating and ascertaining its intensity, and its variation by a variation of distance. The effect of the Earth's tendency to Jupiter during any length of time, may be computed by means of Newton's dynamical propositions, contained in the first book of his Principia, particularly by the 39th. Of these we have given a proper selection in the general doctrines of Dynamics.

455. But the inquisitive mind of Newton did not stop here. He was anxious to learn whether this planetary tendency had any resemblance or relation to forces with which we are more familiarly acquainted. Of this kind are magnetism and gravity. He was the more incited to this investigation by the conjectures on this subject which had arisen in the mind of Kepler. This great astronomer had been much taken with the disco-

very just published by Dr Gilbert of Colchester, stating that this Earth is a great magnet, and he was disposed to ascribe the revolution of the Moon to the magnetical influence of the Earth. It appears from Newton's papers, that he had made a great many experiments for discovering the law of magnetic action. But he had found it so dependant on circumstances of form and situation, and so changeable by time, that it seemed susceptible of no comparison with the solar force; and he soon gave it up. He was more successful in tracing the resemblances observable in the phenomena of common gravity. It has been already remarked (435.), that, very early in life, he had conjectured that it was the same with the solar force; and that after he had formed the opinion that the solar force varied in the inverse duplicate ratio of the distance, he put his conjecture to the test, by comparing the fall of a stone with the deflection of the Moon. The distance of the Moon is estimated to be 60 semidiameters of the Earth. Therefore, if gravity and the lunar deflecting force be the same, the stone should deflect as much in one second as the Moon does in a minute. For we may, without any sensible error, suppose that the lunar force acts uniformly during one minute. If so, the linear deflections must be as the squares of the times. The deflection in a minute must be 60×60 times, or 3600 times the deflection in a second. But, according to the law of planetary force, the deflection at the Earth's surface must be 60×60 , or 3600 times the deflection at the Moon.

Now,

Now, in a second, a stone falls 16 feet and an inch. Therefore the Moon should deflect 16 feet and an inch in a minute from the tangent of her orbit. Newton calculated the versed sine of the arch described by the Moon in a minute, to a radius equal to 60 semidiameters of the Earth. He found it only about $13\frac{1}{2}$ feet, and he gave over any farther inquiry. But he had hastily supposed a degree to contain 60 miles, not attending to the difference between a geographical mile, or 60th of a degree, and an English statute mile. A degree contains $69\frac{1}{2}$ such miles; so that he had made the Moon's orbit, and consequently her deflection, too small in the same proportion. If we increase the calculated deflection in this proportion, it comes out exactly $16\frac{1}{12}$; and the conjecture is fully established.

When Picard's accurate measure of the Earth had enabled Newton to confirm his former conjecture concerning the identity of the planetary force and terrestrial gravity, he again made the calculation and comparison *in the most scrupulous manner*. For we now see that several circumstances must be taken into the account, which he had omitted in his first computation from Picard's measure of the Earth. The fall in a second is not the exact measure of terrestrial gravity. A stone would fall farther, were it not that its gravity is diminished by the Earth's rotation. It is also diminished by the action of the Sun and Moon, and by the weight of the air which the stone displaces. All these diminutions of the accelerating force of gravity are susceptible of exact calculation,

tion, and were accordingly calculated by Newton, and the amount added to the observed acceleration of a falling body. In the next place, the real radius of the Moon's orbit must be reckoned only from the common centre of the Earth and Moon. And then the force deduced from this deflection must be increased in the subduplicate ratio of the matter in the Earth to the matter in the Earth and Moon added together (231.) All this has been done, and the result coincides precisely with observation.

This may be demonstrated in another way. We can tell in what time a body would revolve round the Earth, close to its surface. For we must have t^2 proportional to d^3 . It will be found to be 84 minutes and 34 seconds. Then we know the arch described in one second, and can calculate its deflection from the tangent. We shall find it $16\frac{1}{2}$ feet, the same with that produced by common gravity.

456. *Terrestrial gravity, therefore, or that force which causes bodies to fall, or to press on their supports, is only a particular example of that universal tendency, by which all the bodies of the solar system are retained in their orbits.*

We must now extend to those bodies the other symptoms of common gravity. It is by gravity that water arranges itself into a level surface, that is, a surface which makes a part of the great sphere of the ocean. The weight of this water keeps it together, in a round form. We must ascribe the globular forms of the Sun and planets

nets to a similar operation. A body on their surface will press it as a heavy body presses the ground. Dr Hooke remarks that all the protuberances on the surface of the Moon are of forms consistent with a gravity toward its centre. They are generally sloping, and, though in some places very rugged and precipitous, yet nowhere overhang, or have any shape that would not stand on the ground. The more rugged parts are most evidently matter which has been thrown up by volcanic explosion, and have fallen down again by their lunar gravity.

457. That property by which bodies are heavy is called GRAVITY, HEAVINESS—the being heavy; and the *fact* that it moves toward the Earth, may be called GRAVITATION. While it falls, or presses on its supports, it may be said to *gravitate*, to give indication of its being *gravis* or heavy. In this sense the planets *gravitate* to the Sun, and the secondary planets to their primaries, and, in short, every body in the solar system to every other body. By the verb *to gravitate*, nothing is meant but the fact, that they either actually approach, or manifest, by a very sensible pressure, tendencies to approach the body to which they are said to gravitate. The verb, or the noun, should not be considered as the expression of any quality or property, but merely of a phenomenon, a fact or event in nature.

458. But this deviation from uniform rectilinear motion is considered as an *effect*, and it is of importance to discover

discover the *cause*. Now, in the most familiar instance, the fall or pressure of a heavy body, we ascribe the fall, or pressure indicating the tendency to fall, to its heaviness. But we have no other notion of this heaviness than the very thing which we ascribe to it as an effect. The feeling the heaviness of the piece of lead that lies in our hand, is *the sum of all that we know about it*. But we consider this heaviness as a *property* of all terrestrial matter, because all bodies give some of those appearances which we consider as indications of it. All move toward the Earth if not supported, and all press on the support. The feeling of pressure which a heavy body excites might be considered as its characteristic phenomenon; for it is this feeling that makes us think it a force—we must oppose our force to it; but we cannot distinguish it from the feeling of any other equal pressure. It is most distinguishable as the cause of motion, as a moving or accelerating force. In short, we know nothing of gravity but the phenomena, which we consider, not as gravity, but as its indication. It is, like every other force—an unknown quality.

The *weight* of a body should be distinguished from its gravity or heaviness, and the term should be reserved for expressing the measure of the united gravitation of all the matter in the body. This is indeed the proper sense of the term *weight*—*pondus*. In ordinary business, we measure the weights of bodies by means of known units of weight. A piece of lead is said to be of twenty pounds weight, when it balances twenty pieces of matter, each of which

is a pound; but we frequently measure it by means of other pressures, as when we judge of it by the division to which it draws the scale of a spring steelyard.

459. We estimate the quantity of matter in a body by its weight, and say that there is nineteen times as much matter in a cubic foot of gold as there is in a cubic foot of water. This evidently presupposes that *all matter is heavy, and equally heavy*—that every primitive atom of matter is equally heavy. But this seems to be more than we are entitled to say, without some positive proof. There is nothing inconceivable or absurd in supposing one atom to be twice or thrice as heavy as another. As gravity is a contingent quality of matter, its absolute strength or force is also contingent and arbitrary. We can conceive an atom to have no weight. Nay, we can as clearly conceive an atom of matter to be endowed with a tendency upwards as with a tendency downwards. Accordingly, during the prevalence of the Stahlian doctrine of combustion, that matter which imparts inflammability to bodies was supposed to be not only without weight, but positively light, and to diminish the weight of the other ingredients with which it was combined in a combustible body. In this way, the abettors of that doctrine accounted for the increase of weight observable when a body is burnt.

There is nothing absurd or unreasonable in all this; and had we no other indication of gravity but its pressure, we do not see how this question can be decided. But gravity is not only a pressing power, but also a

moving or accelerating power. If a body consisted of a thousand atoms of gravitating matter, and as many atoms of matter which does not gravitate, and if the gravity of each atom exerted the pressure of one grain, this body would weigh a thousand grains, either by a balance or a spring steelyard, yet it contains two thousand atoms of matter. But take another body of the same weight, but consisting wholly of gravitating atoms; drop these two bodies at once from the hand—the last mentioned will fall 16 feet in the first second—the other will fall only 8 feet. For in both there is the same moving force; therefore the same quantity of motion will be produced in both bodies; that is, the products of the quantities of matter by the velocities generated will be the same. Therefore the velocity acquired by the mixed body will be one half of that acquired in the same time by the simple body. The phenomenon will be what was asserted, one will fall 16 and the other only 8 feet.

This will be still more forcibly conceived, if we take two bodies *a* and *b*, each containing 1000 atoms of gravitating matter, and attach *a* to another body *c*, containing 1000 atoms which do not gravitate. Now, unless we suppose *c* moveable and arrestable by a thought or a word, we can have no hesitation in saying that the mass *a* + *c* will fall with half the velocity of *b*.

We see therefore that the *accelerating power* alone of gravity enables us to decide the question, ‘whether all terrestrial matter gravitates,’ and gravitates alike. We have only to try whether all terrestrial bodies fall equally

far

far in the same time, or receive an equal increment of velocity in the same time. This test of the matter did not escape the penetrating genius of young Newton. He made experiments on every kind of substance, metals, stones, woods, grain, salts, animal substances, &c. and made them in a way susceptible of the utmost accuracy, as we shall see afterwards. The result was, that all these substances were equally accelerated; and, on this authority, Newton thought himself entitled to say that ALL TERRESTRIAL MATTER IS EQUALLY HEAVY.

This however may be disputed. For it is plain that if all bodies contain *an equal proportion* of gravitating and nongravitating matter, they will be equally accelerated; nay, the unequal gravitation of different substances, and even positive levity, may be so compensated by the proportion of those different kinds of matter, that the total gravitation may still be proportional to the whole quantity of matter.

But, till we have some authority for saying that there is a difference in the gravitation of different atoms, the just rules of philosophical discussion oblige us to believe that all gravitate alike. This is corroborated by the universality of the law of mutual and equal reaction. This is next to demonstration that the primitive atoms are alike in every respect, and therefore in their gravitation.

We are entitled therefore to say that all terrestrial matter is equally heavy, and that the weight of a body is the measure of the united gravitation of every atom, and therefore is a measure of, or is proportional to, the quantity of matter contained in it.

460. Newton naturally, and justly, extended the affirmation to the planets and to the Sun. But here arises a question, at once nice and important. The law of gravitation, so often mentioned, is exhibited in the mutual deflections of great masses of matter. These deflections are in the inverse duplicate ratio of the distances between the centres of the masses. Are we warranted by this observation to say that this is also the law of action between every atom of one body and every atom of another? Can we say in general that the law of corpuscular action is the same with that of masses, resulting from the combined action of each atom on each? We are assured by experience that it is not. For we observe that, in magnets, the law of action (that is, the relation subsisting between the distances and the intensities of force) is different in almost every different magnet, and seems to depend in a great measure on their form.

Newton was too cautious, and too good a logician, to advance such a proposition without proof; and therefore, confining himself to the single case of spherical and spheroidal bodies, the forms in which we observe the planetary masses to be compacted, he inquired what sensible action between the masses will result from an action between their particles inversely proportional to the square of their distances.

Let $ALBM$, $albm$ (fig. 49.) be two spherical surfaces, of which C is the common centre, and let the space between them be filled with gravitating matter,
uniformly

uniformly dense. Let p be a particle placed any where within this spherical shell, to every particle of which it gravitates with a force inversely as the square of its distance from it. This particle will have no tendency to move in any direction, because its gravitation in any one direction is exactly balanced by an equal gravitation in the opposite direction.

Draw through p the two straight lines $dp\epsilon$, $ep\delta$, making a very small angle at p . This may represent the section of a very slender double cone dpe , $\delta p\epsilon$, having p for the common vertex, and de , $\delta\epsilon$ for the diameters of the circular bases. The gravitation of p to the matter in the base de is equal to its gravitation to the matter in the base $\delta\epsilon$. For the number of particles in de is to the number in $\delta\epsilon$ as the surface of the base de to that of the base $\delta\epsilon$, that is, as de^2 to $\delta\epsilon^2$, that is, as $p d^2$ to $p \delta^2$, that is, as the gravitation to a particle in $\delta\epsilon$ to the gravitation to a particle in de . Therefore the whole gravitation to the matter in de is the same with the whole gravitation to the matter in $\delta\epsilon$ —since it is also in the opposite direction, the particle p is in equilibrio. The same thing may be demonstrated of the gravitation to the matter in qr and in st , and, in like manner, of the gravitation to the matter in the sections of the cones dpe , $\delta p\epsilon$ by any other concentric surface. Consequently, the gravitation to the whole matter contained in the solid $dgre$ is equal to the gravitation to the whole matter in the solid $\delta t s \epsilon$, and the particle p is still in equilibrio.

Now,

Now, since the lines $dp\epsilon$, $ep\delta$ may be drawn in any direction, and thus be made to occupy the whole sphere, it is evident that the gravitation of p is balanced in every direction, and therefore it has no tendency to move in any direction in consequence of this gravitation to the spherical shell of matter comprehended between the surfaces $ALBM$ and $albm$.

It is also evident that this holds true with respect to all the matter comprehended between $ALBM$ and the concentric surface $pnuv$ passing through p ; in short, p is in equilibrio in its gravitation to all the matter more remote than itself from the centre of the sphere, and appears as if it did not gravitate at all to any matter more remote from the centre.

461. We have supposed the spherical shell to be uniformly dense. But p will still be in equilibrio, although the shell be made up of concentric strata of different density, provided that each stratum be uniformly dense. For, should we suppose that, in the space comprehended between $ALBM$ and $pnuv$, there occurs a surface $albm$ of a different density from all the rest, the gravitation to the intercepted portions qr and st are equal, because these portions are of equal density, and are proportional to pq^2 and ps^2 inversely. The proposition may therefore be expressed in the following very general terms.

*“ A particle placed any where within a spherical shell of
 “ gravitating matter, of equal density at all equal distances
 “ from the centre, will be in equilibrio, and will have no
 “ tendency to move in any direction.”*

Remark.

Remark —The equality of the gravitation to the surface ed and to the surface ϵd is affirmed, because the numbers of particles in the two surfaces are inverfely as the gravitations towards one in each. For the very fame reason, the gravitations to the surfaces ed , and qr , and ts , are all equal. Hence may be derived an elementary propofition, which is of great ufe in all inquiries of this kind ;—namely,

462. If a cone or pyramid dpe , of uniform gravitating matter, be divided by parallel fections de , qr , &c. the gravitation of a particle p in the vertex to each of thofe fections is the fame, and the gravitations to the folids pqr , pde , $qder$, &c. are proportional to their lengths pq , pd , qd , &c. The firft part of this propofition is already demonftrated. Now, conceive the cone to be thus divided into innumerable flices of equal thicknefs. It is plain that the gravitation to each of thefe is the fame, and therefore the gravitation to the folid pqr is to the gravitation to the folid $qder$ as the number of flices in the firft to the number in the fecond, that is, as pq , the length of the firft, to qd , the length of the fecond.

The cone dpe was fuppofed extremely flender. This was not neceffary for the demonftration of the particular cafe, where all the fections were parallel. But in this elementary propofition, the angle at p is fuppofed fmaller than any affigned angle, that the cone or pyramid may be confidered as one of the elements into which we may
 refolve

resolve a body of any form. In this resolution, the bases are supposed, if not otherwise expressly stated, to be parallel, and perpendicular to the axes; indeed they are supposed to be portions xr , ye , $z\epsilon$, &c. of spherical surfaces, having their centres in p . The small portions xrq , yed , $z\epsilon\delta$, &c. are held as insignificant, vanishing in the ultimate ratios of the whole solids.

It is easy also to see that the equilibrium of p is not limited to the case of a spherical shell, but will hold true of any body composed of parallel strata, or strata so formed that the lines pd , $p\delta$ are cut in the same proportion by the sections de , qr , &c. In a spheroidal shell, for example, whose inner and outer surfaces are similar, and similarly posited spheroids, the particle p will be in equilibrium any where within it, because in this case, the lines $p\delta$ and ne are equal; so are the lines $p\epsilon$ and od , the lines $t\delta$ and re , the lines $s\epsilon$ and qd , &c. In most cases, however, there is but one situation of the particle p that will insure this equilibrium. But we may, at the same time, infer the following very useful proposition.

463. *If there be two solids perfectly similar, and of the same uniform density, the gravitation to each of these solids by a particle similarly placed on or in each, is proportional to any homologous lines of the solids.*

For, the solids being similar, they may be resolved into the same number of similar pyramids similarly placed in the solids. The gravitations to each of any corresponding pair of pyramids are proportional to the lengths of

of

of those pyramids. These lengths have the same proportion in every corresponding pair. Therefore the absolute gravitations to the whole pyramids of one solid has the same ratio to the absolute gravitation to the whole pyramids of the other solid. And, since the solids are similar, and the particles are at the similarly placed vertexes of all the similar and similarly placed pyramids, the gravitation compounded of the absolute gravitations to the pyramids of one solid has the same ratio to the gravitation similarly compounded of the absolute gravitations to the pyramids of the other.

464. *The gravitation of an external particle to a spherical surface, shell, or entire sphere, which is equally dense at all equal distances from the centre, is the same as if the whole matter were collected in its centre.*

Let $ALBM$ (fig. 49.) represent such a sphere, and let P be the external particle. Draw $PACB$ through the centre C of the sphere, and cross it by LCM at right angles. Draw two right lines PD, PE , containing a very small angle at P , and cutting the great circle $ALBM$ in D, E, D', E' . About P as a centre, with the distance PC , describe the arch Cdm , cutting DP in d , and EP in e . About the same centre describe the arc DO . Draw dF, eG parallel to AB , and cutting LC in f and g . Draw CK perpendicular to PD , and $dH, D\delta$, and $FI\phi$ perpendicular to AB . Join CD and CF .

Now let the figure be supposed to turn round the axis PB . The semicircumference ALB will generate

a complete spherical surface. The arch Cdm will generate another spherical surface, having P for its centre. The small arches DE , de , FG will generate rings or zones of those spherical surfaces. DO will also generate a zone of a surface having P for its centre. fg and FI will generate zones of flat circular surfaces.

It is evident that the zones generated by DE and DO (which we may call the zones DE and DO), having the same radius $D\delta$, are to each other as their respective breadths DE and DO . In like manner, the zones generated by de , fg , FI , FG , being all at the same distance from the axis AB , are also as their respective breadths de , fg , FI , FG . But the zone DO is to the zone de as PD^2 to Pd^2 . For DO is to de as PD to Pd , and the radius of rotation $D\delta$ is to the radius dH , also as PD to Pd . The circumferences described by DO and de are therefore in the same proportion of PD to Pd . Therefore the zones, being as their breadths and as their circumferences jointly, are as PD^2 and Pd^2 .

CK and dH , being the sines of the same arch Cd , are equal. Therefore KD and fF , the halves of chords equally distant from the centre, are also equal. Therefore the triangles CDK and CFf are equal and similar. But CDK is similar to EDO . For the right angles PDO and CDE are equal. Taking away the common angle CDO , the remainders CDK and EDO are equal. In like manner, CFf and GFI are similar, and therefore (since CDK and CFf are similar) the elementary

mentary triangles $E D O$ and $G F I$ are similar, and $D O : D E = F I : F G$.

The absolute gravitation or tendency of P to the zone $D O$ is equal to its absolute gravitation to the zone $d e$, because the number of particles of the first is to the number in the last in $P D^2$ to $P d^2$, that is, inversely as the gravitation to a particle in the first to the gravitation to a particle in the last. Therefore let c express the circumference of a circle whose radius is r . The surface of the zone generated by $D O$ will be $D O \times c \times D \delta$, and the gravitation to it will be $\frac{D O \times c \times D \delta}{P D^2}$, to which $\frac{d e \times c \times d H}{P d^2}$, or $\frac{d e \times c \times d H}{P C^2}$ is equal. This expresses the absolute gravitation to the zone generated by $D O$, this gravitation being exerted in the direction $P D$.

But it is evident that the tendency of P , arising from its gravitation to every particle in the zone, must be in the direction $P C$. The oblique gravitation must therefore be estimated in the direction $P C$, and must (178.) be reduced, in the proportion of $P d$ to $P H$. It is plain that $P d : P H = d e : f g$, because $d e$ and $f g$ are perpendicular to $P d$ and $P H$. Therefore the reduced or central gravitation of P to the zone generated by $D O$ will be expressed by $\frac{f g \times c \times d H}{P C^2}$.

But the gravitation to the zone generated by $D O$ is to the gravitation to the zone generated by $D E$ as $D O$ to $D E$, that is, as $F I$ (or $f g$) to $F G$. Therefore the central gravitation to the zone generated by $D E$ will be

expressed by $\frac{FG \times c \times dH}{PC^2}$. Now $FG \times c \times dH$ is the value of the surface of the zone generated by FG . And if all this matter were collected in C , the gravitation of P to it would be exactly $\frac{FG \times c \times dH}{PC^2}$, and it would be in the direction PC . Hence it follows that the central gravitation of P to the zone generated by DE , is the same as its gravitation to all the matter in the zone generated by FG , if that matter were placed in C .

What has been demonstrated respecting the arch DE is true of every portion of the circumference. Each has a substitute FG , which being placed in the centre C , the gravitation of P is the same. If PT touch the sphere in T , every portion of the arch TLB will have its substitute in the quadrant LB , and every part of the arch AT has its substitute in the quadrant ATL , as is easily seen. And hence it follows that the gravitation to a particle P to a spherical surface $ALBM$ is the same as if all the matter of that surface were collected in its centre.

We see also that the gravitation to the surface generated by the rotation of AT round AB is equal to the gravitation to the surface generated by TLB , which is much larger, but more remote.

What we have now demonstrated with respect to the surface generated by the semicircle ALB is equally true with regard to the surface generated by any concentric semicircle, such as a/b . It is true, therefore, in regard to the shell comprehended between those surfaces; for
this

this shell may be resolved into innumerable concentric strata, and the proposition may be affirmed with respect to each of them, and therefore with respect to the whole. And this will still be true if the whole sphere be thus occupied.

Lastly, it follows that the proposition is still true, although those strata should differ in density, provided that each stratum is uniformly dense in every part.

It may therefore be affirmed in the most general terms, that a particle P, placed without a spherical surface, shell, or entire sphere, equally dense at equal distances from the centre, tends to the centre with the same force as if the whole matter of the surface, shell, or sphere, were collected there.

This will be found to be a very important proposition, greatly assisting us in the explanation of abstruse phenomena in other departments of natural philosophy.

465. *The gravitation of an external particle to a spherical surface, shell, or entire sphere, of uniform density at equal distances from the centre, is as the quantity of matter in that body, directly, and as the square of the distance from its centre, inversely.*

For, if all the matter were collected in its centre, the gravitation would be the same, and it would then vary in the inverse duplicate ratio of the distance.

466. *Cor. 1.* Particles placed on the surface of spheres of equal density gravitate to the centres of those spheres with forces proportional to the radii of the spheres.

For

For the quantities of matter are as the cubes of the radii. Therefore the gravitation g is as $\frac{d^3}{d^2}$, that is, as d . This is a particular case of Prop. 463.

467. *Cor. 2.* The same thing holds true, if the distance of the external particles from the centres of the spheres are as the diameters or radii of the spheres.

468. *Cor. 3.* If a particle be placed within the surface of a sphere of uniform density, its gravitation, at different distances from the centre, will be as those distances. For it will not be affected by any matter of the sphere that is more remote from the centre (463.); and its gravitation to what is less remote is as its distance from the centre, by the last corollary.

469. *The mutual gravitation of two spheres of uniform density in their concentric strata is in the inverse duplicate ratio of the distance between their centres.*

For the gravitation of each particle in the sphere A to the sphere B is the same as if all the matter in B were collected at its centre. Suppose it so placed. The gravitation of B to A will be the same as if all the matter in A were collected in its centre. Therefore it will be as d^2 inversely. But the gravitation of A to B is equal to that of B to A. Therefore, &c.

470. The absolute gravitation of two spheres whose quantities of matter are a and b , and d the distance of their

their

their centres, is $\frac{a \times b}{d^2}$. For the tendency of one particle of a to b , being the aggregate of its tendencies to every particle of b , is $\frac{b}{d^2}$. Therefore the tendency of the whole of a to b must be $\frac{a \times b}{d^2}$. And the tendency of b to a is equal to that of a to b .

471. This consequence of a mutual gravitation between particles proportional to $\frac{1}{d^2}$, is agreeable to what is observed in the solar system. The planets are very nearly spherical, and they are observed to gravitate mutually in this proportion of the distance between their centres. This mutual action of two spheres could not result from any other law of action between the particles. Therefore we conclude that the particles of gravitating matter of which the planets are formed gravitate to each other according to this law, and that the observed gravitation of the planets is the united effect of the gravitation of each particle to each. There is just one other case, in which the law of corpuscular action is the same with the law of action between the masses; and this is when the mutual action of the corpuscles is as their distance directly. But no such law is observed in all the phenomena of nature.

The general inference drawn by Sir Isaac Newton from the phenomena, may be thus expressed: *Every particle of matter gravitates to every other particle of matter*
with

with a force inversely proportional to the square of the distance from it. Hence this doctrine has been called THE DOCTRINE OF UNIVERSAL GRAVITATION.

The description of a conic section round the focus fully proves that this law of the distances is the law competent to all the gravitating particles. But, whether all particles gravitate, and gravitate alike, is not demonstrated. The analogy between the distance of the different planets and their periodic times only proves that the total gravitation of the different planets is in the same proportion with their quantity of matter. For the force observed by us, and found to be in the inverse duplicate ratio of the distance of the planet, is the *accelerating* force of gravity, being measured by the acceleration which it produces in the different planets. But if one half of a planet be matter which does not gravitate, and the other half gravitates twice as much as the matter of another planet, these two planets will still have their periods and distances agreeable to Kepler's third law. But, since no phenomenon indicates any inequality in the gravitation of different substances, it is proper to admit its perfect equality, and to conclude with Sir Isaac Newton.

472. The general consequence of this doctrine is, that any two bodies, at perfect liberty to move, should approach each other. This may be made the subject of experiment, in order to see whether the mutual tendencies of the planets arise from that of their particles.

For

For it must still be remembered that although this constitution of the particles will produce this appearance, it may arise from some other cause.

Such experiments have accordingly been made. Bodies have been suspended very nicely, and they have been observed to approach each other. But a more careful examination of all circumstances has shewn that most of those mutual approaches have arisen from other causes. Several philosophers of reputation have therefore refused to admit a mutual gravitation as a phenomenon competent to all matter.

But no such approach should be observed in the experiments now alluded to: The mutual approach of two spheres A and B, at the distance D of their centres, must be to the approach to the Earth E at the distance d of their centres in the proportion of $\frac{A \times B}{D^2}$ to $\frac{A \times E}{d^2}$, that is, of $\frac{B}{D^2}$ to $\frac{E}{d^2}$. Therefore, if a particle be placed at the surface of a golden sphere one foot in diameter, its gravitation to the Earth must be more than ten millions of times greater than its gravitation to the gold. For the diameter of the Earth is nearly forty millions of feet, and the density of gold is nearly four times the mean density of the Earth. And therefore, in a second, it would approach less than the ten millionth part of 16 feet—a quantity altogether insensible.

If we could employ in these experiments bodies of sufficient magnitude, a sensible effect might be expected: Suppose T (fig. 50.) to be a ball of equal density with

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the Earth, and two geographical miles in diameter, and let the particle B be at its surface. Its gravity to T will be to its gravitation to the Earth nearly as 1 to 2300, and therefore, if suspended like a plummet, it would certainly deviate $1'$ from the perpendicular. A mountain two miles high, and hemispherical, rising in a level country, would produce the same deviation of the plummet.

474. Accordingly, such deviation of a plumb line has been observed. First by the French academicians employed to measure a degree of the meridian in Peru. Having placed their observatories on the north and south sides of the vast mountain Chimboracao, they found that the plummets of their quadrants were deflected toward the mountain. Of this they could accurately judge, by means of the stars which they saw through the telescope of their quadrant, when they were pointed vertically by means of the plummet.

Thus, if the plummets take the positions AB, CD (fig. 51.), instead of hanging in the verticals AF and CH, a star I, will seem to have the zenith distances ϵI , $g I$, instead of $E I$, $G I$, which it ought to have; and the distance FH on the Earth's surface will seem the measure of the difference of latitude ϵg , whereas it corresponds to EG. The measure of a degree including the space FH, and estimated by the declination of a star I, will be too short, and the measure of a degree terminating either at F or H will be too long, when the space FH is excluded.

Considerable

Considerable doubts remaining as to the inferences drawn from this observation, the philosophers were very desirous of having it repeated. For this reason, our Sovereign, George III., ever zealous to promote true science, sent the Royal astronomer Dr Maskelyne to Scotland, to make this experiment on the north and south sides of Shihallien, a lofty and solid mountain in Perthshire. The deviation toward the mountain on each side exceeded 7"; thus confirming, beyond doubt, the noble discovery of our illustrious countryman.

Perhaps a very sensible effect might be observed at Annapolis-Royal in Nova Scotia, from the vast addition of matter brought on the coast twice every day by the tides. The water rises there above a hundred feet at spring-tide. If a leaden pipe, a few hundred feet long, were laid on the level beach at right angles with the coast, and a glass pipe set upright at each end, and the whole filled with water; the water will rise at the outer end, and sink at the end next the land, as the tide rises. Such an alternate change of level would give the most satisfactory evidence. Perhaps the effect might be sensible on a very long plummet, or even a nice spirit level.

475. A very fine and satisfactory examination was made in 1788 by Mr H. Cavendish. Two leaden balls were fastened to the ends of a slender deal rod, which was suspended horizontally at its middle by a fine wire. This arm, after oscillating some time horizontally by the

twisting and untwisting of the wire, came to rest in a certain position. Two great masses of lead were now brought within a proper distance of the two suspended balls, and their approach produced a deviation of the arms from the points of rest. By the extent of this deviation, and by the times of the oscillations when the great masses were withdrawn, the proportion was discovered between the elasticity of the wire and the gravitation of the balls to the great masses; and a medium of all the observations was taken.

By these experiments, the mutual gravitation of terrestrial matter, even at considerable distances, was most evincingly demonstrated; and it was legitimately deduced from them that the medium density of the Earth was more than five times the density of water. These curious and valuable experiments are narrated in the Philosophical Transactions for 1798.

476. The oblate form of the Earth also affords another proof that gravity is directed, not to any singular point within the Earth, but that its direction is the combined effect of a gravitation to every particle of matter. Were gravity directed to the centre, by any peculiar virtue of that point, then, as the rotation takes away $\frac{1}{89}$ of the gravity at the equator, the equatorial parts of a fluid sphere must rise one half of this, or $\frac{1}{178}$, before all is in equilibrio.

For, suppose CN and CQ (fig. 33.) to be two canals reaching from the pole and from the equator to the centre.

centre. Since the diminution of gravity at Q is observed to be $\frac{1}{289}$, and the gravitation of every particle in CQ is diminished by rotation in proportion to its distance from the axis of rotation, the diminution occasioned in the weight of the whole canal will be one half of the diminution it would sustain if the weight of every particle were as much diminished as that of the particle Q is. Therefore the canal presses less on the centre by $\frac{1}{578}$, and must be lengthened so much before it will balance NC , which sustains no diminution of weight. Every other canal parallel to CQ sustains a similar loss of weight, and must be similarly compensated. This will produce an elliptical spheroidal form.

But the equatorial parts of our globe are much more elevated than this; not less than $\frac{1}{112}$. The reason is this. When the rotation of the Earth has raised the equatorial points $\frac{1}{289}$, the plummet, which at a (fig. 33.) would have hung in the direction aD , tangent to the evolute $ABDF$, is attracted sidewise by the protuberant matter toward the equator. But the surface of the ocean must still be such that the plummet is perpendicular to it. Therefore it cannot retain the elliptical form produced by the rotation alone, but swells still more at the equator; and this still increases the deviation of the plummet. This must go on, till a new equilibrium is produced by a new figure. This will be considered afterwards. No more is mentioned at present than what is necessary for shewing that the protuberance produced by the rotation causes, by its attraction, the plummet to deviate

deviate from the position which it had acquired in consequence of the same rotation.

477. By such induction, and such reasoning, is established the doctrine of universal gravitation, a doctrine which is placed beyond the reach of controversy, and has immortalized the fame of its illustrious inventor.

Sir Isaac Newton has been supposed by many to have assigned this mutual gravitation, or, as he sometimes calls it, this attraction, as a property inherent in matter, and as the *cause* of the celestial phenomena; and for this reason, he has been accused of introducing the occult qualities of the peripatetics into philosophy. Nay, many accuse him of introducing into philosophy a manifest absurdity, namely, that a body can act where it is not present. This, they say, is equivalent with saying that the Sun attracts the planets, or that any body acts on another that is at a distance from it.

Both of those accusations are unjust. Newton, in no place of that work which contains the doctrine of universal gravitation, that is, in his *Mathematical Principles of Natural Philosophy*, attempts to explain the general phenomena of the solar system from the principle of universal gravitation. On the contrary, it is in those general phenomena that he discovers it. The only discovery to which he professes to have any claim is, 1st, the matter of fact, that every body in the solar system is continually deflected toward every other body in it, and that the deflection of any individual body A toward any other
body

body B is *observed* to be in the proportion of the quantity of matter in B directly, and of the square of the distance A B inversely; and, *2dly*, that the falling of terrestrial bodies is just a particular example of this universal deflection. He employs this discovery to explain phenomena that are more particular; and all the explanation that he gives of these is the shewing that they are modified cases of this general phenomenon, of which he knows no explanation but the mere description. Newton was not more eminent for mathematical genius, and penetrating judgement, than for logical accuracy. He uses the word gravitation as the expression, not of a quality, but of a fact; not of a cause, but of an event. Having established this fact beyond the power of controversy, by an induction sufficiently copious, nay without a single exception, he explains the more particular phenomena, by shewing with what modifications, arising from the circumstances of the case, they are included in the general fact of mutual deflection; and, *finally*, as all changes of motion are conceived by us as the effects of force, he says that there is a deflecting force continually acting on every particle of matter in the solar system, and that this deflecting force is what we call weight, heaviness. Few persons think themselves chargeable with absurdity, or with the abetting of occult qualities, when they really consider the heaviness of a body as one of its properties. So far from being occult, it seems one of the most manifest. It is not the heaviness of this body that is the occult quality; it is the cause of this heaviness. In thus
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considering gravity as competent to all matter, Newton does nothing that is not done by others, when they ascribe impulsiveness or inertia to matter. Without scruple, they say that impulsiveness is an universal property of matter. Impulsiveness and heaviness are on precisely the same footing—mere phenomena; and the most general phenomena that we know. We know none more general than impulsiveness, so as to include it, and thus enable us to explain it. Nor do we know any that includes the phenomena of universal deflection, with all the modifications of the heaviness of matter. Whether one of these can explain the other is a different question, and will be considered on another occasion, when we shall see with how little justice philosophers have refused all action at a distance.

But it would seem that there is some peculiarity in this explanation of the planetary motions which hinders it from giving entire satisfaction to the mind. If this be the case, it is principally owing to mistake; to carelessly imputing to Newton views which he did not entertain. His doctrine of universal gravitation does not attempt to explain *how* the operating cause retards the Moon's motion in the first and third quarters of a lunation; it merely narrates in what direction, and with what velocity this change is produced; or rather, it shews how the Moon's deflection toward the Earth, joined to her deflection toward the Sun, both of which are matters of fact, constitute this *seeming* irregularity of motion which we consider as a disturbance. But with respect to the operating cause
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of this general deflection, and the manner in which it produces its effect, so as to explain that effect, Newton is altogether silent. He was as anxious as any person not to be thought to ascribe inherent gravity to matter, or to assert that a body could act on another at a distance, without some mechanical intervention. In a letter to Dr Bentley he expresses this anxiety in the strongest terms. It is difficult to know Newton's precise meaning by the word *action*. In very strict language, it is absurd to say that matter acts at all,—in contact, or at a distance. But, if one should assert that the condition of a particle *a* cannot depend on another particle *b* at a distance from it, hardly any person will say that he makes this assertion from a clear perception of the absurdity of the contrary proposition. Should a person say that the mere presence of the particle *b* is a sufficient reason for *a* approaching it, it will be difficult to prove the assertion to be absurd.

478. Such, however, has been the general opinion of philosophers; and numberless attempts have been made to thrust in some material agent in all the cases of seeming action at a distance. Hence the hypotheses of magnetical and electrical atmospheres; hence the vortices of Des Cartes, and the celestial machinery of Eudoxus and Callippus.

Of all those attempts, perhaps the most rash and unjustifiable is that of Leibnitz, published in the Leipzig Acts 1689, two years after the publication of Newton's

Principia, and of the review of it in those very acts. It may be called rash, because it trusted too much to the deference which his own countrymen had hitherto shewn for his opinions. In this attempt to account for the elliptical motion of the planets, Leibnitz pays no regard to the acknowledged laws of motion. He assumes as principles of explanation, motions totally repugnant to those laws, and motions and tendencies incongruous and contradictory to each other. And then, by the help of geometrical and analytical errors, which compensate each other, he makes out a strange conclusion, which he calls a demonstration of the law of planetary gravitation; and says that he sees that this theorem is known to Mr Newton, but that he cannot tell how he has arrived at the knowledge of it. This is something very remarkable. Newton's process is sufficiently pointed out in the *Acta Eruditorum*, which M. Leibnitz acknowledges that he had seen. A copy of the *Principia* was sent to him, by order of the Royal Society, in less than two months after the publication.—It was soon known over all Europe.

It is without the least foundation that the partisans of M. Leibnitz give him any share in the discovery of the law of gravitation. None of them has ventured to quote this dissertation as a proposition justly proved, nor to defend it against the objections of Dr Gregory and Dr Keill. M. Leibnitz's remarks on Dr Gregory's criticism were not admitted into the *Acta Eruditorum*, though under the management of his particular friends. In October

tober 1706 they inserted an extract from a letter, containing some of those remarks;—if possible, they are more absurd and incongruous than the original dissertation.

It is worth while, as a piece of amusement, to read the account of this dissertation by Dr Gregory in his *Astronomy*, and the observations by Dr Keill in the *Journal Litteraire de la Haye*, August 1714.

479. Sir Isaac Newton has also shewn some disposition to account for the planetary deflection by the action of an elastic æther. The general notion of the attempt is this. The space occupied by the solar system is supposed to be filled with an elastic fluid, incomparably more subtile and more elastic than our air. It is supposed to be of greater and greater density as we recede from the Sun, and in general, from all bodies. In consequence of this, Newton thinks that a planet placed any where in it will be impelled from a denser into a rarer part of the æther, and in this manner have its course incurvated toward the Sun.

But, without making any remarks on the impossibility of conceiving this operation with any distinctness that can entitle the hypothesis to be called an *explanation*, it need only be observed that it is, in its first conception, quite unfit for answering the very purpose for which it is employed, namely, to avoid the absurdity of bodies acting on others at a distance. For, unless this be allowed, an æther of different density and elasticity in its different strata cannot exist. It must either be uniform-

ly dense and elastic throughout, or there must exist a repulsive force operating between very distant particles—perhaps extending its influence as far as the solar influence extends—nay, elasticity without an action *e. distanti*, even between the adjoining particles, is inconceivable. What is meant by elasticity? Surely such a constitution of the assemblage of particles as makes them recede from each other; and the absurdity is as great at the distance of the millionth part of a hair's breadth as at the distance of a million of leagues. If we attempt to evade this, by saying that the particles are in contact, and are elastic, we must grant that they are compressible, and are really compressed, otherwise they are not exerting any elastic force; therefore they are dimpled, and can no more constitute a fluid than so many blown bladders compressed in a box.

The last attempt of this kind that shall be mentioned is that of M. Le Sage of Geneva, put into a better shape by M. Prevôt, in a Memoir published by the Academy of Berlin, under the name of *Lucrece Newtonien*. This philosopher supposes that through every point of space there is continually passing a stream of æther in every direction, with immense rapidity. This will produce no effect on a solitary body; but if there are two, one of them intercepts part of the stream which would have acted on the other. Therefore the bodies, being less impelled on that side which faces the other, will move toward each other. Le Sage adds some circumstances respecting the structure of the bodies, which may give a

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sort of progression in the intensity of the impulse, which may produce a deflection diminishing as the distance or its square increases. But this hypothesis also requires that we make light of the acknowledged laws of motion. It has other insuperable difficulties, and, so far from affording any *explanation* of the planetary motions, its most trifling circumstance is incomparably more difficult to comprehend, or even to conceive, than the most intricate phenomenon in astronomy.

481. Indeed this difficulty obtains in every attempt of the kind, it being necessary to consider the combined motion of millions of bodies, in order to explain the motion of one. But such hypotheses have a worse fault than their difficulty; they transgress a great rule of philosophical disquisition, "never to admit as the cause of a phenomenon any thing of which we do not know the existence." For, even if the legitimate consequences of the hypothesis were agreeable to the phenomena, this only shews the *possibility* of the theory, but gives no explanation whatever. The hypothesis is good, only as far as it agrees with the phenomena; we therefore understand the phenomena as far as we understand the explanation. The *observed* laws of the phenomena are as extensive as our explanation, and the hypothesis is useless. But, alas, none of those hypotheses agree, in their legitimate consequences, with the phenomena; the laws of motion must be thrown aside, in order to employ them, and new laws must be adopted. This is unwise;
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it were much better to give those *pro re nata* laws to the planets themselves.

Mr Cotes, a philosopher and geometer of the first eminence, wrote a preface to the second edition of the *Principia*, which was published in 1713 with many alterations and improvements by the author. In this preface Mr Cotes gives an excellent account of the principles of the Newtonian philosophy, and many very pertinent remarks on the maxim which made philosophers so adverse to the admission of attracting and repelling forces. Whatever may have been Newton's sentiments in early life about the competency of an elastic æther to account for the planetary deflections, he certainly put little value on it afterwards. For he never made any serious use of it for the explanation of any phenomenon susceptible of mathematical discussion. He had certainly rejected all such hypotheses, otherwise he never would have permitted Mr Pemberton to prefix that preface of Mr Cotes to an edition carried on under his own eye. For in this preface the absurdity of the hypothesis of an elastic æther is completely exposed, and it is declared to be a contrivance altogether unworthy of a philosopher. Yet, when Mr Cotes died soon after, Sir Isaac Newton spoke of him in terms of the highest respect. Alas, said he, *we have lost Mr Cotes; had he lived, we should soon have learned something excellent.*

At present the most eminent philosophers and mathematicians in Europe profess the opinion of Mr Cotes, and see no validity in the philosophical maxim that bodies
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cannot act at a distance. M. de la Place, the excellent commentator of Newton, and who has given the finishing stroke to the universality of the influence of gravitation on the planetary motions, by explaining, by this principle, the secular equation of the Moon, which had resisted the efforts of all the mathematicians, endeavours, on the contrary, to prove that an action in the inverse duplicate ratio of the distances results from the very essence or existence of matter. Some remarks will be made on this attempt of M. de la Place afterwards. But at present we shall find it much more conducive to our purpose to avoid altogether this metaphysical question, and strictly to follow the example of our illustrious Instructor, who clearly saw its absolute insignificance for increasing our knowledge of Nature.

Newton saw that any inquiry into the *manner of acting* of the efficient cause of the planetary deflections was altogether unnecessary for acquiring a complete knowledge of all the phenomena depending on the law which he had so happily discovered. Such was its perfect simplicity, that we wanted nothing but the assurance of its constancy—an assurance established on the exquisite agreement of phenomena with every legitimate deduction from the law.

Even Newton's perspicacious mind did not see the number of important phenomena that were completely explained by it, and he thought that some would be found which required the admission of other principles. But the first mathematicians of Europe have acquired

quired most deserved fame in the cultivation of this philosophy, and in their progress have found that there is not one appearance in the celestial motions that is inconsistent with the Newtonian law, and scarcely a phenomenon that requires any thing else for its complete explanation.

Hitherto we have been employed in the establishment of a general law. We are now to shew how the motions actually observed in the individual members of the solar system result from, or are examples of the operation of the power called Gravity, and how its effects are modified, and made what we behold, by the circumstances of the case.—To do this in detail would occupy many volumes; we must content ourselves with adducing one or two of the most interesting examples. The student in this noble department of mechanical philosophy will derive great assistance from *Mr M'Laurin's Account of Sir Isaac Newton's Discoveries*. *Dr Pemberton's View of the Newtonian Philosophy* has also considerable merit, and is peculiarly fitted for those who are less habituated to mathematical discussion. The *Cosmographia* of the *Abbé Frisi* is one of the most valuable works extant on this subject. This author gives a very compendious, yet a clear and perspicuous account of the Newtonian doctrines, and of all the improvements in the manner of treating them which have resulted from the unremitting labour of the great mathematicians in their assiduous cultivation of the Newtonian philosophy. He follows, in general, the geometrical method, and his geometry is elegant.

elegant, and yet he exhibits (also with great neatness) all the noted analytical processes by which this philosophy has been brought into its present state.

What now follows may be called an outline of

The Theory of the Celestial Motions.

482. The first general remark that arises from the establishment of universal and mutual gravitation is that the common centre of the whole system is not affected by it, and is either at rest, or, if in motion, this motion is produced by a force which is external to the system (98.), and acts equally and in the same direction on every body of the system (229.)

483. A force has been discovered pervading the whole system, and determining or regulating the motions of every individual body in it. The problem which naturally offers itself first to our discussion is, to ascertain *what will be the motion of a body, projected from any given point of the solar system, in any particular direction, and with any particular velocity—what will be the form of its path, how will it move in this path, and where will it be at any instant we choose to name.*

Sir Isaac has given, in the 41st proposition of his first book, the solution of this problem, in the most general terms, not limited to the observed law of gravitation, but extended to any conceivable relation between the dis-

tances and the intensity of the force. This is, unquestionably, the most sublime problem that can be proposed in mechanical philosophy, and is well known by the name of the INVERSE PROBLEM OF CENTRIPETAL FORCES.

But, in this extent, it is a problem of pure dynamics, and does not make a part of physical astronomy. Our attention is limited to the centripetal force which connects this part of the creation of God—a force inversely proportional to the square of the distances. It may be stated as follows.

Let a body P, (fig. 52.) which gravitates to the Sun in S, be projected in the direction PN, with the velocity which the gravitation at P to the Sun would generate in it by impelling it along PT, less than PS.

Draw PQ perpendicular to PN. Take PO equal to twice PT, and draw OQ perpendicular to PQ, and QR perpendicular to PS. Also draw Ps, making the angle QPs equal to QPS. Join SQ, and produce SQ till it meet Ps in s.

The body will describe an ellipsis, which PN touches in P, whose foci are S and s, and whose principal parameter is twice PR.

For, draw SN perpendicular to PN. Make PO' = 2PO or = 4PT, and draw O'Q' perpendicular to PO', and describe a circle passing through P, O' and Q'. It will touch PN, because PO'Q' was made a right angle, and therefore PQ' is the diameter of the circle.

We know that an ellipse may be described by a body influenced by gravitation. This ellipse may have S and s

for

for its foci, and PN for a tangent in P , because the angles are equal which PN makes with the two focal lines. This being the case, we know that if PQ , OQ , and QR be drawn as directed in the foregoing construction, $PO'Q'$ is the circle which has the same curvature with the ellipse in P , whose foci are S and s , and tangent PN , and PT is $\frac{1}{4}$ of the chord of curvature in P , and PR is half the parameter of the ellipse. Therefore (212.) PT is the space along which the body must be uniformly impelled by the force in P , that it may acquire the velocity with which the body, actually describing this ellipse, passes through P . If this body, which we shall call A , thus revolves in an ellipse, we should infer that it is deflected toward S , by a force inversely proportional to the square of its distance from S , and of such magnitude in P , that it would generate the velocity with which the body passes through P , by uniformly impelling it along PT .

Now, the other body (which we shall call P) was actually projected in the direction PN , that is, in the direction of A 's motion, with the very velocity with which A passes through P in the same direction, and it is under the influence of a force precisely the same that must have influenced A in the same place. The two bodies A and P are therefore in precisely the same mechanical condition; in the same place; moving in the same direction; with the same velocity; deflected by the same intensity of force, acting in the same direction. Their motions in the next moment cannot be different, and

they must, at the end of the moment, be again in the same condition; and this must continue. A describes a certain ellipse; P must describe the same; for two motions that are different cannot result from the same force acting in the same circumstances.

484. This demonstration is given by Sir Isaac Newton in four lines, as a corollary from the proposition in which he deduces the law of planetary deflection from the motion in a conic section. But it seemed necessary here to expand his process of reasoning a little, because the validity of the inference has been denied by Mr John Bernoulli, one of the first mathematicians of that age. He even hinted that Newton had taken that illogical method, because he could not accommodate his 4th proposition to the particular law of gravitation observed in the system. And he claims to himself the honour of having the first demonstrated that a centripetal force, inversely as the square of the distance, necessarily produces a motion in a conic section. The argument by which he supports this bold claim is very singular, coming from a consummate mathematician, who could not be ignorant of its nullity; so that it was not a serious argument, but a trick to catch the uninformed. Newton, says he, might with equal propriety have inferred, from the description of the logarithmic spiral by a body influenced by a force inversely proportional to the cube of the distance, that a body so deflected will describe the logarithmic spiral, whereas we know that it may describe the
hyperbolic

hyperbolic spiral. Not satisfied with this triumph, he attacks Newton's process in his 41st or general proposition of central forces, saying that it is deduced from principles foreign to the question; and, after all, does not exhibit the body in a state of continued motion, but merely informs us where it will be found, and in what condition, in any assigned moment. He concludes by vaunting his own process as accomplishing all that can be wanting in the problem.

These assertions are the most unfounded and bold vauntings of this vainglorious mathematician; and his own solution is a manifest plagiarism from the writings of Newton, except in the method taken by him to demonstrate the lemma which he as well as Newton premises. Newton's demonstration of this lemma is by the purest principles of free curvilinear motion; and it is, in this respect, a beautiful and original proposition. It makes our § 222. Bernoulli considers it as synonymous with motion on an inclined plane; with which it has no analogy. The solution of the great problem by Bernoulli is, in every principle, and in every step, the same with Newton's; and the only difference is, that Newton employs a geometrical, and Bernoulli an algebraical expression of the proceeding. Newton exhibits continued motion, whereas Bernoulli employs the differential calculus, which *essentially* exhibits only a succession of points of the path. It is worth the student's while to read Dr Keill's Letter to John Bernoulli, and his examination of this boasted solution of the celebrated problem. But it is still more worth

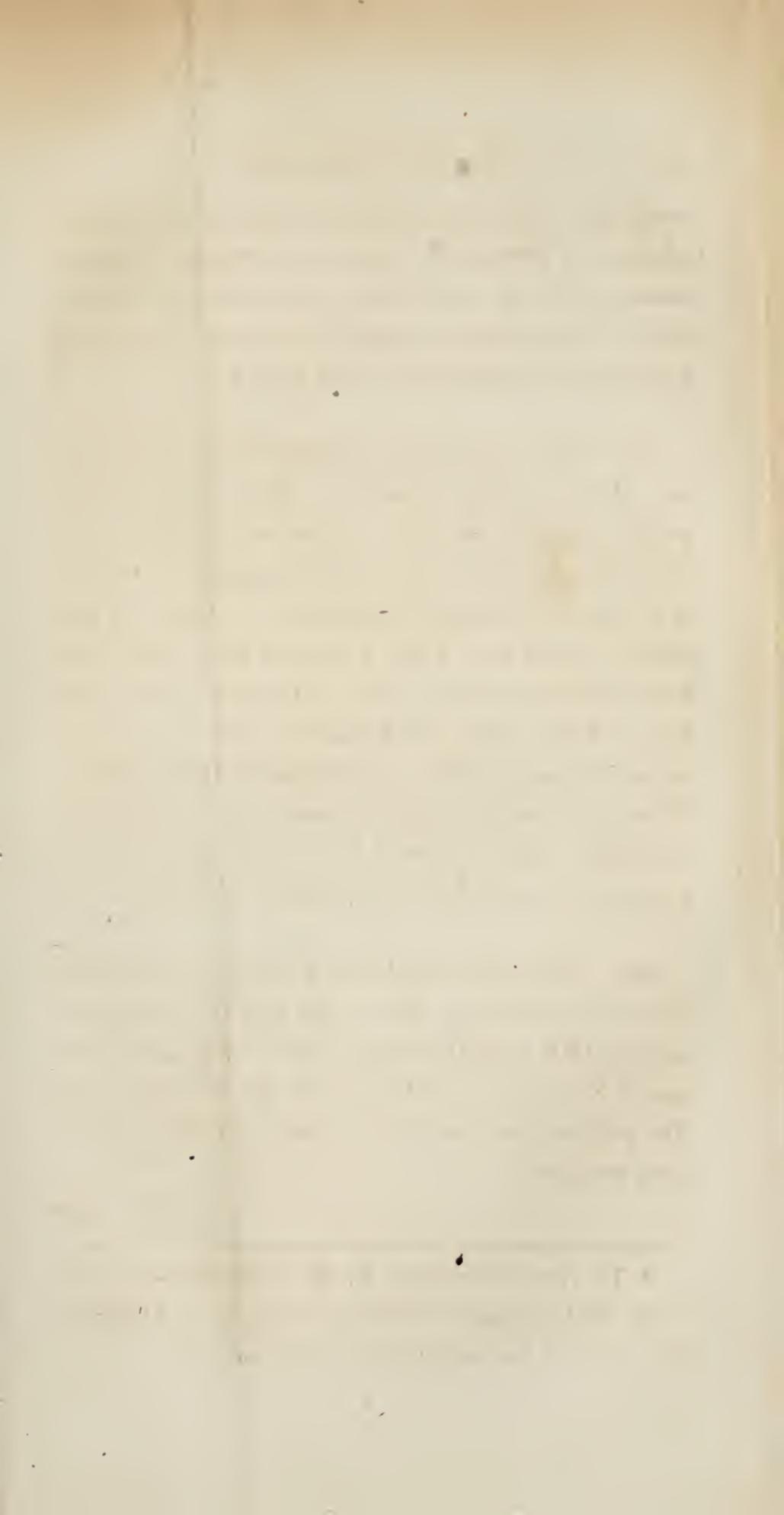
worth his while to read Newton's solution, and the propositions in M'Laurin's Fluxions and Hermann's Phoronomia, which are immediately connected with this problem. This reading will greatly conduce to the forming a good taste in disquisitions of this kind. *

485. Our occupation at present is much more limited. We are chiefly interested to shew that gravitation produces an elliptical motion, when the space PT , along which the body must be uniformly impelled by the force as it exists in P , in order to acquire the velocity of projection, is less than PS . But every step would have been the same, had we made PT equal to PS (as in fig. 52. N^o 2.) But we should then have found that when the angle $QP\prime$ is made equal to QPS , the line $P\prime$ will be parallel to SQ , so that SQ will not intersect it, and the path will not have another focus. It is a parabola, of which PR is the principal parameter.

486. We shall also find that if PT be made greater than PS (as in fig. 52. N^o 3.) the line $P\prime$ (making the angles QPS and $QP\prime$ equal) will cut SQ on the other side of S , so that S and \prime are on the same side of Q . The path will be a hyperbola, of which PR is the principal parameter.

487.

* The propositions given by M. de Moivre in No. 352. of the Philosophical Transactions, and those by Dr Keill in No. 317. and 340. are peculiarly simple and good.



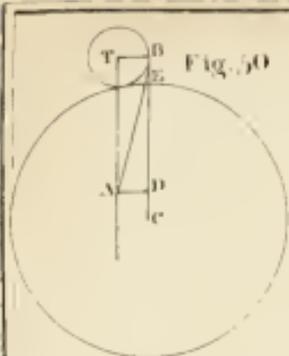


Fig. 50

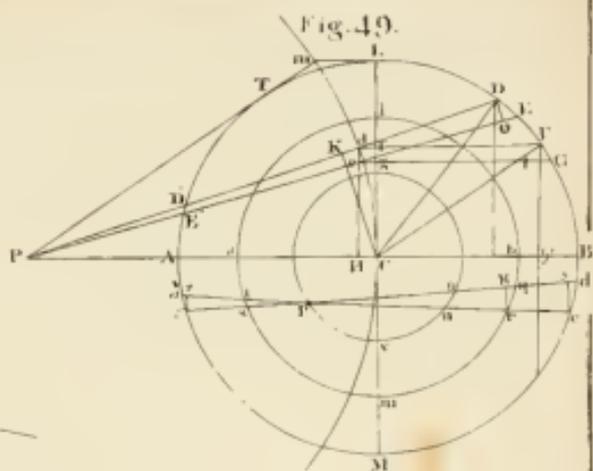


Fig. 49

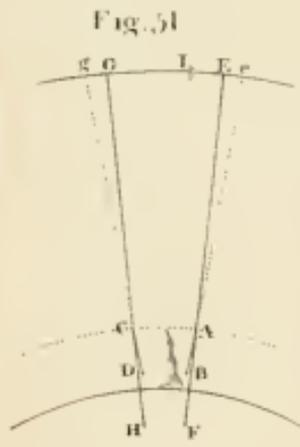


Fig. 51

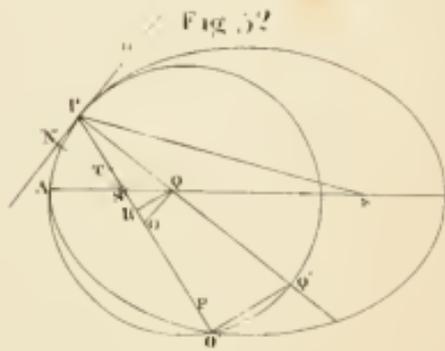


Fig. 52

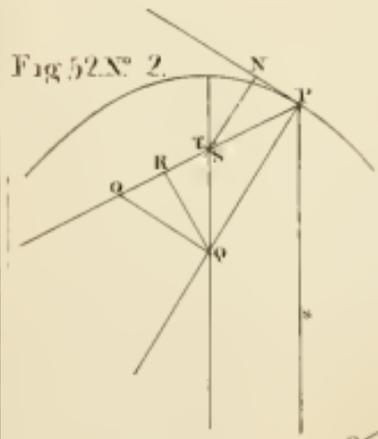


Fig. 52 N° 2.

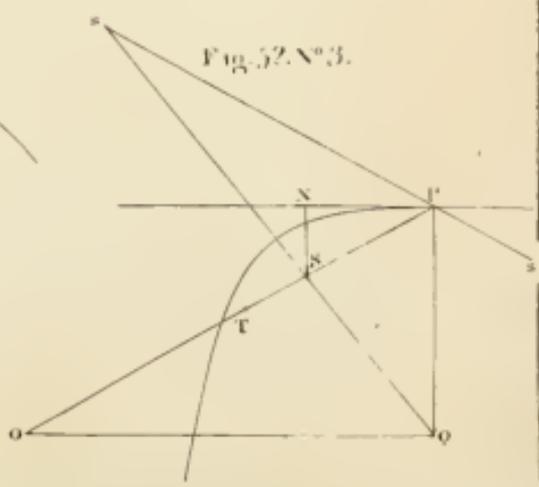


Fig. 52 N° 3.

487. This restriction to the conic sections plainly follows from the line PR , the third proportional to PO and PQ , being the principal parameter, whether the path be an ellipse, parabola, hyperbola, or circle. *

It remains to point out the general circumstances of this elliptical motion, and their physical connexions. For this purpose, the following proposition is useful.

488. When a body describes any curve line $BDPA$ (fig. 53.) by means of a deflecting force directed to a focus S , the angle SPN , which the radius vector makes with the direction of the motion, diminishes, if the velocity in the point P be less than what would enable the body to describe a circle round S , and increases, if the velocity be greater.

If

* The only difficulty in the inference of a conic section as the necessary path of a projectile influenced by a force in the inverse duplicate ratio of the distance from the centre, has arisen from the practice of the algebraic analysts, of defining all curve lines by the relation of an abscissa to parallel ordinates. But this is by no means necessary; and all curves which enclose space, are as naturally referable to a focus, and definable by the relation between the radii and a circular arch. An equation expressing the focal chord of curvature is as distinctive as the usual equation, and leads us with ease to the chief properties of the figure. Therefore

Let SP , the *given* distance, be a , and any indeterminate distance be x . Let the perpendicular SN (also given by SP and

Faint, illegible text, possibly bleed-through from the reverse side of the page. The text is arranged in several paragraphs and is mostly obscured by the low resolution and blurriness of the scan.

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If the velocity of the body in P be less than that which might produce a circular motion round S, then its path will coalesce with the nascent arch P p of a circle whose deflective chord of curvature is less than 2 PS (212.) Let its half be PO, less than PS, and let P p be a very minute arch. Draw the tangents PN, $p n$, and the perpendiculars SN, S π . P q perpendicular to PN will meet $p q$ perpendicular to $p n$ (P p being evanescent) in q the centre of curvature. Draw $p S$ and $p O$.

It is evident that the angles P $q p$ and P O p are ultimately equal, as they stand on the same arch P p of the equicurve

and the given angle S P N) be b , and let p be the perpendicular and q the focal chord of curvature, corresponding to the distance x . Let 4 P T be $= d$. Then (102. 210.) we have

$$\frac{1}{b^2 d} : \frac{1}{p^2 q} = \frac{1}{a^2} : \frac{1}{x^2}$$

$$b^2 d : p^2 q = a^2 : x^2$$

$$b^2 d x^2 = p^2 q a^2$$

$$\text{therefore } q = \frac{b^2 d x^2}{a^2 p^2}, = \frac{b^4}{a^2} d \times \frac{x^2}{p^2}$$

Let $\frac{b^2}{a^2} d = e$ then $q = \frac{e x^2}{p^2}$, which is an equation to a conic section, of which e is the parameter, S the focus, and PN a tangent in P. Now e is a given magnitude, because a, b, d , are all given. Expressing the angle S P N by ϕ , we have $e = d \times \sin.^2 \phi$. See also for the particular case of a force proportional to $\frac{1}{x^2}$ the dissertations by Dr Jo. Keill in the Philosophical Transactions, No. 317. and No. 340.

equicurve circle, and are, respectively, the doubles of the angles at the circumference. $P\zeta p$ is evidently equal to NSn . Therefore POp is equal to NSn , and PSp is less than NSn . Therefore PSN is less than pSn , and SPN is greater than Spn . Therefore the angle SPN diminishes when PO is less than PS , that is, when the velocity in P is less than what would enable the centripetal force in P to retain the body in a circle round S .

