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ELEMENTS

OF

MECHANICAL PHILOSOPHY,

BEING THE SUBSTANCE OF

A COURSE OF LECTURES

ON THAT

SCIENCE.

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THE following pages contain the fubftance of a Courfe of Lectures, which have been read by me during the annual feffions of the Colleges, ever fince the year 1774. Any perfon, well acquainted with Natural Philofophy muft be fenfible that, in the fhort fpace of a fix months feffion, juftice cannot be done to the various branches of this extenfive fcience. I found that I muft either treat in a loofe manner fubjects which require and admit of ftrict reafoning, or muft omit fome articles ufually taught in this clafs; and I was induced to prefer the latter method, becaufe I was of opinion that a loofer manner of proceeding is neither fuitable to the Inftitution in this Univerfity, nor calculated to convey ufeful knowledge. In one feffion I omitted the confideration of Magnetifm and Electricity, and in the next feffion thefe were treated of, and Optics was omitted.

a But

336

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 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 Boſcovich.
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.

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.

EDINBURGH, *October* 31. 1804.

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

Page.	*Line.*		
6	12	*c'*	C'
	13	*c'*	C'
9	8	AFB*a*F*b*	AFB, *a*F*b*
35	17	LKIH	LIH
	18	KICB, KHDB	LICB,LHDB
	23	KC	IC
47	11	*dg*	*cg*
	12	*v'*	*v̇*
	20	AP	AC
53	14	to *m*	from E to *m*
	15	*o*	*m*
63	14	ADV	ADO
64	17	AK	BK
73	23	ultimate	alternate
75	2	*deflecting forces*	*deflections*
79	12	parallel	particle
82	12	divefting	dividing
149	4	S	C
153	28	equal,	equally
188	11	ACI	AEI
189	20	AEM	ACM
190	12	MI	M*i*
231	1	( )	(345)
239	19	caculation	calculation
326	10	*p n*	*p v n*
332	17	to	of
384	9	confidering	confider
385	27	retreats	rerreat
394	26	*pollente*	*pellente*
395	9	are	is
398	28	MP	MR
418	4	clotted	dotted
449	12	PI	GI
	13	PF—PG	PH—PI
	16	PF'—PG'	PH'—PI'
452	26	Fig 64.	Fig. 64. N° 1.
458	28	after P	(Fig.64.N°2.)
471	18	L *e* Q *q*	E *e* Q *q*
476	19	BDF	PDF
	Note.	Boſſuet	Boſſut
591	26	S	*s*
592	3	Coſ.² 2 *x*	Coſ 2 *x*
647	19	corroſponds	correſpond

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 Inſert 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*) *a b* is the product of two numbers, or the rectangle of two lines, *a* and *b*.

(*d*) $a \doteqdot b$ 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) $x \doteqdot \frac{1}{y}$ 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) $x \doteqdot \frac{y}{z}$ 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 A D and A F (fig. A) be two right lines diverging from A, and let B C, B c, B D, be ſucceſſive values of x, and the parallel ordinates C E, $c\,e$, D F be correſponding values of y. Draw E G and $e\,g$ parallel to A D, and conſequently equal to C D and $c\,D$, then C D and G F are correſponding differences of the ſucceſſive

ceſſive values of x and y. So are c D and g F. Now it is plain that C D : G F = c D : g F, 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 E e F (fig. B) be an arch of a curve, ſuch as a hyperbola, of which A is the centre, we know that C D has not the ſame ratio to G F that c D has to g F, 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 C D to G F, or c D to g F is leſs than that of the ſubtangent T D to the ordinate D F. But when c' gets below D, the ratio of E′ G′, or c' D, to G′ F is greater than that of T D to D F; 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 T D to D F as c approaches to D from either ſide. For this reaſon, the ratio of T D to D F 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 T D : D F is called the *prime* ratio of the *naſcent* magnitudes x' and y'.

We know alſo that the ratio of the ſubtangent $t\,c$ to the ordinate $c\,e$ is leſs than that of T D to D F, and that the ratio of the ſubtangent to the ordinate increaſes continually, as D is taken further from the vertex V of the

the hyperbola. But we know alſo that it never is ſo great as the ratio of A D to Df (the ordinate produced to the aſſymptote) but approaches nearer to it than by any difference that can be aſſigned. For this reaſon, A D : Df has been called the *ultimate* ratio of the ſubtangent and ordinate—in the ſame manner, the ultimate ratio of D F 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. T D : D F 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 $\dot{x} : \dot{y}$. 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

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 $\dot{x}:\dot{z} = TD:TF$, and $\dot{y}:\dot{z} = DF:TF$.

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, $= \dot{x}:\dot{z}$, 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 BFG (fig. C) be an ellipſe, whoſe centre is C, and focus D. Let Fe be a very ſmall arch of the curve. Draw DF and De, and about the pole D, with the diſtance De, deſcribe the circular arch Eeg, 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 $\dot{x}:\dot{z} = FD:FT$, and $\dot{x}:\dot{y} = FD:DT$. 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 A B and *a b* (fig. D) round the same pole F, from which are drawn two right lines F A, F B, cutting both lines in A, *a*, B, and *b*. Let F B, by revolving round F, continually approach to F A. Let it come, for example, into the situation F *c* C very near to F A *a*. Let S and *s* represent the mixtilineal spaces A F B *a* F *b*. Then S′ and *s*′ may express the spaces A F C and *a* F *c*. It is plain that the limiting ratio of A F C to *a* F *c* 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 F H K and *f h k* (fig. E) be two curves, having the abscissæ A E and *a e*. Let these abscissæ be divided into an equal number of small equal parts, such as A B, B C, D E and *a b*, *b c*, *d e*; and let ordinates be drawn through the points of division. And on these ordinates, as bases, let parallelograms, such as A B L F, B C N G, &c. and *a b l f*, *b c n g*, &c. be inscribed, and others, such as A B G M, A C H O, and *a b g m*, *a c h o*, &c. be circumscribed.—It is affirmed, 1*st*, that if the subdivision be carried on without end, the mixtilineal areas A E K F and *a e k f* 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 A E K F to the space *a e k f* 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*.

1*ſt*, Make D S and *d s* equal to A F and *a f*, and draw S R, *s r*, parallel to A E, *a e*. It is evident that the parallelogram S R K Q is equal to the exceſs of all the circumſcribed over all the inſcribed parallelograms. Therefore, by continuing the ſubdiviſion of A E 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 A E to *a e*, and that of A F to *a f*, or are as A E × A F to *a e* × *a f*. This is evident. For, by the ſuppoſition, $CN : cn = AF : af$. And, ſince the number of parallelograms is the ſame in both figures, B C and *b c* are ſimilar parts of A E and *a e*; that is, $BC : bc = AE : ae$. Therefore $BCNG : bcng = AE \times AF : ae \times af$.

$\times af$. Since this is true of every correfponding pair of parallelograms, it is true of their fums, and of the mixtilineal fpaces A E K F and *a e k f*, which are ultimately equal to thofe fums.

(*u*) It may be thought that in thefe cafes where the limiting ratio is not an ultimate ratio actually attained, there remains fome fmall error. The foreign mathematicians feem to acquiefce in this, and content themfelves with affuming *d x* or *d y* as infinitely fmall; inferring from thence that the remaining error is infinitely fmall, fo that it will not amount to a fenfible quantity, though multiplied by any number, however great. But this conceffion leads them *neceffarily* into the fuppofition of quantities infinitely fmaller than quantities already affumed as infinitely fmall; a fuppofition plainly abfurd or unintelligible. But no error whatever lurks in this method of limiting ratios. For it is all founded on the following unqueftionable axiom.

(*v*) If the ratio of *a* to *b* be greater than *any ratio whatever* that is lefs than the ratio of *c* to *d*, but lefs 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 lefs than $m:n$. If $m:n$ be lefs than $c:d$, then $a:b$ is greater than $m:n$, both which confequences are contrary to the conditions affumed. Therefore $a:b$ *muft* be $c:d$.

The propoſition (*s*) may be demonſtrated in this way. The ſpace A 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 A 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, A

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

(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. MAN 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 increafe man's power over nature.

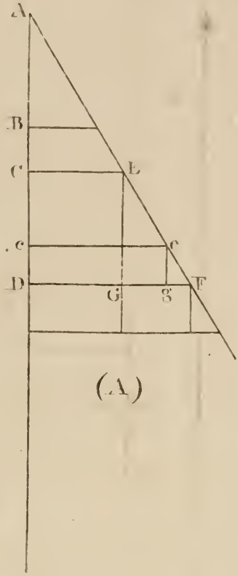
This definition of the ftudy points out Motion, with all its affections and varieties, as the objects of our firft attention, a knowledge of thefe being indifpenfably neceffary for perceiving and appreciating its changes, from which alone we are to derive all our knowledge of their caufes, the mechanical powers of nature.

OF MOTION.

5. In motion we obferve the *fucceffive* appearance of the thing moved in *different* parts of fpace. 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 fmall clafs of quantities of which we have the cleareft and moft diftinct conceptions. We conceive them as magnitudes made up of their own diftinguifhable parts, and meafurable by one of thefe as a unit. We cannot conceive fo clearly of heat, or preffure, or many other things which are magnitudes, capable of increafe and diminution, but not diftinguifhable into feparate parts.

7. In our fimpleft conception of fpace, it is mere extenfion; we think of nothing but a diftance between two places. This is the moft ufual conception of it in mechanical



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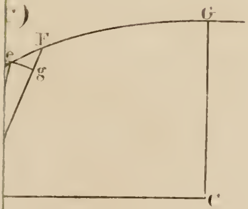

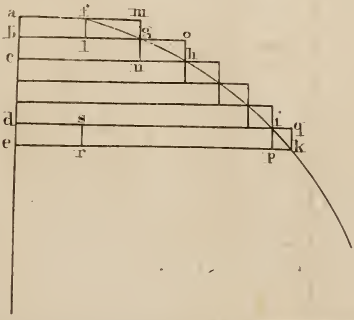

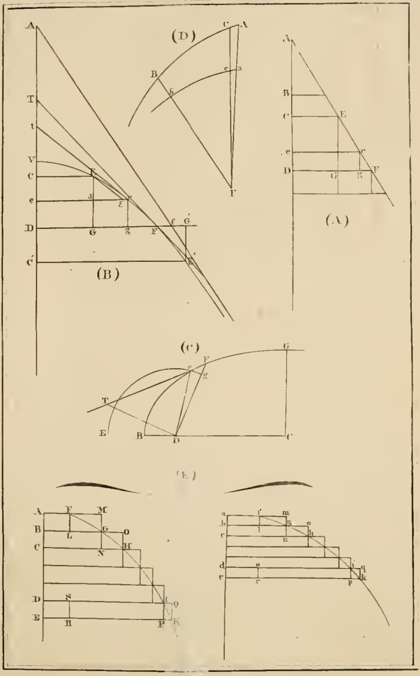
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mechanical difquifitions—the path along which a thing moves; and we fay, figuratively, that the thing *defcribes* this path.

But the geometer confiders fpace as having not only length, but alfo breadth, and he then calls it a *furface;* and, in order to have a complete notion of the capacioufnefs of a portion of fpace, he confiders not only its length and breadth, but alfo its thicknefs—and fuch fpace he calls a *folid fpace.* But, by folid, he means nothing but the fufceptibility of meafure in three ways. He calls it extenfion of three dimenfions.

But, in pure mechanics, we feldom have occafion to confider more than one dimenfion of fpace.—In our inveftigations, however, we make ufe of geometrical reafonings, which include both furfaces and folids—but our reafoning always terminates in a mechanical theorem, of which diftance alone is the fubject.

8. The adjoining parts or portions of fpace are diftinguifhed or feparated from one another by their mutual boundaries. Contiguous portions of a line are feparated by points—contiguous portions of a furface are feparated by lines—and contiguous portions of a folid are feparated by furfaces.

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

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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.

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

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; becaufe a pendulum $39\frac{1}{8}$ inches long makes three vibrations in the firft cafe, and one in the laft.

17. There is an analogy between the affections of fpace and time fo obvious, that, in moft languages, the fame words are ufed to exprefs the affections of both.—Hence it is that time may be reprefented by lines, and meafured by motion; for uniform motion is the fimpleft fucceffion of events that can be conceived.

18. All things are placed in fpace, in the order of fituation.—All events happen in time, in the order of fucceffion.

19. No motion can be conceived as inftantaneous. For, fince a moveable, in paffing from the beginning to the end of its path, paffes through the intermediate points; to fuppofe the motion along the moft minute portion of the path inftantaneous, is to fuppofe the moveable in every intervening point at the fame inftant.—This is inconceivable, or abfurd.

20. Absolute Motion is the change of abfolute place. Relative Motion is the change of fituation among other objects. Thefe 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 ſuccefsful 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 other-

wiſe.

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

26. In rectilineal 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 A B, B C (fig. 2.) in ſucceſſion, the direction is changed in the point B. From B *c*, the prolongation of A B, it is changed to B C.

This change may be called DEFLECTION; and this deflection may be meaſured, either by the angle *c* B C, or by a line *c* C 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 A D B E C (fig. 3.) the direction is *continually* changing. The direction in the point B is that of the tangent B T, that direction alone lying between *any* pair of polygonal directions, ſuch as B C and B *c*, or B D and B E, however near we take the points A and C, or D and E, to the point B.

29. A curvilineal motion ſuppoſes the deviation and deflection

deflection to be continual, and a continual deflection constitutes a curvilineal 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 rectilineal; 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 de-* "*ſcribed 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—ſandglaſs—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 veloci-* "*ties.*"

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-

 cauſe

caufe the rectangles A C, D F (fig. 4.) are in this cafe 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 fpaces, directly, and as the velocities, inverfely.*

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 fpaces, directly, and as the times, inverfely.*

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 abfolute magnitudes of the fpace and time do not change the values of the refults of thefe propofitions, provided both are changed in the fame ratio. The value of $\frac{20 \text{ feet}}{40''}$, or of $\frac{6 \text{ feet}}{12''}$, is the fame with $\frac{1}{2}$ of a foot per fecond. Therefore, if s' be taken to exprefs an extremely minute portion of fpace defcribed with this velocity in the minute portion of time

t',

t', we ftill have the velocity v accurately expreffed by $\frac{s'}{t'}$. Alfo $\frac{s'}{v}$ is the accurate expreffion of the time t'.

-2. *Of Variable Motions.*

42. It rarely happens that the phenomena of nature prefent to our obfervation motions perfectly uniform. Yet we diftinctly conceive them, with all their properties; and the deviations from thefe are the only marks and meafures of the variations, and, therefore, of the changing caufes. Therefore it is plain, that it is of the firft importance that all thefe deviations be thoroughly underftood.

43. If a body continue to move uniformly in the fame direction, its motion, or condition in refpect to motion, is unchanged. Its condition, therefore, muft be allowed to be the fame in any two portions of its path, however diftant they may be. The difference of place does not imply any change, becaufe a change of place is involved in the very conception of motion. If, therefore, two bodies be moving with the fame velocity in this path, or in two lines parallel to it, their condition in refpect of motion muft be allowed to be the fame. They have the fame direction, and move at the fame rate. No circumftance, therefore, feems to enter into our conception of the ftate of a body, in refpect of motion, except its velocity and its direction. Changes in one

or both of thefe circumftances conftitute all the changes of which this condition is fufceptible. We fhall firft confider changes of velocity.

Of Accelerated and Retarded Motions.

44. Every one is fenfible that a falling ftone is carried downward with greater rapidity in every fucceffive moment of its fall. During the firft fecond 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 fo on: falling, during every fecond, 32 feet more than during the preceding.

Such a motion is, with propriety, called an ACCELERATED MOTION. On the contrary, an arrow fhot perpendicularly upward is obferved to rife with a motion continually RETARDED. Thefe bodies are therefore conceived to be in *different* ftates of motion in every fucceeding inftant. The velocity of the falling body is conceived to be greater in a certain inftant than in any preceding inftant. Mechanicians fay 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 comparifon of the fpaces defcribed in the following moments; for in thefe, it falls 112 and 48 feet: nor from the fpaces defcribed in the moments immediately preceding; for in thefe, the body fell 80 and 16 feet. The affertion however fuppofes that this variable condition, called Velocity, is fuf-

ceptible

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.

45. Let the ſtraight line A B D (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 *a b d* repreſent the time of this motion along the path A D, 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 *a b*, *b c*, *c d*, may expreſs the times of deſcribing A B, B C, C D, that is, may be in the proportion of thoſe times. Moreover, let *a e*, perpendicular to *a d*, expreſs the velocity of the moveable at the inſtant *a*, or in the point A.

Let *e g h* be a line, ſo related to the axis *a d*, that the areas *a b f e*, *b c g f*, *c d h g*, comprehended between the ordinates *a e*, *b f*, *c g*, *d h*, all perpendicular to *a d*, may be proportional to the ſpaces A B, B C, C D, deſcribed in the times *a b*, *b c*, *c d*, 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 *a e* to *b f*, or *c g*, or *d h*, &c. In other words,

If the abſciſſa a d *of a curve* e g h *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 *b c* and *c d* equal, ſo as to repreſent very ſmall and equal moments of time, and make *p a* equal to one of them, and complete the rectancle *p a e q*. This will repreſent the ſpace uniformly deſcribed in the moment

p a,

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 h* 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 $c\,k$.

Therefore the velocity in A is to that in C in a leſs ratio than that of $a\,e$ to $c\,m$, but in a greater ratio than that of $a\,e$ to $c\,k$. But, in this example, as long as the inſtant b is prior and d poſterior, to the inſtant c, $c\,m$ is leſs, and $c\,k$ is greater, than $c\,g$. Therefore the velocity in A is to that in C in a ratio that is greater than any ratio leſs than that of $a\,e$ to $c\,g$, but leſs than any ratio greater than that of $a\,e$ to $c\,g$. And, conſequently, the velocity in A is to that in C as $a\,e$ to $c\,g$. (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 B C D been retarded.

46. *Cor.* 1. *The velocities in different points of the path* A D *are in the ultimate ratio of the ſpaces deſcribed in equal ſmall moments of time.* For, drawing $g\,o$ parallel to $a\,d$, the velocity in the inſtant a is to that in the inſtant c as $a\,e$ to $c\,g$, that is, as the rectangle $p\,e$ to the rectangle $c\,o$, that is, as $p\,a\,e\,q$ to $c\,d\,h\,g$ very nearly. As the moments are diminiſhed, the difference $g\,o\,h$ between the rectangle $c\,g\,o\,d$ and $c\,g\,h\,d$, 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 $c\,d$, the error $g\,o\,h$ is re-

duced

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 P A to B C 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 B C and C D for the meaſure of the velocity in the point C; or, which is the ſame thing, if we take B C for the meaſure of the velocity in the middle of the moment *b c*. For the ſpaces B C and C D 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 P A, C D, 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 $\dot{s} \doteqdot v \dot{t}$.

49. Consequently $v \doteqdot \frac{\dot{s}}{\dot{t}}$, and $\dot{t} \doteqdot \frac{\dot{s}}{v}$. The equation $\dot{s} \doteqdot v \dot{t}$, $v \doteqdot \frac{\dot{s}}{\dot{t}}$, and $\dot{t} \doteqdot \frac{\dot{s}}{v}$ 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 a 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 $\dot{s} \doteqdot v \dot{t}$ 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 $abfe$, $adhe$, they must be proportional to some other areas $abf'e$, $adh'e$, of another line $ef'h'$, passing through e. But, if so, then, by art. 45, the velocity in A is to that in B as ae to bf'. But the velocity in A was stated to that in B as ae to bf. Therefore $ae : bf = ae : bf'$, which is absurd. Therefore, &c.

51. The only immediate observation that we can make on these variable motions is the relation between

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 A D (fig. 6.) with a motion any how continually changed, and let the curve L K I H be ſo related to A D that the area K I C B is to the area K H D B as the time of moving along B C to that of moving along B D; and let this be true in every point of the line A D. Let C c, D d be two very ſmall ſpaces deſcribed in equal times, draw the ordinates *i c*, *h d*, and draw *i k*, *h l* perpendicular to K C, H D.

It is evident that the areas I C *c i* and H D *d h* are equal, becauſe they repreſent equal moments of time. It is alſo plain that as the ſpaces C *c* and D *d* are continually diminiſhed, the ratio of I C *c i* and H D *d h* to the rectangles *k* C *c i* and *l* D *d h* continually approaches to that of equality, and that the ratio of equality is the limiting or

 ultimate

ultimate ratio. Therefore, ſince the areas I C *c i* and H D *d h* are equal, the rectangles *k* C *c i* and *l* D *d h* are ultimately in the ratio of equality. Therefore their baſes *i c* and *h d* are inverſely as their altitudes C *c* and D *d*, that is, $ic : hd = Dd : Cc$. But C *c* and D *d* being deſcribed in equal times, are ultimately as the velocities in *c* and *d* (46). Therefore *i c* and *h d* are inverſely as the velocities in *c* and *d*. Becauſe this may be ſimilarly demonſtrated in reſpect of every point of the abſciſſa, the propoſition is demonſtrated.

52. It now appears that in all caſes in which we can diſcover by obſervation the relation between the ſpaces deſcribed and the times elapſed during the deſcription, we diſcover the velocities and the mechanical condition of the moveable. To make any practical application of our concluſions, we muſt always have recourſe to arithmetical calculations. Theſe are indicated by the algebraic ſymbols of our geometrical reaſonings. We repreſent any ordinate *c g* of fig. 5. by v, and the portion *c d* of the abſciſſa by $\dot{t}$, and the area *c d h g*, or rather, its equal, the rectangle *c d o g*, by $v\dot{t}$. And ſince this rectangle is as the correſponding portion C D of the line of motion, and C D is repreſented by $\dot{s}$, we have the equation $\dot{s} = v\dot{t}$.

We may now aſſume as true, all the mathematical conſequences of theſe repreſentations. Therefore $\dot{t} = \frac{\dot{s}}{v}$, as in art. 41. For the algebraic ſymbols are the repreſentations of arithmetical operations, and they repreſent the

the operations of geometry more remotely, and only becaufe the area of a rectangle is analogous to the product of numbers which are proportioned to its fides. If we ufe the fymbol $\int v\,\dot{t}$ to reprefent the fum of all thefe rectangles, it will exprefs the whole area $adhe$, and will alfo exprefs the whole line of motion A D, and we may ftate the equation $s = \int v\,\dot{t}$. In like manner $\int \frac{\dot{s}}{v}$ will be equivalent to $\int \dot{t}$, that is, to t, and will exprefs the whole time ad. It is alfo eafy to fee that $\frac{\dot{s}}{v}$ reprefents the ordinate D H of the line L K I H of fig. 6, becaufe any portion D d of its abfciffa is properly reprefented by $\dot{s}$, and the ordinates are reciprocally proportional to the velocities, that is, are proportional to the quotients of fome conftant number divided by the velocities, and therefore, to $\frac{1}{v}$. Now $\dot{t}$ being reprefented by the rectangle k C $c\,i$, which is alfo reprefented by $\dot{s} \times \frac{1}{v}$, we have $\dot{t} = \frac{\dot{s}}{v}$, and $t = \int \frac{\dot{s}}{v}$, as before.

Such fymbolical reprefentations will frequently be employed in our future difcuffions, and will enable us greatly to fhorten our manner of proceeding.

53. There is one cafe of varied motion, which has very particular and ufeful characters, namely, when the line $e\,f\,g\,h$ of fig. 5. is a ftraight line. Let fig. 7. reprefent this cafe of motion along the line A D, and let $p\,a$, $b\,c$, $c\,d$ reprefent equal moments of time, in which

the

the moveable deſcribes P A, B C, C D; draw *f m*, *g n*, *e s* parallel to the abſciſs *a d*.

It is evident that *m g* and *n h* are equal, or that equal increments of velocity are acquired in equal times. Alſo *e q*, *e r*, *e s* are proportional to *q f*, *r g*, *s h*, and therefore the increments *q f*, *r g*, *s h*, of velocity, are proportional to the times *a b*, *a c*, *a d*, 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 *e h* cut the abſciſſa in ſome point *v*, it will repreſent a motion uniformly accelerated from reſt, during the time *v d*, 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 *a e*, *b f*, *c g*, *d h*, repreſent the velocities acquired during the times *v a*, *v b*, *v c*, *v d*, 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

reprefented 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 oftenfible character of an uniformly accelerated motion. For all that we can immediately obferve in a motion, is a fpace defcribed, and a time elapfed. Velocity is not an obfervation, but the name of an obferved relation between the increafe of the fpace and that of the time. The fpace defcribed in the time $v\,b$ is obferved to be to that in the time $v\,d$, as $v\,b^2$ to $v\,d^2$. We can reprefent the proportion of $v\,b^2$ and $v\,d^2$ by the triangles $v\,b\,f$ and $v\,d\,h$, which have the fame proportion. We then fee that the points v, f, h are in a ftraight line, and therefore $b\,f$ and $d\,h$ are as $v\,b$ and $v\,d$, that is, when we obferve a motion fuch that the fpaces defcribed are proportional to the fquares 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. Alfo, *the increments of the fpaces are as the increments of the fquares 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. Alfo, *the fpaces defcribed from the beginning of the motion are as the fquares 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 $b\,c\,g\,f : c\,d\,h\,g = c\,g^2 - b\,f^2 : d\,h^2 - c\,g^2$ &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 $v\,d\,h$ meaſures the ſpace deſcribed in the time $v\,d$ by the accelerated motion, and the rectangle $v\,d\,h\,H$ meaſures the ſpace uniformly deſcribed in the time $v\,d$ with the velocity $d\,h$.

Here it is to be remarked, that $c\,g\,h\,d$ is only half of the difference between the rectangles $v\,d\,h\,H$ and $v\,c\,g\,G$. If we make $d\,h = v\,d$, then $v\,d\,h\,H$ and $v\,c\,g\,G$ will be the ſquares of the velocities $d\,h$ and $c\,g$. In this caſe, $n\,h$, the increment of velocity, is alſo equal to $g\,n$, and $d\,n \times n\,h$ is $= c\,g \times n\,h$. Employing v and $\dot{v}$ to expreſs velocity and its momentary increment, $v\,\dot{v}$ will be the expreſſion of the rectangle $c\,g \times n\,h$. Now $2\,v\,\dot{v}$ 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 $2\,v\,\dot{v}$, 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

be defcribed in the fame time with the mean between the velocities at the beginning and end of this portion of time. For $b\,d\,h\,f = b\,d \times c\,g$.

Thefe properties of uniformly accelerated motion will be found of very great fervice in the inveftigation of all other varied motions, particularly in cafes where an approximation is all that can be effected without very tedious and complicated proceffes.

61. Acceleration may be confidered as a meafureable quantity. A ftone falling in the vertical line, much fooner acquires a great velocity, than when rolling down a flope, and all are fenfible that the acceleration is lefs as the declivity is more gentle.

If we fuppofe the acceleration to be always the fame, the conception that we have of this conftancy is, furely, that in equal times equal increments of velocity are acquired; and, confequently, that the augmentations of velocity are proportional to the times of acquiring them. This being fuppofed, that acceleration muft furely 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, muft be taken for the meafure of the acceleration.

62. *Cor.* Therefore *accelerations are proportional to the fpaces defcribed in equal times with motions uniformly accelerated from a ftate of reft*, (in which the velocities gradually increafe from nothing). For, in this cafe the fpaces are the halves of what would be uniformly defcribed 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 $a \doteqdot \frac{v'}{t'}$.

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 $v \doteqdot a\,t$,

and $v' \doteqdot a\,t'$.

65. Since all that we can obſerve in a motion is a ſpace deſcribed, and a time elapſed during the deſcription, it is deſireable 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 $\frac{s}{t^2}$.

 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 $a \doteqdot \frac{v}{t}$ (64) we have $a \doteqdot \frac{v\,v}{v\,t}$; but $v\,t \doteqdot s$, therefore $a \doteqdot \frac{v^2}{s}$. 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 $a \doteqdot v^2$; 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 v^2 increaſes in the proportion, both of the acceleration and of the ſpace. Therefore we have, in general, $v^2 \doteqdot a\,s$.

Again (as in art. 64, 65) we ſhall have $v^2 \doteqdot a\,S$, and $V^2 - v^2 \doteqdot a\,S - a\,s$, or $\doteqdot S - s$, which we may expreſs in this manner $\overline{v\,v'} \doteqdot a\,s'$. 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 meafures of acceleration, even when continually varying.

We may obtain this in the very fame way that we get meafures of a velocity which varies continually. We can conceive a line to increafe along with our velocity, and to increafe precifely at the fame rate. It is evident that this rate of increafe of the velocity is the very thing that we call Acceleration, juft as the rate at which the line now mentioned increafes is the very thing that we call Velocity. We have only therefore to confider the areas of fig. 5. or the line A D of that figure, as reprefenting a velocity; then it is plain that the ordinates to the line *e g h*, which we demonftrated to be proportional to the rate of variation of this area, will reprefent, or be proportional to the variation of this velocity, that is, to the acceleration. Hence the following propofition.

69. *If the abfciffa* a d *of a curve line* e g h *reprefent the time of a motion, and if tho areas* a b f e, a c g e, a d h e, &c. *are proportioned to the velocities at the inftants* b, c, d, &c. *then the ordinates* a e, b f, c g, d h, &c. *are proportional to the accelerations at the inftants* a, b, c, d, &c.

This is demonftrated precifely in the fame manner as in art. 45. and we need not repeat the procefs. We have only to fubftitute the word *acceleration* for the word *velocity*.

From this propofition, we may deduce fome corollaries which are of continual ufe in every mechanical difcuffion.

70.

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

For, the increment of velocity in the moment $c\,d$ (for example) is accurately reprefented by the area $c\,d\,h\,g$, or by the rectancle $c\,d\,n\,k$; and $c\,d$ accurately reprefents the moment. Alfo, the ultimate ratio of $c\,k$ to fuch another ordinate $b\,i$, is the ratio of $c\,g$ to $b\,f$ (45); that is, the ratio of the acceleration in the inftant c to the acceleration in the inftant b. Therefore the increment of velocity during the moment $p\,a$ is to that during the moment $c\,d$ as $p\,a \times a\,e$ to $c\,d \times d\,g$.—We may exprefs this by the proportional equation $\dot{v} \doteq a\,\dot{t}$.

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

When, from the circumftances of the cafe, we can meafure the area of this figure, as it is analogous to the fum of all the infcribed rectangles, we may exprefs it by $\int a\,\dot{t}$; and thus we obtain the whole velocity acquired during the time A P, and we fay $v \doteq \int a\,\dot{t}$.

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

72. *If the abfciffa* A E *of a line* a c c (fig. 8.) *be the fpace along which a body is moving with a motion continually varied,*

varied, and if the ordinates A *a*, B *b*, C *c*, &c. *be proportional to the accelerations in the points* A, B, C, &c. *then, the areas* A B *b a*, A D *d a*, A E *e a*, &c. *are proportional to the augmentations of the ſquare of the velocity in* A *at the points* B, D, E, &c.

Let B C, C D, be two very ſmall portions of the line A E, and draw *b f*, *c g*, parallel to A E. Then, if we ſuppoſe that the acceleration B *b* continues through the ſpace B C, the rectangle B *b f* C will expreſs the augmentation made on the ſquare of the velocity in B (67). In like manner, C *c g* D 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 A E is carried on without end, it is evident that the ultimate ratio of the area A E *e a* to the aggregate of inſcribed rectangles, is that of equality; that is, when the acceleration varies, not by ſtarts, but continually, the area A B *b a* will expreſs the augmentation made on the ſquare of the initial velocity in A, during the motion along A B. The ſame muſt be affirmed of the motion along B E.—Therefore the intercepted areas A B *b a*, B D *d b*, D E *e d*, are proportional

Fig. 1.

Fig. 2.

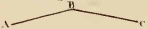


Fig. 3.

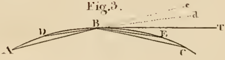


Fig. 6.

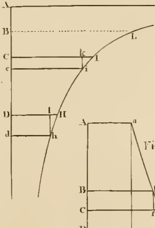


Fig. 5

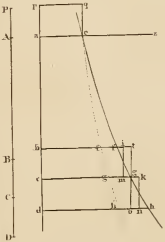


Fig. 8.

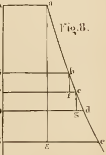


Fig. 4.

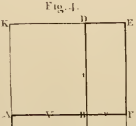
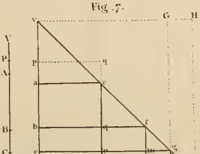


Fig. 7.



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 A B *b a*, A D *d a*, &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 A E *e a*, 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 unequably; 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 defcribed in the fame time with the initial velocity undiminifhed, &c. &c. &c.

Having now obtained proper marks and meafures of all variations of velocity, it remains to obtain the fame for all changes of direction. Thus we fhall obtain a knowledge of the greateft part of thofe motions which the fpontaneous phenomena of nature exhibit to our view. It is very doubtful whether we have ever feen a motion ftrictly rectilineal.

3. *Of Compound Motions.*

79. In our endeavours to obtain a general mark or characteriftic of any change of motion, it is evident that when the change is fuppofed to be the fame in any two or more inftances, the oftenfible marks muft be the fame, whatever have been the previous conditions of the two moveables. There muft be obferved, in all the cafes of change, fome circumftance in the difference between the former motions and the new motions, which is precifely the fame, both in refpect of kind and of quantity, that is, in refpect of direction and of velocity. We may therefore fuppofe one of the bodies to have been previoufly at reft. In this cafe, the whole change produced on it is unqueftionably the very motion which we fee it acquire, or the determination to this motion.

Therefore, in the firft place, *a change of motion is, itfelf, a motion, or determination to motion.* In the cafe now mentioned, it is the new motion, and that only.

But

But it is by no means the new motion in every other cafe. For, if the *previous* condition of the body has been different from that of a body at reft, and if the *fame* change has been produced on it, the *new* condition muft alfo be different from the new condition of the other, and therefore the *new condition* cannot be the *change*, becaufe this is fuppofed to be the fame in both cafes. But, farther, when the fame change is made in any previous motion, we muft fee, in the difference between the former motion and the new motion, fomething that is equivalent to, or the fame with, this motion produced in the body that was previoufly at reft, and which has received the fame change. And alfo, the difference between the new motions of thefe two bodies muft be fuch as fhall indicate the difference between thefe previous conditions of each.

Affuming therefore as a principle, that the change of motion is itfelf a motion, let us endeavour to find out a motion, which alone fhall produce that difference from the former motion which is really obferved in the new motion, in all cafes whatever. This, undoubtedly, is the proper mark and meafure of the change.

Something very analogous to thefe indifpenfable conditions may be obferved in the following motions. Suppofe the ftraight line E I (fig. 9.) lying eaft and weft, croffed by the line E K from north to fouth. Let the line E K (which we fuppofe to be material, fuch as a rod or wire) be carried along the line E I in a minute, keeping always parallel to its firft pofition, that is, always lying north and ſouth. At the end of 20″ it will have the poſition G g γ, its end E having moved uniformly along $\frac{1}{3}$ of E I; at the end of 40″ it will have the poſition H h χ, E having deſcribed $\frac{2}{3}$ of E I; and at the end of the minute it will have the poſition I i.

In the mean time, let the line E I (alſo ſuppoſed material) move uniformly from north to ſouth, keeping always parallel to its firſt poſition E I. At the end of 20″ it will have the poſition $m\,g\,n$, its end E having moved along $\frac{1}{3}$ of E K. At the end of 40″ it has the poſition $o\,h\,p$, and E o is $\frac{2}{3}$ of E K. 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 E i of the parallelogram E K i I; for E m g G is a parallelogram ſimilar to E K i I, becauſe E G : E I = E m : E K. In like manner E o h H is a parallelogram ſimilar to E K i I. Theſe parallelograms are therefore about a common diameter E i.

Further, the motion of the point of interſection of theſe lines is uniform; becauſe E G : E I = E g : E i, and E H : E I = E h : E i, &c.; and therefore the ſpaces E g, E h, E i 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 γ is, at the inſtant, moving eaſtward, and the ſame point *g* of the line *m g 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 previoufly at reft in E, and we obferve it defcribe E K in a minute, then the motion E K is, unqueftionably, the change which it has fuftained. The motion E *i* is not the change; for had E L been the primitive motion, the fame motion E *i* would have refulted from compounding the motion E M with it. Now, fince E L is different from E I, it is impoffible that the *fame* change can make the new conditions the fame.

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

And laftly, the motion E K is the only circumftance of famenefs between changing the motion E I into the motion E *i*, and giving the motion E K to a body previoufly at reft.

After a mature confideration of all thefe conditions, we may fay, that

A change of motion is that motion which, by compofition with the former ftate of motion, produces the new motion.

80. This compofition of motion is ufually prefented to the mind in a way fomewhat different. A body is fuppofed to move uniformly in the direction E I, while the fpace in which this motion is performed is carried uniformly in the direction E K. But we *cannot conceive* a portion of fpace to be moved out of its place. We can conceive the compofition very diftinctly by fuppofing a man walking along a line E I drawn on a field of ice, while the ice is floating in the direction E K. This will produce the very motion E *i*, and affords the cleareft notion

notion of the compofition. If one man ftands ftill, 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 firft man will be the motion E K, that of the fecond will be E *i*, and that of the third will be E Q. There can be no doubt of thefe three men having fuftained the very fame change of motion. Now, the only circumftance of famenefs in thefe three new conditions is the compofition of their former condition with a motion E K.

The reflecting reader will perceive, however, that this way of illuftrating the fubject, by the motion on moving ice, is not precifely a compofition of two *determinations to motion*. This is completed in the firft inftant. As foon 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 defcribed. But it ferves very well to exhibit to the mind the *mathematical* compofition of *two motions*, which is all we want at prefent. We have fhewn that, in the refult of this combination, all the characteriftics of the two determinations are to be found, becaufe the point of interfection, whether we confider it as a material exiftence, or as a mere mathematical conception, partakes of both motions. There is a phyfical queftion which will come under confideration afterwards, that is very different from the prefent, namely, Whether two natural powers, which are known to be productive, feparately,

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 E I, E K, *of a parallelogram, compoſe a uniform motion in the diagonal.* This is already demonſtrated. For the motion of the point of interſection of thefe two lines, while each moves uniformly in all its points, in the direction of the other, is, in every inftant, compofed of thefe two motions, and is the fame as if a point defcribed E I uniformly, while E I is uniformly carried in the direction E K. And this motion is along the diagonal E *i*, and is uniform, as has been already fhewn. Alfo, becaufe E I and E *i* are defcribed in the fame time, the velocities of the motions along E I, E K, and E *i*, are proportional to thofe lines.

84. *Cor.* 1. The COMPOUND MOTION E *i* is in the fame plane with the two CONSTITUENT or SIMPLE MOTIONS E I and E K. For a parallelogram lies all in one plane.

85. *Cor.* 2. The motion E *i* may arife from the compofition of any two uniform motions, which have the direction and velocities reprefented by the fides of any parallelogram E L *i* M, or E I *i* K, of which E *i* is the diagonal.

Cafes frequently occur, where we know the directions of the two fimple motions which compofe an obferved motion, but do not know the proportion of their velocities. The velocity is afcertained by this propofition, becaufe the direction of the three motions, viz. the two fimple and the compound motions, determines the fpecies of parallelogram, and the ratio of the fides.

Sometimes we have the direction and the velocity of one of the fimple motions, and therefore its proportion to that of the obferved compound motion. The direction and velocity of the other is alfo found by this propofition, becaufe thefe data alfo determine the parallelogram.

The motion in the diagonal is evidently equivalent to the motions in the fides combined. Thus, if the moveable firft defcribe E I, and then I *i* (or E K), it will be in the fame point *i* as if it had defcribed E *i*. Therefore E *i* is frequently called the EQUIVALENT motion, the RESULTING motion.

It frequently gives great affiftance in our inveftigations, if we fubftitute for an obferved motion fuch motions as will produce it by compofition. This is called the RESOLUTION OF MOTIONS. It is in this way that the navigator generally computes the fhip's change of fituation at the end of a day, in which fhe has perhaps failed in many different courfes. He confiders how much he has gone to the eaftward, or weftward, and how much to the northward or fouthward, on each courfe; and he then adds together all his eaftings, and all his fouthings, and then fuppofes that the fhip has failed for the whole day on that unvaried courfe which would be produced by the fame eafting and fouthing combined.

In like manner, it is very ufeful for the mechanician to confider how much his obferved motion has advanced the body in fome particular direction, E F, for example (fig. 10). To do this, he confiders the motion A B as compofed of a motion A C parallel to the given line E F, and another motion A D perpendicular to E F, A B forming the diagonal of a parallelogram A C B D, of which one ſide A C is parallel, and the other A D is perpendicular to E F. It is plain that the motion A D neither promotes nor obſtructs the progreſs in the direction E F, and that the body has advanced in the direction of E F, 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 A B (fig. 11.) in a given plane E F G H, when he conſiders it as compoſed of a motion A D perpendicular to that plane, and A C parallel to it. The lines D A, B C being drawn perpendicular to the plane, cut it in two points *a* and *b*, and A C is parallel to *a b*.

86. Any number of motions A B, A C, A D, A E (fig. 12.) may be thus compounded, forming a motion A F. The method for aſcertaining the motion reſulting from this compoſition is as follows. A B, compounded with A C, produces the motion A G. This, compounded with A D, produces A H; and this, compounded with A E, produces A F.

The ſame final ſituation F will be found by ſuppoſing all the motions A B, A C, A D, A E, to be performed in ſucceſſion. Thus the moveable deſcribes A B; then B G, equal and parallel to A C; then G H, equal and parallel to A D; and then, H F, equal and parallel to A E.

NOTE.

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

87. Three motions A B, A C, A D (fig. 13.) which have the direction and proportions of the ſides of a parallelopiped, compoſe a motion in the diagonal A F of that parallelopiped; for A B and A C compoſe A E, and A E and A D compoſe A F.

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 A B, A C and A D of a rectangular box.

This is now the conſtant procedure of the mechanician, 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 mechanician refers all motions to three *co-ordinate* lines A B, A C, A D, which are perpendicular to each other, and his ultimate reſult is the diagonal A F of ſome parallelopiped.

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

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

Thus, a uniform motion in the direction A B (fig. 14.) may be compounded with a uniformly accelerated motion in the direction A C. Such a motion is obferved when we fee a ftone fall from the maft-head of a fhip failing fteadily forward in the direction A B; for this ftone will be obferved to fall down parallel to a plummet hung from the maft-head. The real motion of the ftone will therefore be a parabolic arch A *b f g*, which A B touches in A; for while the maft-head defcribes the equal lines A B, B F, F G, the ftone has fallen to β and φ and γ, and the line A C A′ has got into the pofitions B B′, F F′, G G′, fo that A ϕ is four times A β; and A γ is nine times A β. Therefore A β, A φ, A γ, are as the fquares of $\beta\, b$, $\varphi\, f$, and $\gamma\, g$, and the line A *b f g* is a parabola.

It is in this way that a nail in the fole of a cart-wheel defcribes a cycloid, while the cart moves along a fmooth plane. This is the compofition of a progreffive motion with an equal circular motion. The geometrical lectures of Dr Barrow contain many beautiful examples of fuch compofitions of motion; and it was by introducing this procefs into mathematical reafoning, that this celebrated geometer gave a new department to the fcience, which quickly extended it far beyond the pale of the ancient geometry of the Greeks, and fuggefted to Sir Ifaac Newton his doctrine of Fluxions.

89.

89. When two motions, however variable, are compounded, we difcover the direction and velocity of the compound motions in any inftant, if we know the direction and velocities of each of the fimple motions *at that inftant*. For we may fuppofe, that, at that inftant, each motion proceeds unchanged. Then we conftruct a parallelogram, the fides of which have the directions and proportions of the velocities of the fimple motions. The diagonal of this parallelogram will exprefs 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 fimple motions, we difcover their velocities.

91. When a curvilineal motion A D V (fig. 15.) refults from the compofition of two motions, whofe directions we know to be A C and A F, we learn the velocities of the three motions in any point D, by drawing the tangent D I, and the ordinate D *b* parallel to one of the fimple motions, and from any point L in that ordinate, drawing L I parallel to the other motion, cutting the tangent in I. The three velocities are in the proportion of the three lines I L, L D, and I D. This is of very frequent ufe.

Since the phenomena are our only marks and meafures of their fuppofed caufes, it is plain that every miftake with refpect to a change of motion, is accompanied by a miftake in our inference of its caufe. Such mif-

takes

takes are avoided with great difficulty, becaufe the motions which we obferve are, at all times, extremely different from what we take them to be. A book lying on the table feems to be at reft; but it is really moving with a prodigious fpeed, and is defcribing a figure very like the figure defcribed by a nail in the nave of a coach-wheel while the carriage is going over the fummit of a gentle rifing. We imagine that we are at reft, and we judge of the motion of another body merely by its change of diftance and direction from ourfelves.

Thus, if a fhip is becalmed at B (fig. 16.) in a part of the ocean where there is an unknown current in the direction B D; and if the light of another fhip is feen at A, and if A really fails to C while B floats to D, A will not appear to have failed along A C, but along A K; for when B is at D, and A at C, A appears at C, having the bearing and diftance D C. Therefore, if A K be made equal and parallel to D C, it will have the fame bearing by the compafs, and the fame diftance from B that C has from D; and therefore the fpectator in B, not knowing that he has moved from B to D, but believing himfelf ftill at B, muft form this opinion of the motion of A.—In the fame manner it muft follow, that our notions of the planetary motions muft be extremely different from the motions themfelves, if it be true that this earth is moving to the eaftward at the rate of nearly twenty miles in every fecond. It would feem a defperate attempt therefore for us to fpeculate concerning the powers of nature by which thefe 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 A B, and draw A E equal and parallel to B D, and complete the parallelogram A C F E, and join E D and D C. Alſo produce E A till A L is equal to A E or B D, and complete the parallelogram L A C K, and draw A K and B K. Had A moved along A E while B moves along B D, 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 D C are the ſame with B K, and that if the ſpectator in B imagines himſelf at reſt, the line A K will be conſidered as the motion of A. This is evidently compoſed of the motion A C, which is the real motion of A, and the motion A L, which is equal and oppoſite to the motion B D.