On the other hand, if the velocity in P be greater than what suits a circular motion round S , it is plain that PO will be greater than PS , and the angle PSp will be greater than NSn , and the angle PSN greater than pSn , and therefore the angle SPN will be less than Spn , &c.

489. Applying this observation to the case of elliptical motion, we get a more distinct notion of its different affections, and their dependence on their physical causes.

In the half DAB (fig. 46.) of the ellipse described by a planet round the Sun in its focus S , the middle point of the deflective or focal chord of curvature lies between the planet and the focus. Therefore, during the whole motion from D to B , along the semiellipse DAB , the angle contained between the radius vector and the line of the planet's motion is continually diminishing. But during the motion in the semiellipse, BPD , the angle is continually increasing. It is therefore the greatest possible in D , and the smallest in B .

Let the planet set out from its aphelion A, with its due velocity, moving in the direction AF. The velocity in A, being equal to that acquired by a uniform acceleration along $\frac{1}{4}$ of the parameter, is vastly less than what would make it move in the circular arch AL, of which S is the centre, and the planet must fall within that circle. Therefore its path will no longer be perpendicular to the radius vector, but must now make with it an angle somewhat acute. The centripetal force therefore is now resolvable into two forces, one of which accelerates the planet's motion, and the other incurvates its path. Its direction brings it nearer to the Sun. While in the quadrant AFB, the velocity is always less than what is required for a circular motion. For, if from any point F in this quadrant, FG be drawn perpendicular to the tangent, meeting the transverse axis in G, and if GH be drawn perpendicular to the normal FG, HF is one half of the focal chord of curvature, and H lies between P and S. Now, it has been shewn that when this is the case, the angle SF ν diminishes, and, with it, the ratio of S ν to SF (this ratio is that of CB to the semi-diameter CO, the conjugate of CF, (§ 6. Ell.) Consequently, there will be continually more and more of the centripetal force employed in accelerating the motion, and less employed in incurvating the path, the first part being F ν and the other S ν . When the planet arrives at B, the point H falls upon S, and the velocity is precisely what would suffice for a circular motion round S, if the direction of the motion were perpendicular to the

the radius vector. But the direction of the motion brings it still nearer to S. A great part of the centripetal force is still employed in accelerating the motion; and the moment the planet passes B, the velocity becomes greater than what might produce a circular motion round S. For H now lies beyond S from B. Therefore the angle SBN, which is now in its smallest possible state, begins to open again; and this diminishes the proportion of the centripetal force which accelerates the motion, and increases the proportion of the incurvating force. The planet is, however, still accelerated, preserving the equable description of areas. The angle SBN increases with the increasing velocity, and becomes a right angle, when the planet arrives at its perihelion P.

It has been shewn (Ellipse, § 4.) that the chord PI cut off from any diameter PA by the equicurve circle PaI, is equal to the parameter of that diameter. Therefore the centre *o* of this circle lies beyond S. The planet, passing through P, is describing a nascent arch of this circle. Consequently, the curve which it is describing passes without a circle described round S, and the planet is now receding from the Sun. This is usually accounted for, by saying that its velocity is now too great for describing a circle round the Sun. And this is true, when the intensity of the deflecting force is considered. But it has been thought difficult to account for the planet now retiring from the Sun, in the perihelion, where the centripetal force is the greatest of all—greater than what has already been able to bring it

continually nearer to the Sun. We are apt to expect that it will come still nearer. But the fact is, that the planet, in passing through P, is really moving so that, if the Sun were suddenly transferred to o , it would circulate round it for ever. But, in describing the smallest portion of the circle PaI , it goes without the circle which has S for its centre, and its motion now makes an obtuse angle with the radius vector, although it is perpendicular to a radius drawn to o . There is now a portion of the centripetal force employed in retarding the motion of the planet, and its velocity is now diminished; and the angle of the radius vector and the path is now increased, by the same degrees by which they had been increased and diminished during the approach to the Sun. At D, the planet has the same distance from the Sun that it had in B, and the same velocity. The angle SDv is now as much greater than a right angle as SBN was less; and at A, it is reduced to a right angle, and the velocity is again the same as the first. In this way the planet will revolve for ever.

It was shewn in § 223. that in the curvilinear motion of bodies by the action of a central force, the velocities are inversely as the perpendiculars from the centre of forces on the lines of their directions. In the perihelion, the radius vector is perpendicular to the path. The perihelion distance may therefore be taken as the unit of the scale on which all the other velocities are measured. The other velocities may therefore be considered as fractions of the perihelion velocity, which is the greatest of all.

In elliptical motions, the velocities in every point are as the perpendiculars drawn from the other focus on the tangents in that point. For the perpendiculars on any tangent drawn from the two foci are reciprocal.

490. Hence it appears that if a body sets out from P, with the velocity acquired by uniform acceleration along PS, and describes a parabola by means of a centripetal force directed to S, the velocity diminishes without limit. For the perpendicular drawn from the focus on a tangent to a parabola may be greater than any line that can be assigned, if the point in the parabola be taken sufficiently remote from the vertex.

491. If the body set out from P with a velocity exceeding what it would acquire by uniform acceleration along PS, it will describe a hyperbola, and its velocity will diminish continually. But it will never be less than a certain determinable magnitude, to which it continually approximates. For the perpendicular from the focus on the tangent in the most remote point of the hyperbola that can be assigned, is still less than the perpendicular to the asymptote, to which the tangent continually approaches.

But, when the velocity in the perihelion is less than that acquired by uniform acceleration along PS, there will always be a limit to its diminution by the recess from the centre of force. For the velocity being so moderate, the path is more incurvated by the centripetal force;

force; so that the body is made to describe a curve which has an upper apsis *A*, as well as a lower apsis *P*. The body, after passing through *A* at right angles to the radius vector, is now accelerated, because its path now makes an acute angle with the radius vector; and thus the velocity is again increased.

492. The velocity in any point of the ellipse described by a planet is to the velocity that would enable the same force to retain it in a circle at the same distance, in the subduplicate ratio of its distance from the upper focus *f* to the semitransverse axis. That is, calling the elliptic velocity *V*, and the circular velocity *v*, we have $V^2 : v^2 = P f : C A$. (fig. 53.)

For (488.) $V^2 : v^2 = P O : P S$.

But (Ellipse 9.) it was shewn that $P O \times C A$ was equal to CK^2 , $= P S \times P f$. Therefore $P O : P S = P f : C A$ and $V^2 : v^2 = P f : C A$.

493. The angular motion in the ellipse is to the angular motion in a circle at the same distance, and by the action of the same force, in the subduplicate ratio of half the parameter to the distance from *S*.

Take *Pp*, a small arch of the ellipse, and, with the centre *S*, and distance *SP*, describe the circular arch *PzV*, cutting *Sp* in *z*. Make *Pp* to *PV* as the velocity in the ellipse to that in the circle. Then it is plain that *Pz* is to *PV* as the angular motion in the ellipse is to the angular motion in the circle.

The

The angle $z P p$ being the complement of $N P S$ (because NP may be considered as coinciding with pP) it is equal to NSP . Therefore

$$P z^2 : P p^2 = S N^2 : S P^2, = P Q^2 : P O^2$$

$$\text{therefore } P z^2 : P p^2 = P R : P O$$

$$\text{but } P p^2 : P V^2 = P O : P S$$

$$\text{therefore } P z^2 : P V^2 = P R : P S.$$

Cor. The angular motion in the circle exceeds that in the ellipse, when the point R lies between P and S , and falls short of it when R lies beyond S . They are equal when PS is perpendicular to AC , or when the true anomaly of the planet is 90° . For then R and S coincide. Here the approach to S is most rapid.

494. In any point of the ellipse, the gravitation or centripetal force is to that which would produce the same angular motion in a circle, at the same distance from the Sun, as this distance is to half the parameter, that is, as PS to PR .

For, by the last proposition, when the forces in the circle and ellipse are the same, the angular motion in the circle was to that in the ellipse as PV to Pz , which has been shewn to be as \sqrt{PS} to \sqrt{PR} . Therefore, when the angular velocity in the circle, and consequently the real velocity, is changed from PV to Pz , in order that it may be the same with that in the ellipse, the centripetal force must be changed in the proportion of PV^2 to Pz^2 , that is, of PS to PR . Therefore the force which retains the body in the ellipse is to that which will retain

it with the same angular motion in a circle at that distance as P S to P R.

These are the chief affections of a motion regulated by a centripetal force in the inverse duplicate ratio of the distance from the centre of forces. The comparison of them with motions in a circle gives us, in most cases, easy means of stating every change of angular motion, or of approach to or recess from the centre, by means of any change of centripetal force, or of velocity.

Such changes frequently occur in the planetary spaces; and the regular elliptical motion of any individual planet, produced by its gravitation to the Sun, is continually disturbed by its gravitation to the other planets. This disturbance is proportional to the square of the distance from the disturbing planet inversely, and to the quantity of matter in that planet directly. Therefore, before we can ascertain the disturbance of the Earth's motion, for example, by the action of Jupiter, we must know the proportion of the quantity of matter in Jupiter to that in the Sun. This may seem a question beyond the reach of human understanding. But the Newtonian philosophy furnishes us with infallible means for deciding it.

Of the Quantity of Matter in the Sun and Planets.

SINCE it appears that the mutual tendency which we have called **Gravitation** is competent to every particle of
matter,

matter, and therefore the gravitation of a particle of matter to any mass whatever is the sum or aggregate of its gravitation to every atom of matter in that mass, it follows that the gravitation to the Sun or to a planet is proportional to the quantity of matter in the Sun or the planet. As the gravitation may thus be computed, when we know the quantity of matter, so this may be computed when we know the gravitation towards it. Hence it is evident that we can ascertain the proportion of the quantities of matter in any two bodies, if we know the proportion of the gravitations toward them.

495. The tendency toward a body, of which m is the quantity of matter and d the distance, is $\div \frac{m}{d^2}$. It is this tendency which produces deflection from a straight line, and it is measured by this deflection. Now this, in the case of the planets, is measured by the distance at which the revolution is performed, and the velocity of that revolution. We found (224.) that this combination is expressed by the proportional equation $g \div \frac{d}{p^2}$, where p is the periodic time. Therefore we have $\frac{m}{d^2} \div \frac{d}{p^2}$, and, consequently, $m \div \frac{d^3}{p^2}$.

By this means we can compare the quantity of matter in all such bodies as have others revolving round them. Thus, we may compare the Sun with the Earth, by comparing the Moon's gravitation to the Earth with the Earth's gravitation to the Sun. It will be convenient

to consider the Earth as the unit in this comparison with the other bodies of the system.

The Sun's distance in miles is - - - 93726900

The Moon's distance - - - - - 240144

The Earth's revolution (fydereal) days - 365,25

The moon's fydereal revolution (days) - 27,322

Therefore $\frac{93726900^3 \times 27,322^2}{240144^3 \times 365,25^2} = 332669.$

But this must be increased by about $\frac{1}{10}$, because the gravitation to the Earth is stated beyond its real value by the supposition that the revolution of the Moon is performed round the centre of the Earth, whereas it is really performed round their common centre (231.) Thus increased, the Sun's quantity of matter may be estimated at 337422 times that of this Earth.

It must be observed that this computation is not of very great accuracy. It depends on the distance of the Sun; and any mistake in this is accompanied by a similar mistake, but in a triplicate proportion. Now our estimation of the Sun's distance depends entirely on the Sun's horizontal parallax, as measured by means of the transits of Venus. The error of $\frac{1}{10}$ of a second in this parallax, (which is only about 8",7 or 8",8) will induce an error of $\frac{1}{30}$ of the whole.

In like manner, we compare Jupiter with the Earth, by comparing the gravitation of the first satellite with that of the Moon. This makes Jupiter about 313 times more massive than the Earth.

The quantity of matter in Saturn deduced from the
revolution

revolution of his second Cassinian satellite, is about 103 times that of the Earth.

Herschel's planet contains about 17 times as much matter as our globe, as we learn by the revolution of its first satellite.

We have no such means for obtaining a knowledge of the quantity of matter in Venus, Mars, or Mercury. These are therefore only guessed at, by means of certain physical considerations which afford some data for an opinion. Venus is thought to be about $\frac{1}{2}$ of the Earth, Mars about $\frac{1}{4}$, and Mercury about $\frac{1}{10}$. But these are very vague guesses. We judge of the Moon's quantity of matter with some more confidence, by comparing the influence of the Sun and Moon on the tides, and on the precession of the equinoxes. The Moon is supposed about $\frac{1}{80}$ of the Earth.

From this comparison it will appear that the Sun contains nearly 800 times as much matter as all the planets combined into one mass. Therefore the gravitation to the Sun so much exceeds that of any one planet to another, that their mutual disturbances are but inconsiderable.

496. The proportion of the quantities of matter, discovered by this process of reasoning, is very different from what we should have deduced from the observed bulk of the different bodies. Thus, Saturn's diameter being about ten times that of the Earth, we should have inferred that he contained a thousand times as much

matter, whereas he contains only about 103 or 104. We must therefore conclude that the densities of the Sun and planets are very different. Still taking the Earth as the unit of the scale, and combining the ratios of the bulks and the quantities of matter, we may say that the density of the Sun is

density of the Sun is	-	-	-	-	0,25
Venus	-	-	-	-	1,27
Earth	-	-	-	-	1
Mars	-	-	-	-	0,73
Jupiter	-	-	-	-	0,292
Saturn	-	-	-	-	0,184
Georgian Planet	-	-	-	-	0,212

It appears by this statement that the density of the planets is less as they are more remote from the centre of revolution. Herschel's planet is an exception; but a small change on his apparent diameter, not exceeding half a second, will perfectly reconcile them.

497. Knowing the quantity of matter, and the diameter of the bodies of the system, we can easily tell the accelerative force of gravity acting on a body at their surfaces by article 465, that is, what velocity gravity will generate in a second of time, or how far a body will fall in a second. In like manner, we can tell the pressure occasioned by the weight or heaviness of a body, as this may be measured by the scale of a spring steel-yard, graduated by additions of equal known pressures. It cannot be measured by a balance, which only compares one mass of equally heavy matter with another.

Thus,

Thus, the space fallen through, and the apparent weight of a lump of matter, by a spring steelyard, will be

	<i>Fall in 1".</i>	<i>Weight.</i>
At the surface of the Sun	- 451 feet.	28,2
Earth	- 16,09	1
Jupiter	- 41,64	2,6
Saturn	- 14,4	0,89
Herschel	18,7	1,16

Of the Mutual Disturbances of the Planetary Motions.

498. The questions which occur in this department of the study are generally of the most delicate nature, and require the most scrupulous attention to a variety of circumstances. It is not enough to know the direction and intensity of the disturbing force in every point of a planet's motion. We must be able to collect into one aggregate the minute and almost imperceptible changes that have accumulated through perhaps a long tract of time, during which the forces are continually changing, both in direction and in intensity, and are frequently combined with other forces. This requires the constant employment of the inverse method of fluxions, which is by far the most difficult department of the higher geometry, and is still in an imperfect state. These problems have been exclusively the employment of the most eminent

eminent mathematicians of Europe, the only persons who are in a condition to improve the Newtonian philosophy; and the result of their labours has shewn, in the clearest manner, its supreme excellence, and total dissimilitude to all the physical theories which have occupied the attention of philosophers before the days of the admired inventor. For the seeming anomalies that are observed in the solar system are, all of them, the consequences of the universal operation of one simple force, without the interference of any other, and are all susceptible of the most precise measurement and comparison with observation; so that what we choose to call anomalies, irregularities, and disturbances, are as much the result of the general pervading principle as the elliptical motions, of which they are regarded as the disturbances. *

It is in this part of the study also in which the penetrating and inventive genius of Newton appear most conspicuously. The first law of Kepler, the equable description of areas, led the way to all the rest, and made the detection of the law of planetary force a much easier task. But the most discriminating attention was necessary for separating from each other the deviations from simple elliptical motion which result from the mutual gravitation of the planets, and a consummate knowledge of dynamics for computing and summing up all those deviations. The science was yet to create; and it is chiefly to this that the first book of Newton's great work is dedicated. He has given the most beautiful specimen of the investigation in his theory of the lunar inequalities. To every
one

one who has acquired a just taste in mathematical composition, that theory will be considered as one of the most elegant and *pleasing* performances ever exhibited to the public. It is true, that it is but a commencement of a most delicate and difficult investigation, which has been carried to successive degrees of much greater improvement, by the unceasing labours of the first mathematicians. But in Newton's work are to be found all the helps for the prosecution of it, and the first application of his new geometry, contrived on purpose; and all the steps of the process, and the methods of proceeding, are pointed out—all of Newton's invention, *sua mathefi facem preferente*.

It must be farther remarked that the knowledge of the anomalies of the planetary motions is of the greatest importance. Without a very advanced state of it, it would have been impossible to construct accurate tables of the lunar motions. But, by the application of this theory, Mayer has constructed tables so accurate, that by observing the distance of the Moon from a properly selected star, the longitude may be found at sea with an exactness quite sufficient for navigation. This method is now universally practised on board of our East India ships. This requires such accurate theory and tables of the Moon's motion, that we must at all times be able to determine her place within the 30th part of her own diameter. Yet the Moon is subject to more anomalies than any other body in the solar system.

But the study is no less valuable to the speculative philosopher.

philosopher. Few things are more pleasing than the being able to trace order and harmony in the midst of seeming confusion and derangement. No where, in the wide range of speculation, is order more completely effected. All the seeming disorder terminates in the detection of a class of subordinate motions, which have regular periods of increase and diminution, never arising to a magnitude that makes any considerable change in the simple elliptical motions; so that, finally, the solar system seems calculated for almost eternal duration, without sustaining any deviation from its present state that will be perceived by any besides astronomers. The display of wisdom, in the selection of this law of mutual action, and in accommodating it to the various circumstances which contribute to this duration and constancy, is surely one of the most engaging objects that can attract the attention of mankind.

In this elementary course of instruction, we cannot give a detail of the mutual disturbances of the planetary motions. Yet there are points, both in respect of doctrine and of method, which may be called elementary, in relation to this particular subject. It is proper to consider these with some attention.

499. The regularity of the motions of a planet A round the Sun would not be disturbed by the gravitation of both to another planet B, if the Sun and the planet A gravitate to B with equal force, and in the same or in a parallel direction (98.) The disturbance arises entirely

firely from the inequality and the obliquity of the gravitations of the Sun and of the planet A to B. The manner in which these disturbances may be considered, and the grounds of computation, will be more clearly understood by an example.

Let S (fig. 54.) represent the Sun, E the Earth, and J the planet Jupiter. Let it be farther supposed (which may be done without any great error) that the Earth and Jupiter describe concentric circles round the Sun, and that the Sun contains 1000 times as much matter as Jupiter. Make JS to EA as the square of EJ to the square of SJ. Then, if we take SJ to represent the gravitation of the Sun to Jupiter, it is plain that EA will represent the gravitation of the Earth, placed in E, to Jupiter. Draw EB, parallel and equal to JS, and complete the parallelogram EBAD. The force with which Jupiter deranges the motion of the Earth round the Sun will be represented by ED.

For the force EA is equivalent to the combined forces EB and ED. But if the Sun and Earth were impelled only by the equal and parallel forces SJ and EB acting on every particle of each, it is plain that their relative motions would not be affected (98.) It is only by the impulsion arising from the force ED that their relative situations will sustain any derangement.

500. This derangement is of two kinds, affecting either the gravitation of the Earth to the Sun, or her angular motion round him. Let ED be considered as the

diagonal of a rectangle $EFDG$, EG lying in the direction of the radius SE , and EF being in the direction of the tangent to the Earth's orbit. It is plain that the force EG affects the Earth's gravitation to the Sun, while EF affects the motion round him. As EG is in the direction of the radius, it has no tendency to accelerate or retard her motion round the Sun. EF , on the other hand, does not affect the gravitation, but the motion in the curve only.

This disturbing force ED varies, both in direction and magnitude, by a variation in the Earth's position in relation to the Sun and Jupiter. Thus, in fig. A, which represents the Earth as almost arrived at the conjunction with Jupiter, having Jupiter near his opposition to the Sun, the force EG greatly diminishes the Earth's gravitation to the Sun, and the force EF accelerates her motion round him in the order of the letters $ECP OQ$. In fig. B, the force EG still diminishes the Earth's gravitation to the Sun, but EF retards her motion from O to Q . In fig. C, EG increases the Earth's gravitation to the Sun, and EF accelerates her motion round him. It appears very plainly that the motion round the Sun is accelerated in the quadrants QC and PO , and is retarded in the quadrants CP and OQ . We may also see that the gravitation to the Sun is increased in the neighbourhood of the points P and Q , but is diminished in the neighbourhood of C and O , and that there is an intermediate point in each quadrant where the gravitation suffers no change. The greatest diminution of the
Earth's

Earth's gravitation to the Sun must be in C, when Jupiter is nearest to the Earth, in the time of his opposition to the Sun.

We also see very plainly how all these disturbing forces may be precisely determined, depending on the proportion of EI to ES and to SI. Nor is the construction restricted to circular orbits. Each orbit is to be considered in its true figure, and the parallelogram E G D F is not always a rectangle, but has the side EF lying in the direction of the tangent. But we believe that the computation is found to be sufficiently exact without considering the parallelogram E G D F as oblique. The eccentricity of Jupiter's orbit must not be neglected because it amounts to a fourth part of the Earth's distance from the Sun.

We have taken the Sun's gravitation to Jupiter as the scale on which the disturbing forces are measured; but this was for the greater facility of comparing the disturbing forces with each other. But they must be compared with the Earth's gravitation to the Sun, in order to learn their effect on her motions. It will be exact enough for the present purpose of merely explaining the method, to suppose Jupiter's mean distance five times the Earth's from the Sun, and that the quantity of matter in the Sun is 1000 times that of Jupiter. Therefore the Earth's gravitation to the Sun must be 25000 times greater than to Jupiter, when the Earth is about P or Q. When the Earth is at C, her gravitation to Jupiter is increased in the proportion of 4^2 to 5^2 , and it is now $\frac{1}{16000}$ of her gravitation.

gravitation to the Sun. When the Earth is in O, her gravitation to Jupiter is $\frac{1}{180000}$ of her gravitation to the Sun.

But we are not to imagine that when the Earth is at C, her motion relative to the Sun is affected in the same manner as if $\frac{1}{180000}$ of her gravitation were taken away. For we must recollect that the Sun also gravitates to Jupiter, or is deflected toward him, and therefore toward the Earth at C. The diminution of the relative gravitation of the Earth is not to be measured by EA, but by EG. All the disturbing forces EG and EF, corresponding to every position of the Earth and Jupiter, must be considered as fractions of SJ, the measure taken for the mean gravitation to Jupiter. This is $\frac{1}{125000}$ of the Earth's gravitation to the Sun.

Measuring in this way, we shall find that when the Earth is at P or Q her gravitation to the Sun is increased by $\frac{1}{125000}$. For PS or QS will, in this case, come in the place of EG in fig. C, and there will be no such force as EF. At C the Earth's gravitation is diminished $\frac{1}{110000}$, and at O, $\frac{1}{81000}$.

To be able to ascertain the magnitude of the disturbing force in the different situations of the Earth is but a very small part of the task. It only gives us the momentary impulsion. We must ascertain the accumulated effect of the action during a certain time, or along a certain portion of the orbit of the disturbed planet. This is the celebrated *problem of three bodies*, as it is called, which has employed the utmost efforts of the great mathematicians ever since the time that it first appeared in

Newton's

Newton's lunar theory. It can only be solved by approximation; and even this solution, except in some very particular cases, is of the utmost difficulty, which shews, by the way, the folly of all who pretend to *explain* the motions of the planets by the impulsions of fluids, when not three, but millions of particles are acting at once.

We have to ascertain, in the first place, the accumulated effect of the acceleration and retardation of the angular motion of the Earth round the Sun. The general process is one of the two following.

1st, Suppose it required to determine how far the attraction of Jupiter has made the Earth overpass the quadrantal arch QC of her annual orbit. The arch is supposed to be unfolded into a straight line, and divided into minute portions, described in equal times. At each point of division is erected a perpendicular ordinate equal to the accelerating disturbing force EF corresponding to that point. A curve line is drawn through the extremities of those ordinates. The unfolded arch being considered as the representation of the time, and the ordinates as the accelerating forces, it is plain that the area will represent the acquired velocity (70.) Now let another figure be constructed, having an abscissa to represent the time of the motion. But the ordinates must now be made proportional to the areas of the last figure. It is plain, from article 50, that the area of this new figure will represent, or be proportional to the spaces described, in consequence of the action of the disturbing force; and therefore it will express, nearly, the addition
to

to the space described by the undisturbed planet, or the diminution, if the accelerations have been exceeded by the retardations.

The *other* method is to make the unfolded arch the space described, and the ordinates the accelerations, as before. The area now represents the augmentation of the square of the velocity (75.) A second figure is now constructed, having the same abscissa now representing the time. The ordinates are made proportional to the square roots of the areas of the first figure, and they will therefore represent the velocities. The areas of this new figure will represent the spaces, as in the first process, to be added to the arch described by the undisturbed planet, or subtracted from it.

501. All this being a task of the utmost labour and difficulty, the ingenuity of the mathematicians has been exercised in facilitating the process. The penetrating eye of Newton perceived a path which seemed to lead directly to the desired point. All the lines which represent the disturbing forces are lines connected with circular arches, and therefore with the circular motion of the planet. The main disturbing force ED is a function of the angle of commutation CSE , and EF and EG are the sine and cosine of the angle DEG . Newton, in his lunar theory, has given most elegant examples of the summation of all the successive lines EF that are drawn to every point of the arch. Sometimes he finds the sums or accumulated actions of the forces expressed by the

the sine of an arch; sometimes by the tangent; by a segment of the circular area, &c. &c. &c. Euler, D'Alembert, De la Grange, Simpson, and other illustrious cultivators of this philosophy, have immensely improved the methods pointed out and exemplified by Newton, and, by more convenient representations of the forces than this elementary view will admit, have at last made the whole process tolerably easy and plain. But it is still only fit for adepts in the art of symbolical analysis. Their processes are in general so recondite and abstruse that the analyst loses all conception, either of motions or of forces, and his mind is altogether occupied with the symbols of mathematical reasoning.

502. The second part of the task, the ascertaining the accumulated effect of the force EG , is, in general, much more difficult. It includes both the changes made on the radius vector SE , and the change made in the curvature of the orbit. The department of mathematical science immediately subservient to this purpose, is in a more imperfect state than the quadrature of curves. The process is carried on, almost entirely by means of converging serieses. We cannot add any thing here that tends to make it plainer. The lunar theory of Newton, with the commentary of Le Seur and Jacquier, commonly called *the Jesuits' Commentary*, gives very good examples of the methods which must be followed in this process. We must refer to the works of Euler, Clairaut, Simpson, and De la Place, on the perturbations of Jupiter

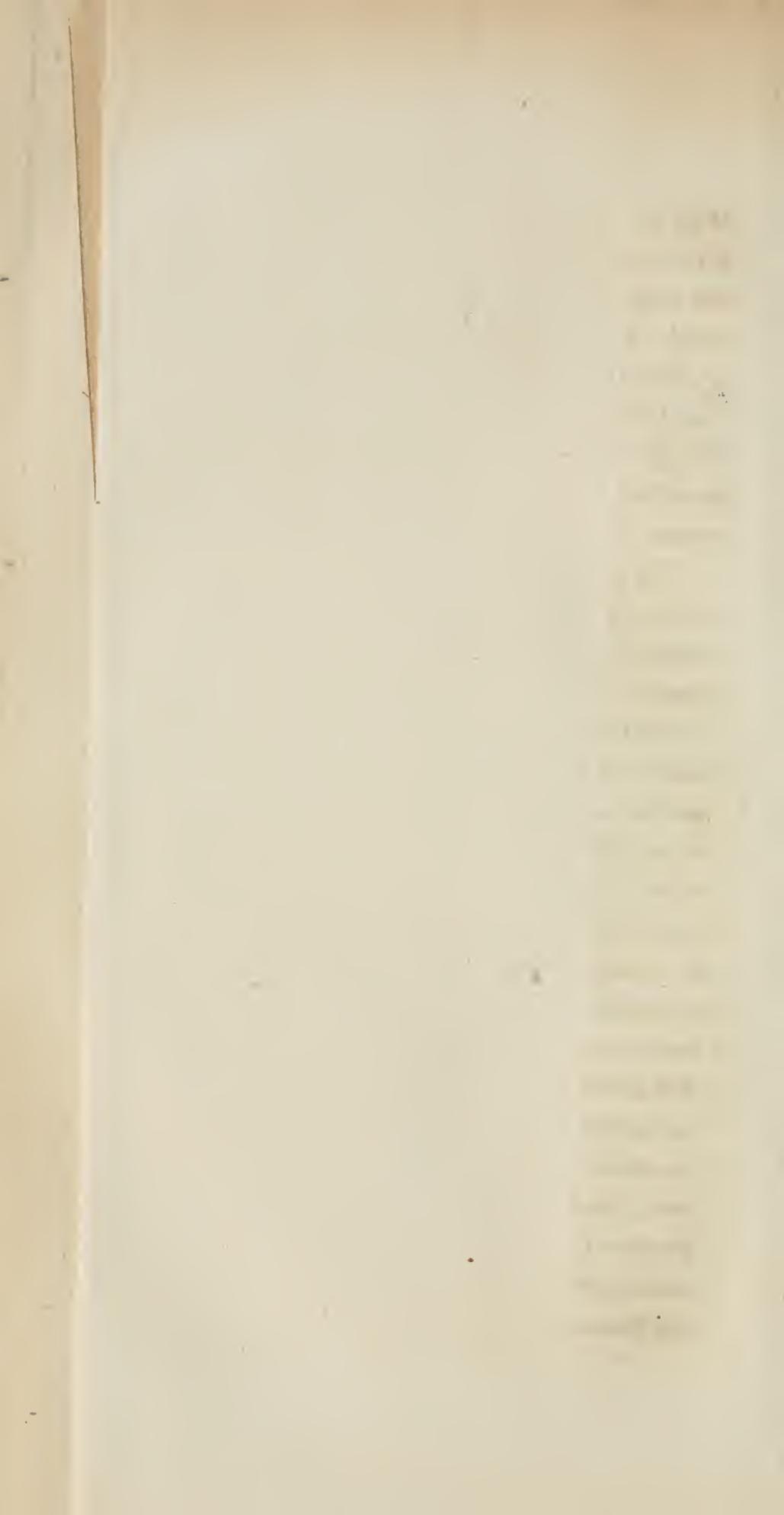
piter and Saturn, &c. and content ourselves with merely pointing out some of the more general and obvious consequences of this mutual action of the planets. La Lande has given in his astronomy a very good synopsis of the most approved method. In the *Traacts Physical and Mathematical*, by Dr Matthew Stewart, and in his *Essay on the Distance of the Sun*, are some beautiful specimens of the geometrical solutions of these problems.

503. When we considering the motion of an inferior planet, disturbed by its gravitation to a superior planet, we see that the inferior planet is retarded in the quadrants CP and OQ, and accelerated in the quadrants PO and QC of its synodical period. Its orbit is more incurvated in the vicinity of the points P and Q, and its curvature is diminished in the vicinity of the points O and C, and most of all in the vicinity of C in the line of conjunction with the superior planet. Therefore, if the aphelion and perihelion of the inferior planet should chance to be near the line JCSO of the synodical motion, these points will seem to shift forward. For, the gravitation of the inferior planet to the Sun being diminished, it will not be able so soon to bend its path to a right angle with the radius vector. On the other hand, should the apfides of the inferior orbit be near the line PSQ, the increase of the inferior planet's gravitation to the Sun must sooner produce this effect, and it will arrive sooner at its aphelion or perihelion, or those points will seem to come westward and to meet it. And thus,

thus, in every synodical revolution, the apsidés of the inferior planet will twice advance and twice retreat, as if the elliptical orbit shifted a little to the eastward or westward. But, as the diminution of the inferior planet's gravitation to the Sun is much greater when it is in the line CSO than the augmentation of it when in the line PSQ, the advances of the apsidés, in the course of a synodical period will exceed the retreats, and, on the whole, they will advance.

The perturbations of the motion of a superior planet by its gravitation to an inferior, are in general opposite, both in kind and in direction, to those of the inferior planet. Therefore, in general, their apsidés retreat.

All these derangements, or deviations from the simple elliptical motion, are distinctly observed in the heavens; and the calculated effect on each planet corresponds with what is observed, with all the precision that can be wished for. It is evident that this calculation must be extremely complicated, and that the effect depends not only on the respective positions, but also on the quantities of matter of the different planets. For these reasons, as Jupiter and Saturn are much larger than any of the other planets, these anomalies are chiefly owing to these two planets. The apsidés of all the planets are observed to advance, except those of Saturn, which sensibly retreat, chiefly by the action of Jupiter. The apsidés of the planet discovered by Dr Herschel doubtless retreats considerably, by the action of the great planets Jupiter and Saturn. It might be imagined that the vast number



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of comets, which are almost constantly without the orbits of the planets, would cause a general advance of all the apfides. But these bodies are so far off, and probably contain so little matter, that their action is insensible.

504. The alternate accelerations and retardations of the planets Mercury, Venus, the Earth, and Mars, in consequence of their mutual gravitations, and their gravitations to Jupiter, nearly compensate each other in every revolution; and no effects of them remain after a long tract of time, except an advance of their apfides. But there are peculiarities in the orbits of Jupiter and Saturn, which occasion very sensible accumulations, and have given considerable trouble to the astronomers in discovering their causes. The period of Saturn's revolution round the Sun increases very sensibly, each being about 7 hours longer than the preceding. On the contrary, the period of Jupiter is observed to diminish about half as much, that is, about $1\frac{1}{2}$ hours in each revolution.

This is owing to the particular position of the aphe-
 lions of those two planets. Let $ABPC$ (fig. 55.) be the elliptical orbit of Jupiter, A being the aphelion and P the perihelion. Suppose the orbit $abpc$ of Saturn to be a circle, having the Sun S in the centre, and let Saturn be supposed to be in a . Then, because Jupiter employs more time (about 140 days) in moving from A to C than in moving from C to P , he must retard the motion of Saturn more than he accelerates him, and Jupiter must

must be more accelerated by Saturn than he is retarded. The contrary must happen if Saturn be in the opposite part p of his orbit. After a tract of some revolutions, all must be compensated, because there will be as many oppositions of Saturn to the Sun on one side of the transverse diameter of Jupiter's orbit as on the other.

But if the orbit of Saturn be an ellipse, as in fig. 55. B, and if the aphelion a be 90 degrees more advanced in the order of the signs than the aphelion A of Jupiter, it is plain that there will be more oppositions of Saturn while Jupiter is moving over the semiellipse ACP, than while he moves over the semiellipse PBA, for Saturn is about 400 days longer in the portion bac of his orbit; and therefore Saturn will, on the whole, be retarded, and Jupiter accelerated.

Now, it is a fact that the aphelion of Saturn is 70 degrees more advanced on the ecliptic than that of Jupiter. Therefore these changes must happen, and the retardations of Saturn must exceed the accelerations. They do so, nearly in the proportion of 353 to 352. This excess will continue for about 2000 years, when the angle ASp will be 90 degrees complete. It will then begin to decrease, and will continue decreasing for 16000 years, after which Saturn will be accelerated, and Jupiter will be retarded. The present retardation of Saturn is about 2', or a day's motion, in a century, and the concomitant acceleration of Jupiter is about half as much. (See *Mem. Acad. Par.* 1746.)

M. de la Place has happily succeeded in account-

ing for several irregularities in this gradual change of the mean motions of these two planets, which had considerably perplexed the astronomers in their attempts to ascertain their periods and their maximum by mere observation. These were accompanied by an evident change in the elliptical equations of the orbit, indicating a change of eccentricity. M. de la Place has shewn that all are precise consequences of universal gravitation, and depend on the *near* equality of five times the angular motion of Saturn to twice that of Jupiter, while the deviation from *perfect* equality of those two motions introduces a variation in these irregularities, which has a very long period (about 877 years). He has at last given an equation, which expresses the motions with such accuracy, that the calculated place agrees with the modern observations, and with the most ancient, without an error exceeding 2'. (See *Mem. Acad. Par.* 1785.)

505. In consequence of the mutual gravitation of the planets, the node of the disturbed planet retreats on the orbit of the disturbing planet. Thus, let EK (fig. 56.) be the plane of the disturbing planet's orbit, and let AB be the path of the other planet, approaching to the node N. As the disturbing planet is somewhere in the plane EK, its attraction for A tends to make A approach that plane. We may suppose the oblique attraction resolved into two forces, one of which is parallel to EK, and the other perpendicular to it. Let this last be such that, in the time that the planet A, if not disturbed,

would

would move from A to B, the perpendicular force would cause it to describe the small space AC. By the combined action of this force AC with the motion AB, the planet describes the diagonal AD, and crosses the plane EK in the point *n*. Thus the node has shifted from N to *n*, in a direction contrary to that of the planet's motion. The planet now proceeds in the line *na*, getting to the other side of the plane EK. The attraction of the disturbing planet now becomes oblique again to the plane, and is partly employed in drawing A (now in *a*) toward the plane. Let this part of the attraction be again represented by a small space *ac*. This, compounded with the progressive motion *ab*, produces a motion in the diagonal *ad*, as if the planet had come, not from *n*, but from N', a point still more to the westward. The node seems again to have shifted in *antecedentiâ signorum*. And thus it appears that, both in approaching the node, and in quitting the node, the node itself shifts its place, in a direction contrary to that of the motion of the disturbed planet.

It is farther observable that the inclination of the disturbed orbit increases while the planet approaches the node, and diminishes during the subsequent recess from it. The original inclination ANE becomes AnE, which is greater than ANE. The angle AnE or *anK* is afterwards changed into aN'K, which is less than *anK*.

In this manner we perceive that when a planet, having crossed the ecliptic, proceeds on the other side of it, the node recedes, that is, the planet moves as if it had

had come from a node situated farther west on the ecliptic; and all the while, the inclination of the orbit to the ecliptic is diminishing. When the planet has got 90° eastward from the node which it quitted, it is at the greatest distance from the ecliptic, and, in its farther progress, it approaches the opposite node. Its path now bends more and more *toward* the ecliptic, and the inclination of its orbit to the ecliptic increases, and it crosses the ecliptic again, in a point considerably to the westward of the point where it crossed it before.

The consequence of this modification of the mutual action of the planets is, that the nodes of all their orbits in the ecliptic recede on the ecliptic, except the node of Jupiter's orbit JJ (fig. 57.), which advances on the ecliptic EK, by retreating on the orbit SS of Saturn, from which Jupiter suffers the greatest disturbance*.

506. We have hitherto considered the ecliptic as a permanent circle of the heavens. But it now appears that the Earth must be attracted out of that plane by the
other

* As this motion of the nodes, and that of the apfides formerly mentioned, become sensible by continual accumulation, and as they are equally susceptible of accurate measure and comparison as the greater gravitations which retain the revolving bodies in their orbits, Mr Machin, professor of astronomy at Gresham College, proposed them as the fittest phenomena for informing us of the distance of the Sun. Dr

Fig. 55.

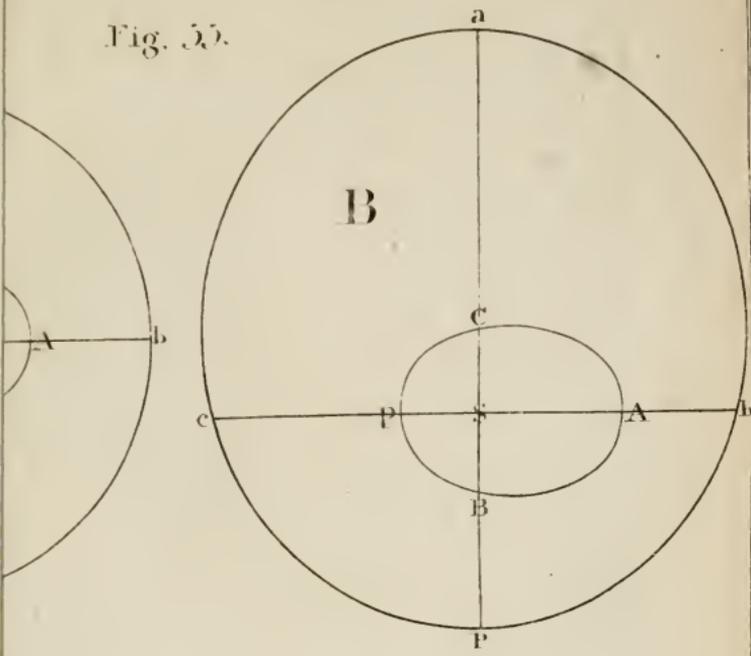


Fig. 56.

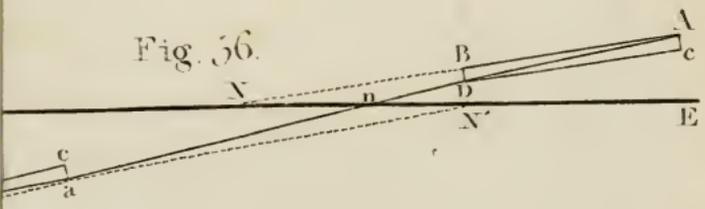
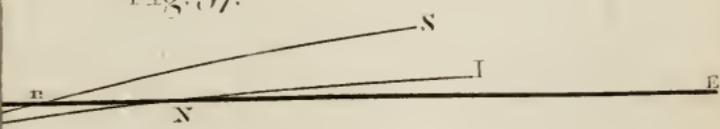


Fig. 57.



61.B.



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Fig. 55.

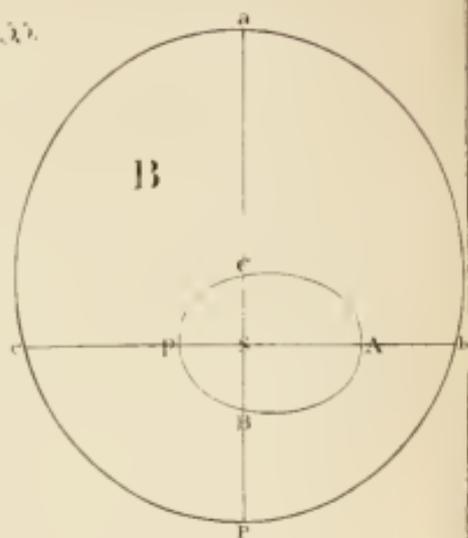
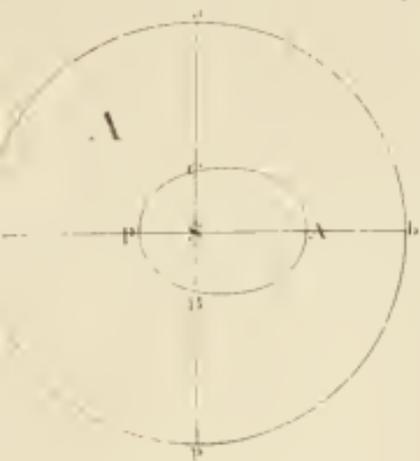


Fig. 56.

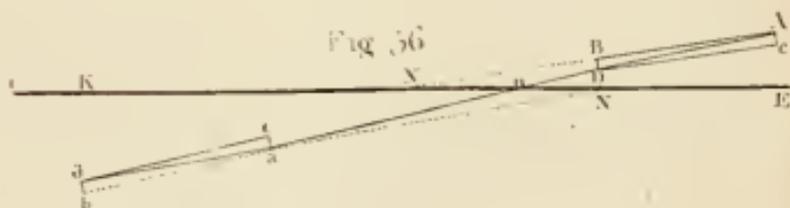
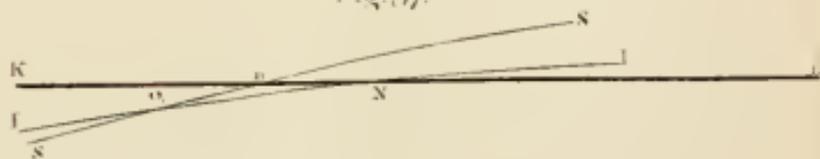
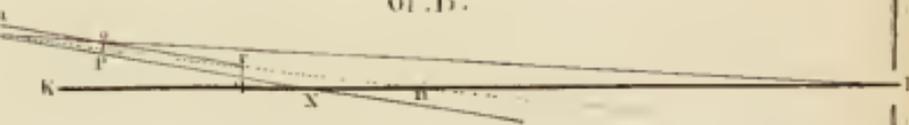


Fig. 57.



Gl. B.



other planets. As we refer every phenomenon to the ecliptic by its latitude and longitude in relation to the apparent path of the Sun, it is plain that this deviation of the Sun from a fixed plane, must change the latitude of all the stars. The change is so very small, however, that it never would have been perceived, had it not been pointed out to the astronomers by Newton, as necessarily following from the universal gravitation of matter. The ecliptic (or rather the Sun's path) has a small irregular motion round two points situated about $7\frac{1}{2}$ degrees westward from our equinoctial points.

507. The comets appear to be very greatly deranged in their motions by their gravitation to the planets. The Halleyan comet has been repeatedly so disturbed, by passing near to Jupiter, that its periods were very considerably altered by this action. A comet, observed in 1770 by Lexel, Prosperin, and other accurate astronomers, has been so much deranged in its motions, that its orbit has been totally changed. Its mean distance, period, and perihelion distance, calculated from good observations, which

Matthew Stewart made a trial of this method, employing chiefly the motion of the lunar apogee, and has deduced a much greater distance than what can be fairly deduced from the transit of Venus. Notwithstanding some oversights in the summations there given of the disturbing forces, the conclusion seems unexceptionable, and the Sun's distance is, in all probability, not less than 110 or 115 millions of miles.

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Matthew Stewart made a trial of this method, employing chiefly the motion of the lunar apogee, and has deduced a much greater distance than what can be fairly deduced from the transit of Venus. Notwithstanding some oversights in the summations there given of the disturbing forces, the conclusion seems unexceptionable, and the Sun's distance is, in all probability, not less than 110 or 115 millions of miles.

which had been continued during three months, agreed with all the observations within 1' of a degree. In its aphelion, it is a small matter more remote than Jupiter, and must have been so near him in 1767 (about $\frac{1}{80}$ of its distance from the Sun) that its gravitation to Jupiter must have been thrice as great as that to the Sun. Moreover, in its revolution following this appearance in 1770, namely on the 23^d of August 1777, it must have come vastly nearer to Jupiter, and its gravitation to Jupiter must have exceeded its gravitation to the Sun more than 200 times. No wonder then that it has been diverted into quite a different path, and that astronomers cannot tell what is become of it. And this, by the way, suggests some singular and momentous reflections. The number of the comets is certainly great, and their courses are unknown. They may frequently come near the planets. The comet of 1764 has one of its nodes very close to the Earth's orbit, and it is very possible that the Earth and it may chance to be in that part of their respective orbits at the same time. The effect of such vicinity must be very remarkable, probably producing such tides as would destroy most of the habitable surface. But, as its continuance in that great proximity must be very momentary, by reason of its great velocity, the effect may not be so great. When the comet of 1770 was so near to Jupiter, it was *in aphelio*, moving slowly, and therefore may have continued some considerable time there. Yet it does not appear that it produced any derangement in the motion of his satellites. We must therefore conclude

clude that either the comet did not continue in the path that was supposed, or that it contained only a very small quantity of matter, being perhaps little more than a dense vapour. Many circumstances in the appearance of comets countenance this opinion of their nature. As they retire to very great distances from the Sun, and in that remote situation move very slowly, they may greatly disturb each other's motion. It is therefore a reasonable conjecture of Sir Isaac Newton that the comet of 1680, at its next approach to the Sun, may really fall into him altogether.

Of the Lunar Inequalities.

508. Of all the heavenly bodies, the Moon has attracted the greatest notice, and her motions have been the most scrupulously examined: and it may be added, that of them all she has been the most refractory. It is but within these few years past that we have been able to ascertain her motions with the precision attained in the cases of the other planets. Not that her apparent path is contorted, like those of Mercury and Venus, running into loops and knots, but because the orbit is continually shifting its place and changing its form; and her real motions in it are accelerated, retarded, and deflected, in a great variety of ways. While the ascertaining the place of Jupiter or Saturn requires the employment of five or six equations, the Moon requires at least forty to

attain the *same* exactness. The corrections introduced by those equations are so various, both in their magnitude and in their periods, and have, of consequence, been so blended and complicated together, that it surpassed the power of observation to discover the greatest part of them, because we did not know the occasions which made them necessary, or the physical connexion which they had with the aspects of the other bodies of the solar system. Only such as arose to a conspicuous magnitude, and had an evident relation to the situation of the Sun, were fished out from among the rest.

509. From all this complication and embarrassment the discovery of universal gravitation has freed us. We have only to follow this into its consequences, as modified by the particular situation of the Moon, and we get an equation, which *must* be made, in order to determine a deviation from simple elliptical motion that *must* result from the action of the Sun. This alone, followed regularly into all its consequences, gives, all the great equations which the sagacity of observers had discovered, and a multitude of other corrections, which no sagacity could ever have detected.

*Discimus hinc tandem quã causã argentea Phœbe
Passibus haud æquis eat, cur subdita nulli
Hactenus astronomo, numerorum frana recusat
Obvia conspicimus, nubem pollente mathefi.*

We have seen (232.) that since the Moon accompanies the Earth in its revolution round the Sun, we must conclude

conclude that she is under the influence of that force which deflects the Earth into that revolution. If, in every instant, the Moon were impelled by precisely the same force which then impels the Earth, and if this force were also in the same direction, the Moon's motion relative to the Earth would not sustain any change (98.) She would describe an accurate ellipse having the Earth in the focus, and would describe areas proportional to the times. But neither of these conditions are agreeable to the real state of things. The Moon is sometimes nearer to the Sun, and sometimes more remote from him than the Earth is, and is therefore more or less attracted by him; and though the distances of both from the Sun are sometimes equal (as when the Moon is in quadrature) the direction of her gravitation to the Sun is then considerably different from that of the Earth's gravitation to him.

These circumstances change considerably all her motions relative to the Earth. But, since the planetary force follows the precise inverse duplicate ratio of the distances, we can tell what its intensity is in every position of the Moon, in what direction it acts, and what deviation it will produce during any interval of time. We may proceed in the following manner.

510. Let S (fig. 59.) represent the Sun, E the Earth, moving in the arch AEB. Let the Moon be supposed to describe round the Earth the circle CBOA. Join ES and MS, and let SM cut the Earth's orbit in N.

Lastly, Let ES be taken as the measure of the Earth's gravitation to the Sun, and as the scale on which we estimate the disturbing forces.

To learn the magnitude and direction of the force which disturbs the Moon's motion when she is in any point M of her orbit, gravitating to the Sun in the direction MS , we must institute the following analogy $MS^2 : ES^2 = ES : MG$. Then it is evident that if the Moon's gravitation to the Sun be represented by ES when she is in the points A or B , equally distant with the Earth, MG will represent her gravitation to the Sun when she is in M ; for it is to ES in the inverse duplicate ratio of the distances from him.

Now this force MG , being neither equal to ES , nor in the same direction, must change or disturb the Moon's motion relative to the Earth. We may suppose MG to result from the combined action of two forces MF and MH (that is, MG may be the diagonal of a parallelogram $MFGH$), of which one, MF , is parallel and equal to ES . Were the Earth and Moon urged by the forces ES and MF only, their relative motions would not be affected (98.) Therefore MH alone disturbs this relative motion, and may be taken for its indication and measure.

The disturbing force may be otherwise represented, by varying the conditions on which the parallelogram $MFGH$ is formed. It may be formed on the supposition that one side of the parallelogram shall have the direction ME . And this is perhaps the best way of resolving

solving MG for the purposes of calculation, and accordingly has been most generally employed by the great geometers who have cultivated this theory. But the method followed in this outline was thought more elementary, and most illustrative of the effects.

The magnitude and direction of this disturbing force depends on the form of the parallelogram $MFGH$, and consequently on the proportion of MF and MG , and on their relative positions. We may obtain an easy expression of the force MH by the consideration that the rate of increase of MS^2 is double of the rate of increase of MS . When a line increases by a very small addition, the ratio of the increment of the line to the line is but the half of that of the square to the square. Thus, let the line MS be supposed 100, and ES 101, differing by one part in a hundred. We have $MS^2 = 10000$, and $ES^2 = 10201$, differing by very nearly two parts in a hundred; the error of this supposition being only one part in ten thousand. Suppose $MS = 1000$, and $ES = 1001$, differing by one part in a thousand. Then $MS^2 = 1000000$, and $ES^2 = 1002001$, differing from MS^2 by two parts in a thousand very nearly, the error of the supposition being only one part in a million, &c. &c.

Now the greatest difference that can occur between ES and MS is at new and full Moon, when the Moon is in C or O . In this case EC is nearly the 390th part of ES , and we have $ES^2 : OS^2 = 390^2 : 391^2$, or $= 390 : 392,026$; and therefore, in supposing ES^2 to OS^2

as 390 to 392, we commit an error of no more than $\frac{1}{40}$ of $\frac{1}{392}$, that is $\frac{1}{15680}$, viz. less than one part in fifteen thousand, in the most unfavourable circumstances. Therefore the difference between NS (or ES) and MG may be supposed equal to MD, without any sensible error, that is, to the double of NM, the difference of NS and MS. Therefore $MG - NS = 2MN$ very nearly, and $MG - MS$, that is, $SG = 3MN$ very nearly. We may also take MI for MH without any sensible error, and may suppose $EI = 3MN$. For the lines MF, IP, HG, being equal and parallel, and SP nearly coinciding with SG, from which it never deviates more than ρ' , EI will nearly coincide with EH, $= SG$, $= 3MN$ nearly.

511. These considerations will give us a very simple manner of representing and measuring the disturbing force in every position of the Moon, which will have no error that can be of any significance. Moreover, any error that inheres in it, is completely compensated by an equal error of an opposite kind in another point of the orbit. Therefore

Let us suppose that the portion of the Earth's path round the Sun sensibly coincides with the straight line AB (fig. 60.) perpendicular to the line OCS, passing through the Sun, and called the line of the SYZIGIES, as AB is called the line of the QUADRATURES. Let MD cross AB at right angles, and produce it to R so that $MP = 3MN$. Join RE, and draw MI parallel to it.

MI

MI will, in all cases, have the position and magnitude corresponding to the disturbing force.

Or, more simply, make $EI = 3MN$, taking the point I on the same side of AB with M , and draw MI . MI is the disturbing force.

512. This force MI may be resolved into two, viz. ML , having the direction of the Moon's motion, and MK , perpendicular to her motion, that is, MK lying in the direction of the radius vector ME , and ML having the direction of the tangent. The force ML affects the Moon's angular motion round the Earth, either accelerating or retarding it, while the force MK either augments or diminishes her gravitation to the Earth.

The disturbing force MI may also be resolved into $MR' = 3MN$, and $R'I$, or ME ; that is, into a force always proportional to MN , and in that direction, and another force in the direction of the Moon's gravitation to the Earth. This is useful on another occasion.

513. When the Moon is in quadrature, the point I coincides with E , because there is no MN . In this case, therefore, the force ML does not exist, and MK coincides with ME . The disturbing force MI is now wholly employed in augmenting the Moon's gravitation to the Earth. The gravitations of the Earth and Moon to the Sun are equal, but not parallel. If ES expresses the magnitude of the Moon's gravitation to the Sun, then ME will express (on the same scale) the augmentation

in quadratures of the Moon's gravitation to the Earth, occasioned by the obliquity of the Sun's action. It is convenient to take this quadrature augment of the Moon's gravitation to the Earth as the unit of the scale on which all the disturbing forces are measured, and to calculate what fraction of her whole gravitation it amounts to.

514. Let G express the Moon's gravitation to the Sun, g her gravitation to the Earth, and g' the increase of this gravitation. Also let y and m be the length of a sydereal year and of a sydereal month. In order to learn in what proportion the Moon's gravitation to the Earth is affected by the disturbing force, it will be convenient to know what proportion its increment in quadrature has to the whole gravitation. We may therefore institute the following proportions.

$$G : g = \frac{D}{p^2} : \frac{d}{p^2} = \frac{ES}{y^2} : \frac{EB}{m^2} *$$

$$g' : G = EB : ES. \text{ Therefore}$$

$$g' : g = \frac{ES \times EB}{y^2} : \frac{EB \times ES}{m^2}, = m^2 : y^2.$$

The

$$* \frac{ES}{y^2} : \frac{EB}{m^2} = \frac{390}{365,256^2} : \frac{1}{27,322^2}, = 2,1833 : 1$$

very nearly. Thus we see that the Moon's gravitation to the Sun is more than twice her gravitation to the Earth. The consequence of this is, that even when the Moon is in conjunction, at new Moon, between the Earth and the Sun, her path in absolute space is concave toward the Sun, and convex toward

The Moon's mean gravitation to the Earth is therefore to its increment in the quadratures by the action of the Sun, in the duplicate ratio of the Earth's period round the Sun to the lunar period round the Earth. This is very nearly in the proportion of 179 to 1. Her gravitation is increased, when in quadrature, about $\frac{1}{179}$. This will diminish the chord of curvature and increase the curvature in the same proportion.

515. In order to see what change it sustains in any other position of the Moon, such as M, join ED, and draw

toward the Earth. Even there she is deflected, not toward the Earth, but toward the Sun. This is a very curious, and seemingly paradoxical assertion. But nothing is better established. The tracing the Moon's motion in absolute space is the completest demonstration of it. It is not a looped curve, as one, at first thinking, would imagine, but a line always concave toward the Sun. Indeed scarcely any things can be more unlike than the real motions of the Moon are to what we first imagine them to be. At new Moon, she appears to be moving to the left, and we see her gradually passing the stars, leaving them to the right; and, calculating from the distance 240000 miles, and the angular motion, about half a degree in an hour, we should say that she is moving to the left at the rate of 38 miles in a minute. But the fact is that she is then moving to the right at the rate of 1100 miles in a minute. But as the Earth, from whence we view her, is moving at the rate of 1140 miles in a minute, the Moon is left behind.

draw DQ perpendicular to EM . It is plain that DQ is the sine of the angle DEQ , which is twice the angle OEQ or CEM , that is, twice the Moon's distance from the nearest syzygy. QE is the cosine of the same angle. The triangles MDQ and EIK are similar. EI is equal to $1\frac{1}{2}MD$. Therefore $EK = 1\frac{1}{2}MQ$, $= 1\frac{1}{2}ME + 1\frac{1}{2}EQ$, using the sign $+$ when DEm is less than 90° , or CEM is less than 45° , and the sign $-$ when CEM is greater than 45° . Therefore $MK = \frac{3}{2}ME + 1\frac{1}{2}EQ$. Therefore, if $\frac{1}{2}ME$ be equal to $1\frac{1}{2}EQ$, that is, if ME be $= 3EQ$, MK is reduced to nothing, or the force MI is then perpendicular to the radius vector, or is a tangent to the circle. The angle CEM , or the arch CM , has then its secant EI equal to thrice its cosine MN . This arch is $54^\circ 44'$. There are therefore four points in the circular orbit distant $54^\circ 44'$ from the line of the syzygies, where the Moon's gravitation to the Earth is not affected by the action of the Sun. If the arch CM exceed this, the point K will lie within the orbit, as in fig. 60. 2. indicating an augmentation of the Moon's gravitation to the Earth.

At B , $1\frac{1}{2}EQ = 1\frac{1}{2}EM$, and therefore $1\frac{1}{2}EQ - \frac{1}{2}EM = EM$, as before.

516. At O and at C , $1\frac{1}{2}EQ + \frac{1}{2}EM = 2EM$. Therefore, in the syzygies, the diminution of the Moon's gravitation to the Earth is double of the augmentation of it in quadratures, or it is $\frac{1}{89\frac{1}{2}}$ of her gravitation to the Earth.

517. With respect to the force ML , it is evidently $= 1\frac{1}{2} DQ$ or $1\frac{1}{2}$ of the sine of twice the Moon's distance from opposition or conjunction. It augments from the syzygy to the octant, where it is a maximum, and from thence it diminishes to nothing in the quadrature. In its maximum state, it is about $\frac{1}{150}$ of the Moon's gravitation to the Earth.

518. It appears, by constructing the figure for the different positions of the Moon in the course of a lunation, that this force ML retards the Moon's motion round the Earth in the first and third quarters CA and OB , but accelerates her motion in the second and last quarters AO and BC . Thus, in fig. 60, ML leads from M in a direction opposite to that of the Moon's motion eastward from her conjunction at C to her first quadrature in A . In fig. 60. 3. ML lies in the direction of her motion; and it is plain that ML will be similarly situated in the quadrants CA and OB , as also in the quadrants AO and BC .

All these disturbing forces depend on the proportion of EB to ES . Therefore, while ES remains the same, the disturbing forces will change in the same proportion with the Moon's distance from the Earth.

519. But let us suppose that ES changes in the course of the Earth's motion in her elliptical orbit. Then, did the Sun continue to act with the same force as before, still the disturbing force would change in the pro-

portion of ES , becoming smaller as ES becomes greater, because the proportion of EB to ES becomes smaller. But, when ES increases, the gravitation to the Sun diminishes in the duplicate ratio of ES . Therefore the disturbing force varies in the inverse proportion of ES^3 , and, in general, is $\div \frac{EB}{ES^3}$. Therefore, as the Earth is nearer to the Sun about $\frac{1}{80}$ in January than in July, it follows that in January all the disturbing forces will be nearly $\frac{1}{80}$ greater than in July.

What has now been said must suffice for an account of the forces which disturb the Moon's motion in the different parts of a circular orbit round the Earth. The same forces operate on the Moon revolving in her true elliptical orbit, but varying, with the Moon's distance from the Earth. They operate in the same manner, producing, not the same motions, but the same changes of motion.

520. It would seem now that it is not a very difficult matter to compute the motion and the place of the Moon for any particular moment. But it is one of the most difficult problems that have employed the talents of the first mathematicians of Europe. Sir Isaac Newton has treated this subject with his usual superiority, in his Principles of Natural Philosophy, and in the separate Essay on the Lunar Theory. But he only began the subject, and contented himself with marking the principal topics of investigation, pointing out the roads that were

to be held in each, and furnishing us with the mathematics and the methods which were to be followed. In all these particulars, great improvements have been made by Euler, D'Alembert, Clairaut, and Mayer of Gottingen. This last gentleman, by a most sagacious examination and comparison of the *data* furnished by observation, and a judicious employment of the physical principles of Sir Isaac Newton, has constructed equations so exactly fitted to the various circumstances of the case, that he has made his lunar tables correspond with observation, both the most ancient and the most recent, to a degree of exactness that is not exceeded in any tables of the primary planets, and far surpassing any other tables of the lunar motions.