93. In like manner, if B H be drawn equal and oppoſite to A C, and the parallelogram B H G D be com-

 pleted,

pleted, and B G and A G be drawn, the diagonal B G will be the motion of B relative to A. (92.) Now, it is plain that K A G B 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 B G, which is equal and oppoſite to A K. 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 B D to be changed into B δ. This has ariſen from a compoſition of the motion B D with another D δ; draw C κ equal and oppoſite to D δ, and complete the parallelogram E C κ ε. The diagonal E κ is the apparent or relative change of motion. For the bearing and diſtance δ C is evidently the ſame with D κ, becauſe the lines δ C and D κ 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 ε is the apparent *change* of motion of the body A, becauſe E C was the apparent motion before the change into E $\varkappa$. Complete the parallelogram A C $\varkappa$ α. The diagonal A $\varkappa$ would have been the motion of A, had its motion A C ſuſtained the compoſition or change A α. It is plain that either the motion D δ, compounded with B D, or the motion A α 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 E A is equal and oppoſite to the known motion B D. It muſt therefore be A C.

97. In like manner, if any other circumſtances have aſſured the ſpectator in B, that A C is the true motion of A, which had appeared to him to move along A K, he muſt conſider A K as the diagonal of a parallelogram A L K C, and then he learns that B has moved over a line B D, equal and oppoſite to A L. 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 curvilineal motion is a caſe of continual deflection. It is ſuſceptible of infinite varieties, and its modifications and chief properties are of difficult inveſtigation.

The

The ſimpleſt caſe of curvilineal motion is that of uniform motion in a circular arch. Here, the deflections in equal times from rectilineal 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 rectilineal motion A B (fig. 17.) is deflected into another B C, 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 *d* D, or *c* C, in this manner, becauſe theſe lines, being proportional to B *d*, B *c*, 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 curvilineal motion. We never have $d\text{D} : c\text{C} = \text{B}d : \text{B}c$; moreover, it is very rarely that *d* D, *c* C, &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

We may learn from theſe general remarks, that the directions and the meaſures of the deflections by which a body deviates *continually* into a curvilineal 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* A B C D E F (fig. 18.) *which is all in one plane, and if there be a point* S *in this plane, ſo ſituated, that the lines* S A, S B, S C, &c. *drawn to the curve, cut off areas* A S B, A S C, A S D, &c. *proportional to the times of deſcribing the arches* A B, A C, A D, &c. *then are the deflections always directed to this point* S.

Let us firſt ſuppoſe that the body deſcribes the polygon A B C D E F, formed of the chords of this curve, and that it deſcribes each chord uniformly, and is de-

flected

flected 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 A S B, B S C, C S D, &c. are all equal.

Continue the chords A B, B C, &c. beyond the arches, making B *c* equal to A B, and C *d* equal to B C, and ſo on. Join *c* C, *d* D, &c. and draw *c* S, *d* S, &c.; alſo draw C *b* parallel to *c* B or B A, cutting B S in *b*, and join *b* A, and draw C A, cutting B *b* in *o*. Laſtly, make a ſimilar conſtruction at E.

Then, becauſe *c* B is equal to B A, the triangles A S B and B S *c*, are equal, and therefore B S *c* is equal to B S C; but they are on the ſame baſe S B. Therefore they are between the ſame parallels; that is, *c* C is parallel to B S, and B C is the diagonal of a parallelogram B *b* C *c*. The motion B C therefore is compounded of the motions B *c* and B *b*, and B *b* is the deflection, by which the motion B *c* is changed into the motion B C; therefore the deflection in B is directed to S.—By ſimilar reaſoning *f* F, or E *i*, 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 curvilineal area, and its ſides with the curvilineal 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* A D F *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 *α*, F *δ*, they may be conſidered as perpendiculars on S A and S E; alſo with the diſtance S F deſcribe the arch *g h*. It is evident that

Fig. 10.

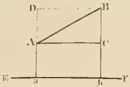

Fig. 12.

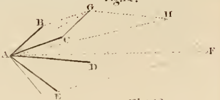

Fig. 13.

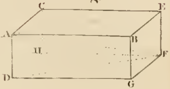

Fig. 9.

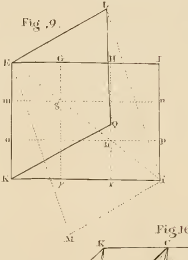

Fig. 14.

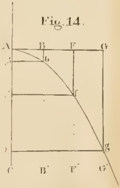

Fig. 16.

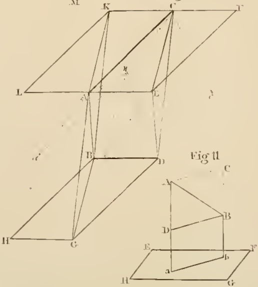

Fig. 15.

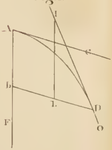

Fig. 11.

that $g\,h$ 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 $g\,h : B\,\alpha = S\,E : S\,A$

therefore $g\,h : F\,\delta = S\,E^2 : S\,A^2$

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 B C and E F. 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 $B\,b : C\,A = B\,C : A\,Z$.

that $g\,h$ 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 $B\,\alpha : F\,\delta = SE : SA$.

But $g\,h : B\,\alpha = SE : SA$

therefore $g\,h : F\,\delta = SE^2 : SA^2$

and $ASB : ESF = SE^2 : SA^2$

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 B C and E F. 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.

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K Now,

Now, ſince, by continually diminiſhing the ſides of the polygon, the points A and C continually approach to B, and C A continually approaches to c A or to 2 c B, or 2 C B, and is ultimately equal to it; alſo A Z is ultimately equal to B Z. Therefore, ultimately, B b : 2 B C = B C : B Z, and B b × B Z = 2 B C², and B $b = \frac{2 BC^2}{BZ}$.

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

Therefore B b : E i = $\frac{2 BC^2}{BZ} : \frac{2 EF^2}{Ez}$.

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 B Z of this circle, which has the direction of the deflection in B, may be called its DEFLECTIVE CHORD.

Since B C and E F 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 curvilineal 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 curvilineal motions, in which the deflections are always directed to one fixed point, but extends to all

all curvilineal motions whatever. For it may evidently be expreffed in this manner; *The deflecting forces are ultimately proportional to the fquares of the arches defcribed in equal times, directly, and to the deflective chords of the equicurve circle, inverfely.*

The equable defcription of areas only enabled us to fee that the lines B C and E F were defcribed in equal times, and therefore are as the velocities.

It will be convenient to have a fymbolical expreffion of this theorem. Therefore, let the deflective chord of the equicurve circle be reprefented by c, and the deflection by d, the theorem may be expreffed by

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

105. REMARK.—The line B b is the linear deflection, by which the uniform motion in the chord A B is changed into a uniform motion in the chord B C, or it is the deviation c C from the point where the moveable would have arrived, had it not been deflected at B. But, in the prefent cafe of curvilineal motion, the lines B b and B c exprefs the meafures of the velocities of thefe motions, or the meafures of the determinations to them. B c is to B b as the velocity of the progreffive motion is to the velocity of the deflection, generated during the defcription of the arch B C. But, becaufe the deflection in the arch has been continual, and becaufe it is to be meafured, 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 B C. 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 B *b*, or it will be B O. Now, this is really the caſe ; for the tangent is ultimately parallel to O C, and biſects *c* C ; 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 curvilineal 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 curvilineal 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 curvilineal

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 curvilineal 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 B C.

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 $x = \frac{1}{4} BZ$.

RECAPITU-

RECAPITULATION.

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

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

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

The momentary variation of the ſquare of velocity $2v\dot{v} = 2a\dot{s}$ (72)

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

The deflective velocity $= \frac{2v^2}{c}$ (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 Q R 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 P*p* of P from this plane, is the average of all the diſtances of each particle. For, let the plane A P B paſs through this point, parallel to the plane Q R. The diſtance C S of a parallel C from this plane is D S — D C, or P*p* — D C; and the diſtance G T of a particle G is H T + G H, or P*p* + G H. Let *n* be the number of particles between Q R and A P; and let *o* be the number on the other ſide of A P; 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 C S is $n \times Pp$ *minus* the ſum of all the diſtances, ſuch as C D. Alſo $o \times Pp$, *plus* the ſum of the diſtances G H, is the ſum of all the diſtances G T. Now, the ſum of the lines C D is equal to that of all the lines G H, and therefore $\overline{n + o} \times Pp$, or $m \times Pp$, is equal to the ſum of all the lines C S and G T, and P*p* is the m^{th} part of this ſum, or the average diſtance.

Now, ſuppoſe the body to have approached to the plane Q R (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 A P B 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 A B 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

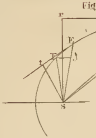
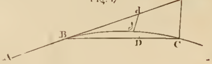


Fig. 17



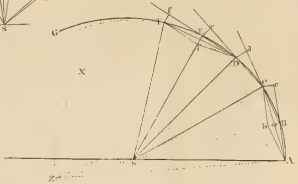


Fig. 18.

Fig. 21.

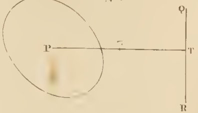
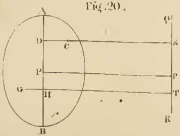


Fig. 20.



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

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 C f to F f as the matter in F is to that in the remaining bodies. It is plain that E f is parallel to D F, and that D F : 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 rectilineal. Therefore, the motion compounded of them all is uniform and rectilineal.

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

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

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.

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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 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 rectilineal.

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 CORPOREAL 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 precifion the properties which we felect as diftinctive of matter from all other things.

119. All men feem agreed in calling that alone matter, which excludes all other fubftances of the fame kind, or prevents them from occupying the fame place, and which requires the exertion of what we call force to remove it from its place, or anyhow change its motion. Thefe 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 fituation. When mobility is afcribed to matter, as a diftinguifhing quality, we always conceive force to be required. We are confcious of exerting force in moving even our own limbs. In like manner, extenfion, and figure, and divifibility, although primary qualities of matter, are common to it with empty fpace.

120. Mobility in confequence of the exertion of force may be ufed as a characteriftic of matter, or of an atom of matter. All poffefs it—and probably all poffefs it alike, their fenfible differences being the confequence 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 confideration of the changes of mo-

tion

tion which refult from its condition as thus defcribed may be called the MECHANISM of the phenomenon.

122. Perhaps all changes of material nature are cafes of local motion (though unperceived by us) by the influence of moving forces. Perhaps they cannot be faid to be *completely* underftood, till it can be fhewn how the atoms of matter have changed their fituations. Perhaps the folution of a bit of filver in aqua fortis is not *completely* explained, till we fhew, as the mechanician can fhew with refpect to the fatellites of Jupiter, how an individual atom of filver is made to quit its connexion with the reft, and by what path, and with what velocity in every inftant of its motion, it gets to its final ftate of reft, in a diftant part of the veffel. But thefe motions are not confidered by the judicious chemift. He confiders the phenomenon as fully explained, when he has difcovered all the cafes in which the folution takes place, and has defcribed, with accurate fidelity, all the circumftances of the operation.

123. We have derived our notions of SOLIDITY or IMPENETRABILITY chiefly from our fenfe of touch. The fenfations got in this way feem to have induced all men to afcribe this property of tangible matter to the mutual contact of the particles—and to fuppofe that no diftance is interpofed between them.

124. But the compreffibility and elafticity of all known bodies, their contraction by cold, and many ex-

amples

amples of chemical union, in which the ingredients occupy lefs room when mixed, than one of them did before mixture, feem incompatible with this conftitution of tangible matter. Did air confift of particles, elaftic in the fame manner that blown bladders are, it would not be fluid when compreffed into half of its ufual bulk, becaufe, in this cafe, each fpherule would be compreffed into a cube, touching the adjoining fix particles in the whole of its furfaces. No liquid, in a ftate of fenfible compreffion, could be fluid; yet the water at the bottom of the deepeft fea is as fluid as at the furface. Some optical phenomena alfo fhew incontrovertibly that very ftrong preffure may be exerted by two bodies in *phyfical* or *fenfible* contact, although a meafurable diftance is ftill interpofed between them. On the whole, it feems more probable that the ultimate atoms of tangible matter are not in mathematical contact.

125. Bodies are penetrated by other matter in confequence of their porofity. Therefore the fame bulk may contain different quantities of matter.

126. DENSITY is a term, which, in ftrict language, expreffes vicinity of particles. But, when ufed by the mechanician as a term of comparifon, it expreffes the proportion of the number of equal particles, or the quantity of matter, in one body, to the number of *equal* particles in the fame bulk of another body.

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

then $$M \doteqdot B\,D$$

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

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

MECHA-

MECHANICAL PHILOSOPHY.

PART I. SECTION II.

DYNAMICS.

128. DYNAMICS is that department of phyfico-mathematical fcience which contains the *abftract* doctrines of moving forces; that is, the neceffary refults of the relations of *our* thoughts concerning motion and the caufes 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 meafures of the intenfity of thofe caufes.

130. We cannot think of motion, *in abftracto*, as a *thing*, properly fo called, that can fubfift feparately, but as a *quality*, or rather as a *condition*, of fome other thing. Therefore we confider this condition as permanent, like the fituation, figure or colour of the thing, unlefs fome caufe of change exert its influence on it.

131.

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

cafes that have yet been obferved, we find that the indicating phenomenon is obferved 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 fome philofophers, who maintain that there are no fuch mutual tendencies, and that the obferved approaches, or, in many cafes, mutual feparations, are effected by the extraneous impulfion of an æthereal fluid, or of certain miniftering fpirits, intrinfic or extrinfic.

133. Thefe mechanical affections of matter have been very generally called POWERS or FORCES; and the body conceived to poffefs them is faid to ACT on the related body. This is figurative or metaphorical language. Power, and force, and action, cannot be predicated, in their original ftrict fenfe, 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 fimple men; they gave thefe denominations to their own exertions with the utmoft propriety. To move a body, they found themfelves obliged to exert their *ftrength*, or *force*, or *power*, and to *act*. When fpeculative men afterwards attended to the changes of motion obferved in the meetings or vicinity of bodies, and remarked that the phenomena very much refembled the refults of exerting their own ftrength or force; and when they would exprefs this occurrence of nature, it was eafier to make ufe

of

of an old term, than to make a new one for things which fo much refembled; becaufe there are always fuch differences in other circumftances of the cafe, that there is little danger of confounding them. We are not to imagine that they thought that inanimate bodies exerted ftrength, as they themfelves did. This was referved for much later times of refinement.—In the progrefs of this refinement, the word power or force was employed to exprefs *any efficiency whatever;* and we now fay, the power of aqua fortis to diffolve filver—the force of argument—the action of motives, &c. &c.

To this notion of conveniency we muft afcribe, not only the employment of the words *power* and *force*, to exprefs *efficiency* in general, but alfo of the terms *attraction*, *repulfion*, *impulfion*, *preffure*, &c. all of which are metaphorical, unlefs when applied to the actions of animals. But they are ufed as terms of diftinction, on account of the refemblance between the phenomena and thofe which we obferve when we pull a thing toward us, pufh it from us, kick it away, or forcibly comprefs it.

134. Much confufion has arifen from the unguarded ufe of this figurative language. Very flight analogies have made fome animate all matter with a fort of mind, a ὁσπερ ψυχη, while other refemblances have made other fpeculatifts materialize intellect itfelf.

The very names which we give to thofe powers which we fancy to be inherent in bodies, fhew that we know nothing about them. Thefe names either, like magnetifm, exprefs a relation to the particular fubftances which we imagine poffefs the power, or they exprefs fomething of the effect which fuggefted their exiftence. Of this laft kind are *cohefion*, *gravity*, &c. They are almoft all verbal derivatives, and fhould be confidered by us merely as abbreviated defcriptions 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 worfe by having fome meaning. For this has frequently mifled us into falfe notions of the manner of acting. Perhaps the only ftrict 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, becaufe our own body, which is the immediate inftrument of the mind, is overlooked, like the plane in the hand of the carpenter, attending to the plank which he dreffes.

135. Forces have been divided into IMPULSIONS and PRESSURES. Impulfions are thofe which produce the changes of motion by the collifion of moving bodies. Preffure 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 preffure is a moving force; for, by preffing 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 fome-

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 contiguo*

' *et*

'*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 afcent 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 characteriftic motion does not follow, we fuppofe that it is oppofed by a force acting in the oppofite direction. Thus the agency of that other force is detected, and its intenfity may be meafured. Thus, the force with which the parts of a ftring cohere, or with which a fpringey body unbends, are detected by their fupporting a weight—and the magnitude of the weight is the meafure of the cohefion of the ftring, or of the elafticity of the fpring.

142. But the body in which this oppofing force is thus detected, is alfo faid to *refift* the force to which it is oppofed. This is figurative language, and, as ufed in mechanical philofophy, it is generally improper. In wreftling, when my antagonift exerts his ftrength, to prevent his being thrown down, I am fenfible of his exertion, and I thus learn that he refifts. But fhould I feel no more exertion neceffary than if he were a mafs of lifelefs matter, I fhould not think that he refifted. In the mechanical operations of nature, a force of any kind always produces its full effect, agreeably to the circumftances of the cafe, and can do no more. The force is indeed expended in producing its effect, becaufe matter is not moved without force. The weight lying on a fpring,

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 inertiæ*. Mr Rutherfurth, 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

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 exprefs this notion in the moft fimple manner by faying, that we confider force merely as fomething that is proportional to the change of velocity.

Of the Laws of Motion.

145. Such being our notions of motion, and of the caufes of its production and changes, there are certain refults, which, by the conftitution of our minds, neceffarily arife from the relations of thefe ideas. Thefe are laws of human judgement, independent of all experience of external nature, juft as it refults from the laws of judgement that the three angles of a right lined triangle are equal to two right angles, although there fhould not be a triangle in the univerfe.

Some of thefe laws may be intuitive, and may be called axioms; others, equally neceffary truths, may not be fo obvious, and may require fteps of argument.

There are three fuch laws, firft propofed in precife terms by Sir Ifaac Newton, which feem to give a fufficient foundation for all the doctrines of Dynamics, and to which, as to firft principles, we may appeal for the explanation of every mechanical phenomenon of nature.

Firft Law of Motion.

146. *Every body continues at reft, or in uniform rectilineal motion, unlefs affected by fome mechanical force.*

If we adhere to our inference of the agency of force only from an obferved change of motion, and to this inference from every fuch change, and if we grant that we have no notion of a force independent of a change of motion, this law feems little more than a tautological propofition. For, unlefs we fuppofe the agency of a mechanical force, we do not fuppofe a change of motion, that is, the abfence of mechanical agency is the abfence of a change of motion, and the body continues in its former ftate of reft or motion. But philofophers have attempted to demonftrate this law in various ways.

147. Some confider it as a neceffary truth, in the nature of the thing. A body, they fay, can neither accelerate, nor retard, nor deflect, becaufe the event is but one, and there is no caufe of determination whether it fhall accelerate, or retard, or deflect, nor whether to the right or to the left, or which determines any one degree of any of thofe changes. This fort of proof is obfcure and unfatisfactory.

148. Others choofe to confider it as a phyfical law, as an univerfal fact, for which, perhaps, we can give no reafon. They offer numerous proofs by induction. Thus, a coach being fuddenly accelerated, or checked in its progrefs, or turned out of its courfe, the fitters are thrown towards the back, or the front of the coach, or to one fide, fhewing, in all cafes, a tendency to continue in their former condition in refpect of motion. Number-

lefs

lefs examples may be given of the fame marks of this tendency to continue in the former ftate.

149. But it may be objected, that it is very far from being a matter of univerfal experience. Whoever grants the truth of the Copernican defcription of the planetary motions, will alfo grant that we perhaps never faw one inftance, either of reft or of uniform rectilineal motion. Our moft familiar obfervations fhew an evident tendency to reft, a fort of fluggifhnefs in all matter. For it is a fact, that all motions gradually diminifh, and, in a fhort time, terminate in reft. No force feems neceffary for maintaining a ftate of reft. But motion, they fay, is a violent ftate, the continual production of an effect, and therefore requiring a continuation of the caufe. Motion therefore requires the continual exertion of the caufe. They fay that a body in motion continues in it, only by the continual agency of a force infufed into it in giving it the motion, and inherent in it while in motion. They call it the *inherent force—vis infita corpori moto*.

150. But this is contrary to our clearest experience, and to any diftinct notions that we can form of motion as an effect of force. We are not confcious of any exertion, in order to continue our motion in fliding or fkating on fmooth ice; and when any obftruction comes in our way, we feel *diftinctly* our natural tendency to continue our fpeed undiminifhed—we feel that we muft refift

refift a tendency to fall forwards—we feel all obftruc-tions as checks on our fpeed, and think that if the ice were *perfectly* fmooth, we fhould go on for ever. It is equally contrary to our notions of a moving force. By its in-ftantaneous action, it produces motion, that is, a fuc-ceffive change of place, otherwife it produces nothing. Or if, in any inftant 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 inftant, the body does not move over any fpace; but it is in a certain condition (however imperfectly underftood by us) or has a certain determination, which we call velocity, by which, if not hindered, a certain length of path is paffed over in a fe-cond. This muft be effected by the inftantaneous action of the moving caufe, otherwife it is not a caufe of mo-tion. In fhort, motion is a *ftate* or *condition*, into which a body may be put, by various caufes, but by no means a *thing* which can be infufed into a body, or taken out of it.

Should it be faid that we have full evidence of a force refiding in a moving body, by obferving its impul-five power, which is not to be found in the fame body at reft, we may anfwer, that there *are* forces refiding in moving bodies, but that they are equally inherent in them when at reft, but that motion is neceffary, in or-der that thefe forces may be able to exert their action on the other body long enough to produce a fenfible ef-fect. Motion in the impelling body is not the *caufe* 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.

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 caufes, and which, like its whitenefs or roundnefs, will remain, till fome efficient caufe fhall change it. This we have called the *mechanical condition of the body*, and have fettled the meaning of the term with fufficient precifion. It confifts in its velocity and direction, and in no other circumftance.

In the next place, let us confider the change which may be induced on it as confifting folely in a change in thefe circumftances, and this change as the only indication, the only mark, and the only proper meafure of the changing caufe, that is, of the force (for we are confidering mechanical caufes only). It is evident that, as far as this procedure will carry us, we acquire certain knowledge, fufceptible of mathematical treatment. In order to make our tafk ufeful, we muft 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 difcoverable only by a comparifon with uniform motions. But we cannot tell whether a propofed motion be uniform, unlefs we have an accurate meafure of time. For it is to be learned only by obferving the proportions of the fpaces, and thofe of the times, and by obferving that thofe proportions are the fame. To obtain a meafure of time, various contrivances have been employed. They are all to this purpofe—An event is felected, in which

we have no reafon to think that any variation occurs in the operation of thofe caufes which effectuate its accomplifhment. It is then prefumed that it will always be accomplifhed in equal times. The rotation of the heavens, in twenty-three hours and fifty-fix minutes and four feconds, has been agreed on as the ftandard to which all other contrivances are referred or compared, and their accuracy is eftimated by their agreement with this ftandard.

Second Law of Motion.

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

This alfo is little more than a tautological propofition. If a force is to be meafured only by the change which it makes in the motion of a body, the propofition is only a repetition of this meafure in different terms; for, furely, quantities are proportional to their accurate meafures. Indeed, this would have been a fufficient demonftration, had not philofophers attempted it in another way, which has given rife to a great fchifm in the eftimation of forces. They have attempted to demonftrate it as an application of the undoubted maxim, that *effects are proportional to their caufes*. But it is eafy to fee that this application *cannot* be made; for it prefuppofes that we know the proportion of the forces, and that of their caufes, and that we perceive thofe proportions to be the fame.—Now, in moft cafes, this is impoffible; for the forces

forces are not objects of our obfervation. We know nothing of their proportions. When Newton fays that gravity at the furface of the earth is 3600 times greater than at the moon, he proves it by fhewing that the deflection caufed by it in a fecond, at the earth's furface, is 3600 times greater than that of the moon. But this is begging the queftion, or affuming this propofition as true, unlefs this law of motion be admitted as an axiom. There are very few cafes indeed, where we can fhew that forces are proportional to the changes of motion produced by them; yet fuch cafes are not altogether wanting. Thus, a fpring ftilyard can be made, the rod of which is divided by hanging on, in fucceffion, a number of perfectly equal weights. The elafticity of the fpring, in its different ftates of tenfion, is proportional to the preffures 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 feem entitled to fay that the preffure of gravity at Quito is to its preffure at Spitzbergen as 312 to 313, on the authority of effects being proportional to their caufes.

But fuch cafes are extremely rare, becaufe it is feldom that a natural power, accurately meafured in fome other way, is *wholly* employed in producing the obferved motion. Part of it is generally expended in fome other way, and therefore we frequently fee that the motions are not in the fame proportion with the fuppofed forces. But even though this could be ftrictly 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 eftimated by all that it is able to do before its motion is completely extinguifhed. When, therefore, it penetrates four times as far, it fhould be confidered as having produced a quadruple effect. The mechanicians of Europe were divided in their opinions; the Germans adhering to that of Leibnitz, and the Britifh and French to that of Des Cartes, who firft affirmed the relation which we have adopted as a fecond law of motion. We fhall fee prefently, that, in the Leibnitzian meafure, many things are gratuitoufly affumed, many contradictions are incurred, and, finally, that *it is only becaufe forces are affumed as proportional to the velocities which they generate, that the facts obferved by Hooke, and employed by Leibnitz, come to be proportional to the fquares of the fame velocities.* It fhall only be noticed at prefent, that when Leibnitz affumes the quadruple penetration as the proof of the quadruple force of a body having twice the velocity, he does not confider that a double time is employed in this penetration. Now, a double force acting equably during a double time, fhould produce a quadruple effect. This circumftance is neglected in one and all of the facts adduced by Mr Leibnitz. It may be added, that his followers, as well as himfelf, agree with us in every confequence which we draw from the meafure adopted by us. They grant that a force which produces a uniformly accelerated motion is a conftant force, and they agree with the Cartefians in all the valuations of accelerating and deflecting forces, and have been among the moft affiduous

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

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 meafure. It is quite allowable to meafure *afcenfional*, or *penetrating force*, by the *afcenfion* and the *penetration*, and to remark that thefe are as the fquare of the velocity. But this muft not be confidered as the general, or the beft, meafure of force, and particularly of *moving force*. This *muft* be meafured by the fimple change of motion which is produced by it. And this meafure has the advantage of being equally applicable to the phenomena of afcenfion and penetration, as we fhall fee very foon. We may now enounce it in a different form, adapted to the characteriftic and meafure of a change of motion, which was fhown in art. 79. to be the moft 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 reft.*

For it was fhewn 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, fince the change of motion is the characteriftic and the meafure of the changing force (161.), determining both its direction and its intenfity, or the velocity produced by it, the propofition follows of courfe.

163.

163. It was remarked in art. 80, that the compofition of motions, and the fimilar compofition of forces, are two very different things. The firft is a truth, purely mathematical, and as certain as any theorem in geometry. The fecond is a phyfical queftion entirely, depending on the nature of the mechanical forces which exift in the univerfe. We do not clearly fee that two forces, each of which will feparately produce motions having the directions and velocities expreffed by the fides of a parallelogram, will, by their joint action, produce a motion in the diagonal. The demonftrations given of this propofition by almoft all the writers of Elements are altogether inconclufive, being all fimilar to the cafe of a man walking on a field of ice, while the ice floats down a ftream. This is only the compofition of *motions*. Other writers, endeavouring to accommodate their reafonings to phyfical principles, have affumed poftulates that appear gratuitous. The firft legitimate demonftration was given by Dan. Bernoulli, in the *Comment. Petropol. Vol. I.* But it employs a feries of many propofitions, fome of which are very abftrufe. Mr D'Alembert greatly fimplified and improved this demonftration, in a Memoire of the Acad. des Sciences 1769. But this alfo requires many propofitions. Fonfenex and Riccati, in vol. III. of the Memoires of the Academy of Turin, have given another very ingenious one. D'Alembert has alfo improved this demonftration, and has given another, in the fame Memoires, and one in his *Dynamique*. The firft is very refined and obfcure, and the fecond does not feem

very conclufive. An attempt is made in the *Encyclop. Britan. Suppl.* § DYNAMICS, to combine Bernoulli's, D'Alembert's, and one by F. Frifi, which is more expeditious than either of the two firft, and appears legitimate. The demonftration given in this place is undoubtedly complete, if the reafoning 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 fo.

164. We have moft abundant proof of this law of motion, if we confider it merely as a phyfical law, or univerfal fact.

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

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

165. 2. If a point or particle of matter A (fig. 23.) be urged at once by two preffures, in the directions A B and A C, and if A B and A C are proportional to the intenfities of thofe preffures, the joint action of thefe

two

two preſſures is equivalent to the action of a third preſſure, in the direction of the diagonal A D, having its intenſity in the proportion of A D. This is completely proved, by obſerving that the point A will be withheld from moving, by a preſſure A E, equal and oppoſite to A D. 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* A B *into* A D, *is that which would produce in a quieſcent body the motion* A C, *which, when compounded with* A B, *produces the motion obſerved* A D.

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

From

From thefe two propofitions combined arifes a third, which is the moft general; viz.

169. *If a body* A *be urged at once by two forces, which would, feparately, caufe it to defcribe* A B *and* A C, *fides of a parallelogram* A B D C, *the body will, by their joint action, defcribe the diagonal* A D *in the fame time.* For, had the body been already moving with the velocity and in the direction A B, and had it been acted on in A by the force A C, it would defcribe A D in the fame time (168.). Now, it is immaterial at what time it got the determination by which it would defcribe A B. Let it therefore be at the inftant that the force A C is applied to it. It muft defcribe A D, becaufe its mechanical condition in A, having the determination to the motion AB, is the fame as in any other point of that line.

170. *Cor.* Two forces, acting on a body in the fame, or in oppofite directions, will caufe it to move with a velocity equal to the fum, or to the difference, of the velocities which it would have received from the forces feparately. For, if A C approach continually to A B, by diminifhing the angle B A C, the points C and D will at laft fall on *c* and *d*, and then A D is equal to the fum of A B and A C. But if the angle B A C increafe continually, the points C and D will, at laft, fall on $\varkappa$ and δ, and then A δ becomes equal to the difference of A B and A C.

In the laft cafe, it is evident that if A C be equal to A B, the point D or δ will coincide with A, and there

will

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

171. In fuch a cafe, the equal and oppofite forces AC and AB are faid to BALANCE each other, and, in general, thofe forces which, by their joint operation, produce no change of motion, are faid, in like manner, to balance each other; and they are accounted equal and oppofite, becaufe each produces on the body a change of motion equal to what it would produce on a body at reft, and at the fame time equal to the motion produced by the other force on a body at reft. Thefe two motions are therefore equal and oppofite, and therefore the forces are fo.

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

But, in making this transference, we muft carefully attend to the effential difference between the compofition of motions and the compofition of forces. In this laft, the compofition is complete, as foon as the body has gotten the determination to move in the diagonal with the proper velocity, and after this there is no more compofition. The body then moves uniformly, till fome force change its condition. But, in the compofition of two or more motions, the two conftituent motions are fuppofed to continue, and *by their continuance only*, does the com-

pound

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 A B or A C, the *joint* action muſt continue during the ſame time, in order to produce the velocity A D. 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 A B, A C, and proportional to thoſe lines, are exactly balanced by a third weight in the proportion of A D, and pulling in the direction A E. 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 intenfities of the compounded forces are accurately known as moving forces, complete the proof of the phyfical law, by their exquifite agreement with the calculations proceeding on the principles of this doctrine. This perfect agreement muft be received as a full proof of the propriety of the meafure of a moving force which we have affumed. Any other meafure would give refults widely different from the phenomena.

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

174. *The two conftituent forces and their refulting force act in one plane; and they are proportional to the three fides of a triangle having their directions, or of any fimilar triangle* (84).

175. *Each force is proportional to the fine of the angle contained by the directions of the other two.* For the fides of any triangle are as the fines of the oppofite angles.

176. A force acting in the direction parallel to any line B D does not affect the approach toward that line, or its recefs from it, occafioned by the action of another force

force A C. For, becaufe 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 fame diftance from B D with the points *d* and *e* to which it really goes in the fame 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 defcribe the line A F, determined as in article 86.; and A F will exprefs the equivalent or refulting force, both in refpect of direction and intenfity.

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

179. Forces may be *eftimated* 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 oppofite to their refulting or equivalent force.

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

To thefe two laws of motion, which we have attempted to fhew to be neceffary confequences of the relations of thofe conceptions which we form of motion and of mechanical force, and alfo to be univerfal facts or phyfical laws, Sir Ifaac 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 ufually expreffed thus—*Reaction is always equal and contrary to action.*

This is indeed a fact, obferved without exception, in all the cafes which we can examine with accuracy. Sir Ifaac Newton, in the general fcholium or remark on the laws of motion, feems to confider this equality of action and reaction as an axiom deduced from the relations of ideas. But this feems doubtful. Becaufe a magnet caufes the iron to approach towards it, it does not appear that we neceffarily fuppofe that iron alfo attracts the magnet. The fact is, that although many obfervations 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 difcovery, and Dr Gilbert is, I think, the earlieft writer, in whofe works we meet with it. He affirms that this *mutual* attraction is obferved between the magnet and iron, and between all electrical fubftances and the light bodies attracted by them. Kepler noticed

noticed this mutual influence between the Earth and the Moon. Wallis, Wren, and Huyghens, firft diftinctly affirmed the mutual, equal, and contrary action of folid bodies in their collifions; and it has been confirmed by innumerable obfervations. Nay, fince that time, Sir Ifaac Newton himfelf only *prefumed* that, becaufe the Sun attracted the planets, thefe alfo attracted the Sun; and he is at much pains to point out phenomena to aftronomers, by which this may be proved, when the art of obfervation fhall be fufficiently improved. Thefe muft be put on the fame footing with the phenomena by which the mutual actions of the planets are proved. Now, this laft action was *altogether* a prefumption, although the proof was by far the moft eafy. The difcovery and complete demonftration of this, as a phyfical law, is certainly the moft illuftrious fpecimen of Newton's genius and nice judgement.

We muft receive it therefore as a law of motion, with refpect to all bodies on which we can make experiment, or obfervation fit for deciding the queftion.

183. As it is an univerfal law, we cannot rid ourfelves of the perfuafion that it depends on fome general principle, which influences all the matter in the univerfe. 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 fame degree, to every atom—and that all the fenfible differences of fubftance which we obferve arife from a different combination of

primary

primary atoms in the formation of a particle of thofe fubftances. A very flight confideration may fhew us that this is perfectly poffible. Now, if fuch be the conftitution 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 oppofite direction. Nothing can be clearer than this. This therefore is, in all probability, the origin of this Third Law of Motion.

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184. The aim of the Newtonian philofophy, which we profefs to follow, is to inveftigate the laws obferved in the production of natural effects, and to comprehend any propofed phenomenon in one or other of thofe laws. We then account it as explained.

Thefe general, but ftill fubordinate, laws are to be eftablifhed only by obfervation and experiment; but when fo eftablifhed as far as obfervation extends, it is only by means of fome obferved analogy that we can ufe them as explanations of many other phenomena. With this we muft reft fatisfied, becaufe it feems impoffible for our faculties to difcover the efficient caufes of thofe general laws, fo as to be able to demonftrate that they *muft* be fuch as we obferve. But in the eftablifhment of them as mere matters of fact, we may obferve them to be of various extent, and that fome are fubdivifions of others. In this fubordination, we can difcern

cern much order, harmony and beauty, and our minds are left deeply impreffed with admiration of the wifdom and fkill of the contrivance, by which this magnificent fabric is fitted for the accomplifhment of a great and beneficent purpofe.

185. The three axioms, and, indeed, the two firft, feem to include the whole firft principles of Dynamics, and enable us, without other help, to accomplifh every purpofe of the fcience. Some authors of eminence have thought that there were other principles, which influenced every natural operation, and that thefe operations could not be fully underftood, nor an explanation properly deduced, without employing thofe principles. Of this kind is the principle of OECONOMY OF ACTION, or SMALLEST ACTION, affirmed by Mr Maupertuis to be purfued in all the operations of nature. This philofopher fays, that the perfect wifdom of Deity muft caufe him to accomplifh every change by the fmalleft poffible expenditure of power of every kind; and he gives a theorem which he fays expreffes this œconomy in all cafes of mechanical action. He then afferts that, in order to fhew in what manner fuch and fuch bodies, fo and fo fituated, fhall change each other's condition, we muft find what change in each will agree with this value of the fmalleft action. He applies this to the folution of many problems, fome of which are intricate, and gives folutions perfectly agreeable to the phenomena.

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

It

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.

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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 accompany

pany thofe fubftances, wherever they are. Thefe changes are the only characteriftics of the powers. It is thus that we difcover and defcribe the power of magnetifm, gravity, &c.

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

To fave us a great labour, which muft be repeated for every queftion, if we make *immediate* application of the laws of motion to the phenomenon, it will be extremely convenient to have in readinefs a few general rules, accommodated to the more frequent cafes of natural operations. The mechanical powers of bodies occafionally accelerate, retard, and deflect the motions of other bodies. Therefore it is proper to premife the principal theorems relating to the action of accelerating, retarding, or deflecting forces. They have got thefe names, becaufe we know nothing of their nature, or of the manner in which they are effective, and therefore name them, as we meafure them, by the phenomena which we confider as their effects.

Of Accelerating and Retarding Forces.

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

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

If the abfciffa a d (fig. 5.) *reprefent the time of any motion, and if the areas* a b f e, a c g e, &c. *are as the velocities at the inftants* b, c, &c. *the ordinates* a e, b f, c g, &c. *are as the accelerating forces at thofe inftants* (69).

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

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

Alfo, 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, inverfely (71.)

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

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

190. *Cor.* 2. *Uniformly accelerated or retarded motion is the indication of a conftant or invariable accelerating force.* For, in this cafe, the areas *a b f e*, *a c g e*, &c. increafe at the fame rate with the times *a b*, *a c*, &c. and therefore the ordinates *a e*, *b f*, *c g*, &c. muft all be equal; therefore the forces reprefented by them are the fame, or the accelerating force does not change its intenfity, or, it is conftant. If, therefore, the circum-

ftances

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 $f\dot{t}$, 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,*

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;

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

and

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

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 $= f \dot{t}$ (189.), so the change on the square of the velocity depends on the space, it being $= f \dot{s}$. 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 $e a$, expreffed by $\int f \dot{s}$, expreffes the whole change made on the fquare of the velocity which the body had in A, whatever this velocity may have been. We may therefore fuppofe the body to have been at reft in A. The area then meafures the fquare 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 paffing through the fpace A E with any initial velocity whatever, fuftains the fame change of the fquare of that velocity, if under the influence of the fame force.

197. This propofition is the fame with the 39th of the Firft Book of Newton's Principia, and is perhaps the moft generally ufeful of all the theorems in Dynamics, in the folution of practical queftions. It is to be found, without demonftration, in his earlieft writings, the Optical Lectures, which he delivered in 1669 and following years.

198. One important ufe may be made of it at prefent. It gives a complete folution of all the facts which were obferved by Dr Hooke, and adduced by Leibnitz with fuch pertinacity in fupport of his meafure of the force of moving bodies. All of them are of precifely the fame nature with the one mentioned in art. 157, or with the fact, " that a ball projected directly upwards " with a double velocity, will rife 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 uniform 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 ε *a*. If therefore A E be the penetration neceſſary for extinguiſhing the velocity 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.

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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 *a c* and *h k* (fig. 24.), they are alſo ſimilarly accelerated along their reſpective paths A C and H K.

Let *a*, *b*, *c* be inſtants of the time *a c*, ſimilar to the inſtants *h*, *i*, *k* of the time *h k*. Then, by the ſimilar accelerations, we have the force $a\,e : h\,l = b\,f : i\,m$. This being the caſe throughout, the area *a f* is to the area *h m* as the area *a g* to the area *h n* (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 $\doteqdot$ f t.

For the abſciſſæ *a c* and *h k* will repreſent the times, and the ordinates *a e* and *h l* will repreſent the forces, and then the areas will repreſent the changes of velocity, by art. 70. And theſe areas are as *a c* × *a e* to *h k* × *h l*, (by Symbols (*s. Cor.*)

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

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* $\doteqdot$ 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 $v \doteqdot f t$, and $v^2 \doteqdot f^2 t^2$; but (203.) $v^2 \doteqdot f s$. Therefore $f s \doteqdot f^2 t^2$, and $s \doteqdot f t^2$.

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

205. The quantity of motion in a body is the ſum 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, $m v$ will expreſs the quantity of motion q, or $q \doteqdot m v$.

206. In like manner, we may conceive the accelerating forces f, which have produced this velocity v in each particle, as added into one ſum, or as combined on one particle, by article 170. They will thus compoſe a force, which, for diſtinction's ſake, it is convenient to mark by a particular name. We ſhall call it the MOTIVE FORCE, and expreſs it by the ſymbol p. It will then be conſidered 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 $m v$, and the ſame quantity of motion q.

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

force be reprefented by π. It is meafured in like manner by $n\,u$. 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 fimilar action of different motive forces, in the fame time, are directly as thofe forces, and inverfely as the quantities of matter to which they are applied.

In general, $\dot{v} \div \frac{p}{m}$.

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

Remark.

208. In the application of the theorems concerning accelerating or retarding forces, it is neceffary to attend carefully to the diftinction between an accelerative and a motive force. The caution neceffary here has been generally overlooked by the writers of Elements, and this has given occafion to very inadequate and erroneous notions of the action of accelerating powers. Thus, if a leaden ball hangs by a thread, which paffes over a pulley, and is attached to an equal ball, moveable along a horizontal plane, without *the fmalleft* obftruction, it is known that, in one fecond, it will defcend 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 fecond. Let the thread be attached to three fuch balls. We know that it will defcend 4 feet in a fecond, and generate

generate the velocity 8 feet per fecond. Moft readers are difpofed to think that it fhould generate no greater velocity than $5\frac{1}{3}$ feet per fecond, or $\frac{1}{3}$ of 16, becaufe it is applied to three times as much matter (207.) The error lies in confidering the motive force as the fame in both cafes, and in not attending to the quantity of matter to which it is applied. Neither of thefe conjectures is right. The motive force changes as the motion accelerates, and in the firft cafe, it moves two balls, and in the fecond it moves four. The motive force decreafes fimilarly in both motions. When thefe things are confidered, we learn by articles 202 and 207, that the motions will be precifely what we obferve.

Of Deflecting Forces, in general.

209. It was obferved, in art. 99, that a curvilineal motion is a cafe of *continual* deflection. Therefore, when fuch motions are obferved, we know that the body is under the *continual* influence of fome natural force, acting in a direction which croffes that of the motion in every point. We muft infer the magnitude and direction of this deflecting force by the magnitude and direction of the obferved deflection. Therefore, all that is affirmed concerning deflections in the 99th and fubfequent articles of the Introduction, may be affirmed concerning deflecting forces. It follows, from what has been eftablifhed concerning the action of accelerating forces, that no force can produce a finite change of velocity in an inftant.

 Now,

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 rectilineal motion muſt increaſe by inſenſible degrees. The curvilineal 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 ſubſultory 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 curvilineal 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 defcribed in a very fmall moment. For it was demonftrated in that article (fee fig. 18.) that B Z : B C = B C : B O.

We fee alfo that B *b*, the double of B O, is the meafure of the velocity, generated by the uniform action of the deflecting force, during the motion in the arch B C of the curve.

212. The art. 106. alfo furnifhes a propofition of frequent and important ufe, 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 propofitions now given proceed on the fuppofition that, when the points A and C of fig. 18, after continually approaching to B, at laft coalefce with it, the laft circle which is defcribed through thefe three points has the fame curvature which the path has in B. It is proper to render this mode of folving thefe queftions more plain and palpable.

If A B C D (fig. 25.) be a material curve or mould, and a thread be made faft 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 defcribe another curve line A *b c*. All curves, in which the curvature is neither infinitely great nor infinitely fmall, may be thus defcribed 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 fhewn how to conftruct the evolved curve A B C which will produce it.

From this genefis of curves we may infer, 1*st*, that the detached portion of the thread is always a tangent to the curve A B C; 2*dly*, that when this is in any fituation B *b*, it is perpendicular to the tangent of the curve A *b c* in the point *b*, and that it is, at the fame time, defcribing an element of that curve, and an element of a circle *α b κ*, whofe momentary centre is B, and which has B *b* for its radius. 3*dly*, That the part *b* A of the curve, being defcribed with radii growing continually fhorter, is *more* incurvated than the circle *b α*, which has B *b* for its conftant radius. For fimilar reafons the arch *b c* of the curve A *b c* is *lefs* incurvated than the circle *α b κ*. 4*thly*, That the circle *α b κ* has the fame 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 fometimes called the INVOLUTE of A B C, and fometimes its EVOLUTRIX.

214. By this way of defcribing curve lines, we fee clearly that a body, when paffing through the point *b* of the

the curve A b c may be confidered as in the fame ftate, in that inftant, as in paffing through the fame point b of the circle $\alpha\, b\, \kappa$; and the ultimate ratio of the deflections in both is that of equality, and they may be ufed indifcriminately.

The chief difficulty in the application of the preceding theorems to the curvilineal motions which are obferved in the fpontaneous phenomena of nature, is in afcertaining the direction of the deflection in every point of a curvilineal motion. Fortunately, however, the moft important cafes, namely thofe 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 defcribe circles with a uniform motion, the deflecting forces are always directed to the centres of the circles, and are proportional to the fquare of the velocities, directly, and to their diftances from the centre, inverfely.*

For, fince 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 cafe, the diameter of the circle, or twice the diftance of the body from the centre. Therefore, if we call the diftance from the centre d, we have $f \doteq \frac{v^2}{d}$.

216.

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*

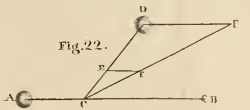
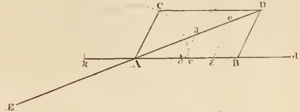


Fig. 22.

Fig. 23.



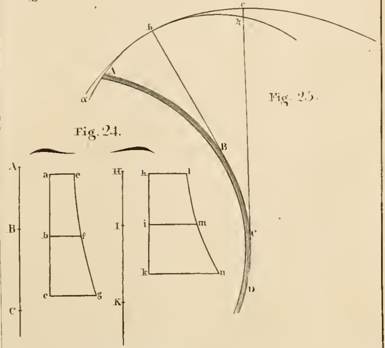


Fig. 25.

Fig. 24.

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

220. Converfely. *If a body is deflected by a force always directed to a fixed point, it will defcribe a curve line lying in one plane which paffes through that point, and the line joining it with the centre of forces will defcribe 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 firft cafe, the curve will have its *concavity* toward the centre, but, in the fecond cafe, it will be *convex* toward the centre. The force which urges a piece of iron towards a magnet is centripetal, and that which caufes two electrical bodies to feparate is centrifugal.

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

This

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. Converfely. *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.)

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 This

This force is often called centrifugal, *the centrifugal force of circular motion*, and it is conceived as always acting in every cafe of curvilineal motion, and to act in oppofition to the centripetal force which produces that motion. But this is inaccurate. We fuppofe this force, merely becaufe we muft employ a centripetal force, juft as we fuppofe a *refifting* vis inertiæ, becaufe we muft employ force to move a body.

222. *If a body defcribe a curve line* A B C *by means of a centripetal* (fig. 26.) *force directed to* S, *and varying according to fome proportion of the diftances from it, and if another body be impelled toward* S *in the ftraight line* a b S *by the fame force, and if the two bodies have the fame velocity in any points* A *and* a *which are equidiftant from* S, *they will have equal velocities in any other two points* C *and* c, *which are alfo equidiftant from* S.

Defcribe round S, with the diftance S A, the circular arch A *a*, which will pafs through the equidiftant point *a*. Defcribe another arch B *b*, cutting off a fmall arc A B of the curve, and alfo cutting A S in D. Draw D E perpendicular to the curve.

The diftances A S and *a* S being equal, the centripetal forces are alfo equal, and may be reprefented by the equal lines A D and *a b*. The velocities at A and *a* being equal, the times of defcribing A B and *a b* will be as the fpaces (31). The force *a b* is wholly employed in accelerating the rectilineal motion along *a* S. But the force A D, being tranfverfe or oblique to the motion along

along A B, is not wholly employed in thus accelerating the motion. It is equivalent (173) to the two forces A E and E D, of which E D, being perpendicular to A B, neither promotes nor oppofes it, but incurvates the motion. The accelerating force in A therefore is A E. It was fhewn, in art. 71, that the change of velocity is as the force and as the time jointly, and therefore it is as A E × A B. For the fame reafon, the change of the velocity at a is as $a\,b \times a\,b$, or $a\,b^2$. But, as the angle A D B is a right angle, as alfo A E D, we have A E : A D = A D : A B, and A E × A B = A D², $= a\,b^2$. Therefore, the increments of velocity acquired along A B and $a\,b$ are equal. But the velocities at A and a were equal. Therefore the velocities at B and b are alfo equal. The fame thing may be faid of every fubfequent increafe of velocity, while moving along B C and $b\,c$; and therefore the velocities at C and c are equal.

The fame thing holds, when the deflecting force is directed in lines parallel to a S, as if to a point S′ infinitely diftant, the one body defcribing the curve line V A′ B′, while the other defcribes the ftraight line V S.

223. The propofitions in art 102. and 103. are alfo true in curvilineal motions by means of central forces.

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

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

224. The formula $f \doteq \frac{v^2}{c}$ ſerves for diſcovering the law of variation of the central force by which a body deſcribes the different portions of its curvilineal path; and the formula $f \doteq \frac{d}{t^2}$ 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 $f = \frac{2\,v^2}{c}$ 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* V P S, *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 of of the body in every point of its motion is increafed in the ratio of m to $n + m$, and it will be in the fame ratio in the different parts of the orbit as before, that is, it will be inverfely as the fquare of the diftance from S (103). Moreover, as the diftances from the centre in the fimultaneous pofitions of the body, in the quiefcent and in the revolving orbit, are the fame, the momentary increments of the area are as the momentary increments of the angle at the centre; and therefore, in both motions, the areas increafe in the conftant ratio of m to $m + n$ (103). Therefore the areas of the abfolute path, produced by the compofition of the two motions, will ftill be proportional to the times; and therefore (101) the deflecting force muft be directed to the centre S; or, a force fo directed will produce this compound motion.

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

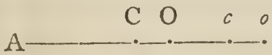
Let V K S B V (fig. 27.) be the fixed orbit, and $u\,p\,k\,b\,u$ the fame orbit moved into another pofition; and let V p n N o N t Q V be the orbit defcribed by the body in abfolute fpace by the compofition of its motion in the orbit with the motion of the orbit itfelf. If the body be fuppofed to defcribe the arch V P of the fixed orbit while the axis V C moves into the fituation u C, and if the arch $u\,p$ be made equal to V P, then p will be the place of the

the body in the moveable orbit, and in the compound path V*p*. 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 VC*p* as *m* to *m* + *n*. Let PK and *p k* be two equal and very ſmall arches of the fixed and moving orbits. PC and *p c* are equal, as are alſo KC and *k*C, and a circle deſcribed round C with the radius CK will paſs through *k*. If we now make VCK to VC*n* as *m* to *m* + *n* : the point *n* of the circle K*k n* 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 *p n* 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 *p n* 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 $oc =$ O C. To each of theſe add O c, and then it is plain that $c\,O = c\,C$, 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 fame orbit moving as determined in article 226, is always in the inverfe triplicate ratio of the diftances from the centre of forces.

In this example, the motion of the body in the orbit is in the fame 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 oppofite direction, the force to be joined would have been fubtractive, unlefs the retrograde motion of the orbit exceeded twice the angular motion of the body. But in all cafes, the reafoning is fimilar.

228. Thus we have confidered the motions of bodies influenced by forces directed to a fixed point. But we cannot conceive a mere mathematical point of fpace as the caufe or occafion of any fuch exertion of forces. Such relations are obferved only between exifting bodies or maffes of matter. The propofitions which have been demonftrated may be true in relation to bodies placed in thofe fixed points. That continual tendency towards a centre, which produces an equable defcription of areas round it, becomes intelligible, if we fuppofe fome body placed in the centre of forces, attracting the revolving body. Accordingly, we fee very remarkable examples of fuch tendencies towards a central body in the motions of the planets round the Sun, and of the fatellites 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 S P (fig. 28.) Their centre of poſition is in a point C, dividing their diſtance in ſuch a manner that S C is to C P 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 ex-

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 $S\,s$ will be to $P\,p$ as P to S, that is as S C to P C. Therefore we ſhall ſtill have $s\,C : p\,C = S\,C : P\,C$. Their diſtances from C will always be in the ſame proportion. Alſo we ſhall have $S\,C : S\,P = P : S + P$, and $s\,C : p\,C = P : S + P$; and therefore $S\,C : S\,P = s\,C : s\,P$. 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 S A and P B. 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 S A and P B inverſely as their quantities of matter. Therefore $S\,A : P\,B = S\,C : P\,C$, 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 $S\,a$ and $P\,b$, ſo that their deflections $A\,a$ and $B\,b$ are as S C and P C; and we ſhall

have

have a C : b C = S C : P C. As this is true of every part of the curve, it follows that they defcribe fimilar curves round C, which remains in its original place.

Laftly, If the motion of P be confidered by an obferver placed in S, unconfcious of its motion, fince he judges of the motion of P only by its change of direction and of diftance, we may make a figure which will perfectly reprefent this motion. Draw the line E F equal and parallel to P S, and E G equal and parallel to $a\,b$. Do this for every point of the curve S a and P b. We fhall then form a curve F G fimilar to the curves S a and P b, having the homologous lines equal to the fum of the homologous lines of thefe two curves. Thus the bodies will defcribe round each other curve lines which are fimilar and equal (lineally) to the lines which they defcribe round their common centre by the fame forces. They may appear to defcribe areas proportional to the times round each other; and they really defcribe areas proportional to the times round their common centre of pofition, and the forces, which really relate to the body which *is fuppofed* to be central, have the fame mathematical relation to their common centre.

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

231. The time of defcribing any arch F G of the curve defcribed round the other body at reft in a centre

 of

of forces (where we may ſuppoſe it forcibly withheld from moving) is to the time of deſcribing the ſimilar arch P b 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 A S B, 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 P p and S s 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

difcover 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 fome known forces.

234. Both of thefe purpofes are to be attained by a careful obfervation of the phenomena. All circumftances of coincidence or refemblance among them are to be taken notice of, and confidered as indications of a fimilarity in their Caufes. The more extenfive the obferved coincidence of appearances is, the more general muft the affection of matter be which is the caufe of this refemblance. If any fimilarity is univerfally obferved, it muft be confidered as the indication of a mechanical quality that is competent to all matter.

235. This confideration points out to us a principle for arranging the mechanical phenomena of the univerfe. Thofe fhould be firft confidered that are moft general. Thus are we made acquainted with the moft general mechanical properties of Bodies, which extend their influence to phenomena in all the fubordinate claffes, and modify even that circumftance which forms the particular clafs. Our previous acquaintance with thofe general properties will enable us to free the more particular phenomena from part of that complication which makes the ftudy of them more difficult; and then to confider apart thofe circumftances of the phenomena which are indications of qualities lefs general.

236. The moſt general phenomenon that we obſerve is the curvilineal 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 curvilineal 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 curvilineal 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 ſuccefsful. 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.

MECHA-

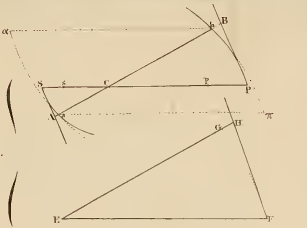


Fig. 28.

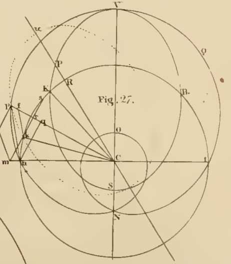


Fig. 27.

Fig. 26.

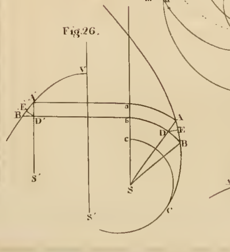
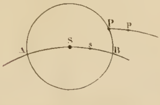


Fig. 29



MECHANICAL HISTORY

OF

NATURE.

SECT. I. ASTRONOMY.

238. ASTRONOMY 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

But the purpofes of the Mechanical philofopher cannot be attained without attending to that beauty, regularity, and fubordination. Thefe features are exhibited in every circumftance of the celeftial motions that renders them fufceptible of fcientific arrangement and inveftigation; and a philofophical view cannot be taken, without the fame accurate knowledge of the motions that is wanted for the arts of life. It muft be added, that fociety never would have derived the benefits which it has received from Aftronomy, without the labours of the philofopher: For, had not Newton, or fome fuch exalted genius as Newton, fpeculated about the deflecting forces which regulate the motions of the Solar fyftem, we never fhould have acquired that exquifite knowledge of the mere phenomena that is abfolutely neceffary for fome of the moft important applications of them to the arts. It was thefe fpeculations alone that have enabled our navigators to proceed with boldnefs through untried feas, and in a few years have almoft completed the furvey of this globe. And thus do we experience the moft beneficial alliance of Philofophy and Art.

Since the motions of bodies are the only indications, characteriftics, and meafures of moving forces, it is plain that the celeftial motions muft be accurately afcertained, that we may obtain the data wanted for the purpofe of philofophical inference. To afcertain thefe is a tafk of great difficulty; and it has required the continual efforts of many ages to acquire juft 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 irreconcileable 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 H B O D (fig. 30.), which ſeparates the viſible hemiſphere H Z O from the inviſible hemiſphere H N O, 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 obferver looks at the heavens, while a plummet is fufpended before his eye, the plumb line will mark out on the heavens a quadrant of a circle, whofe plane is perpendicular to the horizon, and which therefore paffes through the zenith and nadir, and through two oppofite points of the horizon. Z O N H and Z B N D are fuch circles. They are called VERTICAL CIRCLES and AZIMUTH CIRCLES.

243. The ALTITUDE of any celeftial phenomenon fuch as a ftar A, is the angle A C B, formed in the plane of the vertical circle Z A N, by the horizontal line C B and the line C A. This name is alfo given to the arch A B of the vertical circle which meafures this angle. The arch Z A is called ZENITH DISTANCE of the phenomenon.

244. The AZIMUTH of the phenomenon is the angle O C B, or O Z B, formed between the plane of the vertical circle Z A B paffing through the phenomenon, and the plane of fome other noted vertical Z O N. The arch C B of the horizon, which meafures this angle, is alfo frequently called the Azimuth.

245. The ftarry heavens appear to turn round the earth, which feems pendulous in the centre of the fphere; and by this motion, the heavenly bodies come into view in the eaft, or RISE; they attain the greateft altitude, or CULMINATE, and difappear in the weft, or SET. This is called the FIRST MOTION.

246. This motion is performed round an axis N S (fig. 31.), paffing through two points N, S, called the poles of the world. In confequence of this motion, a celeftial object A defcribes a circle A D B F, through the centre C of which the axis N S paffes, perpendicularly to its plane. This motion may be very diftinctly perceived as follows. Let a point, or fight, be fixed in the infide of a fky-light fronting the north, and inclined fouthwards from the perpendicular at an angle equal to the latitude of the place. An eye placed at this point will fee the ftars through the glafs of the window. Let the points of the glafs, through which a ftar 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. Thofe ftars which are fartheft from the poles will defcribe the greateft circles; and thofe will defcribe the largeft poffible circles which are in the circumference of the circle Æ W Q E, which is equidiftant from both poles. This circle is called the EQUATOR, and, being a great circle, it cuts the horizon in two points, E, W, diametrically oppofite to each other. They are the eaft and weft points of the horizon.

248. If a great circle A N Q S Æ paffes through the poles perpendicularly to the horizon H W O E, it will cut it in the north and fouth points; and any ftar A will acquire its greateft elevation when it comes to the

femicircle

femicircle N A S, and its greateft depreffion when it comes to the femicircle N B S; and the arch D A F of its apparition will be bifected in A.

249. If the circle A D B F of revolution be between the equator and that pole N which is above the horizon, the greateft portion of it will be vifible; but if it be on the other fide of the equator, the fmalleft portion will be vifible. One half of the equator is vifible. Some circles of revolution are wholly above the horizon, and fome are wholly below it. A ftar in one of the firft is always feen, and one in the laft is never feen.

250. The diftance A Æ of any point A from the equator is called its DECLINATION, and the circle A D B F, being parallel to the equator, is called a PARALLEL OF DECLINATION.

251. The angle Æ C H, contained by the planes of the equator and horizon, is the complement of the angle N C O, which is the elevation of the pole.

252. The revolution of the ftarry heavens is performed in $23^h\ 56'\ 4''$. It is called the DIURNAL REVOLUTION. No appearance of inequality has been obferved in it; and it is therefore affumed as the moft perfect meafure of time.

253. The time of the diurnal apparition or difparition of a point of the ftarry heavens is bifected 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 Æ S, or N B L S (fig. 32.), paſſing through the poles of the world, is called an HOUR CIRCLE.

256. The angle Æ C L, or Æ 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 Æ S, paſſing through a certain noted point Æ of the equator, is called the RIGHT ASCENSION of the phenomenon. The intercepted arch Æ 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 pofitions of the ftars. This may be acquired. The difference between the meridional altitude of a ftar B, and of the equator, gives the arch A Æ, intercepted between the equator and the parallel of declination, or circle of diurnal revolution A B D, defcribed by the ftar. And the time which elapfes between the paffage of this ftar over the meridian, and the paffage of that point Æ of the equator from which the right afcenfions are computed, gives the arch Æ L of the equator which has paffed 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 obferved diftance A Æ from the equator, the place of the ftar will be found in their interfection B.

258. Globes and maps have been made, on which the reprefentations of the ftars have been placed, in pofitions fimilar to their real pofitions; and catalogues of the ftars have been compofed, in which every ftar is fet down with its declination and right afcenfion, this being the moft convenient arrangement for the practical aftronomer. Their longitudes and latitudes (to be explained afterwards) are alfo fet down, in feparate columns. The moft noted of all thefe is the BRITANNIC CATALOGUE, conftructed by Dr Flamftead, from his own obfervations in the Royal Obfervatory at Greenwich. This catalogue contains the places of 3030 ftars. It is accompanied by a collection of maps, known to all aftronomers 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 accomodating 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° 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 knowldge 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 Flamſtead, 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 profecuting this method of obtaining a minute furvey of the whole ftarry heavens.

In the valuable aftronomical tables publifhed in 1776 by the academy of Berlin, Mr Bode has given a fimilar fynopfis of the catalogues of Flamftead, La Caille, Bradley and Mayer, not indeed fo extenfive, nor fo minute, as Wollafton's, but of great ufe.

262. Having thus obtained maps of the heavens, the place of a celeftial phenomenon is afcertained in a variety of ways. 1. By its obferved diftance from two known ftars. 2. By its altitude and azimuth. 3. Moft accurately, by its right afcenfion and declination.

263. This laft being the moft accurate method of afcertaining the place of any celeftial phenomenon, obfervations of meridional altitude, and of TRANSITS over the meridian, are the moft important. For an account of the manner of conducting thefe obfervations, and a defcription of the inftruments, we may confult Smith's Optics, Vol. II.; Mr Vince's Treatife of Practical Aftronomy; La Lande's Aftronomy, &c. The MURAL QUADRANT, TRANSIT INSTRUMENT, and CLOCK, are therefore the capital furniture of an obfervatory; to which, however, fhould be added an EQUATOREAL INSTRUMENT for obferving phenomena out of the meridian. Other inftruments, fuch as the EQUAL ALTITUDE INSTRUMENT, the RHOMBOIDAL RETICULA, the ZENITH SECTOR, and one or two more, are fitted for aftronomers 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 N S (fig. 31.) to the horizon, or the elevation of the pole N. The beſt method for this purpoſe is to obſerve the greateſt elevation I O, and the leaſt elevation K O, 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 69½ 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 69½ 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 7940⅔ miles. This deviation from perfect ſphericity has been diſcovered by meaſuring,

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in the way now mentioned, a degree of the meridian in different latitudes. One was meafured in Lapland, in latitude 66° 20′, and it meafured 122,457 yards, exceeding 69½ miles by 137 yards. Another was meafured at Peru, croffing the very equator. It contained 121,027 yards, falling fhort of 69½ miles by 1293 yards, and wanting 1430 yards, or almoft a mile, of the other. Other degrees have been meafured in intermediate latitudes; and it is clearly eftablifhed, that the degrees gradually increafe, as we go from the equator towards either pole.

267. The length of a degree is the diftance between two places where the tangents to the furface are inclined to one another one degree, or where two plumb lines, which are perpendicular to the furface of ftanding water, will, when produced downwards, meet one another, intercepting an angle of one degree. The furface of the ftill ocean is therefore lefs incurvated as we approach the poles, or it requires a longer arch to have the fame curvature. It is a degree of a larger circle, and has a longer radius. Perfons who do not confider the thing attentively, are apt to imagine, from this, that the earth is fhaped like an egg; becaufe, if we draw from its centre lines C N (fig. 33. N° 2.) C O, C P, C Q, equally inclined to one another, the arches N O, O P, P Q, will gradually increafe from N towards Q. If thefe lines make angles of one degree with one another, they will meet the furface in points that are farther and farther

afunder,

afunder, and the degree will appear to increafe as we approach the points E and Q, which we fuppofe, at prefent, to be the poles. But let fuch perfons reflect, that if thefe lines from the centre are produced beyond the furface, they cannot be plumb lines, perpendicular to the furface of ftanding water. But if an ellipfe N E S Q (fig. 33. N° 2.) be made to turn round its fhorter axis N S, it will generate a figure flatter round N and S than at E or Q. If we draw two lines *a* D and *b* B perpendicular to the curve in *a* and *b*, and exceedingly near one another, they will be tangents to a curve A B D F, by the evolution of which the elliptic quadrant E *a* N is defcribed. A E is the radius of curvature of the equatoreal degree of the meridian E *a* N. N F is the radius of the polar degree, and *a* D is the radius of curvature at the intermediate latitude of *a*, &c. All thefe radii are plumb lines, perpendicular to the elliptical curve of the ocean.

Thefe plumb lines therefore do not meet in the centre of the earth, as is commonly imagined, but meet, in fucceffion, in the circumference of the evolute A B D F. The earth is not a *prolate* fpheroid like an egg, but an *oblate* fpheroid, like a turnip or bias bowl.

268. Since the axis of diurnal revolution paffes through the centre of the earth, it marks on its furface two points, which are the poles of the earth. Thefe are in the extremities of the axis of the terreftrial fpheroid. In like manner, the plane of the celeftial equator

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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 $23^h\ 56'\ 4''$, 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 - - -	H

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 A S V W (fig. 35.), which cuts the celeſtial equator in two oppoſite points A, V, and is inclined to it at an angle of 23° 28′ 10″ nearly. This circle, or Sun's path, is called the ECLIPTIC.

274.

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

275. The points V, A, are called EQUINOCTIAL POINTS; becaufe, when the fun is in thefe points, his circle of diurnal revolution is the celeftial equator, and therefore the day and night are equal. The point V, through which he paffes 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 fartheft from the equator, are called the SOLSTITIAL POINTS, S being the fummer, and W the winter folftice. The parallels of declination paffing through the folftitial points are called TROPICS.

276. Right afcenfion is always computed eaftward on the equator, from the vernal equinox.

277. The ecliptic paffes through the conftellations

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

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, OF 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° 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 $365^d\ 5^h\ 48'\ 45''$. This is called a TROPICAL year, to diſtinguiſh it from the interval $365^d\ 6^h\ 9'\ 11''$, 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 P V *p*, P A *p*, (fig. 36.) are drawn through the poles P, *p*, of the ecliptic. Theſe are called ECLIPTIC MERIDIANS. The arch A B of one of theſe circles, intercepted between a phenomenon A and the ecliptic, is called the LATITUDE of the phenomenon; and the arch V B, 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° 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° 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.

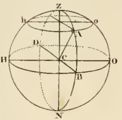


Fig. 31.

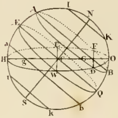


Fig. 32.

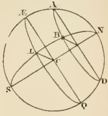


Fig. 33.

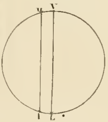


Fig. 33. Nº 2.

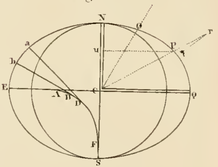


Fig. 34.

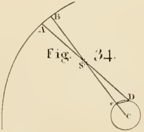


Fig. 35.

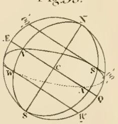
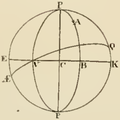


Fig. 36.



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 A *b* P *d* (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 E P to E A 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*. E C is the ECCENTRICITY, and P E is to P C 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,

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ſtronomers, 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″, $\frac{1}{30}$ 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.

294. From this he deduced a method of calculating the ſun's place for any given time. Draw a line E F from the focus of the ellipſe, which ſhall cut off a ſector A E F, 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 ſydereal 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 A B P D (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. B D is the conjugate axis, and C the centre. It is required to draw a line E T which ſhall cut off a ſector A E T, 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 E I, which ſhould cut off from the circumſcribed circle A *b* P *d* the area A E I, which is to the whole circle in the ſame proportion of *m* to *n*. For, then, drawing the perpendicular ordinate I R, cutting the ellipſe in T, he knew that the area A E T has the ſame proportion to the ellipſe that A E I has to the circle. The proof of this is eaſy, and it ſeems greatly to ſimplify the problem. Draw I C through the centre, and make E S perpendicular to I C S. The area A E I conſiſts of the circular ſector A C I, and the triangle C I E. The ſector is equal to half the rectangle of the radius C I and the arch A I, that is, to $\frac{CA \times IA}{2}$. The triangle C I E is equal to $\frac{CI \times ES}{2}$, or $\frac{CA \times ES}{2}$. Therefore it is evident that, if we make the arch I M equal to the ſtraight line E S, the ſector A C M will be equal to the circular area A C I, and the angle A C M 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 R T I; draw I C S and E S, and make I M = E S. Then, 360° is to the arch A M as the time of a revolution to the time in which the body moves over A T. This is (in the aſtronomical language) finding the mean anomaly when the true anomaly is given. The angle A C M, proportional to the time, is called the MEAN ANOMALY, and the angle A E T is the TRUE ANOMALY. The angle A C I 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 A M, fo as to leave A I of a proper magnitude, becaufe the indifpenfable meafure of M I, namely E S, cannot be had till I C S be drawn. Kepler faw this, and faid that his problem could not be folved geometrically. Since the invention of fluxions, however, and of converging feriefes, very accurate folutions have been obtained. That given by *Frifius* in his *Cofmographia* is the fame in principle with all the moft approved methods, and the form in which it is prefented is peculiarly fimple and neat. But, except for the conftruction of original tables, thefe methods are rarely employed, on account of the laborious calculation which they require. Of all the direct approximate folutions, that given by Dr Matthew Stewart at the end of his *Tracts, Phyfical and Mathematical*, publifhed in 1761, feems the moft accurate and elegant; and the calculations founded on it are even fhorter than the indirect methods generally employed. His conftruction is as follows.

298. Let the angle A E M be the mean anomaly, join EM, and draw C *i* parallel to it, and MO perpendicular to C *i*. If the orbit is not more eccentric than that of Mars, make the arch *i* I equal to the excefs of the arch M *i* above its fine M O. Then A I is the eccentric anomaly correfponding to the mean anomaly A M, and the ordinate I R will cut the ellipfe in T, fo that A E T will be the true anomaly required. The error will not amount to two feconds in any part of fuch orbits.

bits. But, for orbits of greater eccentricity, another ſtep is neceſſary. Join E i, and draw C Q parallel to E i, meeting the tangent i Q in Q. Let D repreſent the exceſs of the arch M i above its ſine M O *, and inſtitute the following analogy, ſin. M C i : tan. i C Q = D : i I, taking i I from i towards M. The point, I, will be ſo ſituated that the ſector A E I is very nearly equal to the ſector A C M, or A I is the eccentric anomaly correſponding to the mean anomaly A M. 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 I S, and M I. The triangles i C E and i C M are evidently equal, being on one baſe i C, and between the parallels i C and M E. For ſimilar reaſons, the triangles i S I and i E I are equal. Therefore the triangle i C E, together with the ſegment included between the arch M b i and the chord M i, will be equal to the circular ſector i C M.

Now it is plain, from the conſtruction, that $Si : Ci = SE : iQ, = MO : iQ, = \overline{Mbi} - \overline{MO} : iI$. 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 E S, or the ſeconds in E S, by adding this logarithm to the logarithms of E C (A C being unity), and the logarithm of the ſine of A C I. The ſum is the logarithm of the ſeconds in E S.

M bi is equal to twice the fector M C i, and C i × M O is equal to twice the triangle M C i. Therefore S i × i I is equal to twice the fegment contained between M b i and the chord M i. Therefore this fegment is equal to the triangle i S I, or to the triangle i E I. Therefore the fpace C i I E C is equal to the fector i C M, and the fector A E I to the fector A C M.

The calculation founded on this conftruction is extremely fimple. In the triangle M C E, the fides M C and C E are given, with the included angle M C E; and the angles C E M, C M E are fought. Moreover, A E is the fum of the given fides, and P E is their difference, and A C M is the fum 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, A C i and M C i, are obtained. In the next place, in the triangle i C E, the fides i C, C E, and the included angle i C E, are given, and the angle E i C is fought. We have, in the fame manner as before, A C i equal to the fum of the angles E and i, and therefore $AE : EP = \tan. \frac{E + i}{2} : \tan. \frac{E - i}{2}$. Thus the angle E i C, or its equal, i C Q, is obtained, and then, the arch $i\,I = D \times \frac{i\,Q}{M\,O}, = D \times \frac{\tan.\, i\,C\,Q}{\text{fin. } M\,C\,i}$.

In the very eccentric orbits of the comets, this brings us vaftly nearer to the truth than any of the indirect methods we know does by the firft ftep. So near indeed, that the common method, by the *rule of falfe pofition*, may now be fafely employed. If the point, I, has been accurately found,

found, it is plain that if to the arch A I we add E S, that is, E C × ſin. A C I, we obtain the arch A M with which we began. But if I has not been accurately determined, A M will differ from the primitive A M. Therefore, make ſome ſmall change on A I, and again compute A M. 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 A M are very nearly proportional to the variations of A I; 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 A P 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 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

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 $27^{d}\ 7^{h}\ 43'\ 12''$, but with ſome variations. Her mean daily motion is therefore $13°\ 10'\ 25''$, and her horary motion is $32'\ 56''$.

303. Her orbit is inclined to the plane of the ecliptic in an angle of $5°\ 8'\ 45''$, 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 $6803^{d}\ 2^{h}\ 55'\ 18''$, nearly $18\frac{2}{3}$ 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 thofe points, we fhall fee that the tape croffes the ecliptic more wefterly every turn, and then croffes the laft round very obliquely; and we fee that by continuing this operation, we fhall 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 increafing from 29′ 34″ to 36′ 48″, and the equation of the orbit fometimes amounts to 6° 18′ 32″; fo that if, fetting out from the point where her horary motion is floweft, we calculate her place, for the eighth day thereafter, at the rate of 32′ 56″ per hour, we fhall find her obferved place fhort of our calculation more than half a day's motion. And we fhould have found her as much before it, had we begun our calculation from the oppofite point of her orbit.

307. Her apparent diameter changes from 29′ 26″ to 33′ 47″, and therefore her diftance from the Earth changes. This diftance may be difcovered in miles by means of her parallax.

She was obferved, in her paffage over the meridian, by two aftronomers, one of whom was at Berlin, and the other at the Cape of Good Hope. Thefe two places are diftant from one another above 5000 miles; fo that the obferver at Berlin faw the Moon every day confiderably more to the fouth than the perfon at the Cape. This difference of apparent declination is the meaſure of the angle D S C (fig. 34.) ſubtended at the Moon by the line *c* D of 5443 miles, between the obſervers. The angles S D *c* and S *c* D are given by means of the Moon's obſerved altitudes. Therefore any of the ſides S D or S *c* 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 apſides 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

pofite to the Sun. In this fituation, fhe is a complete luminous circle, and is faid to be FULL. As fhe moves eaftward, fhe becomes deficient on the weft fide, and, after about $7\frac{1}{3}$ days, comes to the meridian about fix in the morning, having the appearance of a femicircle, with the convex fide next the Sun. In this ftate, her appearance is called HALF MOON. Moving ftill eaftward, fhe becomes more deficient on the weft fide, and has now the form of a crefcent, with the convex fide turned towards the Sun. This crefcent becomes continually more flender, till, about 14 days after being full, fhe is fo near the Sun that fhe cannot be feen, on account of his great fplendour. About four days after this difappearance in the morning before funrife, fhe is feen in the evening, a little to the eaftward of the Sun, in the form of a fine crefcent, with the convex fide turned toward the Sun. Moving ftill to the eaftward, the crefcent becomes more full, and when the Moon comes to the meridian about fix in the evening, fhe has again the appearance of a bright femicircle. Advancing ftill to the eaftward, fhe becomes fuller on the eaft fide, and, at laft, after about $29\frac{1}{2}$ days, fhe is again oppofite to the Sun, and again full.

312. It frequently happens that the Moon is ECLIPSED when full; and that the Sun is eclipfed fome time between the difappearance of the Moon in the morning on the weft fide of the Sun, and her reappearance in the evening on the eaft fide of the Sun. This eclipfe 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 E M (fig. 39.) joining the Earth and Moon, is perpendicular to the line M S, joining the Moon and Sun. By obſerving the angle S E M, 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 gueſs 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 S E M 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 S E M 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 S E M 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

is owing to the Sun's change of place during a revolution of the Moon. Suppofe it new Moon, and therefore the Sun and Moon appearing in the fame 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 fomewhat more than two days muft elapfe before the Moon can overtake the Sun, fo as to be feen by us as new moon.

318. The period of this fucceffion of phafes may be found within a few hours of the truth in a very fhort time. We can tell, within four or five hours, the time of the Moon being half illuminated. Suppofe this obferved in the morning of her laft quarter. We fhall fee this twice repeated in 59 days, which gives 29d 12h for a lunation, wanting about three fourths of an hour of the truth. About 433 years before the Chriftian æra, Meton, a Greek aftronomer, reported to the ftates affembled at the Olympic games, that in nineteen years there happened exactly 235 lunations.

319. The lunar motions are fubject to feveral 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 greateft difference being about 24 minutes. Tycho Brahé firft 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 lefs 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 apfides; and the equation of the orbit is then increafed nearly 1° 20′ 34″. This change is called the EVECTION. It was difcovered by Ptolemy.

324. 5. The inclination of the orbit changes.

325. 6. The moon's motion is retarded in the firft and third quarters, and accelerated in the fecond and laft. This anomaly was difcovered by Tycho Brahé, who calls it the VARIATION.

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

Of the Calendar.

327. Aftronomy, like all other fciences, was firft practifed as an art. The chief object of this art was to know the feafons, which, as we have feen, depend either

 immediately,

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*, THOTH, 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 $365^{d}\ 6^{h}\ 9'\ 11''$, 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

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 KALENDARIUM.

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

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 luniſolar year than the Egyptians. Solon got an oracle to direct his Athenians (594 years before our æra), θυεῖν κατα τρια, κατα Ἡλιον, κατα Σελῆνην, 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 τριακις, 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, becaufe he arranged the interpolation of a month fo as never to remove the firft 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 Chriftian æra, at 43 minutes paft 7 in the morning, that being the time of new Moon. The firft year of each cycle is that in which the full Moon of its firft month is the neareft to the fummer folftice.

334. The Roman kalendar was in a much worfe condition than the rudeft of the Greeks. The fuperftitious veneration for their ceremonies, or their paffion for public fports, had diverted the attention of the Romans (who never were cultivators or graziers) from the feafons altogether. They were contented with a year of ten months for feveral centuries, and had the moft abfurd contrivances for producing fome conformity with the feafons. At laft, that accomplifhed general, Julius Cæfar, having attained the height of his vaft ambition, refolved to reform the Roman kalendar. He was profoundly fkilled in aftronomy, and had written fome differtations on different branches of the fcience, which had great reputation, but are now loft. He had no fuperftitious or religious qualms to difturb him, and was determined to make every thing yield to the great purpofe of a kalendar, its ufe in directing the occupations of the people, and for recording the events of hiftory. 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

chofe for the beginning of his kalendar, a year in which there was a new Moon following clofe upon the winter folftice. This opportunity was afforded him in the fecond year of his dictatorfhip, and the 707th year from the foundation of Rome. He found that there would be a new Moon 6 days after the winter folftice. He made this new Moon the 1ft of January of his firft year. But, to do this, he was obliged to keep the preceding year dragging on 90 days longer than ufual, containing 444

 days,

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branch of the mifelto of the oak, called Ghiah in Celtic, and carried it in triumph to the facred grove. The people cut for themfelves, and carried home their prize, confecrated by the druid. At prefent, the pews of our churches, and even the chambers of our cottages, are ornamented with this plant at Chriftmas. In France, till within thefe 150 years, there were ftill more perceptible traces. A man perfonating a prince (*Roi follet*) fet 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 difguife (Guifards) forced into the houfes, 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 fuch riots, that the Bifhop of Soiffons reprefented the enormities to Louis XIV., and the practice was forbidden. May not the guifearts of Edinburgh, with their cry of " Hog menay, troll lollay ; gie's your white bread, none of your gray, " be derived from this ?

days, inftead of the old number 354. As all thefe days were unprovided with folemnities, the year preceding Cæfar's kalendar was called *the year of confufion*. Cæfar alfo, for a particular reafon, chofe to make his firft year confift of 366 days, and he inferted the intercalary day between the 23d and 24th of February, choofing that particular day, as a feparation of the luftrations and other piaculums to the infernal deities, which ended with the 23d, from the worfhip of the celeftial deities, which took place on the 24th of February. The 24th was the *fextus ante kalendas neomenias Martias*. His inferted day, anfwering civil purpofes alone, had no ceremonies, nor any name appropriated to it, and was to be confidered merely as a fupernumerary *fextus ante kalendas*. Hence the year which had this intercalation was ftyled an *annus biffextilis*, a biffextile year. With refpect to the reft of the year, Cæfar being alfo Pontifex Maximus (an office of vaft political importance), or rather, having all the power of the ftate in his own perfon, ordered that attention fhould be given to the days of the month only, and that the religious feftivals alone fhould be regulated by the facred college. He affigned to each month the number of days which has been continued in them ever fince.

335. Such is the fimple kalendar of Julius Cæfar. Simple however as it was, his inftructions were mifunderftood, or not attended to, during the horrors of the civil wars. Inftead of intercalating every fourth year, the intercalation was thrice made on every fucceeding 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 365¼ 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

ufed by the Chriftian world. Any event happening in this year is dated *anno Domini primo*. As Cæfar had made his firft year a biffextile, the year of the nativity was alfo biffextile; and the firft year of our æra begins the fhort cycle of four years, fo that the fourth year of our æra is biffextile.

That we may connect this æra with all the others employed by aftronomers or hiftorians, it will be enough to know that this firft year of the Chriftian æra is the 4714th of the Julian period.

It coincides with the fourth year of the 194th Olympiad till midfummer.

It coincides with the 753d *ab urbe condita*, till April 21st.

It coincides with the 748th of Nabonaffar till Auguft 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 aftronomical tables, the years before the Chriftian æra are counted backwards, calling the year of the nativity 0, the preceding year 1, &c. But chronologifts more frequently reckon the year of the nativity the firft before Chrift. Thus,

Years of Cæfar . . .	41,	42,	43,	44,	45,	46,	47,	48,	49
Aftronomers	4,	3,	2,	1,	0,	1,	2,	3,	4
Chronologifts	5,	4,	3,	2,	1,	1,	2,	3,	4

This kalendar of Julius Cæfar has manifeft advantages in refpect of fimplicity, and in a fhort time fupplanted

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-fecond. 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° D[i] 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-fecond 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

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 indiction, 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 alfo ufed for facilitating the calculation of new and full moon in a grofs way. The epact is nearly the moon's age on the 1ft 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 1ft of January, and the day of the year correfponding to any day of a month, it is eafy to find the Moon's age on that day, by dividing the double of the fum of this number and the epact by 59. The half remainder is nearly the Moon's age.

Although thefe rude computations do not correfpond with the motions of the two luminaries, they deferve notice, being the methods employed by the rules of the Church for fettling the moveable Church feftivals.

Of the proper Motions of the Planets.

343. The planets are obferved to change their fituations in the ftarry heavens, and move among the figns of the zodiac, never receding far from the ecliptic.

Their motions are exceedingly irregular, as may be feen by fig. 65. A, which reprefents the motion of the planet Jupiter, from the beginning of 1708 to the beginning of 1716. E K reprefents the ecliptic, and the initial letters of the months are put to thofe points of the apparent path where the planet was feen on the firft 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 oppofition to the Sun, when in the middle of each loop. His regrefs in each loop is about 10 degrees, and his progreffive motion is continued about 40°. He gradually approaches the ecliptic, croffes it, deviates to the fouthward, then returns towards it; croffes it, about fix years after his former croffing, and in about twelve years comes to where he was at the beginning of thefe obfervations.

344. The other planets, and particularly Venus and and Mercury, are ftill more irregular in their apparent motions, and have but few circumftances of general refemblance.

The firft *general* remark which can be made on thefe intricate motions is, that a planet always appears largeft when in the points R, R, R, which are in the middle of its retrograde motions. Its diameter gradually diminifhes, and becomes the leaft 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 neareft to the Earth when in the middle of its retrograde motion, and fartheft from it when in the middle of its direct motion.

It may alfo 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 alfo in conjunction with the Sun when in the middle of their retrograde motions. But the planets Mars, Jupiter, and Saturn, are always in oppofition 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 *q r s* (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 Q A L. The circle Q A L is called the DEFERENT CIRCLE, and *q r s* 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

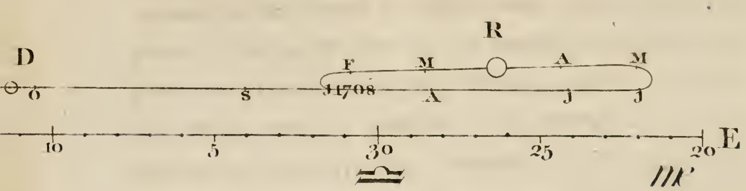
ſucceſſive

fucceffive oppofitions of Jupiter, (for example), was obferved to be variable, being always lefs in a certain part of the zodiac, and gradually increafing to a maximum ftate in the oppofite part of the zodiac.

In order to correfpond 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 aftronomers placed the Earth not in, but at a certain diftance from, the centre of the deferent; fo that an equal arch between two fucceeding oppofitions fhould fubtend a fmaller angle, when it is on the other fide of that centre. Thus, the unequal motion of the epicycle was explained in the fame 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 cafe of the Sun's orbit (292.) In this manner, they at laft compofed a fet of motions which agreed tolerably well with obfervation.

The celebrated geometer Apollonius gave very judicious directions how to proportion the epicycle to the deferent circle. But they feem not to have been attended to, even by Ptolemy; and the aftronomers remained very ignorant of any method of conftruction which agreed fufficiently with the phenomena, till about the thirteenth century, when the doctrine of epicycles was cultivated with more care and fkill.

A very full and diftinct account is given of all the ingenious contrivances of the ancient aftronomers for explaining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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 waneing 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.

Then $b = \frac{a\,t}{a + t}$, and $a = \frac{b\,t}{t - b}$.

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 $\frac{1}{b} = \frac{1}{t} + \frac{1}{a}$, $= \frac{a + t}{a\,t}$, and $b = \frac{a\,t}{a + t}$. Also $\frac{1}{a} = \frac{1}{b} - \frac{1}{t}$, $= \frac{t - b}{t\,b}$, and $a = \frac{b\,t}{t - b}$.

We may also calculate the synodical period t, when we know the real periods of each. For $\frac{1}{t} = \frac{1}{b} - \frac{1}{a} = \frac{a - b}{a\,b}$, and $t = \frac{a\,b}{a - b}$.

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° 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

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 pofition of Venus, as feen from the Sun, for any given inftant.

This however requires more obfervations of this kind than we are yet poffeffed of, becaufe her conjunctions happen fo nearly in the fame points of her orbit, that great part of it is left without obfervations of this kind. But we have other obfervations of almoft equal value, namely, thofe of her greateft elongations from the Sun. There is none of the planets, therefore, of which the equations (which indeed are very fmall) are more accurately determined.

351. We can now determine the form and pofition of the orbit. For we can *obferve* the place of the Sun, or the pofition of the line E S (fig. 41.), joining the Earth and Sun. We know the length of this line (291.) We can *obferve* the GEOCENTRIC place of Venus, or the pofition of the line E D joining the Earth and Venus. And we can compute (350.) the HELIOCENTRIC place of Venus, or the pofition of the line S C joining Venus and the Sun. Venus muft be in V, the interfection of thefe two lines; and therefore that point of her orbit is determined.

352. By fuch obfervations Kepler difcovered that the orbit of Venus is an ellipfe, having the Sun in one focus, the femitranfverfe axis being 72333, and the eccentri-

city

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° 44′ 46″ in a century.

355. The orbit of Venus is inclined to the ecliptic at an angle of 3° 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° 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 $87^{d}\ 23^{h}\ 15'\ 37''$; and areas are deſcribed proportional to the times.

Of the proper Motions of the Superior Planets.

358. Mars, Jupiter, and Saturn, exhibit phenomena confiderably different from thofe 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 feen croffing the Sun's difk. Hence we infer, that their orbits include both the Sun and the Earth.

2. They are always retrograde when in oppofition, and direct when in conjunction.

The planet Jupiter may ferve as an example of the way in which their real motions may be inveftigated.

359. Jupiter is an opaque body, vifible by means of the reflected light of the Sun. For the fhadows of fome of the heavenly bodies are fometimes obferved on his difk, and his fhadow frequently falls on them.