We can, with propriety, only make some very general observations on the effects of the continued action of the disturbing forces.

521. In the syzgies and quadrature, the combined force, arising from the Moon's natural gravitation to the Earth and the Sun's disturbing force, is directed to the Earth. Therefore the Moon will, notwithstanding the disturbing force, continue to describe areas proportional to the times. But as soon as the Moon quits those stations, the tangential force ML begins to operate, and the combined force is no longer directed precisely to the Earth. In the octants, where the tangential force is at its maximum, it causes the combined force to deviate
about

about half a degree from the radius vector, and therefore considerably affects the angular motion.

Let the Moon set out from the second or fourth octant, with her mean angular velocity. Therefore ML , then at its maximum, increases continually this velocity, which augments, till the Moon comes to a syzygy. Here the accelerating force ends, and a retarding force begins to act, and the motion is now retarded by the same degrees by which it was accelerated just before. At the next octant, the sum of the retardations from the syzygy is just equal to the sum of the accelerations from the preceding octant. The velocity of the Moon is now reduced to its mean state. But her place is more advanced by $37'$ than it would have been, had the Moon not been affected by the Sun, but had moved from the syzygy with her mean velocity. Proceeding in her course from this octant, the retardation continues, and in the quadrature the velocity is reduced to its lowest state; but here the accelerating force begins again, and restores the velocity to its mean state in the next octant.

Thus, it appears that in the octants, the velocity is always in its medium state, attains a maximum in passing through a syzygy, and is the least possible in quadrature. In the first and third octant, the Moon is $37'$ east, or a-head, of her mean place; and in the second and fourth, is as much to the westward of it; and in the syzgies and quadratures her mean and true places are the same. Thus, when her velocity differs, most from its medium state, her calculated and observed places

are

are the same, and where her velocity has attained its mean state, her calculated and observed places differ most widely. This is the case with all astronomical equations. The motions are computed first in their mean state; and when the changing causes increase to a maximum, and then diminish to nothing, the effect, which is a change of place, has attained its maximum by continual addition or deduction.

522. This alternate increase and diminution of the Moon's angular motion in the course of a lunation was first discovered, or at least distinguished from the other irregularities of her motion, by Tycho Brahé, and by him called the Equation of VARIATION. The deduction of it from the principle of universal gravitation by Sir Isaac Newton is the most elegant and perspicuous specimen of mechanical investigation that is to be seen. The address which he has shewn in giving sensible representations and measures of the momentary actions, and of their accumulated results, in all parts of the orbit, are peculiarly pleasing to all persons of a mathematical taste, and are so apposite and plain, that the investigation becomes highly instructive to a beginner in this part of the higher mathematics. The late Dr Mathew Stewart, in his *Traacts Physical and Mathematical*, following Newton's example, has given some very beautiful examples of the same method.

523. We have hitherto considered the Moon's orbit as circular, and must now inquire whether its form will
suffer

suffer any change. We may expect that it will, since we see a very great disturbing force diminishing its terrestrial gravity in the syzigies, and increasing it in the quadratures. Let us suppose the Moon to set out from a point $35^{\circ} 16'$ short of a quadrature. The force MK , which we may call a centripetal force, begins to act, increasing the deflecting force. This must render the orbit more incurvated in that part, and this change will be continued through the whole of the arch extending $35^{\circ} 16'$ on each side of the quadrature. At $35^{\circ} 16'$ east of a quadrature, the gravity recovers its mean state; but the path at this point now makes an acute angle with the radius vector, which brings the Moon nearer to the Earth in passing through the point of conjunction or opposition. Through the whole of the arch Vv , extending $54^{\circ} 44'$ on each side of the syzigies, the Moon's gravitation is greatly diminished; and therefore her orbit in this place is flattened, or made less curve than the circle, till at v , $54^{\circ} 44'$ east of the syzigy, the Moon's gravity recovers its mean state, and the orbit its mean curvature.

524. In this manner, the orbit, from being circular, becomes of an oval form, most incurvated at A and B , and least so at O and C , and having its longest diameter lying in the quadratures; not exactly however in those points, on account of the variation of velocity which we have shewn to be greatest in the second and fourth quadrants. The longest diameter lies a small matter short

of the points A and B, that is, to the westward of them. Sir Isaac Newton has determined the proportion of the two diameters of this oval, viz. $AB = 70$ and $OC = 69$. It may seem strange that the Moon comes nearest to the Earth when her gravity is most diminished; but this is owing to the incurvation of the orbit in the neighbourhood of the quadratures.

525. The Moon's orbit is not a circle, but an ellipsis, having the Earth in one of the foci. Still, however, the above assertions will apply, by always conceiving a circle described through the Moon's place in the real orbit. But we must now inquire whether this orbit also suffers any change of form by the action of the Sun.

Let us suppose that the line of the apsides coincides with the line of syzgies, and that the Moon is in apogee. Her gravitation to the Earth is diminished in conjunction and opposition, so that, when her gravitation in perigee is compared with her gravitation in apogee, the gravitations differ more than in the inverse duplicate ratio of the distance. The natural forces in perigee and apogee are inversely as the squares of the distance. If the diminutions by the Sun's action were also inversely as the square of the distance, the remaining gravitations would be in the same proportion still. But this is far from being the case here; for the diminutions are directly as the distance, and the greatest quantity is taken from the smallest force. Therefore the forces thus diminished must differ in a greater proportion than before, that is,

in a greater ratio than the inverse of the square of the distances. *

Let the Moon come from the apogee of this disturbed orbit. Did her gravity increase in the due proportion, she would come to the proper perigee. But it increases in a greater proportion, and will bring the Moon nearer to the focus; that is, the orbit will become more eccentric, and its elliptical equation will increase along with the eccentricity. Similar effects will result in the Moon's motion from perigee to apogee. Her apogean gravity being too much diminished, she will go farther off, and thus the eccentricity and the equation of the orbit will be increased. Suppose the Moon to change when in apogee, and that we calculate her place seven days after, when she should be in the vicinity of the quadrature. We apply her elliptical equation (about $6^{\circ} 20'$) to her mean motion. If we compare this calculation with her real

* Thus, let the following perigee and apogee distances be compared, and the corresponding gravitations with their diminutions and remainders.

	<i>Perigee.</i>	<i>Mean.</i>	<i>Apogee.</i>
Distances	8	10	12
Gravitations	144	100	64
Diminutions	2	$2\frac{1}{2}$	3
Remaining gravities	142	$97\frac{1}{2}$	61

Now $12^2 : 8^2 = 142 : 63, 11$. Therefore 142 is to 61 in a much greater ratio than the inverse of the square of the distance.

real place, we shall find the true place almost 2° behind the calculation. We should find, in like manner, that in the last quadrature, her calculated place, by means of the ordinary equation of the orbit, is more than 2° behind the true or observed place. The orbit has become more eccentric, and the motion in it more unequable, and requires a greater equation. This may rise to $7^\circ 40'$, instead of $6^\circ 20'$, which corresponds to the mean form of the orbit.

But let us next suppose that the apfides of the orbit lie in the quadratures, where the Moon's gravitation to the Earth is increased by the action of the Sun. Were it increased in the inverse duplicate ratio of the distances, the new gravities would still be in this duplicate proportion. But, in the present case, the greatest addition will be made to the smallest force. The apogee and perigee gravities therefore will not differ sufficiently; and the Moon, setting out from the apogee in one quadrature, will not, on her arrival at the opposite quadrature, come so near the Earth as she otherwise would have done. Or, should she set out from her perigee in one quadrature, she will not go far enough from the Earth in the opposite quadrature; that is, the eccentricity of the orbit will, in both cases, be diminished, and, along with it, the equation corresponding. Our calculations for her place in the adjacent opposition or conjunction, made with the ordinary orbital equation, will be faulty, and the errors will be of the opposite kind to the former.

The equation necessary in the present case will not exceed $5^{\circ} 3'$.

In all intermediate positions of the apfides, similar anomalies will be observed, verging to the one or the other extreme, according to the position of the line of the apfides. The equation *pro expediendo calculo*, by Dr Halley, contains the corrections which must be made on the equation of the orbit, in order to bring it into the state which corresponds with the present eccentricity of the orbit, depending on the Sun's position in relation to its transverse axis,

526. All these anomalies are distinctly observed, agreeing with the deductions from the effects of universal gravitation with the utmost precision. The anomaly itself was discovered by Ptolemy, and the discovery is the greatest mark of his penetration and sagacity, because it is extremely difficult to find the periods and the changes of this correction, and it had escaped the observation of Hipparchus and the other eminent astronomers at Alexandria during three hundred years of continued observation. Ptolemy called it the Equation of EJECTION, because he explained it by a certain shifting of the orbit. His explanation, or rather his hypothesis for directing his calculation, is most ingenious and refined, but is the least compatible with other phenomena of any of Ptolemy's contrivances.

527. The deduction of this anomaly from its physical principles was a far more intricate and difficult task
than

than the variation which equation had furnished. It is however accomplished by Newton in the completest manner.

It is an interesting case of the great *problem of three bodies*, which has employed, and continues to employ, the talents and best efforts of the great mathematicians. In 1742, Mr Machin gave a pretty theorem, which seemed to promise great assistance in the solution of this problem. Newton had demonstrated that a body, deflected by a centripetal force directed to a fixed point, moved so that the radius vector described areas proportional to the times. Mr Machin demonstrated that if deflected by forces directed to two fixed points, the triangle connecting it with them (which may be called the *plana vectrix*) also described solids proportional to the times. Little help has been gotten from it. The equations founded on it, or to which it leads, are of inextricable complexity.

528. Not only the form, but also the position of the lunar orbit, must suffer a change by the action of the Sun. It was demonstrated (226.) that if gravity decreased faster than in the proportion of $\frac{1}{d^2}$, the apsidal of an orbit will advance, but will retreat, if the gravitation decrease at a slower rate. Now, we have seen that while the Moon is within $54^{\circ} 44'$ of the syzgies, the gravity is diminished in a greater proportion than that of $\frac{1}{d^2}$. Therefore the apsidal which lie in this part of the synodical

synodical revolution must advance. For the opposite reasons, while they lie within $35^{\circ} 16'$ of the quadratures, they must recede. But, since the diminution in syzygy is double of the augmentation in quadrature, and is continued through a much greater portion of the orbit, the apfides must, in the course of a complete lunation, advance more than they recede, or, on the whole, they must advance. They must advance most, and recede least, when near the syzygies; because at this time the diminution of gravity by the disturbing force bears the greatest proportion to the natural diminution of gravity corresponding to the elliptical motion, and because the augmentation in quadrature will then bear the smallest proportion to it, because the conjugate axis of the ellipse is in the line of quadrature.

The contrary must happen when the apfides are near the quadratures, and it will be found that in this case the recess will exceed the progress. In the octants, the motion of the *apfides in consequentia* is equal to their mean motion; but their place is most distant from their true place, the difference being the accumulated sum of the variations.

But, since in the course of a complete revolution of the Earth and Moon round the Sun, the apfides take every position with respect to the line of the syzygies, they will, on the whole, advance. Their mean progress is about three degrees in each revolution.

529. It has been observed, already, that the investigation of the effects of the force MK is much more
difficult

difficult than that of the effects of the force ML . This last, only treating of acceleration and retardation, rarely employs more than the direct method of fluxions, and the finding of the simpler fluents which are expressed by circular arches and their concomitant lines. But the very elementary part of this second investigation engages us at once in the study of curvature and the variation of curvature; and its simplest process requires infinite series, and the higher orders of fluxions. Sir Isaac Newton has not considered this question in the same systematic manner that he has treated the other, but has generally arrived at his conclusions by more circuitous helps, suggested by circumstances peculiar to the case, and not so capable of a general application. He has not even given us the steps by which he arrived at some of his conclusions. His excellent commentators Le Seur and Jaquier have, with much address, supplied us with this information. But all that they have done has been very particular and limited. The determination of the motion of the lunar apogee by the theory of gravity is found to be only one half of what is really observed. This was very soon remarked by Mr Machin, but without being able to amend it; and it remained, for many years, a sort of blot on the doctrine of universal gravitation.

530. As the Newtonian mathematics continued to improve by the united labours of the first geniuses of Europe, this investigation received successive improvements also. At last, M. Clairaut, about the year 1743, considered

considered the problem of these bodies, mutually gravitating, in general terms. But, finding it beyond the reach of our attainments in geometry, unless considerably limited, he confined his attention to a case which suited the interesting case of the lunar motions. He supposed one of the three bodies immensely larger than the other two, and at a very great distance from them; and the smallest of the others revolving round the third in an ellipse little different from a circle; and limited his attention to the *disturbances only* of this motion.—With this limitation, he solved the problem of the lunar theory, and constructed tables of the Moon's motion. But he too found the motion of the apogee only one half of what is observed.—Euler, and D'Alembert, and Simpson, had the same result; and mathematicians began to suspect that some other force, besides that of a gravitation inversely as the square of the distance, had some share in these motions.

At last, M. Clairaut discovered the source of all their mistakes and their trouble. A term had been omitted, which had a great influence in this particular circumstance, but depended on some of the other anomalies of the Moon, with which he had not suspected any connexion. He found that the disturbances, which he was considering as relating to the Moon's motion in the simple ellipse, should have been considered as relating to the orbit already affected by the other inequalities. When this was done, he found that the motion of the apogee, deduced from the action of the Sun, was precisely

cifely what is observed to obtain. Euler and D'Alembert, who were employed in the fame investigation, acceded without scruple¹ to M. Clairaut's improvement of his analysis; and all are now fatisfied with respect to the competency of the principle of univerfal gravitation to the explanation of all these phenomena of the lunar motions.

531. In the whole of the preceding investigation, we have considered the disturbing force of the Sun as acting in the plane of the Moon's orbit, or we have considered that orbit as coinciding with the plane of the ecliptic. But the Moon's orbit is inclined to the plane of the ecliptic nearly 5° , and therefore the Sun is seldom in its plane. His action must generally have a tendency to draw the Moon out of the plane in which she is then moving, and thus to change the inclination of the Moon's orbit to the ecliptic.

But this oblique force may always be resolved into two others, one of which shall be in the plane of the orbit, and the other perpendicular to it. The first will be the disturbing force already considered in all its modifications. We must now consider the effect of the other. *

532.

* It is very difficult to give such a representation of the lunar orbit, inclined to the plane of the ecliptic, that the lines which represent the different affections of the disturbing force

532. Let $ACBO$ (fig. 61.) be the moon's orbit cutting the ecliptic in the line NN' of the nodes, the half $NMAN'$, being raised above the ecliptic, and the other half $NBON'$ being below it. The dotted circle is the orbit, turned on the line NN' till it coincide with the plane of the ecliptic. C, O, A and B are, as formerly, the points of syzygy and quadrature. Let the Moon be in M . Let AEB be the intersection of a plane perpendicular to the ecliptic. Draw Mn perpendicular to the plane AEB , and therefore parallel to the ecliptic, and to OC . Take EI equal to $3Mn$, and join MI . MI is the Sun's disturbing force (511.), and EM measures the augmentation of the Moon's gravitation when in quadrature. It is plain that MI is in a plane passing through ES , and intersecting the lunar orbit in the line ME , and the ecliptic in the line EI . MI therefore does not lie in the plane of the lunar orbit, nor in that of the ecliptic, but is between them both. The force MI may therefore be conceived as resolvable into two forces, one of which lies in the Moon's orbit, and the other is perpendicular to it. This resolution will be effected, if we draw Ii upward from the ecliptic, till it meet the plane of the lunar orbit perpendicularly in i .

Now

may appear detached from the planes of the orbit and ecliptic, and thus enable us to perceive the efficiency of them, and the nature of the effect produced. The most attentive consideration by the reader is necessary for giving him a distinct notion of these circumstances.

Now join Mi , and complete the parallelogram $MiIm$, having MI for its diagonal. The force MI is equivalent to Mi lying in the plane of the Moon's orbit, and Mm perpendicular to it. By the force Mi the Moon is accelerated or retarded, and has her gravitation to the Earth augmented or diminished, while the force Mm draws the Moon out of the plane NCM ; or that plane is made to shift its position, so that its intersection NN' shifts its place a little. The inclination of the orbit to the ecliptic also is affected. Let a plane IiG be drawn through Ii perpendicular to the line NN' of the nodes. The line EG is perpendicular to this plane, and therefore to the lines GI and Gi . Also IiG is a right angle, because Ii was drawn perpendicular to the plane $MiGE$.

Now, if EM be considered as the radius of the tables, Mn is the sine of the Moon's distance from quadrature. Call this q . Then $EI = 3q$. Also making EI radius, IG is the sine of the node's distance from the line of syzygy. Call this s . Also, IG being made radius, Ii or Mm is the sine of the inclination of the orbit to the ecliptic. Call this i .

$$\text{Therefore we have} \quad EM : EI = R : 3q$$

$$EI : IG = R : s$$

$$IG : Mm = R : i$$

$$\text{Therefore} \quad EM : Mm = R^3 : 3qsi$$

$$\text{and} \quad Mm = 3EM \times \frac{qsi}{R^3}.$$

Thus we have obtained an expression of the force Mm , which tends to change the position and inclination

of the orbit. From this expression we may draw several conclusions which indicate its different effects.

Cor. 1. This force vanishes, that is, there is no such force when the Moon is in quadrature. For then q , or the line Mn , is nothing. Now q being one of the numerical factors of the numerator of the fraction $\frac{q s i}{R^3}$, the fraction itself has no value. We easily perceive the physical cause of the evanescence of the force Mm when M comes into the line of quadrature. When this happens, the whole disturbing force has the direction AE , the then radius vector, and is in the plane of the orbit. There is no such force as Mm in this situation of things, the disturbing force being wholly employed in augmenting the Moon's gravitation to the Earth.

2. The force Mm vanishes also when the nodes are in the syzygy. For there, the factor s in the numerator vanishes. We perceive the physical reason of this also. For, when the nodes are in the syzgies, the Sun is in the plane of the orbit; or this plane, if produced, passes through the Sun. In such case, the disturbing force is in the plane of the orbit, and can have no part, Mm acting out of that plane.

3. The chief varieties of the force Mm depend however on s , the sine of the node's distance from syzygy. For in every revolution, q goes through the same series of successive values, and i remains nearly the same in all revolutions. Therefore the circumstance which will most distinguish the different lunations is the situation of the node.

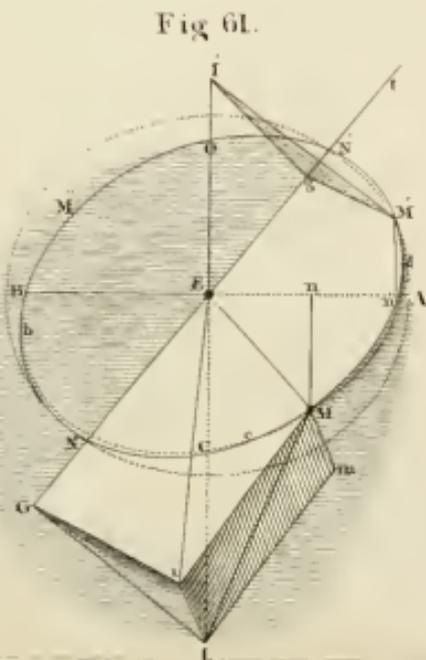
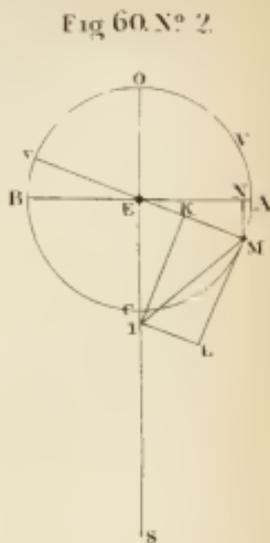
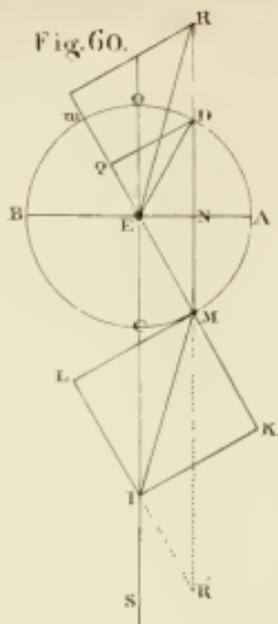
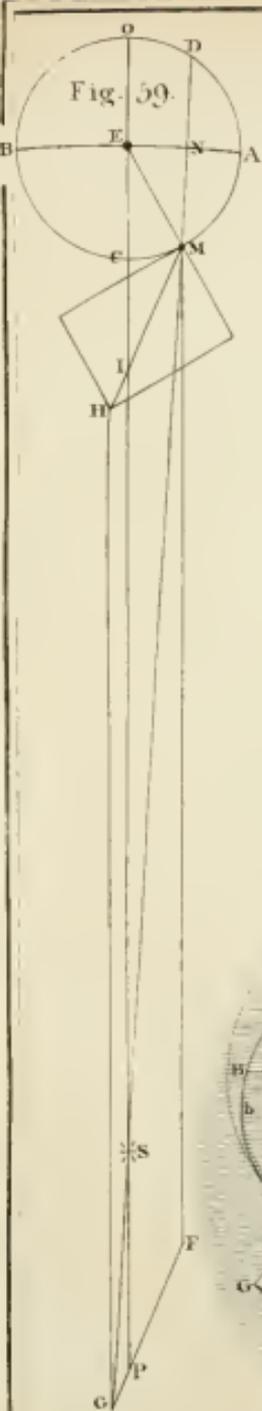
534. This force bends the Moon's path *toward* the ecliptic, when the points M and I are on the same side of the line of the nodes, but bends it away *from* the ecliptic when N lies between I and M. This circumstance kept firmly in mind, and considered with care, will explain all the deviations occasioned by the force Mm . Thus, in the situation of the nodes represented in the figure, let the Moon set out from conjunction in C, moving in the arch CMAO. All the way from C to A, the disturbing force MI is below the elevated half NMN' of the Moon's orbit between it and the ecliptic, and therefore the force Mm pulls the Moon out of the plane of her orbit toward the ecliptic. The same thing happens during the Moon's motion from N to C. This will appear by constructing the same kind of parallelogram on the diagonal MI drawn from any point between N and C.

When the Moon has passed the quadrature A, and is in M', the force M'I' is both above the ecliptic, and above the elevated half of the Moon's orbit. This will appear by drawing M'g perpendicular to EN', and joining gI'. The line M'g is in the orbit, and gI' is in the ecliptic, and the triangle M'gI' stands elevated, and nearly perpendicular on both planes, so that M'I' is above them both. In this case, the force M'm' in pulling the Moon out of the plane of her orbit, separates her from it on that side which is most remote from the ecliptic; that is, causes the path to approach more obliquely to the ecliptic. The figure 61. B will illustrate this.

N'I'

$N'I'$ is the ecliptic, and $M'N'$ is the orbit, both seen edge-wise, as they would appear to an eye placed in t , (fig. 61.) in the line NN' produced beyond the orbit. The disturbing force, acting in the direction $M'I'$, may be resolved into $M'p$ in the direction of the orbit plane, and $M'm'$ perpendicular to it. The part $M'm'$, being compounded with the simultaneous motion $M'q$, composes a motion $M'r$, which intersects the ecliptic in n . When M' in fig. 61. gets to M'' , the path is again bent toward the ecliptic, and continues so all the way from N' to B , where it begins to act in the same manner as in M' between A and N' .

535. By the action of this lateral force, the orbit must be continually shifting its position, and its intersection with the ecliptic; or, to speak more accurately, the Moon is made to move in a line which does not lie all in one plane. In imagination, we conceive an orbital material line, somewhat like a hoop, of an elliptical shape, all in one plane, passing through the Earth, and, instead of conceiving the Moon to quit this hoop, we suppose the hoop itself to shift its position, so that the arch in which the Moon is in any moment takes the direction of the Moon's motion in that moment. Its intersection with the ecliptic (perhaps at a considerable distance from the point occupied by the Moon) shifts accordingly. This hoop may be conceived as having an axis, perpendicular to its plane, passing through the Earth. This axis will incline to one side from the pole of the ecliptic about five degrees, and,

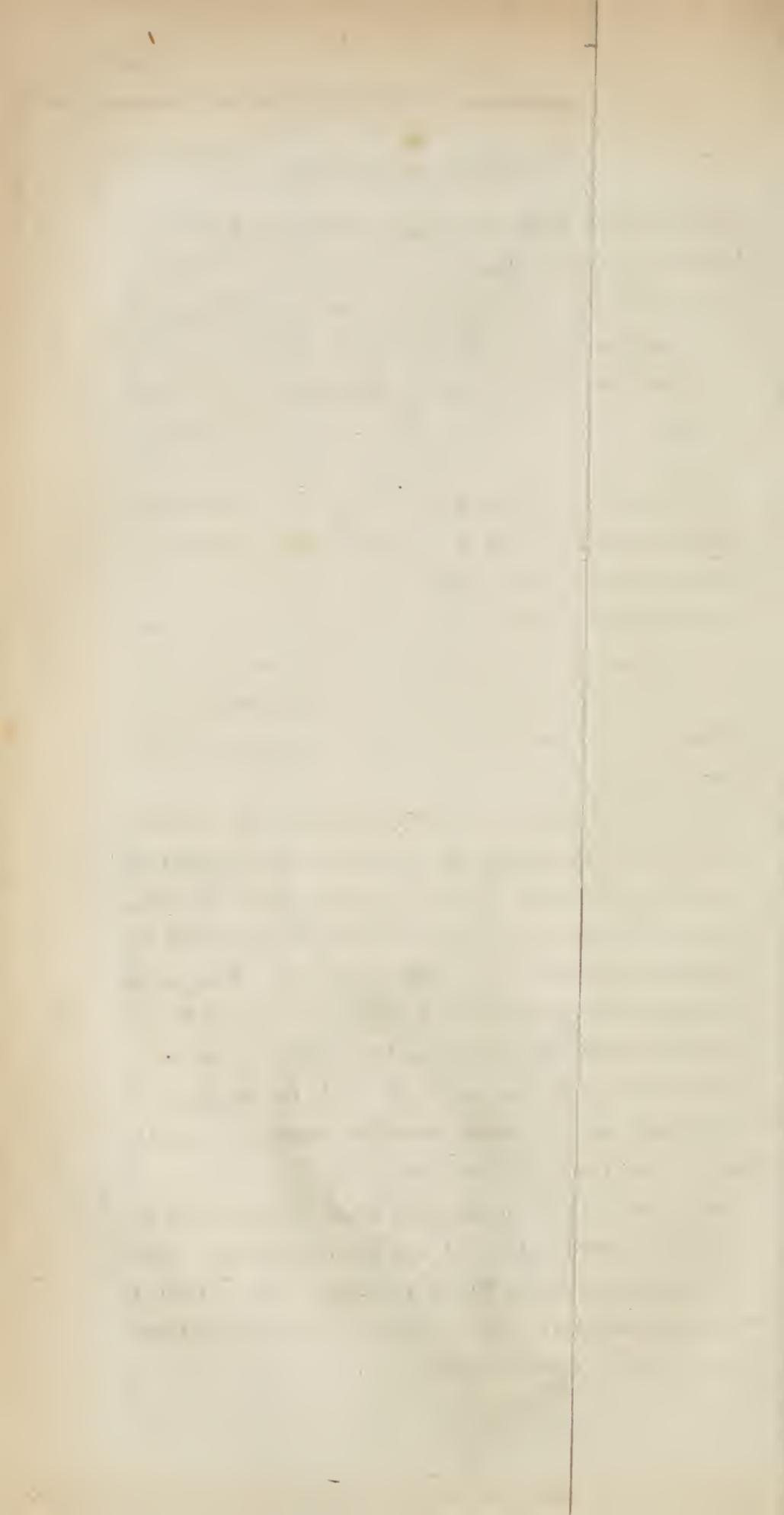


and, as the line NN' of the nodes shifts round the ecliptic, the extremity of this axis will describe a circle round the pole of the ecliptic, distant from it about 5° all round, just as the axis of the Earth describes a circle round the pole of the ecliptic, distant from it about $23\frac{1}{2}$ degrees.

536. When the Moon's path is bent toward the ecliptic, she must cross it sooner than she would otherwise have done. The node will appear to meet the Moon, that is, to shift to the westward, *in antecedentiâ signorum*, or to recede. But if her path be bent more away from the ecliptic, she must proceed farther before she crosses it, and the nodes will shift *in consequentiâ*, that is, will advance.

Cor. 1. Therefore, if the nodes have the situation represented in the figure, in the second and fourth quadrant, the nodes must retreat while the Moon describes the arch NCA, or the arch N'OB, that is, while she passes from a node to the next quadrature. But, while the Moon describes the arch AN', or the arch BN, the force which pulls the Moon from the plane of the orbit, causes her to pass the points N' or N before she reach the ecliptic, and the node therefore advances, while the Moon moves from quadrature to a node.

It is plain that the contrary must happen when the nodes are situated in the first and third quadrants. They will advance while the Moon proceeds from a node to the next quadrature, and recede while she proceeds from a quadrature to the next node.



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Cor. 2. In each synodical revolution of the Moon, the nodes, on the whole, retreat. For, to take the example represented in the figure, all the while that the Moon moves from N to A, the line MI lies between the orbit and ecliptic, and the path is continually inclining more and more towards it, and, consequently, the nodes are all this while receding. They advance while the Moon moves from A to N'. They retreat while she moves from N' to B, and advance while she proceeds from B' to N. The time therefore during which the nodes recede exceeds that during which they advance. There will be the same difference or excess of the regress of the nodes when they are situated in the angle CEA.

It is evident that the excess of the arch NCA above the arch BN or AN', is double of the distance NC of the node from syzgy. Therefore the retreat or westerly motion of the nodes will gradually increase as they pass from syzgy to quadrature, and again decrease as the node passes from quadrature to the syzgy.

Cor. 3. When the nodes are in the quadratures, the lateral force Mm is the greatest possible through the whole revolution, because the factor s in the formula $\frac{q s i}{r^3}$ is then equal to radius. In the syzgies it is nothing.

The nodes make a complete revolution in $6803^d 2^h 55' 18''$, but with great inequality, as appears from what has been said in the preceding paragraphs. The exact determination of their motions is to be seen in Newton's Principia, B. III. Prop. 32.; and it is a very beautiful example

ample of dynamical analysis. The principal equation amounts to $1^{\circ} 37' 45''$ at its maximum, and in other situations, it is proportional to the sine of twice the arch NC . The annual regress, computed according to the principles of the theory, does not differ two minutes of a degree from what is actually observed in the heavens. This wonderful coincidence is the great boast of the doctrine of universal gravitation. At the same time, the perusal of Newton's investigation will shew that such agreement is not the *obvious* result of the happy simplicity of the great regulating power; we shall there see many abstruse and delicate circumstances, which must be considered and taken into the account before we can obtain a true statement.

This motion of the nodes is accompanied by a variation of the inclination of the orbit to the ecliptic. The inclination increases, when the Moon is drawn from the ecliptic while leaving a node, or toward it in approaching a node. It is diminished, when the Moon is drawn toward the ecliptic when leaving a node, or from it in approaching a node. Therefore, when the nodes are situated in the first and third quadrants, the inclination increases while the Moon passes from a node to the next quadrature, but it diminishes till she is 90° from the node, and then increases till she reaches the other node. Therefore, in each revolution, the inclination is increased, and becomes continually greater, while the node recedes from the quadrature to the syzygy; and it is the greatest possible when the nodes are in the line of the

3 H syzigies.

syzigies, and it is then nearly $5^{\circ} 18' 30''$. When the nodes are situated in the second and fourth quadrants, the inclination of the orbit diminishes while the Moon passes from the node to the 90th degree; it is increased from thence to the quadrature, and then diminishes till the Moon reaches the other node. While the nodes are thus situated, the inclination diminishes in every revolution, and is the least of all when the node is in quadrature, and the Moon in syzygy, being then nearly $4^{\circ} 58'$, and it gradually increases again till the nodes reach the line of syzygy. While the nodes are in the quadratures, or in the syzigies, the inclination is not sensibly changed during that revolution.

Such are the general effects of the lateral force Mm , that appear on a slight consideration of the circumstances of the case. A more particular account of them cannot be given in this outline of the science. We may just add, that the deductions from the general principle agree precisely with observation. The mathematical investigation not only points out the periods of the different inequalities, and their relation to the respective positions of the Sun and Moon, but also determines the absolute magnitude to which each of them rises. The only quantity deduced from mere observation is the mean inclination of the Moon's orbit. The time of the complete revolution of the nodes, and the magnitude and law of variation of this motion, and the change of inclination, with all its varieties, are deduced from the theory of universal gravitation.

539. There is another case of this problem which is considerably different, namely, the satellites of Dr Herschel's planet, the planes of whose orbits are nearly perpendicular to the orbit of the planet. This problem offers some curious cases, which deserve the attention of the mechanician; but as they interest us merely as objects of curiosity, they have not yet been considered.

540. There is still another considerable derangement of the lunar motions by the action of the sun. We have seen that, in quadrature, the Moon's gravitation to the Earth is augmented $\frac{1}{179}$, and that in syzygy it is diminished $\frac{1}{179}$. Taking the whole synodical revolution together, this is equivalent, nearly, to a diminution of $\frac{1}{179}$, or $\frac{1}{338}$. That is to say, in consequence of the Sun's action, the general gravitation of the Moon to the Earth is $\frac{1}{338}$ less than if the Sun were away. If the Sun were away, therefore, the Moon's gravitation would be $\frac{1}{338}$ greater than her present mean gravitation. The consequence would be, that the Moon would come nearer to the Earth. As this would be done without any change on her velocity, and as she now will be retained in a smaller orbit, she will describe it in a proportionally less time; and we can compute exactly how near she would come before this increased gravitation will be balanced by the velocity (224.) We must conclude from this, that the mean distance and the mean period of the Moon which we observe, are greater than her natural distance and period.

From this it is plain that if any thing shall increase or diminish the action of the Sun, it must equally increase or diminish the distance which the Moon assumes from the Earth, and the time of her revolution at that distance.

Now there actually is such a change in the Sun's action. When the Earth is *in perihelio*, in the beginning of January, she is nearer the Sun than in July by 1 part in 30; consequently the ratio of EM to ES is increased by $\frac{1}{30}$, or in the ratio of 30 to 31. But her gravitation (and consequently the Moon's) to the Sun is increased $\frac{1}{3}$, or in the ratio of 30 to 32. Therefore the disturbing force is increased by 1 part in 10 nearly. The Moon must therefore retire farther from the Earth 1 part in 1790. She must describe a larger orbit, and employ a greater time.

We can compute exactly what is the extent of this change. The sydereal period of the Moon is $27^d 7^h 43'$, or $39343'$. This must be increased $\frac{1}{1790}$, because the Moon retains the same velocity in the enlarged orbit. This will make the period $39365'$, which exceeds the other 22'. The observed difference between a lunation in January and one in July somewhat exceeds 25'. This, when reduced in the proportion of the synodical to the periodical revolution, agrees with this mechanical conclusion with great exactness, when the computation is made with due attention to every circumstance that can affect the conclusion. For it must be remarked that the computation here given proceeds on the legitimacy of assuming a general diminution of $\frac{1}{3}$ of the Moon's gravitation as equivalent to the variable change of gravity that really takes

takes place. In the particular circumstances of the case, this is very nearly exact. The true method is to take the average of all the disturbing forces MK through the quadrant, multiplying each by the time of its action. And, here, Euler makes a sagacious remark, that, if the diameter of the Moon's orbit had exceeded its present magnitude in a very considerable proportion, it would scarcely have been possible to assign the period in which she would have revolved round the Earth; and the greatest part of the methods by which the problem has been solved could not have been employed.

541. There still remains an anomaly of the lunar motions that has greatly puzzled the cultivators of physical astronomy. Dr Halley, when comparing the ancient Chaldean observations with those of modern times, in order to obtain an accurate measure of the period of the Moon's revolution, found that some observations made by the Arabian astronomers, in the eighth and ninth centuries, did not agree with this measure. When the lunar period was deduced from a comparison of the Chaldean observations with the Arabian, the period was sensibly greater than what was deduced from a comparison of the Arabian and the modern observations; so that the Moon's mean motion seems to have accelerated a little. This conclusion was confirmed by breaking each of these long intervals into parts. When the Chaldean and Alexandrian observations were compared, they gave a longer period than the Alexandrian compared with the Arabian of the eighth century;

century; and this last period exceeded what is deduced from a comparison of the Arabian with the modern observations; and even the comparison of the modern observations with each other shews a continued diminution. This conjecture was received by the mechanical philosophers with hesitation, because no reason could be assigned for the acceleration; and the more that the Newtonian philosophy has been cultivated, the more confidently did it appear that the mean distances and periods could sustain no change from the mutual action of the planets. Nay, M. de la Grange has at last demonstrated that, in the solar system as it exists, this is strictly true, as to any change that will be permanent: all is periodical and compensatory. Yet, as observation also improved, this acceleration of the Moon's mean motion became undeniable and conspicuous, and it is now admitted by every astronomer, at the rate of about $11''$ in a century, and her change of longitude increases in the duplicate ratio of the times.

Various attempts have been made to account for this acceleration. It was imagined by several that it was owing to the resistance of the celestial spaces, which, by diminishing the progressive velocity of the Moon, caused her to fall within her preceding orbit, approaching the Earth continually in a sort of elliptical spiral. But the free motion of the tails of comets, the rare matter of which seems to meet with no sensible resistance, rendered this explanation unsatisfactory. Others were disposed to think that gravity did not operate instantaneously through
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the whole extent of its influence. The application of this principle did not seem to be obvious, nor its effects to be very clear or definite.

At last, M. de la Place discovered the cause of this perplexing fact; and in a dissertation read to the Royal Academy of Sciences in 1785, he shews that the acceleration of the Moon's mean motion necessarily arises from a small change in the eccentricity of the Earth's orbit round the Sun, which is now diminishing, and will continue to diminish for many centuries, by the mutual gravitation of the planets. He was led to the discovery by observing in the series which expresses the increase of the lunar period by the disturbing force of the Sun (a series formed of sines and cosines of the Moon's angular motion and their multiples) a term equal to $\frac{1}{119}$ of her angular motion multiplied by the square of the eccentricity of the Earth's orbit. Consequently, when this eccentricity becomes smaller, the natural period of the Moon is less enlarged by the Sun's action, and therefore, if the Earth's eccentricity continue to diminish, so will the lunar period, and this in a duplicate proportion. Without entering into the discussion of this analysis, which is abundantly complicated, we may see the general effect of a diminution of the Earth's eccentricity in this manner. The ratio of the cube of the mean distance of the Earth from the Sun to the cube of her perihelion distance is greater than the ratio of the cube of her aphelion distance to that of the mean distance. Hence it follows that the increase of the mean lunar period, during the smaller distances

distances of the Earth from the Sun, is greater than its diminution, during her greater distances; and the sum of all the lunations, during a complete revolution of the Earth, exceeds the sum of the lunations that would have happened in the same time, had the Earth remained at her mean distance from the Sun. Therefore, as the Earth's eccentricity diminishes, the lunar period also diminishes, approximating more and more to her period, undisturbed by the change in the Sun's action. M. de la Place finds the diminution in a century = $11''{,}135$, which differs little from that assumed by Mayer from a comparison of observations. This centurial change of angular velocity must produce a change in the space described, that is, in the Moon's longitude, in the duplicate proportion of the time, as in any uniformly accelerated motion. Therefore $11''{,}135$ multiplied by the square of the number of centuries forward or backward, will give the correction of the Moon's longitude computed by the present tables. La Place finds that, in going back to the Chaldean observations, we must employ another term (nearly $\frac{1}{3}$ of a second) multiplied by the cube of the number of centuries. With these corrections, the computation of the Moon's place agrees with all observations, ancient and modern, with most wonderful accuracy; so that there no longer remains any phenomenon in the system which is not deducible from the Newtonian gravitation.

542. We should, before concluding this account of the perturbations of the planetary motions, pay some attention

tention to the motions of the other secondary planets, and particularly of Jupiter's satellites, seeing that the exact knowledge of their motions is almost as conducive to the improvement of navigation and geography as that of the lunar motions. But there is no room for this discussion, and we must refer to the dissertations of Wargentin, Prosperin, La Place, and others, who have studied the operation of physical causes on those little planets with great assiduity and judgement, and with the greatest success. The little system of Jupiter and his satellites has been of immense service to the philosophical study of the whole solar system. Their motions are so rapid, that, in the course of a few years, many synodical periods are accomplished, in which the perturbations arising from their mutual actions return again in the same order. Nay, such synodical periods have been observed as bring the whole system again into the same relative situation of its different bodies. And, in cases where this is not *accurately* accomplished, the deficiency introduces a small difference between the perturbations of any period and the corresponding perturbations of the preceding one; by which means another and much longer period is indicated, in which this difference goes through all its varieties, swelling to a maximum and again diminishing to nothing. Thus the system of Jupiter and his satellites, as a sort of epitome of the great solar system, has suggested to the sagacious philosopher the proper way of studying the great system, namely, by *looking out* for similar periods in *its* anomalies, and by boldly asserting

the reality of such corresponding equations as can be shewn to result from the operation of universal gravitation. The fact is, that we have now the most demonstrative knowledge of many such periods and equations, which could not be deduced from the observations of many thousand years.

In the course of this investigation, M. de la Grange has made an important observation, which he has demonstrated in the most incontrovertible manner, namely, that it *necessarily* results from the small eccentricity of the planetary orbits—their small inclination to each other—the immense bulk of the Sun—and from the planets all moving in one direction—that all the perturbations that are observed, nay *all that can exist* in this system, are periodical, and are compensated in opposite points of every period. He shews also that the greatest perturbations are so moderate, that none but an astronomer will observe any difference between this perturbed state and the mean state of the system. The mean distances and the mean periods remain for ever the same. In short, the whole assemblage will continue, almost to eternity, in a state fit for its present purposes, and not distinguishable from its present state, except by the prying eye of an astronomer.

Cold, we think, must be the heart that is not affected by this mark of beneficent wisdom in the Contriver of the magnificent fabric, so manifest in selecting for its connecting principle a power so admirably fitted for continuing to answer the purposes of its first formation. And he must be little susceptible of moral impression who
does

does not feel himself highly obliged to the Being who has made him capable of perceiving this display of wisdom, and has attached to this perception sentiments so pleasing and delightful. The extreme simplicity of the constitution of the solar system is perhaps the most remarkable feature of its beauty. To this circumstance are we indebted for the pleasure afforded by the contemplation. For it is this alone that has allowed our limited understanding to acquire such a comprehensive body of well-founded knowledge, far exceeding, both in extent and in accuracy, any thing attained in other paths of philosophical research. But we have not yet seen all the capabilities of this wonderful power of nature. Let us therefore still follow our excellent leader in a new path of investigation.

Of the Figures of the Planets.

544. Sir Isaac Newton, having so happily explained all the phenomena of *progressive* motion exhibited by the heavenly bodies, by shewing that they are all, without exception, modified examples of deflection towards one another, in the inverse duplicate ratio of the distances, was induced to examine the other motions observed in some of those bodies, to see what modification these motions received by the influence of universal gravitation. The Sun, and several planets turn round their axes. The study of celestial mechanism is not complete, till we see

whether this kind of motion is in any way influenced by gravitation.

It does not appear, at first consideration, that there can be any great mystery in the mere rotation of a body round its axis. It seems to be one of the simplest mechanical questions. But the fact is just the opposite. Before the rotative motion that we observe in our Earth can be secured, in the way in which we see it actually performed, adjustments are necessary, which are very abstruse, and required all the sagacity of Newton to discover and appreciate; and it is acknowledged that this is the department of physical astronomy where his acuteness of discernment appears the most remarkable. It is also the class of phenomena in which the effects of universal gravitation are most convincingly seen. For this reason, some more notice will be taken of the rotation of the planets, and of its consequences, than is usually done in our elementary treatises. But, as in the other departments, so here, it is only the more simple and general facts that can be considered. To go a very small step beyond these, engages us at once in the most difficult problems, which have occupied and still occupy the first mathematicians of Europe, and require all the resources of their science. Such discussion, however, would be unsuitable here. But without some attempt of this kind, we must remain ignorant of the mechanism of some phenomena, more familiar and important than many of those which we have already discussed.

When a body turns round an axis, each particle de-
scribes

scribes a circle, to which this axis is perpendicular. Now we know that a particle of matter cannot describe a circle, unless some deflecting force retain it in the periphery. In coherent masses, this retaining force is supplied by the cohesion. But even this is a limited thing. A stone may be so briskly whirled about in a sling that the cord will break. Grindstones are sometimes whirled about in our manufactures with such rapidity that they split, and the pieces fly off with prodigious force. If matters be lying loose on the surface of a revolving planet, their gravitation may be insufficient to retain them in that velocity of rotation. In every case, the force which actually retains such loose bodies on the surface can be found only in their weight; and part of it is thus expended, and they continue to press the ground only with the remainder. If the velocity of rotation be increased to a certain degree, it may require the whole weight of the body for its supply. If the velocity still increase, the body is not retained, but thrown off. If this Earth turned round in 84 minutes, things lying on the equator might remain there; but they would not press the ground, nor stretch the thread of a plummet. For this is precisely the time in which a planet would circulate round the Earth, close to the surface, moving about 17 times faster than a cannon ball. The weight of the body, deflecting it 16 feet in a second, just keeps it in the circumference of a circle close to the surface of the Earth. The Earth, turning as fast, will have the planet always immediately above the same point of its surface; and the planet will

not appear to have any weight, because it will not descend, but keep hovering over the same spot. If the rotation were still swifter, every thing would be thrown off, as we see water flung from a mop briskly whirled round.

545. As things are really adjusted, this does not happen. But yet there is a certain measurable part of the weight of any body expended in keeping it at rest, in the place where it lies loose. At the equator, a body lying on the ground describes, in one second, an arch of 1528 feet nearly. This deviates from the tangent nearly $\frac{67}{100}$ of an inch. This is very nearly $\frac{1}{288}$ part of $16\frac{1}{2}$ feet, the space through which gravity, or its heaviness, would cause a stone to fall in that time. Hence we must infer that the centrifugal tendency arising from rotation is $\frac{1}{288}$ of the sensible weight of a body on the equator, and $\frac{1}{289}$ of its real weight. Were this body therefore taken to the pole, it would manifest a greater heaviness. If, at the equator, it drew out the scale of a spring steel-yard to the division 288, it would draw it to 289 at the pole.

546. M. Richer, a French mathematician, going to Cayenne in 1672, was directed to make some astronomical observations there, and was provided with a pendulum clock for this purpose. He found that his clock, which had been carefully adjusted to mean time at Paris, lost above two minutes every day, and he was obliged to shorten

shorten the pendulum $\frac{1}{80}$ of an inch before it kept right time. Hence he concluded that a heavy body dropped at Cayenne would not fall 193 inches in a second. It would fall only about $192\frac{1}{8}$. Richer immediately wrote an account of this very singular diminution of gravity. It was scouted by almost all the philosophers of Europe, but has been confirmed by many repetitions of the experiment. Here then is a direct proof that the heaviness of a body, whether considered as a mere pressure, or as an accelerating force, is employed, and in part expended, in keeping bodies united to a whirling planet.

547. These considerations are not new. Even in ancient times, men of reflection entertained such thoughts. The celebrated Roman general Polybius, one of the most intelligent philosophers of antiquity, is quoted by Strabo, as saying that in consequence of the Earth's rotation, every body was made lighter, and that the globe itself swelled out in the middle. Were it not so, says he, the waters of the ocean would all run to the shores of the torrid zone, and leave the polar regions dry. Dr Hooke is the first modern philosopher who professed this opinion. Mr Huyghens, however, is the first who gave it the proper attention. Occupied at the time of Richer's remark with his pendulum clocks, he took great interest in this observation at Cayenne, and instantly perceived the true cause of the retardation of Richer's clock. He perceived that pendulums must vibrate more slowly, in proportion as their situation removes them farther from
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the axis of the Earth; and he assigned the proportion of the retardation in different places.

548. Resuming this subject some time after, it occurred to him, that unless the Earth be protuberant all around the equator, the ocean must overflow the lands, increasing in depth till the height of the water compensated for its diminished gravity. He considers the condition of the water in a canal reaching from the surface of the equator to the centre of the Earth (suppose the canal CQ , fig. 33.) and there communicating with a canal CN reaching from the centre to the pole. The water in the last must retain all its natural gravity, because its particles do not describe circles round the axis. But every particle in the column CQ reaching to the surface of the equator must have its weight diminished in proportion to its distance from the centre of the globe. Therefore the whole diminution will be the same as if each particle lost half as much as the outermost particle loses. This is very plain. Therefore these two columns cannot balance each other at the centre, unless the equatoreal column be longer than the polar column by $\frac{1}{373}$ (for the extremity of this column loses $\frac{1}{87}$ of its weight by the centrifugal force employed in the rotation).

Being an excellent and zealous geometer, this subject seemed to merit his serious study, and he investigated the form that the ocean must acquire so as to be *in equilibrio*. This he did by inquiring what will be the position of a plummet in any latitude. This he knew must be perpendicular

perpendicular to the surface of still water. On the supposition of gravity directed to the centre of the Earth, and equal at all distances from that centre, he constructed the meridional curve, which should in every point have the tangent perpendicular to the direction of a plummet determined by him on these principles.

549. At this very time, another circumstance gave a peculiar interest to this question of the figure of the Earth. The magnificent project of measuring the whole arch of the meridian which passes through France was then carrying on. (See § 267.) It seemed to result from the comparison of the lengths of the different portions of this arch, that the degrees increased as they were more southerly. This made the academicians employed in the measurement conclude that the Earth was of an egg-like shape. This was quite incompatible with the reasoning of Mr Huyghens. The contest was carried on for a long while with great pertinacity, and some of the first mathematicians of the age abetted the opinion of those astronomers, and the honour of France was made a party in the dispute. The opinion of Mr Huyghens, the greatest ornament of their academy, could not prevail; indeed his inferences were such, in some respects, that even the impartial mathematicians were dissatisfied with them. The form which he assigned to the meridian was very remarkable, consisting of two paraboloidal curves, which had their vertex in the poles, and their branches intersected each other at the equator, there forming an angu-

lar ridge, elevated about seven miles above the inscribed sphere. No such ridge had been observed by the navigators of that age, who had often crossed the equator. Nor had any person on shore at the line observed that two plummets near each other were not parallel, but sensibly approached each other. All this was unlike the ordinary gradations of nature, in which we observe nothing abrupt.

550. While this question was so keenly agitated in France, Mr Newton was engaged in the speculations which have immortalized his name, and it was to him an interesting thing to know what form of a whirling planet was compatible with an equilibrium of all the forces which act on its parts. He therefore took the question up in its most simple form. He supposed the planet completely fluid, and therefore every particle is at liberty to change its place, if it be not in perfect equilibrium. The particles all attract one another with a force in the inverse duplicate ratio of the distance, and they are at the same time actuated by a centrifugal tendency, in consequence of the rotation; or, to express it more accurately, part of those mutual attractions is employed in keeping the particles in their different circles of rotation. He demonstrated that this was possible, if the globe have the form of an elliptical spheroid, compressed at the poles, and protuberant at the equator $\frac{1}{231}$ part of the axis. He also pointed out the phenomena by which this may be ascertained, namely, the variation of gravity as we recede

cede from the equator to the poles, shewing that the increments of sensible gravity are as the squares of the sines of the latitude. This can easily be decided by experiments with nice pendulum clocks. He shewed also that the remaining gravity, on different parts of the Earth's surface, is inversely proportional to the distance from the centre, when estimated in the direction of the centre, &c. &c. His demonstration of the precise elliptical form consists in proving two things: 1st, That on this supposition, gravity is always perpendicular to the surface of the spheroid: 2d, That all rectilineal canals leading from the centre to the surface will balance one another. Therefore the ocean will maintain its form.

It was some time before the philosophy of Newton could prevail in France over the hypothesis of the French philosopher Des Cartes; and the great mathematician Bernoulli endeavoured to shew that the oblong form of the Earth which had been demonstrated (he says) by the measurement of the degrees, was the effect of the pressure of the vortices in which the Earth was carried about.

551. Mr Hermann, a mathematician of most respectable talents, took another view of the question of the figure of the Earth. Newton had demonstrated in the most convincing manner that particles gravitated to the centre of similar solids, or portions of a solid, with forces proportional to their distances from the centre. Hermann availed himself of this, and of another theorem

of Newton founded on it, viz. that superficial gravity in different latitudes is inversely as the distance from the centre.* But he observed that Newton had by no means demonstrated the elliptical form, but had merely assumed it, or, as it were, guessed at it. This is indeed true, and his application is made by means of the vulgar rule of false position. Hermann therefore set himself to inquire what form a fluid will assume when turning round an axis, its particles situated in the same diameter gravitating to the centre proportionally to their distance, yet exhibiting a superficial gravity in different parts inversely as the distance from the centre. He found it to be an ellipse, with such a protuberancy, that the radius of the equator is to the semiaxis in the subduplicate ratio of the primitive equatoreal gravity to the remaining equatoreal gravity. This gives the same proportion of the axes which had been assigned by Huyghens, though accompanied by a very different form. He then inverted his process, and demonstrated the perpendicularity of gravity to the surface, the equilibrium of canals, and some other conditions that appeared indispensable; and he found all right. This confirmed him in his theory, and he found fault with Dr D. Gregory, the commentator of Newton, for adhering to Newton's form of the ellipse. He desired them to point out any fault in his own demonstration of the

* Both of these propositions are easily inferred from Art. 463, and need not be particularly insisted on in this place, for reasons which will soon appear.

the elliptical figure, and considered this as sufficient for proving the inaccuracy of the Newtonian conjecture, for it could get no higher name.

552. By very slow degrees, the French academicians began to acknowledge the compressed form of the Earth, and to reexamine their observations, by which it had seemed that the degrees increased to the southward. They now affected to find that their measurement had been good, but that some circumstances had been overlooked in the calculations, which should have been taken into the account. But they were not aware that they were now vindicating the goodness of their instruments and of their eyesight at the expence of their judgement.

All these things made the problem of the figure of the Earth extremely interesting to the great mathematical philosophers. Newton took no part in the further discussion, being satisfied with the evidence which he had for his own determination of the precise species of the terraqueous spheroid. His philosophy gradually acquired the ascendancy; but the comparison made of the degrees of the meridian argued a smaller ellipticity than he had assigned to the Earth, on the supposition of uniform density and primitive fluidity. He had however sufficiently pointed out the varieties of ellipticity which might arise from a difference of density in the interior parts. These were acquiesced in, and the mathematicians speculated on the ways by which the observations
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and the theory of universal gravitation might be adapted to each other. But, all this while, the original problem was considered as too difficult to be treated in any case remarkably deviating from a sphere, and even this case was solved by Newton and his followers only in an indirect manner.

553. The first person who attempted a direct general solution was Mr James Stirling. In 1735 he communicated to the Royal Society of London two elegant propositions (but without demonstration), which determine the form of a homogeneous spheroid turning round its axis, and which, when applied to the particular case of the Earth, perfectly coincided with Newton's determination. In 1737 Mr Clairaut communicated to our Royal Society, and also to the Royal Academy at Paris, very elaborate and elegant performances on the same subject, which he afterwards enlarged in a separate publication. This is the completest work on the subject, and is full of the most curious and valuable research, in which are discussed all the circumstances which can affect the question. It is also remarkable for an example of candour very rare among rivals in literary fame. The author, in extending his memoir to a more complete work, quits his own method of investigation, though remarkable for its perspicuity and neatness, for that of another mathematician, because it was superior; and this with unaffected acknowledgement of its superiority. The results of Clairaut's theory perfectly coincide with the Newtonian

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hian theory, making the equatoreal diameter to the polar diameter as 231 to 230, though it is agreed by all the mathematicians that Newton's method had a chance of being inaccurate. So true is the saying of Daniel Bernoulli, when treating this subject in his theory of the tides, "*The sagacity of that great man (Newton) saw clearly through a mist what others can scarcely discover through a microscope.*"

Mr Stirling had said that the revolving figure was not an accurate elliptical spheroid, but approached infinitely near to it. Mr Clairaut's solutions, in most cases, suppose the spheroid very nearly a sphere, or suppose lines and angles equal which are only very nearly so. Without this allowance, the treatment of the problem seemed impracticable. This made Mr Stirling's assertion more credited; and we apprehend that it became the general opinion that the solutions obtainable in our present state of mathematical knowledge were only approximations, exact indeed, to any degree that we please, in the cases exhibited in the figures of the planets, but still they were but approximations.

554. But in 1740, Mr M'Laurin, in a dissertation on the tides, which shared the prize given by the Academy of Paris, demonstrated, in all the rigour and elegance of ancient geometry, that an homogeneous elliptical spheroid, of *any eccentricity whatever*, if turning in a proper time round its axis, will for ever preserve its form. He gave the rule for investigating this form, and the

the ratio of its axes. His final propositions to this purpose are the same that Mr Stirling had communicated without demonstration: This performance was much admired, and settled all doubts about the figure of a homogeneous spheroid turning round its axis. It is indeed equally remarkable for its simplicity, its perspicuity and its elegance. Mr M'Laurin had no occasion to prosecute the subject beyond this simple case. Proceeding on his fundamental propositions, the mathematical philosophers have made many important additions to the theory. But it still presents many questions of most difficult solution, yet intimately connected with the phenomena of the solar system.

In this elementary outline of physical astronomy, we cannot discuss those things in detail. But it would be a capital defect not to include the *general* theory of the figure of planets which turn round their axes. No more, however, will be attempted than to shew that a homogeneous elliptical spheroid will answer all the conditions that are required, and to give a *general* notion of the change which a variable density will produce in this figure. *

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* The student will consult, with advantage, the original dissertations of Mr Clairaut and Mr M'Laurin, and the great additions made by the last in his valuable work on Fluxions. The *Cosmographia* of *Frisius* also contains a very excellent epitome of all that has been done before his time; and the *Mechanique*

The following lemma from Mr M'Laurin must be premised.

555. Let $AEBQ$ and $aebq$ (fig. 64. No. 1.) be two concentric and similar ellipses, having their shorter axes AB and ab coinciding. Let PaL touch the interior ellipse in the extremity a of the shorter axis, to which let PK , a chord of the exterior ellipse be parallel, and therefore equal. Let the chords af and ag of the interior ellipse make equal angles with the axis, and join their extremities by the chord fg perpendicular to it in i . Draw PF and PG parallel to af and ag , and draw FH and PI perpendicular to PK .

Then, PF together with PG are equal to twice ai , when PF and PG lie on different sides of PK . But if they are on the same side (as PF' and PG') then $2ai$ is equal to the difference of PF' and PG' .

Draw Kk parallel to PG or ag , and therefore equal to PF , being equally inclined to KP . Draw the diameter MCz , bisecting the ordinates Kk , PG , and ag , in m , s , and z , and cutting PK in n .

By similarity of triangles, we have

$$Km : Kn = Ps : Pn, = az : aC, = ag : ab.$$

Therefore

chanique Celeste of *La Place* contains some very curious and recondite additions. A work of *F. Boscovich* on the Figure of the Earth has peculiar merit. This author, by employing geometrical expressions of the acting forces, wherever it can be done, gives us very clear ideas of the subject.

Therefore $K m + P s : K n + P n = a g : a b$,

and $K k$ (or $P F$) $+ P G : 2 P K = 2 a g : 2 a b$,

and $P F + P G : 2 a g = 2 P K : 2 a b$;

and, by similitude of triangles, we have

$$P H + P I : 2 a i = 2 P K : 2 a b.$$

But $2 P K = 2 a b$. Therefore $P H + P I = 2 a i$, and
 $P I' - P H' = 2 a i'$.

556. Let the two planes $A G g B$ (fig. 63.) $A E e B$, intersecting in the line $A B$, and containing a very small angle $G A E$, be supposed to comprehend a thin elementary wedge or slice of a solid consisting of gravitating matter. If two planes $G P E$, $F P D$, standing perpendicularly on the plane $A D d B$, contain a very small angle $E P D$, they will comprehend a slender, or elementary pyramid of this slice, having its vertex in P , and a quadrilateral base $G E D F$. If two other planes $g p e$, $f p d$, be drawn from another point p , respectively parallel to the planes $G P E$, $F P D$, they will comprehend another pyramid, having its sides parallel to those of the other, and containing equal angles, and the elementary pyramids $F P E$, $f p e$, may therefore be considered as similar. The base $g e d f$ is not indeed always parallel and similar to $G E D F$. But for each of them may be substituted spherical surfaces, having their centres in P and in p , and then they will be similar.

The gravitation of a particle P to the pyramid $G P D$ is to the gravitation of p to the pyramid $g p d$ as any
side

side PD of the one to the homologous side pd of the other. This is evident, by what was shewn in § 462.

The same proportion will hold when the absolute gravitation in the direction of the axis of the pyramid is estimated in any other direction, such as Pm . For, drawing pn parallel to Pm , and the perpendiculars Dm , dn , it is plain that the ratio $PD : pd = Pm : pn$, $= Dm : dn$.

This proposition is of most extensive use. For we thus estimate the gravitation of a particle to any solid, by resolving it into elementary pyramids; and having found the gravitation to each, and reduced them all to one direction, the aggregate of the reduced forces is the whole gravitation of the particle estimated in that direction. The application of this is greatly expedited by the following theorem.

558. Two particles similarly situated in respect of similar solids, that is to say, situated in similar points of homologous lines, have their whole gravitations proportional to any homologous lines of the solids.

For, we can draw through the two particles straight lines similarly posited in respect of the solids, and then draw planes passing through those lines, and through similar points of the solids. The sections of the solids made by those two planes must be similar, for they are similarly placed in similar solids. We can then draw other planes through the same two straight lines, containing with the former planes very small equal angles. The

sections of these two planes will also be similar, and there will be comprehended between them and the two former planes similar slices of the two solids.