360. His apparent diameter, when in oppofition, is about 46″, and, when in conjunction, it is about 31″, and his difk is always round. Hence we infer, that he is neareft when in oppofition, and that his leaft and greateft diftance 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 $\alpha \beta \varkappa \delta \varepsilon \varphi \gamma \chi \alpha$ 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 α, β, $\varkappa$, δ, ε, φ, γ, χ, α, make A a parallel to T α, and B b parallel to

T β,

T β, and C *c* parallel to T $\varkappa$, &c., and make thefe 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, $\varkappa$, &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 abfolute fpace between two fucceeding oppofitions 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 arifing from this conftruction will very nearly agree with the obfervation. Only we may obferve that the oppofitions which fucceed each other near the conftellation Virgo, are lefs diftant from one another than thofe obferved in the oppofite part of the heavens; fo that the centre of the epicycle feems to move flower in the firft cafe than in the laft. To reconcile this with the perfect uniformity of the motion of that centre in the circumference of the deferent circle, the ancient aftronomers faid that the earth was not exactly in the centre of the deferent, but fo placed that the equable motion of the centre of the epicycle appeared flower, becaufe 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 obfervations, and really differed very little from them. Copernicus fhews that their hypothefis for Jupiter never deviates more than half a degree from obfervation, if it be properly employed. They found that the epicycle moved round the deferent

ferent in $4332\frac{1}{3}$ days, with an equation gradually increafing to near 6 degrees; fo 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 aftronomers had no fuch data for determining the abfolute magnitude of the deferent circles and epicycles for the fuperior planets, as Mercury and Venus afforded them. The rules given them by Apollonius only taught them what proportion the epicycle of each planet muft have to its deferent circle, but gave no information as to the abfolute magnitude of either, or the proportion between the deferent circles of any two fuperior planets. Accordingly, no two ancient aftronomers agree in their meafures, farther than in faying 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 fizes fufficiently different, fo that the epicycles of two neighbouring planets fhould not crofs and juftle each other. Yet they might eafily have come very near the truth, by a fmall and very allowable addition to their hypothefis of epicyclical motion, namely, by fuppofing that the epicycle of each planet is equal to the Sun's orbit. This was quite allowable.

364. If we do this, we fhall deduce confequences that are very remarkable, and which would have put the

ancient aſtronomy on a footing very near to perfection. For, if C c (fig. 40.) be not only parallel to T κ, but alſo equal to it, then C T κ c is a parallelogram, and κ c is equal and parallel to T C. 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, α a is equal to T A; β b to T B; κ c to T C, &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 A a to T α, of B b to T β, &c., that whatever eccentricity, or whatever form it has been thought neceſſary to aſſign to the deferent, the diſtances α a, β b, κ c, &c. will ſtill be reſpectively equal to T A, T B, T C, &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 apſides (362.) keeping parallel to itſelf, that is, in our figure, to T A. 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 C c to T $\varkappa$ 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 $\varkappa c$ is found parallel to T C.

366. We now can tell the preciſe point in which Jupiter is found in any moment of time. Having made the radius T $\varkappa$ to the radius T A in the due proportion of 10 to 52, and having placed the Earth at the proper diſtance from the centre of the deferent Q A L, we can calculate (298.) the poſition and length of the line T $\varkappa$ joining the Earth with the Sun. We can draw the line T C 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 $\varkappa$ V parallel to it. This muſt paſs through Jupiter, or Jupiter muſt be ſomewhere in this line. We obſerve Jupiter, however, in the direction T Z. Jupiter muſt there-

 fore

fore be in the interfection c of the lines $\varkappa$ V and T Z. And then we can meafure $c\varkappa$, Jupiter's diftance from the Sun.

367. Kepler, by taking this method with a feries of obfervations made by Tycho Brahé, difcovered that Jupiter was always found in the circumference of an ellipfe, having the Sun in its focus. Its femitranfverfe axis is 520098, the mean diftance of the Earth from the Sun being fuppofed 100000. Its eccentricity is 25277. Its inclination to the ecliptic is 1° 20′, and the nodes move eaftward about 1′ in a year.

368. The revolution in this orbit is completed in $4332\frac{1}{3}$ days, and areas are defcribed proportional to the times.

369. Proceeding in the fame manner, we difcover that the planets Mars, Saturn, and the one difcovered by Dr Herfchel in 1781, are always found in the circumference of ellipfes, with the Sun in one focus, and defcribe round him areas proportional to the times.

The chief circumftances of their motions are ftated as follows:

	Mean Diftance.	*Eccentricity.*	*Period in Days.*
Georgian planet	1908584	90738	30456,07
Saturn - - - -	953941	53210	10759,27
Mars - - - - -	152369	14218	686,98

370.

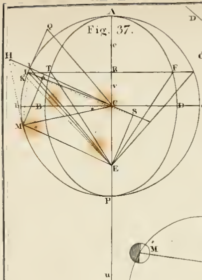

Fig. 37.

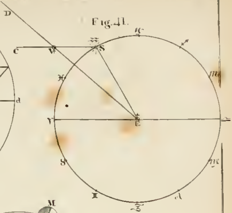

Fig. 41.

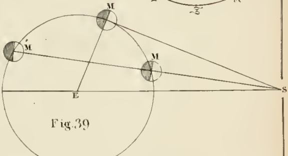

Fig. 39

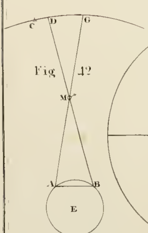

Fig 42

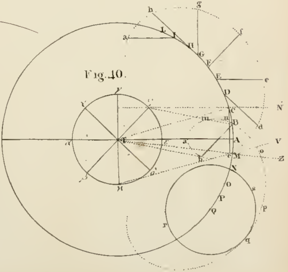

Fig. 40.

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 *q r s* as

having

370. Two other bodies have been lately detected in the planetary regions, revolving round the Sun in orbits which do not feem very eccentric, and feem placed between thofe of Mars and Jupiter. The firft was obferved in 1801 by Mr Piazzi of Palermo, and by him named Ceres. The other was difcovered in 1802 by Mr Olberg of Bremen, who has called it Pallas. They are exceedingly fmall, and we have feen too little of their motions as yet to enable us to ftate their elements with any precifion.

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372. But now, inftead of fuppofing that the centre of a fmall epicycle is carried round the circumference of a greater deferent circle, different for each planet, we may rather confider the Sun's orbit round the Earth as the only deferent circle, and fuppofe that the planets defcribe 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 ftill the fame looped curves in both cafes. For, in either cafe, the motion of a planet is compounded of the fame motions. But the latter fuppofition is much more probable. We can fcarcely conceive the motion of Jupiter in the epicycle *q r s* as

having

having any phyfical relation to its centre, a mere mathematical point of fpace. We cannot confider this point as having any phyfical properties that fhall influence the motions of the planet. This point alfo is fuppofed 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 circumftance. This combination of circles, therefore, cannot be confidered as any thing but a mere mathematical hypothefis, to furnifh fome means of calculation, or for the delineation of the looped path of the planet. Accordingly, the firft propofers of thefe epicycles, fenfible of the mere nothingnefs of their centre, and the impoffibility of a nothing moving in the circumference of a circle, and drawing a planet along with it, farther fuppofed that the epicycles were vaft folid tranfparent globes, and that the planet was a luminous point or ftar, fticking in the furface of this globe. And, to complete the hypothefis, they fuppofed that the globe turned round its centre, carrying the planet round with it, and thus produced the direct and retrograde motions that we obferve. Ariftotle taught that this motion was effected by the genius of the planet refiding 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 philofophers fuppofed that the deferent was alfo a vaft cryftalline, or, at leaft, tranfparent material fpherical fhell, turning round the earth, and that this fhell was of fufficient thicknefs to receive the epi-

cyclic

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 caculation 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 deſcrip-

tion

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 G D of the obſerved diſtances C G, C D, will give the angle D M G, or its equal A M B. The angles M A B and M B A are given by obſervation, and the line A B is given; and therefore A M, and conſequently E M, 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 C D (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 ſha-

 dow,

dow, and by other methods. They are obferved to defcribe ellipfes, having Jupiter in one focus; and they defcribe areas round Jupiter, which are proportional to the times. Alfo the fquares of their periods are in the proportion of the cubes of their mean diftances from Jupiter.

378. It has been difcovered by means of the eclipfes of Jupiter's fatellites, that light is propagated in time, and employs about 8′ 11″ in moving along a line equal to the mean diftance of the Earth from the Sun.

The times of the revolutions of thefe little bodies had been ftudied with the greateft care, on account of the eafy and accurate means which their frequent eclipfes gave us for afcertaining the longitudes of places. But it was found that, after having calculated the time of an eclipfe in conformity to the periods, which had been moft accurately determined, the eclipfe happened later than the calculation, in proportion as Jupiter was farther from the Earth. If an eclipfe, when Jupiter is in oppofition, be obferved to happen precifely at the time calculated; an eclipfe three months before, or after, when Jupiter is in quadrature, will be obferved to happen about eight minutes later than the calculated time. An eclipfe happening about fix weeks before or after oppofition, will be about four minutes later than the calculation, when thofe about the time of Jupiter's oppofition happen at the exact time. In general, this *retardation* of the eclipfes is obferved to be exactly proportional to the increafe 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 $16\frac{1}{3}$ 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 diſperſer 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^s 20°, or 5^s 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 Coſmogoniſts, 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

Of the Rotation of the Heavenly Bodies.

384. In 1611, Scheiner, profeffor at Ingolftadt, obferved fpots on the difk of the Sun, which come into view on the eaftern limb, move acrofs his difk in parallel circles, difappear on the weftern limb, and, after fome time, again appear on the eaftern limb, and repeat the fame motions. Hence it is inferred that the Sun revolves from weft to eaft in the fpace of $25^d\ 14^h\ 12'$, round an axis inclined to the plane of the ecliptic $7\frac{1}{2}$ degrees, and having the afcending node of his equator in longitude $2^s\ 10°$.

Philofophers have formed various opinions concerning the nature of thefe fpots. The moft probable is, that the Sun confifts of a dark nucleus, furrounded by a luminous covering, and that the nucleus is fometimes laid bare in particular places. For the general appearance of a fpot during its revolution is like fig. 43.

385. A feries of moft interefting obfervations has been lately made by Dr Herfchel, by the help of his great telefcopes. Thefe obfervations are recorded in the Philofophical Tranfactions for the years 1801 and 1802. They lead to very curious conclufions refpecting the peculiar conftitution of the Sun. It would feem that the Sun is immediately furrounded by an atmofphere, heavy and tranfparent, like our air. This reaches to the height of feveral thoufand miles. On this atmofphere feems 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 obfervations, firft made by Dr Hooke in 1664, on fpots in the difk of Jupiter, fhow that he revolves from weft to eaft in $9^h\ 56'$, round an axis inclined to the plane of his orbit $2\frac{1}{2}°$. It is alfo obferved that his equatoreal diameter is to his axis nearly as 14 to 13.

389. There are fome remarkable circumftances in the rotation of this planet. The fpots, by whofe change of place on the difk we judge of the rotation, are not permanent, any more than thofe obferved on the Sun's difk. We muft therefore conclude that, either the furface of the planet is fubject to very confiderable variations of brightnefs, or that Jupiter is furrounded by a cloudy atmofphere. The laft is, of itfelf, the moft probable; and it becomes ftill more fo from another circumftance. There is a certain part of the planet that is fenfibly brighter than the reft, and fometimes remarkably fo. It is known to be one and the fame part by its fituation. This fpot turns round in fomewhat lefs time than the reft. That is, if a dark fpot remains during feveral revolutions, it is found to have feparated a little from this bright fpot, to the left hand, that is, to the weftward. There is a minute or two of difference between the rotation of Jupiter, as deduced from the fucceffive appearances of the bright fpot, and that deduced from obfervations made on the others.

390. Thefe circumftances lead us to imagine that Jupiter is really covered with a cloudy atmofphere, 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 fo ill defined, and fo fubject to confiderable changes, that it was long before aftronomers could be certain of a rotation, fo as to afcertain the time. Dr Herfchel has been at much pains to do this with accuracy, and, by comparing many fucceffive apparitions of the fame 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° 18′ with his own orbit.

392. It is midfummer-day in Mars when he is in long. 11^s 19° from our vernal equinox. As the planet is of a very oblate form, and probably hollow, there may be a confiderable preceffion of his equinoctial points, by a change in the direction of his axis.

393. Being fo much inclined to the ecliptic, the poles of Mars come into fight in the courfe of a revolution. When either pole comes firft into view, it is obferved to be remarkably brighter than the reft of the difk. This brightnefs gradually diminifhes, and is generally altogether gone, before this pole goes out of fight by the change of the planet's pofition. The other pole now comes into view, and exhibits fimilar appearances.

394. This appearance of Mars greatly refembles what our own globe will exhibit to a fpectator placed on Venus or Mercury. The fnows 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 $\frac{1}{24}$ 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 obferved in Mercury, on account of his apparent minutenefs; nor is any obferved in the Georgian planet for the fame reafon.

399. Many philofophers have imagined that the Earth revolves round its axis in $23^h\ 56'\ 4''$ from weft to eaft: and that this is the caufe of the obferved diurnal motion of the heavens, which is therefore only an appearance. It muft be acknowledged that the appearances will be the fame, and that we muft be infenfible of the motion. There are alfo many circumftances which render this rotation very probable.

400. 1. All the celeftial motions will be rendered incomparably more moderate and fimple. If the heavens really turn round the Earth in $23^h\ 56'\ 4''$, the motion of the Sun, or of any of the planets, is fwifter than any motion of which we have any meafure; and this to a degree almoft beyond conception. The motion of the Sun would be 20,000 times fwifter 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 fwifteft motion neceffary for the appearances is that of the Earth's equator, which does not exceed that of a cannon ball.

The motions alfo become incomparably fimpler. For the combination of diurnal motion with the proper motion of the planets makes it vaftly more complex, and impoffible to account for on any mechanical principles.

This

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 obferve phenomena on our globe that are neceffary confequences of rotation, but cannot be accounted for without it. We know that the equatoreal regions are about twenty miles higher than the circumpolar; 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 fo fwift, that the waters would all flow toward the equator, and inundate the torrid zone; nay, fo fwift, that every thing loofe would be thrown off, as we fee the water difperfed from a twirled mop. Now, a very fimple calculation will fhew us that a rotation in $23^h\ 56'$ is precifely what will balance the tendency of the waters to flow from the elevated equator towards the poles, and will keep it uniformly fpread over the whole fpheroid. We alfo obferve that a lump of matter of any kind weighs more (by a fpring fteelyard) at Spitzbergen than at Quito, and that the diminution of gravity is precifely what would arife from the fuppofed rotation, viz. $\frac{1}{229}$.

There are arguments which give the moft convincing demonftration of the Earth's rotation.

404. 1. Did the heavens turn round the Earth, as has long been believed, it is almoft certain that no zodiacal fixed ftar could be feen by us. For it is highly probable that light is an emiffion of matter from the luminous body. If this be the cafe, fuch is the diftance of any fixed ftar A (fig. 44.) that, when its velocity A C is compounded with the velocity of light emitted in

any

any direction A B, or A *b*, it would produce a motion in a direction A D, 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 equatoreal 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° 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

of

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

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 *a* equal to T α. Jupiter is in *a*, and his bearing and distance from the Earth is T *a*, nearly $\frac{4}{5}$ of T A. Six weeks after, the Sun is in β; the centre of Jupiter's epicycle is in B. Draw B *b* equal and parallel to T β, and *b* is now the place of Jupiter, and T *b* 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 *a* T *b*. 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 A B E L 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 *a* being equal to ε T, we have ε A, the distance of Jupiter from the Earth, equal to T *a* of the former hypothesis. Six weeks after, the Earth is at φ, and Jupiter at B. Join φ B, and draw φ Ṅ parallel to T A. It is evident that the distance φ B of Jupiter from the Earth, is equal to the distance T *b* of the former construction. Also the angle Ṅ φ B, which is Jupiter's change of bearing, (by the astronomer's compass, the ecliptic), is equal to the angle

a T *b*

a T b of the former conftruction. Jupiter therefore, inftead of moving to the left hand, has moved to the right, or weftward, and has diminifhed his ecliptical bearing or longitude by the degrees in the angle N′ φ B.

409. In the fame manner may the apparent motion of Jupiter be afcertained for every fituation of the Earth and Jupiter; and it will be found that, in every cafe, the line correfponding to φ B is equal and parallel to the line correfponding to T b; thus γ C is equal and parallel to T c; χ D is equal and parallel to T d, &c.

The apparent motions of the planets are therefore precifely the fame in either hypothefis, fo that we are left to follow either opinion, as it appears beft fupported by other arguments.

410. Accordingly, it has been the opinion of fome philofophers, 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 fame manner as Jupiter and Saturn are by their fatellites.

The following are the reafons for preferring this opinion to that contained in the 371st and 373d articles, which equally explains all the phenomena hitherto mentioned, and is more confiftent with our firft judgements.

411. 1. The celeftial motions become incomparably more fimple, and free of thofe looped contortions which

muft

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 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° 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-

 moſt

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 1200dth 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 ſurpriſed 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

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.

PHYSI-

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Fig. 43.

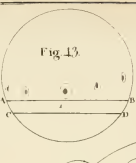
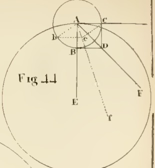
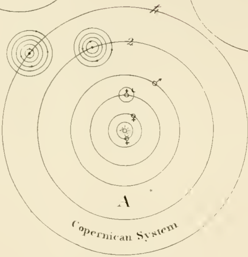


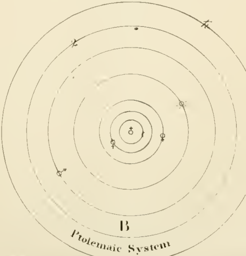
Fig. 44.





A

Copernican System



B

Ptolemaic System

PHYSICAL ASTRONOMY.

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428. It 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

PHYSICAL ASTRONOMY.

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with

with all thofe peculiarities of motion that diftinguifh the individuals of the magnificent affemblage.

We are now in a condition to inveftigate the particular characters of thofe extenfive powers of nature, thofe mechanical affections of matter, which caufe the obferved deviations from that uniform rectilineal motion which would have been obferved in every body, had it been under no mechanical influence. And we fhall alfo be able to explain or account for the diftinguifhing peculiarities of motion which characterife the individuals of the fyftem, if we fhall fo far fucceed in our firft inveftigation as to fhew that no other force operates in the fyftem, and that thefe peculiarities are only particular and accurately narrated cafes of the three general laws, precifely conformable to their legitimate confequences. *

In

* I think it neceffary here to forewarn the well-informed mathematician, if any fuch fhall honour thefe pages with a perufal, that he will be difappointed if he look for any thing profound, or curious, or new, in what follows. My fole aim is to affift the ignorant in the elements of phyfical aftronomy; and I mean to infert nothing but what feems to me to be elementary in the Newtonian philofophy. This ftudy requires (I think) a few more fteps than are ufually given in the elementary publications of this country. Thefe performances generally leave the ftudent too fcantily prepared for reading the valuable works on this fubject, unlefs by a very obftinate and fatiguing ftudy. They are deterred by the great difficulties

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,

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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

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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 perdidi 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.

Even

Even this would not correfpond with tolerable exactnefs with the obferved 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 fpace was almoft crammed full of folid matter; and after all thefe efforts, fome mathematicians affirmed that there are motions in the heavens that are neither uniform nor circular, nor can be compounded of fuch motions. If fo, this fpherical machinery is impoffible. In modern times, Tycho Brahé proved beyond all contradiction that the comet of 1574 paffed through all thofe fpheres, and therefore their exiftence was a mere fiction.

One fhould think the whole of this contrivance fo artlefs and rude, that we wonder that it ever obtained the leaft credit; yet was it adopted by the prince of ancient philofophers,—by Ariftotle; and his authority gave it poffeffion of all the fchools till modern times.

But where, all this while, is the mover of all this machinery? Ariftotle taught that each globe was conducted, or turned round its axis, by a peculiar genius or dæmon. This was worthy of the reft; and when fuch affertions are called *explanations*, nothing in nature need remain unexplained. We muft however do Hipparchus and Ptolemy the juftice to fay that they never adopted this hypothefis of Eudoxus and Callippus; they did not fpeculate about the caufes, but only endeavoured to afcertain the motions; and their epicycle and deferent circles are given by them merely as fteps of mathematical contemplation, and in order to have fome principle

to

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 ὁϱπης ψυχη 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 obferve in the motions of magnetical and electrical bodies, or in the motion of common heavy bodies, where one body feems to influence the motion of another at a diftance from it, without any intervening impulfion. It is thus that a ftone is bent continually from the line of its direction towards the Earth. In the fame manner, an iron ball, rolling along a level table, will be turned afide toward a magnet, and, by properly adjufting the diftance and the velocity, the ball may be made to revolve round the pole of the magnet. Many of the ancients faid that the curvilineal motions of the planets were produced by *tendencies* to one another, or to a common centre. Among the moderns, Fermat is the firft who faid in precife terms that the weight of a body is the fum of the tendencies of each particle to every particle of the Earth. Kepler faid ftill more exprefsly, that if there be fuppofed two bodies, placed out of the reach of all external forces, and at perfect liberty to move, they would approach each other, with velocities inverfely proportional to their quantities of matter. The Moon (fays he) and the Earth mutually attract each other, and are prevented from meeting by their revolution round their common centre of attraction. And he fays 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 Colchefter, that the Earth is a great magnet, he explains how this mutual attraction will produce a deflection into a curvilineal path, and adds, '*Veritatis* '*in me fit amor an gloriæ, loquantur dogmata mea, quæ ple-*

'*raque ab aliis accepta fero. Totam aſtronomiam Coper-*
'*nici hypotheſibus de mundo, Tychonis vero Brahei obſerva-*
'*tionibus, denique Gulielmi Gilberti Angli philoſophiæ mag-*
'*neticæ inædifico.*' EPIT. ASTR. COPERN.

433. The moſt expreſs ſurmiſe 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 gra-"vitation 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 is confidered as the mark and meafure 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 thefe principles. A ball fufpended by a long thread from the ceiling, was made to fwing round another ball laid on a table immediately below the point of fufpenfion. When the pufh given to the pendulum was nicely adjufted to its deviation from the perpendicular, it defcribed a perfect circle round the ball on the table. But when the pufh was very great, or very fmall, it defcribed an ellipfe, having the other ball in its centre. Hooke fhewed that this was the operation of a deflecting force proportional to the diftance from the other ball. He added, that although this illuftrated the planetary motions in fome degree, yet it was not fuitable to their caufe. For the planets defcribe ellipfes having the Sun, not in the centre, but in the focus. Therefore they are not retained by a force proportional to their diftance from the Sun. This was ftrict reafoning, from good pinciples. It is worthy of remark, that in this clear, and candid, and modeft expofition of a rational theory, he anticipated the difcoveries of Newton, as he anticipated, with equal diftinctnefs and precifion, the difcoveries of Lavoifier, a philofopher inferior perhaps only to Newton.

Thus we fee that many had noticed certain points of refemblance between the celeftial motions and the motions of magnets and heavy bodies. But thefe obfervers let the remark remain barren in their hands, becaufe 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 de-"flecting 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 fyftem of univerfal mechanics. Dr Halley was even obliged to correct the manufcript, to get the figures engraved, and, finally, to take charge of the printing and publication. Newton employed but eighteen months to compofe this immortal work. It was publifhed at laft, in 1687, under the title of *Mathematical Principles of Natural Philofophy*, and will be accounted the facred oracles of natural philofophy as long as any knowledge remains in Europe.

437. It is plain, that in this procefs of inveftigation, in order to explain the planetary motions by means of our knowledge of motions that are more familiar, Newton was obliged to fuppofe that the planets confift of common matter, in which we infer the nature of the moving caufe from the motions that we obferve. Newton's firft ftep, therefore, was a fcrupulous obfervation of the celeftial motions, knowing that any miftake with regard to thefe muft bring with it a fimilar miftake with regard to the natural power inferred from it. Every force, and every degree of it, is merely a philofophical interpretation of fome change of motion according to the Copernican fyftem. The Earth is faid to gravitate toward the Sun, becaufe, and only becaufe fhe defcribes 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 reafon, a doubt was expreffed (415.), whether the Newtonian difcoveries were ufed with propriety as arguments for the truth of the Copernican fyftem.

Moft

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.

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* 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 $=V^2c:v^2C$. 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.) F A is perpendicular to the tangent in A, and F N is perpendicular to the tangent P N. 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 P O is half the deflective chord at P, and (§ 8.) that P R is half the principal parameter L. Moreover, the triangles F N P and P Q O and P Q R 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, inverſely as the ſquare of the diſtance from F.

441. This propoſition may be demonſtrated more briefly, and perhaps more palpably, as follows:

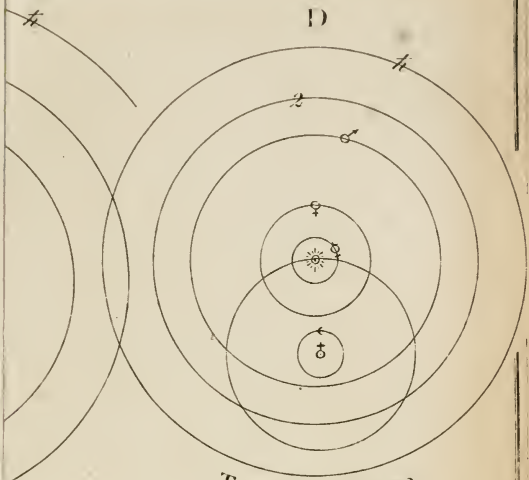
It was ſhewn (Ellipſe, § 10. Cor.) that if Pp be a very minute arch, and pr be perpendicular to the radius vector PF, then qp, the linear deflection from the tangent is, ultimately, in the proportion of pr^2. But, becauſe equal areas are deſcribed in equal times, the elementary triangle PFp is a conſtant quantity, when the moments are ſuppoſed equal, and therefore pr is inverſely as PF, and pr^2 inverſely as PF^2. Therefore qp is inverſely as PF^2, or the momentary deflection from the tangent is inverſely as the ſquare of PF, the diſtance from the focus. Now, the momentary deflection is the meaſure of the deflecting force, and the force is inverſely as the ſquare of the diſtance from the focus.

Here then is exhibited all that we know of that property or mechanical affection of the maſſes of matter which compoſe the ſolar ſyſtem. Each is under the continual influence of a force directed toward the Sun, urging the planet in that direction; 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 inverſe duplicate ratio of its diſtance from the Sun. It will free us entirely from many metaphyſical 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 and that the momentary deflections are in the inverſe duplicate ratio of the diſtances from him.

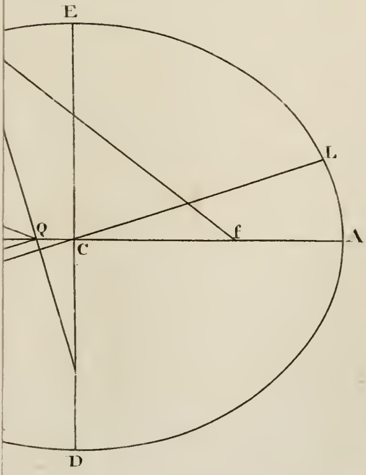
442. We muſt affirm the ſame thing of the forces which retain the ſatellites in their elliptical orbits round their primary planets. For they alſo deſcribe ellipſes having the primary planet in the focus; and we muſt alſo include the Halleyan comet, which ſhewed, by its reapparition in 1759, that it 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 $q\,p$ 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



Tychonic System

Fig. 45.



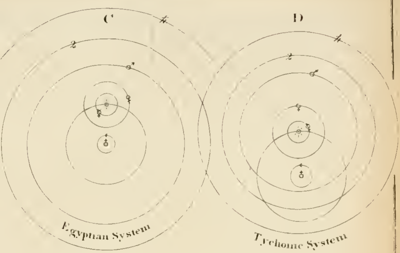
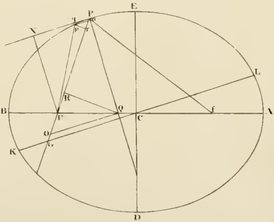


Fig. 45.



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 curvilineal 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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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 $f \doteqdot \frac{d}{t^2}$. Now, in the planetary motions, we have $t^2 \doteqdot d^3$. Therefore, in this caſe, $f \doteqdot \frac{d}{d^3}$, or $\doteqdot \frac{1}{d^2}$, 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 gueſs 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 propofition has fome intricacy, it is not fo clear as we fhould wifh in an elementary courfe like ours. The fame truth may be eafily made appear in the following manner.

444. *If a planet, when at its mean diftance from the Sun, be projected in a direction perpendicular to the radius vector, with the fame velocity which it has in that point of its orbit, it will defcribe a circle round the Sun in the fame time that it defcribes the ellipfe.*

Let A B P D (fig. 46.) be the elliptical orbit, having the Sun in the focus S. Let A P, B D, be the two axes, C the centre, A the aphelion, P the perihelion, and B, D, the two fituations of mean diftance. About S defcribe the circle B D M. Let B K and B N be very fmall equal arches of the circle and the ellipfe, and let B E be one half of B S.

B M, the double of B S, is the deflective chord of the circle of curvature in the point B of the orbit (ellipfe, § 9.), and B E 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 B E. But a body projected with this velocity in the direction B K will defcribe the circle B K M D. (106—212.)

The arches B K and B N, being equal, and defcribed with equal velocities, will be defcribed in equal times. The triangles B K S, B N S, having equal bafes B K and B N, B N, are proportional to their altitudes B S and B C (for the elementary arch B N may be confidered as coinciding with the tangent in B, and B C is perpendicular to this tangent). But, becaufe B S is equal to C A, the area of the circle B M D is to that of the ellipfe A B P D as A C to B C, that is, as B S to B C, that is, as the triangle B K S to the triangle B N S. Thefe triangles are therefore fimilar portions of the whole areas, and therefore, fince they are defcribed in equal times, the circle B M D and the ellipfe A B P D will alfo be defcribed in equal times.

Thus it appears that Newton's firft conjecture was perfectly juft. For if the planets, inftead of defcribing their elliptical orbits, were defcribing circles at the fame diftances, and in the fame times, they would do it by the influence of the fame forces. Therefore fince, in this cafe, we fhould have $t^2 \doteqdot d^3$, the forces will be proportional to d^2 inverfely.

445. We now fee 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 decreafes in intenfity as the fquare of the diftance from the Sun increafes. The intenfity at any particular diftance is the fame, in whatever direction the diftance is taken. Although the planetary courfes 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 impulſions 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*

'——*quas dum primordia rerum*
'*Pangeret, omniparens leges violare Creator*
'*Noluit, eternique operis fundamina fixit.*
'*Sol solio 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.

Principia. Suppose the shorter axis B D of the ellipse A B P D (fig. 47.) to diminish continually, the longer axis A P remaining the same. As the extremity B of the invariable line B S 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 P A, whose length is twice the mean distance S B.

In all the successive ellipses produced by this gradual diminution of C B, the periodic time remains unchanged. Just before the perfect coincidence of B with C, the ellipse may be conceived as undistinguishable from the line P A. 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 A D P B A. Therefore the time of descending from any distance B S 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 accompanies

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 B C measures the angle B S C, or A S E, subtended at the Sun by the distance E A 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.

Fig. 46.

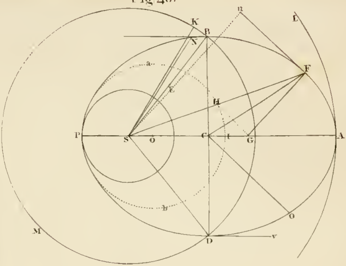


Fig. 47.

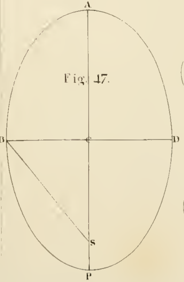
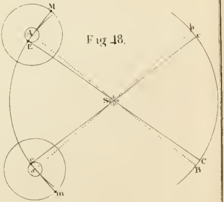


Fig. 48.



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

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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 apſides 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 apſides 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

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 fecond, a ftone falls 16 feet and an inch. Therefore the Moon fhould deflect 16 feet and an inch in a minute from the tangent of her orbit. Newton calculated the verfed fine of the arch defcribed by the Moon in a minute, to a radius equal to 60 femidiameters of the Earth. He found it only about 13½ feet, and he gave over any farther inquiry. But he had haftily fuppofed a degree to contain 60 miles, not attending to the difference between a geographical mile, or 60th of a degree, and an Englifh ftatute mile. A degree contains 69½ fuch miles; fo that he had made the Moon's orbit, and confequently her deflection, too fmall in the fame proportion. If we increafe the calculated deflection in this proportion, it comes out exactly $16\frac{1}{12}$; and the conjecture is fully eftablifhed.

When Picard's accurate meafure of the Earth had enabled Newton to confirm his former conjecture concerning the identity of the planetary force and terreftrial gravity, he again made the calculation and comparifon *in the moft fcrupulous manner*. For we now fee that feveral circumftances muft be taken into the account, which he had omitted in his firft computation from Picard's meafure of the Earth. The fall in a fecond is not the exact meafure of terreftrial gravity. A ftone would fall farther, were it not that its gravity is diminifhed by the Earth's rotation. It is alfo diminifhed by the action of the Sun and Moon, and by the weight of the air which the ftone difplaces. All thefe diminutions of the accelerating force of gravity are fufceptible 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 $16\frac{1}{12}$ 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 pla-

nets

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 rectilineal motion is conſidered as an *effect*, and it is of importance to

diſcover

difcover the *caufe*. Now, in the moft familiar inftance, the fall or preffure of a heavy body, we afcribe the fall, or preffure indicating the tendency to fall, to its heavinefs. But we have no other notion of this heavinefs than the very thing which we afcribe to it as an effect. The feeling the heavinefs of the piece of lead that lies in our hand, is *the fum of all that we know about it*. But we confider this heavinefs as a *property* of all terreftrial matter, becaufe all bodies give fome of thofe appearances which we confider as indications of it. All move toward the Earth if not fupported, and all prefs on the fupport. The feeling of preffure which a heavy body excites might be confidered as its characteriftic phenomenon; for it is this feeling that makes us think it a force—we muft oppofe our force to it; but we cannot diftinguifh it from the feeling of any other equal preffure. It is moft diftinguifhable as the caufe of motion, as a moving or accelerating force. In fhort, we know nothing of gravity but the phenomena, which we confider, not as gravity, but as its indication. It is, like every other force—an unknown quality.

The *weight* of a body fhould be diftinguifhed from its gravity or heavinefs, and the term fhould be referved for expreffing the meafure of the united gravitation of all the mater in the body. This is indeed the proper fenfe of the term *weight—pondus*. In ordinary bufinefs, we meafure the weights of bodies by means of known units of weight. A piece of lead is faid 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 juftly, extended the affirmation to the planets and to the Sun. But here arifes a queftion, at once nice and important. The law of gravitation, fo often mentioned, is exhibited in the mutual deflections of great maffes of matter. Thefe deflections are in the inverfe duplicate ratio of the diftances between the centres of the maffes. Are we warranted by this obfervation to fay that this is alfo the law of action between every atom of one body and every atom of another? Can we fay in general that the law of corpufcular action is the fame with that of maffes, refulting from the combined action of each atom on each? We are affured by experience that it is not. For we obferve that, in magnets, the law of action (that is, the relation fubfifting between the diftances and the intenfities of force) is different in almoft every different magnet, and feems to depend in a great meafure on their form.

Newton was too cautious, and too good a logician, to advance fuch a propofition without proof; and therefore, confining himfelf to the fingle cafe of fpherical and fpheroidal bodies, the forms in which we obferve the planetary maffes to be compacted, he inquired what fenfible action between the maffes will refult from an action between their particles inverfely proportional to the fquare of their diftances.

Let A L B M, *a l b m* (fig. 49.) be two fpherical furfaces, of which C is the common centre, and let the fpace 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 pd^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 $dqre$ is equal to the gravitation to the whole matter in the ſolid $\delta ts\epsilon$, and the particle p is ſtill in equilibrio.

Now,

Now, ſince the lines *dp* ε, *ep* δ 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 A L B M and *a l b m*.

It is alſo evident that this holds true with reſpect to all the matter comprehended between A L B M and the concentric ſurface *p n v* 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 A L B M and *p n v*, there occurs a ſurface *a l b m* of a different denſity from all the reſt, the gravitation to the intercepted portions *q r* and *s t* are equal, becauſe theſe portions are of equal denſity, and are proportional to $p q^2$ and $p s^2$ 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 furface *e d* and to the furface $\varepsilon \delta$ is affirmed, becaufe the numbers of particles in the two furfaces are inverfely as the gravitations towards one in each. For the very fame reafon, the gravitations to the furfaces *e d*, and *q r*, and *t s*, 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 *d p e*, of uniform gravitating matter, be divided by parallel fections *d e*, *q r*, &c. the gravitation of a particle *p* in the vertex to each of thofe fections is the fame, and the gravitations to the folids *p q r*, *p d e*, *q d e r*, &c. are proportional to their lengths *p q*, *p d*, *q d*, &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 *q p r* is to the gravitation to the folid *q d e r* as the number of flices in the firft to the number in the fecond, that is, as *p q*, the length of the firft, to *q d*, the length of the fecond.

The cone *d p e* 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

refolve a body of any form. In this refolution, the bafes are fuppofed, if not otherwife exprefsly ftated, to be parallel, and perpendicular to the axes; indeed they are fuppofed to be portions *x r*, *y e*, *z ε*, &c. of fpherical furfaces, having their centres in *p*. The fmall portions *κ r q*, *y e d*, *z ε δ*, &c. are held as infignificant, vanifhing in the ultimate ratios of the whole folids.

It is eafy alfo to fee that the equilibrium of *p* is not limited to the cafe of a fpherical fhell, but will hold true of any body compofed of parallel ftrata, or ftrata fo formed that the lines *p d*, *p δ* are cut in the fame proportion by the fections *d e*, *q r*, &c. In a fpheroidal fhell, for example, whofe inner and outer furfaces are fimilar, and fimilarly pofited fpheroids, the particle *p* will be in equilibrio any where within it, becaufe in this cafe, the lines *p δ* and *n e* are equal; fo are the lines *p ε* and *o d*, the lines *t δ* and *r e*, the lines *s ε* and *q d*, &c. In moft cafes, however, there is but one fituation of the particle *p* that will infure this equilibrium. But we may, at the fame time, infer the following very ufeful propofition.

463. *If there be two folids perfectly fimilar, and of the fame uniform denfity, the gravitation to each of thefe folids by a particle fimilarly placed on or in each, is proportional to any homologous lines of the folids.*

For, the folids being fimilar, they may be refolved into the fame number of fimilar pyramids fimilarly placed in the folids. The gravitations to each of any correfponding pair of pyramids are proportional to the lengths 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 A L B M (fig. 49.) repreſent ſuch a ſphere, and let P be the external particle. Draw P A C B through the centre C of the ſphere, and croſs it by L C M at right angles. Draw two right lines P D, P E, containing a very ſmall angle at P, and cutting the great circle A L B M in D, E, D′, E′. About P as a centre, with the diſtance P C, deſcribe the arch C *d m*, cutting D P in *d*, and E P in *e*. About the ſame centre deſcribe the arc D O. Draw *d* F, *e* G parallel to A B, and cutting L C in *f* and *g*. Draw C K perpendicular to P D, and *d* H, D ∂, and F I φ perpendicular to A B. Join C D and C F.

Now let the figure be ſuppoſed to turn round the axis P B. The ſemicircumference A L B will generate a complete ſpherical ſurface. The arch C *d m* will generate another ſpherical ſurface, having P for its centre. The ſmall arches D E, *d e*, F G will generate rings or zones of thoſe ſpherical ſurfaces. D O will alſo generate a zone of a ſurface having P for its centre. *f g* and F I will generate zones of flat circular ſurfaces.

It is evident that the zones generated by D E and D O (which we may call the zones D E and D O), having the ſame radius D δ, are to each other as their reſpective breadths D E and D O. In like manner, the zones generated by *d e*, *f g*, F I, F G, being all at the ſame diſtance from the axis A B, are alſo as their reſpective breadths *d e*, *f g*, F I, F G. But the zone D O is to the zone *d e* as PD^2 to Pd^2. For D O is to *d e* as P D to P *d*, and the radius of rotation D δ is to the radius *d* H, alſo as P D to P *d*. The circumferences deſcribed by D O and *d e* are therefore in the ſame proportion of P D to P *d*. Therefore the zones, being as their breadths and as their circumferences jointly, are as PD^2 and Pd^2.

C K and *d* H, being the ſines of the ſame arch C *d*, are equal. Therefore K D and *f* F, the halves of chords equally diſtant from the centre, are alſo equal. Therefore the triangles C D K and C F *f* are equal and ſimilar. But C D K is ſimilar to E D O. For the right angles P D O and C D E are equal. Taking away the common angle C D O, the remainders C D K and E D O are equal. In like manner, C F *f* and G F I are ſimilar, and therefore (ſince C D K and C F *f* are ſimilar) the ele-

mentary

mentary triangles E D O and G F I are fimilar, and D O : D E = F I : F G.

The abfolute gravitation or tendency of P to the zone D O is equal to its abfolute gravitation to the zone de, becaufe the number of particles of the firft is to the number in the laft in PD^2 to Pd^2, that is, inverfely as the gravitation to a particle in the firft to the gravitation to a particle in the laft. Therefore let c exprefs the circumference of a circle whofe radius is 1. The furface of the zone generated by D O will be $DO \times c \times D\delta$, and the gravitation to it will be $\frac{DO \times c \times D\delta}{PD^2}$, to which $\frac{de \times c \times dH}{Pd^2}$, or $\frac{de \times c \times dH}{PC^2}$ is equal. This expreffes the abfolute 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, arifing from its gravitation to every particle in the zone, muft be in the direction P C. The oblique gravitation muft therefore be eftimated in the direction P C, and muft (178.) be reduced, in the proportion of Pd to P H. It is plain that $Pd : PH = de : fg$, becaufe de and fg are perpendicular to Pd and P H. Therefore the reduced or central gravitation of P to the zone generated by D O will be expreffed by $\frac{fg \times c \times dH}{PC^2}$.

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 fg) to F G. Therefore the central gravitation to the zone generated by D E will be

 expreffed

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 F G. 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 P C. Hence it follows that the central gravitation of P to the zone generated by D E, is the ſame as its gravitation to all the matter in the zone generated by F G, if that matter were placed in C.

What has been demonſtrated reſpecting the arch D E is true of every portion of the circumference. Each has a ſubſtitute F G, which being placed in the centre C, the gravitation of P is the ſame. If P T touch the ſphere in T, every portion of the arch T L B will have its ſubſtitute in the quadrant L B, and every part of the arch A T has its ſubſtitute in the quadrant A T L, as is eaſily ſeen. And hence it follows that the gravitation to a particle P to a ſpherical ſurface A L B M 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 A T round A B is equal to the gravitation to the ſurface generated by T L B, which is much larger, but more remote.

What we have now demonſtrated with reſpect to the ſurface generated by the ſemicircle A L B is equally true with regard to the ſurface generated by any concentric ſemicircle, ſuch as *a l 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 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 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 A B, C D (fig. 51.), inſtead of hanging in the verticals A F and C H, a ſtar I, will ſeem to have the zenith diſtances *e* I, *g* I, inſtead of E I, G I, which it ought to have; and the diſtance F H on the Earth's ſurface will ſeem the meaſure of the difference of latitude *e g*, whereas it correſponds to E G. The meaſure of a degree including the ſpace F H, 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 F H 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 $\frac{1}{289}$ of the gravity at the equator, the equatorial parts of a fluid sphere must rise one half of this, or $\frac{1}{578}$, before all is in equilibrio.

For, suppose C N and C Q (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 N C, 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 equatoreal parts of our globe are much more elevated than this; not leſs than $\frac{1}{312}$. The reaſon is this. When the rotation of the Earth has raiſed the equatoreal points $\frac{1}{578}$, the plummet, which at *a* (fig. 33.) would have hung in the direction *a* D, tangent to the evolute A B D F, 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, 1*ſt*, 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, 2*dly*, 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

conſidering

confidering gravity as competent to all matter, Newton does nothing that is not done by others, when they afcribe impulfivenefs or inertia to matter. Without fcruple, they fay that impulfivenefs is an univerfal property of matter. Impulfivenefs and heavinefs are on precifely the fame footing—mere phenomena; and the moft general phenomena that we know. We know none more general than impulfivenefs, fo as to include it, and thus enable us to explain it. Nor do we know any that includes the phenomena of univerfal deflection, with all the modifications of the heavinefs of matter. Whether one of thefe can explain the other is a different queftion, and will be confidered on another occafion, when we fhall fee with how little juftice philofophers have refufed all action at a diftance.

But it would feem that there is fome peculiarity in this explanation of the planetary motions which hinders it from giving entire fatisfaction to the mind. If this be the cafe, it is principally owing to miftake; to carelefsly imputing to Newton views which he did not entertain. His doctrine of univerfal gravitation does not attempt to explain *how* the operating caufe retards the Moon's motion in the firft and third quarters of a lunation; it merely narrates in what direction, and with what velocity this change is produced; or rather, it fhews how the Moon's deflection toward the Earth, joined to her deflection toward the Sun, both of which are matters of fact, conftitute this *feeming* irregularity of motion which we confider as a difturbance. But with refpect to the operating caufe of 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 vortexes 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,

Principia, and of the review of it in thofe very acts. It may be called rafh, becaufe it trufted too much to the deference which his own countrymen had hitherto fhewn 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 affumes as principles of explanation, motions totally repugnant to thofe laws, and motions and tendencies incongruous and contradictory to each other. And then, by the help of geometrical and analytical errors, which compenfate each other, he makes out a ftrange conclufion, which he calls a demonftration of the law of planetary gravitation; and fays that he fees that this theorem is known to Mr Newton, but that he cannot tell how he has arrived at the knowledge of it. This is fomething very remarkable. Newton's procefs is fufficiently pointed out in the *Acta Eruditorum*, which M. Leibnitz acknowledges that he had feen. A copy of the *Principia* was fent to him, by order of the Royal Society, in lefs than two months after the publication.—It was foon known over all Europe.

It is without the leaft foundation that the partifans of M. Leibnitz give him any fhare in the difcovery of the law of gravitation. None of them has ventured to quote this differtation as a propofition juftly proved, nor to defend it againft the objections of Dr Gregory and Dr Keill. M. Leibnitz's remarks on Dr Gregory's criticifm were not admitted into the *Acta Eruditorum*, though under the management of his particular friends. In October

tober 1706 they inferted an extract from a letter, containing fome of thofe remarks;—if poffible, they are more abfurd and incongruous than the original differtation.

It is worth while, as a piece of amufement, to read the account of this differtation by Dr Gregory in his Aftronomy, and the obfervations by Dr Keill in the *Journal Literaire de la Haye*, Auguft 1714.

479. Sir Ifaac Newton has alfo fhewn fome difpofition to account for the planetary deflection by the action of an elaftic æther. The general notion of the attempt is this. The fpace occupied by the folar fyftem is fuppofed to be filled with an elaftic fluid, incomparably more fubtile and more elaftic than our air. It is fuppofed to be of greater and greater denfity as we recede from the Sun, and in general, from all bodies. In confequence of this, Newton thinks that a planet placed any where in it will be impelled from a denfer into a rarer part of the æther, and in this manner have its courfe incurvated toward the Sun.

But, without making any remarks on the impoffibility of conceiving this operation with any diftinctnefs that can entitle the hypothefis to be called an *explanation*, it need only be obferved that it is, in its firft conception, quite unfit for anfwering the very purpofe for which it is employed, namely, to avoid the abfurdity of bodies acting on others at a diftance. For, unlefs this be allowed, an æther of different denfity and elafticity in its different ftrata cannot exift. It muft either be uniformly 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 Neutonien*. 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; it

it were much better to give thofe *pro re natæ* laws to the planets themfelves.

Mr Cotes, a philofopher and geometer of the firft eminence, wrote a preface to the fecond edition of the Principia, which was publifhed 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 philofophy, and many very pertinent remarks on the maxim which made philofophers fo adverfe to the admiffion of attracting and repelling forces. Whatever may have been Newton's fentiments in early life about the competency of an elaftic æther to account for the planetary deflections, he certainly put little value on it afterwards. For he never made any ferious ufe of it for the explanation of any phenomenon fufceptible of mathematical difcuffion. He had certainly rejected all fuch hypothefes, otherwife 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 abfurdity of the hypothefis of an elaftic æther is completely expofed, and it is declared to be a contrivance altogether unworthy of a philofopher. Yet, when Mr Cotes died foon after, Sir Ifaac Newton fpoke of him in terms of the higheft refpect. Alas, faid he, *we have loft Mr Cotes; had he lived, we fhould foon have learned fomething excellent.*

At prefent the moft eminent philofophers and mathematicians in Europe profefs the opinion of Mr Cotes, and fee no validity in the philofophical maxim that bodies cannot

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 diftances 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 P N, with the velocity which the gravitation at P to the Sun would generate in it by impelling it along P T, leſs than P S.

Draw P Q perpendicular to P N. Take P O equal to twice P T, and draw O Q perpendicular to P Q, and Q R perpendicular to P S. Alſo draw P s, making the angle Q P s equal to Q P S. Join S Q, and produce S Q till it meet P s in s.

The body will deſcribe an ellipſis, which P N touches in P, whoſe foci are S and s, and whoſe principal parameter is twice P R.

For, draw S N perpendicular to P N. Make P O′ = 2 P O or = 4 P T, and draw O′ Q′ perpendicular to P O′, and deſcribe a circle paſſing through P, O′ and Q′. It will touch P N, becauſe P O′ Q′ was made a right angle, and therefore P Q′ 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, becaufe the angles are equal which PN makes with the two focal lines. This being the cafe, we know that if PQ, OQ, and QR be drawn as directed in the foregoing conftruction, P O′ Q′ is the circle which has the fame curvature with the ellipfe in P, whofe 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 ellipfe. Therefore (212.) PT is the fpace along which the body muft be uniformly impelled by the force in P, that it may acquire the velocity with which the body, actually defcribing this ellipfe, paffes through P. If this body, which we fhall call A, thus revolves in an ellipfe, we fhould infer that it is deflected toward S, by a force inverfely proportional to the fquare of its diftance from S, and of fuch magnitude in P, that it would generate the velocity with which the body paffes through P, by uniformly impelling it along PT.

Now, the other body (which we fhall 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 paffes through P in the fame direction, and it is under the influence of a force precifely the fame that muft have influenced A in the fame place. The two bodies A and P are therefore in precifely the fame mechanical condition; in the fame place; moving in the fame direction; with the fame velocity; deflected by the fame intenfity of force, acting in the fame 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 41ſt 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 curvilineal 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 P T, 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 P S. But every ſtep would have been the ſame, had we made P T equal to P S (as in fig. 52. N° 2.) But we ſhould then have found that when the angle Q P s is made equal to Q P S, the line P s will be parallel to S Q, ſo that S Q will not interſect it, and the path will not have another focus. It is a parabola, of which P R is the principal parameter.

486. We ſhall alſo find that if P T be made greater than P S (as in fig. 52. N° 3.) the line P s (making the angles Q P S and Q P s equal) will cut S Q on the other ſide of S, ſo that S and s are on the ſame ſide of Q. The path will be a hyperbola, of which P R 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.

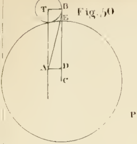
Fig. 50


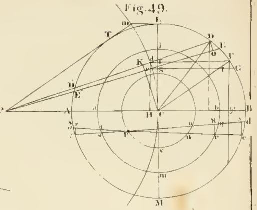
Fig. 49.


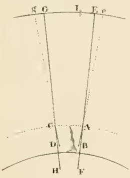
Fig. 51


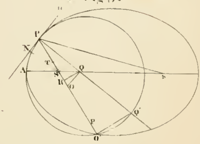
Fig. 52


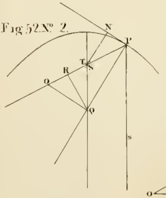
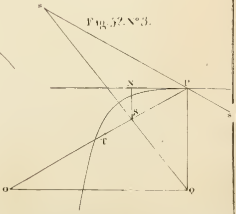
Fig. 52. N° 2.


Fig. 52. N° 3.


487. This reſtriction to the conic ſections plainly follows from the line P R, the third proportional to P O and P Q, 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 B D P A (fig. 53.) by means of a deflecting force directed to a focus S, the angle S P N, 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 S P, the *given* diſtance, be a, and any indeterminate diſtance be x. Let the perpendicular S N (alſo given by S P

and

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Let S P, the *given* diſtance, be a, and any indeterminate diſtance be x. Let the perpendicular S N (alſo given by S P

and

If the velocity of the body in P be lefs than that which might produce a circular motion round S, then its path will coalefce with the nafcent arch P p of a circle whofe deflective chord of curvature is lefs than 2 P S (212.) Let its half be P O, lefs than P S, and let P p be a very minute arch. Draw the tangents P N, $p\,n$, and the perpendiculars S N, S π. P q perpendicular to P N will meet $p\,q$ perpendicular to $p\,n$ (P p being evanefcent) 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 ftand on the fame arch P p of the equicurve

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and the given angle S P N) be b, and let p be the perpendicular and q the focal chord of curvature, correfponding to the diftance x. Let 4 P T be $= d$. Then (102. 210.) we have

$$\frac{1}{b^2 d} : \frac{1}{p^2 q} = \frac{1}{a^2} : \frac{1}{x^2}$$

$$b^2 d : p^2 q = a^2 : x^2$$

$$b^2 d\, x^2 = p^2\, q\, a^2$$

$$\text{therefore } q = \frac{b^2 d\, x^2}{a^2 p^2}, = \frac{b^2}{a^2} d \times \frac{x^2}{p^2}$$

Let $\frac{b^2}{a^2} d = e$ then $q = \frac{e\,x^2}{p^2}$, which is an equation to a conic fection, of which e is the parameter, S the focus, and P N a tangent in P. Now e is a given magnitude, becaufe a, b, d, are all given. Expreffing the angle S P N by φ, we have $e = d \times \text{fin.}^2\, \varphi$. See alfo for the particular cafe of a force proportional to $\frac{1}{x^2}$ the differtations by Dr Jo. Keill in the Philofophical Tranfactions, No. 317. and No. 340.

equicurve circle, and are, refpectively, the doubles of the angles at the circumference. P $q p$ is evidently equal to N S n. Therefore P O p is equal to N S n, and P S p is lefs than N S n. Therefore P S N is lefs than p S n, and S P N is greater than S p n. Therefore the angle S P N diminifhes when P O is lefs than P S, that is, when the velocity in P is lefs 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 fuits a circular motion round S, it is plain that P O will be greater than P S, and the angle P S p will be greater than N S n, and the angle P S N greater than p S n, and therefore the angle S P N will be lefs than S p n, &c.

489. Applying this obfervation to the cafe of elliptical motion, we get a more diftinct notion of its different affections, and their dependence on their phyfical caufes.

In the half D A B (fig. 46.) of the ellipfe defcribed 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 femiellipfe D A B, the angle contained between the radius vector and the line of the planet's motion is continually diminifhing. But during the motion in the femiellipfe, B P D, the angle is continually increafing. It is therefore the greateft poffible in D, and the fmalleft in B.

 Let

Let the planet fet out from its aphelion A, with its due velocity, moving in the direction A F. The velocity in A, being equal to that acquired by a uniform acceleration along $\frac{1}{4}$ of the parameter, is vaftly lefs than what would make it move in the circular arch A L, of which S is the centre, and the planet muft fall within that circle. Therefore its path will no longer be perpendicular to the radius vector, but muft now make with it an angle fomewhat acute. The centripetal force therefore is now refolvable 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 A F B, the velocity is always lefs than what is required for a circular motion. For, if from any point F in this quadrant, F G be drawn perpendicular to the tangent, meeting the tranfverfe axis in G, and if G H be drawn perpendicular to the normal F G, H F is one half of the focal chord of curvature, and H lies between P and S. Now, it has been fhewn that when this is the cafe, the angle S F n diminifhes, and, with it, the ratio of S n to S F (this ratio is that of C B to the femidiameter C O, the conjugate of C F, (§ 6. Ell.) Confequently, there will be continually more and more of the centripetal force employed in accelerating the motion, and lefs employed in incurvating the path, the firft part being F n and the other S n. When the planet arrives at B, the point H falls upon S, and the velocity is precifely what would fuffice 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 ftill nearer to S. A great part of the centripetal force is ftill employed in accelerating the motion; and the moment the planet paffes 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 S B N, which is now in its fmalleft poffible ftate, begins to open again; and this diminifhes the proportion of the centripetal force which accelerates the motion, and increafes the proportion of the incurvating force. The planet is, however, ftill accelerated, preferving the equable defcription of areas. The angle S B N increafes with the increafing velocity, and becomes a right angle, when the planet arrives at its perihelion P.

It has been fhewn (Ellipfe, § 4.) that the chord P I cut off from any diameter P A by the equicurve circle P *a* I, is equal to the parameter of that diameter. Therefore the centre *o* of this circle lies beyond S. The planet, paffing through P, is defcribing a nafcent arch of this circle. Confequently, the curve which it is defcribing paffes without a circle defcribed round S, and the planet is now receding from the Sun. This is ufually accounted for, by faying that its velocity is now too great for defcribing a circle round the Sun. And this is true, when the intenfity of the deflecting force is confidered. 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 greateft 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 ftill nearer. But the fact is, that the planet, in paffing through P, is really moving fo that, if the Sun were fuddenly transferred to *o*, it would circulate round it for ever. But, in defcribing the fmalleft portion of the circle P *a* I, it goes without the circle which has S for its centre, and its motion now makes an obtufe 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 diminifhed; and the angle of the radius vector and the path is now increafed, by the fame degrees by which they had been increafed and diminifhed during the approach to the Sun. At D, the planet has the fame diftance from the Sun that it had in B, and the fame velocity. The angle S D *v* is now as much greater than a right angle as S B N was lefs; and at A, it is reduced to a right angle, and the velocity is again the fame as the firft. In this way the planet will revolve for ever.

It was fhewn in § 223. that in the curvilineal motion of bodies by the action of a central force, the velocities are inverfely 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 diftance may therefore be taken as the unit of the fcale on which all the other velocities are meafured. The other velocities may therefore be confidered as fractions of the perihelion velocity, which is the greateft of all.

In

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 P S, 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 P S, 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 aſſymptote, to which the tangent continually approaches.

But, when the velocity in the perihelion is leſs than that acquired by uniform acceleration along P S, 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 th·t 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 ve?or, 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 $V^2 : v^2 = P\,s : C\,A$. (fig. 53.)

For (488.) $V^2 : v^2 = PO : PS$.

But (Ellipſe 9.) it was ſhewn that $PO \times CA$ was equal to $CK^2, = PS \times Ps$. Therefore $PO : PS = Ps : CA$ and $V^2 : v^2 = Ps : CA$.

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 (becaufe N P may be confidered as coinciding with p P) it is equal to N S P. Therefore

$$Pz^2 : Pp^2 = SN^2 : SP^2, = PQ^2 : PO^2$$

therefore $Pz^2 : Pp^2 = PR : PO$

but $Pp^2 : PV^2 = PO : PS$

therefore $Pz^2 : PV^2 = PR : PS.$

Cor. The angular motion in the circle exceeds that in the ellipfe, when the point R lies between P and S, and falls fhort of it when R lies beyond S. They are equal when P S is perpendicular to A C, or when the true anomaly of the planet is 90°. For then R and S coincide. Here the approach to S is moft rapid.

494. In any point of the ellipfe, the gravitation or centripetal force is to that which would produce the fame angular motion in a circle, at the fame diftance from the Sun, as this diftance is to half the parameter, that is, as P S to P R.

For, by the laft propofition, when the forces in the circle and ellipfe are the fame, the angular motion in the circle was to that in the ellipfe as P V to P z, which has been fhewn to be as $\sqrt{PS}$ to $\sqrt{PR}$. Therefore, when the angular velocity in the circle, and confequently the real velocity, is changed from P V to P z, in order that it may be the fame with that in the ellipfe, the centripetal force muft be changed in the proportion of PV^2 to Pz^2, that is, of P S to P R. Therefore the force which retains the body in the ellipfe is to that which will retain

it

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 mafs whatever is the fum or aggregate of its gravitation to every atom of matter in that mafs, 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, fo this may be computed when we know the gravitation towards it. Hence it is evident that we can afcertain 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 diftance, is $\doteqdot \frac{m}{d^2}$. It is this tendency which produces deflection from a ftraight line, and it is meafured by this deflection. Now this, in the cafe of the planets, is meafured by the diftance at which the revolution is performed, and the velocity of that revolution. We found (224.) that this combination is expreffed by the proportional equation $g \doteqdot \frac{d}{p^2}$, where p is the periodic time. Therefore we have $\frac{m}{d^2} \doteqdot \frac{d}{p^2}$, and, confequently, $m \doteqdot \frac{d^3}{p^2}$.

By this means we can compare the quantity of matter in all fuch 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 confider the Earth as the unit in this comparifon with the other bodies of the fyftem.

The Sun's diftance in miles is - - -	93726900
The Moon's diftance - - - - - -	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 muft be increafed by about $\frac{1}{70}$, becaufe the gravitation to the Earth is ftated beyond its real value by the fuppofition 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 increafed, the Sun's quantity of matter may be eftimated at 337422 times that of this Earth.

It muft be obferved that this computation is not of very great accuracy. It depends on the diftance of the Sun; and any miftake in this is accompanied by a fimilar miftake, but in a triplicate proportion. Now our eftimation of the Sun's diftance depends entirely on the Sun's horizontal parallax, as meafured by means of the tranfits of Venus. The error of $\frac{1}{10}$ of a fecond 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 firft fatellite with that of the Moon. This makes Jupiter about 313 times more maffive than the Earth.

The quantity of matter in Saturn deduced from the revolution

revolution of his fecond Caffinian fatellite, is about 103 times that of the Earth.

Herfchel's planet contains about 17 times as much matter as our globe, as we learn by the revolution of its firft fatellite.

We have no fuch means for obtaining a knowledge of the quantity of matter in Venus, Mars, or Mercury. Thefe are therefore only gueffed at, by means of certain phyfical confiderations which afford fome data for an opinion. Venus is thought to be about $\frac{19}{20}$ of the Earth, Mars about $\frac{1}{4}$, and Mercury about $\frac{1}{10}$. But thefe are very vague gueffes. We judge of the Moon's quantity of matter with fome more confidence, by comparing the influence of the Sun and Moon on the tides, and on the preceffion of the equinoxes. The Moon is fuppofed about $\frac{1}{70}$ of the Earth.

From this comparifon it will appear that the Sun contains nearly 800 times as much matter as all the planets combined into one mafs. Therefore the gravitation to the Sun fo much exceeds that of any one planet to another, that their mutual difturbances are but inconfiderable.

496. The proportion of the quantities of matter, difcovered by this procefs of reafoning, is very different from what we fhould have deduced from the obferved bulk of the different bodies. Thus, Saturn's diameter being about ten times that of the Earth, we fhould have inferred that he contained a thoufand times as much

 matter,

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	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

			Fall in 1″.	Weight.
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, *ſuâ matheſi facem præferente*.

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 en-

tirely

tirely from the inequality and the obliquity of the gravitations of the Sun and of the planet A to B. The manner in which thefe difturbances may be confidered, and the grounds of computation, will be more clearly underftood by an example.

Let S (fig. 54.) reprefent the Sun, E the Earth, and J the planet Jupiter. Let it be farther fuppofed (which may be done without any great error) that the Earth and Jupiter defcribe concentric circles round the Sun, and that the Sun contains 1000 times as much matter as Jupiter. Make J S to E A as the fquare of E J to the fquare of S J. Then, if we take S J to reprefent the gravitation of the Sun to Jupiter, it is plain that E A will reprefent the gravitation of the Earth, placed in E, to Jupiter. Draw E B, parallel and equal to J S, and complete the parallelogram E B A D. The force with which Jupiter deranges the motion of the Earth round the Sun will be reprefented by E D.

For the force E A is equivalent to the combined forces E B and E D. But if the Sun and Earth were impelled only by the equal and parallel forces S J and E B acting on every particle of each, it is plain that their relative motions would not be affected (98.) It is only by the impulfion arifing from the force E D that their relative fituations will fuftain 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 E D be confidered as the

 diagonal

diagonal of a rectangle E F D G, E G lying in the direction of the radius S E, and E F being in the direction of the tangent to the Earth's orbit. It is plain that the force E G affects the Earth's gravitation to the Sun, while E F affects the motion round him. As E G is in the direction of the radius, it has no tendency to accelerate or retard her motion round the Sun. E F, on the other hand, does not affect the gravitation, but the motion in the curve only.

This disturbing force E D 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 E G greatly diminishes the Earth's gravitation to the Sun, and the force E F accelerates her motion round him in the order of the letters E C P O Q. In fig. B, the force E G still diminishes the Earth's gravitation to the Sun, but E F retards her motion from O to Q. In fig. C, E G increases the Earth's gravitation to the Sun, and E F accelerates her motion round him. It appears very plainly that the motion round the Sun is accelerated in the quadrants Q C and P O, and is retarded in the quadrants C P and O Q. 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 E I to E S and to S I. 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 E F 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 $\frac{1}{36000}$ 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 fame manner as if $\frac{1}{16000}$ of her gravitation were taken away. For we muft recollect that the Sun alfo 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 meafured by E A, but by E G. All the difturbing forces E G and E F, correfponding to every pofition of the Earth and Jupiter, muft be confidered as fractions of S J, the meafure taken for the mean gravitation to Jupiter. This is $\frac{1}{25000}$ of the Earth's gravitation to the Sun.

Meafuring in this way, we fhall find that when the Earth is at P or Q her gravitation to the Sun is increafed by $\frac{1}{125000}$. For P S or Q S will, in this cafe, come in the place of E G in fig. C, and there will be no fuch force as E F. At C the Earth's gravitation is diminifhed $\frac{1}{44643}$, and at O, $\frac{1}{81800}$.

To be able to afcertain the magnitude of the difturbing force in the different fituations of the Earth is but a very fmall part of the tafk. It only gives us the momentary impulfion. We muft afcertain the accumulated effect of the action during a certain time, or along a certain portion of the orbit of the difturbed planet. This is the celebrated *problem of three bodies*, as it is called, which has employed the utmoft efforts of the great mathematicians ever fince the time that it firft 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.

1*ſt*, Suppoſe it required to determine how far the attraction of Jupiter has made the Earth overpaſs the quadrantal arch Q C 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 E F 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 E D is a function of the angle of commutation C S E, and E F and E G are the ſine and coſine of the angle D E G. Newton, in his lunar theory, has given moſt elegant examples of the ſummation of all the ſucceſſive lines E F 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.

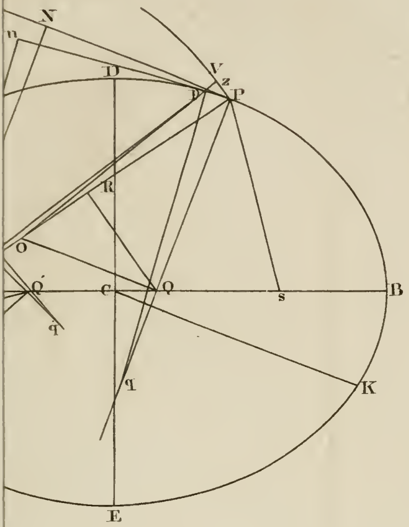
502. The ſecond part of the taſk, the aſcertaining the accumulated effect of the force E G, is, in general, much more difficult. It includes both the changes made on the radius vector S E, and the change made in the curvature of the orbit. The department of mathematical ſcience immediately ſubſervient 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 *Tracts 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 C P and O Q, and accelerated in the quadrants P O and Q C 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 J C S O 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 apſides of the inferior orbit be near the line P S Q, 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,

Fig. 53.



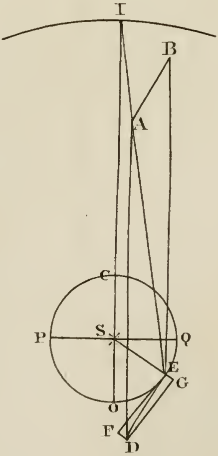


Fig. 54.

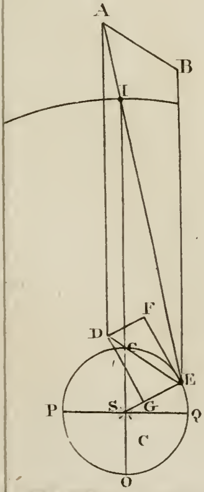


Fig.53.

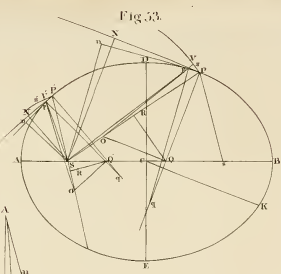
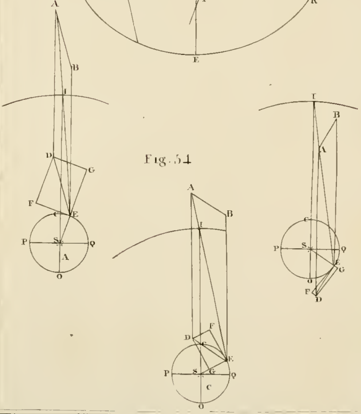


Fig.54.



thus, in every fynodical revolution, the apfides of the inferior planet will twice advance and twice retreat, as if the elliptical orbit fhifted a little to the eaftward or weftward. But, as the diminution of the inferior planet's gravitation to the Sun is much greater when it is in the line C S O than the augmentation of it when in the line P S Q, the advances of the apfides, in the courfe of a fynodical period will exceed the retreats, and, on the whole, they will advance.

The perturbations of the motion of a fuperior planet by its gravitation to an inferior, are in general oppofite, both in kind and in direction, to thofe of the inferior planet. Therefore, in general, their apfides retreat.

All thefe derangements, or deviations from the fimple elliptical motion, are diftinctly obferved in the heavens; and the calculated effect on each planet correfponds with what is obferved, with all the precifion that can be wifhed for. It is evident that this calculation muft be extremely complicated, and that the effect depends not only on the refpective pofitions, but alfo on the quantities of matter of the different planets. For thefe reafons, as Jupiter and Saturn are much larger than any of the other planets, thefe anomalies are chiefly owing to thefe two planets. The apfides of all the planets are obferved to advance, except thofe of Saturn, which fenfibly retreat, chiefly by the action of Jupiter. The apfides of the planet difcovered by Dr Herfchel doubtlefs retreats confiderably, by the action of the great planets Jupiter and Saturn. It might be imagined that the vaft number

thus, in every ſynodical revolution, the apſides 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 C S O than the augmentation of it when in the line P S Q, the advances of the apſides, 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 apſides 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 apſides of all the planets are obſerved to advance, except thoſe of Saturn, which ſenſibly retreat, chiefly by the action of Jupiter. The apſides 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 of comets, which are almost constantly without the orbits of the planets, would cause a general advance of all the apsides. 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 apsides. 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 aphelions 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 *a b p c* 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 A C P, than while he moves over the ſemiellipſe P B A, for Saturn is about 400 days longer in the portion *b a c* 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 A S *p* 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 accounting 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 A B be the path of the other planet, approaching to the node N. As the diſturbing planet is ſomewhere in the plane E K, 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 E K, 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 A C. By the combined action of this force A C with the motion A B, the planet describes the diagonal A D, and crosses the plane E K 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 *n a*, getting to the other side of the plane E K. 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 *a c*. This, compounded with the progressive motion *a b*, produces a motion in the diagonal *a d*, 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 A N E becomes A *n* E, which is greater than A N E. The angle A *n* E or *a n* K is afterwards changed into *a* N′ K, which is less than *a n* K.

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 J J (fig. 57.), which advances on the ecliptic E K, by retreating on the orbit S S 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 apſides 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 Matthew

Fig. 55.

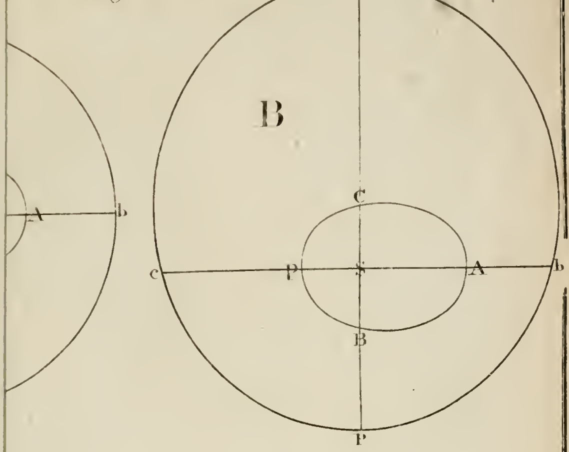


Fig. 56.

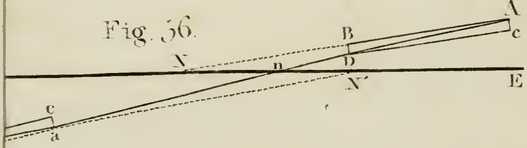
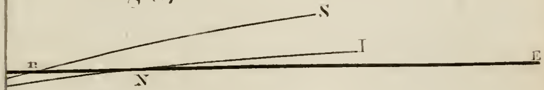


Fig. 57.



61 .B.

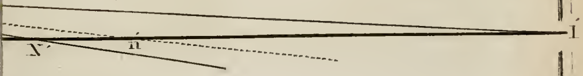


Fig. 55.

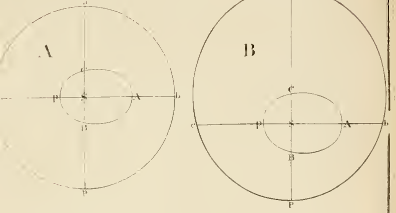


Fig. 56

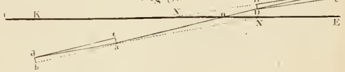
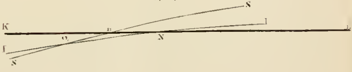


Fig. 57.



61. 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

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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.

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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 7½ 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.

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 $\frac{1}{60}$ 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 23d 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

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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 quā cauſā argentea Phœbe
Paſſibus haud æquis eat, cur ſubdita nulli
Hactenus aſtronomo, numerorum fræna recuſat
Obvia conſpicimus, nubem pollente matheſi.

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 A E B. Let the Moon be ſuppoſed to deſcribe round the Earth the circle C B O A. Join E S and M S, and let S M cut the Earth's orbit in N.

 Laſtly,

Laſtly, Let E S 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 M S, 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 E S when ſhe is in the points A or B, equally diſtant with the Earth, M G will repreſent her gravitation to the Sun when ſhe is in M; for it is to E S in the inverſe duplicate ratio of the diſtances from him.

Now this force M G, being neither equal to E S, nor in the ſame direction, muſt change or diſturb the Moon's motion relative to the Earth. We may ſuppoſe M G to reſult from the combined action of two forces M F and M H (that is, M G may be the diagonal of a parallelogram M F G H), of which one, M F, is parallel and equal to E S. Were the Earth and Moon urged by the forces E S and M F only, their relative motions would not be affected (98.) Therefore M H 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 M F G H is formed. It may be formed on the ſuppoſition that one ſide of the parallelogram ſhall have the direction M E. And this is perhaps the beſt way of re-

ſolving

ſolving M G 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 M F G H, and conſequently on the proportion of M F and M G, and on their relative poſitions. We may obtain an eaſy expreſſion of the force M H by the conſideration that the rate of increaſe of MS^2 is double of the rate of increaſe of M S. 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 M S be ſuppoſed 100, and E S 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 E S and M S is at new and full Moon, when the Moon is in C or O. In this caſe E C is nearly the 390th part of E S, 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. lefs than one part in fifteen thoufand, in the moft unfavourable circumftances. Therefore the difference between N S (or E S) and M G may be fuppofed equal to M D, without any fenfible error, that is, to the double of N M, the difference of N S and M S. Therefore M G — N S = 2 M N very nearly, and M G — M S, that is, S G = 3 M N very nearly. We may alfo take M I for M H without any fenfible error, and may fuppofe E I = 3 M N. For the lines M F, I P, H G, being equal and parallel, and S P nearly coinciding with S G, from which it never deviates more than 9′, E I will nearly coincide with E H, = S G, = 3 M N nearly.

511. Thefe confiderations will give us a very fimple manner of reprefenting and meafuring the difturbing force in every pofition of the Moon, which will have no error that can be of any fignificance. Moreover, any error that inheres in it, is completely compenfated by an equal error of an oppofite kind in another point of the orbit. Therefore

Let us fuppofe that the portion of the Earth's path round the Sun fenfibly coincides with the ftraight line A B (fig. 60.) perpendicular to the line O C S, paffing through the Sun, and called the line of the SYZIGIES, as A B is called the line of the QUADRATURES. Let M D crofs A B at right angles, and produce it to R fo that M P = 3 M N. Join R E, and draw M I parallel to it.

M I will, in all cafes, have the pofition and magnitude correfponding to the difturbing force.

Or, more fimply, make E I = 3 M N, taking the point I on the fame fide of A B with M, and draw M I. M I is the difturbing force.

512. This force M I may be refolved into two, viz. M L, having the direction of the Moon's motion, and M K, perpendicular to her motion, that is, M K lying in the direction of the radius vector M E, and M L having the direction of the tangent. The force M L affects the Moon's angular motion round the Earth, either accelerating or retarding it, while the force M K either augments or diminifhes her gravitation to the Earth.

The difturbing force M I may alfo be refolved into M R′ = 3 M N, and R′ I, or M E; that is, into a force always proportional to M N, and in that direction, and another force in the direction of the Moon's gravitation to the Earth. This is ufeful on another occafion.

513. When the Moon is in quadrature, the point I coincides with E, becaufe there is no M N. In this cafe, therefore, the force M L does not exift, and M K coincides with M E. The difturbing force M I 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 E S expreffes the magnitude of the Moon's gravitation to the Sun, then M E will exprefs (on the fame fcale) the augmentation in

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.

$$G : g = \frac{D}{P^2} : \frac{d}{p^2} = \frac{ES}{y^2} : \frac{EB}{m^2} \ *$$

$$g' : G = EB : ES. \quad \text{Therefore}$$

$$g' : g = \frac{ES \times EB}{y^2} : \frac{EB \times ES}{m^2}, = m^2 : y^2.$$

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 increafed, when in quadrature, about $\frac{1}{179}$. This will diminifh the chord of curvature and increafe the curvature in the fame proportion.

515. In order to fee what change it fuftains in any other pofition of the Moon, fuch as M, join ED, and draw

toward the Earth. Even there fhe is deflected, not toward the Earth, but toward the Sun. This is a very curious, and feemingly paradoxical affertion. But nothing is better eftablifhed. The tracing the Moon's motion in abfolute fpace is the completeft demonftration of it. It is not a looped curve, as one, at firft thinking, would imagine, but a line always concave toward the Sun. Indeed fcarcely any things can be more unlike than the real motions of the Moon are to what we firft imagine them to be. At new Moon, fhe appears to be moving to the left, and we fee her gradually paffing the ftars, leaving them to the right; and, calculating from the diftance 240000 miles, and the angular motion, about half a degree in an hour, we fhould fay that fhe is moving to the left at the rate of 38 miles in a minute. But the fact is that fhe 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 D Q perpendicular to E M. It is plain that D Q is the ſine of the angle D E Q, which is twice the angle O E Q or C E M, that is, twice the Moon's diſtance from the neareſt ſyzigy. Q E is the coſine of the ſame angle. The triangles M D Q and E I K are ſimilar. E I is equal to $1\frac{1}{2}$ M D. Therefore E K = $1\frac{1}{2}$ M Q, = $1\frac{1}{2}$ M E + $1\frac{1}{2}$ E Q, uſing the ſign + when D E *m* is leſs than 90°, or C E M is leſs than 45°, and the ſign — when C E M is greater than 45°. Therefore M K = $\frac{1}{2}$ M E + $1\frac{1}{2}$ E Q. Therefore, if $\frac{1}{2}$ M E be equal to $1\frac{1}{2}$ E Q, that is, if M E be = 3 E Q, M K is reduced to nothing, or the force M I is then perpendicular to the radius vector, or is a tangent to the circle. The angle C E M, or the arch C M, has then its ſecant E I equal to thrice its coſine M N. This arch is 54° 44′. There are therefore four points in the circular orbit diſtant 54° 44′ from the line of the ſyzigies, where the Moon's gravitation to the Earth is not affected by the action of the Sun. If the arch C M 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, $1\frac{1}{2}$ E Q = $1\frac{1}{2}$ E M, and therefore $1\frac{1}{2}$ E Q — $\frac{1}{2}$ E M = E M, as before.

516. At O and at C, $1\frac{1}{2}$ E Q + $\frac{1}{2}$ E M = 2 E M. Therefore, in the ſyzigies, 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.

517. With refpect to the force M L, it is evidently $= 1\frac{1}{2}$ D Q or $1\frac{1}{2}$ of the fine of twice the Moon's diftance from oppofition or conjunction. It augments from the fyzigy to the octant, where it is a maximum, and from thence it diminifhes to nothing in the quadrature. In its maximum ftate, it is about $\frac{1}{110}$ of the Moon's gravitation to the Earth.

518. It appears, by conftructing the figure for the different pofitions of the Moon in the courfe of a lunation, that this force M L retards the Moon's motion round the Earth in the firft and third quarters C A and O B, but accelerates her motion in the fecond and laft quarters A O and B C. Thus, in fig. 60, M L leads from M in a direction oppofite to that of the Moon's motion eaftward from her conjunction at C to her firft quadrature in A. In fig. 60. 3. M L lies in the direction of her motion; and it is plain that M L will be fimilarly fituated in the quadrants C A and O B, as alfo in the quadrants A O and B C.

All thefe difturbing forces depend on the proportion of E B to E S. Therefore, while E S remains the fame, the difturbing forces will change in the fame proportion with the Moon's diftance from the Earth.

519. But let us fuppofe that E S changes in the courfe of the Earth's motion in her elliptical orbit. Then, did the Sun continue to act with the fame force as before, ftill the difturbing force would change in the pro-

 portion

portion of E S, becoming ſmaller as E S becomes greater, becauſe the proportion of E B to E S becomes ſmaller. But, when E S increaſes, the gravitation to the Sun diminiſhes in the duplicate ratio of E S. 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 $\frac{1}{60}$ in January than in July, it follows that in January all the diſturbing forces will be nearly $\frac{1}{20}$ 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

to be held in each, and furnifhing us with the mathematics and the methods which were to be followed. In all thefe particulars, great improvements have been made by Euler, D'Alembert, Clairaut, and Mayer of Gottingen. This laft gentleman, by a moft fagacious examination and comparifon of the *data* furnifhed by obfervation, and a judicious employment of the phyfical principles of Sir Ifaac Newton, has conftructed equations fo exactly fitted to the various circumftances of the cafe, that he has made his lunar tables correfpond with obfervation, both the moft ancient and the moft recent, to a degree of exactnefs that is not exceeded in any tables of the primary planets, and far furpaffing any other tables of the lunar motions.

We can, with propriety, only make fome very general obfervations on the effects of the continued action of the difturbing forces.

521. In the fyzigies and quadrature, the combined force, arifing from the Moon's natural gravitation to the Earth and the Sun's difturbing force, is directed to the Earth. Therefore the Moon will, notwithftanding the difturbing force, continue to defcribe areas proportional to the times. But as foon as the Moon quits thofe ftations, the tangential force M L begins to operate, and the combined force is no longer directed precifely to the Earth. In the octants, where the tangential force is at its maximum, it caufes 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 M L, then at its maximum, increases continually this velocity, which augments, till the Moon comes to a syzigy. 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 syzigy 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 syzigy 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 syzigy, 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 syzigies 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 fame, and where her velocity has attained its mean ftate, her calculated and obferved places differ moft widely. This is the cafe with all aftronomical equations. The motions are computed firft in their mean ftate; and when the changing caufes increafe to a maximum, and then diminifh to nothing, the effect, which is a change of place, has attained its maximum by continual addition or deduction.

522. This alternate increafe and diminution of the Moon's angular motion in the courfe of a lunation was firft difcovered, or at leaft diftinguifhed 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 univerfal gravitation by Sir Ifaac Newton is the moft elegant and perfpicuous fpecimen of mechanical inveftigation that is to be feen. The addrefs which he has fhewn in giving fenfible reprefentations and meafures of the momentary actions, and of their accumulated refults, in all parts of the orbit, are peculiarly pleafing to all perfons of a mathematical tafte, and are fo appofite and plain, that the inveftigation becomes highly inftructive to a beginner in this part of the higher mathematics. The late Dr Mathew Stewart, in his *Tracts Phyfical and Mathematical*, following Newton's example, has given fome very beautiful examples of the fame method.

523. We have hitherto confidered the Moon's orbit as circular, and muft now inquire whether its form will fuffer

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° 16′ ſhort of a quadrature. The force M K, 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° 16′ on each ſide of the quadrature. At 35° 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 V *v*, extending 54° 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° 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

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. A B = 70 and O C = 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 ſyzigies, 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° 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.

	Perigee.	*Mean.*	*Apogee.*
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° 40′, inſtead of 6° 20′, which correſponds to the mean form of the orbit.

But let us next ſuppoſe that the apſides 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° 3′.

In all intermediate poſitions of the apſides, ſimilar anomalies will be obſerved, verging to the one or the other extreme, according to the poſition of the line of the apſides. 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 EVECTION, 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 furnifhed. It is however accomplifhed by Newton in the completeft manner.

It is an interefting cafe of the great *problem of three bodies*, which has employed, and continues to employ, the talents and beft efforts of the great mathematicians. In , Mr Machin gave a pretty theorem, which feemed to promife great affiftance in the folution of this problem. Newton had demonftrated that a body, deflected by a centripetal force directed to a fixed point, moved fo that the radius vector defcribed areas proportional to the times. Mr Machin demonftrated that if deflected by forces directed to two fixed points, the triangle connecting it with them (which may be called the *plana vectrix*) alfo defcribed folids 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 alfo the pofition of the lunar orbit, muft fuffer a change by the action of the Sun. It was demonftrated (226.) that if gravity decreafed fafter than in the proportion of $\frac{1}{d^2}$, the apfides of an orbit will advance, but will retreat, if the gravitation decreafe at a flower rate. Now, we have feen that while the Moon is within 54° 44′ of the fyzigies, the gravity is diminifhed in a greater proportion than that of $\frac{1}{d^2}$. Therefore the apfides which lie in this part of the

fynodical

ſynodical revolution muſt advance. For the oppoſite reaſons, while they lie within 35° 16′ of the quadratures, they muſt recede. But, ſince the diminution in ſyzigy is double of the augmentation in quadrature, and is continued through a much greater portion of the orbit, the apſides 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 ſyzigies; 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 apſides 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 *apſides 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 apſides take every poſition with reſpect to the line of the ſyzigies, 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 M L. This laft, only treating of acceleration and retardation, rarely employs more than the direct method of fluxions, and the finding of the fimpler fluents which are expreffed by circular arches and their concomitant lines. But the very elementary part of this fecond inveftigation engages us at once in the ftudy of curvature and the variation of curvature; and its fimpleft procefs requires infinite feriefes, and the higher orders of fluxions. Sir Ifaac Newton has not confidered this queftion in the fame fyftematic manner that he has treated the other, but has generally arrived at his conclufions by more circuitous helps, fuggefted by circumftances peculiar to the cafe, and not fo capable of a general application. He has not even given us the fteps by which he arrived at fome of his conclufions. His excellent commentators Le Seur and Jaquier have, with much addrefs, fupplied 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 obferved. This was very foon remarked by Mr Machin, but without being able to amend it; and it remained, for many years, a fort of blot on the doctrine of univerfal gravitation.

530. As the Newtonian mathematics continued to improve by the united labours of the firft geniufes of Europe, this inveftigation received fucceffive improvements alfo. At laft, M. Clairaut, about the year 1743, confidered

confidered the problem of thefe bodies, mutually gravitating, in general terms. But, finding it beyond the reach of our attainments in geometry, unlefs confiderably limited, he confined his attention to a cafe which fuited the interefting cafe of the lunar motions. He fuppofed one of the three bodies immenfely larger than the other two, and at a very great diftance from them; and the fmalleft of the others revolving round the third in an ellipfe little different from a circle; and limited his attention to the *difturbances only* of this motion.—With this limitation, he folved the problem of the lunar theory, and conftructed tables of the Moon's motion. But he too found the motion of the apogee only one half of what is obferved.—Euler, and D'Alembert, and Simpfon, had the fame refult; and mathematicians began to fufpect that fome other force, befides that of a gravitation inverfely as the fquare of the diftance, had fome fhare in thefe motions.

At laft, M. Clairaut difcovered the fource of all their miftakes and their trouble. A term had been omitted, which had a great influence in this particular circumftance, but depended on fome of the other anomalies of the Moon, with which he had not fufpected any connexion. He found that the difturbances, which he was confidering as relating to the Moon's motion in the fimple ellipfe, fhould have been confidered 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 precifely

cifely what is obferved to obtain. Euler and D'Alembert, who were employed in the fame inveftigation, acceded without fcruple to M. Clairaut's improvement of his analyfis ; and all are now fatisfied with refpect to the competency of the principle of univerfal gravitation to the explanation of all thefe phenomena of the lunar motions.

531. In the whole of the preceding inveftigation, we have confidered the difturbing force of the Sun as acting in the plane of the Moon's orbit, or we have confidered 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 feldom in its plane. His action muft generally have a tendency to draw the Moon out of the plane in which fhe is then moving, and thus to change the inclination of the Moon's orbit to the ecliptic.

But this oblique force may always be refolved into two others, one of which fhall be in the plane of the orbit, and the other perpendicular to it. The firft will be the difturbing force already confidered in all its modifications. We muft now confider the effect of the other. *

532.

* It is very difficult to give fuch a reprefentation of the lunar orbit, inclined to the plane of the ecliptic, that the lines which reprefent the different affections of the difturbing force

532. Let A C B O (fig. 61.) be the moon's orbit cutting the ecliptic in the line N N′ of the nodes, the half N M A N′, being raifed above the ecliptic, and the other half N B O N′ being below it. The clotted circle is the orbit, turned on the line N N′ till it coincide with the plane of the ecliptic. C, O, A and B are, as formerly, the points of fyzigy and quadrature. Let the Moon be in M. Let A E B be the interfection of a plane perpendicular to the ecliptic. Draw M *n* perpendicular to the plane A E B, and therefore parallel to the ecliptic, and to O C. Take E I equal to 3 M *n*, and join M I. M I is the Sun's difturbing force (511.), and E M meafures the augmentation of the Moon's gravitation when in quadrature. It is plain that M I is in a plane paffing through E S, and interfecting the lunar orbit in the line M E, and the ecliptic in the line E I. M I therefore does not lie in the plane of the lunar orbit, nor in that of the ecliptic, but is between them both. The force M I may therefore be conceived as refolvable into two forces, one of which lies in the Moon's orbit, and the other is perpendicular to it. This refolution will be effected, if we draw I *i* upward from the ecliptic, till it meet the plane of the lunar orbit perpendicularly in *i*. Now

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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 moft attentive confideration by the reader is neceffary for giving him a diftinct notion of thefe circumftances.

Now join M *i*, and complete the parallelogram M *i* I *m*, having M I for its diagonal. The force M I is equivalent to M *i* lying in the plane of the Moon's orbit, and M *m* perpendicular to it. By the force M *i* the Moon is accelerated or retarded, and has her gravitation to the Earth augmented or diminifhed, while the force M *m* draws the Moon out of the plane N C M; or that plane is made to fhift its pofition, fo that its interfection N N′ fhifts its place a little. The inclination of the orbit to the ecliptic alfo is affected. Let a plane I *i* G be drawn through I *i* perpendicular to the line N N′ of the nodes. The line E G is perpendicular to this plane, and therefore to the lines G I and G *i*. Alfo I *i* G is a right angle, becaufe I *i* was drawn perpendicular to the plane M *i* G E.