We can now divide the slices into two serieses of similar pyramids, by drawing planes such as GPE , gpe , and FPD , fpd , of fig. 63. the points P and p being supposed in different lines, related to each of the two solids. By the reasonings employed in the last proposition, it appears that when the whole of each slice is occupied by such pyramids, the gravitations to the corresponding pyramids are all in one proportion. Therefore the gravitation compounded of them all is in the same proportion. As the whole of each of the two similar slices may be thus occupied by serieses of similar and similarly situated pyramids, so the whole of each of the two similar solids may be occupied by similar slices, consisting of such pyramids. And as the compound gravitations to those slices are similarly formed, they are not only in the proportion of the homologous lines of the solids, but they are also in similar directions. Therefore, finally, the gravitations compounded of these compound gravitations are similarly compounded, and are in the same proportion as any homologous lines of the solids.

These things being premised, we proceed to consider the particular case of elliptical spheroids.

559. Let $AEBQ$, $ae bq$ (fig. 64.) be concentric and similar ellipses, which, by rotation round their shorter axis

axis $A a b B$, generate similar concentric spheroids. We may notice the following particulars.

560. (a) A particle r , on the surface of the interior spheroid, has no tendency to move in any direction in consequence of its gravitation to the matter contained between the surfaces of the exterior and interior spheroids. For, drawing through r the straight line $P r t G$, it is an ordinate to some diameter $C M$, which bisects it in s . The part $r t$ comprehended by the interior spheroid is also an ordinate to the same diameter and is bisected in s . Therefore $P r$ is equal to $t G$. Now r may be conceived as at the vertex of two similar cones or pyramids, on the common axis $P r G$. By what was demonstrated in art. 462. & 557, it appears that the gravitation of r to the matter of the cone or pyramid whose axis is $r P$ is equal and opposite to the gravitation to the matter contained in the *frustum* of the similar cone or pyramid, whose axis is $t G$. As this is true, in whatever direction $P r G$ be drawn through r , it follows that r is *in equilibrio* in every direction, or, it has no tendency to move in any direction.

561. (b) The gravitations of two particles P and p (fig. 64. No. 2.) situated in one diameter $P C$, are proportional to their distances $P C$, $p C$, from the centre. For the gravitation of p is the same as if all the matter between the surfaces $A E B Q$ and $a e b q$ were away (by the last article), and thus P and p are similarly situated

ated on similar solids; and PC and pC are homologous lines of those solids; and the proposition is true, by § 558.

562. (c) All particles equally distant from the plane of the equator gravitate towards that plane with equal forces.

Let P be the particle (fig. 64. No. 1.) and Pa a line perpendicular to the axis, and parallel to the equator EQ . Let Pd be perpendicular to the equator. Let $aebq$ be the section of a concentric and similar spheroid, having its axis ab coinciding with AB . Drawing any ordinate fg to the diameter ab of the interior ellipse, join af and ag , and draw PF and PG parallel to af and ag , and therefore making equal angles with PdK . Let fg cut ab in i , and draw FH , GI , perpendicular to PI .

The lines PF and PG may be considered as the axes of two very slender pyramids, comprehended between the plane of the figure and another plane intersecting it in the line PaL and making with it a very minute angle. These pyramids are constituted according to the conditions described in art. 556. The lines af , ag are, in like manner, the axes of two pyramids, whose sides are parallel to those of PF and PG . The gravitation of P to the matter contained in the pyramids PF and PG , and the gravitation of a to the pyramids af and ag , are as the lines PF , PG , af , and ag , respectively. These gravitations, estimated in the direction Pd ,

Pd , aC , perpendicular to the equator, are as the lines PH , PI , ai , ai , respectively. Now it has been shewn (555.) that $PH + PI$ are equal to $ai + ai$. Therefore the gravitations of P to this pair of pyramids, when estimated perpendicularly to the equator, is equal to the gravitation of a to the corresponding pyramids lying on the interior ellipse $ae bq$.

It is evident that by carrying the ordinate fg along the whole diameter from b to a , the lines af , ag , will diverge more and more (always equally) from ab and the pyramids of which these lines are the axes, will thus occupy the whole surface of the interior ellipse. And the pyramids on the axes PF and PG , will, in like manner, occupy the whole of the exterior ellipse. It is also evident that the whole gravitation of P , estimated in the direction Pd , arising from the combined gravitations, to every pair of pyramids estimated in the same direction, is equal to the whole gravitation of a , arising from the combined gravitation to every corresponding pair of pyramids. That is, the gravitation of P in the direction Pd to the whole of the matter contained in the elementary slice of the spheroid comprehended between the two planes which intersect in the line PaL , is equal to the gravitation of a to the matter contained in that part of the same slice which lies within the interior spheroid.

But this is not confined to that slice which has the ellipse $AEBQ$ for one of its bounding planes. Let the spheroid be cut by any other plane passing through the line PaL . It is known that this section also is an ellipse,

lipse, and that it is concentric with and similar to the ellipse formed by the intersection of this plane with the interior spheroid $a e b q$. They are concentric similar ellipses, although not similar to the generating ellipses $A E B Q$ and $a e b q$. Upon this section may another slice be formed by means of another section through $P a L$, a little more oblique to the generating ellipse $A E B Q$. And the solidity of this section may, in like manner, be occupied by pyramids constituted according to the conditions mentioned in art. 558.

From what has been demonstrated, it appears that the gravitation of P to the whole matter of this slice, estimated in the direction perpendicular to $P a L$, is equal to the gravitation of a to the matter in the portion of this slice contained in the interior spheroid.

Hence it follows that when these slices are taken in every direction through the line $P a L$, they will occupy the whole spheroid, and that the gravitation of P to the matter in the whole solid, estimated perpendicularly to $P a L$, is equal to the gravitation of a to the matter that is contained in the interior spheroid, estimated in the same manner.

This gravitation will certainly be in the direction perpendicular to the plane of the equator of the two spheroids. For the slices which compose the solid, all passing through the generating ellipse $A E B Q$, may be taken in pairs, each pair consisting of equal and similar slices, equally inclined to the plane of the generating ellipse. The gravitations to each slice of a pair are equal, and

equally

equally inclined to the plane $AEBQ$. Therefore they compose a gravitation in the direction which bisects the angle contained by the slices, that is, in the direction of the plane $AEBQ$, and parallel to its axis AB , or perpendicular to the equator.

From all this it follows, that the gravitation of P to the whole spheroid, when estimated in the direction Pd perpendicular to the plane of its equator, is equal to the gravitation of a to the interior spheroid $ae bq$, which is evidently in the same direction, being directed to the centre C .

In like manner, the gravitation of another particle P' (in the line PaL), in a direction perpendicular to the equator of the spheroid, is equal to the gravitation of a to the interior spheroid $ae bq$; for P' may be conceived as on the surface of a concentric and similar spheroid. When thus situated, it is not affected by the matter in the spheroidal stratum without it, and therefore its gravitation is to be estimated in the same way with that of the particle P . Consequently the gravitation of P and of P' , estimated in a direction perpendicular to the equator, are equal, each being equal to the central gravitation of a to the spheroid $ae bq$. Therefore all particles equidistant from the equator gravitate equally toward it.

563. (d) By reasoning in the same manner, we prove that the gravitation of a particle P in the direction Pa , perpendicular to the axis AB , is equal to the

gravitation of the particle d to the concentric similar spheroid $daqb$; and therefore all particles equidistant from the axis gravitate equally in a direction perpendicular to it.

564. (e) The gravitation of a particle to the spheroid, estimated in a direction perpendicular to the equator, or perpendicular to the axis, is proportional to its distance from the equator, or from the axis. For the gravitation of P in the direction Pd is equal to the gravitation of a to the spheroid $aebq$. But the gravitation of a to the spheroid $aebq$, is to the gravitation of A to $AEBQ$ as aC to AC (558.) Therefore the gravitation of P in the direction Pd is to the gravitation of A to the spheroid $AEBQ$ as aC to AC , or as Pd to AC ; and the same may be proved of any other particle. The gravitation of A is to the gravitation of any particle as the distance AC is to the distance of that particle. All particles therefore gravitate towards the equator proportionally to their distances from it.

In the same manner, it is demonstrated that the gravitation of E to the spheroid in the direction EC perpendicular to the axis, is to the gravitation of any particle P in the same direction as EC to Pa , the distance of that particle from the axis.

Therefore, &c.

565. (f) We are now able to ascertain the direction and intensity of the compound or absolute gravitation of any particle P .

For

For this purpose let A represent the gravitation of the particle A in the pole, and E the gravitation of a particle E on the surface of the equator; also let the force with which P is urged in the direction Pd be expressed by the symbol f, Pd , and let f, Pa express its tendency in the direction Pa . We have

$$f, Pd : A = Pd : AC$$

and $A : E = A : E$

and $E : f, Pa = EC : Pa$. Therefore

$$f, Pd : f, Pa = Pd \times A \times EC : AC \times E \times Pa,$$

Now make $dC : dv = A \times EC : E \times AC$, and draw Pv . We have now $f, Pd : f, Pa = Pd \times dC : Pa \times dv$, $= Pd \times Pa : Pa \times dv$, $= Pd : dv$. P is therefore urged by two forces, in the directions Pd and Pa , and these forces are in the proportion of Pd and dv . Therefore the compound force has the direction Pv .

Moreover, this compound force is to the gravity at the pole, or the gravitation of the particle A , as Pv to AC . For the force Pv is to the force Pd as Pv to Pd ; and the force Pd is to A as Pd to AC . Therefore the force Pv is to A as Pv to AC .

In like manner, it may be compared with the force at E . Make $aC : au = E \times CA : A \times CE$. We shall then have $f, Pa : f, Pd = Pa : au$; and the force in the direction Pa , when compounded with that in the direction Pd , form a force in the direction Pu , and having to the force at E the proportion of Pu to EC .

Thus have we obtained the direction of gravitation for any individual particle on the surface, and its magni-

tude when compared with the forces at A and at E, which are supposed known.

566. (g) But it is necessary to have the measure of the accumulated force or pressure occasioned by the gravitation of a column or row of particles.

Draw the tangent ET, and take any portion of it, such as ET, to represent the gravitation of the particle E. Join CT, cutting the perpendicular $d\delta$ in δ . Since the gravitations of particles in one diameter are as their distances from the centre (561.) $d\delta$ will express the gravitation of a particle d . Thus, the gravitation of the whole column EC will be represented by the area of the triangle CET, and the gravitation of the part Ed , or the pressure exerted by it at d , is represented by the area $ET\delta d$. We may also conveniently express the pressure of the column EC at C by $\frac{E \times EC}{2}$, and, in like manner, $\frac{A \times AC}{2}$ expresses the weight of the column AC, or the pressure exerted by it at C.

Should we express the gravitation of E by a line ET equal to EC, the weight of the whole column EC would be expressed by $\frac{EC^2}{2}$, and that of the portion Ed by $\frac{EC^2 - dC^2}{2}$, or by its equal $\frac{Ed \times dQ}{2}$. We see also that whatever value we assign to the force E, the gravitations or pressures of the columns EC and Ed are proportional to EC^2 , and $EC^2 - dC^2$, or to EC^2 and $Ed \times dQ$. This remark will be frequently referred to.

567. From these observations it appears that the two columns AC and EC will exert equal or unequal pressures at the centre C , according to the adjustment of the forces in the direction of the axis, and perpendicular to the axis. If the ellipse do not turn round an axis, then, in order that the fluid in the columns AC and EC may press equally at C , we must have $A \times AC = E \times EC$, or $AC : EC = E : A$. The gravitation at the pole must be to that at the equator as the radius of the equator to the semiaxis. But we shall find, on examination, that such a proportion of the gravitations at A and E cannot result solely from the mutual gravitation of the particles of a homogeneous spheroid, and that this spheroid, if fluid, and at rest, cannot preserve its form.

568. The six preceding articles ascertain the mechanical state of a particle placed any where in a homogeneous spheroid, inasmuch as it is affected solely by the mutual gravitation to all the other particles. We are now to inquire what conditions of form and gravitating force will produce an exact equilibrium in every particle of an elliptical spheroid of gravitating fluid when turning round its axis. For this purpose, it is necessary, in the first place, that the direction of gravity, affected by the centrifugal force of rotation, be every where perpendicular to the surface of the spheroid, otherwise the waters would flow off toward that quarter to which gravity inclines. Secondly, all canals reaching from the centre to the surface must balance at the centre, otherwise the
preponderating

preponderating column will subside, and press up the other, and the form of the surface will change. And, lastly, any particle of the whole mass must be *in equilibrio*, being equally pressed in *every* direction. These three conditions seem sufficient for insuring the equilibrium of the whole.

569. These conditions will be secured in an elliptical fluid spheroid of uniform density turning round its axis, *if the gravity at the pole be to the equatorial gravity, diminished by the centrifugal force arising from the rotation, as the radius of the equator to the semiaxis.*

We shall first demonstrate that in this case gravity will be every where perpendicular to the spheroidal surface.

Let p express the polar gravity, e the primitive equatorial gravity, and c the centrifugal force at the surface of the equator, and let $e - c = s$, be the sensible gravity remaining at the equator. Then, by hypothesis, we have $p : s = CE : CA$. Considering the state of any individual particle P on the surface of the spheroid, we perceive that that part of its compound gravitation which is in a direction perpendicular to the plane of the equator is not affected by the rotation. It still is therefore to the force p at the pole as Pd to AC (564.) But the other constituent of the whole gravitation of P , which is estimated perpendicular to the axis, is diminished by the centrifugal force of rotation, and this diminution is in proportion to its distance from the axis, that is, in proportion

portion to this primitive constituent of its whole gravitation. Therefore its remaining gravity in a direction perpendicular to the axis is still in the proportion of its distance from it. And this is the case with every individual particle. Each particle therefore may still be considered as urged only by two forces, one of which is perpendicular to the equator and proportional to its distance from it, and the other is perpendicular to the axis and proportional to its distance from it. Therefore, if we draw a line Pvu , so that dC may be to dv as $p \times EC$ to $s \times AC$, Pv will be the direction of the compound force of gravity at P , as affected by the rotation.

But, by hypothesis $p : s = EC : AC$; therefore $p \times EC : s \times AC = EC^2 : AC^2$, and $EC^2 : AC^2 = dC : dv$, $= Pv : Pu$. But (Ellipse 7.) if Pu be to Pv as EC^2 to AC^2 , the line Pvu is perpendicular to the tangent to the ellipse in the point P , and therefore to the spheroidal surface, or to the surface of the still ocean.

Thus, then, the first condition is secured, and the superficial waters of the ocean will have no tendency to move in any direction. Having therefore ascertained a suitable *direction* of the affected gravitation of P , we may next inquire into its intensity.

570. The sensible gravity of any superficial particle P is every where to the polar gravity as the line Pu (the normal terminating in the axis) to the radius of meridional curvature at the pole; and it is to the sensible gravity at the equator as the portion Pv of the same normal terminating

terminating in the equator is to the radius of meridional curvature at the equator. For it was shewn (565.) to be to the force at E as $P u$ to $E C$. If, therefore, the radius of the equator be taken as the measure of the gravitation there, $P u$ will measure the sensible gravitation at P. And since the ultimate situation of the point u , when P is at the pole, is the centre of curvature of the ellipse at A, the radius of curvature there will measure the polar gravity. That is, the sensible gravity at the equator is to the gravity at the pole, as the radius of the equator to the radius of polar curvature. By a perfectly similar process of reasoning, it is proved that if the gravity at the pole be measured by $A C$, the gravity at P is measured by $P v$, and at the equator by the radius of curvature of the ellipse in E.

571. *Cor. 1.* The sensible gravity in every point P of the surface is reciprocally as the perpendicular $C t$ from the centre on the tangent in that point. For every where in the ellipse, $C t \times P u = C E^2$, and $C t \times P v = C A^2$, as is well known.

572. *Cor. 2.* The central gravity of every superficial particle P, that is, its absolute gravity $P u$ or $P v$ estimated in the direction $P C$, is inversely proportional to its distance from the centre, that is, the central gravity at P is to the central gravity at E as $E C$ to $P C$, and to the polar gravity as $A C$ to $P C$. For, if the gravity $P v$ be reduced to the direction $P C$ by drawing $v o$ perpendicular to $C P$, $P o$ will measure this central gravity.

vity. Now, it is well known that $P o \times P C$ is every where $= A C^2$; and, in like manner, $P \omega \times P C = E C^2$. Therefore $P o$, or $P \omega$, are every where reciprocally as $P C$.

Hence it follows that the sensible increment of gravity in proceeding from the equator to the pole is very nearly as the square of the sine of the latitude; for, without entering on a more curious investigation, it is plain that the increments of gravity, when so minute in comparison with the whole gravity, are very nearly as the decrements of the distance. Now, in a spheroid very little compressed, these decrements are in that proportion. It may be demonstrated that in the latitude where $\sin.^2 = \frac{1}{3}$, namely, lat. $35^\circ 16'$, the gravity is the same as to a perfect sphere of the same capacity, having for its radius the semidiameter of the ellipse in that point. It is also a distinguishing property of this latitude that, if this semidiameter be produced, the gravitation of a particle, at any distance in this direction, is the same as to a perfect sphere of the same capacity. This is not the case in any other direction.

573. *Cor.* 3. Lastly, the force estimated in the direction $P d$ is to the force in the direction $P a$ as $E C^2 \times P d$ to $A C^2 \times P a$. For we had (564.) $f, P d : f, P a = A \times E C \times P d : E \times A C \times P a$, which, by substituting p and s for A and E , it becomes $p \times E C \times P d : s \times A C \times P a, = E C^2 \times P d : A C^2 \times P a$.

Hitherto we have considered only the particles on the

surface of the spheroid. But we must know the condition of a particle any where within it.

574. A particle p , in any internal point of a diameter, has its sensible gravity in the direction perpendicular to the surface of a concentric and similar spheroid passing through the particle. For the gravity at p is compounded of forces perpendicular to the axis and to the equator, and proportional to the distances from them, and therefore proportional to the similar forces acting on the particle P (558.) Therefore the compound force of p will be parallel, and in the same proportion, to the compound force Pv of P , and must therefore be perpendicular to the tangent of the surface in p . It is as $p v'$.

575. *Cor.* Hence we must infer that if there were a cavern at p , containing water, the surface of this still water would be a part of the spheroidal surface $aebq$. Should this cavern extend all the way to e or a , the water should arrange itself according to this surface; or, if erp be a pipe or conduit, the water in it should be still, except so far as it is affected by the pressures of the columns Aa and Pp and Ee (these pressures will be proved to be equal).

It would seem, from these premises, that if the elliptical spheroid consist of different fluids, which do not mix, and which differ in density, they will be disposed in concentric similar elliptical strata, so that their bounding

ing surfaces shall be similar. The proof of this seems the same with what is received for a demonstration of the horizontal surface of the boundary between water and oil contained in a vessel. Accordingly, this has been supposed by many respectable writers, as a thing that needed no other proof. But this is by no means the case. It can be strictly demonstrated that the denser fluids occupy the lowest place, and that the strata become less and less eccentric as we approach the centre, where the ultimate evanescent figure may be denominated a spherical point. It may be seen, even at present, that they cannot be similar, unless homogeneous. For, without this condition, it cannot be generally demonstrated that the gravitation of a particle p to the equator, and to the axis, is as the distance from them, which is the foundation of all the subsequent demonstrations.

576. In the next place, all rectilinear columns, extending from the centre to the surface, will balance in the centre. For, drawing vo , $v'o'$ perpendicular to PC , it is plain that Po and $p'o'$ represent the gravities of P and p estimated in the direction PC . Now $Po : p'o' = PC : pC$. Therefore the gravitation of the whole column, or the pressure on C , is represented by $\frac{Po \times PC}{2}$

(566.) Now, in the ellipse $Po \times PC = CA^2$, a constant quantity. Therefore the pressure of every column at C is the same. In like manner, the pressure of the columns, Cp and Ca are equal, and therefore also the

pressures of $P p$, $E e$, and $A a$, at p , e , and a , are all equal.

577. Lastly, any particle of the fluid is equally pressed in every direction, and if the whole were fluid, would be *in equilibrio*, and remain at rest.

To prove this, let $P p$ (fig. 64. 3.) be a column reaching from P to the surface, and taken in any direction, but, first, in one of the meridional planes, of which AB is the axis, and $E Q$ the intersection by the equatorial plane. In the tangent $A a$ take $A a$ equal to EC , and $A \alpha$ equal to AC . Draw $a C e$ and $\alpha C \varepsilon$ to the tangent $E \varepsilon$ at the equator. It is evident that $E e = AC$, and $E \varepsilon = EC$. Through p and P draw the lines $p L l$, $NP z$, parallel to EC , and the lines $p N \varphi$, $IP \delta$ parallel to AB . Draw also $IK k$ parallel to EC .

Since, by hypothesis, the whole forces at A and E are inversely as AC and EC , $A a$ and $E e$ are as the forces acting at A and E . Consequently, the weights of the columns FD , LZ , and KL , will be represented by the areas $FfdD$, $LlzZ$, and $KklL$ (566.)

All the pressures or forces which act on the particles of the column pP may be resolved into forces acting parallel to AC , and forces acting parallel to EC , and the force acting on each particle is as its distance from the axis to which it is directed (564.) Therefore the whole force with which the column pP is pressed in the direction AC is to the force with which the column OP is pressed in the same direction, as the number of particles

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in pP to the number in OP , that is, as pP to OP . But there is only a part of this force employed in pressing the particles in the direction of the canal. Another part merely presses the fluid to the side of the canal pP . Draw Og perpendicular to pP . The force acting in the direction AC on any particle in pP is to its efficacy in the direction pP as OP to gP , that is, as pP to OP . Therefore, the pressure which the particle P sustains in the direction pP , from the action of all the particles in pP in the direction AC , is precisely equal to the pressure it sustains from the action of the column OP , acting in the same direction AC . But it has been shewn (566.) that the pressure of OP in the direction AC is precisely the same with the weight of the column LZ , which weight is represented by the area $Ll \approx Z$.

In the very same manner, the whole pressure on P in the direction pP arising from the pressure of each of the particles in pP in the direction EC , is precisely the same with the pressure on P , arising from the pressure of the column NP in this direction EC , that is, it is equal to the weight of the column FD , which is represented by the area $FfdD$.

Because E_s is equal to EC , we have $F\phi\delta D = \frac{CF^2 - CD^2}{2}$, $= \frac{Lp^2 - LO^2}{2}$, $= \frac{pO \times Om}{2}$. And in like manner, $K \times \lambda L = \frac{IO \times Oi}{2}$. But $pO \times Om :$
 $IO \times Oi = EC^2 : AC^2$, and therefore

$$F\phi\delta D : K \times \lambda L = EC^2 : AC^2$$

$$\text{but } K \times \lambda L : Kk / L = AC : EC$$

and

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in pP to the number in OP , that is, as pP to OP . But there is only a part of this force employed in pressing the particles in the direction of the canal. Another part merely presses the fluid to the side of the canal pP . Draw Og perpendicular to pP . The force acting in the direction AC on any particle in pP is to its efficacy in the direction pP as OP to gP , that is, as pP to OP . Therefore, the pressure which the particle P sustains in the direction pP , from the action of all the particles in pP in the direction AC , is precisely equal to the pressure it sustains from the action of the column OP , acting in the same direction AC . But it has been shewn (566.) that the pressure of OP in the direction AC is precisely the same with the weight of the column LZ , which weight is represented by the area $Ll \approx Z$.

In the very same manner, the whole pressure on P in the direction pP arising from the pressure of each of the particles in pP in the direction EC , is precisely the same with the pressure on P , arising from the pressure of the column NP in this direction EC , that is, it is equal to the weight of the column FD , which is represented by the area $FfdD$.

Because $E\varepsilon$ is equal to EC , we have $F\phi\delta D = \frac{CF^2 - CD^2}{2}$, $= \frac{Lp^2 - LO^2}{2}$, $= \frac{pO \times Om}{2}$. And in like manner, $K \times \lambda L = \frac{IO \times Oi}{2}$. But $pO \times Om :$
 $IO \times Oi = EC^2 : AC^2$, and therefore

$$F\phi\delta D : K \times \lambda L = EC^2 : AC^2$$

$$\text{but } K \times \lambda L : Kk / L = AC : EC$$

and

and $FfdD : F\phi\delta D = \Lambda C : EC$, therefore

$$FfdD : Kk/L = EC^2 \times \Lambda C^2 : AC^2 \times EC^2,$$

that is, in the ratio of equality. Now the area Kk/L represents the weight of the column KL , or the pressure exerted in the direction AC by the column IO .

Thus it appears that when the forces acting on the particles in the column pP are estimated in the direction of the canal, the pressure exerted on the particle P is equal to the united pressures of the columns OP and IO acting in the direction AC , that is, to the pressure of the fluids in the canal IP in its own direction.* Therefore the fluid in the canal IP will balance the fluid in the canal pP , and the particle P will have no tendency to move in either direction. And, since this is equally true, whatever may be the direction of the canal Pp , or $P\pi$, it follows that the particle P is equally pressed in every direction in the plane of the figure, and would remain at rest, if the whole spheroid were fluid.

But now let the canal Pp be in a plane different from a meridional plane (as in fig. 64. 4.) In whatever direction

* The student must not confound this with a composition of two pressures or forces NP and OP , composing a pressure or force pP . There is no such composition in the present case. It is only meant that the pressure in the direction pP arising from the gravitation of the particles in the canal, is the same, in respect of magnitude, with the pressure in the direction IP , arising from the gravitation of the fluid in IP .

tion Pp is disposed, a plane may be made to pass through it, perpendicular to the plane $EeQq$ of the equator of the spheroid. Let $pIqie$ be this plane. Its section with the spheroid will be an ellipse, similar to the generating ellipse $AEBQ$, as is well known. Let the meridional section $AEBQ$ pass through the point P of the canal pP . It will cut the section $eIqi$ in a line IPi perpendicular to its intersection eq with the equator of the spheroid, and therefore parallel to the axis acb of the section, if it do not coincide with this axis. Let CDE be the semidiameter of the generating ellipse which passes through the intersection D of Ii and eq ; and draw PZ parallel to DC , and Pz parallel to eq cutting acb in z , and join zZ and cC . It is plain that the plane passing through the axis AB of the spheroid and the axis ab of the section $eIqi$ is perpendicular to that section (for it bisects eq , which is a chord of the equatorial circle $LeQq$), and that the planes DcC and PzZ are parallel, and the angles at c and z right angles.

Let us now consider the forces which act on the particles of fluid in the canal pP . They are, as before, all resolvable into two, one of them parallel to AC , and the other perpendicular to it. Thus, the particle P is urged by a force in the direction PD parallel to AC , and proportional to its distance PD from the equator of the spheroid. It is also urged by a force in the direction PZ perpendicular to AC , and proportional to its distance PZ . This force PZ may be resolved into Pz and zZ .

The

The force zZ remains the same, for all the particles in the canal pP , zZ being equal to cC . But the force Pz is always proportional to the distance of the particle in the canal pP from the axis acb of the section $eIqi$. It is also to the axipetal force in the direction PZ as Pz to PZ .

Moreover, it has been shewn (573.) that the force in the direction PZ is to the force in the direction PD in the ratio of $AC^2 \times PZ$ to $EC^2 \times PD$, that is (on account of the similarity of the sections $AEBQ$ and $aebq$), as $ac^2 \times PZ$ to $ec^2 \times PD$. Therefore the force in the direction Pz is to the force in the direction PD as $ac^2 \times Pz$ to $ec^2 \times PD$. Wherefore, since from these elements it has been proved already that the whole pressure on P in the canal pP , lying in the plane $AEBQ$, is equal to the pressure of the canal IP , it follows that the pressure of the canal pP , lying in the plane $aebq$ is also equal to the pressure of the canal IP .

Thus it now appears that the particle P is urged in every direction with the same force by the fluid in any rectilinear canal whatever reaching to the surface. It is therefore *in equilibrio*; and, as it is taken at random, in any part of the spheroid, the whole fluid spheroid is *in equilibrio*.

We also see that the whole force with which any particle P is pressed in any direction whatever is to the pressure at the centre C as the rectangle IPi to AC^2 . For that is the proportion of the pressure of the canal IP

to

to that of the canal *AC*; and all canals terminating in the centre exert equal pressures.

578. It is now demonstrated that a mass of uniformly dense matter, influenced in every particle by gravitation, and so constituted that an equilibrium of force on every particle is necessary for the maintenance of its form, may exist, with a motion of rotation, in the form of an elliptical spheroid, if there be a proper adjustment between the proportion of the two axes and the time of the rotation. Whatever may be the proportion of the axes of an oblate spheroid, there is a rapidity of rotation which will induce that proportion between the undiminished gravity at the pole and the diminished gravity on the surface of the equator, which is required for the preservation of that form. But it has not been proved that a fluid sphere, when set in motion round its axis, must assume the form of an elliptical spheroid, but only that this is a possible form. This was all that Newton aimed at, and his proof is not free from reasonable objections. The great mathematicians since the days of Newton have done little more. They have not determined the figure that a fluid sphere, or a nucleus covered with a fluid, *must* assume when set in motion round its axis.* But they have added to the number of conditions that must be implemented, in order to produce *another kind* of assurance that an elliptical
spheroid

* Montucla says. (Vol. IV.) that M. le Gendre has demonstrated that an elliptical spheroid is the only possible form for a homogeneous fluid turning round its axis.

spheroid will answer the purpose, and by this limitation have greatly increased the difficulty of the question. M. Clairaut, who has carried his scruples farther than the rest, requires, besides the three conditions which have been shewn to consist with the permanence of the elliptical form, that it also be demonstrated, *1mo*, That a canal of any form whatever must every where be *in equilibrio*: *2do*, That a canal of any shape, reaching from one part of the surface, through the mass, or along the surface, to any other part, shall exert no force at its extremities: *3tio*, That a canal of any form, returning into itself, shall be *in equilibrio* through its whole extent.

579. I apprehend that in the case of uniform density, all these conditions are involved in the proposition in art. (577.) For we can suppose the canal pP of fig. 64. N^o 4. to communicate with the canal $P\delta$. It has been shewn that they are *in equilibrio* in P . The canal 4β may branch off from $P\delta$. These are *in equilibrio* in the point 4. The canal 3α may branch off at 3, and they will be still *in equilibrio*; and the canal 2γ will be *in equilibrio* with all the foregoing. Now these points of derivation may be multiplied, till the polygonal canal $pP432\gamma$ becomes a canal of continual curvature of any form. In the next place, this canal exerts no force at either end. For the *equilibrium* is proved in every state of the canal pP —it may be as short as we please—it may be evanescent, and actually cease to have any length, without any interruption of the *equilibrium*.

Therefore,

Therefore, there is no force exerted at its extremity to disturb the form of the surface. It may be observed that this very circumstance proves that the direction of gravity is perpendicular to the surface. And it must be observed that the perpendicularity of gravity to the surface is not employed in demonstrating this proposition. The whole rests on the propositions in art. 562. 563. and 564, both of which we owe to Mr M'Laurin.

580. Having now demonstrated the competency of the elliptical spheroid for the rotation of a planet, we proceed to investigate the precise proportion of diameters which is required for any proposed rotation. For example, What protuberancy of the equator will diffuse the ocean of this Earth uniformly, consistently with a rotation in $23^{\text{h}} 56' 04''$, the planet being uniformly dense?

Let p and e express the primitive gravity of a particle placed at the pole and at the surface of the equator, arising solely from the gravitation to every particle in the spheroid, and let c represent the centrifugal tendency at the surface of the equator, arising from the rotation. We shall have an elliptical spheroid of a permanent form, if AC be to EC as $e - c$ is to p (569.) We must therefore find, first of all, what is the proportion of p to e resulting from any proportion of AC to EC .

To accomplish this in general terms with precision, appeared so difficult a task, even to Newton, that he avoided it, and took an indirect method, which his sagacity

shewed him to be perfectly safe ; and even this was difficult. It is in the complete solution of this problem that the genius of M'Laurin has shewn itself most remarkable both for acuteness and for geometrical elegance. It is not exceeded (in the opinion of the first mathematicians) * by any thing of Archimedes or Apollonius. For this reason, it is to be regretted that we have not room for the series of beautiful propositions that are necessary in his method. We must take a shorter course, limited indeed to spheroids of very small eccentricity (whereas the method of M'Laurin extends to any degree of eccentricity), but, with this limitation, perfectly exact, and abundantly easy and simple. It is, in its chief steps, the method followed by M. Boscovich.

580. Let $AEBQ$ (fig. 65.) represent the terrestrial spheroid, nearly spherical, and let $AeBq$ and $EaQb$ represent the inscribed and circumscribed spheres. With the axis and parameter AB describe the parabola AFG , drawing the ordinates $BD F$, ECH , &c. Describe also the curve line $A I L G$, such, that we have, in every point of it, $AB : AD = DF : DI$; $AB : AC = CH : CI$, &c.

Our first aim shall be to find an expression and value of the polar gravity. We may conceive the spheroid as a sphere, on which there is spread the redundant matter contained between the spherical and the spheroidal surfaces.

* See Bossuet *Hist. des Mathematiques*,

faces. We know the gravitation of the polar particle A to the sphere, and now want to have the measure of its gravitation to this redundant matter. Suppose the figure to turn round the axis AB . The semiellipsis AEB will generate a spheroidal surface; the semicircle AeB will generate a spherical surface, and the intercepted portions Pp , Ee , &c. of the ordinates will generate flat rings of the redundant matter. As the deviation from a sphere is supposed very small (Ee not exceeding the 500th part of EQ), we may suppose, without any sensible error, that Ap is the distance of A from the whole of the ring generated by Pp .

Proceeding on this assumption, we say that the gravitation of A to the rings generated by Pp , Ee , &c. is proportional to the portions FI , HL , &c. of the corresponding ordinates DF , CH , &c., and that the gravitation of A to the whole redundant matter may be expressed by the surface $AFHGLIA$ comprehended between the lines $AFHG$ and $AILG$.

For, the absolute gravitation of A to the ring Pp is directly as the surface of the ring, and inversely as the square of its distance from A . Now, the surface of the ring is as its breadth, and its circumference jointly. Its breadth Pp , and also its circumference, being proportional to Dp , the surface is proportional to Dp^2 . The absolute gravitation is therefore proportional to $\frac{Dp^3}{Ap^2}$. This may be resolved into forces in the directions AD and Dp . The force in the direction Dp is balanced by

by an equal force on the other side of the axis. Therefore, to have the gravitation in the direction of the axis, the value of the absolute gravitation in the direction $A\rho$ must be reduced in the proportion of $A\rho$ to AD . It therefore becomes $\frac{D\rho^2 \times AD}{A\rho^2 \times A\rho}$, = $\frac{D\rho^2 \times AD}{A\rho^3}$, or, which is the same thing, $\frac{D\rho^2 \times AD \times A\rho}{A\rho^4}$. But $A\rho^2 = AB \times AD$, and $A\rho^4 = AB^2 \times AD^2$. Also $D\rho^2 = AD \times DB$. Therefore the value last found becomes $\frac{AD \times DB \times AD \times A\rho}{AB^2 \times AD^2}$, which is equal to, or the same thing with $\frac{DB \times A\rho}{AB^2}$. Since AB^2 is a constant quantity, the gravitation in the direction AC to the ring generated by $P\rho$ is proportional to $DB \times A\rho$.

It is very obvious that DF , CH , BG , &c. are respectively equal to $A\rho$, Ae , AB , &c. Therefore the gravitation to the matter in the ring generated by $P\rho$ is proportional to $DB \times DF$.

Now, by the construction of the curve line ALG , we have

$$AB : AD = DF : DI$$

therefore $AB : DB = DF : IF$

and $AB \times IF = DB \times DF$

Therefore, since AB is constant, IF is proportional to $DB \times DF$, that is, to the gravitation to the ring generated by $P\rho$. Therefore the gravitation to the whole redundant matter may be represented by the space $AHGLA$.

Let

Let π be the periphery of a circle of which the radius is 1. The circumference of that generated by Ee will be $\pi \times Ce$, and its surface = $\pi \times Ce \times Ee$, and the absolute gravitation to it is $\frac{\pi \times Ce \times Ee}{Ae^2}$, or $\frac{\pi \times Ce \times Ee}{2AC^2}$, that is, $\frac{\pi \times Ee}{2AC}$. This, when reduced to the direction AC , becomes $\frac{\pi \times Ee \times AC}{2Ae \times AC}$, that is, $\frac{\pi \times Ee}{2Ae}$, or $\frac{\pi \times Ee \times Ae}{2Ae^2}$. And because $Ae^2 = 2AC^2$, and $LH = \frac{1}{2}CH$, = $\frac{1}{2}Ae$, the reduced gravitation becomes $\frac{\pi \times Ee}{2AC^2} \times LH$.

This being the measure or representative of the gravitation to the material surface or ring generated by Ee , the gravitation to the whole redundant matter contained between the spheroid and the inscribed sphere will be represented by $\frac{\pi \times Ee}{2AC^2}$ multiplied by the space comprehended between the curve lines AFG and ALG . We must find the value of this space.

The parabolic space $AHGBA$ is known to be = $\frac{2}{3}AB \times BG$, = $\frac{2}{3}AB^2$. The square of DI is proportional to the cube of BD . For, by the construction of the curve $AB^2 : AD^2 = DF^2 : DI^2$, and $DI^2 = \frac{AD^2 \times DF^2}{AB^2}$, = $\frac{AD^2}{AB} \times \frac{DF^2}{AB}$, = $\frac{AD^2}{AB} AD$, = $\frac{AD^3}{AB}$. Therefore DI is proportional to $AD^{\frac{3}{2}}$, and the area $ABGLA$ is = $\frac{2}{3}AB \times BG$, = $\frac{2}{3}AB^2$. Take this from the parabolic area $\frac{2}{3}AB^2$, and there remains $\frac{4}{15}AB^2$

$\frac{4}{15} A B^2$ for the value of $A L G H A$. This is equal to $\frac{16}{15} A C^2$.

Now, the gravitation of A to the redundant matter was shewn to be $= A L G H A \times \frac{\pi \times E e}{2 A C^2}$. This now becomes $\frac{16}{15} A C^2 \times \frac{\pi \times E e}{2 A C^2}$, or $\frac{8}{15} \pi \times E e$. Such is the gravitation of a particle in the pole of the spheroid to the redundant matter spread over the inscribed sphere.

The gravitation of a particle situated on the surface of the equator to the same redundant matter is not quite so obvious as the polar gravity, but may be had with the same accuracy, by means of the following considerations.

581. Let $A B a b$ (fig. 66.) represent an oblate spheroid, formed by rotation round the shorter axis $B b$ of the generating ellipse, and viewed by an eye situated in the plane of its equator. Let $A E a e$ be the circumscribed sphere. This spheroid is deficient from the sphere by two meniscuses or cups, generated by the rotation of the lunulæ $A E a B A$ and $A e a b A$.

Now suppose the same generating ellipse $A B a b A$ to turn round its longer axis $A a$. It will generate an oblong spheroid, touching the oblate spheroid in the whole circumference of one elliptical meridian, *viz.* the meridian $A B a b A$ which passes through the poles A and a of this oblong spheroid. It touches the equator of the oblate spheroid only in the points A and a , and has the
diameter

diameter Aa for its axis. This oblong spheroid is otherwise wholly within the oblate spheroid, leaving between their surfaces two meniscuses of an oblong form. This may be better conceived by first supposing that both the spheroids and also the circumscribed sphere are cut by a plane $PGgp$, perpendicular to the axis Aa of the oblong spheroid, and to the plane of the equator of the oblate spheroid. Now suppose that the whole figure makes the quarter of a turn round the axis Bb of the oblate spheroid, so that the pole a of the oblong spheroid comes quite in front, and is at C , the eye of the spectator being in the axis produced. The equator of the oblong spheroid will now appear a circle $OBobO$, touching the oblate spheroid in its poles B and b . The section of the plane Pp with the circumscribed sphere will now appear as a circle $P'Rp'r$. Its section with the oblate spheroid will appear an ellipse $RG'rg'$ similar to the generating ellipse $ABab$, as is well known. And its section with the oblong spheroid will now appear a circle $IG'ig'$ parallel to its equator $OBob$. Pp is equal to $P'p'$, and Gg to $G'g'$. Thus it appears that as every section of the oblate spheroid is deficient from the concomitant section of the circumscribed sphere by the want of two lunulæ $RP'rG'$ and $Rp'r g'$, so it exceeds the concomitant section of the oblong spheroid by two lunulæ $G'Rg'I$ and $G'rg'i$. It is also plain that if these spheroids differ very little from perfect spheres, as when EB does not exceed $\frac{1}{100}$ of Ee , the deficiency of each section Gg from the concomitant section of the circum-

scribed sphere is very nearly equal to its excess above the concomitant section of the inscribed oblong spheroid. It may safely be considered as equal to one half of the space contained between the circles on the diameters $P'p'$ and $G'g'$,* in the same way that we considered the lunula $APeBepA$ of fig. 65. as one half of the space contained between the semicircles AeB and aEb .

From this view of the figure, it appears that the gravitation of a particle a in the equator of the oblate spheroid to the two cups or meniscuses $RP'rG'$ and $Rp'r g'$, by which the oblate spheroid is less than the circumscribed sphere, may be computed by the very same method that we employed in the last proposition. But, instead of computing (as in last proposition) the gravitation of a to the ring generated by the revolution of PG (fig. 66.), that is, to the surface contained between the two circles $RP'r p'$ and $IG'ig'$, we must employ only the two lunulæ $RP'rG'R$ and $Rp'r g'R$. In this way, we may account the gravitation to the deficient matter (or the deficiency of gravitation) to be one half of the quantity determined by that proposition, and therefore $= \frac{4}{15} \pi \times Ee$ of fig. 65. The last proposition gave us the gravitation to all the matter by which the spheroid exceeded the inscribed sphere. The present proposition gives

* For the circumscribed circle is to the ellipse as the ellipse to the inscribed circle. When the extremes differ so little, the geometrical and arithmetical mean will differ but insensibly.

gives the gravitation to all the matter by which it falls short of the circumscribed sphere.

582. We can now ascertain the primitive gravitation at the pole and at the equator, by adding or subtracting the quantities now found to or from the gravitation to the spheres. Let r be the radius of the sphere, and πr the circumference of a great circle. The diameter is $2r$. The area of a great circle is $\frac{\pi r^2}{2}$, and the whole surface of the sphere is $2\pi r^2$, and its solid contents is $\frac{2}{3}\pi r^3$. Therefore, since the gravitation to a sphere of uniform density is the same as if all its matter were collected in its centre, and is as the quantity of matter directly, and as the square of the distance r inversely, the gravitation to a sphere will be proportional to $\frac{2}{3} \frac{\pi r^3}{r^2}$, that is, to $\frac{2}{3}\pi r$. *

Now

* I beg leave to mention here a circumstance which should have been taken notice of in art. 464, when the first principles of spherical attractions were established. It was shewn that the gravitation of the particle P to the spherical surface generated by the rotation of the arch A D' T is equal to its gravitation to the surface generated by the rotation of B D T. Therefore if P be infinitely near to A, so that the surface generated by A D' T may be considered as a point or single particle, the gravitation to that particle is equal to the gravitation to all the rest of the surface; that is, it is one half of

Now let $AEBQ$ (fig. 65.) be an oblate spheroid, whose poles are A and B . The gravity of a particle A to the sphere whose radius is AC is $\frac{2}{3}\pi \times AC$, $= \frac{2}{3}\pi \times EC - \frac{2}{3}\pi \times Ee$, or $\frac{2}{3}\pi \times EC - \frac{10}{15}\pi \times Ee$. Add to this its gravitation $\frac{8}{15}\pi \times Ee$, to the redundant matter. The sum is evidently $\frac{2}{3}\pi \times EC - \frac{2}{15}\pi \times Ee$.

The gravitation of the particle E on the surface of the equator to a sphere whose radius is EC is $\frac{2}{3}\pi \times EC$. From this subtract its deficiency of gravitation, viz. $\frac{4}{15}\pi \times Ee$, and there remains the equatoreal primitive gravity $= \frac{2}{3}\pi \times EC - \frac{4}{15}\pi \times Ee$.

Therefore, in this spheroid, the polar gravity is to the equatoreal gravity as $\frac{2}{3}\pi \times EC - \frac{2}{15}\pi \times Ee$ to $\frac{2}{3}\pi \times EC - \frac{4}{15}\pi \times Ee$, or (dividing all by $\frac{2}{3}\pi$) as $EC - \frac{1}{5}Ee$ to $EC - \frac{2}{5}Ee$, or (because Ee is supposed to be very small in comparison with EC) as EC to $EC - \frac{1}{5}Ee$. In general terms, let g represent the mean gravity, p the polar, and e the equatoreal gravity, r the radius of the inscribed sphere, and x the elevation Ee of the equator above the inscribed sphere. We have, for a general expression of this proportion of the primitive

the whole gravitation. If we suppose P and A to coincide, that is, make P one of the particles of the surface, its gravitation to the spherical surface will be only one half of what it was when it was without the surface; and if we suppose P adjoining to A internally, it will exhibit no gravitation at all.

tive gravitations, $p : e = r + \frac{1}{3}x : r$, or (because x is very small in comparison with r), $p : e = r : r - \frac{1}{3}x$. This last is generally the most convenient, and it is exact, if r be taken for the equatoreal radius.

583. Had the spheroid been prolate (oblong) the same reasoning would have given us $p : e = r : r + \frac{1}{3}x$.

I may add here that the gravitation at the pole of an oblong spheroid, the gravitation at the equator of an oblate spheroid (having the same axes) and the gravitation to the circumscribed sphere, on any point of its surface, are proportional, respectively, to $\frac{1}{3}r + \frac{1}{15}x$; $\frac{1}{3}r + \frac{1}{3}x$; and $\frac{1}{3}r + \frac{1}{3}x$. *

584. It now appears, as was formerly hinted (567.) that we cannot have an elliptical spheroid of uniform density, in

* Many questions occur, in which we want the gravitation of a particle P' situated in the direction of any diameter CP (fig. 65.) Draw the conjugate diameter CM , and suppose the spheroid cut by a plane passing through CM perpendicular to the plane of the figure. This section will be an ellipse, of which the femiaxes are CM and $CE (= r + x)$. A circle whose radius is the mean proportional between CM and CE has the same area with this section, and the gravitation to this circle will be the same (from a particle placed in the axis) with the gravitation to this section. Therefore, as the angle PCM is very nearly a right angle, the gravitation of P'

in which the gravitation at the pole is to that at the equator as the equatoreal radius to the polar radius. This would make $p : e = r : r - x$, a ratio five times greater than that which results from a gravitation proportional to $\frac{1}{d^2}$.

Thus have we obtained, with sufficient accuracy, the ratio of polar and equatoreal gravity, unaffected by any external force, and we are now in a condition to tell what velocity of rotation will so diminish the equatoreal gravitation that the remaining gravity there shall be to the polar gravity as AC to EC .

585. Let c be taken to represent the centrifugal tendency generated at the surface of the equator by the rotation of the planet round its axis, and let the other symbols be retained. The sensible gravity at the equator is $e - c$, the polar gravity p , and the excess of the equatoreal radius above the femiaxis r is x .

We have shewn (582.) that the primitive gravities at the pole and the equator are in the ratio of r to $r - \frac{1}{5}x$,

OR,

P to the spheroid will be the same with its polar (or axicular) gravitation to another spheroid, whose polar femiaxis is PC , and whose equatoreal radius is the mean proportional between CM and CE . This is easily computed. If the arch PE be $35^\circ 16'$, a sphere having the radius PC has the same capacity with the spheroid $AEBQ$ (when Ee is very small). Hence follows what was said in the note on art. 572.

α , (because α is a very small part of r), in the ratio of $r + \frac{1}{5}\alpha$ to r . That is, $r : r + \frac{1}{5}\alpha = e : p$. This gives $p = e + \frac{e\alpha}{5r}$. Therefore the ratio of the *sensible* equatorial gravity to the gravity at the pole is $e - c : e + \frac{e\alpha}{5r}$, or, very nearly, $e : e + \frac{e\alpha}{5r} + c$. Therefore we must have, for a revolving sphere of small eccentricity,

$$e : e + \frac{e\alpha}{5r} + c = r : r + \alpha$$

and
$$e : \frac{e\alpha}{5r} + c = r : \alpha$$

consequently
$$e\alpha = \frac{e\alpha}{5} + rc$$

and
$$e\alpha - \frac{e\alpha}{5} \text{ or } \frac{4e\alpha}{5} = rc$$

and
$$4e\alpha = 5rc, \text{ and } \alpha = \frac{5rc}{4e}$$

and the ellipticity $\frac{\alpha}{r} = \frac{5c}{4e}$, that is,

Four times the primitive gravity at the equator is to five times the centrifugal force of rotation as the semiaxis to the elevation of the equator above the inscribed sphere.

586. It is a matter of observation that the diminution of equatoreal gravity by the Earth's rotation in $23^{\text{h}} 56' 4''$ is nearly $\frac{1}{289}$. Therefore $4 \times 289 : 5 = r : \alpha = 231\frac{1}{7} : 1$, very nearly. This is the ratio deduced by Newton in his indirect, and seemingly incurious, method. That method has been much criticised by his scholars, as if it could be supposed that Newton was ignorant that the proportionality

proportionality employed by him, in a rough way, was not *necessarily* involved in the nature of the thing. But Newton knew that, in the present case, the error, if any, must be altogether insignificant. He did not demonstrate, but assumed as granted, that the form is elliptical, or that an elliptical form is competent to the purpose. His justness of thought has been so repeatedly verified in many cases as abstruse as this, that it is unreasonable to ascribe it to conjecture, and it should rather, as by Dan. Bernoulli, be ascribed to his penetration and sagacity. He had so many new wonders to communicate, that he had not time for all the lemmas that were requisite for enabling inferior minds to trace his steps of investigation.

587. When considering the astronomical phenomena, some notice was taken of the attempts which have been made to decide this matter by observation alone, by measuring degrees of the meridian in different latitudes.

But such irregularity is to be seen among the measures of a degree, that the question is still undecided by this method. All that can be made evident by the comparison is that the Earth is oblate, and much more oblate than the ellipse of Mr Hermann; and that the medium deduction approaches much nearer to the Newtonian form. When we recollect that the error of one second in the estimation of the latitude induces an error of more than thirty yards in the measure of the degree, and that the form of this globe is to be learned, not from the lengths of the degrees, but from the differences of those lengths,

it must be clear that, unless the lengths, and the celestial arc corresponding, can be ascertained with great precision indeed, our inference of the variation of curvature must be very vague and uncertain. The perusal of any page of the daily observations in the observatory of Paris will shew that errors of 5" in declination are not uncommon, and errors of 2" are very frequent indeed. * So many circumstances may also affect the measure of the terrestrial arc, that there is too much left to the judgement and *choice* of the observer, in drawing his conclusions. The history of the first measurement of the French meridian by Cassini and La Hire is a proof of this. The degrees seemed to increase to the southward—the observations were affirmed to be excellent—and for some time the Earth was held to be an oblong spheroid. Philosophy prevailed, and this was allowed to be impossible;—yet the observations were still held to be faultless, and the blame was laid on the neglect of circumstances which should have been considered. It was afterwards found that the deduced measures

* I mention particularly the daily observations of the Parisian Observatory, because the French astronomers are disposed to rest the question on the observations of their own academicians, who have certainly surpassed all the astronomers of Europe in the extent of their measurement of degrees. I see no reason for giving their observations made in distant places a greater accuracy than what is to be found in the Royal Observatory, with capital instruments, fixed up in the most solid manner.



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fures did not agree with some others of unquestionable authority, but would agree with them if the corrections were left out;—they were left out, and the observations declared excellent, because agreeable to the doctrine of gravitation. *

588. The theory of universal gravitation affords another means of determining the form of the terraqueous globe directly from observation. Mr Stirling says, very justly, that the diminution of gravity deducible from the remark of M. Richer, and confirmed by many similar observations, gives an incontestible proof, both of the rotation of the Earth, and of its oblate figure. It could not be of an oblate figure, and have the ocean uniformly distributed,

* They were reconciled with the doctrine of gravitation by attributing the enlargement of the southern degrees to the action of the Pyrenean mountains, and those in the south of France, upon the plummets. But it appears clearly, by the examination of these observations by Professor Celsius, that the observations were very incorrect, and some of them very injudiciously contrived (See Phil. Trans. N^o 457. and 386.) The palpable inaccuracies gave such latitude for adjustment that it was easy for the ingenious Mr Mairan to combine them in such a manner as to deduce from them inferences in support of opinions altogether contradictory of those of the academy. Have we not a remarkable example of the doubtfulness of such measures, in the measurement of the Lapland degree? It is found to be almost 200 fathoms too long.

distributed, without turning round its axis ; and it could not turn round its axis without inundating the equator, unless it have an oblate form, accompanied with diminished equatoreal gravity. By the Newtonian theory, the increments of gravity as we approach the poles are in the duplicate ratio of the sines of the latitude. The increments of the length of a seconds pendulum will have the same proportion. Nothing can be ascertained by observation with greater accuracy than this. For the London artists can make clocks which do not vary one second from mean motion in three or four days. We need not measure the change in the length of the pendulum, a very delicate task—but the change of its rate of vibration by a change of place, which is easily done ; and we can thus ascertain the force of gravity without an error of one part in 86400. This surpasses all that can be done in the measurement of an angle. Accordingly, the ellipticities deduced from the experiments with pendulums are vastly more consistent with each other, and it were to be wished that these experiments were more repeated. We have but very few of them.

589. Yet even these experiments are not without anomalies. Since, from the nature of the experiment, we cannot ascribe these to errors of observation, and the doctrine of universal gravitation is established on too broad a foundation to be called in question for these anomalies, philosophers think it more reasonable to attribute the anomalies to local irregularity in terrestrial gravity.

If, in one place, the pendulum is above a great mass of solid and dense rock, perhaps abounding in metals, and, in another place, has below it a deep ocean, or a deep and extensive stratum of light sand or earth, we should certainly look for a retardation of the pendulum in the latter situation. The French academicians compared the vibrations of the same pendulum on the sea-shore in Peru, and near the top of a very lofty mountain, and they observed that the retardation of its motion in the latter situation was not so great as the removal from the centre required, according to the Newtonian theory, viz. in the proportion of the distance (the gravity being in the inverse duplicate proportion). * But it should not be so much retarded. The pendulum was not raised aloft in the air, but was on the top of a great mountain, to which, as well as to the rest of the globe, its gravitation was directed. Some observations were reported to have been made in Switzerland, which shewed a greater gravitation on the summit of a mountain than in the adjacent valleys; and much was built on this by the partizans of vortices.

* The length of a pendulum vibrating seconds was found to be 439,21 French lines on the sea-shore at Lima; when reduced to time at Quito, 1466 fathoms higher, it was 438,88; and on Pichinka, elevated 2434 fathoms, it was 438,69. Had gravity diminished in the inverse duplicate ratio of the distances, the pendulum at Quito should have been 438,80, and at Pichinka it should have been 438,55.

tices. But, after due inquiry, the observations were found to be altogether fictitious. It may just be noticed here, that some of the anomalies in the experiments with pendulums may have proceeded from magnetism. The clocks employed on those occasions probably had gridiron pendulums, having five or seven iron rods, of no inconsiderable weight. We know, for certain, that the lower end of such rods acquires a very distinct magnetism by mere upright position. This may be considerable enough, especially in the circumpolar regions, to affect the vibration, and it is therefore adviseable to employ a pendulum having no iron in its composition.

Although the deduction of the form of this globe from observations on the variations of gravity is exposed to the same cause of error which affects the position of the plummet, occasioning errors in the measure of a degree, yet the errors in the variations of gravity are incomparably less. What would cause an error of a whole mile in the measure of a degree will not produce the $\frac{1}{1000}$ part of this error in the difference of gravity.

590. These observations naturally lead to other reflections. Newton's determination of the form of the terraqueous globe, is really the form of a homogeneous and fluid or perfectly flexible spheroid. But will this be the form of a globe, constituted as ours in all probability is, of beds or layers of different substances, whose density probably increases as they are farther down?

This is a very pertinent and momentous question.

But

But this outline of mechanical philosophy will not admit of a discussion of the many cases which may reasonably be proposed for solution. All that can, with propriety, be attempted here is to give a *general* notion of the change of form that will be induced by a varying density. And even in this, our attention must be confined to some simple and probable case. We shall therefore suppose the density to increase as we penetrate deeper, and this in such sort, that at any one depth the density is uniform. It is highly improbable that the internal constitution of this globe is altogether irregular.

591. We shall therefore suppose a sphere of solid matter, equally dense at equal distances from the centre, and covered with a less dense fluid; and we shall suppose that the whole has a form suitable to the velocity of its rotation. It is this form that we are to find out. With this view, let us suppose that all the matter, by which the solid globe or nucleus is denser than the fluid, is collected in the centre. We have seen that this will make no change in the gravitation of any particle of the incumbent fluid. Thus, we have a solid globe, covered with a fluid of the same density, and, besides the mutual gravitation of the particles of the fluid, we have a force of the same nature acting on every one of them, directed to the central redundant matter. Now, let the globe liquefy or dissolve. This can induce no change of force on any particle of the fluid. Let us then determine the form of the now fluid spheroid, which will
maintain

maintain itself in rotation. This being determined, let the globe again become solid. The remaining fluid will not change its form, because no change is induced on the force acting on any particle of the fluid. Call this Hypothesis A.

592. In order to determine this state of *equilibrium*, or the form which insures it, which is the chief difficulty, let us form another hypothesis B, differing from A only in this circumstance, that the matter collected in the centre, instead of attracting the particles of the incumbent fluid with a force decreasing in the inverse duplicate ratio of their distances, attracts them with a force increasing in the direct ratio of their distances, keeping the same intensity at the distance of the pole as in hypothesis A. This fictitious hypothesis, similar to Hermann's, is chosen, because a mass so constituted will maintain the form of an accurate elliptical spheroid, by a proper adjustment of the proportion of its axis to the velocity of its rotation. This will easily appear. For we have already seen that the mutual gravitation of the particles of the elliptical fluid spheroid produces, in each particle, a force which may be resolved into two forces, one of them perpendicular to the axis, and proportional to the distance from it, and the other perpendicular to the equator, and proportional to the distance from its plane. There is now by hypothesis B superadded, on each particle, a force proportional to its distance from the centre, and directed to the centre. This may also be resolved into a force perpendicular

perpendicular to the axis, and another perpendicular to the equator, and proportional to the distances from them. Therefore the whole combined forces acting on each particle may be thus resolved into two forces in those directions and in those proportions. Therefore a mass so constituted will maintain its elliptical form, provided that the velocity of its rotation be such that the whole forces at the pole and the equator are inversely as the axes of the generating ellipse. We are to ascertain this form, or this required magnitude of the centrifugal force. Having done this, we shall restore to the accumulated central matter its natural gravitation, or its action on the fluid in the inverse duplicate ratio of the distances, and then see what change must be made on the form of the spheroid in order to restore the *equilibrium*.

593. Let $B A b a$ (fig. 67.) be the fictitious elliptical spheroid of hypothesis B. Let $B E b e$ be the inscribed sphere. Take $E G$, perpendicular to $C E$, to represent the force of gravitation of a particle in E to the central matter, corresponding to the distance $C E$ or $C B$. Draw $C G$. Draw also $A I$ perpendicular to $C A$, meeting $C G$ in I . Describe the curve $G L R$, whose ordinates $G E$, $L A$, $R M$, &c. are proportional to $\frac{1}{C E^2}$, $\frac{1}{C A^2}$, $\frac{1}{C M^2}$, &c. These ordinates will express the gravitations of the particles E , A , M , &c. to the central matter by hypothesis A.

In hypothesis A, the gravitation of A is represented by

by AL, but in hypothesis B it is represented by AI. For in hypothesis B the gravitations to this matter are as the distances. EG is the gravitation of E in both hypotheses. Now, $EG : AL = CA^2 : CE^2$, but $EG : AI = CE : CA$.—In hypothesis A the weight of the column AE is represented by the space ALGE, but by AIGE in hypothesis B. If therefore the spheroid of hypothesis B was in *equilibrio*, while turning round its axis, the *equilibrium* is destroyed by merely changing the force acting on the column EA. There is a loss of pressure or weight sustained by the column EA. This may be expressed by the space LGI, the difference between the two areas EGIA and EGLA. But the *equilibrium* may be restored by adding a column of fluid AM, whose weight ALRM shall be equal to LGI, which is very nearly $= \frac{LI \times AE}{2}$.

In order to find the height of this column, produce GE on the other side of E, and make EF to EG as the density of the fluid to the density by which the nucleus exceeded it. EF will be to EG as the gravitation of a particle in E to the globe (now of the same density with the fluid) is to its gravitation to the redundant matter collected in the centre. Now, take DE to represent the gravitation of E to the fluid contained in the concentric spheroid $E\beta e\beta$, which is somewhat less than its gravitation to the sphere $EBeb$. Draw CDN. Then AN represents the gravitation of A to the whole fluid spheroid, by § 558. In like manner, NI is the u-

nited gravitation of A to both the fluid and the central matter, in the same hypothesis. But in hypothesis A, this gravitation is represented by NL.

Let NO represent the centrifugal force affecting the particle A, taken in due proportion to NA or NL, its whole gravitation in hypothesis A. Draw CKO. DK will be the centrifugal force at E. The space OKGI will express the whole sensible weight of the fluid in AE, according to hypothesis B, and OKGL will express the same, according to hypothesis A. LGI is the difference, to be compensated by means of a due addition AM.

This addition may be defined by the quadrature of the spaces GEAL and GLI. But it will be abundantly exact to suppose that GLR sensibly coincides with a straight line, and then to proceed in this manner. We have, by the nature of the curve GLR,

$$AL : EG = EC^2 : AC^2$$

Also AH, or $EG : AI = EC : AC$

Therefore $AL : AI = EC^3 : AC^3$.

Now, when a line changes by a very small quantity, the variation of a line proportional to its cube is thrice as great as that of the line proportional to the root. HI is the quantity proportional to EA the increment of the root EC. IL is proportional to the variation of the cube, and is therefore very nearly equal to thrice HI.

Therefore

Therefore since $EG : HI = EC : AE$, we may

state $EG : LI = EC : 3 AE$,

or $3 EG : LI = EC : AE$.

Now, $QOLR$ may be considered as equal to $QR \times AM$, or as equal to $KG \times AM$, and LGI may be considered as equal to $LI \times \frac{1}{2} AE$, and $2 KG \times AM = LI \times AE$.

Therefore $2 KG : AE = LI : AM$

but $EC : AE = 3 EG : LI$

therefore $2 KG \times EC : AE^2 = 3 EG : AM$

and $2 KG : \frac{AE^2}{EC} = 3 EG : AM$

and $2 KG : 3 EG = \frac{AE^2}{EC} : AM$

That is, twice the sensible gravity at the equator is to thrice the gravitation to the central matter as a third proportional to radius and the elevation of the equator is to the addition necessary for producing the *equilibrium* required in hypothesis A.