Now, if E M be confidered as the radius of the tables, M *n* is the fine of the Moon's diftance from quadrature. Call this *q*. Then E I = 3 *q*. Alfo making E I radius, I G is the fine of the node's diftance from the line of fyzigy. Call this *s*. Alfo, I G being made radius, I *i* or M *m* is the fine of the inclination of the orbit to the ecliptic. Call this *i*.

Therefore we have $\mathrm{EM} : \mathrm{EI} = \mathrm{R} : 3\,q$

$$\mathrm{EI} : \mathrm{IG} = \mathrm{R} : s$$

$$\mathrm{IG} : \mathrm{M}m = \mathrm{R} : i$$

Therefore $\mathrm{EM} : \mathrm{M}m = \mathrm{R}^3 : 3\,q\,s\,i$

and $\mathrm{M}m = 3\,\mathrm{EM} \times \frac{q\,s\,i}{\mathrm{R}^3}.$

Thus we have obtained an expreffion of the force M *m*, which tends to change the pofition and inclination

 of

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 M n, 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 M m when M comes into the line of quadrature. When this happens, the whole diſturbing force has the direction A E, the then radius vector, and is in the plane of the orbit. There is no ſuch force as M m in this ſituation of things, the diſturbing force being wholly employed in augmenting the Moon's gravitation to the Earth.

2. The force M m vaniſhes alſo when the nodes are in the ſyzigy. For there, the factor s in the numerator vaniſhes. We perceive the phyſical reaſon of this alſo. For, when the nodes are in the ſyzigies, 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, M m acting out of that plane.

3. The chief varieties of the force M m depend however on s, the ſine of the node's diſtance from ſyzigy. 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.

534. This force bends the Moon's path *toward* the ecliptic, when the points M and I are on the fame fide of the line of the nodes, but bends it away *from* the ecliptic when N lies between I and M. This circumftance kept firmly in mind, and confidered with care, will explain all the deviations occafioned by the force M *m*. Thus, in the fituation of the nodes reprefented in the figure, let the Moon fet out from conjunction in C, moving in the arch C M A O. All the way from C to A, the difturbing force M I is below the elevated half N M N′ of the Moon's orbit between it and the ecliptic, and therefore the force M *m* pulls the Moon out of the plane of her orbit toward the ecliptic. The fame thing happens during the Moon's motion from N to C. This will appear by conftructing the fame kind of parallelogram on the diagonal M I drawn from any point between N and C.

When the Moon has paffed 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 E N′, and joining *g* I′. The line M′*g* is in the orbit, and *g* I′ is in the ecliptic, and the triangle M′ *g* I′ ftands elevated, and nearly perpendicular on both planes, fo that M′ I′ is above them both. In this cafe, the force M′ *m*′ in pulling the Moon out of the plane of her orbit, feparates her from it on that fide which is moft remote from the ecliptic; that is, caufes the path to approach more obliquely to the ecliptic. The figure 61. B will illuftrate this.

N′ I′

N′ I′ is the ecliptic, and M′ N′ is the orbit, both ſeen edgewiſe, as they would appear to an eye placed in *t*, (fig. 61.) in the line N N′ 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,

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Fig. 59.

Fig. 60.

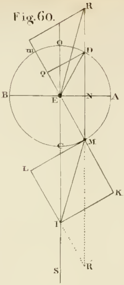


Fig 60. N° 2.

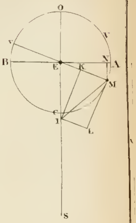


Fig 61.

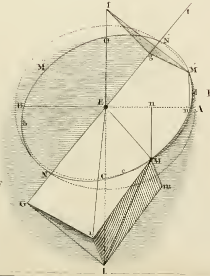
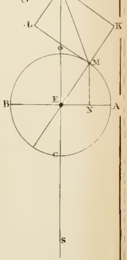


Fig 60 N° 3.



and, as the line N N′ 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 23½ 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 antecedentia ſignorum*, or to recede. But if her path be bent more away from the ecliptic, ſhe muſt proceed farther before ſhe croſs it, and the nodes will ſhift *in conſequentia*, 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 N C A, or the arch N′ O B, that is, while ſhe paſſes from a node to the next quadrature. But, while the Moon deſcribes the arch A N′, or the arch B N, 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.

Cor.

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Cor.

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 M l 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 C E A.

It is evident that the exceſs of the arch N C A above the arch B N or A N′, is double of the diſtance N C of the node from ſyzigy. Therefore the retreat or weſterly motion of the nodes will gradually increaſe as they paſs from ſyzigy to quadrature, and again decreaſe as the node paſſes from quadrature to the ſyzigy.

Cor. 3. When the nodes are in the quadratures, the lateral force M *m* 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 ſyzigies 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 analyfis. The principal equation amounts to 1° 37′ 45″ at its maximum, and in other fituations, it is proportional to the fine of twice the arch N C. The annual regrefs, computed according to the principles of the theory, does not differ two minutes of a degree from what is actually obferved in the heavens. This wonderful coincidence is the great boaft of the doctrine of univerfal gravitation. At the fame time, the perufal of Newton's inveftigation will fhew that fuch agreement is not the *obvious* refult of the happy fimplicity of the great regulating power; we fhall there fee many abftrufe and delicate circumftances, which muft be confidered and taken into the account before we can obtain a true ftatement.

This motion of the nodes is accompanied by a variation of the inclination of the orbit to the ecliptic. The inclination increafes, when the Moon is drawn from the ecliptic while leaving a node, or toward it in approaching a node. It is diminifhed, when the Moon is drawn toward the ecliptic when leaving a node, or from it in approaching a node. Therefore, when the nodes are fituated in the firft and third quadrants, the inclination increafes while the Moon paffes from a node to the next quadrature, but it diminifhes till fhe is 90° from the node, and then increafes till fhe reaches the other node. Therefore, in each revolution, the inclination is increafed, and becomes continually greater, while the node recedes from the quadrature to the fyzigy; and it is the greateft poffible when the nodes are in the line of the fyzigies.

ſyzigies, and it is then nearly 5° 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 ſyzigy, being then nearly 4° 58′, and it gradually increaſes again till the nodes reach the line of ſyzigy. 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 M *m*, 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.

539. There is another cafe of this problem which is confiderably different, namely, the fatellites of Dr Herfchel's planet, the planes of whofe orbits are nearly perpendicular to the orbit of the planet. This problem offers fome curious cafes, which deferve the attention of the mechanician; but as they intereft us merely as objects of curiofity, they have not yet been confidered.

540. There is ftill another confiderable derangement of the lunar motions by the action of the fun. We have feen that, in quadrature, the Moon's gravitation to the Earth is augmented $\frac{1}{179}$, and that in fyzigy it is diminifhed $\frac{2}{179}$. Taking the whole fynodical revolution together, this is equivalent, nearly, to a diminution of $\frac{\frac{1}{2}}{179}$, or $\frac{1}{358}$. That is to fay, in confequence of the Sun's action, the general gravitation of the Moon to the Earth is $\frac{1}{358}$ lefs than if the Sun were away. If the Sun were away, therefore, the Moon's gravitation would be $\frac{1}{358}$ greater than her prefent mean gravitation. The confequence would be, that the Moon would come nearer to the Earth. As this would be done without any change on her velocity, and as fhe now will be retained in a fmaller orbit, fhe will defcribe it in a proportionally lefs time; and we can compute exactly how near fhe would come before this increafed gravitation will be balanced by the velocity (224.) We muft conclude from this, that the mean diftance and the mean period of the Moon which we obferve, are greater than her natural diftance 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 E M to E S 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 $\frac{1}{15}$, 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 $\frac{1}{178}$ 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 M K 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 deminiſhing 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 the

the whole extent of its influence. The application of this principle did not feem to be obvious, nor its effects to be very clear or definite.

At laft, M. de la Place difcovered the caufe of this perplexing fact; and in a differtation read to the Royal Academy of Sciences in 1785, he fhews that the acceleration of the Moon's mean motion neceffarily arifes from a fmall change in the eccentricity of the Earth's orbit round the Sun, which is now diminifhing, and will continue to diminifh for many centuries, by the mutual gravitation of the planets. He was led to the difcovery by obferving in the feries which expreffes the increafe of the lunar period by the difturbing force of the Sun (a feries formed of fines and cofines of the Moon's angular motion and their multiples) a term equal to $\frac{1}{119}$ of her angular motion multiplied by the fquare of the eccentricity of the Earth's orbit. Confequently, when this eccentricity becomes fmaller, the natural period of the Moon is lefs enlarged by the Sun's action, and therefore, if the Earth's eccentricity continue to diminifh, fo will the lunar period, and this in a duplicate proportion. Without entering into the difcuffion of this analyfis, which is abundantly complicated, we may fee the general effect of a diminution of the Earth's eccentricity in this manner. The ratio of the cube of the mean diftance of the Earth from the Sun to the cube of her perihelion diftance is greater than the ratio of the cube of her aphelion diftance to that of the mean diftance. Hence it follows that the increafe of the mean lunar period, during the fmaller diftances 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. Thereſore 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 $\frac{1}{23}$ 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 fecondary planets, and particularly of Jupiter's fatellites, feeing that the exact knowledge of their motions is almoft as conducive to the improvement of navigation and geography as that of the lunar motions. But there is no room for this difcuffion, and we muft refer to the differtations of Wargentin, Profperin, La Place, and others, who have ftudied the operation of phyfical caufes on thofe little planets with great affiduity and judgement, and with the greateft fuccefs. The little fyftem of Jupiter and his fatellites has been of immenfe fervice to the philofophical ftudy of the whole folar fyftem. Their motions are fo rapid, that, in the courfe of a few years, many fynodical periods are accomplifhed, in which the perturbations arifing from their mutual actions return again in the fame order. Nay, fuch fynodical periods have been obferved as bring the whole fyftem again into the fame relative fituation of its different bodies. And, in cafes where this is not *accurately* accomplifhed, the deficiency introduces a fmall difference between the perturbations of any period and the correfponding perturbations of the preceding one; by which means another and much longer period is indicated, in which this difference goes through all its varieties, fwelling to a maximum and again diminifhing to nothing. Thus the fyftem of Jupiter and his fatellites, as a fort of epitome of the great folar fyftem, has fuggefted to the fagacious philofopher the proper way of ftudying the great fyftem, namely, by *looking out* for fimilar periods in *its* anomalies, and by boldly afferting

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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

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 deſcribes

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, becaufe it will not defcend, but keep hovering over the fame fpot. If the rotation were ftill fwifter, every thing would be thrown off, as we fee water flirted from a mop brifkly whirled round.

545. As things are really adjufted, this does not happen. But yet there is a certain meafurable part of the weight of any body expended in keeping it at reft, in the place where it lies loofe. At the equator, a body lying on the ground defcribes, in one fecond, 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 $16\frac{1}{12}$ feet, the fpace through which gravity, or its heavinefs, would caufe a ftone to fall in that time. Hence we muft infer that the centrifugal tendency arifing from rotation is $\frac{1}{288}$ of the fenfible 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 manifeft a greater heavinefs. If, at the equator, it drew out the fcale of a fpring fteelyard to the divifion 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 fome aftronomical obfervations there, and was provided with a pendulum clock for this purpofe. He found that his clock, which had been carefully adjufted to mean time at Paris, loft above two minutes every day, and he was obliged to fhorten

ſhorten the pendulum $\frac{1}{10}$ 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 $192\frac{1}{7}$. 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

the axis of the Earth; and he affigned the proportion of the retardation in different places.

548. Refuming this fubject fome time after, it occurred to him, that unlefs the Earth be protuberant all around the equator, the ocean muft overflow the lands, increafing in depth till the height of the water compenfated for its diminifhed gravity. He confiders the condition of the water in a canal reaching from the furface of the equator to the centre of the Earth (fuppofe the canal C Q, fig. 33.) and there communicating with a canal C N reaching from the centre to the pole. The water in the laft muft retain all its natural gravity, becaufe its particles do not defcribe circles round the axis. But every particle in the column C Q reaching to the furface of the equator muft have its weight diminifhed in proportion to its diftance from the centre of the globe. Therefore the whole diminution will be the fame as if each particle loft half as much as the outermoft particle lofes. This is very plain. Therefore thefe two columns cannot balance each other at the centre, unlefs the equatoreal column be longer than the polar column by $\frac{1}{578}$ (for the extremity of this column lofes $\frac{1}{289}$ of its weight by the centrifugal force employed in the rotation).

Being an excellent and zealous geometer, this fubject feemed to merit his ferious ftudy, and he inveftigated the form that the ocean muft acquire fo as to be *in equilibrio*. This he did by inquiring what will be the pofition of a plummet in any latitude. This he knew muft be

perpendicular

perpendicular to the furface of ftill water. On the fuppofition of gravity directed to the centre of the Earth, and equal at all diftances from that centre, he conftructed the meridional curve, which fhould in every point have the tangent perpendicular to the direction of a plummet determined by him on thefe principles.

549. At this very time, another circumftance gave a peculiar intereft to this queftion of the figure of the Earth. The magnificent project of meafuring the whole arch of the meridian which paffes through France was then carrying on. (See § 267.) It feemed to refult from the comparifon of the lengths of the different portions of this arch, that the degrees increafed as they were more foutherly. This made the academicians employed in the meafurement conclude that the Earth was of an egg-like fhape. This was quite incompatible with the reafoning of Mr Huyghens. The conteft was carried on for a long while with great pertinacity, and fome of the firft mathematicians of the age abetted the opinion of thofe aftronomers, and the honour of France was made a party in the difpute. The opinion of Mr Huyghens, the greateft ornament of their academy, could not prevail; indeed his inferences were fuch, in fome refpects, that even the impartial mathematicians were diffatisfied with them. The form which he affigned to the meridian was very remarkable, confifting of two paraboloidal curves, which had their vertex in the poles, and their branches interfected each other at the equator, there forming an angu-

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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 defied 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 confidered this as fufficient for proving the inaccuracy of the Newtonian conjecture, for it could get no higher name.

552. By very flow degrees, the French academicians began to acknowledge the compreffed form of the Earth, and to reexamine their obfervations, by which it had feemed that the degrees increafed to the fouthward. They now affected to find that their meafurement had been good, but that fome circumftances had been overlooked in the calculations, which fhould have been taken into the account. But they were not aware that they were now vindicating the goodnefs of their inftruments and of their eyefight at the expence of their judgement.

All thefe things made the problem of the figure of the Earth extremely interefting to the great mathematical philofophers. Newton took no part in the further difcuffion, being fatisfied with the evidence which he had for his own determination of the precife fpecies of the terraqueous fpheroid. His philofophy gradually acquired the afcendancy; but the comparifon made of the degrees of the meridian argued a fmaller ellipticity than he had affigned to the Earth, on the fuppofition of uniform denfity and primitive fluidity. He had however fufficiently pointed out the varieties of ellipticity which might arife from a difference of denfity in the interior parts. Thefe were acquiefced in, and the mathematicians fpeculated on the ways by which the obfervations and

and the theory of univerfal gravitation might be adapted to each other. But, all this while, the original problem was confidered as too difficult to be treated in any cafe remarkably deviating from a fphere, and even this cafe was folved by Newton and his followers only in an indirect manner.

553. The firft perfon who attempted a direct general folution was Mr James Stirling. In 1735 he communicated to the Royal Society of London two elegant propofitions (but without demonftration), which determine the form of a homogeneous fpheroid turning round its axis, and which, when applied to the particular cafe of the Earth, perfectly coincided with Newton's determination. In 1737 Mr Clairaut communicated to our Royal Society, and alfo to the Royal Academy at Paris, very elaborate and elegant performances on the fame fubject, which he afterwards enlarged in a feparate publication. This is the completeft work on the fubject, and is full of the moft curious and valuable refearch, in which are difcuffed all the circumftances which can affect the queftion. It is alfo remarkable for an example of candour very rare among rivals in literary fame. The author, in extending his memoire to a more complete work, quits his own method of inveftigation, though remarkable for its perfpicuity and neatnefs, for that of another mathematician, becaufe it was fuperior; and this with unaffected acknowledgement of its fuperiority. The refults of Clairaut's theory perfectly coincide with the Newto-

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nian 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 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 propofitions to this purpofe are the fame that Mr Stirling had communicated without demonftration. This performance was much admired, and fettled all doubts about the figure of a homogeneous fpheroid turning round its axis. It is indeed equally remarkable for its fimplicity, its perfpicuity and its elegance. Mr M'Laurin had no occafion to profecute the fubject beyond this fimple cafe. Proceeding on his fundamental propofitions, the mathematical philofophers have made many important additions to the theory. But it ftill prefents many queftions of moft difficult folution, yet intimately connected with the phenomena of the folar fyftem.

In this elementary outline of phyfical aftronomy, we cannot difcufs thofe 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 fhew that a homogeneous elliptical fpheroid will anfwer all the conditions that are required, and to give a *general* notion of the change which a variable denfity will produce in this figure. *

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* The ftudent will confult, with advantage, the original differtations of Mr Clairaut and Mr M'Laurin, and the great additions made by the laft in his valuable work on Fluxions. The *Cofmographia* of *Frifius* alfo 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 A E B Q and *a e b q* (fig. 64. No. 1.) be two concentric and ſimilar ellipſes, having their ſhorter axes A B and *a b* coinciding. Let P *a* L touch the interior ellipſe in the extremity *a* of the ſhorter axis, to which let P K, a chord of the exterior ellipſe be parallel, and therefore equal. Let the chords *a f* and *a g* of the interior ellipſe make equal angles with the axis, and join their extremities by the chord *f g* perpendicular to it in *i*. Draw P F and P G parallel to *a f* and *a g*, and draw F H and P I perpendicular to P K.

Then, P F together with P G are equal to twice *a i*, when P F and P G lie on different ſides of P K. But if they are on the ſame ſide (as P F′ and P G′) then 2 *a i* is equal to the difference of P F′ and P G′.

Draw K *k* parallel to P G or *a g*, and therefore equal to P F, being equally inclined to K P. Draw the diameter M C *z*, biſecting the ordinates K *k*, P G, and *a g*, in *m*, *s*, and *z*, and cutting P K in *n*.

By ſimilarity of triangles, we have

$$\text{K}m : \text{K}n = \text{P}s : \text{P}n, = az : a\text{C}, = ag : ab.$$

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 $Km + Ps : Kn + Pn = ag : ab$,
and Kk (or PF) $+ PG : 2PK = 2ag : 2ab$,
and $PF + PG : 2ag = 2PK : 2ab$;
and, by fimilarity of triangles, we have

$$PH + PI : 2ai = 2PK : 2ab.$$

But $2PK = 2ab$. Therefore $PH + PI = 2ai$, and $PI' - PH' = 2ai'$.

556. Let the two planes A G g B (fig. 63.) A E e B, interfecting in the line A B, and containing a very fmall angle G A E, be fuppofed to comprehend a thin elementary wedge or flice of a folid confifting of gravitating matter. If two planes G P E, F P D, ftanding perpendicularly on the plane A D d B, contain a very fmall angle E P D, they will comprehend a flender, or elementary pyramid of this flice, having its vertex in P, and a quadrilateral bafe G E D F. If two other planes $g p e$, $f p d$, be drawn from another point p, refpectively parallel to the planes G P E, $f p d$, they will comprehend another pyramid, having its fides parallel to thofe of the other, and containing equal angles, and the elementary pyramids F P E, $f p e$, may therefore be confidered as fimilar. The bafe $g e d f$ is not indeed always parallel and fimilar to G E D F. But for each of them may be fubftituted fpherical furfaces, having their centres in P and in p, and then they will be fimilar.

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 fide ſide P D of the one to the homologous ſide $p\,d$ 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 P m. For, drawing $p\,n$ parallel to P m, and the perpendiculars D m, $d\,n$, 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 G P E, *g p e*, and F P D, *f p d*, 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 A E B Q, *a e b q* (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 ſitu-

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ated on ſimilar ſolids; and P C and *p* C 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 P *a* a line perpendicular to the axis, and parallel to the equator E Q. Let P *d* be perpendicular to the equator. Let *a e b q* be the ſection of a concentric and ſimilar ſpheroid, having its axis *a b* coinciding with A B. Drawing any ordinate *f g* to the diameter *a b* of the interior ellipſe, join *a f* and *a g*, and draw P F and P G parallel to *a f* and *a g*, and therefore making equal angles with P *d* K. Let *f g* cut *a b* in *i*, and draw F H, G I, perpendicular to P I.

The lines P F and P G 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 P *a* L and making with it a very minute angle. Theſe pyramids are conſtituted according to the conditions deſcribed in art. 556. The lines *a f*, *a g* are, in like manner, the axes of two pyramids, whoſe ſides are parallel to thoſe of P F and P G. The gravitation of P to the matter contained in the pyramids P F and P G, and the gravitation of *a* to the pyramids *a f* and *a g*, are as the lines P F, P G, *a f*, and *a g*, reſpectively. Theſe gravitations, eſtimated in the direction

P *d*,

P *d*, *a* C, perpendicular to the equator, are as the lines P H, P I, *a i*, *a i*, refpectively. Now it has been fhewn (555.) that P H + P I are equal to *a i* + *a i*. Therefore the gravitations of P to this pair of pyramids, when eftimated perpendicularly to the equator, is equal to the gravitation of *a* to the correfponding pyramids lying on the interior ellipfe *a e b q*.

It is evident that by carrying the ordinate *f g* along the whole diameter from *b* to *a*, the lines *a f*, *a g*, will diverge more and more (always equally) from *a b* and the pyramids of which thefe lines are the axes, will thus occupy the whole furface of the interior ellipfe. And the pyramids on the axes P F and P G, will, in like manner, occupy the whole of the exterior ellipfe. It is alfo evident that the whole gravitation of P, eftimated in the direction P *d*, arifing from the combined gravitations to every pair of pyramids eftimated in the fame direction, is equal to the whole gravitation of *a*, arifing from the combined gravitation to every correfponding pair of pyramids. That is, the gravitation of P in the direction P *d* to the whole of the matter contained in the elementary flice of the fpheroid comprehended between the two planes which interfect in the line P *a* L, is equal to the gravitation of *a* to the matter contained in that part of the fame flice which lies within the interior fpheroid.

But this is not confined to that flice which has the ellipfe A E B Q for one of its bounding planes. Let the fpheroid be cut by any other plane paffing through the line P *a* L. It is known that this fection alfo is an el-

lipfe,

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 A E B Q. Therefore they compofe a gravitation in the direction which bifects the angle contained by the flices, that is, in the direction of the plane A E B Q, and parallel to its axis A B, or perpendicular to the equator.

From all this it follows, that the gravitation of P to the whole fpheroid, when eftimated in the direction P *d* perpendicular to the plane of its equator, is equal to the gravitation of *a* to the interior fpheroid *a e b q*, which is evidently in the fame direction, being directed to the centre C.

In like manner, the gravitation of another particle P′ (in the line P *a* L), in a direction perpendicular to the equator of the fpheroid, is equal to the gravitation of *a* to the interior fpheroid *a e b q*; for P′ may be conceived as on the furface of a concentric and fimilar fpheroid. When thus fituated, it is not affected by the matter in the fpheroidal ftratum without it, and therefore its gravitation is to be eftimated in the fame way with that of the particle P. Confequently the gravitation of P and of P′, eftimated in a direction perpendicular to the equator, are equal, each being equal to the central gravitation of *a* to the fpheroid *a e b q*. Therefore all particles equidiftant from the equator gravitate equally toward it.

563. (*d*) By reafoning in the fame manner, we prove that the gravitation of a particle P in the direction P *a*, perpendicular to the axis A B, is equal to the

gravitation of the particle d to the concentric ſimilar ſpheroid $d\alpha q\beta$; 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 P d is equal to the gravitation of a to the ſpheroid $a e b q$. But the gravitation of a to the ſpheroid $a e b q$, is to the gravitation of A to A E B Q as a C to A C (558.) Therefore the gravitation of P in the direction P d is to the gravitation of A to the ſpheroid A E B Q as a C to A C, or as P d to A C; and the ſame may be proved of any other particle. The gravitation of A is to the gravitation of any particle as the diſtance A C 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 E C perpendicular to the axis, is to the gravitation of any particle P in the ſame direction as E C to P a, 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 P *d* be expressed by the symbol *f*, P *d*, and let *f*, P *a* express its tendency in the direction P *a*. We have

$$f, \text{P}\,d : \text{A} = \text{P}\,d : \text{A C}$$

and $\text{A} : \text{E} = \text{A} : \text{E}$

and $\text{E} : f, \text{P}\,a = \text{E C} : \text{P}\,a.$ Therefore

$$f, \text{P}\,d : f, \text{P}\,a = \text{P}\,d \times \text{A} \times \text{E C} : \text{A C} \times \text{E} \times \text{P}\,a.$$

Now make $d\text{C} : d\,v = \text{A} \times \text{E C} : \text{E} \times \text{A C}$, and draw P *v*. We have now $f, \text{P}\,d : f, \text{P}\,a = \text{P}\,d \times d\,\text{C} : \text{P}\,a \times d\,v$, $= \text{P}\,d \times \text{P}\,a : \text{P}\,a \times d\,v$, $= \text{P}\,d : d\,v$. P is therefore urged by two forces, in the directions P *d* and P *a*, and these forces are in the proportion of P*d* and *d v*. Therefore the compound force has the direction P *v*.

Moreover, this compound force is to the gravity at the pole, or the gravitation of the particle A, as P *v* to A C. For the force P *v* is to the force P *d* as P *v* to P *d*; and the force P *d* is to A as P *d* to A C. Therefore the force P *v* is to A as P *v* to A C.

In like manner, it may be compared with the force at E. Make $a\,\text{C} : a\,u = \text{E} \times \text{C A} : \text{A} \times \text{C E}$. We shall then have $f, \text{P}\,a : f, \text{P}\,d = \text{P}\,a : a\,u$; and the force in the direction P *a*, when compounded with that in the direction P *d*, form a force in the direction P *u*, and having to the force at E the proportion of P *u* to E C.

Thus have we obtained the direction of gravitation for any individual particle on the surface, and its magnitude when compared with the forces at A and at E, which are fuppofed known.

566. (*g*) But it is neceffary to have the meafure of the accumulated force or preffure occafioned by the gravitation of a column or row of particles.

Draw the tangent E T, and take any portion of it, fuch as E T, to reprefent the gravitation of the particle E. Join C T, cutting the perpendicular $d\delta$ in δ. Since the gravitations of particles in one diameter are as their diftances from the centre (561.) $d\delta$ will exprefs the gravitation of a particle d. Thus, the gravitation of the whole column E C will be reprefented by the area of the triangle C E T, and the gravitation of the part E d, or the preffure exerted by it at d, is reprefented by the area E T δ d. We may alfo conveniently exprefs the preffure of the column E C at C by $\frac{E \times EC}{2}$, and, in like manner, $\frac{A \times AC}{2}$ expreffes the weight of the column A C, or the preffure exerted by it at C.

Should we exprefs the gravitation of E by a line E T equal to E C, the weight of the whole column E C would be expreffed by $\frac{EC^2}{2}$, and that of the portion E d by $\frac{EC^2 - dC^2}{2}$, or by its equal $\frac{Ed \times dQ}{2}$. We fee alfo that whatever value we affign to the force E, the gravitations or preffures of the columns E C and E d 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.

567. From thefe obfervations it appears that the two columns A C and E C will exert equal or unequal preffures at the centre C, according to the adjuftment of the forces in the direction of the axis, and perpendicular to the axis. If the ellipfe do not turn round an axis, then, in order that the fluid in the columns A C and E C may prefs equally at C, we muft have $A \times AC = E \times EC$, or $AC : EC = E : A$. The gravitation at the pole muft be to that at the equator as the radius of the equator to the femiaxis. But we fhall find, on examination, that fuch a proportion of the gravitations at A and E cannot refult folely from the mutual gravitation of the particles of a homogeneous fpheroid, and that this fpheroid, if fluid, and at reft, cannot preferve its form.

568. The fix preceding articles afcertain the mechanical ftate of a particle placed any where in a homogeneous fpheroid, inafmuch as it is affected folely 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 fpheroid of gravitating fluid when turning round its axis. For this purpofe, it is neceffary, in the firft place, that the direction of gravity, affected by the centrifugal force of rotation, be every where perpendicular to the furface of the fpheroid, otherwife the waters would flow off toward that quarter to which gravity inclines. Secondly, all canals reaching from the centre to the furface muft balance at the centre, otherwife 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 equatoreal 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 equatoreal 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 P d to A C (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 pro-

portion

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 $P\,v\,u$, so that $d\,C$ may be to $d\,v$ as $p \times EC$ to $s \times AC$, $P\,v$ 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 $EC^2 : AC^2 = d\,C : d\,v, = P\,u : P\,v$. But (Ellipse 7.) if $P\,u$ be to $P\,v$ as EC^2 to AC^2, the line $P\,v\,u$ 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 $P\,u$ (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 $P\,v$ 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 $\text{ſin.}^2 = \frac{1}{3}$, namely, lat. 35° 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 furface of the fpheroid. But we muft know the condition of a particle any where within it.

574. A particle *p*, in any internal point of a diameter, has its fenfible gravity in the direction perpendicular to the furface of a concentric and fimilar fpheroid paffing through the particle. For the gravity at *p* is compounded of forces perpendicular to the axis and to the equator, and proportional to the diftances from them, and therefore proportional to the fimilar forces acting on the particle P (558.) Therefore the compound force of *p* will be parallel, and in the fame proportion, to the compound force P *v* of P, and muft therefore be perpendicular to the tangent of the furface in *p*. It is as *p v'*.

575. *Cor.* Hence we muft infer that if there were a cavern at *p*, containing water, the furface of this ftill water would be a part of the fpheroidal furface *a e b q*. Should this cavern extend all the way to *e* or *a*, the water fhould arrange itfelf according to this furface; or, if *e r p* be a pipe or conduit, the water in it fhould be ftill, except fo far as it is affected by the preffures of the columns A *a* and P *p* and E *e* (thefe preffures will be proved to be equal).

It would feem, from thefe premifes, that if the elliptical fpheroid confift of different fluids, which do not mix, and which differ in denfity, they will be difpofed in concentric fimilar elliptical ftrata, fo 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 rectilineal columns, extending from the centre to the ſurface, will balance in the centre. For, drawing $v\,o$, $v'\,o'$ perpendicular to P C, it is plain that P o and $p\,o'$ repreſent the gravities of P and p eſtimated in the direction P C. Now $P\,c : p\,o' = PC : p\,C$. Therefore the gravitation of the whole column, or the preſſure on C, is repreſented by $\frac{P\,o \times P\,C}{2}$ (566.) Now, in the ellipſe $P\,o \times P\,C = C\,A^2$, a conſtant quantity. Therefore the preſſure of every column at C is the ſame. In like manner, the preſſure of the columns, C p and C a are equal, and therefore alſo the

 preſſures

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 A B is the axis, and E Q the interſection by the equatoreal plane. In the tangent A a take A a equal to E C, and A α equal to A C. Draw a C e and α C ε to the tangent E ε at the equator. It is evident that E e = A C, and E ε = E C. Through p and P draw the lines p L l, N P z, parallel to E C, and the lines p N φ, I P δ parallel to A B. Draw alſo I K k parallel to E C.

Since, by hypotheſis, the whole forces at A and E are inverſely as A C and E C, A a and E e are as the forces acting at A and E. Conſequently, the weights of the columns F D, L Z, and K L, will be repreſented by the areas F f d D, L l z Z, and K k l L (566.)

All the preſſures or forces which act on the particles of the column p P may be reſolved into forces acting parallel to A C, and forces acting parallel to E C, 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 p P is preſſed in the direction A C is to the force with which the column O P is preſſed in the ſame direction, as the number of particles

in

Fig 63.

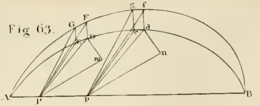


Fig. 64 N°1.

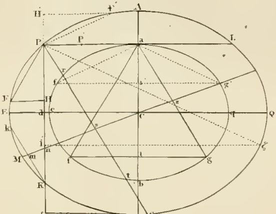
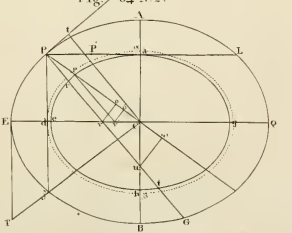


Fig. 64 N°2.



in p P to the number in O P, that is, as p P to O P. 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 p P. Draw O g perpendicular to p P. The force acting in the direction A C on any particle in p P is to its efficacy in the direction p P as O P to g P, that is, as p P to O P Therefore, the preſſure which the particle P ſuſtains in the direction p P, from the action of all the particles in p P in the direction A C, is preciſely equal to the preſſure it ſuſtains from the action of the column O P, acting in the ſame direction A C. But it has been ſhewn (566.) that the preſſure of O P in the direction A C is preciſely the ſame with the weight of the column L Z, which weight is repreſented by the area L l z Z.

In the very ſame manner, the whole preſſure on P in the direction p P ariſing from the preſſure of each of the particles in p P in the direction E C, is preciſely the ſame with the preſſure on P, ariſing from the preſſure of the column N P in this direction E C, that is, it is equal to the weight of the column F D, which is repreſented by the area F f d D.

Becauſe E ε is equal to E C, we have $F\varphi\delta D = \frac{CF^2 - CD^2}{2}, = \frac{Lp^2 - LO^2}{2}, = \frac{pO \times Om}{2}$. And in like manner, $K\varkappa\lambda L = \frac{IO \times Oi}{2}$. But $pO \times Om :$ $IO \times Oi = EC^2 : AC^2$, and therefore

$$F\varphi\delta D : K\varkappa\lambda L = EC^2 : AC^2$$

but $K\varkappa\lambda L : KklL = AC : EC$

and

in p P to the number in O P, that is, as p P to O P. But there is only a part of this force employed in preffing the particles in the direction of the canal. Another part merely preffes the fluid to the fide of the canal p P. Draw O g perpendicular to p P. The force acting in the direction A C on any particle in p P is to its efficacy in the direction p P as O P to g P, that is, as p P to O P Therefore, the preffure which the particle P fuftains in the direction p P, from the action of all the particles in p P in the direction A C, is precifely equal to the preffure it fuftains from the action of the column O P, acting in the fame direction A C. But it has been fhewn (566.) that the preffure of O P in the direction A C is precifely the fame with the weight of the column L Z, which weight is reprefented by the area L l z Z.

In the very fame manner, the whole preffure on P in the direction p P arifing from the preffure of each of the particles in p P in the direction E C, is precifely the fame with the preffure on P, arifing from the preffure of the column N P in this direction E C, that is, it is equal to the weight of the column F D, which is reprefented by the area F f d D.

Becaufe E ε is equal to E C, we have $F \varphi \delta D = \frac{CF^2 - CD^2}{2}, = \frac{Lp^2 - LO^2}{2}, = \frac{pO \times Om}{2}$. And in like manner, $K \varkappa \lambda L = \frac{IO \times Oi}{2}$. But $pO \times Om : IO \times Oi = EC^2 : AC^2$, and therefore

$$F \varphi \delta D : K \varkappa \lambda L = EC^2 : AC^2$$

but $K \varkappa \lambda L : K k l L = AC : EC$

and

and $F f d D : F \phi \delta D = A C : E C$, therefore

$$F f d D : K k l L = E C^2 \times A C^2 : A C^2 \times E C^2,$$

that is, in the ratio of equality. Now the area $K k l L$ reprefents the weight of the column K L, or the preffure exerted in the direction A C by the column I O.

Thus it appears that when the forces acting on the particles in the column p P are eftimated in the direction of the canal, the preffure exerted on the particle P is equal to the united preffures of the columns O P and I O acting in the direction A C, that is, to the preffure of the fluids in the canal I P in its own direction. * Therefore the fluid in the canal I P will balance the fluid in the canal p P, and the particle P will have no tendency to move in either direction. And, fince this is equally true, whatever may be the direction of the canal P p, or P π, it follows that the particle P is equally preffed in every direction in the plane of the figure, and would remain at reft, if the whole fpheroid were fluid.

But now let the canal P p be in a plane different from a meridional plane (as in fig. 64. 4.) In whatever direc-tion

* The ftudent muft not confound this with a compofition of two preffures or forces N P and O P, compofing a preffure or force p P. There is no fuch compofition in the prefent cafe. It is only meant that the preffure in the direction p P arifing from the gravitation of the particles in the canal, is the fame, in refpect of magnitude, with the preffure in the direction I P, arifing from the gravitation of the fluid in I P.

tion Pp is difpofed, a plane may be made to pafs through it, perpendicular to the plane E e Q q of the equator of the fpheroid. Let p I q i e be this plane. Its fection with the fpheroid will be an ellipfe, fimilar to the generating ellipfe A E B Q, as is well known. Let the meridional fection A E B Q pafs through the point P of the canal p P. It will cut the fection e I q i in a line I P i perpendicular to its interfection eq with the equator of the fpheroid, and therefore parallel to the axis a c b of the fection, if it do not coincide with this axis. Let C D E be the femidiameter of the generating ellipfe which paffes through the interfection D of I i and e q; and draw P Z parallel to D C, and P z parallel to e q cutting a c b in z, and join z Z and c C. It is plain that the plane paffing through the axis A B of the fpheroid and the axis a b of the fection e I q i is perpendicular to that fection (for it bifects e q, which is a chord of the equatoreal circle L e Q q), and that the planes D c C and P z Z are parallel, and the angles at c and z right angles.

Let us now confider the forces which act on the particles of fluid in the canal p P. They are, as before, all refolvable into two, one of them parallel to A C, and the other perpendicular to it. Thus, the particle P is urged by a force in the direction P D parallel to A C, and proportional to its diftance P D from the equator of the fpheroid. It is alfo urged by a force in the direction P Z perpendicular to A C, and proportional to its diftance P Z. This force P Z may be refolved into P z and z Z.

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 rectilineal 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 A C; 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, 1*mo*, That a canal of any form whatever muſt every where be *in equilibrio*: 2*do*, 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: 3*tio*, 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 p P of fig. 64. N° 4. to communicate with the canal P δ. It has been ſhewn that they are *in equilibrio* in P. The canal 4 β may branch off from P δ. Theſe are *in equilibrio* in the point 4. The canal 3 α may branch off at 3, and they will be ſtill *in equilibrio;* and the canal 2 1 will be *in equilibrio* with all the foregoing. Now theſe points of derivation may be multiplied, till the polygonal canal p P 4 3 2 1 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 p P—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^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 A C be to E C 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 A C to E C.

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 regreted 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 A E B Q (fig. 65.) repreſent the terreſtrial ſpheroid, nearly ſpherical, and let A *e* B *q* and E *a* Q *b* repreſent the inſcribed and circumſcribed ſpheres. With the axis and parameter A B deſcribe the parabola A F G, drawing the ordinates B D F, E C H, &c. Deſcribe alſo the curve line A I L G, ſuch, that we have, in every point of it, A B : A D = D F : D I ; A B : A C = C H : C L, &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 A B. The ſemiellipſis A E B will generate a ſpheroidal ſurface; the ſemicircle A *e* B will generate a ſpherical ſurface, and the intercepted portions P *p*, E *e*, &c. of the ordinates will generate flat rings of the redundant matter. As the deviation from a ſphere is ſuppoſed very ſmall (E *e* not exceeding the 500dth part of E Q), we may ſuppoſe, without any ſenſible error, that A *p* is the diſtance of A from the whole of the ring generated by P *p*.

Proceeding on this aſſumption, we ſay that the gravitation of A to the rings generated by P *p*, E *e*, &c. is proportional to the portions F I, H L, &c. of the correſponding ordinates D F, C H, &c., and that the gravitation of A to the whole redundant matter may be expreſſed by the ſurface A F H G L I A comprehended between the lines A F H G and A I L G.

For, the abſolute gravitation of A to the ring P *p* 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 P *p*, and alſo its circumference, being proportional to D *p*, the ſurface is proportional to Dp^2. The abſolute gravitation is therefore proportional to $\frac{Dp^2}{Ap^2}$. This may be reſolved into forces in the directions A D and D *p*. The force in the direction D *p* 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 Ap muſt be reduced in the proportion of Ap to A D. It therefore becomes $\frac{Dp^2 \times AD}{Ap^2 \times Ap}$, $= \frac{Dp^2 \times AD}{Ap^3}$, or, which is the ſame thing, $\frac{Dp^2 \times AD \times Ap}{Ap^4}$. But $Ap^2 = AB \times AD$, and $Ap^4 = AB^2 \times AD^2$. Alſo $Dp^2 = AD \times DB$. Therefore the value laſt found becomes $\frac{AD \times DB \times AD \times Ap}{AB^2 \times AD^2}$, which is equal to, or the ſame thing with $\frac{DB \times Ap}{AB^2}$. Since AB^2 is a conſtant quantity, the gravitation in the direction A C to the ring generated by Pp is proportional to $DB \times Ap$.

It is very obvious that D F, C H, B G, &c. are reſpectively equal to Ap, Ae, A B, &c. Therefore the gravitation to the matter in the ring generated by Pp is proportional to $DB \times DF$.

Now, by the conſtruction of the curve line A L G, we have $AB : AD = DF : DI$

therefore $AB : DB = DF : IF$

and $AB \times IF = DB \times DF$

Therefore, ſince A B is conſtant, I F is proportional to $DB \times DF$, that is, to the gravitation to the ring generated by Pp. Therefore the gravitation to the whole redundant matter may be repreſented by the ſpace A H G L A.

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 $= \pi \times$ C$e \times$ 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 A C, 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 A F G and A L G. We muſt find the value of this ſpace.

The parabolic ſpace A H G B A is known to be $= \frac{2}{3}AB \times BG, = \frac{2}{3}AB^2$. The ſquare of D I is proportional to the cube of B D. 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 D I is proportional to $AD^{\frac{3}{2}}$, and the area A B G L A is $= \frac{2}{5}AB \times BG, = \frac{2}{5}AB^2$. Take this from the parabolic area $\frac{2}{3}AB^2$, and there remains

$\frac{4}{15}AB^2$

$\frac{4}{15}$ A B² for the value of A L G H A. This is equal to $\frac{16}{15}$ A C².

Now, the gravitation of A to the redundant matter was ſhewn to be $= \text{A L G H A} \times \frac{\pi \times \text{E}e}{2\,\text{A C}^2}$. This now becomes $\frac{16}{15}\,\text{A C}^2 \times \frac{\pi \times \text{E}e}{2\,\text{A C}^2}$, or $\frac{8}{15}\,\pi \times \text{E}e$. 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 A a 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 P G g p, perpendicular to the axis A a 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 B b 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 O B o b O, touching the oblate ſpheroid in its poles B and b. The ſection of the plane P p with the circumſcribed ſphere will now appear as a circle P′ R p' r. Its ſection with the oblate ſpheroid will appear an ellipſe R G′ r g' ſimilar to the generating ellipſe A B a b, as is well known. And its ſection with the oblong ſpheroid will now appear a circle I G′ i g' parallel to its equator O B o b. P p is equal to P′ p', and G g 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 R P′ r G′ and R p' r g', ſo it exceeds the concomitant ſection of the oblong ſpheroid by two lunulæ G′ R g' I and G′ r g' i. It is alſo plain that if theſe ſpheroids differ very little from perfect ſpheres, as when E B does not exceed $\frac{1}{100}$ of E e, the deficiency of each ſection G g from the concomitant ſection of the circumſcribed

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 A P E B ep A of fig. 65. as one half of the ſpace contained between the ſemicircles A e B and a E b.

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 $R P' r G'$ and $R p' r g'$, 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 P G (fig. 66.), that is, to the ſurface contained between the two circles $R P' r p'$ and $I G' i g'$, we muſt employ only the two lunulæ $R P' r G' R$ and $R p' r g' R$. 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

the

Now let A E B Q (fig. 65.) be an oblate ſpheroid, whoſe poles are A and B. The gravity of a particle A to the ſphere whoſe radius is A C is $\frac{2}{3}\pi \times$ A C, $= \frac{2}{3}\pi \times$ E C $- \frac{2}{3}\pi \times$ E e, or $\frac{2}{3}\pi \times$ E C $- \frac{10}{15}\pi \times$ E e. Add to this its gravitation $\frac{8}{15}\pi \times$ E e, to the redundant matter. The ſum is evidently $\frac{2}{3}\pi \times$ E C $- \frac{2}{15}\pi \times$ E e.

The gravitation of the particle E on the ſurface of the equator to a ſphere whoſe radius is E C is $\frac{2}{3}\pi \times$ E C. From this ſubtract its deficiency of gravitation, viz. $\frac{4}{15}\pi \times$ E e, and there remains the equatoreal primitive gravity $= \frac{2}{3}\pi \times$ E C $- \frac{4}{15}\pi \times$ E e.

Therefore, in this ſpheroid, the polar gravity is to the equatoreal gravity as $\frac{2}{3}\pi \times$ E C $- \frac{2}{15}\pi \times$ E e to $\frac{2}{3}\pi \times$ E C $- \frac{4}{15}\pi \times$ E e, or (dividing all by $\frac{2}{3}\pi$) as E C $- \frac{1}{5}$ E e to E C $- \frac{2}{5}$ E e, or (becauſe E e is ſuppoſed to be very ſmall in compariſon with E C) as E C to E C $- \frac{1}{5}$ E e. 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 E e 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, $p : e = r + \frac{1}{5} x : r$, or (becaufe x is very fmall in comparifon with r), $p : e = r : r - \frac{1}{5} x$. This laft is generally the moft convenient, and it is exact, if r be taken for the equatoreal radius.

583. Had the fpheroid been prolate (oblong) the fame reafoning would have given us $p : e = r : r + \frac{1}{5} x$.

I may add here that the gravitation at the pole of an oblong fpheroid, the gravitation at the equator of an oblate fpheroid (having the fame axes) and the gravitation to the circumfcribed fphere, on any point of its furface, are proportional, refpectively, to $\frac{1}{3} r + \frac{1}{15} x$; $\frac{1}{3} r + \frac{1}{5} x$; and $\frac{1}{3} r + \frac{1}{3} x$. *

584. It now appears, as was formerly hinted (567.) that we cannot have an elliptical fpheroid of uniform denfity, in

* Many queftions occur, in which we want the gravitation of a particle P′ fituated in the direction of any diameter C P (fig. 65.) Draw the conjugate diameter C M, and fuppofe the fpheroid cut by a plane paffing through C M perpendicular to the plane of the figure. This fection will be an ellipfe, of which the femiaxes are C M and C E ($= r + x$). A circle whofe radius is the mean proportional between C M and C E has the fame area with this fection, and the gravitation to this circle will be the fame (from a particle placed in the axis) with the gravitation to this fection. Therefore, as the angle P C M 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 refults from a gravitation proportional to $\frac{1}{d^2}$.

Thus have we obtained, with fufficient 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 fo diminifh the equatoreal gravitation that the remaining gravity there fhall be to the polar gravity as A C to E C.

585. Let c be taken to reprefent the centrifugal tendency generated at the furface of the equator by the rotation of the planet round its axis, and let the other fymbols be retained. The fenfible gravity at the equator is $e - c$, the polar gravity p, and the excefs of the equatoreal radius above the femiaxis r is x.

We have fhewn (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 fpheroid will be the fame with its polar (or axicular) gravitation to another fpheroid, whofe polar femiaxis is P C, and whofe equatoreal radius is the mean proportional between C M and C E. This is eafily computed. If the arch P E be 35° 16′, a fphere having the radius P C has the fame capacity with the fpheroid A E B Q (when E e is very fmall). Hence follows what was faid in the note on art. 572.

or, (becaufe x is a very fmall part of r), in the ratio of $r + \frac{1}{5}x$ to r. That is, $r : r + \frac{1}{5}x = e : p$. This gives $p = e + \frac{ex}{5r}$. Therefore the ratio of the *fenfible* equatoreal gravity to the gravity at the pole is $e - c : e + \frac{ex}{5r}$, or, very nearly, $e : e + \frac{ex}{5r} + c$. Therefore we muft have, for a revolving fphere of fmall eccentricity,

$$e : e + \frac{ex}{5r} + c = r : r + x$$

and $$e : \frac{ex}{5r} + c = r : x$$

confequently $$ex = \frac{ex}{5} + rc$$

and $$ex - \frac{ex}{5} \text{ or } \frac{4ex}{5} = rc$$

and $$4ex = 5rc, \text{ and } x = \frac{5rc}{4e}$$

and the ellipticity $\frac{x}{r} = \frac{5c}{4e}$, that is,

Four times the primitive gravity at the equator is to five times the centrifugal force of rotation as the femiaxis to the elevation of the equator above the infcribed fphere.

586. It is a matter of obfervation that the diminution of equatoreal gravity by the Earth's rotation in $23^h\ 56'\ 4''$ is nearly $\frac{1}{289}$. Therefore $4 \times 289 : 5 = r : x = 231\frac{1}{5} : 1$, very nearly. This is the ratio deduced by Newton in his indirect, and feemingly incurious, method. That method has been much criticifed by his fcholars, as if it could be fuppofed 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

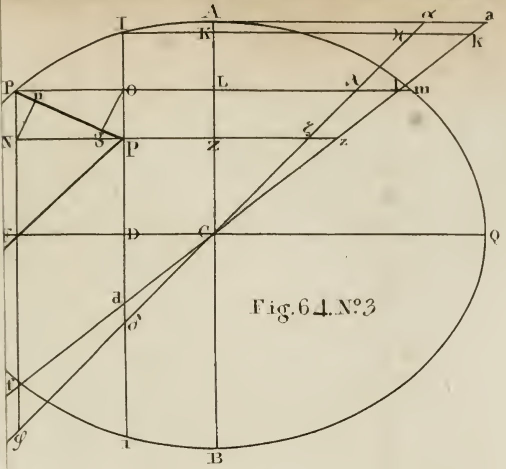


Fig. 64. N°. 3

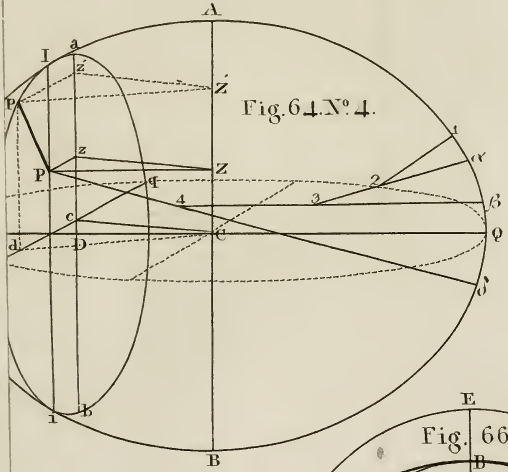
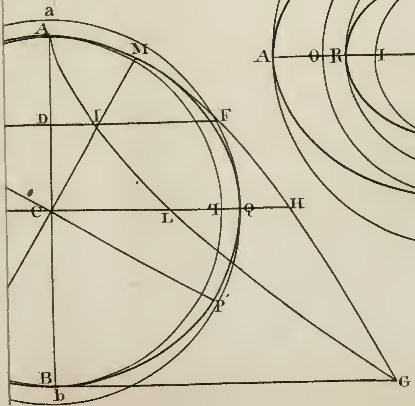


Fig. 64. N°. 4.

.65.



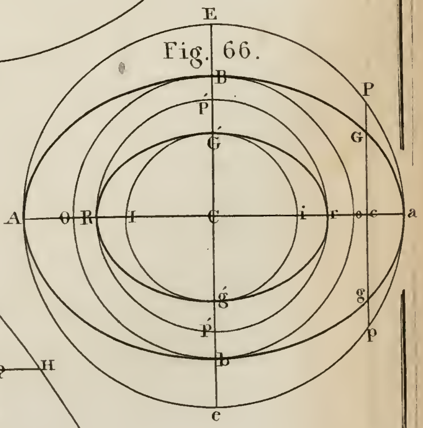


Fig. 66.

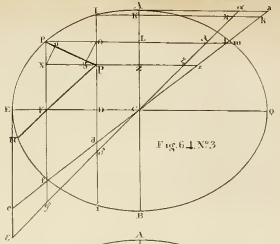


Fig. 64. N° 3

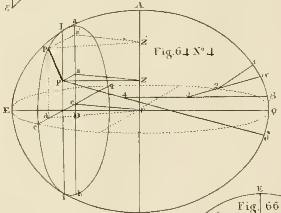


Fig. 64. N° 4

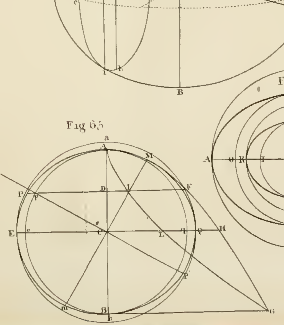


Fig. 65

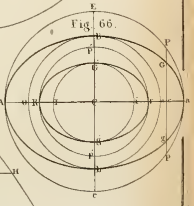


Fig. 66.

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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ſures 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° 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 mafs of folid and denfe rock, perhaps abounding in metals, and, in another place, has below it a deep ocean, or a deep and extenfive ftratum of light fand or earth, we fhould certainly look for a retardation of the pendulum in the latter fituation. The French academicians compared the vibrations of the fame pendulum on the fea-fhore in Peru, and near the top of a very lofty mountain, and they obferved that the retardation of its motion in the latter fituation was not fo great as the removal from the centre required, according to the Newtonian theory, viz. in the proportion of the diftance (the gravity being in the inverfe duplicate proportion). * But it fhould not be fo much retarded. The pendulum was not raifed aloft in the air, but was on the top of a great mountain, to which, as well as to the reft of the globe, its gravitation was directed. Some obfervations were reported to have been made in Switzerland, which fhewed a greater gravitation on the fummit of a mountain than in the adjacent vallies; and much was built on this by the partizans of vortices.

* The length of a pendulum vibrating feconds was found to be 439,21 French lines on the fea-fhore 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 diminifhed in the inverfe duplicate ratio of the diftances, the pendulum at Quito fhould have been 438,80, and at Pichinka it fhould 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 $\frac{1}{100}$ 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 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 $\frac{1}{CE^2}$, $\frac{1}{CA^2}$, $\frac{1}{CM^2}$, &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 A L, but in hypothesis B it is represented by A I. For in hypothesis B the gravitations to this matter are as the distances. E G 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 A E is represented by the space A L G E, but by A I G E 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 E A. There is a loss of pressure or weight sustained by the column E A. This may be expressed by the space L G I, the difference between the two areas E G I A and E G L A. But the *equilibrium* may be restored by adding a column of fluid A M, whose weight A L R M shall be equal to L G I, which is very nearly $= \frac{LI \times AE}{2}$.

In order to find the height of this column, produce G E on the other side of E, and make E F to E G as the density of the fluid to the density by which the nucleus exceeded it. E F will be to E G 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 D E 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 E B *e b*. Draw C D N. Then A N represents the gravitation of A to the whole fluid spheroid, by § 558. In like manner, N I is the united

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 N L.

Let N O repreſent the centrifugal force affecting the particle A, taken in due proportion to N A or N L, its whole gravitation in hypotheſis A. Draw C K O. D K will be the centrifugal force at E. The ſpace O K G I will expreſs the whole ſenſible weight of the fluid in A E, according to hypotheſis B, and O K G L will ex-expreſs the ſame, according to hypotheſis A. L G I is the difference, to be compenſated by means of a due addition A M.

This addition may be defined by the quadrature of the ſpaces G E A L and G L I. But it will be abundantly exact to ſuppoſe that G L R ſenſibly coincides with a ſtraight line, and then to proceed in this manner. We have, by the nature of the curve G L R,

$$AL : EG = EC^2 : AC^2$$

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. H I is the quantity proportional to E A the increment of the root E C. I L is proportional to the variation of the cube, and is therefore very nearly equal to thrice H I.

Therefore

Therefore ſince $EG : HI = EC : AE$, we may ſtate $EG : LI = EC : 3AE$,
or $3EG : LI = EC : AE$.

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 $2KG \times AM = LI \times AE$.

Therefore $2KG : AE = LI : AM$

but $EC : AE = 3EG : LI$

therefore $2KG \times EC : AE^2 = 3EG : AM$

and $2KG : \frac{AE^2}{EC} = 3EG : AM$

and $2KG : 3EG = \frac{AE^2}{EC} : AM$

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 $AE : Ee = 2KG : 3EG$. 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 $2KG : 3EG = \frac{TV^2}{CV} : Tt$, 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

the ſolid nucleus, or where there is a central hollow. For E G 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 B *b*. The ſpace *g i l* in this caſe is negative, and indicates a diminution of the column *a e*, 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, T *t* would be in a conſtant ratio to V T, 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 E *e* is a maximum. It can never be ſenſibly greater in proportion to A E than $\frac{3}{2}$ of A E is in proportion to E C, unleſs the centrifugal force F D be very great in compariſon of the gravity D E. In the caſe of the Earth, where E A is nearly $\frac{1}{230}$ of E C, if we ſuppoſe the mean denſity of the Earth to be five times that of ſea water, *a m* will not exceed $\frac{1}{213444}$ of E C, or $\frac{1}{924}$ of E A.

596. We are not to imagine that, ſince central matter requires an addition A M 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 B C or E C be r, A E 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.

1*mo*. C A, or C E, is to $\frac{1}{5}$ A E 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, $r : \frac{1}{5}\,x = \frac{2}{3}\,\pi f r : \frac{2}{15}\,\pi f x$, and $\frac{2}{15}\,\pi f x$ expreſſes this exceſs.

2*do*. In hypotheſis B, we have C E to C A as the gravitation of B or E to the central matter is to the gravitation of A to the ſame. Therefore C E is to E A 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 $r : x = \frac{2}{3}\pi q r : \frac{2}{3}\pi q x$.

3*tio.* 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 $g : c = \frac{2}{3}\pi n r : \frac{2\pi n r c}{3g}$, and this centrifugal tendency of the particle A is $\frac{2\pi n r c}{3g}$. This is what is expreſſed by N O in our conſtruction.

The whole difference between the gravitations of B and A is therefore $\frac{2}{15}\pi f x - \frac{2}{3}\pi q x + \frac{2\pi n r c}{3g}$. The gravitation of B is to this difference as $\frac{2}{3}\pi n r$ to $\frac{2}{15}\pi f x - \frac{2}{3}\pi q x + \frac{2\pi n r c}{3g}$ or (dividing all by $\frac{2}{3}\pi n$) as r to $\frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$.

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 equatoreal gravitation. That is $r : x = r : \frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$, and therefore $x = \frac{fx}{5n} - \frac{qx}{n} + \frac{cr}{g}$. Hence we have $\frac{cr}{g} = x + \frac{qx}{n} - \frac{fx}{5n}$. But $q = n - f$. Therefore $\frac{cr}{g} = x + \frac{nx}{n} - \frac{fx}{n} - \frac{fx}{5n}$, $= x + x - \frac{6fx}{5n}$, $= 2x - \frac{6fx}{5n} = x \times \left(2 - \frac{6f}{5n}.\right)$

Wherefore

Wherefore $x = \frac{c\,r}{g \times \left(2 - \frac{6f}{5n}\right)}, = \frac{5ncr}{g \times \overline{10n - 6f}}$, which is more conveniently expreffed in this form $x = \frac{5cr}{2g} \times \frac{n}{5n - 3f}$. The fpecies, or ellipticity of the fpheroid is $\frac{x}{r}, = \frac{5c}{2g} \times \frac{n}{5n - 3f}$.

Such then is the elliptical fpheroid of hypothefis B; and we faw that, in refpect of form, it is fcarcely diftinguifhable from the figure which the mafs will have when the fictitious force of the central matter gives place to the natural force of the denfe fpherical nucleus. This is true at leaft in all the cafes where the centrifugal force is very fmall in comparifon with the mean gravitation.

We muft therefore take fome notice of the influence which the variations of denfity may have on the form of this fpheroid. We may learn this by attending to the formula

$$\frac{x}{r} = \frac{5c}{2g} \times \frac{n}{5n - 3f}.$$

The value of this formula depends chiefly on the fraction $\frac{n}{5n - 3f}$.

597. If the denfity of the interior parts be immenfely greater than that of the furrounding fluid, the value of this fraction becomes nearly $\frac{1}{5}$, and $\frac{x}{r}$ becomes nearly $= \frac{c}{2g}$, and the ellipfe nearly the fame with what Hermann affigned to a homogeneous fluid fpheroid.

If

If $n = 5f$; then $\frac{n}{5n - 3f} = \frac{5}{22}$; and, in the cafe of the Earth, $\frac{x}{r}$ would be nearly $= \frac{1}{508,6}$, making an equatoreal elevation of nearly 7 miles.

598. If $n = f$, the fraction $\frac{n}{5n - 3f}$ becomes $\frac{1}{2}$, and $\frac{x}{r} = \frac{5c}{4g}$, which we have already fhewn to be fuitable to a homogeneous fpheroid, with which this is equivalent. The protuberance or ellipticity in this cafe is to that when the nucleus is incomparably denfer than the fluid in the proportion of 5 to 2. This is the greateft ellipticity that can obtain when the fluid is not denfer than the nucleus.

Between thefe two extremes, all other values of the formula are competent to homogeneous fpheroids of gravitating fluids, covering a fpherical nucleus of greater denfity, either uniformly denfe or confifting of concentric fpherical ftrata, each of which is uniformly denfe.

From this view of the extreme cafes, we may infer in general, that as the incumbent fluid becomes rarer in proportion to the nucleus, the ellipticity diminifhes. M. Bernoulli (Daniel), mifled by a gratuitous affumption, fays in his theory of the tides that the ellipticity produced in the aëreal fluid which furrounds this globe will be 800 times greater than that of the folid nucleus; but this is a miftake, which a jufter affumption of *data* would have prevented. The aëreal fpheroid will be fenfibly lefs 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 $\frac{x}{r} = \frac{5c}{4g}$ 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 equatoreal 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 equatoreal gravity is nearly equal to $\frac{g\,x}{5\,r}$ (for $r : \frac{1}{5}x = g : \frac{g\,x}{5\,r}$). When, in addition to this, we take into account the diminution c, produced by rotation, we have $\frac{g\,x}{5\,r} + c$ for the whole difference between the po-

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lar and the ſenſible equatoreal gravity. But, in a homogeneous ſpheroid, we have $x = \frac{5cr}{4g}$. 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 x as g to $\frac{gx}{r}$, and the exceſs of polar gravity in this hypotheſis is $\frac{gx}{r}$. But we have alſo ſeen that in this hypotheſis, $\frac{x}{r} = \frac{5c}{2g} \times \frac{n}{5n-3f}$. 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 $-\frac{qx}{n}$. This muſt now be changed into $+\frac{2qx}{n}$, as may eaſily be deduced from

 § 593,

§ 593, where $-\frac{qx}{n}$ is reprefented by H I in fig. 67, and the excefs, forming the compenfation for hypothefis A is reprefented by H L, nearly double of H I, and in the oppofite direction, diminifhing the gravitation of A. The difference of thefe two ftates is $\frac{3qx}{n}$, by which the tendency of A to the central matter in hypothefis A falls fhort of what it was in hypothefis B. Therefore, as $\frac{fx}{5n}-\frac{qx}{n}+\frac{cr}{g}$ is to $\frac{3qx}{n}$, fo is the excefs ε to a quantity ε', which muft be added to ε, in order to produce the difference of gravities e, conformable to the ftatement of hypothefis A. Now, in hypothefis B, we had $x = \frac{fx}{5n}-\frac{qx}{n}+\frac{cr}{g}$, and we may, without fcruple, fuppofe x the fame in hypothefis A. Therefore $\varepsilon : \varepsilon' = x : \frac{3qx}{n}$, $= 1 : \frac{3q}{n}$, and $\varepsilon' = \varepsilon \times \frac{3q}{n} = \varepsilon \times \frac{3n-3f}{n}$, $= \frac{5c}{2} \times \frac{n}{5n-3f} \times \frac{3n-3f}{n}$, $= \frac{5c}{2} \times \frac{3n-3f}{5n-3f}$. Add to this ε, which is $\frac{5c}{2} \times \frac{n}{5n-3f}$, and we obtain for the excefs e of polar gravity in hypothefis A $= \frac{5c}{2} \times \frac{4n-3f}{5n-3f}$.

603. Let us now compare this excefs of polar gravity above the fenfible equatoreal gravity in the three hypothefes: 1*ft*, A, fuited to the fluid furrounding a fpherical nucleus of greater denfity: 2*d*, B, fuited to the fame fluid, furrounding a central nucleus which attracts with a force proportional to the diftance: and, 3*d*,

C,

C, ſuited to a homogeneous fluid ſpheroid, or encloſing a ſpherical nucleus of equal denſity. Theſe exceſſes are

A $\frac{5c}{2} \times \frac{4n-3f}{5n-3f}$

B $\frac{5c}{2} \times \frac{n}{5n-3f}$.

C $\frac{5c}{4}$, or $\frac{5c}{4} \times \frac{5n-3f}{5n-3f}$.

It is evident that the ſum of A and B is $\frac{5c}{2} \times \frac{5n-3f}{5n-3f}$, which is double of C, or $\frac{5c}{4} \times \frac{5n-3f}{5n-3f}$, and therefore C is the arithmetical mean between them.

Now we have ſeen that $\frac{5c}{2g} \times \frac{4n-3f}{5n-3f}$ expreſſes the ratio of the exceſs of polar gravity to the mean gravity in the hypotheſis A. We have alſo ſeen that $\frac{5c}{2g} \times \frac{n}{5n-3f}$ 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*

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fum of two fractions, one of which expreffes the ratio of the excefs of polar gravity to mean gravity, and the other expreffes the ellipticity of any fpheroid of fmall eccentricity, which confifts of a fluid covering a denfer fpherical nucleus.

If therefore any other phenomena give us, in the cafe of a revolving fpheroid, the proportion of polar and equatoreal gravities, we can find its ellipticity, by fubtracting the fraction expreffing the ratio of the excefs of polar gravity to the mean gravity from twice the ellipticity of a homogeneous fpheroid. Thus, in the cafe of the Earth, twice the ellipticity of the homogeneous fpheroid is $\frac{1}{115}$. A medium of feven comparifons of the rate of pendulums gives the proportion of the excefs of polar gravity above the mean gravity $= \frac{1}{180}$. If this fraction be fubtracted from $\frac{1}{115}$, it leaves $\frac{1}{319}$ for the medium ellipticity of the Earth. Of thefe feven experiments, five are fcarcely different in the refult. 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 comparifons of degrees of the meridian. All the comparifons that have been publifhed concur in giving a confiderably fmaller eccentricity to the terraqueous fpheroid than fuits a homogeneous mafs, and which is ufually called Newton's determination. It is indeed his determination, on the fuppofition of homogeneity; but he exprefsly fays that a different denfity in the interior parts will induce a different form, and he points out fome fuppofititious cafes, not indeed very probable, where the form will be different. Newton has not conceived this fubject with his ufual fagacity, 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 $\frac{1}{231}$ 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 Myſore in India, with excellent inſtruments. It lies in lat. 12° 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 $\frac{8}{95}$ 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 desireable 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 gravitation

vitation 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^h $44'$, and its diſtance is 56,2 (the elongation in ſeconds). Now $\overline{65^h\,44'}|^2 : \overline{10^h\,32\frac{1}{4}}|^2 = 56{,}2^3 : 16{,}4^3$. This is the diſtance at which a ſatellite would revolve in $10^h\,32'$.

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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 22½, 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 flirted 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 equatoreal 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

The whole of this analyfis of the mechanifm of Saturn's ring is of the moft intricate kind, and is carried on by the author by calculus alone, fo as not to be inftructive to any but very learned and expert analyfts. Several points of it however might have been treated more familiarly. But, after all, it muft reft entirely on the truth of the conjectures or affumptions made for procuring the poffible application of the fundamental equations.

607. The Moon prefents to the reflecting mind a phenomenon that is curious and interefting. She always prefents the fame face to the Earth, and her appearance juft now perfectly correfponds with the oldeft accounts we have of the fpots on her difk. Thefe indeed are not of very ancient date, as they cannot be anterior to the telefcope. But this is enough to fhew that the Moon turns round her axis in precifely the fame time that fhe revolves round the Earth. Such a precife coincidence is very remarkable, and naturally induces the mind to fpeculate about the caufe of it. Newton afcribed it to an oblong oval figure, more denfe, or at leaft heavier, at one end than at the other. This he thought might operate on the Moon fomewhat in the way that gravity operates on a pendulum. He defines this figure in Propofition 38. B. III.; and as the eccentricity, or any deviation of its centre of gravity from that of its figure, is extremely fmall, the *vis difponens*, by which one diameter is directed towards the Earth, is alfo 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

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fmaller difference in the pofition 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 pofition; and no obfervation can difcover any deviation from perfect parallelifm of the axis in all fituations. It furely requires much lefs action of the directing force to produce this change in the pofition of the axis, than to overcome even a very fmall difference in angular motion, becaufe this laft difference accumulates, and makes a great difference of longitude.

Thefe confiderations feem to prove that the conftant appearance of one and the fame part of the Moon's furface has not been produced by the caufe fufpected by Newton. The coincidence has more probably been original. We have no reafon to doubt that the fame confummate fkill that is manifeft in every part of the fyftem, in which every thing has an accurate adjuftment, *pondere et menfurâ*, alfo made the primitive revolution rotation of the Moon that which we now behold and admire.

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* The axis round which the rotation of the Moon is performed is inclined to the plane of the ecliptic in an angle of 88½°, and it is inclined to the plane of the lunar orbit 82½. It is always fituated in the plane paffing through the poles of the ecliptic and of the lunar orbit. It therefore deviates about 1½ from the axis of the ecliptic, and 7 from that of the Moon's orbit. The defcending node of the Moon's equator coincides with the afcending 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 immerged, 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.

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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 equatoreal 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,

Sun, if confirmed by future obfervation, render this production of the zodiacal light inconceivable. For motions and changes are obferved there, which fhew a perfect freedom, not conftrained by the adhefion of any fuperior ftrata. This would give a conftant wefterly motion on the furface of the Sun.

The difficulty in accounting for this phenomenon is greatly increafed by the fact that when a comet paffes through this atmofphere, the tail of the comet is not perceptibly affected by it. The comet of 1743 gave a very good opportunity of obferving this. It was not attended to; but the defcriptions that are given of the appearances of that comet fhew clearly that the tail was (as ufual) directed almoft ftraight upward from the Sun, and therefore it mixed with this vapour, or whatever it may be, without any mutual difturbance.

It appears therefore, on the whole, that we are yet ignorant of the nature and mechanifm of the zodiacal light.

609. Before concluding this fubject, it is not improper to take fome notice of an obfervation to which great importance has been attached by a certain clafs of philofophers. We fhall find it demonftrated in its proper place, that when the force which impels a firm body forward acts in a direction which paffes through its centre of gravity, it merely impels it forward. The body moves in that direction, and every particle moves alike, fo that, during its progrefs, the body preferves the fame attitude

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(ſo to ſpeak). Taking any tranſverſe line of the body for a diameter, we expreſs the circumſtance by ſaying that this diameter keeps parallel to itſelf, that is, all its ſucceſſive poſitions are parallel to its firſt poſition. But, when the moving force acts in a line which paſſes on one ſide of the centre of the body, the body not only advances in the direction of the force, but alſo changes its attitude, by turning round an axis. This is eaſily ſeen and underſtood in ſome ſimple caſes. Thus, if a beam of timber, floating on water, be puſhed or pulled in the middle, at right angles to its length, it will move in that direction, keeping parallel to its firſt poſition. But, if it be puſhed or pulled in the ſame direction, applying the force to a point ſituated at the third of its length, that end is moſt affected (as we ſhall ſee fully demonſtrated) and advances faſteſt, while the remote end is left a little behind. In this particular caſe, the *initial* motion of all the parts of the beam is the ſame as if the remote end were held faſt for an inſtant. If the impulſe has been nearer to one end than $\frac{1}{3}$ of the length, the remote end will, *in the firſt inſtant*, even move a little backward. We ſhall be able to ſtate preciſely the relation that will be obſerved between the progreſſive motion and the rotation, and to ſay how far the centre of the body will proceed while it makes one turn round the axis. We ſhall demonſtrate that this axis, round which the body turns, always paſſes through its centre of gravity, in a certain determined direction.

It very rarely happens that the direction of the impelling 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 equatoreal 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

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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

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only explanation that could be given, correfponding to the natural fuggeftion by this remark, would be the fhewing fome general fact in the fyftem, in which this fingle force may be found that muft thus impel the planets eccentrically, and thus urge them into revolution and rotation at once, as they would be urged by a ftroke from fome other planet or comet. With refpect to this Earth, there is not the leaft appearance of the effect which muft have been produced on it, had it been urged into motion by a fingle force applied to one point. The force has been applied alike to every particle; there is no appearance of any fuch general force competent to the production of fuch motions. Nay, did we clearly perceive the exiftence of fuch a force, we fhould be as far from an explanation as ever. It is not enough that Jupiter receives an impulfe which impreffes both the progreffive and rotative motion. His four fatellites muft receive, each feparately, an impulfe of a certain precife intenfity, and in a certain precife 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 fyftem (for it is a fyftem) that gives the fmalleft fatisfaction even to themfelves, and they are obliged to reft fatisfied with expreffing their hopes that it may yet be accomplifhed.

But fuppofe that an expert mechanician fhould fhew how the planets, fatellites, and comets may be fo placed that an impulfe may at once be given to them all, precifely

competent

competent to the production of the very motions that we obferve, which motions will now be maintained for ever by the univerfal operation of gravity. We fhould certainly admire his fagacity and his knowledge of nature. But we ftill wonder as much as ever at the nice adjuftment of all this to ends which have evidently all the excellence that order and fymmetry can give, while many of them are indifpenfably fubfervient to purpofes which we cannot help thinking good. The fuggeftion of purpofe and final caufes is as ftrong as ever. It is no more eluded than it would be, fhould any man perfectly explain the making of a watch wheel, by fhewing that it was the neceffary refult of the fhape and hardnefs of the files and drills and chizels employed, and the intenfity and direction of the forces by which thofe tools were moved; and having done all this, fhould fay that he had accounted for the nice and fuitable form of the wheel as a part of a watch. And, with refpect to the fubfequent oblate form of the planet fet in rotation, the mechanical explanation of this is incompatible with the fuppofition that the revolution and rotation are the effects of one fimple force. The oblate form, if acquired by rotation, requires primitive fluidity, which is incompatible with the operation of one fimple force as the primitive mover. There is no proof whatever that this Earth was originally fluid; it is not nearly fo oblate as primitive fluidity requires; yet its form is fo nicely adjufted to its rotation, that the thin film of water on it is diftributed with perfect uniformity. We are obliged

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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 obſtiterit 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.

610. In § 584, and feveral following paragraphs, we explained the effects arifing from the inclination of the Moon's orbit round the Earth to the plane of the Earth's orbit round the Sun. We faw, for example, that when the interfection of the two planes is in the line A B (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 femicircle A C B, and fhe defcribes another path A *c b*, croffing the ecliptic in *b*, nearer to A than B is. In the other half of her orbit, the fame deviation is continued, and the Moon again croffes the ecliptic before fhe come to A, croffes her laft path near to *c*, and the ecliptic a third time at *d*, and fo on continually. Hence arifes the retrograde motion of the nodes of the lunar orbit. We fhewed that this obtains, in a greater or lefs degree, in every pofition of the nodes, except when they are in the line of fyzigy.

What is true of one moon, would be true of any number: It would be true, were there a complete ring of moons furrounding the Earth, not adhering to one another. We faw that the inclination of the orbit is continually changing, being greateft when the nodes are in the line of the fyzigies, and fmalleft when they are in quadrature. Now, if we apply this to a ring of moons, we fhall find that it will never be a ring that is all in one plane, except when the nodes are in the fyzigies, and at all other times will be warped, or out of fhape. Now, let the moons all cohere, and the ring become ſtiff; and let this happen when its nodes are in ſyzigy. 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 ſyzigy 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 ſyzigies. 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 ſyzigies, 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 cannot now produce the effect, without dragging with it the whole mafs of the Earth. But the Earth is at perfect liberty in empty fpace, and being retained by nothing, yields to every impulfe, and therefore yields to this action of the ring.

Now, there is fuch a ring furrounding the Earth, having precifely this tendency. The Earth may be confidered as a fphere, on which there is fpread a quantity of redundant matter which makes it fpheroidal. The gravitation of this redundant matter to the Sun fuftains all thofe difturbing forces which act on the inflexible ring of moons; and it will be proved, in its proper place, that the effect in changing the pofition of the globe is $\frac{1}{5}$ of what it would be, if all this redundant matter were accumulated on the equator. It will alfo appear that the force by which every particle of it is urged to or from the plane of the ecliptic, is as its diftance from that plane. Indeed, this appears already, becaufe all the difturbing forces acting on the particles of this ring are fimilar, both in direction and proportion, to thofe which we fhewed to influence the Moon in the fimilar fituations of her monthly courfe round the Earth. Similar effects will therefore be produced.

Let us now fee what thofe effects will be.—The lunar nodes continually recede; fo will the nodes of this equatoreal ring, that is, fo will the nodes of the equator, or its interfection with the ecliptic. But the interfections 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 (23½ 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 23½ 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

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of the hole, as the axis of the top veers round. The axis becomes continually more oblique, without any periods of recovering its former pofition, and, in this refpect only the phenomena differ from thofe of the preceffion.

It was affirmed that the obliquity of the equator is greateft at the equinoxes, and fmalleft at the folftices. This would be the cafe, did the redundant ring inftantly attain the pofition which makes an *equilibrium* of action. But this cannot be; chiefly for this reafon, that it muft drag along with it the whole infcribed fphere. During the motion from the equinox to the next folftice, the Earth's equator has been urged toward the ecliptic, and it muft approach it with an accelerated motion. Suppofe, at the inftant of the folftice, all action of the Sun to ceafe; this motion of the terreftrial globe would not ceafe, but would go on for ever, equably. But the Sun's action continuing, and now tending to raife 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 rifes again from the ecliptic. When the Sun is in the equinox, the elevation of the equator fhould be greateft; but, as it arrived at this pofition with an accelerated motion, it continues to rife (with a retarded motion) till the continuance of the Sun's depreffing force puts an end to this rifing; and now the effect of the depreffing force begins to appear. For thefe reafons, it happens that the greateft obliquity of 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 conſpicimus, 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

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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 equatoreal 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 23½ 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 fame. But as, in 19 years, the Moon's nodes take all fituations, the action of the Moon is very variable. It was one of the effects of this variation that Bradley difcovered. 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 depreffed towards the ecliptic, and the diminution of its inclination at laft amounted to 18 feconds. Dr Bradley faw this by its increafing the declination of a ftar properly fituated. After nine years, when the Moon was in fuch a fituation that fhe never went more than 17° from the Earth's equator, the fame ftar had 18″ lefs declination.

614. This change in the inclination of the Earth's equator is accompanied with a change in the preceffion of the equinoxes. This muft increafe as the equator is more open when viewed from the Moon. In the year in which the lunar afcending node is in the vicinity of the vernal equinox, the preceffion is more than 58″; and it is but 43″ when the node is near the autumnal equinox. Thefe are very confpicuous changes, and of eafy obfervation, although long unnoticed, while blended with other anomalies equally unknown.

Few difcoveries in aftronomy have been of more fervice to the fcience than this of the nutation, and that of aberration, both by Dr Bradley. For till they were known, there was an anomaly, which might fometimes amount to 58″ (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 Friſius

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 $\frac{1}{7}$ 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

ſius in the ſecond part of his Coſmographia, as the moſt perſpicuous of any that I am acquainted with. The elaborate performance of Mr Walmeſely, 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 confider but the ring itfelf, it would be a very eafy 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 fatellites, make it next to unmanageable. It has not been attempted, that I know of. It may (I think) be deduced, from the Greenwich obfervations fince 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^s 17° 13′ and 11^s 17° 13′. This may be received as more exact than the determination given in art. 380.

I faid, in art. 370, that we have feen too little of the motions of Ceres and Pallas to announce the elements of their theories with any thing like precifion. But, that they may not be altogether omitted, the following may be received as of moft authority.

	Ceres.	*Pallas.*
Mean diftance - - - - - -	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

Thefe bodies prefent fome very fingular circumftances to our ftudy; their diftances and periods being almoft the fame, and their longitudes at prefent differing very little. They differ confiderably 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. Tranſ.* 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 enthuſiaſm) that we can judge of what is moſt excellent, we take it for granted that this creature of our heated imagination muſt alſo appear moſt excellent to the Supreme Mind. From this principle, theologians have ventured to lay down the laws by which God himſelf muſt regulate his actions. No wonder that, on ſo fanciful a foundation as our capacity to judge of what is moſt excellent, have been erected the moſt extravagant fabrics, and that, in the exuberance of religious zeal, the Author of all has been deſcribed as the moſt limited Agent in the univerſe, forced, in every action, to regulate himſelf by our poor and imperfect notions of what is excellent. We, who vaniſh from the ſight, at the diſtance of a neighbouring hill—whoſe greateſt works are inviſible from the Moon—whoſe whole habitation is not viſible to a ſpectator in Saturn—ſhall ſuch creatures pretend to judge of what is ſupremely excellent?

Let us not pretend even to gueſs at the ſpecific laws by which the conduct of the Divinity muſt be directed, except in ſo far as it has pleaſed him to declare them to us. We ſhall purſue the only ſafe road in this ſpeculation, if we endeavour to diſcover the laws by which his viſible and comprehenſible works are actually conducted. The more we diſcover of theſe, the more do we find to fill us with admiration and aſtoniſhment. The only ſpeculations in which we can indulge, without the continual danger of going aſtray, are thoſe which enlarge our notions of the ſcene on which it has pleaſed the Almighty to diſplay his perfections. This will be

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the undoubted effect of enlarging the field of our own obfervation. After examining this lower world, and obferving the nice and infinitely various adjuftments of means to ends here below, we may extend our obfervation beyond this globe. Then fhall we find that, as far as our knowledge can carry us, there is the fame art, and the fame production of good effects by beautifully contrived means. We have lately difcovered a new planet, far removed beyond the formerly imagined bounds of the planetary world. This difcovery fhews us that if there are thoufands more, they may be for ever hid from our eyes by their immenfe diftance. Yet *there* we find the fame care taken that their condition fhall be permanent. They are influenced by a force directed to the Sun, and inverfely as the fquare of the diftance from him; and they defcribe ellipfes. This planet is alfo accompanied by fatellites, doubtlefs rendering to the primary and its inhabitants fervices fimilar to what this Earth receives from the Moon. All the comets of whofe motions we have any precife knowledge, are equally fecured; none feems to defcribe a parabola or hyperbola, fo as to quit the Sun for ever.

This mark of an intention that this noble fabric fhall continue for ever to declare itfelf the work of an Almighty and Kind Hand, naturally carries forward the mind into that unbounded fpace, of which our folar fyftem occupies fo inconfiderable a portion. The mind revolts at the thought that this is ftudded with ftars for no other purpofe than to affift the aftronomer in his com-

 putations,

putations, and to furnifh a gay fpectacle to the unthinking multitude. We fee nothing here below, or in our fyftem, which anfwers but one folitary purpofe, and we require that a pofitive reafon fhall be given for limiting the Hoft of Heaven to fo ignoble an office. As fuch has not been given, we indulge ourfelves in the pleafing thought that the ftars make a part of the univerfe, no lefs important in purpofe than great in extent. We are juftifiable, by what we in fome meafure underftand, in fuppofing each ftar a fun, the centre of a planetary fyftem, full of enjoyment like our own, and fo conftructed as to laft for ever.

When the philofopher indulges himfelf in thofe amazing, but pleafing thoughts, he muft regulate his fpeculations by analogies and refemblances to things more familiarly known to him. We muft fuppofe thofe fyftems to refemble our own, and that they are kept together by a gravitation in the inverfe duplicate ratio of the diftances. For we know that this alone will infure permanancy and good order.

But in fo doing, we extend the influence of gravity to diftances inconceivably greater than any that we have yet confidered, and we come at laft to believe that gravitation is the bond of connexion which unites the moft diftant bodies of the vifible univerfe, rendering the whole one great machine, for ever operating the moft magnificent purpofes, worthy of its All-Perfect Creator. And, when we fee that fuch a connexion is neceffary for this end, we are apt to imagine that gravity is *effential* to or

indifpenfable

indifpenfable in that matter that is to be moulded into a world.

But let not our ignorance miflead us, nor let us meafure every thing by that fmall fcale which God has enabled us to ufe, unlefs we can fee fome circumftances of refemblance in the appearances, which may juftify the application.

* A frame of material nature of any kind cannot be conceived by the mind, without fuppofing that the matter of which it confifts is influenced by fome active powers, conftituting 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 fymmetrical forms, unlefs the fpirit of its author were to animate thofe dead maffes, fo as to bring forth change, and order, and beauty. Our illuftrious Newton

* For many of the thoughts in what follows, the reader is indebted to a very ingenious pamphlet, publifhed by Caddel & Davies in 1777, entitled, *Thoughts on General Gravitation.* It is much to be regreted that the author has not availed himfelf of the fuccefsful refearches of aftronomers fince that time, and profecuted his excellent hints. If it be the performance of the perfon whom I fuppofe to be the author, I have fuch an opinion of his acutenefs, and of his juftnefs of thought, that I take this opportunity of requefting him to turn his attention afrefh to the fubject. His advantages, from his prefent fituation and connexions, are precious, and fhould not be loft.

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.

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That the influence of gravity extends into the boundlefs void, to the greateft affignable diftance, feems to be almoft the hinge of the Newtonian philofophy. At leaft, there is nothing that warrants any limit to its action. Father Bofcovich indeed fhews that all the phenomena may be what they are, without this as a neceffary confequence. But he is plainly induced to bring forward the limitation in order to avoid what has been thought a neceffary confequence of the indefinite extenfion of gravity; and what he offers is a mere poffibility.

Now, if fuch extenfion of gravitation be infeparable, in fact, from its nature, then, if all the bodies of our fyftem are at reft in abfolute fpace, no fooner does the influence of general gravitation go abroad into the fyftem, than all the planets and comets muft begin to approach the Sun, and, in a very fmall number of days, the whole of the folar fyftem muft fall into the Sun, and be deftroyed.

But, that this fair order may be preferved, and accommodated to this extended influence of gravity, which appears fo effential to the conftitution of the feveral parts of the fyftem, we fee a moft fimple and effectual prevention, by the introduction of *projectile forces*, and *progreffive motion*. For upon thefe being now combined, and properly adjufted with the variation of gravity, the planets are made to revolve round the Sun in ftated courfes, by which their continual approach to the Sun and to one another is prevented, and the adjuftment is made with fuch exquifite 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, foraſmuch 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 inconceiveable 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

are feen is the fame with that by which we behold our Sun and his attending planets. It moves with the fame velocity, as we difcover by comparing the aberration of the fixed ftars with the eclipfes of Jupiter's fatellites. It is refracted and reflected according to the fame laws. It confifts of the fame colours. No opinion can be formed therefore of the folar light, which muft not alfo be adopted with refpect to the light of the fixed ftars. The medium of vifion muft be acted on in the fame manner by both, whether we fuppofe it the undulation of an æther, or the emiffion of matter from the luminous body. In either cafe, a mechanical connexion obtains between thofe bodies, however diftant, and our fyftem. Such a connexion in mechanical properties induces us to fuppofe that gravitation, which we know reaches to a diftance which exceeds all our diftinct conceptions, extends alfo to the fixed ftars.

If this be really the cafe, motion muft enfue, even in producing the final ruin of the vifible univerfe; and periodic motion is indifpenfably neceffary for its permanency.

If all the fixed ftars, and our Sun, were equal, and placed at equal diftances, in the angles of regular folids, their mutual ruinous approach could hardly be perceived. For in every moment, they would ftill have the fame relative pofitions, and an increafe of brightnefs is all that could enfue after many ages. But if they were irregularly placed, and unequal, their relative pofitions 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° 355.) Aldebaran has moved to the ſouth about 35′. Syrius 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.) Syrius 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 *∂*.

Theſe

Thefe motions feem to indicate a motion in our fyftem. Moft of the ftars have moved toward the fouth. The ftars in the northern quarters feem to widen their relative pofitions, while thofe in the fouth feem to contract their diftances. Dr Herfchel thinks that a comparifon of all thefe changes indicates a motion of our Sun with his attending planets toward the conftellation Hercules (Phil. Tranf. 1788.) A learned and ingenious friend thinks it not impoffible to difcover this motion by means of the aberration of the ftars. Suppofe the Sun and planets to be moving toward the Pole-ftar, and that his motion is 100 times greater than that of the Earth in her orbit (a very moderate fuppofition, when we compare the orbital motion of the Earth with that of the Moon), every equatoreal ftar will appear about 34′ north of its true place, when viewed through a common telefcope, but only 23′ when viewed through a telefcope filled with water. The declination of every fuch ftar will be 11′ lefs through a water telefcope than through a common telefcope. Stars out of the equator will have their declination diminifhed by a water telefcope 11′ × cof. declin.

In 1761, the ingenious Mr Lambert publifhed his Letters on Cofmology (in the German language), in which he has confidered this fubject with much attention and ingenuity. He treats of the motion of the Sun round a central body—of fyftems of fyftems, or milky ways, carried round an immenfe body—of fyftems of fuch galaxies—and of the great central body of the univerfe. In thefe

fpeculations

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. Tranſ.* 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 Goödricke, 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. Tranſ.* 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 χ Cygni, having a period of $415\frac{1}{2}$ days. See *Phil. Tranſ.* N° 343.; alſo *Mem. Acad. Paris*, 1719, 1759.

Another remarkable ſtar is o Ceti, having a period of 334 days. (See *Phil. Tranſ.* N° 134. 346.; *Mem. Par.* 1719.)

There is another ſuch, cloſe to γ Cygni.

The double ſtar ζ Lyræ exhibits very ſingular appearances, the ſouthernmoſt ſometimes appearing double, and ſometimes accompanied by more little ſtars. Griſchoff

ſchoff 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. Tranſ.* 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. Tranſ.* 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 unſearchable 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 Brahé 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 became 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 Flamſtead, 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 milſtone. They are of various colours, white, yellow, roſe-coloured, &c. Dr Herſchel, in ſeveral of the late volumes of the Philoſophical

cal Tranfactions, has given us the places of a vaft number of nebulæ, with curious defcriptions of their peculiar appearances, and a feries of moft ingenious and interefting reflections on their nature and conftitution. His *Thoughts on the Structure of the Heavens* are full of moft curious fpeculation, and fhould be read by every philofopher.

When we reflect that thefe fingular objects are not, like the fixed ftars, brilliant points, which become fmaller when feen through finer telefcopes, but have a fenfible, and meafureable diameter, fometimes exceeding 2′; and when we alfo 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 fubtend an angle of two feconds when taken to the neareft fixed ftar, what muft we think of thefe nebulæ? One of them is certainly fome thoufands of times bigger than the Earth's orbit. Although our fineft telefcopes cannot feparate it into ftars, it is ftill probable that it is a clufter. It is not unreafonable to think, with Dr Herfchel, that this object, which requires a telefcope to find it out, will appear to a fpectator in its centre much the fame as the vifible heavens do to us, and that this ftarry heaven, which, to us, appears fo magnificent, is but a nebulous ftar to a fpectator placed in that nebula.