This addition may be more readily conceived by means of a construction. Make $AE : Ee = 2 KG : 3 EG$. Draw ea parallel to EA , and draw Cem , cutting AN in m . Then am is the addition that must be made to the column AC . A similar addition must be made to every diameter CT , making $2 KG : 3 EG = \frac{TV^2}{CV} : Tt$, and the whole will be *in equilibrio*.

594. This determination of the ellipticity will equally suit those cases where the fluid is supposed denser than

the solid nucleus, or where there is a central hollow. For EG may be taken negatively, as if a quantity of matter were placed in the centre acting with a repelling or centrifugal force on the fluid. This is represented on the other side of the axis Bb . The space gil in this case is negative, and indicates a diminution of the column ae , in order to restore the *equilibrium*.

595. It is evident that the figure resulting from this construction is not an accurate ellipse. For, in the ellipse, Tt would be in a constant ratio to VT , whereas it is as VT^2 by our construction. But it is also evident that in the cases of small deviation from perfect sphericity, the change of figure from the accurate ellipse of hypothesis B is very small. The greatest deviation happens when Ee is a maximum. It can never be sensibly greater in proportion to AE than $\frac{3}{2}$ of AE is in proportion to EC , unless the centrifugal force FD be very great in comparison of the gravity DE . In the case of the Earth, where EA is nearly $\frac{1}{110}$ of EC , if we suppose the mean density of the Earth to be five times that of sea water, am will not exceed $\frac{1}{11111}$ of EC , or $\frac{1}{9111}$ of EA .

596. We are not to imagine that, since central matter requires an addition AM to the spheroid, a greater density in the interior parts of this globe requires a greater equatoreal protuberancy than if all were homogeneous; for it is just the contrary. The spheroid to which
the

the addition must be made is not the figure suited to a homogeneous mass, but a fictitious figure employed as a step to facilitate investigation. We must therefore define its ellipticity, that we may know the shape resulting from the final adjustment.

Let f be the density of the fluid, and n the density of the nucleus, and let $n - f$ be $= q$, so that q corresponds with $E G$ of our construction, and expresses the redundant central matter (or the central deficiency of matter, when the fluid is denser than the nucleus). Let BC or EC be r , AE be x , and let g be the mean gravity (primitive), and c the centrifugal force at A . Lastly, let π be the circumference when the radius of the circle is 1 .

The gravitation of B to the fluid spheroid is $\frac{2}{3} \pi f r$ (582.), and its gravitation to the central matter is $\frac{2}{3} \pi q r$. The sum of these, or the whole gravitation of B , is $\frac{2}{3} \pi n r$. This may be taken for the mean gravitation on every point of the spheroidal surface.

But the whole gravitation of B differs considerably from that of A .

1mo. CA , or CE , is to $\frac{1}{3} AE$ as the primitive gravity of B to the spheroid is to its excess above the gravitation (primitive) of A to the same, (582.) That is, $r : \frac{1}{3} x$ $= \frac{2}{3} \pi f r : \frac{2}{15} \pi f x$, and $\frac{2}{15} \pi f x$ expresses this excess.

2do. In hypothesis B , we have CE to CA as the gravitation of B or E to the central matter is to the gravitation of A to the same. Therefore CE is to EA as the gravitation of E to this matter is to the excess of A 's gravitation

gravitation to the same. This excess of A's gravitation is expressed by $\frac{2}{3} \pi q x$, for $r : x = \frac{2}{3} \pi q r : \frac{2}{3} \pi q x$.

3th. Without any sensible error, we may state the ratio of g to c as the ratio of the whole gravitation of A to the centrifugal tendency excited in A by the rotation. Therefore $g : c = \frac{2}{3} \pi n r : \frac{2 \pi n r c}{3 g}$, and this centrifugal tendency of the particle A is $\frac{2 \pi n r c}{3 g}$. This is what is expressed by NO in our construction.

The whole difference between the gravitations of B and A is therefore $\frac{2}{15} \pi f x - \frac{2}{3} \pi q x + \frac{2 \pi n r c}{3 g}$. The gravitation of B is to this difference as $\frac{2}{3} \pi n r$ to $\frac{2}{15} \pi f x - \frac{2}{3} \pi q x + \frac{2 \pi n r c}{3 g}$ or (dividing all by $\frac{2}{3} \pi n$) as r to $\frac{f x}{5 n} - \frac{q x}{n} + \frac{c r}{g}$.

Now the equilibrium of rotation requires that the whole polar force be to the sensible gravitation at the equator as the radius of the equator to the semiaxis (569.) Therefore we must make the radius of the equator to its excess above the semiaxis as the polar gravitation to its excess above the sensible equatorial gravitation.

That is $r : x = r : \frac{f x}{5 n} - \frac{q x}{n} + \frac{c r}{g}$, and therefore $x = \frac{f x}{5 n} - \frac{q x}{n} + \frac{c r}{g}$. Hence we have $\frac{c r}{g} = x + \frac{q x}{n} - \frac{f x}{5 n}$. But $q = n - f$. Therefore $\frac{c r}{g} = x + \frac{n x}{n} - \frac{f x}{n} - \frac{f x}{5 n}$,
 $= x + x - \frac{6 f x}{5 n}$, $= 2 x - \frac{6 f x}{5 n} = x \times \left(2 - \frac{6 f}{5 n} \right)$

Wherefore

Wherefore $\kappa = \frac{cr}{g \times \left(2 - \frac{6f}{5n}\right)} = \frac{5ncr}{g \times 10n - 6f}$, which

is more conveniently expressed in this form $\kappa = \frac{5cr}{2g} \times$

$\frac{n}{5n - 3f}$. The species, or ellipticity of the spheroid is

$$\frac{\kappa}{r}, = \frac{5c}{2g} \times \frac{n}{5n - 3f}.$$

Such then is the elliptical spheroid of hypothesis B; and we saw that, in respect of form, it is scarcely distinguishable from the figure which the mass will have when the fictitious force of the central matter gives place to the natural force of the dense spherical nucleus. This is true at least in all the cases where the centrifugal force is very small in comparison with the mean gravitation.

We must therefore take some notice of the influence which the variations of density may have on the form of this spheroid. We may learn this by attending to the formula

$$\frac{\kappa}{r} = \frac{5c}{2g} \times \frac{n}{5n - 3f}.$$

The value of this formula depends chiefly on the fraction

$$\frac{n}{5n - 3f}.$$

597. If the density of the interior parts be immensely greater than that of the surrounding fluid, the value of this fraction becomes nearly $\frac{1}{3}$, and $\frac{\kappa}{r}$ becomes nearly $= \frac{c}{2g}$, and the ellipse nearly the same with what Hermann assigned to a homogeneous fluid spheroid.

If $n = 5f$; then $\frac{n}{5n - 3f} = \frac{5}{22}$; and, in the case of the Earth, $\frac{\kappa}{r}$ would be nearly $= \frac{1}{508,6}$, making an equatorial elevation of nearly 7 miles.

598. If $n = f$, the fraction $\frac{n}{5n - 3f}$ becomes $\frac{1}{2}$; and $\frac{\kappa}{r} = \frac{5c}{4g}$, which we have already shewn to be suitable to a homogeneous spheroid, with which this is equivalent. The protuberance or ellipticity in this case is to that when the nucleus is incomparably denser than the fluid in the proportion of 5 to 2. This is the greatest ellipticity that can obtain when the fluid is not denser than the nucleus.

Between these two extremes, all other values of the formula are competent to homogeneous spheroids of gravitating fluids, covering a spherical nucleus of greater density, either uniformly dense or consisting of concentric spherical strata, each of which is uniformly dense.

From this view of the extreme cases, we may infer in general, that as the incumbent fluid becomes rarer in proportion to the nucleus, the ellipticity diminishes. M. Bernoulli (Daniel), misled by a gratuitous assumption, says in his theory of the tides that the ellipticity produced in the æreal fluid which surrounds this globe will be 300 times greater than that of the solid nucleus; but this is a mistake, which a juster assumption of *data* would have prevented. The æreal spheroid will be sensibly less oblate than the nucleus.

It was said that the value of the formula depended chiefly on the fraction $\frac{n}{5n-3f}$. But it depends also on the fraction $\frac{5c}{2g}$, increasing or diminishing as c increases or diminishes, or as g diminishes or increases. It must also be remarked that the theorem $\frac{a}{r} = \frac{5c}{4g}$ for a homogeneous spheroid was deduced from the supposition that the eccentricity is very small (See § 580. 585.) When the rotation is very rapid, there is another form of an elliptical spheroid, which is in that kind of *equilibrium*, which, if it be disturbed, will not be recovered, but the eccentricity will increase with great rapidity, till the whole dissipates in a round flat sheet. But within this limit, there is a kind of stability in the *equilibrium*, by which it is recovered when it is disturbed. If the rotation be too rapid, the spheroid becomes more oblate, and the fluids which accumulate about the equator, having less velocity than that circle, retard the motion. This goes on however some time, till the true shape is overpassed, and then the accumulation relaxes. The motion is now too slow for this accumulation, and the waters flow back again toward the poles. Thus an oscillation is produced by the disturbance, and this is gradually diminished by the mutual adhesion of the waters, and by friction, and things soon terminate in the resumption of the proper form.

599. When the density of the nucleus is less than that of the fluid, the varieties which result in the form

from a variation in the density of the fluid are much greater, and more remarkable. Some of them are even paradoxical. Cases, for example, may be put, (when the ratio of n to f differs but very little from that of 3 to 5), where a very small centrifugal force, or very slow rotation, shall produce a very great protuberance, and, on the contrary, a very rapid rotation may consist with an oblong form like an egg. But these are very singular cases, and of little use in the explanation of the phenomena actually exhibited in the solar system. The *equilibrium* which obtains in such cases may be called a *tottering equilibrium*, which, when once disturbed, will not be again recovered, but the dissipation of the fluid will immediately follow with accelerated speed. Some cases will be considered, on another occasion, where there is a deficiency of matter in the centre, or even a hollow.

600. The chief distinction between the cases of a nucleus covered with an equally dense fluid, and a dense nucleus covered with a rarer fluid, consists in the difference between the polar and equatorial gravities; for we see that the difference in shape is inconsiderable. It has been shewn already that, in the homogenous spheroid of small eccentricity, the excess of the polar gravity above the sensible equatorial gravity is nearly equal to $\frac{g^2}{5r}$ (for $r : \frac{1}{5}a = g : \frac{g^2}{5r}$). When, in addition to this, we take into account the diminution c , produced by rotation, we have $\frac{g^2}{5r} + c$ for the whole difference between the po-

lar and the sensible equatoreal gravity. But, in a homogeneous spheroid, we have $\kappa = \frac{5cr}{4g}$. Therefore the excess of polar gravity in a homogeneous revolving spheroid is $\frac{c}{4} + c$ or $\frac{5c}{4}$. We may distinguish this excess in the homogeneous spheroid by the symbol E.

601. But, in hypothesis B, the equilibrium of rotation requires that r be to κ as g to $\frac{g^\kappa}{r}$, and the excess of polar gravity in this hypothesis is $\frac{g^\kappa}{r}$. But we have also seen that in this hypothesis, $\frac{\kappa}{r} = \frac{5c}{2g} \times \frac{n}{5n-3f}$. Therefore the excess of polar gravity in this hypothesis is $\frac{5c}{2} \times \frac{n}{5n-3f}$. Let this excess be distinguished by the symbol ϵ .

602. The excess of polar gravity must be greater than this in hypothesis A. For, in that hypothesis the equatoreal gravity to the fluid part of the spheroid is already smaller. And this smaller gravity is not so much increased by the natural gravitation to the central matter, in the inverse duplicate ratio of the distance, as it was increased by the fictitious gravity to the same matter, in the direct ratio of the distances. The second of the three distinctions noticed in § 596. between the gravitations of B and A was $-\frac{q^\kappa}{n}$. This must now be changed into $+\frac{2q^\kappa}{n}$, as may easily be deduced from

§ 593, where $-\frac{q^x}{n}$ is represented by HI in fig. 67, and the excess, forming the compensation for hypothesis A is represented by HL, nearly double of HI, and in the opposite direction, diminishing the gravitation of A. The difference of these two states is $\frac{3q^x}{n}$, by which the tendency of A to the central matter in hypothesis A falls short of what it was in hypothesis B. Therefore, as $\frac{f^x}{5n} - \frac{q^x}{n} + \frac{cr}{g}$ is to $\frac{3q^x}{n}$, so is the excess ϵ to a quantity ϵ' , which must be added to ϵ , in order to produce the difference of gravities e , conformable to the statement of hypothesis A. Now, in hypothesis B, we had $\kappa = \frac{f^x}{5n} - \frac{q^x}{n} + \frac{cr}{g}$, and we may, without scruple, suppose κ the same in hypothesis A. Therefore $\epsilon : \epsilon' = \kappa : \frac{3q^x}{n}$, $= 1 : \frac{3q}{n}$, and $\epsilon' = \epsilon \times \frac{3q}{n} = \epsilon \times \frac{3^n - 3f}{n}$, $= \frac{5c}{2} \times \frac{n}{5^n - 3f} \times \frac{3^n - 3f}{n}$, $= \frac{5c}{2} \times \frac{3^n - 3f}{5^n - 3f}$. Add to this ϵ , which is $\frac{5c}{2} \times \frac{n}{5^n - 3f}$, and we obtain for the excess e of polar gravity in hypothesis A $= \frac{5c}{2} \times \frac{4^n - 3f}{5^n - 3f}$.

603. Let us now compare this excess of polar gravity above the sensible equatoreal gravity in the three hypotheses: 1st, A, suited to the fluid surrounding a spherical nucleus of greater density: 2^d, B, suited to the same fluid, surrounding a central nucleus which attracts with a force proportional to the distance: and, 3^d,

C, suited to a homogeneous fluid spheroid, or enclosing a spherical nucleus of equal density. These excesses are

$$A \quad \frac{5c}{2} \times \frac{4^n - 3f}{5^n - 3f}$$

$$B \quad \frac{5c}{2} \times \frac{n}{5^n - 3f}$$

$$C \quad \frac{5c}{4}, \text{ or } \frac{5c}{4} \times \frac{5^n - 3f}{5^n - 3f}.$$

It is evident that the sum of A and B is $\frac{5c}{2} \times \frac{5^n - 3f}{5^n - 3f}$, which is double of C, or $\frac{5c}{4} \times \frac{5^n - 3f}{5^n - 3f}$, and therefore C is the arithmetical mean between them.

Now we have seen that $\frac{5c}{2g} \times \frac{4^n - 3f}{5^n - 3f}$ expresses the ratio of the excess of polar gravity to the mean gravity in the hypothesis A. We have also seen that $\frac{5c}{2g} \times \frac{n}{5^n - 3f}$ may safely be taken as the value of the ellipticity in the same hypothesis. It is not perfectly exact, but the deviation is altogether insensible in a case like that of the Earth, where the rotation and the eccentricity are so moderate. And, lastly, we have seen that the same fraction that expresses the ratio of the excess of polar gravity to mean gravity, in a homogeneous spheroid, also expresses its ellipticity, and that twice this fraction is equal to the sum of the other two.

604. Hence may be derived a beautiful theorem, first given by M. Clairaut, that *the fraction expressing twice the ellipticity of a homogeneous revolving spheroid is the sum*

sum of two fractions, one of which expresses the ratio of the excess of polar gravity to mean gravity, and the other expresses the ellipticity of any spheroid of small eccentricity, which consists of a fluid covering a denser spherical nucleus.

If therefore any other phenomena give us, in the case of a revolving spheroid, the proportion of polar and equatoreal gravities, we can find its ellipticity, by subtracting the fraction expressing the ratio of the excess of polar gravity to the mean gravity from twice the ellipticity of a homogeneous spheroid. Thus, in the case of the Earth, twice the ellipticity of the homogeneous spheroid is $\frac{1}{115}$. A medium of seven comparisons of the rate of pendulums gives the proportion of the excess of polar gravity above the mean gravity = $\frac{1}{180}$. If this fraction be subtracted from $\frac{1}{115}$, it leaves $\frac{1}{319}$ for the medium ellipticity of the Earth. Of these seven experiments, five are scarcely different in the result. Of the other two, one gives an ellipticity not exceeding $\frac{1}{355}$. The agreement in general is incomparably greater than in the forms deduced from the comparisons of degrees of the meridian. All the comparisons that have been published concur in giving a considerably smaller eccentricity to the terraqueous spheroid than suits a homogeneous mass, and which is usually called Newton's determination. It is indeed his determination, on the supposition of homogeneity; but he expressly says that a different density in the interior parts will induce a different form, and he points out some supposititious cases, not indeed very probable, where the form will be different. Newton has not conceived this subject with his usual sagacity, and has

has made some inferences that are certainly inconsistent with his law of gravitation.

That the protuberancy of the terrestrial equator is certainly less than $\frac{1}{311}$ proves the interior parts to be of a greater mean density than the exterior, and even gives us some means for determining how much they exceed in density. For, by making the fraction $\frac{5e}{2g} \times \frac{4n-3f}{5n-3f} = \frac{1}{180}$, as indicated by the experiments with pendulums, we can find the value of n .

605. The length of the seconds pendulum is the measure of the accelerating force of gravity. Therefore let l be this length at the equator, and $l + d$ the length at the pole. We have $\frac{5c}{2g} \times \frac{4n-3f}{5n-3f} = \frac{d}{l}$, whence

$\frac{4n-3f}{5n-3f} = \frac{2gd}{5cl}$. This equation, when properly treated, gives $\frac{n}{f} = \frac{15cl-6gd}{20cl-10gd}$, &c. &c.*

The same principles may be applied to any other planet as well as to this Earth. Thus, we can tell what portion of the equatoreal gravity of Jupiter is expended in keeping bodies on his surface, by comparing the time of

* We have information very lately of the measurement of a degree, by Major Lambton in the Myfore in India, with excellent instruments. It lies in lat. $12^{\circ} 32'$, and its length is 60494 British fathoms. We are also informed by Mr Melanderhielm of the Swedish academy that the measure of the degree in Lapland by Maupertuis is found to be 208 toises too great. This was suspected.

of his rotation with the period of one of his satellites. We find that the centrifugal force at his equator is $\frac{2}{9}$ of the whole gravity, and from the equation $\frac{5cr}{4g} = x$, we should infer that if Jupiter be a homogeneous fluid or flexible spheroid, his equatoreal diameter will exceed his polar axis nearly 10 parts in 113, which is not very different from what we observe; so much however as to authorise us to conclude that his density is greater near the centre than on his surface.

These observations must suffice as an account of this subject. Many circumstances, of great effect, are omitted, that the consideration might be reduced to such simplicity as to be discussed without the aid of the higher geometry. The student who wishes for more complete information must consult the elaborate performances of Euler, Clairaut, D'Alembert, and La Place. The dissertation of Th. Simpson on the same subject is excellent. The dissertation of F. Boscovich will be of great service to those who are less versant in the fluxionary calculus, that author having every where endeavoured to reduce things to a geometrical construction. To these I would add the *Cosmographia* of Frisius, as a very masterly performance on this part of his subject.

It were desirable that another element were added to the problem, by supposing the planet to consist of coherent flexible matter. It is apprehended that this would give it a form more applicable to the actual state of things. If a planet consist of such matter, ductile like melted glass, the shape which rotation, combined with gra-

itation

itation and this kind of cohesion, would induce, will be considerably different from what we have been considering, and susceptible of great variety, according to the thickness of the shell of which it is supposed to consist. The form of such a shell will have the chief influence on the form which will be assumed by an ocean or atmosphere which may surround it. If the globe of Mars be as eccentric as the late observations indicate it to be, it is very probable that it is hollow, with no great thickness. For the centrifugal force must be exceedingly small.

606. The most singular example of this phenomenon that is exhibited in the solar system, is the vast arch or ring which surrounds the planet Saturn, and turns round its axis with most astonishing rapidity. It is above 200000 miles in diameter, and makes a complete rotation in ten hours and thirty-two minutes. A point on its surface moves at the rate of $1000\frac{1}{2}$ miles in a minute, or nearly 17 miles in one beat of the clock, which is 58 times as swift as the Earth's equator.

M. La Place has made the mechanism of this motion a subject of his examination, and has prosecuted it with great zeal and much ingenuity. He thinks that the permanent state of the ring, in its period of rotation, may be explained, on the supposition that its parts are without connexion, revolving round the planet like so many satellites, so that it may be considered as a vapour. It appears to me that this is not at all probable.

He says that the observed inequalities in the circle of the ring are necessary for keeping it from coalescing with the planet. Such inequalities seem incompatible with its own constitution, being inconsistent with the *equilibrium* of forces among incoherent bodies. Besides, as he supposes no cohesion in it, any inequalities in the constitution of its different parts cannot influence the general motion of the whole *in the manner he supposes*, but merely by an inequality of gravitation. The effect of this, it is apprehended, would be to destroy the permanency of its construction, without securing, as he imagines, the steadiness of its position. But this seems to be the point which he is eager to establish; and he finds, in the numerous list of possibilities, conditions which bring things within his general equation for the *equilibrium* of revolving spheroids; but the equation is so very general, and the conditions are so many, and so implicated, that there is reason to fear that, in some circumstances, the *equilibrium* is of that kind that has no stability, but, if disturbed in the smallest degree, is destroyed altogether, being like the *equilibrium* of a needle poised upright on its point. There is a stronger objection to M. La Place's explanation. He is certainly mistaken in thinking that the period of the rotation of the ring is that which a satellite would have at the same distance. The second Cassinian satellite revolves in $65^{\text{h}} 44'$, and its distance is 56,2 (the elongation in seconds). Now $\overline{65^{\text{h}} 44'}^2 : \overline{10^{\text{h}} 32\frac{1}{4}'}^2 = 56,2^2 : 16,4^2$. This is the distance at which a satellite would revolve in $10^{\text{h}} 32'$.

It

It must be somewhat less than this, on account of the oblate figure of the planet. Yet even this is less than the radius of the very inmost edge of the ring. The radius of the outer edge is not less than $22\frac{1}{2}$, and that of its middle is 20.

It is a much more probable supposition (for we can only suppose) that the ring consists of coherent matter. It has been represented as supporting itself like an arch; but this is less admissible than La Place's opinion. The rapidity of rotation is such as would immediately scatter the arch, as water is flung about from a mop. The ring must cohere, and even cohere with considerable force, in order to counteract the centrifugal force, which considerably exceeds its weight. If this be admitted, and surely it is the most obvious and natural opinion, there will be no difficulty arising from the velocity of rotation or the irregularity of its parts. M. La Place might easily please his fancy by contriving a mechanism for its motion. We may suppose that it is a viscid substance like melted glass. If matter of this constitution, covering the equator of a planet, turn round its axis too swiftly, the viscid matter will be thrown off, retaining its velocity of rotation. It will therefore expand into a ring, and will remove from the planet, till the velocity of its equatorial motion correspond with its diameter and its curvature. However small we suppose the cohesive or viscid force, it will cause this ring to stop at a dimension smaller than the orbit of a planet moving with the same velocity.—These seem to be legitimate consequences of what we know of coherent matter, and they

greatly resemble what we see in Saturn's ring. This constitution of the ring is also well fitted for admitting those irregularities which are indicated by the spots on the ring, and which M. La Place employs with so much ingenuity for keeping the ring in such a position that the planet always occupies its centre. This is a very curious circumstance, when considered attentively, and its importance is far from being obvious. The planet and the ring are quite separate. The planet is moving in an orbit round the Sun. The ring accompanies the planet in all the irregularities of its motion, and has it always in the middle. This ingenious mathematician gives strong reasons for thinking that, if the ring were perfectly circular and uniform, although it is *possible* to place Saturn exactly in its centre, yet the smallest disturbance by a satellite or passing comet would be the beginning of a derangement, which would rapidly increase, and, after a very short time, Saturn would be in contact with the inner edge of the ring, never more to separate from it. But if the ring is not uniform, but more massive on one side of the centre than on the other, then the planet and the ring may revolve round a common centre, very near, but not coinciding with the centre of the ring. He also maintains that the oblate form of the planet is another circumstance absolutely necessary for the stability of the ring. The redundancy of the equator, and flatness of the ring, fit these two bodies for acting on each other like two magnets, so as to adjust each other's motions.

The whole of this analysis of the mechanism of Saturn's ring is of the most intricate kind, and is carried on by the author by calculus alone, so as not to be instructive to any but very learned and expert analysts. Several points of it however might have been treated more familiarly. But, after all, it must rest entirely on the truth of the conjectures or assumptions made for procuring the possible application of the fundamental equations.

607. The Moon presents to the reflecting mind a phenomenon that is curious and interesting. She always presents the same face to the Earth, and her appearance just now perfectly corresponds with the oldest accounts we have of the spots on her disk. These indeed are not of very ancient date, as they cannot be anterior to the telescope. But this is enough to shew that the Moon turns round her axis in precisely the same time that she revolves round the Earth. Such a precise coincidence is very remarkable, and naturally induces the mind to speculate about the cause of it. Newton ascribed it to an oblong oval figure, more dense, or at least heavier, at one end than at the other. This he thought might operate on the Moon somewhat in the way that gravity operates on a pendulum. He defines this figure in Proposition 38. B. III. ; and as the eccentricity, or any deviation of its centre of gravity from that of its figure, is extremely small, the *vis disponens*, by which one diameter is directed towards the Earth, is also very minute, and
its

its operation must be too slow to keep one face steadily turned to the Earth, in opposition to the momentum of rotation round the axis, seven or eight days being all the time that is allowed for producing this effect. Therefore we observe what is called the *Libration* of the Moon, arising from the uniform rotation of the Moon, combined with her unequable orbital motion. One diameter of the Moon is always turned to the upper focus of her orbit, because her angular motion round that focus is almost perfectly uniform, and therefore corresponds with her uniform rotation. But that diameter which is towards us when the Moon is in her apogee or perigee, deviates from the Earth almost six degrees when she is in quadrature. But although, in the short space of eight days, the pendulous force of the Moon cannot prevent this deviation altogether, it undoubtedly lessens it. It is said to produce another effect. If the original projection of the Moon in the tangent of her orbit did not precisely, but very nearly, correspond with the rotation impressed at the same time, this pendulous tendency would, in the course of many ages, gradually lessen the difference, and at last make the rotation perfectly commensurate with the orbital revolution.

But we apprehend that this conclusion cannot be admitted. For, in whatever way we suppose this arranging force to operate, if it has been able, in the course of ages, to do away some small primitive difference between the velocity of rotation and the velocity of revolution, it must certainly have been able to annihilate a much smaller

smaller difference in the position of the Moon's figure, namely, the obliquity of the axis to the plane of the orbit. * It deviates about 1 or 2 degrees from the perpendicular, and it firmly retains this obliquity of position; and no observation can discover any deviation from perfect parallelism of the axis in all situations. It surely requires much less action of the directing force to produce this change in the position of the axis, than to overcome even a very small difference in angular motion, because this last difference accumulates, and makes a great difference of longitude.

These considerations seem to prove that the constant appearance of one and the same part of the Moon's surface has not been produced by the cause suspected by Newton. The coincidence has more probably been original. We have no reason to doubt that the same consummate skill that is manifest in every part of the system, in which every thing has an accurate adjustment, *pondere et mensurâ*, also made the primitive revolution rotation of the Moon that which we now behold and admire.

* The axis round which the rotation of the Moon is performed is inclined to the plane of the ecliptic in an angle of $88\frac{1}{2}^{\circ}$, and it is inclined to the plane of the lunar orbit $82\frac{1}{2}$. It is always situated in the plane passing through the poles of the ecliptic and of the lunar orbit. It therefore deviates about $1\frac{1}{2}$ from the axis of the ecliptic, and 7 from that of the Moon's orbit. The descending node of the Moon's equator coincides with the ascending node of her orbit.

mire. The manifest subserviency to great and good purposes, in every thing that we in some measure understand, leaves us no room to imagine that this adjustment of the lunar motions is not equally proper.

608. Philosophers have speculated about the nature of that body of faintly shining matter in which the Sun seems immersed, and is called the *zodiacal light*, because it lies in the zodiac. It is rarely perceptible in this climate, yet may sometimes be seen in a clear night in February and March, appearing in the west, a little to the north of where the Sun set, like a beam of faint yellowish grey light, slanting toward the north, and extending, in a pointed or leaf shape, about eight or ten degrees. The appearance is nearly what would be exhibited by a shining or reflecting atmosphere surrounding the Sun, and extending, in the plane of the ecliptic, at least as far as the orbit of Mercury, but of small thickness, the whole being flat like a cake or disk, whose breadth is at least ten times its thickness in the middle.

This has been the subject of speculation to the mechanical philosophers. It is something connected with the Sun. We have no knowledge of any connecting principle but gravitation. But simple gravitation would gather this atmosphere into a globular shape, whereas it is a very oblate disk or lens. Gravitation, combined with a proper revolution of the particles round the Sun, might throw the vapour into this form; and the object of the speculation is to assign the rotation that is suitable to it.

If the zodiacal light be produced by the reflection of an atmosphere that is retained by gravity alone, without any mutual adhesion of its particles, it cannot have the form that we observe. The greatest proportion that the equatorial diameter can have to the polar is that of 3 to 2; for, beyond that, the centrifugal force would more than balance its gravitation, and it would dissipate. A very strong adhesion is necessary for giving so oblate a form as we observe in the zodiacal light. Combined with this, it may indeed expand to any degree, by rapidly whirling about, as we see in the manufacture of crown-glass. But how is this whirling given to the solar atmosphere? It may get it by the mere action of the surface of the Sun, in the manner described by Newton in his account of the production of the Cartesian vortices. The surface drags round what is in contact with it. This stratum acts on the next, and communicates to it part of its own motion. This goes on from stratum to stratum, till the outermost stratum begins to move also. All this while, each interior stratum is circulating more swiftly than the one immediately without it. Therefore they are still acting on one another. It is very evident that a permanent state is not acquired, till all turn round in the same time with the Sun's body. This circumstance limits the possible expansion of an atmosphere that does not cohere. It cannot exceed the orbit of a planet which would revolve round the Sun in that time. But the zodiacal light extends much farther.

The discoveries of Dr Herschel on the surface of the

Sun, if confirmed by future observation, render this production of the zodiacal light inconceivable. For motions and changes are observed there, which shew a perfect freedom, not constrained by the adhesion of any superior strata. This would give a constant westerly motion on the surface of the Sun.

The difficulty in accounting for this phenomenon is greatly increased by the fact that when a comet passes through this atmosphere, the tail of the comet is not perceptibly affected by it. The comet of 1743 gave a very good opportunity of observing this. It was not attended to; but the descriptions that are given of the appearances of that comet shew clearly that the tail was (as usual) directed almost straight upward from the Sun, and therefore it mixed with this vapour, or whatever it may be, without any mutual disturbance.

It appears therefore, on the whole, that we are yet ignorant of the nature and mechanism of the zodiacal light.

609. Before concluding this subject, it is not improper to take some notice of an observation to which great importance has been attached by a certain class of philosophers. We shall find it demonstrated in its proper place, that when the force which impels a firm body forward acts in a direction which passes through its centre of gravity, it merely impels it forward. The body moves in that direction, and every particle moves alike, so that, during its progress, the body preserves the same attitude

(10.

PELLING force passes exactly through the centre of a body; and accordingly we very rarely observe a body moving forward in free space without rotation. A stone thrown from the hand never does. A bomb-shell, or a cannon bullet, has commonly a very rapid motion of rotation, which greatly deranges its intended direction.

The speculative philosophers who wish to explain all the celestial motions mechanically, think that they explain the rotation of the planets, and all the phenomena depending on it, by saying that one and the same force produced the revolution round the Sun, and the rotation round the axis; and produced those motions, because the direction of the primitive impulse did not pass precisely through the centre of the planet. They even shew by calculation the distance between the centre and the line of direction of the impelling force. Thus, they shew that the point of impulsion on this Earth is distant from its centre $\frac{1}{157}$ of its diameter.

Having thus accounted, as they imagine, for the Earth's rotation, they say that this rotation causes the Earth to swell out all around the equator, and they assign the precise eccentricity that the spheroid must acquire. They then shew that the action of the Sun and Moon on this equatorial protuberance deranges the rotation, so that the axis does not remain parallel to itself, and produces the phenomenon called the precession of the equinoxes. And thus all is explained mechanically. And on this explanation a conjecture is founded, which leads

to

to very magnificent conceptions of the visible universe. The Sun turns round an axis. Analogy should lead us to ascribe this to the same cause—to the action of a force whose direction does not pass through his centre. If so, the Sun has also a progressive motion through the boundless space, carrying all the planets and comets along with him, just as we observe Jupiter and Saturn carrying their satellites round their annual orbits.

This is, for the most part, perfectly just. A planet turns round its axis and advances, and therefore the force which results from the actual composition of *all the forces* which cooperated in producing both motions, does not pass through the centre of the planet, but precisely at the distance assigned by these gentlemen. But there is nothing of explanation in all this. From the manner in which the remark and its application are made, we are misled in our conception of the fact, and the imagination immediately suggests a *single force*, such as we are accustomed to apply in our operations, acting in one precise line, and therefore on one point of the body. It is this simplification of conception alone which gives the remark the appearance of explanation. A mathematician may thus give an explanation of a first rate ship of war turning to windward, by shewing how a rope may be attached to the ship, and how this rope may be pulled, so as to make her describe the very line she moves in. But the seaman knows that this is no explanation, and that he produced this motion of the ship by various manœuvres of the sails and rudder. The
only

only explanation that could be given, corresponding to the natural suggestion by this remark, would be the shewing some general fact in the system, in which this single force may be found that must thus impel the planets eccentrically, and thus urge them into revolution and rotation at once, as they would be urged by a stroke from some other planet or comet. With respect to this Earth, there is not the least appearance of the effect which must have been produced on it, had it been urged into motion by a single force applied to one point. The force has been applied alike to every particle; there is no appearance of any such general force competent to the production of such motions. Nay, did we clearly perceive the existence of such a force, we should be as far from an explanation as ever. It is not enough that Jupiter receives an impulse which impresses both the progressive and rotative motion. His four satellites must receive, each separately, an impulse of a certain precise intensity, and in a certain precise direction, very different in each, and which cannot be deduced from any thing that we know of matter and motion. No principle of general influence has been contrived by the zealous patrons of this system (for it is a system) that gives the smallest satisfaction even to themselves, and they are obliged to rest satisfied with expressing their hopes that it may yet be accomplished.

But suppose that an expert mechanician should shew how the planets, satellites, and comets may be so placed that an impulse may at once be given to them all, precisely competent

competent to the production of the very motions that we observe, which motions will now be maintained for ever by the 'universal operation of gravity. We should certainly admire his sagacity and his knowledge of nature. But we still wonder as much as ever at the nice adjustment of all this to ends which have evidently all the excellence that order and symmetry can give, while many of them are indispensably subservient to purposes which we cannot help thinking good. The suggestion of purpose and final causes is as strong as ever. It is no more eluded than it would be, should any man perfectly explain the making of a watch wheel, by shewing that it was the necessary result of the shape and hardness of the files and drills and chizels employed, and the intensity and direction of the forces by which those tools were moved; and having done all this, should say that he had accounted for the nice and suitable form of the wheel as a part of a watch. And, with respect to the subsequent oblate form of the planet set in rotation, the mechanical explanation of this is incompatible with the supposition that the revolution and rotation are the effects of one simple force. The oblate form, if acquired by rotation, requires primitive fluidity, which is incompatible with the operation of one simple force as the primitive mover. There is no proof whatever that this Earth was originally fluid; it is not nearly so oblate as primitive fluidity requires; yet its form is so nicely adjusted to its rotation, that the thin film of water on it is distributed with perfect uniformity. We are obliged

to grant that a form has been originally given it suitable to its destination, and we enjoy the advantages of this exquisite adjustment.

I acknowledge that the influence of final causes has been frequently and egregiously misapplied, and that these ignorant and precipitate attempts to explain phenomena, or to account for them, and even sometimes to authenticate them, have certainly obstructed the progress of true science. But what gift of God has not been thus abused? A true philosopher will never be so regardless of logic as to adduce final causes as arguments for the reality of any fact; but neither will he have such a horror at the appearances of wisdom, as to shun looking at them. And we apprehend that unless some

‘*Frigidus obstitit circum præcordia sanguis,*’

it is not in any man’s power to hinder himself from perceiving and wondering at them. Surely

‘*To look thro’ nature up to Nature’s God,*’

cannot be an unpleasant task to a heart endowed with an ordinary share of sensibility; and the face of nature, expressing the Supreme Mind which gives animation to its features, is an object more pleasing than the mere workings of blind matter and motion.

But enough of this.—We shall close this subject of planetary figures by slightly noticing, for the present, a consequence of the oblate form perceptible in all the planets which turn round their axes; in the explanation of which the penetration of Newton’s intellect is eminently conspicuous.

610. In § 584, and several following paragraphs, we explained the effects arising from the inclination of the Moon's orbit round the Earth to the plane of the Earth's orbit round the Sun. We saw, for example, that when the intersection of the two planes is in the line AB (fig. 61.) of quadrature, the Moon is perpetually drawn out of that plane, and her path is continually bent down toward the ecliptic, during her moving along the semicircle ACB , and she describes another path Acb , crossing the ecliptic in b , nearer to A than B is. In the other half of her orbit, the same deviation is continued, and the Moon again crosses the ecliptic before she come to A , crosses her last path near to c , and the ecliptic a third time at d , and so on continually. Hence arises the retrograde motion of the nodes of the lunar orbit. We shewed that this obtains, in a greater or less degree, in every position of the nodes, except when they are in the line of syzygy.

What is true of one moon, would be true of any number: It would be true, were there a complete ring of moons surrounding the Earth, not adhering to one another. We saw that the inclination of the orbit is continually changing, being greatest when the nodes are in the line of the syzgies, and smallest when they are in quadrature. Now, if we apply this to a ring of moons, we shall find that it will never be a ring that is all in one plane, except when the nodes are in the syzgies, and at all other times will be warped, or out of shape. Now, let the moons all cohere, and the ring become

stiff; and let this happen when its nodes are in syzygy. It will turn round without disturbance of this sort. But this position of the nodes of the ring soon changes, by the Sun's change of relative situation, and now all the derangements begin again. The ring can no longer go out of shape or warp, because we may suppose it inflexible. But, as in the course of any one revolution of the Moon round the Earth, the inclination of the orbit would either be increased, on the whole, or diminished, on the whole, and the nodes would, on the whole, recede, this effect must be observed in the ring. When the nodes are so situated that, in the course of one revolution of a single Moon, the inclination will be more increased in one part than it is diminished in another, the opposite actions on the different parts of a coherent and inflexible ring will destroy each other, as far as they are equal, and the excess only will be perceived on the whole ring. Hence we can infer, with great confidence, that from the time that the nodes of the ring are in syzygy to the time they are in quadrature, the inclination of the ring of moons will be continually diminishing; will be least of all when the Sun is in quadrature with the line of the nodes; and will increase again to a maximum, when the Sun again gets into the line of the nodes, that is, when the nodes are in the line of the syzgies. But the inertia of the ring will cause it to continue any motion that is accumulated in it till it be destroyed by contrary forces. Hence, the times of the maximum and minimum of inclination will be considerably different
from

from what is now stated. This will be attended to by and by.

For the same reason, the nodes of the ring will continually recede; and this retrograde motion will be most remarkable when the nodes are in quadrature, or the Sun in quadrature with the line of the nodes; and will gradually become less remarkable, as the nodes approach the line of the syzgies, where the retrograde motion will be the least possible, or rather ceases altogether.

All these things may be distinctly perceived, by steadily considering the manner of acting of the disturbing force. This steady contemplation however is necessary, as some of the effects are very unexpected.

Suppose now that this ring contracts in its dimensions. The disturbing force, and all its effects, must diminish in the same proportion as the diameter of the ring diminishes. But they will continue the same in kind as before. The inclination will increase till the Sun comes into the line of the nodes, and diminish till he gets into quadrature with them. Suppose the ring to contract till almost in contact with the Earth's surface. The recess of the nodes, instead of being almost three degrees in a month, will now be only three minutes, and the change of inclination in three months will now be only about five seconds.

Suppose the ring to contract still more, and to cohere with the Earth. This will make a great change. The tendency of the ring to change its inclination, and to change its intersection with the ecliptic, still continues. But it can-

not now produce the effect, without dragging with it the whole mass of the Earth. But the Earth is at perfect liberty in empty space, and being retained by nothing, yields to every impulse, and therefore yields to this action of the ring.

Now, there is such a ring surrounding the Earth, having precisely this tendency. The Earth may be considered as a sphere, on which there is spread a quantity of redundant matter which makes it spheroidal. The gravitation of this redundant matter to the Sun sustains all those disturbing forces which act on the inflexible ring of moons; and it will be proved, in its proper place, that the effect in changing the position of the globe is $\frac{1}{7}$ of what it would be, if all this redundant matter were accumulated on the equator. It will also appear that the force by which every particle of it is urged to or from the plane of the ecliptic, is as its distance from that plane. Indeed, this appears already, because all the disturbing forces acting on the particles of this ring are similar, both in direction and proportion, to those which we shewed to influence the Moon in the similar situations of her monthly course round the Earth. Similar effects will therefore be produced.

Let us now see what those effects will be.—The lunar nodes continually recede; so will the nodes of this equatoreal ring, that is, so will the nodes of the equator, or its intersection with the ecliptic. But the intersections of the equator with the ecliptic are what we call the Equinoctial Points. The plane of the Earth's
equator,

equator, being produced to the starry heavens, intersects that seemingly concave sphere in a great circle, which may be traced out among the stars, and marked on a celestial globe. Did the Earth's equator always keep the same position, this circle of the heavens would always pass through the same stars, and cut the ecliptic in the same two opposite points. When the Sun comes to one of those points, the Earth turning round under him, every point of its equator has him in the zenith in succession; and all the inhabitants of the Earth see him rise and set due east and west, and have the day and night of the same length. But, in the course of a year, the action of the Sun on the protuberance of our equator deranges it from its former position, in such a manner that each of its intersections with the ecliptic is a little to the westward of its former place in the ecliptic, so that the Sun comes to the intersection about 20' before he reaches the intersection of the preceding year. This anticipation of the equal division of day and night is therefore called the PRECESSION OF THE EQUINOXES.

The axis of diurnal revolution is perpendicular to the plane of the equator, and must therefore change its position also. If the inclination of the equator to the ecliptic were always the same ($23\frac{1}{2}$ degrees), the pole of the diurnal revolution of the heavens (that is, the point of the heavens in which the Earth's axis would meet the concave) would keep at the same distance of $23\frac{1}{2}$ degrees from the pole of the ecliptic, and would therefore always be found in the circumference of a circle, of which the pole

pole of the ecliptic is the centre. The meridian which passes through the poles of the ecliptic and equator must always be perpendicular to the meridian which passes through the equinoctial points, and therefore, as these shift to the westward, the pole of the equator must also shift to the westward, on the circumference of the circle above mentioned.

But we have seen that the ring of redundant matter does not preserve the same inclination to the ecliptic. It is most inclined to it when the Sun is in the nodes, and smallest when he is in quadrature with respect to them. Therefore the obliquity of the equator and ecliptic should be greatest on the days of the equinoxes, and smallest when the Sun is in the solstitial points. The Earth's axis should twice in the year incline downward toward the ecliptic, and twice, in the intervals, should raise itself up again to its greatest elevation.

Something greatly resembling this series of motions may be observed in a child's humming top, when set a spinning on its pivot. An equatoreal circle may be drawn on this top, and a circular hole, a little bigger than the top, may be cut in a bit of stiff paper. When the top is spinning very steadily, let the paper be held so that half of the top is above it, the equator almost touching the sides of the hole. When the whirling motion abates, the top begins to stagger a little. Its equator no longer coincides with the rim of the hole in the paper, but intersects it in two opposite points. These intersections will be observed to shift round the whole circumference

of the hole, as the axis of the top veers round. The axis becomes continually more oblique, without any periods of recovering its former position, and, in this respect only the phenomena differ from those of the precession.

It was affirmed that the obliquity of the equator is greatest at the equinoxes, and smallest at the solstices. This would be the case, did the redundant ring instantly attain the position which makes an *equilibrium* of action. But this cannot be; chiefly for this reason, that it must drag along with it the whole inscribed sphere. During the motion from the equinox to the next solstice, the Earth's equator has been urged toward the ecliptic, and it must approach it with an accelerated motion. Suppose, at the instant of the solstice, all action of the Sun to cease; this motion of the terrestrial globe would not cease, but would go on for ever, equably. But the Sun's action continuing, and now tending to raise the equator again from the ecliptic, it checks the contrary motion of the globe, and, at length, annihilates it altogether; and then the effect of the elevating force begins to appear, and the equator rises again from the ecliptic. When the Sun is in the equinox, the elevation of the equator should be greatest; but, as it arrived at this position with an accelerated motion, it continues to rise (with a retarded motion) till the continuance of the Sun's depressing force puts an end to this rising; and now the effect of the depressing force begins to appear. For these reasons, it happens that the greatest obliquity of the

the

the equator to the ecliptic is not on the days of the equinoxes, but about six weeks after, viz. about the first of May and November; and the smallest obliquity is not at midsummer and midwinter, but about the beginning of February and of August.

And thus, we find that the same principle of universal gravitation, which produces the elliptical motion of the planets, the inequalities of their satellites, and determines the shape of such as turn round their axes, also explains this most remarkable motion, which had baffled all the attempts of philosophers to account for—a motion, which seemed to the ancients to affect the whole host of heaven; and when Copernicus shewed that it was only an appearance in the heavens, and proceeded from a real small motion of the Earth's axis, it gave him more trouble to conceive this motion with distinctness, than all the others. All these things—*obvia conspiciamus, nubem pellente matheſi.*

611. Such is the method which Sir Isaac Newton, the sagacious discoverer of this mechanism, has taken to give us a notion of it. Nothing can be more clear and familiar in general. He has even subjected his explanation to the severe test of calculation. The forces are known, both in quantity and direction. Therefore the effects must be such as legitimately flow from those forces. When we consider what a minute portion of the globe is acted upon, and how much inert matter is to be moved by the force which affects so small a portion,

portion, we must expect very feeble effects. All the change that the action of the Sun produces on the inclination of the equator amounts only to the fraction of a second, and is therefore quite insensible. The change in the position of the equinoxes is more conspicuous, because it accumulates, amounting to about 9" annually, by Newton's calculation. We shall take notice of this calculation at another time, and at present shall only observe that this motion of the equinox is but a small part of the precession actually observed. This is about $50\frac{1}{3}$ " annually. It would therefore seem that the theory and observation do not agree, and that the precession of the equinoxes is by no means explained by it.

612. It must be remarked that we have only given an account of the effect resulting from the unequal gravitation of the terrestrial matter to the Sun. But it gravitates also to the Moon. Moreover, the inequality of this gravitation (on which inequality the disturbance depends) is vastly greater. The Moon being almost 400 times nearer than the Sun, the gravitation to a pound of lunar matter is almost 640,000,000 times greater than to as much solar matter. When the calculation is made from proper data, (in which Newton was considerably mistaken) the effect of the lunar action must very considerably exceed that of the Sun. He was mistaken, in respect to the quantity of matter in the Sun and in the Moon. The transit of Venus, and the observations which have been made on the tides, have

brought us much nearer the truth in both these respects. When the calculation is made on such assumptions of the matter in the Sun and Moon as are best supported by observation, we find that the annual precession occasioned by the Sun's action on the equatorial protuberance is about $14''$ or $15''$, and that produced by the Moon is about $35''$. The precession really observed is about $50''$, and the agreement is abundantly exact. It must be farther remarked that this agreement is no longer inferred from a due proportioning of the whole observed precession between the Sun and the Moon, as we were formerly obliged to do; but each share is an independent thing, calculated without any reference to the whole precession. It is thus only that the phenomenon may be affirmed to be truly explained.

613. For this demonstration we are indebted to Dr Bradley. His discovery of what is now called the NUTATION of the Earth's axis, gave us a precise measure of the lunar action which removed every doubt. It therefore must be considered here.

The action of the luminaries on the Earth's equator, by which the position of it is deranged, depends on the magnitude of the angle which the equator makes with the line joining the Earth with the disturbing body. The Sun is never more than $23\frac{1}{2}$ degrees from the equator. But when the Moon's ascending node is in the vernal equinox, she may deviate nearly 29 degrees from it. And when the node is in the autumnal equinox, she cannot go more than 17
degrees

degrees from it. Thus, the action of the Sun is, from year to year, the same. But as, in 19 years, the Moon's nodes take all situations, the action of the Moon is very variable. It was one of the effects of this variation that Bradley discovered. While the Earth's equator continued to open farther and farther from the line joining the Earth with the Moon, the axis of the Earth was gradually depressed towards the ecliptic, and the diminution of its inclination at last amounted to 18 seconds. Dr Bradley saw this by its increasing the declination of a star properly situated. After nine years, when the Moon was in such a situation that she never went more than 17° from the Earth's equator, the same star had 18" less declination.

614. This change in the inclination of the Earth's equator is accompanied with a change in the precession of the equinoxes. This must increase as the equator is more open when viewed from the Moon. In the year in which the lunar ascending node is in the vicinity of the vernal equinox, the precession is more than 58"; and it is but 43" when the node is near the autumnal equinox. These are very conspicuous changes, and of easy observation, although long unnoticed, while blended with other anomalies equally unknown.

Few discoveries in astronomy have been of more service to the science than this of the nutation, and that of aberration, both by Dr Bradley. For till they were known, there was an anomaly, which might sometimes

amount to 53" (the sum of nutation and aberration), and affected every motion and every observation. No theory of any planet could be freed from this uncertainty. But now, we can give to every phenomenon its own proper motions, with all the accuracy that modern instruments can attain. Without these two discoveries, we could not have brought the solution of the great nautical problem of the longitude to any degree of perfection, because we could not render either the solar or lunar tables perfect. The changes in the position of the Earth's axis by nutation, and the concomitant equation of the precession, by recurring in the most regular manner, have given us the most exact measure of the changes in the Moon's action; and therefore gave an incontrovertible measure of her whole action, because the proportion between the variation and the whole action was distinctly known.

This not only completes the practical solution of the problem, but gives the most unquestionable proof of the soundness of the theory, shewing that the oblate form of the Earth is the cause of this nutation of its axis, and establishing the universal and mutual attraction of all matter. It shews with what confidence we may proceed, in following this law of gravitation into all its consequences, and that we may predict, without any chance of mistake, what will be the effect of any combination of circumstances that can be mentioned. And it surely shews, in the most conspicuous manner, the penetration and sagacity of Newton, who gave encouragement to a surmise so singular and so unlike all the usual questions of
progressive

progressive motion, even in all their varieties. Yet this most recondite and delicate speculation was one of his early thoughts, and is one of the twelve propositions which he read to the Royal Society.

615. It must be acknowledged however that this manner of exhibiting the theory of the precession of the equinoxes is not complete, or even accurate in the selection of the physical circumstances on which the proof proceeds. It is merely a popular way of leading the mind to the view of actions, which are indeed of the same kind with those actually concurring in the production of the effect. But it is not a narration of the real actions. Nor are the effects of those that are employed estimated according to their real manner of acting. The whole is rather a shrewd guess, in which Newton's great penetration enabled him to catch at a very remote analogy between the libration of the Moon and the wavering motion of the Earth's axis. We are not in a condition in this part of the course to treat this question in the proper manner. We must first understand the properties of the lever as a mechanical power, and the operation of the connecting forces of firm or rigid bodies. What we have said will suffice however for giving a distinct enough conception of the general effects of the action of remote bodies on a spheroidal planet turning round its axis.* It is

* To those who wish to study this very curious and difficult problem, I should recommend the solution given by Frius

is scarcely necessary to add that the other planets cannot sensibly influence the motion of the Earth's axis. Their accumulated action may add about $\frac{1}{5}$ of a second to the annual precession of the equinoxes.

The planets Mars, Jupiter, and Saturn, being vastly more oblate than the Earth, must be more exposed to this derangement of the rotative motion. Jupiter and Saturn, having so many satellites, which take various positions round the planet, the problem becomes immensely complicated. But the small inclination of the equator, and the great mass of the planet, and its very rapid rotation, must greatly diminish the effect we are now considering. Mars, being small, turning slowly, and yet being very oblate, must sustain a greater degree of this derangement; and if Mars had a satellite, we might expect such a change in the position of his axis as should become very sensible, even at this distance.

The ring of Saturn must be subject to similar disturbances, and must have a retrogradation of its intersection
with

his in the second part of his *Cosmographia*, as the most perspicuous of any that I am acquainted with. The elaborate performance of Mr Walmesley, Euler, D'Alembert, and La Grange, are accessible only to expert analysts. The essay by T. Simpson in the *Philosophical Transactions*, Vol. L. is remarkable for its simplicity, but, by employing the symbolical or algebraic analysis, the student is not so much aided by the constant accompaniment of physical ideas, as in the geometrical method of Frisius.

with the plane of the orbit. Had we nothing to consider but the ring itself, it would be a very easy problem to determine the motion of its nodes. But the proximity, and the oblate form, of the planet, and, above all, the complicated action of the satellites, make it next to unmanageable. It has not been attempted, that I know of. It may (I think) be deduced, from the Greenwich observations since 1750, that the nodes retreat on the orbit of Saturn about 34' or 36' in a century, and that their longitude in 1801 was $5^{\circ} 17' 13''$ and $11^{\circ} 17' 13''$. This may be received as more exact than the determination given in art. 380.

I said, in art. 370, that we have seen too little of the motions of Ceres and Pallas to announce the elements of their theories with any thing like precision. But, that they may not be altogether omitted, the following may be received as of most authority.

	<i>Ceres.</i>	<i>Pallas.</i>
Mean distance - - - - -	2767231	2767123
Eccentricity to m. d. 1. - - -	0,079	0,2463
Long. aphelion - - - - -	4.26.44	4.1.7.—
Period (fydereal) in days - - -	1682,25	1681,22
Mean long. Jan. 1804. - - - -	10.11.59	9.29.53
Inclin. orbit - - - - -	—.10.37	—.34.39
Long. node - - - - -	2.21. 7	5.22.27

These bodies present some very singular circumstances to our study; their distances and periods being almost the same, and their longitudes at present differing very little. They differ considerably in eccentricity, the place of the node,

node, and the inclination of their orbits. They must be greatly disturbed by each other, and by Jupiter, and it will be long before we shall obtain exact elements.

With these observations I might conclude the discussion of the mechanism of the solar system. The facts observed in the appearances of the comets are too few to authorise me to add any thing to what has been already said concerning them. I refer to Newton's *Principia* for an account of that great philosopher's conjectures concerning the luminous train which generally attends them; acknowledging that I do not think these conjectures well supported by the established laws of motion. Dr Winthorp has given, in the 57th volume of the *Phil. Transf.* a geometrical explanation of the mechanism of this phenomenon that is ingenious and elegant, but founded on a hypothesis which I think inadmissible.

616. No notice has yet been taken of the relations of the solar system to the rest of the visible host of heaven, and we have, hitherto, only considered the starry heavens as affording us a number of fixed points, by which we may estimate the motions of the bodies which compose our system. It will not therefore be unacceptable should I now lay before the reader some reflections, which naturally arise in the mind of any person who has been much occupied in the preceding researches and speculations, and which lead the thoughts into a scene of contemplation far exceeding in magnificence any thing yet

yet laid before the reader. As they are of a miscellaneous nature; and not susceptible of much arrangement, I shall not pretend to mark them by any distinctions, but shall take them as they naturally offer themselves.

The fitness for almost eternal duration, so conspicuous in the constitution of the solar system, cannot but suggest the highest ideas of the intelligence of the Great Artist. No doubt these conceptions will be very obscure, and very inadequate. But we shall find that the farther we advance in our knowledge of the phenomena, we shall see the more to admire, and the more numerous displays of great wisdom, power, and kind intentions.

It is not therefore fearful superstition, but the cheerful anticipation of a good heart, which will make a student of nature even endeavour to form to himself still higher notions of the attributes of the Divine Mind. He cannot do this in a direct manner. All he can do is to abstract all notions of imperfection, whether in power, skill, or benevolent intentions, and he will suppose the Author of the universe to be infinitely powerful, wise and good.

It is impossible to stop the flights of a speculative mind, warmed by such pleasing notions. Such a mind will form to itself notions of what is most excellent in the designs which a perfect being may form, and it finds itself under a sort of necessity of believing that the Divine Mind will really form such designs. This romantic wandering has given rise to many strange theological opinions. Not doubting (at least in the moment of en-

thufiasm) that we can judge of what is moft excellent, we take it for granted that this creature of our heated imagination muft alfo appear moft excellent to the Supreme Mind. From this principle, theologians have ventured to lay down the laws by which God himfelf muft regulate his actions. No wonder that, on fo fanciful a foundation as our capacity to judge of what is moft excellent, have been erected the moft extravagant fabrics, and that, in the exuberance of religious zeal, the Author of all has been described as the moft limited Agent in the univerfe, forced, in every action, to regulate himfelf by our poor and imperfect notions of what is excellent. We, who vanifh from the fight, at the diftance of a neighbouring hill—whose greateft works are invifible from the Moon—whose whole habitation is not vifible to a fpectator in Saturn—fhall fuch creatures pretend to judge of what is fupremely excellent?

Let us not pretend even to guefs at the fpecific laws by which the conduct of the Divinity muft be directed, except in fo far as it has pleafed him to declare them to us. We fhall purfue the only fafe road in this speculation, if we endeavour to difcover the laws by which his vifible and comprehenfible works are actually conducted. The more we difcover of thefe, the more do we find to fill us with admiration and aftonifhment. The only fpeculations in which we can indulge, without the continual danger of going aftray, are thofe which enlarge our notions of the fcene on which it has pleafed the Almighty to difplay his perfections. This will be
the

the undoubted effect of enlarging the field of our own observation. After examining this lower world, and observing the nice and infinitely various adjustments of means to ends here below, we may extend our observation beyond this globe. Then shall we find that, as far as our knowledge can carry us, there is the same art, and the same production of good effects by beautifully contrived means. We have lately discovered a new planet, far removed beyond the formerly imagined bounds of the planetary world. This discovery shews us that if there are thousands more, they may be for ever hid from our eyes by their immense distance. Yet *there* we find the same care taken that their condition shall be permanent. They are influenced by a force directed to the Sun, and inversely as the square of the distance from him; and they describe ellipses. This planet is also accompanied by satellites, doubtless rendering to the primary and its inhabitants services similar to what this Earth receives from the Moon. All the comets of whose motions we have any precise knowledge, are equally secured; none seems to describe a parabola or hyperbola, so as to quit the Sun for ever.

This mark of an intention that this noble fabric shall continue for ever to declare itself the work of an Almighty and Kind Hand, naturally carries forward the mind into that unbounded space, of which our solar system occupies so inconsiderable a portion. The mind revolts at the thought that this is studded with stars for no other purpose than to assist the astronomer in his com-

putations, and to furnish a gay spectacle to the unthinking multitude. We see nothing here below, or in our system, which answers but one solitary purpose, and we require that a positive reason shall be given for limiting the Host of Heaven to so ignoble an office. As such has not been given, we indulge ourselves in the pleasing thought that the stars make a part of the universe, no less important in purpose than great in extent. We are justifiable, by what we in some measure understand, in supposing each star a sun, the centre of a planetary system, full of enjoyment like our own, and so constructed as to last for ever.

When the philosopher indulges himself in those amazing, but pleasing thoughts, he must regulate his speculations by analogies and resemblances to things more familiarly known to him. We must suppose those systems to resemble our own, and that they are kept together by a gravitation in the inverse duplicate ratio of the distances. For we know that this alone will insure permanency and good order.

But in so doing, we extend the influence of gravity to distances inconceivably greater than any that we have yet considered, and we come at last to believe that gravitation is the bond of connexion which unites the most distant bodies of the visible universe, rendering the whole one great machine, for ever operating the most magnificent purposes, worthy of its All-Perfect Creator. And, when we see that such a connexion is necessary for this end, we are apt to imagine that gravity is *essential* to or indispensable

indispensable in that matter that is to be moulded into a world.

But let not our ignorance mislead us, nor let us measure every thing by that small scale which God has enabled us to use, unless we can see some circumstances of resemblance in the appearances, which may justify the application.

* A frame of material nature of any kind cannot be conceived by the mind, without supposing that the matter of which it consists is influenced by some active powers, constituting the relations between its different parts. Were there only the mere inert materials of a world, it would hardly be better than a chaos, although moulded into symmetrical forms, unless the spirit of its author were to animate those dead masses, so as to bring forth change, and order, and beauty. Our illustrious Newton

* For many of the thoughts in what follows, the reader is indebted to a very ingenious pamphlet, published by Cadell & Davies in 1777, entitled, *Thoughts on General Gravitation*. It is much to be regretted that the author has not availed himself of the successful researches of astronomers since that time, and prosecuted his excellent hints. If it be the performance of the person whom I suppose to be the author, I have such an opinion of his acuteness, and of his justness of thought, that I take this opportunity of requesting him to turn his attention afresh to the subject. His advantages, from his present situation and connexions, are precious, and should not be lost.

ton therefore says, with great propriety, that the business of a true philosophy is to investigate those active powers, by which the course of natural events, to a very great extent at least, is perpetually governed. Philosophising with this view, he discovered the law of universal gravitation, and has thus given the brightest specimen of the powers of human understanding.

The notion of something like gravity seems inseparable from our conception of any established order of things. For unless some principle of general union obtain among the parts of matter, we can have no conception of the very first formation of the individuals of which a world may be composed.

But *general* gravitation, or that power by which the *distant* bodies belonging to any system are connected, and act on one another, does not seem so indispensably necessary to the very being of the system, as *particular* gravity is to the being of any individual in it. We cannot discern any absurdity in the supposition of bodies, such as the planets, so situated with respect to another great body, such as the Sun, as to receive from it suitable degrees of light and heat, without their having any tendency to approach the Sun, or each other. But then, how far such limitation of gravity may be a possible thing, or how far its indefinite extension in every direction may be involved in its very nature, we cannot tell, until we are able to consider gravity as an effect, and to deduce the laws of its operation from our knowledge of its cause.

That

That the influence of gravity extends into the boundless void, to the greatest assignable distance, seems to be almost the hinge of the Newtonian philosophy. At least, there is nothing that warrants any limit to its action. Father Boscovich indeed shews that all the phenomena may be what they are, without this as a necessary consequence. But he is plainly induced to bring forward the limitation in order to avoid what has been thought a necessary consequence of the indefinite extension of gravity; and what he offers is a mere possibility.

Now, if such extension of gravitation be inseparable, in fact, from its nature, then, if all the bodies of our system are at rest in absolute space, no sooner does the influence of general gravitation go abroad into the system, than all the planets and comets must begin to approach the Sun, and, in a very small number of days, the whole of the solar system must fall into the Sun, and be destroyed.

But, that this fair order may be preserved, and accommodated to this extended influence of gravity, which appears so essential to the constitution of the several parts of the system, we see a most simple and effectual prevention, by the introduction of *projectile forces*, and *progressive motion*. For upon these being now combined, and properly adjusted with the variation of gravity, the planets are made to revolve round the Sun in stated courses, by which their continual approach to the Sun and to one another is prevented, and the adjustment is made with such exquisite propriety, that the perfect order

der of things is almost unchangeable. This adjustment is no less manifest in the subordinate systems of a primary planet and its satellites, which are not only regular in their own orbital motions, but are the constant attendants of their primaries in their revolution round the Sun.

In this view of the subject, so far as gravity seems essential to the constitution of all the great bodies of the system, and in so far as its indefinite extension may be inseparable from its nature, it appears that *periodical motion* must be necessary for the permanency and order of every system of worlds whatever.

But here a thought is suggested which obviously leads to a new and a very grand conception of the universe. If periodical motion be thus necessary for the preservation of a small assemblage of bodies, and if Newton's law present to us the whole host of heaven as one great assemblage affected by gravitation, we must still have recourse to periodical motion, in order to secure the establishment of this grand universal system. For if there be no bounds to the influence of gravitation, and if all the stars be so many suns, the centres of as many systems (as is most reasonable to believe) the immensity of their distance cannot satisfy us for their being long able to remain in any settled order. Those that are situated towards the confines of this magnificent creation must forsake their stations, and, with an approach, continually accelerated, must move onwards to the centre of general

ral gravitation, and, after a series of ages, the whole glory of nature must end in a universal wreck.

As the system of Jupiter and his satellites is but an epitome of the great solar system to which he belongs, may not this, in its turn, be a faint representation of that grand system of the universe, round whose centre this Sun, with his attending planets, and an inconceivable multitude of like systems, do in reality revolve according to the law of gravitation? Now, will our anticipation of disorder and ruin be changed into the contemplation of a countless number of nicely adjusted motions, all proclaiming the sustaining hand of God.

This is indeed a grand, and almost overpowering thought; yet justified both by reason and analogy. The grandeur however of this universal system only opens upon us by degrees. If it resemble our solar system in construction, what an inconceivable display of creation is suggested, when we turn our thoughts towards that place which the motions of so many revolving systems are made to respect! Here may be an unthought of universe of itself, an example of material creation, which must individually exceed all the other parts, though added into one amount. As our Sun is almost four thousand times bigger than all his attendants put together, it is not unreasonable to suppose the same thing here. It is not necessary that this central body should be visible. The great use of it is not to illuminate, but to govern the motions of all the rest. We know, however, that the existence of such a central body is not

necessary. Two bodies, although not very unequal, may be projected with such velocities, and in such directions, that they will revolve for ever round their common centre of position and gravitation. But such a system could hardly maintain any regularity of motion when a third body is added. It may indeed be said that the same transcendent wisdom, which has so exquisitely adapted all the circumstances of our system, may so adjust the motions of an immense number of bodies, that their disturbing actions shall accurately compensate each other. But still, the beautiful simplicity that is manifest in what we see and understand, seems to warrant a like simplicity in this great system, and therefore renders the existence of such a great central Regulator of the movements of all, the most probable supposition.

Sober reason will not be disposed to revolt at so glorious an extension of the works of God, however much it may overpower our feeble conceptions. Nay this analogy acquires additional weight and authority even from the transcendent nature of the universe to which it directs our thoughts. Nothing less magnificent seems suitable to a Being of infinite perfections.

But we are not left to mere conjecture in support of this conception of a great universe, connected by mutual powers. There are circumstances of analogy which tend greatly to persuade us of the reality of our conjecture—circumstances which seem to indicate a connexion among the most distant objects of the creation visible from our habitation. The light by which the fixed stars

are seen is the same with that by which we behold our Sun and his attending planets. It moves with the same velocity, as we discover by comparing the aberration of the fixed stars with the eclipses of Jupiter's satellites. It is refracted and reflected according to the same laws. It consists of the same colours. No opinion can be formed therefore of the solar light, which must not also be adopted with respect to the light of the fixed stars. The medium of vision must be acted on in the same manner by both, whether we suppose it the undulation of an æther, or the emission of matter from the luminous body. In either case, a mechanical connexion obtains between those bodies, however distant, and our system. Such a connexion in mechanical properties induces us to suppose that gravitation, which we know reaches to a distance which exceeds all our distinct conceptions, extends also to the fixed stars.

If this be really the case, motion must ensue, even in producing the final ruin of the visible universe; and periodic motion is indispensably necessary for its permanency.

If all the fixed stars, and our Sun, were equal, and placed at equal distances, in the angles of regular solids, their mutual ruinous approach could hardly be perceived. For in every moment, they would still have the same relative positions, and an increase of brightness is all that could ensue after many ages. But if they were irregularly placed, and unequal, their relative positions would change, with an accelerated motion, and

this change might become sensible after a long course of ages. If they have periodical motions, suited to the permanency of the grand system of the universe, the changes of place may be much more sensible; and if we suppose that their difference in brilliancy is owing to the differences in their distance from us, we may expect that these changes will be most sensible in the brightest stars.

Facts are not wanting to prove that such changes really obtain in the relative positions of the fixed stars. This was first observed by that great astronomer, mathematician and philosopher, Dr Halley. He found, after comparing the observations of Aristillus, Timochares and Ptolemy with those of our days, that several of the brighter stars had changed their situation remarkably (See *Phil. Trans.* N^o 355.) Aldebaran has moved to the south about 35'. Sirius has moved south about 42', and Arcturus, also to the south, about 33'. The eastern shoulder of Orion has moved northward about 61'. Observations in modern times shew that Arcturus has moved in 78 years about 3' 3". This is a very sensible quantity, and is easily observed, by means of the small star *b* in its immediate neighbourhood. (See *Phil. Trans.* LXIII. also 1748.; and *Mem. Par.* 1755.) Sirius in like manner increases its latitude about 2' in a century (*Mem. Par.* 1758.) Aldebaran moves very irregularly. The bright star in *Aquila* has changed its latitude 36' since the time of Ptolemy, and 3' since the time of Tycho. This is easily seen by its continual separation from the small star δ .

These

These motions seem to indicate a motion in our system. Most of the stars have moved toward the south. The stars in the northern quarters seem to widen their relative positions, while those in the south seem to contract their distances. Dr Herschel thinks that a comparison of all these changes indicates a motion of our Sun with his attending planets toward the constellation Hercules (Phil. Transf. 1788.) A learned and ingenious friend thinks it not impossible to discover this motion by means of the aberration of the stars. Suppose the Sun and planets to be moving toward the Pole-star, and that his motion is 100 times greater than that of the Earth in her orbit (a very moderate supposition, when we compare the orbital motion of the Earth with that of the Moon), every equatorial star will appear about 34' north of its true place, when viewed through a common telescope, but only 23' when viewed through a telescope filled with water. The declination of every such star will be 11' less through a water telescope than through a common telescope. Stars out of the equator will have their declination diminished by a water telescope $11' \times \cos.$ declin.

In 1761, the ingenious Mr Lambert published his Letters on Cosmology (in the German language), in which he has considered this subject with much attention and ingenuity. He treats of the motion of the Sun round a central body—of systems of systems, or milky ways, carried round an immense body—of systems of such galaxies—and of the great central body of the universe. In these speculations

speculations he infers much from final causes, and is often ingeniously romantic. But Lambert was also a true inductive philosopher, and makes no assertion with confidence that is not supported by good analogies. The rotation of the Sun is a strong ground of belief to Mr Lambert that he has also a progressive motion.

Tobias Mayer of Gottingen speaks in the same manner, in some of his dissertations published after his death by Lichtenberg. See also *Bailli's Account of Modern Astronomy*, Vol. II. 664, 689. Mayer of Manheim has also published thoughts to this effect. See *Comment. Acad. Palatin.* IV. Prevost, *Mem. Berlin* 1781. Mitchel *Phil. Transf.* LVII. 252.

The gravitation to the fixed stars can produce no sensible disturbances of the motions of our system. This gravitation must be inconceivably minute, by reason of the immense distance; and, as they are in all quarters of the heavens, they will nearly compensate each other's action; and the extent of our system being but as a point, in comparison with the distance of the nearest star, the gravitation to that star in all the parts of our system must be so nearly equal and parallel, that (98.) no sensible derangement can be effected, even after ages of ages.

As a further circumstance of analogy with a periodical motion in the whole visible universe, we may adduce the remarkable periodical changes of brilliancy that are observed in many of the fixed stars.

This

This was first observed (I think) in a star of the constellation Hydra. Montanari had observed it in 1670, and left some account of it in his papers, which Maraldi took notice of. Maraldi, after long searching in vain, found it in 1704, and saw several alternations of its brightness and dimness, but without being able to ascertain their period. It was long lost again, till Mr Edward Pigot found it in 1786. He determined its period to be 404 days. Since that time, this gentleman, and his father, with a Mr Goodricke, have given more attention to this department of astronomy, and their example has been followed by other astronomers. Mr Pigot has given us, in *Phil. Transf.* 1786, a list of a great number of stars (above fifty) in which such periodical changes have been observed, and has given particular determinations of twelve or thirteen, ascertaining their periods with precision. The whole is followed by some very curious reflections.

Of these stars, one of the most remarkable is α Cygni, having a period of $415\frac{1}{2}$ days. See *Phil. Transf.* N^o 343.; also *Mem. Acad. Paris*, 1719, 1759.

Another remarkable star is σ Ceti, having a period of 334 days. (See *Phil. Transf.* N^o 134. 346.; *Mem. Par.* 1719.)

There is another such, close to γ Cygni.

The double star ζ Lyrae exhibits very singular appearances, the southernmost sometimes appearing double, and sometimes accompanied by more little stars. Gri-

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fchoff of Berlin is positive that it has planets moving round it.

Some of those stars have very short periods. The most remarkable is Algol, in the head of Medusa. Its period is $2^d 20^h 49'$, in which its changes are very irregular, although perfectly alike in every period. Its ordinary appearance is that of a star of the second magnitude. It suffers, for about $3\frac{1}{2}$ hours, a reduction to the appearance of a star of the fourth or fifth magnitude.

Mr Goodricke observed similar variations in the star δ Cephei. During $5^d 8^h 37'$ it is a star of the fifth magnitude. For $1^d 13^h$ it is of the second or third. It diminishes during $1^d 18^h$; remains 36 hours in its faintest state, and regains its brilliancy in 13^h more (*Phil. Transf.* 1786.)

Mr Pigot observed the star η Antinoi to maintain its utmost brilliancy during 44 hours, and then gradually to fade during 62 hours, and, after remaining 30 hours of the fifth magnitude, it regains its greatest brilliancy in 36 hours (*Phil. Transf.* 1786.)

Whatever may be the cause of these alternations, they are surely very analagous to what we observe in our system, the individuals of which, by varying their positions, and turning their different sides toward us, exhibit alternations of a similar kind; as, for example, the apparition and disparition of Saturn's ring. These circumstances, therefore, encourage us to suppose a similarity of constitution in our system to the rest of the
heavenly

heavenly Host, and render it more probable that all are connected by one general bond, and are regulated by similar laws. Nothing is so likely for constituting this connexion as gravitation, and its combination with projectile force and periodic motion tends to secure the permanency of the whole.

But I must at the same time observe that such appearances in the heavens make it evident that, notwithstanding the wise provision made for maintaining that order and utility which we behold in our system, the day may come 'when the heavens shall pass away like a scroll that is folded up, when the stars in heaven shall fail, and the Sun shall cease to give his light.' The sustaining hand of God is still necessary, and the present order and harmony which he has enabled us to understand and to admire, is wholly dependent on his will, and its duration is one of the unfathomable measures of his providence. What is become of that dazzling star, surpassing Venus in brightness, which shone out all at once in November 1572, and determined Tycho Brahe to become an astronomer? He did not see it at half an hour past five, as he was crossing some fields in going to his laboratory. But, returning about ten, he came to a crowd of country folks who were staring at something behind him. Looking round, he saw this wonderful object. It was so bright that his staff had a shadow. It was of a dazzling white, with a little of a bluish tinge. In this state it continued about three weeks, and then be-

came yellowish and less brilliant. Its brilliancy diminished fast after this, and it became more ruddy, like glowing embers. Gradually fading, it was wholly invisible after fifteen months.

A similar phenomenon is said to have caused Hipparchus to devote himself to astronomy, and to his vast project of a catalogue of the stars, that posterity might know whether any changes happened in the heavens. And, in 1604, another such phenomenon, though much less remarkable, engaged for some time the attention of astronomers. Nor are these all the examples of the perishable nature of the heavenly bodies. Several stars in the catalogues of Hipparchus, of Ulugh Beigh, of Tycho Brahé, and even of Flamsteed, are no more to be seen. They are gone, and have left no trace.

Should we now turn our eyes to objects that are nearer us, we shall see the same marks of change. When the Moon is viewed through a good telescope, magnifying about 150 times, we see her whole surface occupied by volcanic craters; some of them of prodigious magnitude. Some of them give the most unquestionable marks of several successive eruptions, each destroying in part the crater of a former eruption. The precipitous and craggy appearance of the brims of those craters is precisely such as would be produced by the ejection of rocky matter. In short, it is impossible, after such a view of the Moon, to doubt of her being greatly changed from her primitive state.

Even

Even the Sun himself, the source of light, and heat, and life, to the whole system, is not free from such changes.

If we now look round us, and examine with judicious attention our own habitation, we see the most incontrovertible marks of great and general changes over the whole face of the Earth. Besides the slow degradation by the action of the winds and rains, by which the soil is gradually washed away from the high lands, and carried by the rivers into the bed of the ocean, leaving the Alpine summits stripped to the very bone, we cannot see the face of any rock or crag, or any deep gully, which does not point out much more remarkable changes. These are not confined to such as are plainly owing to the horrid operations of volcanoes, but are universal. Except a few mountains, where we cannot confidently say that they are factitious, and which for no better reason we call primitive, there is nothing to be seen but ruins and convulsions. What is now an elevated mountain has most evidently been at the bottom of the sea, and, previous to its being there, has been habitable surface.

It is very true that all our knowledge on this subject is merely superficial. The highest mountains, and deepest excavations, do not bear so great a proportion to the globe as the thickness of paper that covers a terrestrial globe bears to the bulk of that philosophical toy. We have no authority from any thing that we have seen, for forming

any judgement concerning the internal constitution of the Earth. But we see enough to convince us that it bears no marks of eternal duration, or of existing as it is, by its own energy. No!—all is perishable—all requires the sustaining hand of God, and is subject to the unsearchable designs of its Author and Preserver.

There is yet another class of objects in the heavens, of which I have taken no notice. They are called NEBULÆ, or NEBULOUS STARS. They have not the sparkling brilliancy that distinguishes the stars, and they are of a sensible diameter, and a determinate shape. Many of them, when viewed through telescopes, are clusters of stars, which the naked eye cannot distinguish. The most remarkable of these is in the constellation Cancer, and is known by the name *Præsepe*. Ptolemy mentions it, and another in the right eye of Sagittarius. Another may be seen in the head of Orion. Many small clusters have been discovered by the help of glasses. The whole galaxy is nothing else.

But there is another kind, in which the finest telescopes have discovered no clustering stars. Most of them have a star in or near the middle, surrounded with a pale light, which is brightest in the middle, and grows more faint toward the circumference. This circumference is distinct, or well defined, and is not always round. One or two nebulæ have the form of a luminous disk, with a hole in the middle like a millstone. They are of various colours, white, yellow, rose-coloured, &c. Dr Herschel, in several of the late volumes of the Philosophical

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cal Transactions, has given us the places of a vast number of nebulae, with curious descriptions of their peculiar appearances, and a series of most ingenious and interesting reflections on their nature and constitution. His *Thoughts on the Structure of the Heavens* are full of most curious speculation, and should be read by every philosopher.

When we reflect that these singular objects are not, like the fixed stars, brilliant points, which become smaller when seen through finer telescopes, but have a sensible, and measureable diameter, sometimes exceeding 2'; and when we also recollect that a ball of 200,000,000 miles in diameter, which would fill the whole orbit of the Earth round the Sun, would not subtend an angle of two seconds when taken to the nearest fixed star, what must we think of these nebulae? One of them is certainly some thousands of times bigger than the Earth's orbit. Although our finest telescopes cannot separate it into stars, it is still probable that it is a cluster. It is not unreasonable to think, with Dr Herschel, that this object, which requires a telescope to find it out, will appear to a spectator in its centre much the same as the visible heavens do to us, and that this starry heaven, which, to us, appears so magnificent, is but a nebulous star to a spectator placed in that nebula.

The human mind is almost overpowered by such a thought. When the soul is filled with such conceptions of the extent of created nature, we can scarcely avoid exclaiming, ' Lord, what then is man that thou art
 ' mindful

‘ mindful of *him!* ’ Under such impressions, David shrunk into nothing, and feared that he should be forgotten amongst so many great objects of the Divine attention. His comfort, and ground of relief from this dejecting thought, are remarkable. ‘ But,’ says he, ‘ thou hast made man but a little lower than the angels, ‘ and hast crowned him with glory and honour.’ David corrected himself, by calling to mind how high he stood in the scale of God’s works. He recognised his own divine original, and his alliance to the Author of all. Now, cheered, and delighted, he cries out, ‘ Lord, how glorious is thy name!’

THERE remains yet another phenomenon, which is very evidently connected with the mechanism of the solar system, and is in itself both curious and important. I mean the tides of our ocean. Although it appears improper to call this an astronomical phenomenon, yet, as it is most evidently connected with the position of the Sun and Moon, we must attribute this connexion in fact to a natural connexion in the way of cause and effect.

Of the Tides.

617. It is a very remarkable operation of nature that we observe on the shores of the ocean, when, in the calmest weather, and most serene sky, the vast body of waters that bathe our coasts advances on our shores, undating

undating all the flat sands, rising to a considerable height, and then as gradually retiring again to the bed of the ocean; and all this without the appearance of any cause to impel the waters to our shores, and again to draw them off. Twice every day is this repeated. In many places, this motion of the waters is even tremendous, the sea advancing, even in the calmest weather, with a high surge, rolling along the flats with resistless violence, and rising to the height of many fathoms. In the bay of Fundy, it comes on with a prodigious noise, in one vast wave, that is seen thirty miles off; and the waters rise 100 and 120 feet in the harbour of Annapolis-Royal. At the mouth of the Severn, the flood also comes up in one head, about ten feet high, bringing certain destruction to any small craft that has been unfortunately left by the ebbing waters on the flats; and as it passes the mouth of the Avon, it sends up that small river a vast body of water, rising forty or fifty feet at Bristol.

Such an appearance forcibly calls the attention of thinking men, and excites the greatest curiosity to discover the cause. Accordingly, it has been the object of research to all who would be thought philosophers. We find very little however on the subject in the writings of the Greeks. The Greeks indeed had no opportunity of knowing much about the ebbing and flowing of the sea, as this phenomenon is scarcely perceptible on the shores of the Mediterranean and its adjoining seas. The Persian expedition of Alexander gave them the only opportunity they ever had, and his army was astonished at finding

finding the ships left on the dry flats when the sea retired. Yet Alexander's preceptor Aristotle, the prince of Greek philosophers, shews little curiosity about the tides, and is contented with barely mentioning them, and saying that the tides are most remarkable in great seas.

618. When we search after the cause of any recurring event, we naturally look about for recurring concomitant circumstances; and when we find any that generally accompany it, we cannot help inferring some connexion. All nations seem to have remarked that the flood-tide always comes on our coasts as the Moon moves across the heavens, and comes to its greatest height when the Moon is in one particular position, generally in the south-west. They have also remarked that the tides are most remarkable about the time of new Moon, and become more moderate by degrees every day, as the Moon draws near the quadrature, after which they gradually increase till about the time of full Moon, when they are nearly of their greatest height. They now lessen every day as they did before, and are lowest about the last quadrature, after which they increase daily, and, at the next new Moon are a third time at the highest.

These circumstances of concomitancy have been noticed by all nations, even the most uncultivated; and all seem to have concurred in ascribing the ebbing and flowing of the sea to the Moon, as the efficient cause, or, at least, as the occasion, of this phenomenon, although
without

without any comprehension, and often without any thought, in what manner, or by what powers of nature, this or that position of the Moon should be accompanied by the tide of flood or of ebb.

Although this accompaniment has been every where remarked, it is liable to so many and so great irregularities, by winds, by freshes, by the change of seasons, and other causes, that hardly any two succeeding tides are observed to correspond with a precise position of the Moon. The only way therefore to acquire a knowledge of the connexion that may be useful, either to the philosopher or to the citizen, is to multiply observations to such a number, that every source of irregularity may have its period of operation, and be discovered by the return of the period. The inhabitants of the sea-coasts, and particularly the fishermen, were most anxiously interested in this research.

619. Accordingly, it was not long after the conquests of the Romans had given them possession of the coasts of the ocean, before they learned the chief circumstances or laws according to which the phenomena of the tides proceed. Pliny says that they had their source in the Sun and the Moon. It had been inferred from the gradual change of the tides between new Moon and the quadrature, that the Sun was not unconcerned in the operation. Pytheas, a Greek merchant, and no mean philosopher, resident at Marseilles, the oldest Grecian colony, had often been in Britain, at the tin mines in Cornwall and its ad-

jacent islands. He had observed the phenomena with great sagacity, and had collected the observations of the natives. Plutarch and Pliny mention these observations of Pytheas, some of them very delicate, and, the whole taken together, containing almost all that was known of the subject, till the discoveries of Sir Isaac Newton taught the philosophers what to look for in their inquiries into the nature of the tides, and how to class the phenomena. Pytheas had not only observed that the tides gradually abated from the times of new and full Moon to the time of the quadratures, and then increased again, but had also remarked that this vulgar observation was not exact, but that the greatest tide was always two days after new or full Moon, and the smallest was as long after the quadratures. He also corrected the common observation of the tides falling later every day, by observing that this retardation of the tides was much greater when the Moon was in quadrature than when new or full. The tide-day, about the time of new and full Moon, is really shorter by 50' than at the time of her quadrature.

620. This variation in the interval of the tides is called the PRIMING or the LAGGING of the tides, according as we refer them to lunar or solar time. Pytheas probably learned much of this nicety of observation from the Cornish fishermen. By Ælian's accounts, they had nets extended along shore for several miles, and were therefore much interested in this matter.

621. Many observations on the series of phenomena which completes a period of the tides are to be found in the books of hydrography, and the instructions for mariners, to whom the exact knowledge of the course of the tides is of the utmost importance. But we never had any good collection of observations, from which the laws of their progress could be learned, till the Academy of Paris procured an order from government to the officers at the ports of Brest and Rochefort, to keep a register of all the phenomena, and report it to the Academy. A register of observations was accordingly continued for six years, without interruption, at both ports, and the observations were published, forming the most complete series that is to be met with in any department of science, astronomy alone excepted. The younger Cassini undertook the examination of these registers, in order to deduce from them the general laws of the tides. This task he executed with considerable success; and the general rules which he has given contain a much better arrangement of all the phenomena, their periods and changes, than any thing that had yet appeared. Indeed there had scarcely any thing been added to the vague experience of illiterate pilots and fishermen, except two dissertations by Wallis and Flamsteed, published in the Philosophical Transactions.

622. It is not likely, notwithstanding this excellent collection of observations, that our knowledge would have proceeded much farther, had not Newton demonstrated

that a series of phenomena perfectly resembling the tides resulted from the mutual attraction of all matter. These consequences pointed out to those interested in the knowledge of the tides what vicissitudes or changes to look for—what to look for as the natural or regular series—what they are to consider as mere anomalies—what periods to expect in the different variations—and whether there are not periods which comprehend the more obvious periods of the tides, distinguishing one period from another. As soon as this clue was obtained, every thing was laid open, and without it, the labyrinth was almost inextricable; for in the variations of the tides there are periods in which the changes are very considerable; and these periods continually cross each other, so that a tide which should be great, considered as a certain tide of one period, should be small, considered as a certain tide of another period. When it arrives, it is neither a great nor a small tide, but it prevents both periods from offering themselves to the mere observer. The tides afford a very strong example of the great importance of a theory for directing even our observations. Aided by the Newtonian theory, we have discovered many periods, in which the tides suffer gradual changes, both in their hour and in their height, which commonly are so implicated with one another, that they never would have been discovered without this monitor, whereas now, we can predict them all.

623. The phenomena of the tides are, in general, the following.

1. The waters of the ocean rise, from a medium height to that of high water, and again ebb away from the shores, falling nearly as much below that medium state, and then rise again in a succeeding tide of flood, and again make high water. The interval between two succeeding high waters is about $12^{\text{h}} 25'$, the half of the time of the Moon's daily circuit round the Earth, so that we have two tides of flood and two ebb tides in every $24^{\text{h}} 50'$. This is the shortest period of phenomena observed in the tides. The gradual subsidence of the waters is such that the diminutions of the height are nearly as the squares of the times from high water. The same may be said of the subsequent rise of the waters in the next flood. The time of low water is nearly half way between the two hours of high water; not indeed exactly, it being observed at Brest and Rochefort that the flood tide commonly takes ten minutes less than the ebb tide.

624. As the different phenomena of the tides are chiefly distinguishable by the periods, or intervals of time in which they recur, it will be convenient to mark those periods by different names. Therefore, let the time of the apparent diurnal revolution of the Moon, viz. $24^{\text{h}} 50'$, be called A LUNAR DAY, and the 24th part of it be called A LUNAR HOUR. To this interval almost all the vicissitudes of the tides are most conveniently referred. Let the name TIDE DAY be given to the interval between two high waters, or two low waters, succeeding each other with the Moon nearly in the same position. This interval

val comprehends two complete tides, one of the full seas happening when the Moon is above the horizon, and the next, when she is under the horizon. We shall also find it convenient to distinguish these tides, by calling the first the SUPERIOR TIDE, and the other the INFERIOR TIDE. At new Moon they may be called the *Morning* and *Evening* tides.

625. 2. It is not only observed that we always have high water when the Moon is on some particular point of the compass (S. W. nearly) but also that the height of full sea from day to day has an evident reference to the phases of the Moon. At Brest, the highest tide is always about a day and a half after full or change. If it should happen that high water falls at the very time of new or full Moon, the third full sea after that one is the highest of all. This is called the SPRING-TIDE. Each succeeding full sea is less than the preceding, till we come to the third full sea after the Moon's quadrature. This is the lowest tide of all, and it is called NEAP-TIDE. After this, the tides again increase, till the next full or new Moon, the third after which is again the greatest tide.

626. The higher the tide of flood rises, the lower does the ebb tide generally sink on that day. The total magnitude of the tide is estimated by taking the difference between high and low water. As this is continually varying, the best way of computing its magnitude
seems

seems to be, to take the half sum of two succeeding tides. This must always give us a mean value for the tide whose full sea was in the middle. The medium spring-tide at Brest is about nineteen feet, and the neap-tide is about nine.

Here then we have a period of phenomena, the time of which is half of a lunar month. This period comprehends the most important changes, both in respect of magnitude, and of the hours of high and low water, and several modifications of both of those circumstances, such as the daily difference in height, or in time.

627. 3. There is another period, of nearly twice the same duration, which greatly modifies all those leading circumstances. This period has a reference to the distance of the Moon, and therefore depends on the Moon's revolution in her orbit. All the phenomena are increased when the Moon is nearer to the Earth. Therefore the highest spring-tide is observed when the Moon is *in perigeo*, and the next spring-tide is the smallest, because the Moon is then nearly *in apegeo*. This will make a difference of $2\frac{3}{4}$ feet from the medium height of spring tide at Brest, and therefore occasion a difference of $5\frac{1}{2}$ between the greatest and the least. It is evident that as the perigean and apogean situation of the Moon may happen in every part of a lunation, the equation for the height of tide depending upon this circumstance may often run counter to the equation corresponding to the regular

regular monthly series of tides, and will seemingly destroy their regularity.

628. 4. The variation in the Sun's distance also affects the tides, but not nearly so much as those in the distance of the Moon. In our winter, the spring-tides are greater than in summer, and the neap-tides are smaller.

629. 5. The declination, both of the Sun and Moon, affects the tides remarkably; but the effects are too intricate to be distinctly seen, till we perceive the causes on which they depend.

630. 6. All the phenomena are also modified by the latitude of the place of observation; and some phenomena occur in the high latitudes, which are not seen at all when the place of observation is on the equator. In particular, when the observer is in north latitude, and the Moon has north declination, that tide in which the Moon is above the horizon is greater than the other tide of the same day, when the Moon is below the horizon. It will be the contrary, if either the observer or the Moon (but not both) have south declination. If the polar distance of the observer be equal to the Moon's declination, he will see but one tide in the day, containing twelve hours flood and twelve hours ebb.

631. 7. To all this it must be added, that local circumstances of situation alter all the phenomena remarkably,

markably, so as frequently to leave scarcely any circumstances of resemblance, except the order and periods in which the various phenomena follow one another.

We must now endeavour to account for these remarkable movements and vicissitudes in the waters of the ocean.

632. Since the phenomena of the planetary motions demonstrate that every particle of matter in this globe gravitates to the Sun, and since they are at various distances from his centre, it is evident that they gravitate unequally, and that, from this inequality, there must arise a disturbance of that equilibrium which terrestrial gravitation alone might produce. If this globe be supposed either perfectly fluid and homogeneous, or to consist of a spherical nucleus covered with a fluid, it is clear that the fluid must assume a perfectly spherical form, and that in this form alone, every particle will be in equilibrio. But when we add to the forces now acting on the waters of the ocean their unequal gravitation to the Sun, this equilibrium is disturbed, and the ocean cannot remain in this form. We may apply to the particles of the ocean every thing that we formerly said of the gravitation of the Moon to the Sun in the different points of her orbit; and the same construction in fig. 59, that gave us a representation and measure of the forces which deranged the lunar motions, may be employed for giving us a notion of the manner in which the particles of water in the ocean are affected. The circle $OBCA$ may represent

4 D represent

present the watery sphere, and M any particle of the water. The central particle E gravitates to the Sun with a force which may be represented by ES. The gravitation of the particle M must be measured by MG. This force MG may be conceived as compounded of MF, equal and parallel to ES, and of MH. The force MF occasions no alteration in the gravitation of M to the Earth, and MH is the only disturbing force. We found that this construction may be greatly simplified, and that MI may be substituted for MH without any sensible error, because it never differs from it more than $\frac{1}{392}$. We therefore made EI, in fig. 60, = 3MN, and considered MI as the disturbing force. This construction is applicable to the present question, with much greater accuracy, because the radius of the Earth is but the sixtieth part of that of the Moon's orbit. This reduces the error to $\frac{1}{23520}$, a quantity altogether insensible.

633. Therefore let OACB (fig. 68.) be the terraqueous globe, and CS a line directed to the Sun, and BEA the section by that circle which separates the illuminated from the dark hemisphere. Let P be any particle, whether on the surface or within the mass. Let QPN be perpendicular to the plane BA. Make EI = 3PN, and join PI. PI is the disturbing force, when the line ES is taken to represent the gravitation of the particle E to the Sun. This force PI may be conceived to be compounded of two forces PE and PQ.

PE

PE tends to the centre of the Earth. PQ tends from the plane BA , or toward the Sun.

If this construction be made for every particle in the fluid sphere, it is evident that all the forces PE balance one another. Therefore they need not be considered in the present question. But the forces PQ evidently diminish the terrestrial gravitation of every particle. At C the force PQ acts in direct opposition to the terrestrial gravity of the particle. And, in the situation P , it diminishes the gravity of the particle as estimated in the direction PN . There is therefore a force acting in the direction NP on every particle in the canal PN . And this force is proportional to the distance of the particle from the plane BA (for PQ is always $= 3PN$). Therefore the water in this canal cannot remain in its former position, its equilibrium being now destroyed. This may be restored, by adding to the column NP a small portion Pp , whose weight may compensate the diminution in the weight of the column NP . A similar addition may be made to every such column perpendicular to the plane BEA . This being supposed, the spherical figure of the globe will be changed into that of an elliptical spheroid, having its axis in the line OC , and its poles in O and C (569.)

Without making this addition to every column NP , we may understand how the *equilibrium* may be restored by the waters subsiding all around the circle whose section is BA , and rising on both sides of it. For it was shewn (564.) that in a fluid elliptical spheroid of gravi-

tating matter, the gravitation of any particle P to all the other particles may be resolved into two forces PN and PM perpendicular to the plane BA and to the axis OC, and proportional to PN and PM; and that if the forces be really in this proportion, the whole will be in equilibrio, provided that the whole forces at the poles and equator are inversely as the diameters OC and BA. Now this may be the case here. For the forces super-added to the terrestrial gravitation of any particle are, *1st*, A force PE, proportional to PE. When this is resolved into the directions PN and PM, the forces arising in this resolution are as PN and PM, and therefore in the due proportion: *2^d*, The force PQ, which is also as PN. It is evident therefore that this mass may acquire such a protuberancy at O and C, that the force at O shall be to the force at B as BA to OC, or as EA to EC. We are also taught in § 585. what this protuberance must be. It must be such that four times the mean gravity of a particle on the surface is to five times the disturbing force at O or C as the diameter BA is to the excess of the diameter OC. This ellipticity is expressed by the same formula as in the former case, viz. $\frac{x}{r} = \frac{4c}{5g}, = \frac{EC - FA}{EC}$.

634. Thus we have discovered that, in consequence of the unequal gravitation of the matter in the Earth to the Sun, the waters will assume the form of an oblong elliptical spheroid, having its axis directed to the Sun, and its
poles

poles in those points of the surface which have the Sun in the zenith and nadir. There the waters are highest above the surface of a sphere of equal capacity. All around the circumference B E A, the waters are below the natural level. A spectator placed on this circumference sees the Sun in the horizon.

We can tell exactly what this protuberance E O — E A must be, because we know the proportions of all the forces. Let W represent the terrestrial gravitation, or the weight of the particle C, and G the gravitation of the same particle to the Sun, and let F be the disturbing force acting on a particle at C or at O, and therefore = 3 C E. Let S and E be the quantity of matter in the Sun and in the Earth.

Then (fig. 59.) $F : G = 3 C E : C G$

$$G : W = \frac{S}{C S^2} : \frac{E}{C E^2} \quad (465.)$$

therefore $F : W = \frac{3 C E \times S}{C S^2} : \frac{C G \times E}{C E^2} =$

$\frac{3 S}{C S^2} \times \frac{E}{C G} : \frac{E}{C E^3}$. But, because $C S^2 : E S^2 = E S : C G$,

we have $C S^2 \times C G = E S^2 \times E S, = E S^3$. There-

fore $F : W = \frac{3 S}{E S^3} : \frac{E}{E C^3}$. Now $E : S = 1 : 33^8 343$,

and $E C : E S = 1 : 23668$. This will give $\frac{3 S}{E S^3} : \frac{E}{E C^3} = 1 : 12773541, = F : W$.

Finally, $4 W : 5 F = C E : C E - A E$. We shall find this to be nearly $24\frac{1}{2}$ inches.

635. Such is the figure that this globe would assume, had it been originally fluid, or a spherical nucleus covered with

with a fluid of equal density. The two summits of the watery spheroid would be raised about two feet above the equator or place of greatest depression.

But the Earth is an oblate spheroid. If we suppose it covered, to a moderate depth, with a fluid, the waters would acquire a certain figure, which has been considered already. Let the disturbing force of the Sun act on this figure. A *change* of figure must be produced, and the waters under the Sun, and those in the opposite parts, will be elevated above their natural surface, and the ocean will be depressed on the circumference B E A. It is plain that this *change* of figure will be almost the same in every place as if the Earth were a sphere. For the difference between the *change* produced by the Sun's disturbing force on the figure of the fluid sphere or fluid spheroid, arises solely from the difference in the gravitation of a particle of water to the sphere and to the spheroid. This difference, in any part of the surface, is exceedingly small, not being $\frac{1}{300^2}$ of the whole gravitation. The difference therefore in the *change* produced by the Sun cannot be $\frac{1}{300^2}$ of the whole change. Therefore, since it is from the *proportion* of the disturbing force to the force of gravity that the ellipticity is determined, it follows that the *change* of figure is, to all sense, the same, whether the Earth be a sphere or a spheroid whose eccentricity is less than $\frac{1}{231}$.

Let us suppose, for the present, that the watery spheroid always has that form which produces an equilibrium

in all its particles. This cannot ever be the case, because some time must elapse before an accelerating force can produce any finite change in the disposition of the waters. But the contemplation of this figure gives us the most distinct notion of the forces that are in action, and of their effects; and we can afterwards state the difference that must obtain because the figure is not completely attained.

Supposing it really attained, it follows that the ocean will be most elevated in those places which have the Sun in the zenith or nadir, and most depressed in those places where the Sun is seen in the horizon. While the Earth turns round its axis, the pole of the spheroid keeps still toward the Sun, as if the waters stood still, and the solid nucleus turned round under it. The phenomena may perhaps be easier conceived by supposing the Earth to remain at rest, and the Sun to revolve round it in 24 hours from east to west. The pole of the spheroid follows him, as the card of a mariner's compass follows the magnet; and a spectator attached to one part of the nucleus will see all the vicissitudes of the tide. Suppose the Sun in the equinox, and the observer also on the Earth's equator, and the Sun just rising to him. The observer is then in the lowest part of the watery spheroid. As the Sun rises above the horizon, the water also rises; and when the Sun is in the zenith, the pole of the spheroid has now reached the observer, and the water is two feet deeper than it was at sun-rise. The Sun now approaching the western horizon, and the pole of the ocean going
along

along with him, the observer sees the water subside again, and at sun-set, it is at the same level as at sun-rise. As the Sun continues his course, though unseen, the opposite pole of the ocean now advances from the east, and the observer sees the water rise again by the same degrees as in the morning, and attain the height of two feet at midnight, and again subside to its lowest level at six o'clock in the following morning.

Thus, in 24 hours, he has two tides of flood and two ebb tides; high water at noon and midnight, and low water at six o'clock morning and evening. An observer not in the equator will see the same *gradation* of phenomena, at the same hours; but the rise and fall of the water will not be so considerable, because the pole of the spheroid passes his meridian at some distance from him. If the spectator is in the pole of the Earth, he will see no change, because he is always in the lowest part of the watery spheroid.

From this account of the simplest case, we may infer that the depth of the water, or its change of depth, depends entirely on the shape of the spheroid, and the place of it occupied by the observer.

636. To judge of this with accuracy, we must take notice of some properties of the ellipse which forms the meridian of the watery spheroid. - Let $A E a Q$ (fig. 69.) represent this elliptical spheroid, and let $B E b Q$ be the inscribed sphere, and $A G a g$ the circumscribed sphere. Also let $D F d f$ be the sphere of equal capacity with the spheroid.

spheroid. This will be the natural figure of the ocean, undisturbed by the gravitation to the Sun.

In a spheroid like this, so little different from a sphere, the elevation AD of its summit above the equally capacious sphere is very nearly double of the depression FE of its equator below the surface of that sphere. For spheres and spheroids, being equal to $\frac{2}{3}$ of the circumscribing cylinders, are in the ratio compounded of the ratio of their equators and the ratio of their axes. Therefore, since the sphere $DFdf$ is equal to the spheroid $AEaQ$, we have $CF^2 \times CD = CE^2 \times CA$, and $CE^2 : CF^2 = CD : CA$. Make $CE : CF = CF : Cx$, then $CE : Cx = CD : CA$, and $CE : Ex = CD : DA$, and $CE : CD = Ex : DA$. Now CE does not differ sensibly from CD (only eight inches in near 4000 miles), therefore Ex may be accounted equal to DA . But Ex is not sensibly different from twice EF . Therefore the proposition is manifest.

637. In such an elliptical spheroid, the elevation IL of any point I above the inscribed sphere is proportional to the square of the cosine of its distance from the pole A , and the depression KI of this point below the surface of the circumscribed sphere is as the square of the sine of its distance from the pole A . Draw through the point I , HIM perpendicular to CA , and Ipn perpendicular to CE . The triangles CIN and pIL are similar.

4 E

Therefore

Therefore $\rho I : IL = CI : IN, = \text{rad.} : \text{cof. } ICA$
 but, by the ellipse $AB : \rho I = AC : IN, = \text{rad.} : \text{cof. } ICA$
 therefore $AB : IL = \text{rad.}^2 : \text{cof.}^2 ICA$
 and IL is always in the proportion of $\text{cof.}^2, ICA$, and
 is $= AB \times \text{cof.}^2, ICA$, radius being $= 1$.
 In like manner $HI : IK = CI : IM = \text{rad.} : \text{fin. } ICA$.
 and $GE : HI = EC : IM = \text{rad.} : \text{fin. } ICA$
 therefore $GE : KI = \text{rad.}^2 : \text{fin.}^2 ICA$
 and KI is $= AB \times \text{fin.}^2 ICA$.

638. We must also know the elevations and depressions in respect of the natural level of the undisturbed ocean. This elevation for any point i is evidently $il - ml = AB \times \text{cof.}^2 iCA - \frac{1}{3} AB, = AB \times \text{cof.}^2 iCA - \frac{1}{3}$, and the depression nr of a point r is $kr - kn = AB \times \text{fin.}^2 rCA - \frac{2}{3} AB, = AB \times \text{fin.}^2 rCA - \frac{2}{3}$.

It will be convenient to employ a symbol for expressing the whole difference AB or GE between high and low water produced by the action of the Sun. Let it be expressed by the symbol S . Also let the angular distance from the summit, or from the Sun's place, be α .

The elevation mi is $= S \times \text{cof.}^2 \alpha - \frac{1}{3} S$.

The depression nr is $= S \times \text{fin.}^2 \alpha - \frac{2}{3} S$.

639. The spheroid intersects the equicapacious sphere in a point so situated that $S \times \text{cof.}^2 \alpha - \frac{1}{3} S = 0$, that is, where $\text{cof.}^2 \alpha = \frac{1}{3}$. This is $54^\circ 44'$ from the pole
 of

Fig. 60.

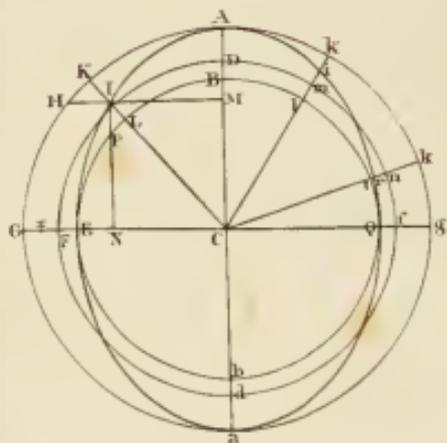


Fig. 68.

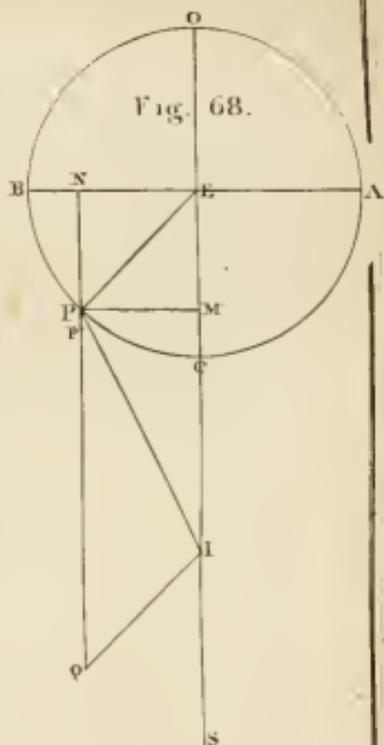
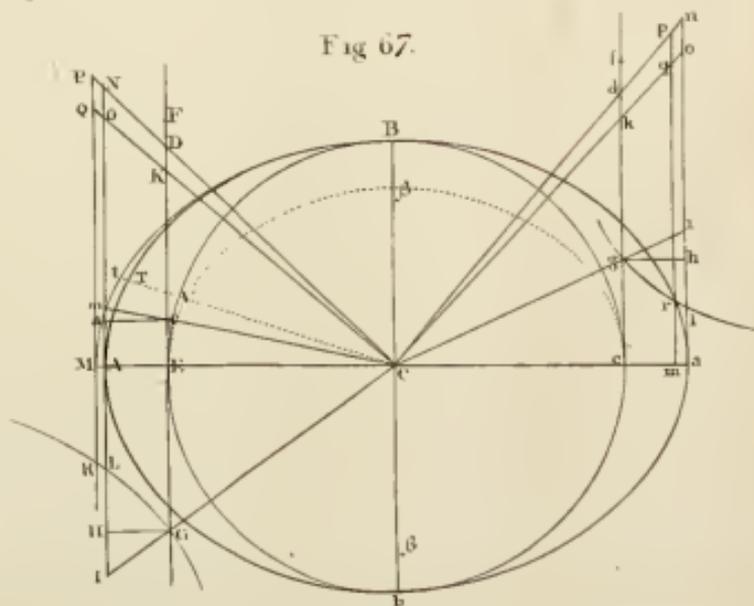


Fig 67.



of the spheroid, and $35^{\circ} 16'$ from its equator, a situation that has several remarkable physical properties. We have already seen (572.) that on this part of the surface the gravitation is the same as if it were really a perfect sphere.

640. The ocean is made to assume an eccentric form, not only by the unequal gravitation of its waters to the Sun, but also by their much more unequal gravitation to the Moon; and, although her quantity of matter is very small indeed, when compared with the Sun, yet being almost 400 times nearer, the inequality of gravitation is increased almost $400 \times 400 \times 400$ times, and may therefore produce a sensible effect.* We cannot help presuming that it does, because the vicissitudes of the tides have a most distinct reference to the position of the Moon. Without going over the same ground again, it is plain that the waters will be accumulated under the Moon, and in the opposite part of the spheroid, in the same manner as they are affected by the Sun's action.

Therefore

* The distance of the Sun being about 392 times that of the Moon, and the quantity of matter in the Sun about 338000 times that in the Earth, if the quantity of matter in the Moon were equal to that in the Earth, her accumulating force would be 178 times greater than that of the Sun. We shall see that it is nearly $2\frac{1}{2}$ times greater. From which we should infer that the quantity of matter in the Moon is nearly $\frac{1}{77}$ of that in the Earth. This seems the best information that we have on this subject.

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Therefore let M represent the elevation of the pole of the spheroid above the equicapacious sphere that is produced by the unequal gravitation to the Moon, and let y be the angular distance of any part of this spheroid from its pole. We shall then have

$$\text{The elevation of any point} = M \times \text{cof.}^2 y - \frac{1}{3} M.$$

$$\text{The depression} = M \times \text{fin.}^2 y - \frac{2}{3} M.$$

641. In consequence of the simultaneous gravitation to both luminaries, the ocean must assume a form differing from both of these regular spheroids. It is a figure of difficult investigation, but all that we are concerned in may be determined with sufficient accuracy by means of the following considerations.

We have seen that the *change* of figure induced on the spheroidal ocean of the revolving globe is nearly the same as if it were induced on a perfect sphere. Much more securely may we say that the change of figure, induced on the ocean already disturbed by the Sun, is the same that the Moon would have occasioned on the undisturbed revolving spheroid. We may therefore suppose, without any sensible error, that the change produced in any part of the ocean by the joint action of the two luminaries is the sum or the difference of the changes which they would have produced separately.

642. Therefore, since the poles of both spheroids are in those parts of the ocean which have the Sun and the Moon in the zenith, it follows that if z be the zenith

nith distance of the Sun from any place, and y the zenith distance of the Moon, the elevation of the waters above the natural surface of the undisturbed ocean will be $S \times \text{cof.}^2 x - \frac{1}{3} S + M \times \text{cof.}^2 y - \frac{1}{3} M$. And the depression in any place will be $S \times \text{fin.}^2 x - \frac{2}{3} S + M \times \text{fin.}^2 y - \frac{2}{3} M$. This may be better expressed as follows.

$$\text{Elevation} = S \times \text{cof.}^2 x + M \times \text{cof.}^2 y - \overline{\frac{1}{3} S + M.}$$

$$\text{Depression} = S \times \text{fin.}^2 x + M \times \text{fin.}^2 y - \overline{\frac{2}{3} S + M.}$$

643. Suppose the Sun and Moon to be in the same part of the heavens. The solar and lunar tides will have the same axes, poles, and equator, the gravitations to each conspiring to produce a great elevation at the combined pole, and a great depression all round the common equator. The elevation will be $\frac{2}{3} \overline{S + M}$, and the depression will be $\frac{1}{3} \overline{S + M}$. Therefore the elevation above the inscribed sphere (or rather the spheroid similar and similarly placed with the natural revolving spheroid) will be $\overline{S + M}$.

644. Suppose the Moon in quadrature in the line EDM (fig. 70.) It is plain that one luminary tends to produce an elevation above the equicapacious sphere $AOBC$, in the point of the ocean A immediately under it, where the other tends to produce a depression, and therefore their forces counteract each other. Let the Sun be in the line ES .

The

The elevation at S = $S - \frac{1}{3} \overline{S + M}$, = $\frac{2}{3} S - \frac{1}{3} M$.

The depression at M = $S - \frac{2}{3} \overline{S + M}$, = $\frac{1}{3} S - \frac{2}{3} M$.

The elevation at S above the inscribed spheroid = $S - M$.

The elevation at M above the same = $M - S$.

Hence it is evident that there will be high water at M or at S, when the Moon is in quadrature, according as the accumulating force of the Moon exceeds or falls short of that of the Sun. Now, it is a matter of observation, that when the Moon is in quadrature, it is high water in the open seas under the Moon, and low water under the Sun, or nearly so. This observation confirms the conclusion drawn from the nutation of the Earth's axis, that the disturbing force of the Moon exceeds that of the Sun. This criterion has some uncertainty, owing to the operation of local circumstances, by which it happens that the summit of the water is never situated either under the Sun or under the Moon. But even in this case, we find that the high water is referable to the Moon, and not to the Sun. It is always six hours of the day later than the high water at full or change. This corresponds with the elongation of the Moon six hours to the eastward. The phenomena of the tides shew further that, at this time, the waters under the Sun are depressed below the natural surface of the ocean. This shews that M is more than twice as great as S.

645. When the Moon has any other position besides these two, the place of high water must be some intermediate

mediate position. It must certainly be in the great circle passing through the simultaneous places of the two luminaries. As the place and time of high and low water, and the magnitude of the elevation and depression, are the most interesting phenomena of the tides, they shall be the principal objects of our attention.

The place of high water is that where the sum of the elevations produced by both luminaries above the natural surface of the ocean is a maximum. And the place of low water, in the great circle passing through the Sun and Moon, is that where the depression below the natural level of the ocean is a maximum. Therefore, in order to have the place of high water we must find where $S \times \text{cof.}^2 x + M \times \text{cof.}^2 y - \frac{1}{3} \overline{S + M}$ is a maximum. Or, since $\frac{1}{3} \overline{S + M}$ is a constant quantity, we must find where $S \times \text{cof.}^2 x + M \times \text{cof.}^2 y$ is a maximum. Now, accounting the tabular sines and cosines as fractions of radius, = 1, we have

$$\text{Cof.}^2 x = \frac{1}{2} + \frac{1}{2} \text{cof. } 2x$$

$$\text{and } \text{Cof.}^2 y = \frac{1}{2} + \frac{1}{2} \text{cof. } 2y.$$

For let $ABSD$ (fig. 71.) be a circle, and AS , BD two diameters crossing each other at right angles. Describe on the semidiameter CS the small circle $CmSb$, having its centre in d . Let HC make any angle x with CS , and let it intersect the small circle in b . Draw db , Sb , producing Sb till it meet the exterior circle in S , and join As , Cs . Lastly, draw bo and sr perpendicular to CS .

Sb is perpendicular to Cb , and $CS : Cb = \text{rad.} : \text{cof.}$

cof. HCS , and $\text{CS} : \text{Co} = \text{R}^2 : \text{cof.}^2 \text{HCS}$. The angle SCr is evidently $= 2 \text{SCH} = \text{Sdb}$ and $\text{Ar} = 2 \text{Co}$. Now if CS be $= 1$; $\text{Cr} = \text{cof.}^2 2x$; $\text{Ar} = 1 + \text{cof.} 2x$. Therefore $\text{Co} = \frac{1}{2} + \frac{1}{2} \text{cof.} 2x$. In like manner $\text{cof.}^2 y = \frac{1}{2} + \frac{1}{2} \text{cof.} 2y$.

Therefore we must have $\frac{\text{S}}{2} + \frac{\text{S} \times \text{cof.} 2x}{2} + \frac{\text{M}}{2} + \frac{\text{M} \times \text{cof.} 2y}{2}$ a maximum, or, neglecting the constant quantities $\frac{\text{S}}{2}$, $\frac{\text{M}}{2}$, and the constant divisor 2, we must have $\text{S} \times \text{cof.} 2x + \text{M} \times \text{cof.} 2y$ a maximum.

Let ABSD (fig. 71.) be now a great circle of the Earth, passing through those points S and M of its surface which have the Sun and the Moon in the zenith. Draw the diameter SCA , and cross it at right angles by BCD . Let Sd be to da as the accumulating force of the Moon to the accumulating force of the Sun, that is, as M to S , which proportion we suppose known. Draw CM in the direction of the Moon's place. It will cut the small circle in some point m . Join ma . Let H be any point of the surface of the ocean. Draw CH , cutting the small circle in b . Draw the diameter bb' . Draw mt and ax perpendicular to bb' , and ay parallel to bb' , and join md . Also draw the chords mb and mb' .

In this construction, md and da represent M and S , the angle $\text{MCH} = y$, and $\text{SCH} = x$. It is farther manifest that the angle $mdb = 2mbC$, $= 2y$, and that $dt = \text{M} \times \text{cof.} 2y$. In like manner $bdS = 2 \text{HCS}$, $= 2x$,

$= 2x$, and $dx = da \times \text{cof. } 2x$, $= S \times \text{cof. } 2x$. Therefore $tx = S \times \text{cof. } 2x + M \times \text{cof. } 2y$. Moreover $tx = ay$, and is a maximum when ay is a maximum. This must happen when ay coincides with am , that is, when bd is parallel to am .

Hence may be derived the following construction.

Let AMS (fig. 72.) be, as before, a great circle whose plane passes through the Sun and the Moon. Let S and M be those points which have the Sun and the Moon in the zenith. Describe, as before, the circle CmS , cutting CM in m . Make $Sd : da = M : S$, and join ma . Then, for the place of high water, draw the diameter bdb' parallel to ma , cutting the circle CmS in b . Draw CbH cutting the surface of the ocean in H and H' . Then H and H' are the places of high water. Also draw Cb' , cutting the surface of the ocean in L and L' . L and L' are the places of low water in this circle.

For, drawing mt and ax perpendicular to bb' , it is plain that $tx = M \times \text{cof. } 2y + S \times \text{cof. } 2x$. And what was just now demonstrated shews that tx is in its maximum state. Also, if the angle $LCS = u$, and $LCM = z$, it is evident that $dx = S \times \text{cof. } adx$, $= S \times \text{cof. } b'dS$, $= S \times \text{cof. } 2b'CS$, $= S \times \text{cof. } 2LCS$, $= S \times \text{cof. } 2u$; and in like manner, $td = M \times \text{cof. } 2z$; and therefore $tx = S \times \text{cof. } 2u + M \times \text{cof. } 2z$, and it is a maximum.

It is plain, independent of this construction, that the places of high and low water are 90° asunder; for the

two hemispheres of the ocean must be similar and equal, and the equator must be equidistant from its poles.

648. Draw df perpendicular to ma . Then, if dS be taken to represent the whole tide produced by the Moon, that is, the whole difference in the height of high and low water, ma will represent the compound tide at H , or the difference between high and low water corresponding to that situation of the place H with respect to the Sun and Moon. mf will be the part of it produced by the Moon and af the part produced by the Sun.

For, the elevation at H above the natural level is $S \times \overline{\text{cof.}^2 x - \frac{1}{3}} + M \times \overline{\text{cof.}^2 y - \frac{1}{3}}$, and the depression below it at L is $S \times \overline{\text{fin.}^2 u - \frac{2}{3}} + M \times \overline{\text{fin.}^2 z - \frac{2}{3}}$. But $\text{fin.}^2 u = \text{cof.}^2 x$, and $\text{fin.}^2 z = \text{cof.}^2 y$. Therefore the depression at L is $S \times \overline{\text{cof.}^2 x - \frac{2}{3}} + M \times \overline{\text{cof.}^2 y - \frac{2}{3}}$. The sum of these makes the whole difference between high and low water, or the whole tide. Therefore the tide is $= S \times \overline{2 \text{cof.}^2 x - 1} + \overline{M \times 2 \text{cof.}^2 y - 1}$. But $2 \text{cof.}^2 x - 1 = \text{cof.} 2x$, and $2 \text{cof.}^2 y - 1 = \text{cof.} 2y$. Therefore the tide $= S \times \text{cof.} 2x + M \times \text{cof.} 2y$. Now it is plain that $mf = md \text{cof.} dmf$, and that the angle $dmf = mdh$, $= 2mCb$, $= 2y$. Therefore $md \times \text{cof.} dmf = M \times \text{cof.} 2y$. In like manner $af = S \times \text{cof.} 2x$.

The point a must be within or without the circle CmS , according as M is greater or less than S , that is, according as the accumulating force of the Moon is greater

or less than that of the Sun. It appears also that, in the first case, H will be nearer to M, and in the second case, it will be nearer to S.

Thus have we given a construction that seems to express all the phenomena of the tides, as they will occur to a spectator placed in the circle passing through those points which have the Sun and Moon in the zenith. It marks the distance of high water from those two places, and therefore, if the luminaries are in the equator, it marks the time that will elapse between the passage of the Sun or Moon over the meridian and the moment of high water. It also expresses the whole height of the tide of that day. And, as the point H may be taken without any reference to high water, we shall then obtain the state of the tide for that hour, when it is high water in its proper place H. By considering this construction for the different relative positions of the Sun and Moon, we shall obtain a pretty distinct notion of the series of phenomena which proceed in regular order during a lunar month.

649. To obtain the greater simplicity in our first and most general conclusions, we shall first suppose both luminaries in the equator. Also, abstracting our attention from the annual motion of the Sun, we shall consider only the relative motion of the Moon in her synodical revolution, stating the phenomena as they occur when the Moon has got a certain number of degrees away from the Sun; and we shall always suppose that the watery spheroid has attained the form suited to its equilibrium

in that situation of the two luminaries. The conclusions will frequently differ much from common observation. But we shall afterwards find their agreement very satisfactory. The reader is therefore expected to go along with the reasoning employed in this discussion, although the conclusions may frequently surprize him, being very different from his most familiar observations.

650. 1. At new and full Moon, we shall have high water at noon, and at midnight, when the Sun and Moon are on the meridian. For in this case CM , am , CS , dh , CH , all coincide.

651. 2. When the Moon is in quadrature in B , the place of high water is also in B , under the Moon, and this happens when the Moon is on the meridian. For when MC is perpendicular to CS , the point m coincides with C , am with aC , and dh with dC .

652. 3. While the Moon passes from a syzygy to the next quadrature, the place of high water follows the Moon's place, keeping to the westward of it. It overtakes the Moon in the quadrature, gets to the eastward of the Moon (as it is represented at $M^2 H^2$, by the same construction), preceding her while she passes forward to the next syzygy, in A , where it is overtaken by the Moon's place. For while M is in the quadrant SB , or AD , the point h is in the arch Sm . But when M is in the quadrant BA or DS , h^2 is without or beyond

yond the arch $S m^2$ (counted *eastward* from S). Therefore, during the first and third quarters of the lunation, we have high water after noon or midnight, but before the Moon's fouthing. But in the second and fourth quarters, it happens after the Moon's fouthing.

653. 4. Since the place of high water coincides with the Moon's place both in syzygy and the following quadrature, and in the interval is between her and the Sun, it follows that it must, during the first and third quarters, be gradually left behind, for a while, and then must gain on the Moon's place, and overtake her in quadrature. There must therefore be a certain greatest distance between the place of the Moon and that of high water, a certain maximum of the angle MCH . This happens when $H'CS$ is exactly 45° . For then $b'dS$ is 90° , $m'a$ is perpendicular to aS , and the angle $am'd$ is a maximum. Now $am'd = m'db', = 2y'$.

654. When things are in this state, the motion of high water, or its separation from the Sun to the eastward, is equal to the Moon's easterly motion. Therefore, at new and full Moon, it must be slower, and at the quadratures it must be swifter. Consequently, when the Moon is in the octant, 45° from the Sun, the interval between two successive fouthings of the Moon, which is always $24^h 50'$ nearly, must be equal to the interval of the two concomitant or superior high waters, and each tide must occupy $12^h 25'$, the half of a lunar day. But
at

at new or full Moon, the interval between the two successive high waters must be less than $12^{\text{h}} 25'$, and in the quadratures it must be more.

655. The tide day must be equal to the lunar day only when the high water is in the octants. It must be shorter at new and full Moon, and while the Moon is passing from the second octant to the third, and from the fourth to the first. And it must exceed a lunar day while the Moon passes from the first octant to the second, and from the third to the fourth. The tide day is always greater than a solar day, or twenty-four hours. For, while the Sun makes one round of the Earth, and is again on the meridian, the Moon has got about 13° east of him, or SM is nearly 13° , and SH is nearly 9° , so that the Sun must pass the meridian about 35 or 36 minutes before it is high water. Such is the law of the daily retardation called the priming or lagging of the tides. At new and full Moon it is nearly $35'$, and at the quadratures it is $85'$, so that the tide day at new and full Moon is $24^{\text{h}} 35'$, and in the quadratures it is $25^{\text{h}} 25'$ nearly.

Our construction gives us the means of ascertaining this circumstance of the tides, or interval between two succeeding full seas, and it may be thus expressed.

656. The synodical motion of the Moon is to the synodical motion of the high water as ma to mf . For, take a point u very near to m . Draw ua and ud , and draw

draw di parallel to au , and with the centre a , and distance au , describe the arch uv , which may be considered as a straight line perpendicular to ma . Then um and ib are respectively equal to the motions of M and H (though they subtend twice the angles). The angles auv , dum are equal, being right angles. Therefore $muv = aud, = amd$, and the triangles muv , dmf , are similar, and the angles uam , idb are equal, and therefore

$$\begin{aligned} uv : ib &= ma : bd, = ma : md \\ um : uv &= md : mf \\ \text{therefore } um : ib &= ma : mf. \end{aligned}$$

When m coincides with S , that is, at new or full Moon, ma coincides with sa , and mf with sd . But when m coincides with C , that is, in the quadratures, ma coincides with ca , and mf with cd .

657. Hence it is easy to see that the retardation of the tides at new and full Moon is to the retardation in the quadratures as Ca to sa , that is, as $M + S$ to $M - S$.

When the high water is in the octant, ma is perpendicular to sa , and therefore a and f coincide, and the synodical motion of the Moon and of high water are the same, as has been already observed.

Let us now consider the elevations of the water, and the magnitude of the tide, and its gradual variation in the course of a lunation. This is represented by the line ma ,

658. This series of changes is very perceptible in our construction. At new and full Moon, ma coincides with sa , and in the quadratures, it coincides with ca . Therefore, the spring-tide is to the neap-tide as sa to ca , that is, as $M + S$ to $M - S$. From new or full Moon the tide gradually lessens to the time of the quadrature. We also see that the Sun contributes to the elevation by the part af , till the high water is in the octants, for the point f lies between m and a . After this, the action of the Sun diminishes the elevation, the point f then lying beyond a .

659. The momentary change in the height of the whole tide, that is, in the difference between the high and low water, is proportional to the sine of twice the arch MH . It is measured by df in our construction. For, let mu be a given arch of the Moon's synodical motion, such as a degree. Then mv is the difference between the tides ma and ua , corresponding to the constant arch of the Moon's momentary elongation from the Sun. The similarity of the triangles muv and mdf gives us $mu : mv = md : df$. Now mu and md are constant. Therefore mv is proportional to df , and $md : df = \text{rad.} : \sin. dmf, = \sin. mdb, = \sin. 2MCH$.

Hence it follows that the diminution of the tides is most rapid when the high water is in the octants. This will be found to be the difference between the twelfth and thirteenth tides, counted from new or full Moon, and between the seventh and eighth tides after the quadratures.

dratures. If mu be taken $= \frac{1}{2}$ the Moon's daily elongation from the Sun, which is $6^{\circ} 30'$ nearly, the rule will give, with sufficient accuracy, $\frac{1}{2}$ the difference between the two superior or the two inferior tides immediately succeeding. It does not give the difference between the two immediately succeeding tides, because they are alternately greater and lesser, as will appear afterwards.

660. Having thus given a representation to the eye of the various circumstances of these phenomena in this simple case, it would be proper to shew how all the different quantities spoken of may be computed arithmetically. The simplest method for this, though perhaps not the most elegant, seems to be the following.

In the triangle $m d a$, the two sides md and da are given, and the contained angle $m d a$, when the proportion of the forces M and S , and the Moon's elongation MCS are given. Let this angle $m d a$ be called a . Then make $M + S : M - S = \tan. a : \tan. b$. Then $y = \frac{a - b}{2}$, and $x = \frac{a + b}{2}$.

For $M + S : M - S = md + da : md - da, =$
 $\tan. \frac{mad + amd}{2} : \tan. \frac{mad - amd}{2} = \tan. \frac{2x + 2y}{2}$
 $: \tan. \frac{2x - 2y}{2}, = \tan. \overline{x + y} : \tan. \overline{x - y} = \tan. a : \tan. b.$
 Now $\overline{x + y} + \overline{x - y} = 2x$ and $\overline{x + y} - \overline{x - y} = 2y$.
 Therefore $a + b = 2x$ and $a - b = 2y$, and $x = \frac{a + b}{2}$,
 and $y = \frac{a - b}{2}$.

661. It is of peculiar importance to know the greatest separation of the high water from the Moon. This happens when the high water is in the octant. In this situation it is plain that $m'd : da$, that is, $M : S$, = $\text{rad.} : \sin. dm'a$, = $\text{rad.} : \sin. 2y'$, and therefore $\sin. 2y' = \frac{S}{M}$. Hence $2y'$ and y' are found.

662. It is manifest that the applicability of this construction to the explanation of the phenomena of the tides depends chiefly on the proportion of Sd to da , that is, the proportion of the accumulating force of the Moon to that of the Sun. This constitutes the species of the triangle mda , on which every quantity depends. The question now is, What is this proportion? Did we know the quantity of matter in the Moon, it would be decided in a minute. The only observation that can give us any information on this subject is the nutation of the Earth's axis. This gives at once the proportion of the disturbing forces. But the quantities observed, the deviation of the Earth's axis from its uniform conical motion round the pole of the ecliptic, and the equation of the precession of the equinoctial points, are much too small for giving us any precise knowledge of this ratio.

Fortunately, the tides themselves, by the modification which their phenomena receive from the comparative magnitude of the forces in question, give us means of discovering the ratio of S to M . The most obvious circumstance of this nature is the magnitude of the spring and neap-tides. Accordingly, this was employed by

Newton

Newton in his theory of the tides. He collected a number of observations made at Bristol, and at Plymouth, and, stating the spring-tide to the neap-tide as $M + S$ to $M - S$, he said that the force of the Moon in raising the tide is to that of the Sun nearly as $4\frac{1}{2}$ to 1. But it was soon perceived that this was a very uncertain method. For there are scarcely any two places where the proportion between the spring-tide and the neap-tide is the same, even though the places be very near each other. This extreme discrepancy, while the proportion was observed to be invariable for any individual place, shewed that it was not the theory that was in fault, but that the local circumstances of situation were such as affected very differently tides of different magnitudes, and thus changed their proportion. It was not till the noble collection of observations was made at Brest and Rochefort that the philosopher could assort and combine the immense variety of heights and times of the tides, so as to throw them into classes to be compared with the aspects of the Sun and Moon according to the Newtonian theory. M. Cassini, and, after him, M. Daniel Bernoulli, made this comparison with great care and discernment; and on the authority of this comparison, M. Bernoulli has founded the theory and explanation contained in his excellent Dissertation on the tides, which shared with M'Laurin and Euler the prize given by the Academy of Paris in 1740.

M. Bernoulli employs several circumstances of the tides for ascertaining the ratio of M to S . He employs

the law of the retardation of the tides. This has great advantages over the method employed by Newton. Whatever are the obstructions or modifications of the tides, they will operate equally, or nearly so, on two tides that are equal, or nearly equal. This is the case with two succeeding tides of the same kind.

The Moon's mean motion from the Sun, in time, is about $50\frac{1}{2}$ minutes in a day. The smallest retardation, in the vicinity of new and full Moon, is nearly $35'$, wanting $15\frac{1}{2}'$ of the Moon's retardation. Therefore, by art. 656,

$$M : S = 35 : 15\frac{1}{2}, = 5 : 2\frac{1}{2} \text{ nearly.}$$

The longest tide-day about the quadratures is $25^h 25'$, exceeding a solar day $85'$, and a lunar day $34\frac{1}{2}'$. Therefore

$$M : S = 85 : 34\frac{1}{2}, = 5 : 2\frac{1}{6} \text{ nearly.}$$

The proportion of M to S may also be inferred by a direct comparison of the tide-day at new Moon and in the quadratures.

$$35 : 85 = M - S : M + S. \text{ Therefore}$$

$$M : S = \frac{85 + 35}{2} : \frac{85 - 35}{2}, = 5 : 2\frac{1}{2}.$$

It may also be discovered by observing the greatest separation of the place of high water from that of the Moon, or the elongation of the Moon when the tide-day and the lunar day are equal. In this case y is observed to be nearly $12^\circ 30'$. Therefore $\frac{S}{M} = \sin. 25^\circ$, and $M : S = 5 : 2\frac{1}{2}$ nearly.

Thus it appears that all these methods give nearly
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the same result, and that we may adopt 5 to 2 as the ratio of the two disturbing forces. This agrees extremely well with the phenomena of nutation and precession.

Instead of inferring the proportion of M to S from the quantity of matter in the Moon, deduced from the phenomena of nutation, as is affected by D'Alembert and La Place, I am more disposed to infer the mass of the Moon from this determination of M : S, confirmed by so many coincidences of different phenomena. Taking 5 : 2,13 as the mean of those determinations, and employing the analogy in § 465, we obtain for the quantity of matter in the Moon nearly $\frac{1}{8}$, the Earth being 1.

If the forces of the two luminaries were equal, there would be no high and low water in the day of quadrature. There would be an elevation above the inscribed spheroid of $\frac{1}{3} \overline{M + S}$ all round the circumference of the circle passing through the Sun and Moon, forming the ocean into an oblate spheroid.

663. Since the gravitation to the Sun alone produces an elevation of $24\frac{1}{2}$ inches, the gravitation to the Moon will raise the waters 58 inches; the spring-tide will be $24\frac{1}{2} + 58$, or $82\frac{1}{2}$ inches, and the neap-tide $33\frac{3}{4}$ inches.

664. The proportion now adopted must be considered as that corresponding to the mean intensity of the accumulating forces. But this proportion is by no means constant, by reason of the variation in the distances of
the

the luminaries. Calling the Sun's mean distance 1000, it is 983 in January and 1017 in July. The Moon's mean distance being 1000, she is at the distance 1055 when in apogeo, and 945 when in perigeo. The action of the luminaries in producing a change of figure varies in the inverse triplicate ratio of their distances (519.) Therefore, if 2 and 5 are taken for the mean disturbing forces of the Sun and Moon, we have the following measures of those forces.

	<i>Sun.</i>	<i>Moon.</i>
Apogean	1,901	4,258
Mean	2,—	5,—
Perigean	2,105	5,925

Hence we see that $M : S$ may vary from 5,925 : 1,901 to 4,258 : 2,105, that is, nearly from 6 : 2 to 4 : 2.

The general expression of the disturbing force of the Moon will be $M = \frac{5}{2} S \times \frac{D^3}{\Delta^3} \times \frac{d^3}{\delta^3}$ where D and d express the mean distances of the Sun and Moon, and Δ and δ any other simultaneous distances.

The solar force does not greatly vary, and need not be much attended to in our computations for the tides. But the change in the lunar action must not be neglected, as this greatly affects both the time and the height of the tide.

665. First, as to the times.

1. The tide-day following spring-tide is $24^h 27^{\frac{1}{2}}'$ when the Moon is in perigeo, and $24^h 33'$ when she is in apogeo.

2. The tide-day following neap-tide is $25^{\text{h}} 15'$ in the first case, and $25^{\text{h}} 40'$ in the second.

3. The greatest interval between the Moon's southing and high water (which happens in the octants) is $39'$ when the Moon is in perigeo, and $61'$ when she is in apogeo, γ being $9^{\circ} 45'$ and $15^{\circ} 15'$.

666. The height of the tide is still more affected by the Moon's change of distance.

If the Moon is in perigeo, when new or full, the spring-tide will be eight feet, instead of the mean spring-tide of seven feet. The very next spring-tide will be no more than six feet, because the Moon is then in apogeo. The neap-tides, which happen between these very unequal tides, will be regular, the Moon being then in quadrature, at her mean distance.

But if the Moon change at her mean distance, the spring-tide will be regular, but one neap-tide will be four feet, and another only two feet.

We see therefore that the regular monthly series of heights and times corresponding to our construction can never be observed, because in the very same, or nearly the same period, the Moon makes all the changes of distance which produce the effects above mentioned. As the effect produced by the same change of the Moon's distance is different according to the state of the tide which it affects, it is by no means easy to apply the equation arising from this cause.

667. As a sort of synopsis of the whole of this description of the monthly series of tides, the following Table by D. Bernoulli will be of some use. The first column contains the Moon's elongation SM (eastward) from the Sun, or from the point opposite to the Sun, in degrees. The second column contains the minutes of solar time that the moment of high water precedes or follows the Moon's southing. This corresponds to the arch HM . The third column gives the arch SH , or nearly the hour and minute of the day at the time of high water; and the fourth column contains the height of the tide, as expressed by the line ma , the space Sa being divided into 1000 parts, as the height of a spring-tide. Note that the elongation is supposed to be that of the Moon at the time of her southing.

TABLE

TABLE I.

SM	H M	Hour.	<i>ma</i>
0	—	—	1000
10	11 $\frac{1}{2}$	-.28 $\frac{1}{2}$	987
20	22	-.58	949
30	31 $\frac{1}{2}$	1.28 $\frac{1}{2}$	887
40	40	2.—	806
50	45	2.35	715
60	46 $\frac{1}{2}$	3.13 $\frac{1}{2}$	610
70	40 $\frac{1}{2}$	3.59 $\frac{1}{2}$	518
80	25	4.55	453
90	—	6.—	429
100	25	7. 5	453
110	40 $\frac{1}{2}$	8. $\frac{1}{2}$	518
120	46 $\frac{1}{2}$	8.46 $\frac{1}{2}$	610
130	45	9.25	715
140	40	10.—	806
150	31 $\frac{1}{2}$	10.31	887
160	22	11.2	949
170	11 $\frac{1}{2}$	11.31	987
180	—	12.—	1000

668. It is proper here to notice a circumstance, of very general observation, and which appears inconsistent with our construction, which states the high water of neap-tides to happen when the Moon is on the meridian. This must make the high water of neap-tides fix

4 H

hours

TABLE I.

S M	H M	Hour.	<i>m a</i>
	Minutes.		
0	—	—	1000
10	11 $\frac{1}{2}$	-.28 $\frac{1}{2}$	987
20	22	-.58	949
30	31 $\frac{1}{2}$	1.28 $\frac{1}{2}$	887
40	40	2.—	806
50	45	2.35	715
60	46 $\frac{1}{2}$	3.13 $\frac{1}{2}$	610
70	40 $\frac{1}{2}$	3.59 $\frac{1}{2}$	518
80	25	4.55	453
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180	—	12.—	1000

668. It is proper here to notice a circumstance, of very general observation, and which appears inconsistent with our construction, which states the high water of neap-tides to happen when the Moon is on the meridian. This must make the high water of neap-tides fix

4 H

hours

hours later than the high water of spring-tides, supposing that to happen when the Sun and Moon are on the meridian. But it is universally observed that the high water of tides in quadrature is only about five hours and ten or twelve minutes later than that of the tides in syzygy.

This is owing to our not attending to another circumstance, namely, that the high water which happens in syzygy, and in quadrature, is not the high water of spring and of neap-tides, but the third before them. They correspond to a position of the Moon 19° westward of the syzygy or quadrature, as will be more particularly noticed afterwards. At these times, the points of high water are $13\frac{1}{2}$ west of the syzygy, and 29 west of the quadrature, as appears by our construction. The lunar hours corresponding to the interval are exactly $5^h 02'$, which is nearly $5^\circ 12'$ solar hours.

669. Hitherto we have considered the phenomena of the tides in their most simple state, by stating the Moon and the Sun in the equator. Yet this can never happen. That is, we can never see a monthly series of tides nearly corresponding with this situation of the luminaries. In the course of one month, the Sun may continue within six degrees of the equator, but the Moon will deviate from it, from 18 to 28 or 30 degrees. This will greatly affect the height of the tides, causing them to deviate from the series expressed by our construction. It still more affects the time, particularly of low water. The phenomena depend primarily on the zenith distances of

of the luminaries, and, when these are known, are accurately expressed by the construction. But these zenith distances depend both on the place of the luminaries in the heavens, and on the latitude of the observer. It is difficult to point out the train of phenomena as they occur in any one place, because the figure assumed by the waters, although its depth be easily ascertained in any single point, and for any one moment, is too complicated to be explained by any general description. It is not an oblong elliptical spheroid, formed by revolution, except in the very moment of new or full Moon. In other relative situations of the Sun and Moon, the ocean will not have any section that is circular. Its poles, and the position of its equator, are easily determined. But this equatoreal section is not a circle, but approaches to an elliptical form, and, in some cases, is an exact ellipse. The longer axis of this oval is in the plane passing through the Sun and Moon, and its extremities are in the points of low water for this circle, as determined by our construction. Its shorter axis passes through the centre of the Earth, at right angles to the other, and its extremities are the points of the *lowest low water*. In these two points, the depression below the natural level of the ocean is always the same, namely, the sum of the greatest depression produced by each luminary. It is subjected therefore only to the changes arising from the changes of distance of the Sun and Moon.

Thus it appears that the surface of the ocean has generally four poles, two of which are prolate or protube-

rant, and two of them are compressed. This is most remarkably the case when the Moon is in quadrature, and there is then a ridge all round that section which has the Sun and Moon in its plane. The section through the four poles, upper and lower, is the place of high water all over the Earth, and the section perpendicular to the axis of this is the place of low water in all parts of the Earth.

Hence it follows that when the luminaries are in the plane of the Earth's equator, the two depressed poles of the watery spheroid coincide with the poles of the Earth; and what we have said of the times of high and low water, and the other states of the tide, are exact in their application. But the heights of the tides are diminished as we recede from the Earth's equator, in the proportion of radius to the cosine of the latitude. In all other situations of the Sun and Moon, the phenomena vary exceedingly, and cannot easily be shewn in a regular train. The position of the high water section is often much inclined to the terrestrial meridians, so that the interval between the transit of the Moon and the transit of this section across the meridian of places in the same meridian is often very different. Thus, on midsummer day, suppose the Moon in her last quadrature, and in the node, therefore in the equator. The ridge which forms high water lies so oblique to the meridians, that when the Moon arrives at the meridian of London, the ridge of high water has passed London about two hours, and is now on the north coast of America. Hence it happens that we have

no satisfactory account of the times of high water in different places, even though we should learn it for a particular day. The only way of forming a good guess of the state of the tides is to have a terrestrial globe before us, and having marked the places of the luminaries, to lap a tape round the globe, passing through those points, and then to mark the place of high water on that line, and cross it with an arch at right angles. This is the line of high water. Or, a circular hoop may be made, crossed by one semicircle. Place the circle so as to pass through the places of the Sun and Moon, setting the intersection with the semicircle on the calculated place of high water. The semicircle is now the line of high water, and if this armilla be held in its present position, while the globe turns once round within it, the succession of tide, or the regular hour of high water for every part of the Earth will then be seen, not very distant from the truth.

At present, in our endeavour to point out the chief modifications of the tides which proceed from the declination of the luminaries, or the latitude of the place of observation, we must content ourselves with an approximation, which shall not be very far from the truth. It will be sufficiently exact, if we attend only to the Moon. The effects of declination are not much affected by the Sun, because the difference between the declination of the Moon and that of the pole of the ocean can never exceed six or seven degrees. When the great circle passing through the Sun and Moon is much inclined to the equator

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tor (it may even be perpendicular to it), the luminaries are very near each other, and the Moon's place hardly deviates from the line of high water. At present we shall consider the lunar tide only.

670. Let $NQSE$ (fig. 73.) represent the terra-queous globe, NS being the axis, EQ the equator, and O the centre. Let the Moon be in the direction OM , having the declination BQ . Let D be any point on the surface of the Earth, and CDL its parallel of latitude, and NDS its meridian. Let $B'Fb'f$ be the elliptical surface of the ocean, having its poles B' and b' in the line OM . Let fOF be its equator.

As the point D is carried along the parallel CDL , it will pass in succession through all the states of the tide, having high water when it is in C , and in L , and low water when it gets into the intersection d of its parallel CL with the equator fdF of the watery spheroid. Draw the meridian NdG through this intersection, cutting the terrestrial equator in G . Then the arch QG , converted into lunar hours, will give the duration of ebb of the superior tide, and GE is the time of the subsequent flood of the inferior tide. It is evident that these are unequal, and that the whole tide GQG , consisting of a flood-tide GQ and ebb-tide QG , while the Moon is above the horizon (which we called the *superior tide*), exceeds the duration of the whole *inferior tide* GEG by four times GO (reckoned in lunar hours.)

If the spheroid be supposed to touch the sphere

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In f and F , then Cc' is the height of the tide. At L , the height of the tide is LL' , and if the concentric circle $L'q$ be described, $C'q$ is the difference between the superior and inferior tides.

From this construction we learn, in general, that when the Moon has no declination, the duration of the superior and inferior tides of one day are equal, over all the Earth.

671. 2. If the Moon has declination, the superior tide will be of longer or of shorter duration than the inferior tide, according as the Moon's declination BQ , and the latitude CQ of the place of observation are of the same or of different denominations.

672. 3. When the Moon's declination is equal to the colatitude of the place of observation, or exceeds it, that is, if BQ is equal to No , or exceeds it, there will be only a superior or inferior tide in the course of a lunar day. For in this case, the parallel of the place of observation will pass through f , or between N and f , as $k\bar{m}$.

673. 4. The sine of the arch GO is $= \tan. \text{lat.} \times \tan. \text{declin.}$ For $\text{rad.} : \cot. dOG = \tan. dG : \text{fin. } GO$, and $\text{fin. } GO = \tan. dG \times \cot. dOG$. Now dG is the latitude, and dOG is the codecl.

674. The heights of the tides are affected in the same way by the declination of the Moon, and by the latitude

titude of the place of observation. The height of the superior tide exceeds that of the inferior, if the Moon's declination is of the same denomination with the latitude of the place, and *vice versa*. It often happens that the reverse of this is uniformly observed. Thus, at the Nore, in the entry to the river Thames, the inferior tide is greater than the superior, when the Moon has north declination, and *vice versa*. But this happens because the tide at the Nore is only the derivation of the great tide which comes round the north of Scotland, ranges along the eastern coasts of Britain, and the high water of a superior tide arrives at the Nore, while that of an inferior tide is formed at the Orkney islands, the Moon being under the horizon.

675. The height of the tide in any place, occasioned by the action of a single luminary, is as the square of the cosine of the zenith or nadir distance of that luminary. Hence we derive the following construction, which will express all the modifications of the lunar tide produced by declination or latitude. It will not be far from the truth, even for the compound tide, and it is perfectly exact in the case of spring or neap-tides.

With a radius CQ (fig. 74.) taken as the measure of the whole elevation of a lunar tide, describe the circle $EPQp$, to represent a terrestrial meridian, where P and p are the poles, and EQ the equator. Bisection CP in O , and round O describe the circle $PBCD$. Let M be that point of the meridian which has the Moon in
the

the zenith, and let Z be the place of observation. Draw the diameter ZCN , cutting the small circle in B , and MCm cutting it in A . Draw AI parallel to EQ . Draw the diameter BOD of the inner circle, and draw IK , GH , and AF perpendicular to BD . Lastly, draw ID , IB , AD , AB , and $CI M'$, cutting the meridian in M' .

After half a diurnal revolution, the Moon comes into the meridian at M' , and the angle $M'CN$ is her distance from the nadir of the observer. The angle ICB is the supplement of ICN , and is also the supplement of IDB , the opposite angle of a quadrilateral in a circle. Therefore IDB is equal to the Moon's nadir distance. Also ADB , being equal to ACB , is equal to the Moon's zenith distance. Therefore, accounting DB as the radius of the tables, DF and DK are as the squares of the cosines of the Moon's zenith and nadir distances; and since PC , or DB , was taken as the measure of the whole lunar tide, DF will be the elevation of high water at the situation Z of the observer, when the Moon is above his horizon, and DK is the height of the subsequent tide, when the Moon is under his horizon, or, more accurately, it is the height of the tide seen at the same moment with DF , by a spectator at z' in the same meridian and parallel. (For the *subsequent* tide, though only twelve hours after, will be a little greater or less, according as they are on the increase or decrease). DF , then, and DK , are proportional to the heights of the superior and inferior tides of that day. Moreover, as AI

is bisected in G, FK is bisected in H, and DH is the arithmetical mean between the heights of the superior and inferior tides. Accounting OC as the radius of the tables, AG is the sine of the arch AC, which measures twice the angle MCQ, the Moon's declination. OG is the cosine of twice the Moon's declination. Also the angle BOG is equal to twice the angle BCQ, the latitude of the observer. Therefore $OH = \cos. 2 \text{ decl.} \times \cos. 2 \text{ lat.}$, and $DH = DO + OH, = M \times \frac{1 + \cos. 2 \text{ decl.} \times \cos. 2 \text{ lat.}}{2}$. This value of the medium tide will be found of continual use.

This construction gives us very distinct conceptions of all the modifications of the height of a lunar tide, proceeding from the various declinations of the Moon, and the position of the observer; and the height of the compound tide may be had by repeating the construction for the Sun, substituting the declination of the Sun for that of the Moon, and S for M in the last formula. The two elevations being added together, and $\frac{1}{2} \overline{M + S}$ taken from the sum, we have the height required. If it is a spring-tide that we calculate for, there is scarcely any occasion for two operations, because the Sun cannot then be more than six degrees from the Moon, and the pole of the spheroid will almost coincide with the Moon's place. We may now draw some inferences from this representation.

676. 1. The greatest tides happen when the Moon
is

is in the zenith or nadir of the place of observation. For as M approaches to Z , A and I approach to B and D , and when they coincide, F coincides with B , and the height of the superior tide is then $= M$. The medium tide however diminishes by this change, because G comes nearer to O , and consequently H comes also nearer to O , and DH is diminished.

If, on the other hand, the place of observation be changed, Z approaching to M , the superior, inferior and medium tides are all increased. For in such case, D separates from I , and DK , DH , and DF are all enlarged.

677. 2. If the Moon be in the equator, the superior and inferior tides are equal, and $= M \times \text{cof.}^2 \text{ lat.}$ For then A and I coincide with C ; and F and K coalesce in i ; and $Di = DB \times \text{cof.}^2 BDC, = DB \times \text{cof.}^2 ZCQ$.

678. 3. If the place of observation be in the equator, the superior and inferior tides are equal every where, and are $= M \times \text{cof.}^2 \text{ declin. } \zeta$. For B then coincides with C ; the points F and K coincide with G ; and $PG = PC \times \text{cof.}^2 CPA, = M \times \text{cof.}^2 MCQ$.

679. 4. The superior tides are greater or less than the inferior tides, according as Z and M are on the same or on opposite sides of the equator. For, by taking QZ' on the other side of the equator, equal to QZ , and

drawing $Z' C z'$, cutting the small circle in β , we see that the figure is simply reversed. The magnitudes and proportions of the tides are the same in either case, but the combination is inverted, and what belongs to a superior tide in the one case belongs to an inferior tide in the other.

680. 5. If the colatitude be equal to the Moon's declination, or less than it, there will be no inferior tide, or no superior tide, according as the latitude and Moon's declination are of the same or of different denominations. For when $PZ = MQ$, D coincides with I , and K also coincides with I . Also when PZ is less than MQ , D falls below I , and the point Z never passes through the equator of the watery spheroid. The low water mm' (fig. 73.) observed in the parallel km is only a lower part of the same tide kk' , of which the high water is also observed in the same place. In such situations, the tides are very small, and are subjected to singular varieties which arise from the Moon's change of declination and distance. Such tides can be seen only in the circumpolar regions. The inhabitants of Iceland notice a period of nineteen years, in which their tides gradually increase and diminish, and exhibit very singular phenomena. This is undoubtedly owing to the revolution of the Moon's nodes, by which her declination is considerably affected. That island is precisely in the part of the ocean where the effect of this is most remarkable. A register kept there would be very instructive;

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and it is to be hoped that this will be done, as in that sequestered Thulé, there is a zealous astronomer, M. Lievog, furnished with good instruments, to whom this series of observations has been recommended.

681. 6. At the very pole there is no daily tide. But there is a gradual rise and subsidence of the water twice in a month, by the Moon's declining on both sides of the equator. The water is lowest at the pole when the Moon is in the equator, and it rises about twenty-six inches when the Moon is in the tropics. Also, when her ascending node is in the vernal equinox, and she has her greatest declination, the water will be thirty inches above its lowest state, by the action of the Moon alone.

682. 7. The medium tide is, as has already been observed, $= M \times \frac{1 + \text{cof. } 2 \text{ decl. } \times \text{cof. } 2 \text{ lat.}}{2}$.

As the Moon's declination never exceeds 30° , the cosine of twice her declination is always a positive quantity, and never less than $\frac{1}{2}$. When the latitude is less than 45° , the cosine of twice the latitude is also positive, but negative when the latitude exceeds 45° . Attending to these circumstances, we may infer,

683. 1. That the mean tides are equally affected by the northerly and southerly declinations of the Moon.

684. 2. If the latitude be exactly 45° , the mean tide is always the same, and $= \frac{1}{2} M$. For in this case

BD

BD is perpendicular to PC, and the point H always coincides with O. This is the reason why, on the coasts of France and Spain, the tides are so little affected by the declination of the luminaries.

685. 3. When the latitude is less than 45° , the mean-tides increase as the declination of the Moon diminishes. For *cofin. 2 lat.* being then a positive quantity, the formula increases when the cosine of the declination of the Moon increases, that is, it diminishes when the declination of the Moon increases. As BQ diminishes, G comes nearer to C, and H separates from O towards B, and DH increases.

But if the latitude exceed 45° , the point H must fall between O and D, and the mean-tide will increase as the declination increases.

686. 5. If the latitude be $= 0$, the point H coincides with G, and the effect of the Moon's declination is then the most sensible. The mean-tide in this case is

$$M \times \frac{1 + \text{cof. } 2 \text{ declin. } \zeta}{2}.$$

685. Every thing that has been determined here for the lunar tide may easily be accommodated to the high and low water of the compound tide, by repeating the computations with S in the place of M, as the constant coefficient. But, in general, it is almost as exact as the nature of the question will admit, to attend only to the lunar

Fig 73.

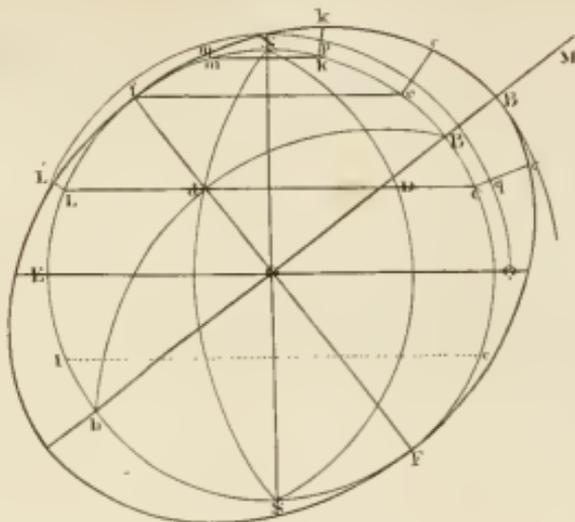
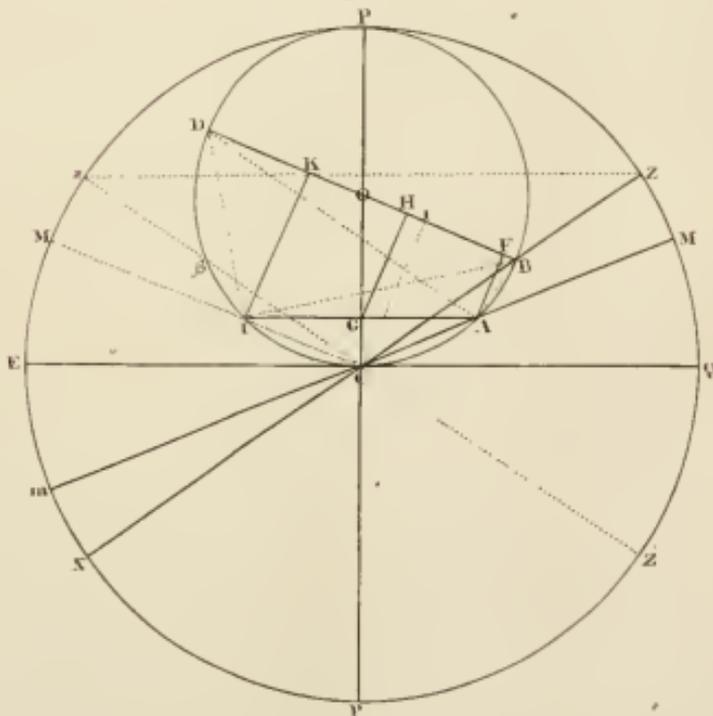


Fig 74



lunar tide. The declination of the real summit of the spheroid, in this case, never differs from the declination of the summit of the lunar tide more than two degrees, and the correction may be made at any time by a little reflection on the simultaneous position of the Sun. What has been said is strictly applicable to the spring-tides.

$\overline{M + S - \text{tide}} \times \text{fin.}^2 d O$ (fig. 73.) is the quantity to be added to the tide found by the construction. It is exact in spring-tides and very near the truth in all other cases. The $\text{fin.}^2 d O$ is $= \frac{S^2 \text{ lat.}}{\text{cos.}^2 \text{ decl. } \mathcal{Q}}$. For $\text{fin. } d O G :$
 $\text{fin. } d G O = \text{fin. } d G : \text{fin. } d O$.

Such, then, are the more simple and general consequences of gravitation on the waters of our ocean, on the supposition that the whole globe is covered with water, and that the ocean always has the form which produces a perfect equilibrium of force in every particle.

686. But the globe is not so covered, and it is clear that there must be a very great extent of open sea, in order to produce that elevation at the summit of the spheroid which corresponds with the accumulating force of the luminaries. A quadrant at least of the ellipse is necessary for giving the whole tide. With less than this, there will not be enough of water to make up the spheroid. And, to produce the full daily vicissitude of high and low water, this extent of sea must be in longitude. An equal extent in latitude may produce the greatest elevation; but it will not produce the series of heights that should

lunar tide. The declination of the real summit of the spheroid, in this case, never differs from the declination of the summit of the lunar tide more than two degrees, and the correction may be made at any time by a little reflection on the simultaneous position of the Sun. What has been said is strictly applicable to the spring-tides.

$\overline{M + S - \text{tide}} \times \sin.^2 d O$ (fig. 73.) is the quantity to be added to the tide found by the construction. It is exact in spring-tides and very near the truth in all other cases. The $\sin.^2 d O$ is $= \frac{S^2 \text{ lat.}}{\cos.^2 \text{ decl. } \mathcal{C}}$. For $\sin. d O G : \sin. d G O = \sin. d G : \sin. d O$.

Such, then, are the more simple and general consequences of gravitation on the waters of our ocean, on the supposition that the whole globe is covered with water, and that the ocean always has the form which produces a perfect equilibrium of force in every particle.

686. But the globe is not so covered, and it is clear that there must be a very great extent of open sea, in order to produce that elevation at the summit of the spheroid which corresponds with the accumulating force of the luminaries. A quadrant at least of the ellipse is necessary for giving the whole tide. With less than this, there will not be enough of water to make up the spheroid. And, to produce the full daily vicissitude of high and low water, this extent of sea must be in longitude. An equal extent in latitude may produce the greatest elevation; but it will not produce the series of heights that should

should occur in the course of a lunar day. In confined seas of small extent, such as the Caspian, the Euxine, the Baltic, and the great lakes in North America, the tides must be almost insensible. For it is evident that the greatest difference of height on the shore of such confined seas can be no more than the deflection from the tangent of the arch of the spheroid contained in that sea. This, in the Caspian Sea, cannot exceed seven inches; a quantity so small, that a slight breeze of wind, setting off shore, will be sufficient for preventing the accumulation, and even for producing a depression. A moderate breeze, blowing along the canal in St James's Park at London, raises the water two inches at one end, while it depresses it as much at the other. The only confined seas of considerable extent are the Mediterranean and the Red Sea. The first has an extent of 40° in longitude, and the tides there might be very sensible, were it on the equator, but being in lat. 35 nearly, the effects are lessened in the proportion of five to four. In such a situation, the phenomena are very different, both in regard to time and to kind, from what they would be, if the Mediterranean were part of the open ocean. Its surface will be *parallel* to what it would be in that case, but *not the same*. This will appear by inspection of fig. 75, where $m r p$ represents the natural level of the ocean, and $M o Q$ represents the watery spheroid, having its pole in M , and its equator at Q . $S s$ may represent a tide post, set up on the shore of Syria, at the east end of the Mediterranean, and $G o$ a post set up at the Gut of Gibraltar, which we shall

shall suppose at present to be dammed up. When the Moon is over M , the waters of the Mediterranean assume the surface grs , parallel to the corresponding portion of the elliptical surface QoM , crossing the natural surface at r , nearly in the middle of its length. Thus, on the Syrian coast, there is a considerable elevation of the waters, and at Gibraltar, there is a considerable depression. In the middle of the length, the water is at its mean height. The water of the Atlantic Ocean, an open and extensive sea, assumes the surface of the equilibrated spheroid, and it stands considerably higher on the outside of the dam, as is seen by Go , than on the inside, as expressed by Gg . It is nearly low water within the Straits, while it is about $\frac{1}{3}$ or $\frac{1}{2}$ flood without. The water has been ebbing for some hours within the Straits, but flowing for great part of the time without. As the Moon moves westward, toward Gibraltar, the water will begin to rise, but slowly, within the Straits, but it is flowing very fast without. When the Moon gets to P , things are reversed. The summit of the spheroid (it being supposed a spring-tide) is at P , and it is nearly high water within the Straits, but has been ebbing for some hours without. It is low water on the coast of Syria. All this while, the water at r , in the middle of the Mediterranean, has not altered its height by any sensible quantity. It will be high water at one end of the Mediterranean, and low water at the other, when the middle is in that part of the general spheroid where the surface makes the most unequal angles with the vertical. This will be

nearly in the octants, and therefore about $1\frac{3}{4}$ hours before and after the Moon's fouthing (fuppofing it fpring-tide).

These obfervations greatly contribute to the explanation of the fingular currents in the Straits of Gibraltar, as they are described by different authors. For although the Mediterranean is not fhut up, and altogether feperated from the Atlantic Ocean at Gibraltar, the communication is extremely scanty, and by no means fufficient for allowing the tide of the ocean to diffufe itfelf into this bafon in a regular manner. Changes of tide, always different, and frequently quite oppofite, are obferved on the eaft and weft fide of the narrow neck which connects the Rock with Spain; and the general tenor of thofe changes has a very great analogy with what has now been described. The tides in the Mediterranean are fmall, and therefore eafily affected by winds. But they are remarkably regular. This may be expected. For as the collection or abfttraction neceffary for producing the change is but fmall, they are foon accomplished. The registers of the tides at Venice and fome other ports in the Adriatic are furprifingly conformable to the theory. See Phil. Tranf. Vol. LXVII.

From this example, it is evident that great deviations may be expected in the obferved phenomena of the tides from the immediate results of the fimple unobftructed theory, and yet the theory may be fully adequate to the explanation of them, when the circumftances of local fituation are properly confidered.

638. The real state of things is such, that there are very few parts of the ocean where the theory can be applied without very great modifications. ; Perhaps the great Pacific Ocean is the only part of the terraqueous globe in which all the forces have room to operate. When we consider the terrestrial globe as placed before the acting luminaries, which have a relative motion round it from east to west, and consider the accumulation of the waters as keeping pace with them on the ocean, we must see that the tides with which we are most familiarly acquainted, namely, those which visit the western shores of Europe and Africa, and the eastern shores of America, must also be irregular, and be greatly diversified by the situation of the coasts. The accumulation on our coasts must be in a great measure supplied by what comes from the Indian and Ethiopic Ocean from the eastward, and what is brought, or kept back, from the South Sea ; and the accumulation must be diffused, as from a collection coming round the Cape of Good Hope, and round Cape Horn. Accordingly, the propagation of high water is entirely consonant with such a supposition. It is high water at the Cape of Good Hope about three o'clock at new and full moon, and it happens later and later, as we proceed to the northward along the coast of Africa ; later and later still as we follow it along the west coasts of Spain and France, till we get to the mouth of the English Channel. In short, the high water proceeds along those shores just like the top of a wave, and it may be followed, hour after hour, to the different harbours

along the coast. The same wave continues its progress northwards (for it seems to be the only supply), part of it going up St George's Channel, part going northward by the west side of Ireland, and a branch of it going up the English Channel, between this island and France. What goes up by the east and west sides of Ireland unites, and proceeds still northward, along the western coasts and islands of Scotland, and then diffuses itself to the eastward, toward Norway and Denmark, and, circling round the eastern coasts of Britain, comes southward, in what is called the German Ocean, till it reaches Dover, where it meets with the branch which went up the English Channel.

689. It is remarkable that this northern tide, after having made such a circuit, is more powerful than the branch which proceeds up the English Channel. It reaches Dover about a quarter of an hour before the southern tide, and forces it backwards for half an hour. It must also be remarked, that the tide which comes up channel is not the same with the tide which meets it from the north, but is a whole tide earlier, if not two tides. For the spring-tide at Rye is a tide earlier than the spring-tide at the Nore. It even seems more nearly two tides earlier, appearing the one as often as the other. This may be better seen by tracing the hour of high water from the Lizard up St George's Channel and along the west coasts of Scotland. Now it is very clear that the superior tide at the Orkney islands is simultaneous with

with the inferior tide at the mouth of the Thames. It is therefore most probable that the Orkney tide is at least one tide later than at the Lizard. The whole of this tide is very anomalous, especially after getting to the Orkneys. It is a derivative from the great tide of the open sea, which being very distant, is subjected to the influence of hard gales, at a distance, and frequently unlike what is going on upon our coasts.

690. A similar progress of the same high water from the southward, is observed along the eastern shores of South America. But, after passing Brazil and Surinam, the Atlantic Ocean becomes so wide that the effect of this high water, as an adventitious thing supplied from the southward, is not so sensible, because the Atlantic itself is now extensive enough to contribute greatly to the formation of the regular spheroid. But it contributes chiefly by abstraction of the waters from the American side, while the accumulation is forming on the European side of the Atlantic. By studying the successive hours of high water along the western coasts of Africa and Europe, it appears that it takes nearly two days, or between four and five tides, to come from the Cape of Good Hope to the mouth of the English Channel. This remark is of peculiar importance.

691. Few observations have, as yet, been made public concerning the tides in the Great Pacific Ocean. They must exhibit phenomena considerably different from

from what are seen in the Atlantic. The vast stretch of uninterrupted coast from Cape Horn to Cook's Straits, prevents all supply from the eastward for making up the spheroid. So far as we have information, it appears that the tides are very unlike the European tides, till we get 40° or 50° west from the coast of America. In the neighbourhood of that coast, there is scarcely any inferior tide. Even in the middle of the vast Pacific Ocean the tides are very small, but abundantly regular.

692. The setting of the tides is affected, not only by the form of the shores, but also by the inequalities which undoubtedly obtain in the bottom of the ocean. A deep and long valley there will give a direction to the waters which move along it, even although they far overtop the higher parts on each side, just as we observe the wind follow the course of the vallies. This direction of the undermost waters affects those that flow above them, in consequence of the mutual adhesion of the filaments; and thus the whole stream is deflected from the direction which it would have taken, had the ground been even. By such deflections the path is lengthened, and the time of its reaching a certain place is protracted; and this produces other deviations from the calculations by the simple theory.

693. These peculiarities in the bed or channel also greatly affect the height of the tides. When a wave of a certain magnitude enters a channel, it has a certain
quantity

quantity of motion, measured by the quantity of water and its velocity. If the channel, keeping the same depth, contract in its width, the water, keeping for a while its momentum, must increase its velocity, or its depth, or both. And thus it may happen that, although the greatest elevation produced by the joint action of the Sun and Moon in the open sea does not exceed eight or nine feet, the tide in some singular situations may mount considerably higher. It seems to be owing to this that the high water of the Atlantic Ocean, which at St Helena does not exceed four or five feet, setting in obliquely on the coast of North America, ranges along that coast, in a channel gradually narrowing, till it is stopped in the Bay of Fundy as in a hook, and there it heaps up to an astonishing degree. It sometimes rises 120 feet in the harbour of Annapolis-Royal. Were it not that we see instances of as strange effects of a sudden check given to the motion of water, we should be disposed to think that the theory is not adequate to the explanation of the phenomena. But the extreme disparity that we may observe in places very near each other, and which derive their tide from the very same tide in the open sea, must convince us that such anomalies do not impugn the general principle, although we should never be able fully to account for the discrepancy.

694. Nothing causes so much irregularity in the tides as the reflection of the tide from shore to shore. If a pendulum, while vibrating, receives little impulses,

at

at intervals that are always the same, and very nearly equal to its own vibrations, or even to an aliquot part of them, the vibrations may be increased to a great magnitude after some time, and then will gradually diminish, and thus have periods of increase and decrease. So it happens in the undulation which constitutes a tide. The situation of the coasts may be such, that the time in which this undulation would, of itself, play backward and forward from shore to shore, may be so exactly fitted to the recurring action of the Moon, that the succeeding impulses, always added to the natural undulation, may raise it to a height altogether disproportioned to what the action of the Moon can produce in open sea, where the undulation diffuses itself to a vast distance. What we see in this way should suffice for accounting for the great height of the tides on the coasts of continents. Dan. Bernoulli, justly thinking that the obstructions of various kinds to the movements of the ocean should make the tides less than what the unobstructed forces are able to produce, concluded, from the great tides actually observed, and compared with the tides producible by the Newtonian theory, that this theory was erroneous. He thought it all derived from Newton's erroneous idea of the proportion of the two axes of the terraqueous globe; which mistake results from the supposition of primitive fluidity, and uniform density. He investigates the form of the Earth, accommodated to a nucleus of great density, covered with a rarer fluid, and he thinks that he has demonstrated that the height of the tide will be
in

in proportion to the comparative density of this nucleus, or the rarity of the fluid. This, says he, alone can account for the tides that we really observe; and which, great as they are, are certainly only a part of what they would be, were they not so much obstructed. This is extremely specious, and, coming from an eminent mathematician, has considerable authority. But the problem of the figure of the Earth has been examined with the most scrupulous attention, since the days of M. Bernoulli, by the first mathematicians of Europe, who are all perfectly agreed in their deductions, and confirm that of Sir Isaac Newton. They have also proved, and we apprehend that it is sufficiently established in art. 603, that a denser nucleus, instead of making a greater tide, will make it smaller than if the whole globe be of one density. The ground of Bernoulli's mistake has also been clearly pointed out. There remains no other way of accounting for the great tides but by causes such as have now been mentioned. When the tides in the open Pacific Ocean never exceed three or four feet, we must be convinced that the extravagant tides observed on the coasts of great continents are anomalies; for there, the obstructions are certainly greater than in the open sea. We must therefore look for an explanation in the motions and collisions of disturbed tides. These anomalies therefore bring no valid objection against the general theory.

695. There are some situations where it is easy to explain the deviations, and the explanation is instructive.

Suppose a great navigable river, running nearly in a meridional direction, and falling into the sea in a southern coast. The high water of the ocean reaches the mouth of this river (we may suppose) when the Sun and Moon are together in the meridian. It is therefore a spring-tide high water at the mouth of the river at noon. This checks the stream at the mouth of the river, and causes it to deepen. This again checks the current farther up the river, and it deepens there also, because there is always the same quantity of land water pouring into it. The stream is not perhaps stopped, but only retarded. But this cannot happen without its growing deeper. This is propagated farther and farther up the stream, and it is perceived at a great distance up the river. But this requires a considerable time. Our knowledge in hydraulics is too imperfect as yet to enable us to say in what number of hours this sensible check, indicated by the smaller velocity, and greater depth, will be propagated to a certain distance. We may suppose it just a lunar day before it arrive at a certain wharf up the river. The Moon, at the end of the day, is again on the meridian, as it was when it was a spring-tide at the mouth of the river the day before. But, in this interval, there has been another high water at the mouth of the river, at the preceding midnight, and there has just been a third high water, about fifteen minutes before the Moon came to the meridian, and thirty-five minutes after the Sun has passed it. There must have been two low waters in the interval, at the mouth of the river.

Now,

Now, in the same way that the tide of yesterday noon is propagated up the stream, the tide of midnight has also proceeded upwards. And thus, there are three coexistent high waters in the river. One of them is a spring-tide, and it is far up, at the wharf above mentioned. The second, or the midnight tide, must be half way up the river, and the third is at the mouth of the river. And there must be two low waters intervening. The low water, that is, a state of the river below its natural level, is produced by the passing low water of the ocean, in the same way that the high water was. For when the ocean falls below its natural level at the mouth of the river, it occasions a greater declivity of the issuing stream of the river. This must augment its velocity—this abstracts more water from the stream above, and that part also sinks below its natural level, and gives a greater declivity to the waters behind it, &c. And thus the stream is accelerated, and the depth is lessened, in succession, in the same way as the opposite effects were produced. We have a low water at different wharfs in succession, just as we had the high waters.

696. This state of things, which must be familiarly known to all who have paid any attention to these matters, being seen in almost every river which opens into a tide way, gives us the most distinct notion of the mechanism of the tides. The daily returning tide is nothing but an undulation or wave, excited and maintained by the action of the Sun and Moon. It is a great mistake

to imagine that we cannot have high water at London Bridge (for example) unless the water be raised to that level all the way from the mouth of the Thames. In many places that are far from the sea, the stream, at the moment of high water, is down the river, and sometimes it is considerable. At Quebec, it runs downward at least three miles per hour. Therefore the water is not heaped up to the level; for there is no stream without a declivity. The harbour at Alloa in the river Forth is dry at low water, and the bottom is about six feet higher than the highest water mark on the stone pier at Leith. Yet there are at Alloa tides of twenty, and even twenty-two feet. All Leith would then be under water, if it stood level from Alloa at the time of high water there.

After considering a tide in this way, any person who has remarked the very strange motions of a tide river, in its various bendings and creeks, and the currents that are frequently observed in a direction opposite to the general stream, will no longer expect that the phenomena of the tides will be such as immediately result from the regular operation of the solar and lunar forces.

697. There is yet another cause of deviation, which is perhaps more dissimulating than any local circumstances, and the operation of which it is very difficult to state familiarly, and yet precisely. This is the inertia, as it is called, of the waters. No finite change of place or of velocity can be produced in an instant by any accelerating force. Time must elapse before a stone can acquire any measurable velocity by falling.

Suppose

Suppose the Earth fluid to the centre, and at rest, without any external disturbing force. The ocean will form a perfect sphere. Let the Moon now act on it. The waters will gradually rise immediately under the Moon and in the opposite part of the Earth, sinking all around the equator of the spheroid. Each particle proceeds to its ultimate situation with an accelerated motion, because, till then, the disturbing force exceeds the tendency of the water to subside. Therefore, when the form is attained which balances those forces, the motion does not stop, just as a pendulum does not stop when it reaches the lowest point of its arch of vibration. Suppose that the Moon ceases to act at this instant. The motion will still go on, and the ocean will overpass the balanced figure, but with a retarded motion, as the pendulum rises on the other side of the perpendicular. It will stop at a certain form, when all the former acceleration is done away by the tendency of the water to subside. It now begins to subside at the poles of the spheroid, and to rise at the equator, and after a certain time, it becomes a perfect sphere, that is, the ocean has its natural figure. But it passes this figure as far on the other side, and makes a flood where there was formerly an ebb; and it would now oscillate for ever, alternately swelling and contracting at the points of syzygy and quadrature. If the Moon do not cease to act, as was just now supposed, there will still be oscillations, but somewhat different from those now mentioned. The middle form, on both sides of which it oscillates in this case, is not the perfect sphere, but the balanced spheroid.

698. All this is on the supposition that there is no obstruction. But the mutual adhesion of the filaments of water will greatly check all these motions. The figure will not be so soon formed; it will not be so far overpassed in the first oscillation; the second oscillation will be less than the first, the third will be less than the second, and they will soon become insensible.

But if it were possible to provide a recurring force, which should tend to raise the waters where they are already rising, and depress them where they are subsiding, and that would always renew those actions in the proper time, it is plain that this force may be such as will just balance the obstructions competent to any particular degree of oscillation. Such a recurring force would just maintain this degree of oscillation. Or the recurring force may be greater than this. It will therefore increase the oscillations, till the obstructions are also so much increased that the force is balanced by them. Or it may be less than what will balance the obstructions to the degree of oscillation excited. In this case the oscillation will decrease, till its obstructions are no more than what this force will balance. Or this recurring force may come at improper intervals, sometimes tending to raise the waters when they are subsiding in the course of an oscillation, and depressing them when they are rising. Such a force must check and greatly derange the oscillations; destroying them altogether, and creating new ones, which it will increase for some time, and then check and destroy them; and will do this again and again.

Now

Now there is such a recurring force. As the Earth turns round its axis, suppose the form of the balanced spheroid attained in the place immediately under the Moon. This elevation or pole is carried to the eastward by the Earth, suppose into the position $D O B$ (fig. 76.), the Moon being in the line $O M$. The pole of the watery spheroid is no longer under the Moon. The Moon will therefore act on it so as to change its figure, making it subside in the remote quadrant $B b C$, and rise a little in the quadrant $B a A$. Thus its pole will come a little nearer to the line $O M$. It is plain that if B is carried farther eastward, but within certain limits, the situation of the particles will be still more unfavourable to the lunar disturbing force, and its action on each to change its position will be greater. The action upon them all will therefore make a more rapid change in the position of the pole of the displaced spheroid. It seems not impossible that this pole may be just so far east, that the changing forces may be able to cause its pole to shift its position fifteen miles in one minute. If this be the case, the pole of the spheroid will keep precisely at its present distance from the line $O M$. For, since it would shift to the westward fifteen miles in one minute by the action of the Moon, and is carried fifteen miles to the eastward in that time by the rotation of the Earth, the one motion just undoes the effect of the other. The pole of the watery spheroid is really made to shift fifteen miles to the westward on the surface of the Earth, and arrives at a place fifteen miles west of its former place

the globe ; but this place of arrival is carried fifteen miles to the eastward ; it is therefore as far from the line OM as before.

This may be illustrated by a very simple experiment, where the operation of the acting forces is really very like that of the lunar disturbing force. Suppose a chain or flexible rope $ABCEDF$ laid over a pulley, and hanging down in a bight, which is a catenarean curve, having the vertical line OD for its axis, and D for its lowest point, which the geometers call its vertex. Let the pulley be turned very slowly round its axis, in the direction ABC . The side CE will descend, and FA will be taken up, every link of the chain moving in the curve $CEDFA$. Every link is in the vertex D in its turn, just as every portion of the ocean is in the vertex or pole of the spheroid in its turn. Now let the pulley turn round very briskly. The chain will be observed to alter its figure and position. OD will no longer be its axis, nor D its vertex. It will now form a curve $CedfA$, lying to the left hand of $CEDFA$. Od will be its new axis, and d will be its vertex. Gravity acts in lines parallel to OD . The motions in the direction CE and FA nearly balance each other. But there is a general motion of every link of the hanging chain, by which it is carried from E towards F . Did the chain continue in the former catenarea, this force could not be balanced. It therefore keeps so much awry, in the form $CedfA$, that its tendency by gravity to return to its former position is just equal to the sum of all the motions

tions in the links from E towards F. And it will shew this tendency by returning to that position, the moment that the pulley gives over turning. The more rapidly we turn the pulley round, the farther will the chain go aside before its attitude become permanent.

700. It surpasses our mathematical knowledge to say with precision how far eastward the pole of the tide must be from the line of the Moon's direction, even in the simple case which we have been considering. The real state of things is far more complicated. The Earth is not fluid to the centre, but is a solid nucleus, on which flows an ocean of very small depth. In the former case, a very moderate motion of each particle of water is sufficient for making the accumulation in one place and the depression in another. The particles do little more than rise or subside vertically. But, in the case of a nucleus covered with an ocean of small depth, a considerable horizontal motion is required for bringing together the quantity of water wanted to make up the balanced spheroid. The obstructions to such motion must be great, both such as arise from the mutual adhesion of the filaments of water, and many that must arise from friction and the inequalities of the bottom, and the configuration of the shores. In some places, the force of the acting luminaries may be able to cause the pole of the spheroid to shift its situation as fast as the surface moves away, when the angle MOB is 20 degrees. In other places, this may not be till it is 25°, and in another, 15° may be e-

nough. But, in every situation, there will be an arrangement that will produce this permanent position of the summit. For when the obstructions are great, the balanced form will not be nearly attained; and when this is the case, the change producible on the position of a particle is more rapidly effected, the forces being great, or rather the resistance arising from gravity alone being small.

701. The consequence of all this must be, in the first place, that that form which the ocean would ultimately assume, did the Earth not turn round its axis, will never be attained. As the waters approach to that form, they are carried eastward, into situations where the disturbing forces tend to depress them on one side, while they raise them on the other, causing a westerly undulation, which keeps its summit at nearly the same distance from the line of the acting luminary's direction. This westerly motion of the summit of the undulation does not necessarily suppose a real transference of the water to the westward at the same rate. It is more like the motion of ordinary waves, in which we see a bit of wood or other light body merely rise and fall without any sensible motion in the direction of the wave. In no case whatever is the horizontal motion of the water nearly equal to the motion of the summit of the wave. It resembles an ordinary wave also in this, that the rate at which the summit of the undulation advances in any direction is very little affected by the height of the wave.

Our

Our knowledge however in hydraulics has not yet enabled us to say with precision what is the relation between the height of the undulation and the rate of its advance.

702. Thus then it appears, in general, that the summit of the tide must always be to the eastward of the place assigned to it by our simple theory, and that experience alone can tell us how much. Experience is more uniform in this respect than one should expect. For it is a matter of almost universal experience that it is very nearly 19 or 20 degrees. In a few places it is less, and in many it is 5 or 6, or 7 degrees more. This is inferred from observing that the greatest and the smallest of all the tides do not happen on the very time of the syzgies and quadratures, but the third, and in some places, the fourth tide after. Subsequent observation has shewn that this is not peculiar to the spring and neap-tides, but obtains in all. At Brest (for example) the tide which bears the mark of the augmentation arising from the Moon's proximity is not the tide seen while the Moon is in perigeo, but the third after. In short, the whole series of monthly tides disagree with the simultaneous position of the luminaries, but correspond most regularly with their positions 37 or 38 hours before.

703. Another observation proper for this place is, that as different extent of sea, and different depth of water, will and do occasion a difference in the time in which a great undulation may be propagated along it, it

may happen that this time may so correspond with the repetition of all the agitating forces, that the action of to-day may so conspire with the remaining undulation of yesterday, as to increase it by its reiterated impulses, to a degree vastly greater than its original quantity. By giving gentle impulses in this way to a pendulum, in the direction of its motion, its vibrations may be increased to fifty times their first size. It is not necessary, for this effect, that the return of the luminary into the favourable situation be just at the interval of the undulation. It will do if it conspire with every second or third or fourth undulation; or, in general, if the amount of its conspiring actions exceeds considerably, and at no great distance of time, the amount of its opposing actions. In many cases this cooperation will produce periods of augmentation and diminution, and many seeming anomalies, which may greatly vary the phenomena.

704. A third observation that should be made here is, that as the obstructions to the motion of the ocean arising from the mutual adhesion and action of the filaments are known to be so very great, we have reason to believe that the change of form actually produced is but a moderate part of what the force can ultimately produce, and that none of the oscillations are often repeated. It is not probable that the repetitions of the great undulations can much exceed four or five. When experiments are made on still water, we rarely see a pure undulation repeated so often. Even in a syphon of glass,
where

where all diffusions of the undulating power is prevented, they are rarely sensible after the fifth or sixth. A gentle smooth undulation on the surface of a very shallow basin, in the view of agitating the whole depth, will seldom be repeated thrice. This is the form which most resembles a tide.

705. After this account of the many causes of deviation from the motions assigned by our theory, many of which are local, and reducible to no rule, it would seem that this theory, which we have taken so much pains to establish, is of no use, except that of giving us a general and most powerful argument for the universal gravitation of matter. But this would be too hasty a conclusion. We shall find that a judicious consideration of the different classes of the phenomena of the tides will suggest such relations among them, that by properly combining them, we shall not only perceive a very satisfactory agreement with the theory, but shall also be able to deduce some important practical inferences from it.

706. Each of the different modifications of a tide has its own period, and its peculiar magnitude. Where the change made by the acting force is but small, and the time in which it is effected is considerable, we may look for a considerable conformity with the theory; but, on the other hand, if the change to be produced on the tide is very great, and the time allowed to the forces for effecting it is small, it is equally reasonable to expect
sensible

fenfible deviations. If this confideration be judiciously applied, we fhall find a very fatisfactory conformity.

707. Of all the modifications of a tide, the greateft, and the moft rapidly effected, is the difference between the fuperior and inferior tides of the fame day. When the Moon has great declination, the fuperior tide at Brest may be three times greater than the fucceeding or inferior tide. But the fact is, that they differ very little. M. de la Place fays that they do not differ at all. We cannot find out his authority. Having examined with the moft fcrupulous attention more than 200 of the obfervations at Brest and Rochefort and Port l'Orient, and made the proper allowance for the diftances of the luminaries, we can fay with confidence that this general af fertion of M. de la Place is not founded on the obfervations that have been published, and it does not agree with what is obferved in the other ports of Europe. There is always obferved a difference, agreeing with theory in the proportions, and in the order of their fuc ceflion, although much fmaller. A very flight confidera tion will give us the reafon of the obferved difcrepancy. It is not poffible to make two immediately fucceeding un dulations of inert water remarkably different from each other. The great undulation, in retiring, caufes the wa ter to heap up to a greater height in the offing; and this, in diffufing itfelf, muft make the next undulation greater on the fhore. That this is the true account of the matter is fully proved by obferving that when the
theoretic

theoretic difference between those two tides is very small, it is as distinctly observed in the harbours as when it is great. This is clearly seen in the Brest observations.

708. The absolute magnitudes of the tides are greatly modified by local circumstances. In some harbours there is but a small difference between the spring and neap-tides, and in other harbours it is very great. But, in either case, the small daily changes are observed to follow the proportion required by the theory with abundant precision. Counted half way from the spring to the neap-tides, the hourly fall of the tide is as the square of the time from spring-tide, except so far as this may be changed by the position of the Moon's perigee. In like manner, the hourly increase of the tides after neap-tide is observed to be as the squares of the time from neap-tide.

709. The priming and lagging of the tides corresponds with the theory with such accuracy, that they seem to be calculated from it, independent of observation. There is nothing that seems less likely to be deranged than this. Tides which differ very little from each other, either as to magnitude or time, should be expected to follow one another just as the forces require. There is indeed a deviation, very general, and easily accounted for. There is a small acceleration of the tides from spring-tide to neap-tide. This is undoubtedly

doubtedly owing to the obstructions. A smaller tide being less able to overcome them, is sooner brought to its maximum. The deviation however is very small, not exceeding $\frac{1}{4}$ of an hour, by which the neap-tide anticipates the theoretical time of its accomplishment. It would rather appear at first sight that a small tide would take a longer time of going up a river than a great one. And it may be so, although it be sooner high water, because the defalcation from its height may sooner terminate its rising. There is no difference observed in this respect, when we compare the times of high water at London Bridge and at the Buoy of the Nore. They happen at the very same time in both places, and therefore the spring-tides and the neap-tides employ the same time in going up the river Thames.

710. This agreement of observation with theory is most fortunate; and indeed without it, it would scarcely have been possible to make any practical use of the theory. But now, if we note the exact time of the high water of spring-tide for any harbour, and the exact position of the Sun and Moon at that time, we can easily make a table of the monthly series for that port, by noticing the difference of that time from our table, and making the same difference for every succeeding phasis of the tide.

711. But, in thus accommodating the theoretical series to any particular place, we must avoid a mistake commonly

commonly made by the composers of tide tables. They give the hour of high water at full and change of the Moon, and this is considered as spring-tide. But perhaps there is no part in the world where that is the case. It is usually the third tide after full or change that is the greatest of all, and the third tide after quadrature is, in most places, the smallest tide. Now it is with the greatest tide that our monthly series commences. Therefore, it is the hour of *this* tide that is to be taken for the hour of the harbour. But, as winds, freshes, and other causes, may affect any individual tide, we must take the medium of many observations; and we must take care that we do not consider as a spring-tide one which is indeed the greatest, but chances to be enlarged by being a perigean tide.

When these precautions are taken, and the tides of one monthly series marked, by applying the same correction to the hours in the third column of Bernoulli's table (I.), it will be found to correspond with observation with sufficient accuracy for all purposes. In making the comparison, it will be proper to take the medium between the superior and inferior tides of each day, both with respect to time and height, because the difference in these respects between those two tides never entirely disappears.

712. The series of changes which depend on the change of the Moon's declination are of more intricate comparison, because they are so much implicated with

the changes depending on her distance. But when freed as much as possible from this complication, and then estimated by the medium between the superior and inferior tide of the same day, they agree extremely well with the theoretical series.

This, by the way, enables us to account for an observation which would otherwise appear inconsistent with the theory, which affirms that the superior tide is greatest when the Moon is in the zenith (676.) The observation is, that on the coasts of France and Spain the tides increase as the Moon is nearer to the equator. But it was shewn in the same article, that in latitudes below 45° , the medium tide increases as the Moon's declination diminishes. Bernoulli justly observes that the tides with which we are most familiarly acquainted, and from which we form all our rules, must be considered as derived from the more perfect and regular tide formed in the widest part of the Atlantic ocean. Extensive however as this may be, it is too narrow for a complete quadrant of the spheroid. Therefore it will grow more and more perfect as its pole advances to the middle of the ocean; and the changes which happen on the bounding coasts, from which the waters are drawn on all sides to make it up, must be vastly more irregular, and will have but a partial resemblance to it. They will however resemble it in its chief features. This tide being formed in a considerably southern latitude, it becomes the more certain that the medium tide will diminish as the Moon's declination increases. But although this seeming objection

occurs

occurs on the French coasts, it is by no means the case on ours, or more to the north. We always observe the superior tide to exceed the inferior, if the Moon have north declination.

The same agreement with theory is observable in the solar tides, or in the effect of the Sun's declination. This indeed is much smaller, but is observed by reason of its regularity. For although it is also complicated with the effects of the Sun's change of distance, this effect having the same period with his declination, one equation may comprehend them both. M. Bernoulli's observation, just mentioned, tends to account for a very general opinion, that the greatest tides are in the equinoxes. I observe, however, that this opinion is far from being well established. Both Sturmy and Coleprefs speak of it as quite uncertain, and Wallis and Flamsteed reject it. It is agreed on all hands that our winter tides exceed the summer tides. This is thought to confirm that point of the theory which makes the Sun's accumulating force greater as his distance diminishes. I am doubtful of the applicability of this principle, because the approach of the Sun causes the Moon to recede, and her recess is in the triplicate ratio of the Sun's approach. Her accumulating force is therefore diminished in the sesquuplicate ratio of the Sun's approach, and her influence on the phenomena of the tides exceeds the Sun's.

713. The changes arising from the Moon's change of distance are more considerable than those arising from

her change of declination. By reason of their implication with those changes, the comparison becomes more difficult. M. Bernoulli did not find it so satisfactory. They are, in general, much less than theory requires. This is probably owing to the mutual effects of undulations which should differ very considerably, but follow each other too closely. In M. de la Place's way of considering the phenomena (to be mentioned afterwards) the diminution in magnitude is very accountable, and, in other respects, the correspondence is greatly improved. When the Moon changes either in perigeo or apogeo, the series is considerably deranged, because the next spring-tide is formed in opposite circumstances. The derangement is still greater, when the Moon is in perigeo or apogeo in the quadratures. The two adjoining spring-tides should be regular, and the two neap-tides extremely unequal.

714. We shall first consider the changes produced on the times of full sea, and then the changes in the height. M. Bernoulli has computed a table for both the perigean and apogean distance of the Moon, from which it will appear what correction must be made on the regular series. It is computed precisely in the same way as the former, the only difference being in the magnitude of M and S, and we may imitate it by a construction similar to fig. 72. To make this table of easier use, M. Bernoulli introduces the important observation, that the greatest tide is not, in any part of the world, the
tide

tide which happens on the day of new or full Moon, nor even the first or the second tide after; and that with respect to the Atlantic Ocean, and all its coasts, it is very precisely the third tide. So that should we have high water in any port precisely at noon on the full or change of the Moon, and on the first day of the month, the greatest tide happens at midnight on the second day of the month, or, expressing it in the common way, it is the tide which happens when the Moon is a day and a half old. The summit of the spheroid is therefore 19 or 20 degrees to the eastward of the Sun and Moon. At this distance, the tendency of the accumulating forces of the Sun and Moon to complete the spheroid, and to bring its pole precisely under them, is just balanced by the tendency of the waters to subside. Therefore it is raised no higher, nor can it come nearer to the Sun and Moon, because then the obliquity of the force is diminished, on which the changing power depends. That this is the true cause, appears from this, that it is, in like manner, on the third tide that all the changes are perceived which correspond to the declination of the Moon, or her distance from the Earth. Every thing falls out therefore as if the luminaries were 19 or 20 degrees eastward of where they are, having the pole of the spheroid in its theoretical situation with respect to this fictitious situation of the luminaries. But, in such a case, were the Sun and Moon 20° farther eastward, they would pass the meridian 80 minutes, or one hour and 20 minutes later. Therefore $1^{\text{h}} 20'$ is added to the hours of

of

of high water of the former table, calculated for the mean distance of the Moon from the Earth. Thus, on the day of new Moon, we have not the spring-tide, but the third tide before it, that is, the tide which should happen when the Moon is 20° west of the Sun, or has the elongation 160° . This tide, in our former table, happens at $11^h 02'$. Therefore add to this $1^h 20'$, and we have $0^h 22'$ for the hour of high water on the day of full and change for a harbour which would otherwise have high water when the Sun and Moon are on the meridian. In this way, by adding $1^h 20'$ to the hours of high water in the former table for a position of the luminaries 20° farther west, it is accommodated to the observed elongation of the Moon, this elongation being always supposed to be that of the Moon when she is on the meridian. Such then is the following table of M. Bernoulli. The first column gives the Moon's elongation from the Sun, or from the opposite point of the heavens, the Moon being then on the meridian. The second column gives the hour of high water when the Moon is in perigeo. The third column (which is the same with the former table, with the addition of $1^h 20'$) gives the hour of high water when the Moon is at her mean distance. And the fourth column gives the hour when she is in apogeo.

TABLE

TABLE II.

$\zeta \alpha' \odot$	ζ in Perigeo.	ζ in M. Dist.	ζ in Apogeo	ζ in Perigeo.	ζ in M. Dist.	ζ in Apogeo
0	-.18	-.22	$-.27\frac{1}{2}$	After 18	After 22	After 27
10	$-.49\frac{1}{2}$	$-.51\frac{1}{2}$	-.54	After. 9 $\frac{1}{2}$	After. 11 $\frac{1}{2}$	After. 14
20	1.20	1.20	1.20	—	—	—
30	$1.50\frac{1}{2}$	$1.48\frac{1}{2}$	1.46	9 $\frac{1}{2}$	11 $\frac{1}{2}$	14
40	2.22	2.18	$2.12\frac{1}{2}$	18	22	$27\frac{1}{2}$
50	2.54	$2.48\frac{1}{2}$	$2.40\frac{1}{2}$	26	$31\frac{1}{2}$	$39\frac{1}{2}$
60	3.27	3.20	3.10	33	40	50
70	$4.2\frac{1}{2}$	3.55	3.44	37	45	56
80	$4.41\frac{1}{2}$	$4.33\frac{1}{2}$	4.22	$38\frac{1}{2}$	$46\frac{1}{2}$	58
90	$5.26\frac{1}{2}$	$5.19\frac{1}{2}$	$5.9\frac{1}{2}$	$33\frac{1}{2}$	$40\frac{1}{2}$	$50\frac{1}{2}$
100	6.19	6.15	6.9	22	25	31
110	7.20	7.20	7.20	—	—	—
120	8.21	8.25	8.31	After the Moon's Southing.	21	25
130	$9.13\frac{1}{2}$	$9.20\frac{1}{2}$	$9.30\frac{1}{2}$	33	40	50
140	$9.58\frac{1}{2}$	$10.6\frac{1}{2}$	10.18	38	46	58
150	$10.37\frac{1}{2}$	10.45	10.56	37	45	56
160	11.13	11.20	11.30	33	40	50
170	11.46	$11.51\frac{1}{2}$	$11.59\frac{1}{2}$	26	31	39
180	-.18	-.22	$-.27\frac{1}{2}$	18	22	27

715. This table, though of considerable service, being far preferable to the usual tide tables, may sometimes deviate a few minutes from the truth, because it is calculated on the supposition of the luminaries being in the equator. But when they have considerable declination, the

the horary arch of the equator may differ two or three degrees from the elongation. But all this error will be avoided by reckoning the high water from the time of the Moon's fouthing, which is always given in our almanacks. This interval being always very small (never 12°) the error will be insensible. For this reason, the three other columns are added, expressing the priming of the tides on the Moon's fouthing.

To accommodate this table to all the changes of the Moon's declination would require more calculation than all the rest. We shall come near enough to the truth, if we lessen the minutes in the three hour-columns $\frac{1}{8}$ when the Moon is in the equator, and increase them as much when she is in the tropic, and if we use them as they stand when she is in a middle situation.

716. All that remains now, is to adjust this general table to the peculiar situation of the port. Therefore, collect a great number of observations of the hour of high water at full or change of the Moon. In making this collection, note particularly the hour on those days where the Moon is new or full precisely at noon; for this is the circumstance necessary for the truth of the elongations in the first column of the table. A small equation is necessary for correcting the observed hour of high water, when the syzygy is not at noon, because in this situation of the luminaries, the tide lags $35'$ behind the Sun in a day, as has been already shewn. Suppose the lagging to be $36'$, this will make the equation $1\frac{1}{2}$ minute

nute for every hour that the full or change has happened before or after the noon of that day. This correction must be added to the observed hour of high water, if the syzygy was before noon, and subtracted, if it happened after noon. Or, if we choose to refer the time of high water to the Moon's southing, which, in general, is the best method, we must add a minute to the time between high sea and the Moon's southing for every hour and half that the syzygy is before noon, and subtract it if the syzygy has happened after noon. For the tides prime 15' in 24 hours.

717. Having thus obtained the medium hour of high water at full and change of the Moon, note the difference of it from $0^h 22'$, and then make a table peculiar to that port, by adding that difference to all the numbers of the columns. The numbers of this table will give the hour of high water corresponding to the Moon's elongation for any other time. It will, however, always be more exact to refer the time to the Moon's southing, for the reasons already given.

By means of a table so constructed, the time of high water for the port, in any day of the lunation, may be depended on to less than a quarter of an hour, except the course of the tides be disturbed by winds or freshes, which admit of no calculation. It might be brought nearer by a much more intricate calculation; but this is altogether unnecessary, on account of the irregularities arising from those causes.

It is not so easy to state in a series the variations which happen in the *height* of the tides by the Moon's change of distance, although they are greater than the variations in the *times* of high water. This is partly owing to the great differences which obtain in different ports between the greatest and smallest tides, and partly from the difficulty of expressing the variations in such a manner as to be easily understood by those not familiar with mathematical computations. M. Bernoulli, whom we have followed in all the practical inferences from the physical theory, imagines that, notwithstanding the great disproportion between the spring and neap-tides in different places, and the differences in the absolute magnitudes of both, the middle between the highest and lowest daily variations will proceed in very nearly the same way as in theory. Instead therefore of taking the values of M and S as already established, he takes the height of spring and neap-tides in any port as indicative of $M + S$ and $M - S$ for that port. Calling the spring-tide A and the neap-tide B , this principle will give us $M = \frac{A + B}{2}$, and $S = \frac{A - B}{2}$. From these values of M and S he computes their apogean and perigean values, and then constructs columns of the height of the tides, apogean and perigean, in the same manner as the column already computed for the mean distance of the Moon, that is, computing the parts mf and af (fig. 72.) of the whole tide ma separately. The same may be done with incomparably less trouble by our construction (fig. 72.) and the values $M = \frac{A + B}{2}$, and $S = \frac{A - B}{2}$.

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Although this is undoubtedly an approximation, and perhaps all the accuracy that is attainable, it is not founded on exact physical principles. The local proportion of A to B depends on circumstances peculiar to the place; and we have no assurance that the changes of the lunar force will operate in the same manner and proportion on these two quantities, however different. We are certain that it will not; otherwise the proportion of spring and neap-tides would be the same in all harbours, however much the springs may differ in different harbours. I compared Bernoulli's apogean and perigean tides, in about twenty instances, selected from the observations at Brest and St Malo, where the absolute quantities differ very widely. I was surprised, but not convinced, by the agreement. I am however persuaded that the table is of great use, and have therefore inserted it, as a model by which a table may easily be computed for any harbour, employing the spring-tide and neap-tide heights observed in that harbour as the A and B for that place. The table is, like the last, accommodated to the easterly deviation of the pole of the spheroid from its theoretical place.

It appears from this table, and also from the last, that the neap-tides are much more affected by the inequalities of the forces than the spring-tides are. The neap-tides vary from 70 to 128, and the springs from 90 to 114. The first is almost doubled, the last is augmented but $\frac{1}{4}$.

TABLE III.

Elongation. ☾ ☉	HEIGHT OF THE TIDE.		
	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo.
0	0,99A + 0,15B	0,88A + 0,12B	0,79A + 0,08B
10	1,10A + 0,04B	0,97A + 0,03B	0,87A + 0,02B
20	1,14A + 0,00B	1,00A + 0,00B	0,90A + 0,00B
30	1,10A + 0,04B	0,97A + 0,03B	0,87A + 0,02B
40	0,99A + 0,15B	0,88A + 0,12B	0,79A + 0,08B
50	0,85A + 0,32B	0,75A + 0,25B	0,68A + 0,18B
60	0,67A + 0,53B	0,59A + 0,41B	0,53A + 0,29B
70	0,46A + 0,75B	0,41A + 0,59B	0,37A + 0,41B
80	0,28A + 0,96B	0,25A + 0,75B	0,23A + 0,53B
90	0,13A + 1,13B	0,12A + 0,88B	0,11A + 0,62B
100	0,03A + 1,24B	0,03A + 0,97B	0,03A + 0,68B
110	0,00A + 1,28B	0,00A + 1,00B	0,00A + 0,70B
120	0,03A + 1,24B	0,03A + 0,97B	0,03A + 0,68B
130	0,13A + 1,13B	0,12A + 0,88B	0,11A + 0,62B
140	0,28A + 0,96B	0,25A + 0,75B	0,23A + 0,53B
150	0,46A + 0,75B	0,41A + 0,59B	0,37A + 0,41B
160	0,67A + 0,53B	0,59A + 0,41B	0,53A + 0,29B
170	0,85A + 0,32B	0,75A + 0,25B	0,68A + 0,18B
180	0,99A + 0,15B	0,88A + 0,12B	0,79A + 0,08B

719. The attentive reader cannot but observe that all the tables of this monthly construction must be very imperfect, although their numbers are perfectly accurate, because, in the course of a month, the declination and distance of the Moon vary, independently of each other,

other, through all their possible magnitudes. The last table is the only one that is immediately applicable, by interpolation. It would require several tables of the same extent, to give us a set of equations, to be applied to the original table of art. 667. ; and the computation would become as troublesome for this approximation as the calculation of the exact value, taking in every circumstance that can affect the question. For that calculation requires only the computation of two right-angled spherical triangles, preparatory to the calculation of the place of high water. But, with all these imperfections, M. Bernoulli's second table is much more exact than any tide table yet published,

Such, on the whole, is the information furnished by the doctrine of universal gravitation concerning this curious and important phenomenon. It is undoubtedly the most irrefragable argument that we have for the truth and universality of this doctrine, and at the same time for the simplicity of the whole constitution of the solar system, so far as it can be considered mechanically. No new principle is required for an operation of nature so unlike all the other phenomena in the system.

720. The method which I have followed in the investigation is nearly the same with that of its illustrious discoverer. We have contented ourselves with shewing various serieses of phenomena, which tally so well with the legitimate consequences of the theory, that the real source

source of them can no longer be doubted. And, notwithstanding the various deviations from those consequences, arising from other circumstances, we have obtained practical rules, which make the mariner pretty well acquainted with the general course of the tides; sufficiently to put him on his guard against the dangers he runs by grossly mistaking them, and even enabling him to take advantage of the course of the tide for prosecuting his voyage. Still, however, a great store of local information is necessary. For there are some parts of the ocean, where the tides follow an order extremely unlike what we have described. The bar of Tonquin in China is one of the most remarkable; and its chief peculiarity consists in its having but one tide in each lunar day. It has been traced to the cooperation of two great tides, coming from opposite quarters, with almost six hours of difference in the time of high water. The result of which is, that the compound tide is the excess of the one above the other, forming a high water when the sum of both their elevations is a maximum. Dr Halley has given a very distinct explanation of this tide in N^o 162 of the Philosophical Transactions.

721. A very different method of investigating this and a similar phenomenon has been employed by the eminent mathematicians D'Alembert and La Place, in which M. La Place, who makes this a chief article of his *Mechanique Celeste*, deduces the whole directly from the interior mechanism of hydrostatical undulations. His main inferences perfectly agree with those already delivered.

vered. The method of Newton and Bernoulli has been preferred here, because by this means the connexion with the operation of universal gravitation is much better kept in sight. At the same time La Place's method allows us, in some cases, to state the individual fact more nearly as it occurs, without considering it as the modification of another fact that is more general. But it may be doubted, whether La Place has explained all the variety of phenomena. His whole application is limited by the data which furnish the arbitrary quantities in his equations. These being wholly taken from the observations in the ports of France and Spain, it may be questioned whether the sameness, arising from the latitude being so near 45° , may not have made the ingenious author simplify too much his theory. He considers every class of phenomena as operations completely accomplished, and the ocean at the end of the action of any one of the forces as in a state of indifference, ready for the free operation of the next. For example, the equality of the superior and inferior tides of one day is deduced by La Place immediately from the circumstance of the ocean being of nearly an uniform depth, saying that the small inferior tide is not affected by the greatness of the preceding superior tide, because the obstructions are such that all motions cease very soon, almost immediately after the force has ceased to act. We doubt the truth of the near uniformity of the sea's depth. The unequal tides are confessedly most remarkable on the coasts, where the depth is the most unequal. The other principle,

principle, that the effects of primitive motions are all obliterated, and therefore every tide is the completed operation of the present force, is still more questionable. It is well known that the roll of a great storm in the Bay of Biscay is very sensible indeed for three days. Of this we have had repeated experience. The *superficial* agitation of a storm (for it is no more) is nothing in comparison with the huge uniform momentum of a tide; and the greatest storm, even while it blows, cannot raise the tide three feet; nor does it even then change what we have called the tide, the difference between high and low water; it raises or keeps down both nearly alike. Besides, how will M. La Place account for the undeniable duration of every tide wave on the coasts of Europe and America for a day and a half? There can be no question about this, because the course of the tides during a month is precisely conformable to it. The tide which bears the mark of the perigean tide is not the tide which happens when the Moon is in perigeo, but the third following that tide, just as in the springs and neaps. In like manner, it is observed at Brest, without one exception for six years, that the morning or superior tide at new Moon is smaller than the inferior tide in summer. In winter it is the contrary, not, however, with such constant accuracy. Now, it should be just the contrary, if the tides observed were the tides corresponding with the then state of the forces. But they are not. They are tides corresponding with the state of the forces thirty-six hours before. (See Mem. Acad. Par. 1720,

p. 206, duodecimo). It is the same at full Moon, that is, the morning tide in summer is less than the evening tide. The morning tide corresponding to the then state of the forces is what we have called an inferior tide, the Moon being then under the horizon, with south declination. The tide therefore should be greater than the subsequent or evening, or superior tide. But, like the last example, it is the tide corresponding to the forces in action thirty-six hours before. Can we now deny that the present state of the waters is affected by the action of forces which have ceased thirty-six hours ago? and if this be granted, it is impossible that two tides immediately succeeding can be very unequal. The contrary can be shewn in an experiment perfectly resembling the great tides of the ocean. An apparatus, made for exhibiting the appearance of a reciprocating spring, was so constructed that one of its runnings was very sudden and copious, and the next was moderate and slow. It emptied into a small basin, which communicated with a long and narrow horizontal channel, shut at the far end, the basin emptying itself by a small spout on the opposite side. Thus, two very unequal floods and ebbs presented themselves at the mouth of this channel, and sent a wave along it, which, at the first, was very unequal. But, when it was mixed with the returning wave from the far end, they were soon brought to an apparent equality. The experiment appearing curious, it was prosecuted, by various changes of the apparatus; and several effects tended very much to explain some of the more

singular appearances of the tides. There is an example of the continuance of former impressions in the tides among the western islands of Scotland, that considerably resembles the tide on the bar of Tonquin. The general course of the flood round the little island of Berneray is N. E. and that of the ebb is S. W. But at a certain time in the spring, both flood and ebb run N. E. during twelve hours, and the next flood and ebb run S. W. The contrary happens in autumn. Yet in the offing, the flood and ebb hold their regular courses. This greatly resembles the tide at Tonquin, and also the Grecian Euripus.

722. The reader will recollect that we stated as our opinion that, in consequence of the inertia of the waters, the pole of the ocean is always to the eastward of its theoretical place. For which reason, the figure actually attained by the ocean is not a figure of equilibration. Did the Earth stand still, it would soon be brought to its proper position, and completed to its due form. Therefore, there is always a motion *towards* this completion: *And this motion is obstructed.* Hence we apprehend that there must be a perpetual current of the waters, especially in the tropical regions, from east to west. We cannot see how this can be avoided; and we think that it is established as a matter of nautical observation. In regard to the Atlantic, this seems to be a general opinion of the navigators. There are two very excellent journals of voyages from Stockholm to China, by Captain

tain Eckhart, in which there is a very frequent comparison of the ship's reckoning with lunar observations and the arrivals on known coasts, from which we cannot help inferring the same general current in the Indian and Ethiopic seas. It seems therefore to obtain over the whole. The part of this current which diffuses itself into the Atlantic is but small, it having a freer passage straight forward. But the part thus diffused produces the gulf stream, in its way along the American coasts, and escapes round the north capes of Europe and America. In all probability, a southerly current may be observed in the straits which separate America from the Asiatic continent. The whole amount of this motion cannot be considerable, but there must be some, if there be two circumpolar communications between the great eastern and western divisions of the ocean. Without this, it must be reduced to a reciprocating motion too intricate for investigation.

723. There is another circumstance which seems to strengthen our confidence in the reality of this westerly current of the ocean. The gravity of the waters being more diminished in conjunction and opposition than it is augmented in quadrature with the acting luminary, each particle tends to recede from the centre, and to describe a larger circle, employing a longer time. Here is a tendency or *nifus* to a relative motion westerly. Water, being almost perfectly fluid, will obey this tendency, and in time acquire such a motion, were it not obstructed by

solid obstacles. But some effect must remain, too intricate to admit any calculation, and perhaps not ultimately sensible.

724. If the height of the atmosphere be equal to the radius of the Earth, we shall have a tide in the air double of that in the ocean. When all the affecting circumstances are considered, it appears that an ebb and flood of the atmosphere may differ in elevation about 120 feet. This might be sensible by affecting the barometer. True, the gravity of the mercury is also diminished, but not so much as that of the more distant air. But the height of the atmosphere is too small to give rise to any such tides. They cannot sensibly exceed those of the ocean, and this cannot change the height of the mercury in the barometer $\frac{1}{100}$ of an inch. Professor Toaldo at Padua kept a register of the barometer for more than thirty years. He has added into one sum all the mercurial heights observed at new Moon. Another sum was made of all the heights observed in the quadratures; another of the perigean; and another of the apogean heights, &c. &c. He thinks that differences were observed in those sums sufficient for proving the accumulation and compression of the air by its unequal gravitation to the Moon. Thus, the apogean heights exceeded the perigean by 14 inches. The heights in syzygy exceeded those in quadrature by 11 inches. (See Mem. Berlin 1777, and a book expressly on the subject).

But there is another effect of this disturbing force
which

which may be much more sensible, namely, the general westerly current of the air. M. D'Alembert has investigated this with great care, and singular address, and has proved that there must be a westerly current in the tropical regions, at the rate of eight feet nearly in a second. This is a very adequate cause of the trade winds which are observed between the tropics. It is indeed increased by the rarefaction of the air occasioned by the heat of the Sun, which expands the air heated by the ground, and it is both raised and diffused laterally. When the Sun has passed the meridian a proper number of degrees, the air must now cool, and in cooling contract behind the Sun. Air from the east comes in greater abundance than from any other quarter to supply the vacancy.

725. The disk of Jupiter, when viewed through a good telescope, is distinguishable into zones, like a bit of striped satin. These zones, or belts, are of changeable breadth and position, but all parallel to his equator. Therefore they are not attached to his surface, but float on it, as clouds float in our atmosphere. This Earth will have somewhat of this appearance, if viewed from the Moon. For each climate has a state of the sky peculiar in some degree to itself in this respect, and there must be a sort of sameness in one climate all round the globe. A series of observations on a particular spot of Jupiter's surface demonstrate his rotation in $9^{\text{h}} 56'$. Spots have been observed in the belts, which have lasted so long as to make several revolutions before they were effaced.

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They appear to require a minute or two more for their rotation, and therefore have a westerly motion relative to the firm surface of the planet. This however cannot be depended on from the time of their rotation. But a few observations have been had of spots in the vicinity of the fixed spot of his surface, and here the relative motion westward was distinctly observed. M. Schroeter at Manheim has observed the atmosphere of Jupiter with great care, and finds it exceedingly variable; and spots are observed to change their situations with amazing rapidity, with great irregularity, but most commonly eastward. The motions and changes are so rapid, and so extensive, that we can scarcely consider them as the transference of matter from one place to another. They more resemble the changes which happen in our atmosphere, which are sometimes progressive, over a great tract of the country. The storm in 1772 was felt from Siberia to America in succession. The gale blew from the west, but the chemical operation which produced it was in the opposite direction, being first observed in Siberia; three days afterward, it was felt at St Petersburg; two days after this, at Berlin; two days more, it was in Britain; and seven days after, it was felt in North America. Here then, while a spectator on the Earth saw the clouds moving to the eastward, a spectator in the Moon would see the change of appearance proceed from east to west. The motions in the atmosphere of Jupiter must be very complicated, because they are the joint operation of four satellites. The inequality of gravitation to the
first

first fatellite must be very great. And as each fatellite produces a peculiar tide, the combination of all their actions must be very intricate. We can draw no conclusions from the variable spots, because their change of place is no proof of the actual transference of matter.

Such a relative motion in our atmosphere and in the ocean may affect the rotation, retarding it, by its action on the eastern surface of every obstacle. Yet no change is observed. The year, and the periods of the planets, in the time of Ptolemy are the same with the present, that is, contain the same number of rotations of the Earth. Perhaps a compensation is maintained by this means for the acceleration that should arise from the transference of soil from the high land to the bottom of the sea, where it is moving round the axis with diminished velocity.

726. With this we conclude our account of physical astronomy, a department of natural philosophy which should ever be cherished with peculiar affection by all who think well of human nature. There is none in which the access to well founded knowledge seems so effectually barred against us, and yet there is none in which we have made such unquestionable progress; none in which we have acquired knowledge so uncontrovertibly supported, or so complete. How much therefore are we indebted to the man who laid the magnificent scene open to our view, and who gave us the optics by which we can examine its most extensive, and its most minute parts!

parts! For Newton not only taught us all that we know of the celestial mechanism, but also gave us the mathematics, without which it would have remained unseen.

‘ *Tu Pater et rerum Inventor. Tu patria nobis*

‘ *Suppeditas præcepta, tuisque ex inclyte chartis*

‘ *Floriferis ut apes in saltibus omnia libant,*

‘ *Omnia nos itidem depascimur aurea dicta*

‘ *Aurea, perpetuâ semper dignissima vitâ.*’

LUCRETIUS.

For surely, the lessons are precious by which we are taught a system of doctrine which cannot be shaken, or share that fluctuation which has attached to all other speculations of curious man. But this cannot fail us, because it is nothing but a well ordered narration of facts, presenting the events of nature to us in a way that at once points out their subordination, and most of their relations. While the magnificence of the objects commands respect, and perhaps raises our opinion of the excellence of human reason as high as is justifiable, we should ever keep in mind that Newton's success was owing to the modesty of his procedure. He peremptorily resisted all disposition to speculate beyond the province of human intellect, conscious that all attainable science consisted in carefully ascertaining nature's own laws, and that every attempt to explain an ultimate law of nature by assigning its cause is absurd in itself, against the acknowledged laws of judgement, and will most certainly lead to error. It is only by following his example that we can hope for his success.

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It is surely another great recommendation of this branch of natural philosophy, that it is so simple. One single agent, a force decreasing as the square of the distance increases, is, of itself, adequate to the production of all the movements of the solar system. If the direction of the projection do not pass through the centre of gravity, the body will not only describe an ellipse round the central body, but will also turn round its axis. By this rotation, the body will alter its form. But the same power enables it to assume a new form, which is perfectly symmetrical, and is permanent. This new form, however, in consequence of the universality of gravitation, induces a new motion in the body, by which the position of the axis is slowly changed, and the whole host of heaven appears to the inhabitants of this Earth to change its motions. Lastly, if the revolving planet have a covering of fluid matter, this fluid is thrown into certain regular undulations, which are produced and modified by the same power.

Thus we see that, by following this simple fact of gravitation of every particle of matter to every other particle, through all its complications, we find an explanation of almost every phenomenon of the solar system that has engaged the attention of the philosopher, and that nothing more is needed for the explanation. Till we were put on this track of investigation, these different movements were solitary facts; and, being so extremely unlike, the wit of man would certainly have attempted to explain them by causes equally dissi-

milar. The happy detection of this simple and easily observed principle, by a genius qualified for following it into its various consequences, has freed us from numberless errors, into which we must have continually run while pertinaciously proceeding in an improper path. But this detection has not merely saved us from errors, but, which is most remarkable, it has brought into view many circumstances in the phenomena themselves, many peculiarities of motion, which would never have been observed by us, had we not gotten this monitor, pointing out to us where to look for peculiarities. We should never have been able to predict, with such wonderful precision, the complicated motions of some of the planets, had we not had this key to all the equations by which every deviation from regular elliptical motion is expressed.

On all these accounts, physical astronomy, or the mechanism of the celestial motions, is a beautiful department of science. I do not know any body of doctrine so comprehensive, and yet so exceedingly simple; and this consideration made me the more readily accede to those reasons of scientific propriety which point it out as the first article of a course of mechanical philosophy. Its simplicity makes it easy, and the exquisite agreement with observation makes it a fine example of the truth and competency of our dynamical doctrines.

727. But it has other recommendations, of a far greater value. Nothing surely so much engages a heart possessed of

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of a proper sensibility, as the contemplation of order and harmony. No philosophy is requisite for being susceptible of this impression. We see it influence the conduct of the most uncultivated. What else does man aim at in all the bustle of cultivated society? Nay, even the savage makes some rude aim at order and ornament.

But what we contemplate in the solar system is something more than mere order and symmetry, such as may be observed in a fine specimen of crystallization. The order of the solar system is made up of many palpable *subserviences*, where we see one thing plainly done for the sake of another thing. And, to render this still more interesting, a manifest *utility* appears in every circumstance of the constitution of the system, as far as we understand its applicability to what we conceive to be useful purposes. We can mean nothing by utility but the subserviency to the enjoyments of sentient beings. Our opportunities for observations of this kind are no doubt very limited, confined to our own sublunary habitation. But this circumscribed scene of observation is even crowded with examples of utility. Surely it is unnecessary to recal our attention to the numberless adaptations of the systematic connexion with the Sun and Moon to the continuance and the diffusion of the means of animal life and enjoyment. As our knowledge of the celestial phenomena is enlarged, the probability becomes stronger that other planets are also stored with inhabitants who share with

us the Creator's bounty. Their rotation, and the evident changes that we see going on in their atmospheres, so much resemble what we experience here, that I imagine that no man, who clearly conceives them, can shut out the thought that these planets are inhabited by sentient beings. And there is nothing to forbid us from supposing that there is the same inexhaustible store of subordinate contrivance for their accommodation that we see here for living creatures in every situation, with appropriate forms, desires, and abilities. I fear not to appeal to the heart of every man who has learned so much of the celestial phenomena, even the man who scouts this opinion, whether he does not feel the disposition to entertain it. And I insist on it, that some good reason is required for rejecting it.

728. When beholding all this, it is impossible to prevent the surmise, at least, of purpose, design, and contrivance, from arising in the mind. We may try to shut it out—We may be convinced, that to allege any purpose as an argument for the reality of any disputed fact, is against the rules of good reasoning, and that final causes are improper topics of argument. But we cannot hinder the anatomist, who observes the exquisite adaptation of every circumstance in the eye to the forming and rendering vivid and distinct a picture of external objects, from believing that the eye was made for seeing—or the hand for handling. Neither can we prevent our
heart

heart from suggesting the thought of transcendent wisdom, when we contemplate the exquisite fitness and adjustment which the mechanism of the solar system exhibits in all its parts.

729. Newton was certainly thus affected, when he took a considerate view of all his own discoveries, and perceived the almost eternal order and harmony which results from the simple and unmixed operation of universal gravitation. This single fact produces all this fair order and utility. Newton was a mathematician, and saw that the law of gravitation observed in the system is the only one that can secure the continuance of order. He was a philosopher, and saw that it was a contingent law of gravitation, and might have been otherwise. It therefore appeared to Newton, as it would to any unprejudiced mind, a law of gravitation selected as the most proper, out of many that were equally possible; it appeared to be a choice, the act of a mind, which comprehended the extent of its influence, and intended the advantages of its operation, being prompted by the desire of giving happiness to the works of almighty power.

Impressed with such thoughts, Newton breaks out into the following exclamation. ‘ *Elegantissima hæcce compages Solis Planetarum et Cometarum, non nisi consilio et dominio Entis cujusdam potentis et intelligentis oriri potuit. Hæc omnia regit, non ut anima mundi, sed ut universorum Dominus mundorum. Et propter dominium Dominus Deus,*

‘ *Deus, Παντοκράτωρ, dici solet. Deitas est dominatio Dei, non in corpus proprium, uti sentiunt quibus Deus est anima mundi, sed in servos,* ’ &c.

These were the effusions of an affectionate heart, sympathising with the enjoyment of those who shared with him the advantages of their situation. Yet Newton did not know the full extent of the harmony that he had discovered. He thought that, in the course of ages, things would go into disorder, and need the restoring hand of God. But, as has been already observed (543.), De la Grange has demonstrated that no such disorder will happen. The greatest deviations from the most regular motions will be almost insensible, and they are all periodical, waneing to nothing, and again rising to their small maximum.

730. These are surely pleasing thoughts to a cultivated mind. It is not surprising therefore that men of affectionate hearts should too fondly indulge them, and that they should sometimes be mistaken in their notions of the purposes answered by some of the infinitely varied and complicated phenomena of the universe. And it would be nothing but what we have met with in other paths of speculation, should we see them consider a subserviency to this fancied purpose as an argument that an operation of nature is effected in one way, and not in another. In this way, the employment of final causes has sometimes obstructed the progress of knowledge, and

and has been productive of error. But the impropriety of this kind of argumentation proceeds chiefly from the great chance of our being mistaken with respect to the aim of nature on the occasion. Could this be properly established as a fact, and could the subserviency of a precise mode of accomplishing a particular operation be as clearly made out, I apprehend that, however unwilling the logician may be to admit this as a good reason, he cannot help feeling its great force. That this is true, is plain from the rules of evidence that are admitted in all courts; where a purpose being proved, the subserviency of a certain deed to that purpose is allowed to be evidence that this was the intention in the commission of that deed. It is, however, very rarely indeed that such argument can be used, or that it is wanted, and it never supersedes the investigation of the efficient cause.

731. But speculative men have of late years shewn a wonderful hostility to final causes. Lord Bacon had said, more wittily than justly, that all use of final causes should be banished from philosophy, because, ‘like Vestals, they produce nothing.’ This is not historically true; for much has been discovered by researches conducted *entirely* by notions of final causes. What other evidence have we for all that we know concerning the nature of man? Is not this a part of the book of Nature, and some of its most beautiful pages? We know them only by
the

the appearances of design, that is, by the adaptations of things in evident subserviency to certain results. Are there no such adaptations to be seen, except in the works of man? Nature is crowded with them on every hand, and some of her most important operations have been ascertained by attending to them. Dr Harvey discovered the circulation of the blood in this very way. He saw that the valves in the arteries and veins were constructed precisely like those of a double forcing pump, and that the muscles of the heart were also fitted for an alternate systole and diastole, so corresponding to the structure of those valves, that the whole was fit for performing such an office. With boldness therefore he asserted that the beatings of the heart were the strokes of this pump; and, laying the heart of a living animal open to the view, he had the pleasure of seeing the alternate expansion and contractions of its auricles and ventricles, exactly as he had expected. Here was a discovery, as curious, as great, as important, as universal gravitation. In precisely the same way have all the discoveries in anatomy and physiology been made. A new object is seen. The discoverer immediately examines its structure—why? To see what it can perform; and if he sees a number of coadaptations to a particular purpose, he does not hesitate to say, ‘this is its purpose.’ He has often been mistaken; but the mistakes have been gradually corrected—how? By discovering what is the real structure, and what the thing is really fit for performing. The anatomist

mist never imagines that what he has discovered is of no use. *

732. So far therefore from banishing the consideration of final causes from our discussions, it would look more like philosophy, more like the love of true wisdom, and it would taste less of an idle curiosity, were we to multiply our researches in those departments of nature where final causes are the chief objects of our attention—the structure and œconomy of organised bodies in the animal and vegetable kingdoms. I cannot help remarking, with regret, that of late years, the taste of naturalists has greatly changed, and, in my humble opinion, for the worse. The study of inert matter has supplanted that of animal life. Chemistry and mineralogy are almost the sole objects of attention. Nay, the *ruins* of nature, the shattered relics of a former world, seems a more engaging object than the numberless beauties that now adorn the present surface of our globe. I acknowledge that, even in those inanimate works, God has not left himself without a witness. Yet
surely

* I would earnestly recommend to my young readers some excellent remarks on the argument of final causes (without which Cicero thought that there is no philosophy) in the preface by the editor of Derham's *Physico-Theology*, published at London in 1798. He there considers the proper province of this argument, its use, and incautious abuse, with the greatest perspicuity and judgement.

surely we do not, in the bowels of the Earth, nor even in the curious operations of chemical affinity, see so palpably, or so pleasantly, the incomprehensible wisdom and the providential beneficence of the Father of all, as in the animated objects. *

It is not easy to account for it, and perhaps the explanation would not be very agreeable, why many naturalists so fastidiously avoid such views of nature as tend to lead the mind to the thoughts of its Author. We see them even anxious to weaken every argument for the appearance of design in the construction and operations of nature. One should think, that, on the contrary, such appearances would be most welcome, and that nothing would be more dreary and comfortless than the belief that chance or fate rules all the events of nature.

733. I have been led into these reflections by reading a passage in M. de la Place's beautiful Synopsis of the Newtonian Philosophy, published by him in 1796, under

* A naturalist repeats a saying of his own to the celebrated crystallographer Häüy, 'That, in future, the name of God would be as distinctly written on a crystal as it had hitherto been seen in the heavens.' This seems to me little better than declamation, if it be not irony. Häüy is the discoverer of the necessity of the crystalline forms; and this philosopher thinks himself the discoverer of a similar necessity in the celestial mechanism. (See *Nicholson's Journal*, October 1804, p. 87.)

under the title of *Système du Monde*. In the whole of this work, the author misses no opportunity of lessening the impression that might be made by the peculiar suitability of any circumstance in the constitution of the solar system to render it a scene of habitation and enjoyment to sentient beings, or which might lead the mind to the notion of the system's being contrived for any purpose whatever. He sometimes, on the contrary, endeavours to shew how the alleged purpose may be much better accomplished in some other way. He labours to leave a general impression on the mind, that the whole frame is the necessary result of the primitive and essential properties of matter, and that it could not be any thing but what it is. He indeed concludes, like the illustrious Newton, with a survey of all that has been done and discovered, followed by some reflections, suggested (as he says) by this survey.

'Astronomy,' says M. de la Place, 'in its present state, is unquestionably the most brilliant specimen of the powers of the human understanding.' He does not however tell us how this is so manifest. He does not say that this object, which has engaged, and so properly occupied this fine understanding, has any thing to justify the choice, either on account of its beautiful symmetry, or exquisite contrivance, or multifarious utility; or, in short, that is an object that is worth looking at. But he gives us to understand that astronomy has now taught us how much we were mistaken, in thinking ourselves an important part of the universe, for whose accommo-

dation much has been done, as if we were objects of peculiar care. But we have been punished, says he, for these mistaken notions of self-importance, by the foolish anxieties to which they have given rise, and by the subjugation to which we have submitted, while under the influence of these superstitious terrors. Mistaking our relations to the rest of the universe, social order has been supposed to have other foundations than justice and truth, and an abominable maxim has been admitted, that it was sometimes useful to deceive and to subdue mankind, in order to secure the happiness of society. But nature resumes her rights, and cruel experience has shewn that she will not allow those sacred laws to be broken with impunity.

734. I think it will require some investigation before we can find out what connexion there is between the discoveries of Sir Isaac Newton and this mysterious detection that M. de la Place has at last deduced from the survey. It is communicated in the dark words of an oracle, and we are left to interpret for ourselves. I can affix no meaning but this, that ignorance and self-conceit have made us imagine that this Earth is the centre, and the principal object of the universe, and that all that we see derives its value from its subserviency to this Earth, and to man its chief inhabitant. We fondly imagined that we are the objects of peculiar care,—that it is for us that the magnificent spectacle is displayed,—and that our fortunes are to be read in the stary heavens. But it

is now demonstrated that this Earth, when compared, even with some single objects of our system, is but like a peppercorn. The whole system is but as a point in the universe. How insignificant then are we! But we have been justly punished for our self-conceit, by imagining that the stars influence our fortunes, and have made ourselves the willing dupes of astrologers and soothsayers.

Thus far I think that M. de la Place's words have some meaning, but, surely, very little importance; nor did it call for any congratulatory address to his contemporaries on their emancipation from such fears. It is more than a century since all thoughts of the central situation and great bulk of the Earth, and of the influence of the stars on human affairs, have been exploded and forgotten.

But the remaining part of the remarks, about social order, and truth, and justice, and about deceiving and enslaving mankind, in order to secure their happiness, is more mysterious. 'More is meant than meets the ear.' M. de la Place carefully abstains, through the whole of this performance, from all reference to a Contriver, Creator, or Governor of the universe, particularly in the present reflections, *which are so pointedly contrasted* with the concluding reflections of the great Newton. The opposition is so remarkable, that it startles every reader who has perused the Principia. I cannot but suspect that M. de la Place would here insinuate that the doctrine of a Deity, the Maker and Governor of this World, and of
his

his peculiar attention to the conduct of men, is not consistent with truth; and that the sanctions of religion, which have long been venerated as the great security of society, are as little consistent with justice. The duties which we are said to owe to this Deity, and the terrors of punishment in a future state of existence for the neglect of them, have enabled wicked men to enslave the world, subjecting mankind to an oppressive hierarchy, or to some temporal tyrant. The priesthood has, in all ages and nations, been the great support of the despot's throne. But now, man has resumed his natural rights. The throne and the altar are overturned, and truth and justice are the order of the day.

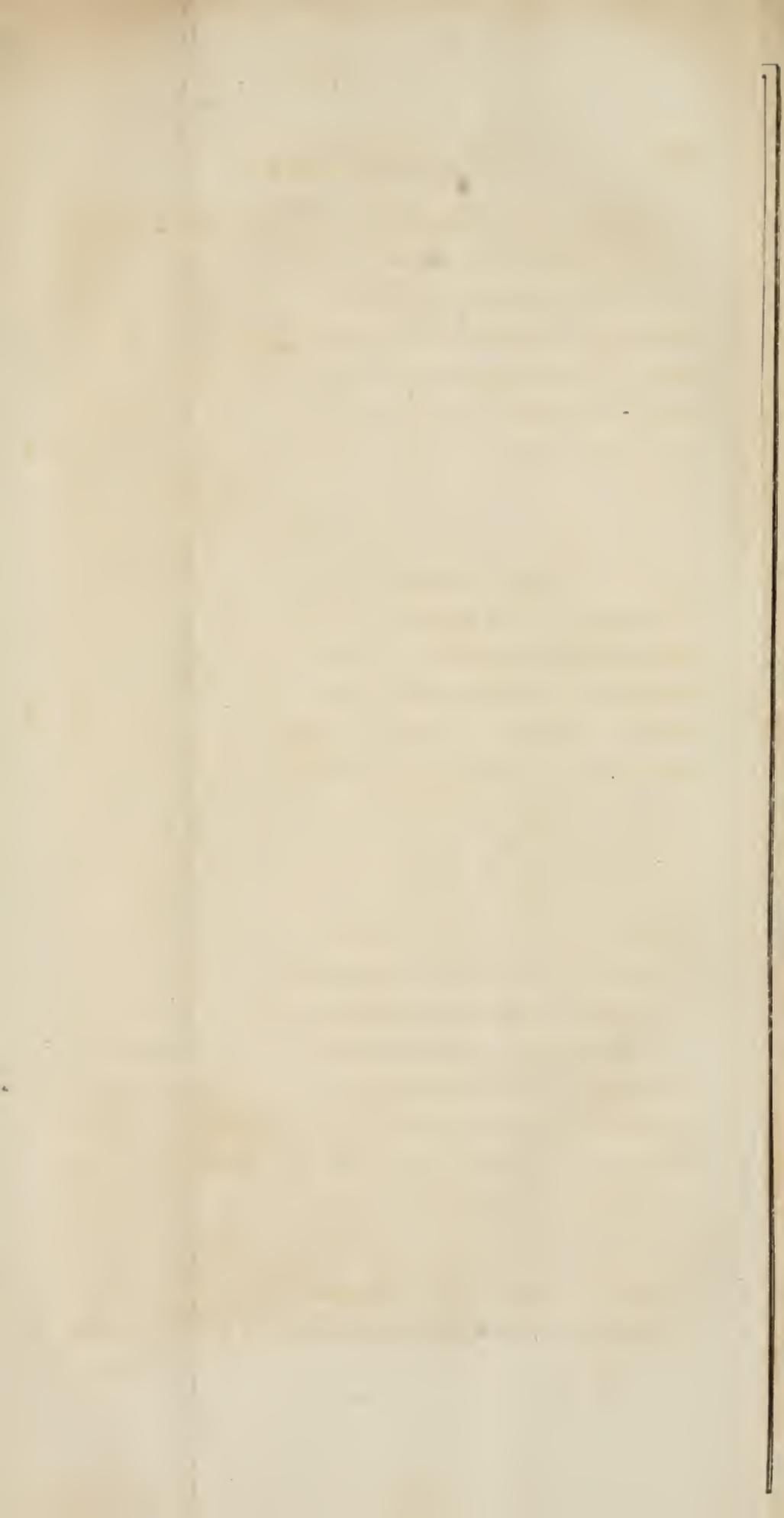
735. This is by no means a groundless interpretation of De la Place's words. He has given abundant proofs of these being his sentiments. It accords completely with his anxious endeavours, on all occasions, to flatten or depress every thing that has the appearance of order, beauty, or subserviency, and to resolve all into the irresistible operation of the essential properties of matter.

736. Of all the marks of purpose and of wise contrivance in the solar system, the most conspicuous is the selection of a gravitation in the inverse duplicate ratio of the distances. Till within these few eventful years, it has been the professed admiration of philosophers of all sects. Even the materialists have not always been on their guard, nor taken care to suppress their wonder at the

the almost eternal duration and order which it secures to the solar system. But M. de la Place annihilates at once all the wisdom of this selection, by saying that this law of gravitation is essential to all qualities that are diffused from a centre. It is the law of action inherent in an atom of matter in virtue of its mere existence. Therefore it is no indication of purpose, or mark of choice, or example of wisdom. It cannot be otherwise. Matter is what it is.

M. de la Place was aware that this assertion, so contrary to a notion long and fondly entertained, would not be admitted without some unwillingness. He therefore gives a demonstration of his proposition. He compares the action of gravity at different distances with the illumination of a surface placed at different distances from the radiant point. Thus, let light, diffused from the point A (fig. 77.) shine through the hole BCDE, which we shall suppose an inch square, and let this light be received on a surface *b c d e* parallel to the hole, and twice as far from A. We know that it will illuminate a surface of four square inches. Therefore, since all the light which covers these four inches came through a hole of one inch, the light in any part of the illuminated surface is four times weaker than in the hole, where it is four times denser. In like manner, the intensity, and efficiency of any quality diffused from A, and operating at twice the distance, must be four times less or weaker; and at thrice the distance it must be nine times weaker, &c. &c.

737. But there is not the least shadow of proof here, nor any similitude, on which an argument may be founded. We have no conception of any degrees or magnitude in the intensity of any such quality as gravitation, attraction, or repulsion, nor any measure of them, except the very effect which we conceive them to produce. At a double distance, gravity will generate one fourth of the velocity in the same time. But this measure of its strength or weakness has no connexion whatever with density, or figured magnitude, on which connexion the whole argument is founded. What can be meant by a double density of gravity? What is this density? It is purely a geometrical notion, and in our endeavour to conceive it with some distinctness, we find our thoughts employed upon a *certain determined number* of lines spreading every way from the radiant point, and passing through the hole B C D E at equal distances among themselves. It is very true that *the number* of those lines which will be intercepted by a given surface at twice the distance will be only one fourth of the number intercepted by the same surface at the simple distance. But I do not see how this can apply to the intensity of a mechanical force, unless we can consider this force as an effect, and can shew the influence of each line in producing the effect which we call the force, and which we consider as the cause of the phenomenon called gravitation. But if we take this view of it, it is no longer an example of his proposition—a force diffused from a centre. For, in order to have the efficiency inversely as the square of the distance,



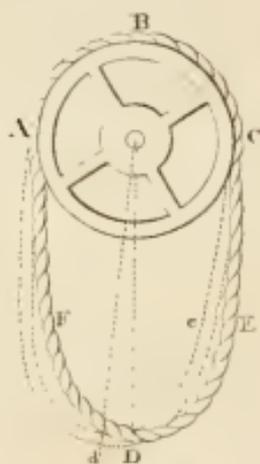
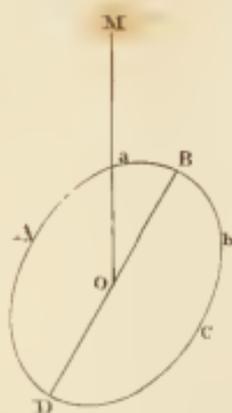
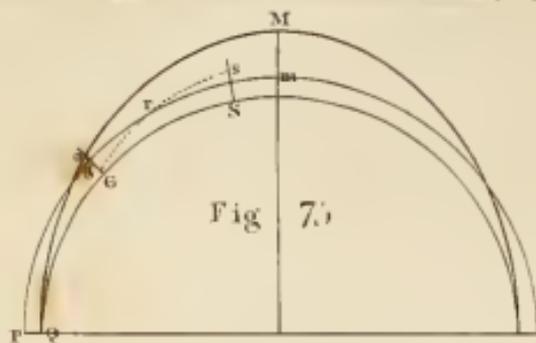
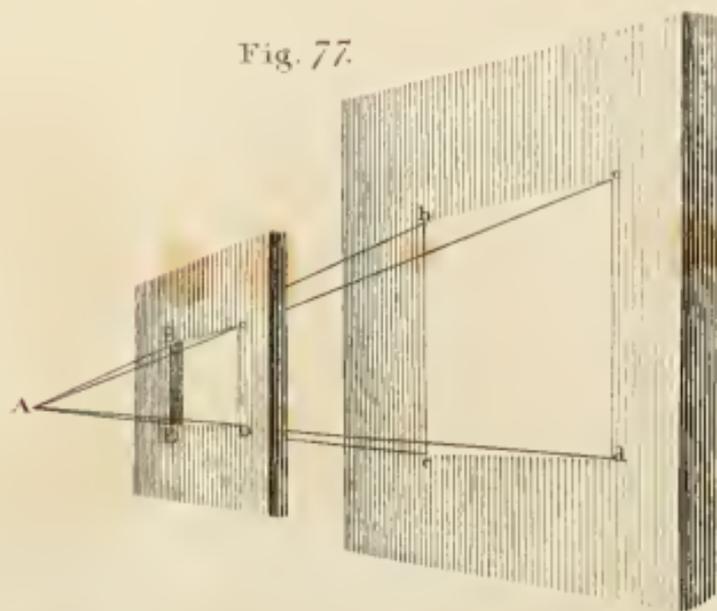


Fig. 77.



distance, it is measured by the number of efficient lines intercepted. Here it is plain that the efficiency of one of those lines is held to be equal at every distance from the centre. Such incongruity is mere nonsense.

This conception of a bundle of lines is the sole foundation for any argument in the present case. La Place indeed tries to avoid this by a different way of expressing his example. A certain quantity of light, says he, goes through the hole. This is uniformly spread over four times the surface, and must be four times thinner spread. But this, besides employing a gratuitous notion of light, which may be refused, involves the same notion of *discrete* numerical quantity. If light be not conceived to consist of atoms, there can be no difference of density; and if we consider gravity in this way, we get into the hypothesis of mechanical impulsion, and are no longer considering gravity as a primordial force or quality.

738. But this pretended demonstration is still more deficient in metaphysical accuracy. The proposition to be demonstrated is, that the gravitation towards an atom of matter is in the inverse duplicate ratio of the distance, *in whatever point of space the gravitating atom is placed.* But, if we take our proof of the ratio from the conception of these lines, and their density, we at once admit that there are an infinity of situations in which there is no gravitation at all, namely, in the intervals of these lines. The number of situations in which the atom gra-

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vitates is a mere nothing in comparison with those in which it does not. We must either suppose that both the quality and the surface influenced by it are continuous, uninterrupted,—or both must be conceived as *discrete* numerical quantities, the quality operating along a *certain number* of lines, and the surface consisting of a *certain number* of points. We must take one of these views. But neither of them gives us any conception of a different energy at different distances. If the surface be *continuous*, and the quality *every where* operative, there can be no difference of effect, unless we at once admit that the energy itself changes with the distance. But this change can have no relation to a change of density, a thing altogether inconceivable in a continuous substance;—where every place is full, there can be no more. On the other hand, if the quality be exerted only along certain lines, and the surface only contain a certain number of points, we can find no ground for establishing any proportion.

739. The simple and true state of the question is this. Suppose only two indivisible atoms, or two mathematical points of such atoms, in the universe. If these atoms be supposed to attract each other, *wherever they are placed*, do we perceive any thing in our conception of this force that can enable us to say that the attraction is equal or unequal, at different distances? For my own part, I know nothing. The gravitation, and its law of action, are mere phenomena, like the thing which

I call matter. This is equally unknown to me. I merely observe certain relations, which have hitherto been constant, and I am led by the constitution of my mind to expect the continuation of these relations. My collection of such observations is my knowledge of its nature. This gravitation is one of them, and this is all that I know about it.

740. The observed relations may be such that they involve certain consequences. This, in particular, has consequences that cannot be disputed. If gravitation in the ratio of $\frac{1}{x^2}$ be the primordial relation of all matter, and the source of all others (which is a part of La Place's system), it is impossible that a particle composed of such atoms can act with a force which decreases more rapidly by an increase of distance. But there are many phenomena which indicate a much more rapid decrease of force. Simple cohesion of solid bodies is one of these. The expansion of some exploding compositions shew the same thing. We may add, that no composition of such atoms can form repelling particles, nor give rise to many expansive fluids, or indeed to any of the ordinary phenomena of elastic bodies. But these things are not immediately before us, and we shall have another and a better opportunity of considering many things connected with this great question.

741. De la Place is not the first person who has attempted a demonstration of this proposition. Dr Da-

vid Gregory, in his valuable work on astronomy, has done the same thing, and nearly in the same way with La Place. Leibnitz, in that strange letter to the editors of the Leipzig Review, in which he answers some of Gregory's objections to his own theory of the celestial motions, mentions an Italian professor who gave the same argument, and affected to consider this ratio of planetary force as known to him before Newton's discovery. Leibnitz thinks the argument a very good one, because, mathematically speaking, it is the same thing whether the rays be illuminative or attractive. If this be not nonsense, I do not know what is.—Several compilers of elements employ the same argument. But nothing can be less to the purpose. Nothing can be more illogical than to speak of demonstrating any primordial quality. Newton was surely more interested in this question than any other person, and we may be certain that if he could have supported his discovery of this law of gravitation by any argument from higher principles, he most certainly would have done it. But there is no trace of any attempt of the kind among his writings; doubtless because he saw the folly of the attempt.

742. I trust that the reader will forgive me for taking up so much of his time with this question. It seems to me of primary importance. Charged as I am with the instruction of youth—the future hopes of our country—it is my bounden duty to guard their minds from every thing that I think hazardous. This is the more incumbent

incumbent on me, when I see natural philosophy calumniated, and accused of lending her support to doctrines which are the abhorrence of all the wise and good. I cannot better discharge this duty than by wiping off this stain, with which careless ignorance, or atheistical perversion, has disfigured the fair features of philosophy. I was grieved when I first saw M. de la Place, after having so beautifully epitomised the philosophy of Sir Isaac Newton, conclude his performance with such a marked and ungraceful parody on the closing reflections of our illustrious master; and, as I warmly recommend this epitome to my pupils, it became the more necessary to take notice of the reprehensible peculiarities which occur in different parts of the work; and particularly of this proposition, from which the materialists seem to entertain such hopes. Nor am I yet done with it. A demonstration has been recently offered, in a work which professes to explain *the intimate constitution of matter*, and to account for *all* the phenomena of the universe. This will come in my way when we shall be employed in considering the force of cohesion. Till then, *requiescat in pace*.

It is somewhat amusing to remark how the authority of Sir Isaac Newton has been eagerly caught at by the atheistical sophists to support their abject doctrines. While some hankering remained in France for the Atomistic philosophy, and there was any chance of bewildering the imaginations, and misleading the understandings, of such as wished to acquire a confident faith in the reveries of Democritus and Epicurus, M. Diderot worked into a
better

better shape the slovenly performance of Robinet, the *Système de la Nature*, and affected to deduce all his vibrations and vibratiuncles from the elastic æther of Sir Isaac Newton, dressing up the scheme with mathematical theorems and corollaries. And thus, Newton, one of the most pious of mankind, was set at the head of the atheistical sect.

But this mode, having had its day, is now passed, and is become obsolete—the tide has completely turned, and the æther is no longer wanted. But the sect would not quit their hold of Sir Isaac Newton. The doctrine of universal fate is now founded on Newton's great discovery of gravitation in the inverse duplicate ratio of the distances. It is still called the discovery of the illustrious Englishman, and is passed from hand to hand with all the authority of his name.

743. But surely to us, the scholars of Newton, the futility of this attempt is abundantly manifest. As the worthy pupils of our accomplished teacher, we will join with him in considering universal gravitation as a noble proof of the existence and superintendance of a SUPREME MIND, and a conspicuous mark of ITS transcendent wisdom. The discovery of this relation between the particles of that matter of which the solar system consists is acknowledged, even by the materialists, to have set Newton at the head of philosophers. They must therefore grant that it has something in it of peculiar excellence. Indeed whoever is able to follow the steps of Newton

over

over the magnificent scene, must be affected as he was, and must pronounce ‘all very good.’ M. de la Place seems to think the less of man on account of the smallness of his habitation. Is ABBA THULE, King of Pelew, a less noble creature than M. de la Place’s CORSICAN MASTER? Or, does the smallness of this globe shew that little has been done for man?—It is peculiarly deserving of remark, that we see many contrivances in this system, which are of manifest subserviency to the enjoyments of man, and which do not appear to have any farther importance. Man is unquestionably the lord of this lower world, and all things are placed under his feet. But we see nothing to which man is exclusively subservient—nothing that is superior to man in excellence, so far as we can judge of what is excellent—nothing but that wisdom, that power, and that beneficence, which seem to indicate and to characterise the Author and Conductor of the whole;—and, I may add, that it is not one of our smallest obligations to the Author of Nature, that He has given us those powers of mind which enable us to perceive and to be delighted with the sight of this bright emanation of all his perfections.

- ‘ *Sanctius his animal, mentisque capacius alta,*
- ‘ *Finxit in effigiem moderantem cuncta Decorum,*
- ‘ *Pronaque cum spectent animalia cætera terram,*
- ‘ *Os homini sublime dedit, cælumque tueri*
- ‘ *Jussit, et erectos ad sidera tollere vultus.’*

OVID.

Allow

Allow me to conclude in the words of Dr Halley.

- ‘ *Talia monstrantem mecum celebrate Camænis,*
- ‘ *Vos, ó cœlicolám gaudentes neçtare vesçi,*
- ‘ *NEWTONUM, clausi referantem scrinia Veri,*
- ‘ *NEWTONUM, Musis charum, cui pectore puro*
- ‘ *Phœbus adest, totoque incessit Numine mentem,*
- ‘ *Nec fas est propiùs mortali attingere divos.’*

HALLEY.

END OF VOLUME FIRST.

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