The human mind is almoft overpowered by fuch a thought. When the foul is filled with fuch conceptions of the extent of created nature, we can fcarcely avoid exclaiming, ' Lord, what then is man that thou art ' mindful

'mindful of *him!*' Under fuch impreffions, David fhrunk into nothing, and feared that he fhould be forgotten amongft fo many great objects of the Divine attention. His comfort, and ground of relief from this dejecting thought, are remarkable. 'But,' fays he, 'thou haft made man but a little lower than the angels, 'and haft crowned him with glory and honour.' David corrected himfelf, by calling to mind how high he ftood in the fcale of God's works. He recognifed 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 mechanifm of the folar fyftem, and is in itfelf both curious and important. I mean the tides of our ocean. Although it appears improper to call this an aftronomical phenomenon, yet, as it is moft evidently connected with the pofition of the Sun and Moon, we muft attribute this connexion in fact to a natural connexion in the way of caufe and effect.

Of the Tides.

617. It is a very remarkable operation of nature that we obferve on the fhores of the ocean, when, in the calmeft weather, and moft ferene fky, the vaft body of waters that bathe our coafts advances on our fhores, inundating

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.

621. Many obfervations on the feries of phenomena which completes a period of the tides are to be found in the books of hydrography, and the inftructions for mariners, to whom the exact knowledge of the courfe of the tides is of the utmoft importance. But we never had any good collection of obfervations, from which the laws of their progrefs could be learned, till the Academy of Paris procured an order from government to the officers at the ports of Breft and Rochefort, to keep a regifter of all the phenomena, and report it to the Academy. A regifter of obfervations was accordingly continued for fix years, without interruption, at both ports, and the obfervations were publifhed, forming the moft complete feries that is to be met with in any department of fcience, aftronomy alone excepted. The younger Caffini undertook the examination of thefe regifters, in order to deduce from them the general laws of the tides. This tafk he executed with confiderable fuccefs; 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 fcarcely any thing been added to the vague experience of illiterate pilots and fifhermen, except two differtations by Wallis and Flamftead, publifhed in the Philofophical Tranfactions.

622. It is not likely, notwithftanding this excellent collection of obfervations, that our knowledge would have proceeded much farther, had not Newton demonftrated

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 $12^h\ 25'$, 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 $24^h\ 50'$. 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 Rocheſort 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. $24^h\ 50'$, 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 apogeo*. 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 O B C A may re-

 preſent

prefent the watery fphere, and M any particle of the water. The central particle E gravitates to the Sun with a force which may be reprefented by E S. The gravitation of the particle M muft be meafured by M G. This force M G may be conceived as compounded of M F, equal and parallel to E S, and of M H. The force M F occafions no alteration in the gravitation of M to the Earth, and M H is the only difturbing force. We found that this conftruction may be greatly fimplified, and that M I may be fubftituted for M H without any fenfible error, becaufe it never differs from it more than $\frac{1}{392}$. We therefore made E I, in fig. 60, $=3$ MN, and confidered M I as the difturbing force. This conftruction is applicable to the prefent queftion, with much greater accuracy, becaufe the radius of the Earth is but the fixtieth part of that of the Moon's orbit. This reduces the error to $\frac{1}{23520}$, a quantity altogether infenfible.

633. Therefore let O A C B (fig. 68.) be the terraqueous globe, and C S a line directed to the Sun, and B E A the fection by that circle which feparates the illuminated from the dark hemifphere. Let P be any particle, whether on the furface or within the mafs. Let Q P N be perpendicular to the plane B A. Make E I $=$ 3 P N, and join P I. P I is the difturbing force, when the line E S is taken to reprefent the gravitation of the particle E to the Sun. This force P I may be conceived to be compounded of two forces P E and P Q.

P E

P E tends to the centre of the Earth. P Q tends from the plane B A, or toward the Sun.

If this conſtruction be made for every particle in the fluid ſphere, it is evident that all the forces P E balance one another. Therefore they need not be conſidered in the preſent queſtion. But the forces P Q evidently diminiſh the terreſtrial gravitation of every particle. At C the force P Q 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 P N. There is therefore a force acting in the direction N P on every particle in the canal P N. And this force is proportional to the diſtance of the particle from the plane B A (for P Q is always = 3 P N). 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 N P a ſmall portion P *p*, whoſe weight may compenſate the diminution in the weight of the column N P. A ſimilar addition may be made to every ſuch column perpendicular to the plane B E A. This being ſuppoſed, the ſpherical figure of the globe will be changed into that of an elliptical ſpheroid, having its axis in the line O C, and its poles in O and C (569.)

Without making this addition to every column N P, we may underſtand how the *equilibrium* may be reſtored by the waters ſubſiding all around the circle whoſe ſection is B A, and riſing on both ſides of it. For it was ſhewn (564.) that in a fluid elliptical ſpheroid of gravi-

 tating

tating matter, the gravitation of any particle P to all the other particles may be refolved into two forces P N and P M perpendicular to the plane B A and to the axis O C, and proportional to P N and P M; 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 inverfely as the diameters O C and B A. Now this may be the cafe here. For the forces fuperadded to the terreftrial gravitation of any particle are, 1*ft*, A force P E, proportional to P E. When this is refolved into the directions P N and P M, the forces arifing in this refolution are as P N and P M, and therefore in the due proportion: 2*d*, The force P Q, which is alfo as P N. It is evident therefore that this mafs may acquire fuch a protuberancy at O and C, that the force at O fhall be to the force at B as B A to O C, or as E A to E C. We are alfo taught in § 585. what this protuberance muft be. It muft be fuch that four times the mean gravity of a particle on the furface is to five times the difturbing force at O or C as the diameter B A is to the excefs of the diameter O C. This ellipticity is expreffed by the fame formula as in the former cafe, viz. $\frac{x}{r} = \frac{4c}{5g}, = \frac{EC - FA}{EC}$.

634. Thus we have difcovered that, in confequence of the unequal gravitation of the matter in the Earth to the Sun, the waters will affume the form of an oblong elliptical fpheroid, 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 $= 3\,CE$. Let S and E be the quantity of matter in the Sun and in the Earth.

Then (fig. 59.) $F : G = 3\,CE : CG$

$$G : W = \frac{S}{CS^2} : \frac{E}{CE^2} \quad (465.)$$

therefore $$F : W = \frac{3\,CE \times S}{CS^2} : \frac{CG \times E}{CE^2} =$$

$\frac{3\,S}{CS^2 \times CG} : \frac{E}{CE^3}$. But, becauſe $CS^2 : ES^2 = ES : CG$, we have $CS^2 \times CG = ES^2 \times ES, = ES^3$. Therefore $F : W = \frac{3\,S}{ES^3} : \frac{E}{EC^3}$. Now $E : S = 1 : 338343$, and $EC : ES = 1 : 23668$. This will give $\frac{3\,S}{ES^3} : \frac{E}{EC^3}$ $= 1 : 12773541, = F : W$.

Finally, $4\,W : 5\,F = CE : CE - AE$. 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 in all its particles. This cannot ever be the cafe, becaufe fome time muft elapfe before an accelerating force can produce any finite change in the difpofition of the waters. But the contemplation of this figure gives us the moft diftinct notion of the forces that are in action, and of their effects; and we can afterwards ftate the difference that muft obtain becaufe the figure is not completely attained.

Suppofing it really attained, it follows that the ocean will be moft elevated in thofe places which have the Sun in the zenith or nadir, and moft depreffed in thofe places where the Sun is feen in the horizon. While the Earth turns round its axis, the pole of the fpheroid keeps ftill toward the Sun, as if the waters ftood ftill, and the folid nucleus turned round under it. The phenomena may perhaps be eafier conceived by fuppofing the Earth to remain at reft, and the Sun to revolve round it in 24 hours from eaft to weft. The pole of the fpheroid follows him, as the card of a mariner's compafs follows the magnet; and a fpectator attached to one part of the nucleus will fee all the viciffitudes of the tide. Suppofe the Sun in the equinox, and the obferver alfo on the Earth's equator, and the Sun juft rifing to him. The obferver is then in the loweft part of the watery fpheroid. As the Sun rifes above the horizon, the water alfo rifes; and when the Sun is in the zenith, the pole of the fpheroid has now reached the obferver, and the water is two feet deeper than it was at fun-rife. The Sun now approaching the weftern 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 A D of its ſummit above the equally capacious ſphere is very nearly double of the depreſſion F E 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 D F df is equal to the ſpheroid A E a Q, 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 C E does not differ ſenſibly from C D (only eight inches in near 4000 miles), therefore E x may be accounted equal to D A. But E x is not ſenſibly different from twice E F. Therefore the propoſition is manifeſt.

637. In ſuch an elliptical ſpheroid, the elevation I L 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 K I 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, H I M perpendicular to C A, and I p N perpendicular to C E. The triangles C I N and p I L are ſimilar.

Therefore $p\,I : I\,L = C\,I : I\,N, = \text{rad.} : \text{cos.}\,I\,C\,A$
but, by the ellipse $A\,B : p\,I = A\,C : I\,N, = \text{rad.} : \text{cos.}\,I\,C\,A$
therefore $A\,B : I\,L = \text{rad.}^2 : \text{cos.}^2\,I\,C\,A$
and I L is always in the proportion of cos.², I C A, and is $= A\,B \times \text{cos.}^2, I\,C\,A$, radius being $= 1$.
In like manner $H\,I : I\,K = C\,I : I\,M = \text{rad.} : \text{sin.}\,I\,C\,A.$
and $G\,E : H\,I = E\,C : I\,M = \text{rad.} : \text{sin.}\,I\,C\,A$
therefore $G\,E : K\,I = \text{rad.}^2 : \text{sin.}^2\,I\,C\,A$
and $K\,I$ is $= A\,B \times \text{sin.}^2\,I\,C\,A.$

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 $i\,l - m\,l = A\,B \times \text{cos.}^2\,i\,C\,A - \frac{1}{3}\,A\,B, = A\,B \times \overline{\text{cos.}^2\,i\,C\,A - \frac{1}{3}}$, and the depression $n\,r$ of a point r is $k\,r - k\,n = A\,B \times \text{sin.}^2\,r\,C\,A - \frac{2}{3}\,A\,B, = A\,B \times \overline{\text{sin.}^2\,r\,C\,A - \frac{2}{3}}$.

It will be convenient to employ a symbol for expressing the whole difference A B or G E 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 x.

The elevation $m\,i$ is $= S \times \text{cos.}^2\,x - \frac{1}{3}\,S.$

The depression $n\,r$ is $= S \times \text{sin.}^2\,x - \frac{2}{3}\,S.$

639. The spheroid intersects the equicapacious sphere in a point so situated that $S \times \text{cos.}^2\,x - \frac{1}{3}\,S = 0$, that is, where $\text{cos.}^2\,x = \frac{1}{3}$. This is $54^\circ\,44'$ from the pole of

Fig. 69.

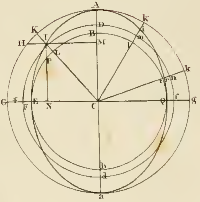


Fig. 68.

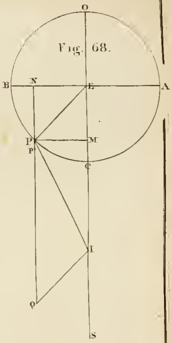
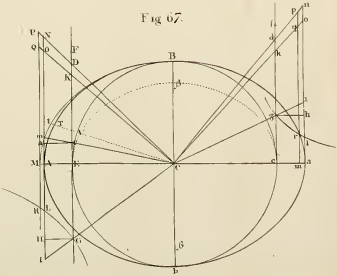


Fig 67.



of the fpheroid, and 35° 16′ from its equator, a fituation that has feveral remarkable phyfical properties. We have already feen (572.) that on this part of the furface the gravitation is the fame as if it were really a perfect fphere.

640. The ocean is made to affume an eccentric form, not only by the unequal gravitation of its waters to the Sun, but alfo by their much more unequal gravitation to the Moon; and, although her quantity of matter is very fmall indeed, when compared with the Sun, yet being almoft 400 times nearer, the inequality of gravitation is increafed almoft 400 × 400 × 400 times, and may therefore produce a fenfible effect. * We cannot help prefuming that it does, becaufe the viciffitudes of the tides have a moft diftinct reference to the pofition of the Moon. Without going over the fame ground again, it is plain that the waters will be accumulated under the Moon, and in the oppofite part of the fpheroid, in the fame manner as they are affected by the Sun's action.

Therefore

* The diftance 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 fhall fee that it is nearly $2\frac{1}{2}$ times greater. From which we fhould infer that the quantity of matter in the Moon is nearly $\frac{1}{71}$ of that in the Earth. This feems the beft information that we have on this fubject.

of the ſpheroid, and 35° 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 × 400 × 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 $\frac{1}{71}$ of that in the Earth. This ſeems the beſt information that we have on this ſubject.

Therefore let M reprefent the elevation of the pole of the fpheroid above the equicapacious fphere that is produced by the unequal gravitation to the Moon, and let y be the angular diftance of any part of this fpheroid from its pole. We fhall then have

The elevation of any point $= M \times \text{cof.}^2 y - \frac{1}{3} M$.
The depreffion $= M \times \text{fin.}^2 y - \frac{2}{3} M$.

641. In confequence of the fimultaneous gravitation to both luminaries, the ocean muft affume a form differing from both of thefe regular fpheroids. It is a figure of difficult inveftigation, but all that we are concerned in may be determined with fufficient accuracy by means of the following confiderations.

We have feen that the *change* of figure induced on the fpheroidal ocean of the revolving globe is nearly the fame as if it were induced on a perfect fphere. Much more fecurely may we fay that the change of figure, induced on the ocean already difturbed by the Sun, is the fame that the Moon would have occafioned on the undifturbed revolving fpheroid. We may therefore fuppofe, without any fenfible error, that the change produced in any part of the ocean by the joint action of the two luminaries is the fum or the difference of the changes which they would have produced feparately.

642. Therefore, fince the poles of both fpheroids are in thofe parts of the ocean which have the Sun and the Moon in the zenith, it follows that if x 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 $S \times \text{coſ.}^2 x - \frac{1}{3} S + M \times \text{coſ.}^2 y - \frac{1}{3} M$. And the depreſſion in any place will be $S \times \text{ſin.}^2 x - \frac{2}{3} S + M \times \text{ſin.}^2 y - \frac{2}{3} M$. This may be better expreſſed as follows.

$$\text{Elevation} = S \times \text{coſ.}^2 x + M \times \text{coſ.}^2 y - \tfrac{1}{3}\overline{S + M}.$$

$$\text{Depreſſion} = S \times \text{ſin.}^2 x + M \times \text{ſin.}^2 y - \tfrac{2}{3}\overline{S + M}.$$

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 E D M (fig. 70.) It is plain that one luminary tends to produce an elevation above the equicapacious ſphere A O B C, 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 E S.

The

The elevation at $S = S - \frac{1}{3}\overline{S + M}, = \frac{2}{3}S - \frac{1}{3}M.$
The depreffion at $M = S - \frac{2}{3}\overline{S + M}, = \frac{1}{3}S - \frac{2}{3}M.$
The elevation at S above the infcribed fpheroid $= S - M.$
The elevation at M above the fame $= M - S.$

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 fhort of that of the Sun. Now, it is a matter of obfervation, that when the Moon is in quadrature, it is high water in the open feas under the Moon, and low water under the Sun, or nearly fo. This obfervation confirms the conclufion drawn from the nutation of the Earth's axis, that the difturbing force of the Moon exceeds that of the Sun. This criterion has fome uncertainty, owing to the operation of local circumftances, by which it happens that the fummit of the water is never fituated either under the Sun or under the Moon. But even in this cafe, we find that the high water is referable to the Moon, and not to the Sun. It is always fix hours of the day later than the high water at full or change. This correfponds with the elongation of the Moon fix hours to the eaftward. The phenomena of the tides fhew further that, at this time, the waters under the Sun are depreffed below the natural furface of the ocean. This fhews that M is more than twice as great as S.

645. When the Moon has any other pofition befides thefe two, the place of high water muft be fome 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 $S \times \text{coſ.}^2 x + M \times \text{coſ.}^2 y - \frac{1}{3}\overline{S + M}$ is a maximum. Or, ſince $\frac{1}{3}\overline{S + M}$ is a conſtant quantity, we muſt find where $S \times \text{coſ.}^2 x + M \times \text{coſ.}^2 y$ is a maximum. Now, accounting the tabular ſines and coſines as fractions of radius, $= 1$, we have

$$\text{Coſ.}^2 x = \tfrac{1}{2} + \tfrac{1}{2}\,\text{coſ.}\ 2\,x$$

and $$\text{Coſ.}^2 y = \tfrac{1}{2} + \tfrac{1}{2}\,\text{coſ.}\ 2\,y.$$

For let A B S D (fig. 71.) be a circle, and A S, B D two diameters croſſing each other at right angles. Deſcribe on the ſemidiameter C S the ſmall circle C m S h, having its centre in d. Let H C make any angle x with C S, and let it interſect the ſmall circle in h. Draw $d\,h$, S h, producing S h till it meet the exterior circle in S, and join A s, C s. Laſtly, draw $h\,o$ and $s\,r$ perpendicular to C S.

S b is perpendicular to C h, and C S : C h = rad. : coſ.

cof. H C S, and $CS : Co = R^2 : \text{cof.}^2\ HCS$. The angle SCs is evidently $= 2\ SCH = Sdh$ and $Ar = 2\ Co$. Now if CS be $= 1$; $Cr = \text{cof.}^2\ 2x$; $Ar = 1 + \text{cof.}\ 2x$. Therefore $Co = \frac{1}{2} + \frac{1}{2}\ \text{cof.}\ 2x$. In like manner $\text{cof.}^2 y = \frac{1}{2} + \frac{1}{2}\ \text{cof.}\ 2y$.

Therefore we muft have $\frac{S}{2} + \frac{S \times \text{cof.}\ 2x}{2} + \frac{M}{2} + \frac{M \times \text{cof.}\ 2y}{2}$ a maximum, or, neglecting the conftant quantities $\frac{S}{2}$, $\frac{M}{2}$, and the conftant divifor 2, we muft have $S \times \text{cof.}\ 2x + M \times \text{cof.}\ 2y$ a maximum.

Let A B S D (fig. 71.) be now a great circle of the Earth, paffing through thofe points S and M of its furface which have the Sun and the Moon in the zenith. Draw the diameter S C A, and crofs it at right angles by B C D. Let S *d* be to *d a* as the accumulating force of the Moon to the accumulating force of the Sun, that is, as M to S, which proportion we fuppofe known. Draw C M in the direction of the Moon's place. It will cut the fmall circle in fome point *m*. Join *m a*. Let H be any point of the furface of the ocean. Draw C H, cutting the fmall circle in *h*. Draw the diameter *h d h'*. Draw *m t* and *a x* perpendicular to *h h'*, and *a y* parallel to *h h'*, and join *m d*. Alfo draw the chords *m h* and *m h'*.

In this conftruction, *m d* and *d a* reprefent M and S, the angle $MCH = y$, and $SCH = x$. It is farther manifeft that the angle $mdh = 2\ mCh, = 2y$, and that $dt = M \times \text{cof.}\ 2y$. In like manner $hdS = 2\ HCS$, $= 2x$,

$= 2x$, and $dx = da \times$ cof. $2x$, $= S \times$ cof. $2x$. Therefore $tx = S \times$ cof. $2x + M \times$ cof. $2y$. Moreover $tx = ay$, and is a maximum when ay is a maximum. This muft happen when ay coincides with am, that is, when hd is parallel to am.

Hence may be derived the following conftruction.

Let A M S (fig. 72.) be, as before, a great circle whofe plane paffes through the Sun and the Moon. Let S and M be thofe points which have the Sun and the Moon in the zenith. Defcribe, as before, the circle CmS, cutting C M in m. Make $S d : d a = M : S$, and join $m a$. Then, for the place of high water, draw the diameter $h d h'$ parallel to $m a$, cutting the circle C m S in h. Draw C h H cutting the furface of the ocean in H and H′. Then H and H′ are the places of high water. Alfo draw C h', cutting the furface of the ocean in L and L′. L and L′ are the places of low water in this circle.

For, drawing $m t$ and $a x$ perpendicular to $h h'$, it is plain that $tx = M \times$ cof. $2y + S \times$ cof. $2x$. And what was juft now demonftrated fhews that tx is in its maximum ftate. Alfo, if the angle L C S $= u$, and L C M $= z$, it is evident that $dx = S \times$ cof. adx, $= S \times$ cof. $h'dS$, $= S \times$ cof. $2h'$CS, $= S \times$ cof. 2 L C S, $= S \times$ cof. $2u$; and in like manner, $td = M \times$ cof. $2z$; and therefore $tx = S \times$ cof. $2u + M \times$ cof. $2z$, and it is a maximum.

It is plain, independent of this conftruction, that the places of high and low water are 90° afunder; for the two hemifpheres of the ocean muft be fimilar and equal, and the equator muft be equidiftant from its poles.

648. Draw df perpendicular to ma. Then, if dS be taken to reprefent the whole tide produced by the Moon, that is, the whole difference in the height of high and low water, ma will reprefent the compound tide at H, or the difference between high and low water correfponding to that fituation of the place H with refpect 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 depreffion below it at L is $\overline{S \times \text{fin.}^2 u - \frac{2}{3}} + M \times \overline{\text{fin.}^2 z - \frac{2}{3}}$. But $\text{fin.}^2 u = \text{cof.}^2 x$, and $\text{fin.}^2 z = \text{cof.}^2 y$. Therefore the depreffion at L is $S \times \overline{\text{cof.}^2 x - \frac{2}{3}} + M \times \overline{\text{cof.}^2 y - \frac{2}{3}}$. The fum of thefe 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 $dmf = mdh, = 2mCh, = 2y$. Therefore $md \times \text{cof. } dmf = M \times \text{cof. } 2y$. In like manner $af = S \times \text{cof. } 2x$.

The point a muft be within or without the circle CmS, according as M is greater or lefs than S, that is, according as the accumulating force of the Moon is greater or or lefs than that of the Sun. It appears alfo that, in the firft cafe, H will be nearer to M, and in the fecond cafe, it will be nearer to S.

Thus have we given a conftruction that feems to exprefs all the phenomena of the tides, as they will occur to a fpectator placed in the circle paffing through thofe points which have the Sun and Moon in the zenith. It marks the diftance of high water from thofe two places, and therefore, if the luminaries are in the equator, it marks the time that will elapfe between the paffage of the Sun or Moon over the meridian and the moment of high water. It alfo expreffes the whole height of the tide of that day. And, as the point H may be taken without any reference to high water, we fhall then obtain the ftate of the tide for that hour, when it is high water in its proper place H. By confidering this conftruction for the different relative pofitions of the Sun and Moon, we fhall obtain a pretty diftinct notion of the feries of phenomena which proceed in regular order during a lunar month.

649. To obtain the greater fimplicity in our firft and moft general conclufions, we fhall firft fuppofe both luminaries in the equator. Alfo, abftracting our attention from the annual motion of the Sun, we fhall confider only the relative motion of the Moon in her fynodical revolution, ftating the phenomena as they occur when the Moon has got a certain number of degrees away from the Sun; and we fhall always fuppofe that the watery fpheroid has attained the form fuited 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 ſurpriſe 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 C M, *a m*, C S, *d h*, C H, 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 M C is perpendicular to C S, the point *m* coincides with C, *a m* with *a* C, and *d h* with *d* C.

652. 3. While the Moon paſſes from a ſyzigy 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 ſyzigy, in A, where it is overtaken by the Moon's place. For while M is in the quadrant S B, or A D, the point *h* is in the arch S *m*. But when M is in the quadrant B A or D S, 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 ſouthing. But in the ſecond and fourth quarters, it happens after the Moon's ſouthing.

653. 4. Since the place of high water coincides with the Moon's place both in ſyzigy 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 M C H. This happens when H′ C S is exactly 45°. For then $h'\,d\,S$ is 90°, $m'\,a$ is perpendicular to $a\,S$, and the angle $a\,m'\,d$ is a maximum. Now $a\,m'\,d = m'\,d\,h'$, $= 2\,y'$.

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 ſouthings 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 $12^h\ 25'$, 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 S M is nearly 13°, and S H 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 $24^h\ 35'$, and in the quadratures it is $25^h\ 25'$ 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 *m a* to *m f*. For, take a point *u* very near to *m*. Draw *u a* and *u d*, and draw

draw $d\,i$ parallel to $a\,u$, and with the centre a, and diſtance $a\,u$, deſcribe the arch $u\,v$, which may be conſidered as a ſtraight line perpendicular to $m\,a$. Then $u\,m$ and $i\,h$ are reſpectively equal to the motions of M and H (though they ſubtend twice the angles). The angles $a\,u\,v$, $d\,u\,m$ are equal, being right angles. Therefore $m\,u\,v = a\,u\,d$, $= a\,m\,d$, and the triangles $m\,u\,v$, $d\,m\,f$, are ſimilar, and the angles $u\,a\,m$, $i\,d\,h$ are equal, and therefore

$$u\,v : i\,h = m\,a : h\,d, = m\,a : m\,d$$
$$u\,m : u\,v = \quad m\,d : m\,f$$
$$\text{therefore } u\,m : i\,h = \quad m\,a : m\,f.$$

When m coincides with S, that is, at new or full Moon, $m\,a$ coincides with S a, and $m\,f$ with S d. But when m coincides with C, that is, in the quadratures, $m\,a$ coincides with C a, and $m\,f$ with C d.

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 C a to S a, that is, as M + S to M — S.

When the high water is in the octant, $m\,a$ is perpendicular to S a, 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 $m\,a$.

658.

658. This ſeries of changes is very perceptible in our conſtruction. At new and full Moon, $m\,a$ coincides with S a, and in the quadratures, it coincides with C a. Therefore, the ſpring-tide is to the neap-tide as S a to C a, 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 $a\,f$, 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 M H. It is meaſured by $d\,f$ in our conſtruction. For, let $m\,u$ be a given arch of the Moon's ſynodical motion, ſuch as a degree. Then $m\,v$ is the difference between the tides $m\,a$ and $u\,a$, correſponding to the conſtant arch of the Moon's momentary elongation from the Sun. The ſimilarity of the triangles $m\,u\,v$ and $m\,d\,f$ gives us $m\,u : m\,v = m\,d : d\,f$. Now $m\,u$ and $m\,d$ are conſtant. Therefore $m\,v$ is proportional to $d\,f$, and $m\,d : d\,f =$ rad. : ſin. $d\,m\,f$, $=$ ſin. $m\,d\,h$, $=$ ſin. 2 M C H.

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 qua-

dratures.

dratures. If $m u$ be taken $= \frac{1}{2}$ the Moon's daily elongation from the Sun, which is 6° 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 $m d$ and $d a$ are given, and the contained angle $m d a$, when the proportion of the forces M and S, and the Moon's elongation M C S 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 $= m d + d a : m d - d a$, $=$ tan. $\frac{m a d + a m d}{2}$: tan. $\frac{m a d - a m d}{2}$ = 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, = rad. : ſin. $dm'a$, = 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 S d 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 4½ 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

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,

$$M : S = 35 : 15\tfrac{1}{2}, = 5 : 2\tfrac{1}{5} \text{ nearly.}$$

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

$$M : S = 85 : 34\tfrac{1}{2}, = 5 : 2\tfrac{1}{30} \text{ nearly.}$$

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.

$$35 : 85 = M - S : M + S. \quad \text{Therefore}$$

$$M : S = \frac{85 + 35}{2} : \frac{85 - 35}{2}, = 5 : 2\tfrac{1}{12}.$$

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° 30′. Therefore $\frac{S}{M} = \text{ſin. } 25°$, and $M : S = 5 : 2\frac{1}{5}$ nearly.

Thus it appears that all theſe methods give nearly the

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 $\frac{1}{70}$, 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.

	Sun.	*Moon.*
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 $25^h\ 15'$ in the firſt caſe, and $25^h\ 40'$ 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, y being $9°\ 45'$ and $15°\ 15'$.

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 S M (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 H M. The third column gives the arch S H, 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 *m a*, the ſpace S *a* 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

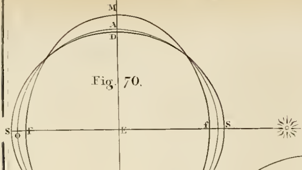


Fig. 70.

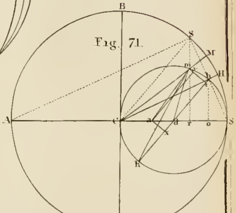


Fig. 71.

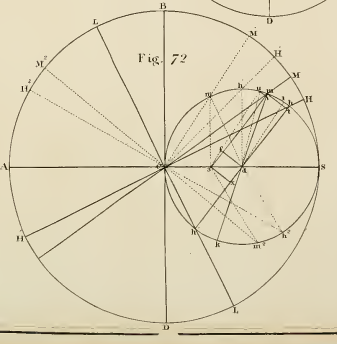


Fig. 72

TABLE I.

SM	HM	Hour.	*ma*
	Minutes.		
	Before the Moon's Southing.		
0	—	–.—	1000
10	11½	–.28½	987
20	22	–.58	949
30	31½	1.28½	887
40	40	2.—	806
50	45	2.35	715
60	46½	3.13½	610
70	40½	3.59½	518
80	25	4.55	453
90	—	6.—	429
	After the Moon's Southing.		
100	25	7. 5	453
110	40½	8. ½	518
120	46½	8.46½	610
130	45	9.25	715
140	40	10.—	806
150	31½	10.31	887
160	22	11.2	949
170	11½	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 ſix

TABLE I.

S M	H M	Hour.	$m\,a$
	Minutes.		
0	—	—.——	1000
	Before the Moon's Southing.		
10	11½	—.28½	987
20	22	—.58	949
30	31½	1.28½	887
40	40	2.——	806
50	45	2.35	715
60	46½	3.13½	610
70	40½	3.59½	518
80	25	4.55	453
90	—	6.—	429
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100	25	7. 5	453
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 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 ſyzigy.

This is owing to our not attending to another circumſtance, namely, that the high water which happens in ſyzigy, 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 ſyzigy or quadrature, as will be more particularly noticed afterwards. At theſe times, the points of high water are 13½ weſt of the ſyzigy, 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° 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 thefe are known, are accurately expreffed by the conftruction. But thefe zenith diftances depend both on the place of the luminaries in the heavens, and on the latitude of the obferver. It is difficult to point out the train of phenomena as they occur in any one place, becaufe the figure affumed by the waters, although its depth be eafily afcertained in any fingle point, and for any one moment, is too complicated to be explained by any general defcription. It is not an oblong elliptical fpheroid, formed by revolution, except in the very moment of new or full Moon. In other relative fituations of the Sun and Moon, the ocean will not have any fection that is circular. Its poles, and the pofition of its equator, are eafily determined. But this equatoreal fection is not a circle, but approaches to an elliptical form, and, in fome cafes, is an exact ellipfe. The longer axis of this oval is in the plane paffing through the Sun and Moon, and its extremities are in the points of low water for this circle, as determined by our conftruction. Its fhorter axis paffes through the centre of the Earth, at right angles to the other, and its extremities are the points of the *loweft low water*. In thefe two points, the depreffion below the natural level of the ocean is always the fame, namely, the fum of the greateft depreffion produced by each luminary. It is fubjected therefore only to the changes arifing from the changes of diftance of the Sun and Moon.

Thus it appears that the furface of the ocean has generally four poles, two of which are prolate or prutube-

 rant,

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

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 N Q S E (fig. 73.) repreſent the terraqueous globe, N S being the axis, E Q the equator, and O the centre. Let the Moon be in the direction O M, having the declination B Q. Let D be any point on the ſurface of the Earth, and C D L its parallel of latitude, and N D S its meridian. Let B′ F *b′ f* be the elliptical ſurface of the ocean, having its poles B′ and *b′* in the line O M. Let *f* O F be its equator.

As the point D is carried along the parallel C D L, 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 C L with the equator *f d* F of the watery ſpheroid. Draw the meridian N *d* G through this interſection, cutting the terreſtrial equator in G. Then the arch Q G, converted into lunar hours, will give the duration of ebb of the ſuperior tide, and G E is the time of the ſubſequent flood of the inferior tide. It is evident that theſe are unequal, and that the whole tide G Q G, conſiſting of a flood-tide G Q and ebb-tide Q G, while the Moon is above the horizon (which we called the *ſuperior tide*), exceeds the duration of the whole *inferior tide* G E G by four times G O (reckoned in lunar hours.)

If the ſpheroid be ſuppoſed to touch the ſphere in in f and F, then $C c'$ is the height of the tide. At L, the height of the tide is $L L'$, and if the concentric circle $L' q$ be defcribed, $C' q$ is the difference between the fuperior and inferior tides.

From this conftruction we learn, in general, that when the Moon has no declination, the duration of the fuperior and inferior tides of one day are equal, over all the Earth.

671. 2. If the Moon has declination, the fuperior tide will be of longer or of fhorter duration than the inferior tide, according as the Moon's declination B Q, and the latitude C Q of the place of obfervation are of the fame or of different denominations.

672. 3. When the Moon's declination is equal to the colatitude of the place of obfervation, or exceeds it, that is, if B Q is equal to N o, or exceeds it, there will be only a fuperior or inferior tide in the courfe of a lunar day. For in this cafe, the parallel of the place of obfervation will pafs through f, or between N and f, as $k \bar{m}$.

673. 4. The fine of the arch G O is = tan. lat. × tan. declin. For rad. : cot. d O G = tan. d G : fin. G O, and fin. G O = tan. d G × cot. d O G. Now d G is the latitude, and d O G is the codecl.

674. The heights of the tides are affected in the fame way by the declination of the Moon, and by the latitude

titude of the place of obfervation. The height of the fuperior tide exceeds that of the inferior, if the Moon's declination is of the fame denomination with the latitude of the place, and *vice versâ*. It often happens that the reverfe of this is uniformly obferved. Thus, at the Nore, in the entry to the river Thames, the inferior tide is greater than the fuperior, when the Moon has north declination, and *vice versâ*. But this happens becaufe the tide at the Nore is only the derivation of the great tide which comes round the north of Scotland, ranges along the eaftern coafts of Britain, and the high water of a fuperior tide arrives at the Nore, while that of an inferior tide is formed at the Orkney iflands, the Moon being under the horizon.

675. The height of the tide in any place, occafioned by the action of a fingle luminary, is as the fquare of the cofine of the zenith or nadir diftance of that luminary. Hence we derive the following conftruction, which will exprefs 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 cafe of fpring or neap-tides.

With a radius C Q (fig. 74.) taken as the meafure of the whole elevation of a lunar tide, defcribe the circle E P Q *p*, to reprefent a terreftrial meridian, where P and *p* are the poles, and E Q the equator. Bifect C P in O, and round O defcribe the circle P B C D. 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 Z C N, cutting the ſmall circle in B, and M C *m* cutting it in A. Draw A I parallel to E Q. Draw the diameter B O D of the inner circle, and draw I K, G H, and A F perpendicular to B D. Laſtly, draw I D, I B, A D, A B, and C I M′, cutting the meridian in M′.

After half a diurnal revolution, the Moon comes into the meridian at M′, and the angle M′ C N is her diſtance from the nadir of the obſerver. The angle I C B is the ſupplement of I C N, and is alſo the ſupplement of I D B, the oppoſite angle of a quadrilateral in a circle. Therefore I D B is equal to the Moon's nadir diſtance. Alſo A D B, being equal to A C B, is equal to the Moon's zenith diſtance. Therefore, accounting D B as the radius of the tables, D F and D K are as the ſquares of the coſines of the Moon's zenith and nadir diſtances; and ſince P C, or D B, was taken as the meaſure of the whole lunar tide, D F will be the elevation of high water at the ſituation Z of the obſerver, when the Moon is above his horizon, and D K 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 D F, 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). D F, then, and D K, are proportional to the heights of the ſuperior and inferior tides of that day. Moreover, as A I is bifected in G, F K is bifected in H, and D H is the arithmetical mean between the heights of the fuperior and inferior tides. Accounting O C as the radius of the tables, A G is the fine of the arch A C, which meafures twice the angle M C Q, the Moon's declination. O G is the cofine of twice the Moon's declination. Alfo the angle B O G is equal to twice the angle B C Q, the latitude of the obferver. Therefore O H = cof. 2 decl. × cof. 2 lat., and D H = D O + O H, = M × $\frac{1 + \text{cof. 2 decl. ☾} \times \text{cof. 2 lat.}}{2}$. This value of the medium tide will be found of continual ufe.

This conftruction gives us very diftinct conceptions of all the modifications of the height of a lunar tide, proceeding from the various declinations of the Moon, and the pofition of the obferver; and the height of the compound tide may be had by repeating the conftruction for the Sun, fubftituting the declination of the Sun for that of the Moon, and S for M in the laft formula. The two elevations being added together, and $\frac{1}{3}\overline{M + S}$ taken from the fum, we have the height required. If it is a fpring-tide that we calculate for, there is fcarcely any occafion for two operations, becaufe the Sun cannot then be more than fix degrees from the Moon, and the pole of the fpheroid will almoft coincide with the Moon's place. We may now draw fome inferences from this reprefentation.

676. 1. The greateft 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 D H 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 D K, D H, and D F are all enlarged.

677. 2. If the Moon be in the equator, the ſuperior and inferior tides are equal, and = M × coſ.² lat. For then A and I coincide with C; and F and K coaleſce in *i*; and D *i* = D B × coſ.² B D C, = D B × coſ.² Z C Q.

678. 3. If the place of obſervation be in the equator, the ſuperior and inferior tides are equal every where, and are = M × coſ.², declin. ☾. For B then coincides with C; the points F and K coincide with G; and P G = P C × coſ.² C P A, = M × coſ.² M C Q.

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 Q Z′ on the other ſide of the equator, equal to Q Z, and

 drawing

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 P Z = M Q, D coincides with I, and K alſo coincides with I. Alſo when P Z is leſs than M Q, D falls below I, and the point Z never paſſes through the equator of the watery ſpheroid. The low water *m m′* (fig. 73.) obſerved in the parallel *k m* is only a lower part of the ſame tide *k k′*, 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; and

and it is to be hoped that this will be done, as in that ſequeſtrated 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, $= M \times \frac{1 + \text{coſ. } 2 \text{ decl. } ☾ \times \text{coſ. } 2 \text{ lat.}}{2}$.

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

B D is perpendicular to P C, 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 *coſin.* 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 B Q diminiſhes, G comes nearer to C, and H ſeparates from O towards B, and D H 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 $M \times \frac{1 + \text{coſ. } 2 \text{ declin. } ☾}{2}$.

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.

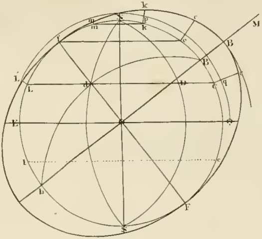
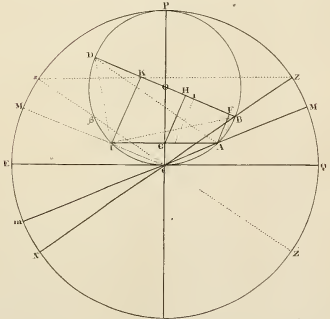


Fig. 74.



lunar tide. The declination of the real fummit of the fpheroid, in this cafe, never differs from the declination of the fummit of the lunar tide more than two degrees, and the correction may be made at any time by a little reflection on the fimultaneous pofition of the Sun. What has been faid is ftrictly applicable to the fpring-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 conftruction. It is exact in fpring-tides and very near the truth in all other cafes. The $\text{fin.}^2\, d\,O$ is $= \frac{S^2 \text{ lat.}}{\text{cof.}^2 \text{ decl. } ☾}$. For $\text{fin.}\, d\,O\,G :$ $\text{fin.}\, d\,G\,O = \text{fin.}\, d\,G : \text{fin.}\, d\,O$.

Such, then, are the more fimple and general confequences of gravitation on the waters of our ocean, on the fuppofition 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 fo covered, and it is clear that there muft be a very great extent of open fea, in order to produce that elevation at the fummit of the fpheroid which correfponds with the accumulating force of the luminaries. A quadrant at leaft of the ellipfe is neceffary for giving the whole tide. With lefs than this, there will not be enough of water to make up the fpheroid. And, to produce the full daily viciffitude of high and low water, this extent of fea muft be in longitude. An equal extent in latitude may produce the greateft elevation; but it will not produce the feries of heights that fhould

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 $= \frac{S^2 \text{ lat.}}{\text{coſ.}^2 \text{ decl. } ☾}$. 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 *g r s*, parallel to the correſponding portion of the elliptical ſurface Q *o* M, 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 G *o*, than on the inſide, as expreſſed by G *g*. 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 springtide).

Thefe obfervations greatly contribute to the explanation of the fingular currents in the Straits of Gibraltar, as they are defcribed by different authors. For although the Mediterranean is not fhut up, and altogether feparated from the Atlantic Ocean at Gibraltar, the communication is extremely fcanty, 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 defcribed. 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 abftraction neceffary for producing the change is but fmall, they are foon accomplifhed. The regifters 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 refults 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.

688.

688. 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.

Suppofe a great navigable river, running nearly in a meridional direction, and falling into the fea in a fouthern coaft. The high water of the ocean reaches the mouth of this river (we may fuppofe) when the Sun and Moon are together in the meridian. It is therefore a fpring-tide high water at the mouth of the river at noon. This checks the ftream at the mouth of the river, and caufes it to deepen. This again checks the current farther up the river, and it deepens there alfo, becaufe there is always the fame quantity of land water pouring into it. The ftream is not perhaps ftopped, but only retarded. But this cannot happen without its growing deeper. This is propagated farther and farther up the ftream, and it is perceived at a great diftance up the river. But this requires a confiderable time. Our knowledge in hydraulics is too imperfect as yet to enable us to fay in what number of hours this fenfible check, indicated by the fmaller velocity, and greater depth, will be propagated to a certain diftance. We may fuppofe it juft 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 fpring-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 juft been a third high water, about fifteen minutes before the Moon came to the meridian, and thirty-five minutes after the Sun has paffed it. There muft 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) unlefs the water be raifed to that level all the way from the mouth of the Thames. In many places that are far from the fea, the ftream, at the moment of high water, is down the river, and fometimes it is confiderable. At Quebec, it runs downward at leaft three miles per hour. Therefore the water is not heaped up to the level; for there is no ftream without a declivity. The harbour at Alloa in the river Forth is dry at low water, and the bottom is about fix feet higher than the higheft water mark on the ftone 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 ftood level from Alloa at the time of high water there.

After confidering a tide in this way, any perfon who has remarked the very ftrange motions of a tide river, in its various bendings and creeks, and the currents that are frequently obferved in a direction oppofite to the general ftream, will no longer expect that the phenomena of the tides will be fuch as immediately refult from the regular operation of the folar and lunar forces.

697. There is yet another caufe of deviation, which is perhaps more diffimilating than any local circumftances, and the operation of which it is very difficult to ftate familiarly, and yet precifely. This is the inertia, as it is called, of the waters. No finite change of place or of velocity can be produced in an inftant by any accelerating force. Time muft elapfe before a ftone can acquire any meafurable velocity by falling.

Suppofe

Suppofe the Earth fluid to the centre, and at reft, without any external difturbing force. The ocean will form a perfect fphere. Let the Moon now act on it. The waters will gradually rife immediately under the Moon and in the oppofite part of the Earth, finking all around the equator of the fpheroid. Each particle proceeds to its ultimate fituation with an accelerated motion, becaufe, till then, the difturbing force exceeds the tendency of the water to fubfide. Therefore, when the form is attained which balances thofe forces, the motion does not ftop, juft as a pendulum does not ftop when it reaches the loweft point of its arch of vibration. Suppofe that the Moon ceafes to act at this inftant. The motion will ftill go on, and the ocean will overpafs the balanced figure, but with a retarded motion, as the pendulum rifes on the other fide of the perpendicular. It will ftop at a certain form, when all the former acceleration is done away by the tendency of the water to fubfide. It now begins to fubfide at the poles of the fpheroid, and to rife at the equator, and after a certain time, it becomes a perfect fphere, that is, the ocean has its natural figure. But it paffes this figure as far on the other fide, and makes a flood where there was formerly an ebb; and it would now ofcillate for ever, alternately fwelling and contracting at the points of fyzigy and quadrature. If the Moon do not ceafe to act, as was juft now fuppofed, there will ftill be ofcillations, but fomewhat different from thofe now mentioned. The middle form, on both fides of which it ofcillates in this cafe, is not the perfect fphere, but the balanced fpheroid.

698.

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 unſuitable 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

on

the globe; but this place of arrival is carried fifteen miles to the eaftward; it is therefore as far from the line O M as before.

This may be illuftrated by a very fimple experiment, where the operation of the acting forces is really very like that of the lunar difturbing force. Suppofe a chain or flexible rope A B C E D F laid over a pulley, and hanging down in a bight, which is a catenarean curve, having the vertical line O D for its axis, and D for its loweft point, which the geometers call its vertex. Let the pulley be turned very flowly round its axis, in the direction A B C. The fide C E will defcend, and F A will be taken up, every link of the chain moving in the curve C E D F A. Every link is in the vertex D in its turn, juft as every portion of the ocean is in the vertex or pole of the fpheroid in its turn. Now let the pulley turn round very brifkly. The chain will be obferved to alter its figure and pofition. O D will no longer be its axis, nor D its vertex. It will now form a curve C *e d f* A, lying to the left hand of C E D F A. O *d* will be its new axis, and *d* will be its vertex. Gravity acts in lines parallel to O D. The motions in the direction C E and F A 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 fo much awry, in the form C *e d f* A, that its tendency by gravity to return to its former pofition is juft equal to the fum of all the mo-

tions

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 M O B is 20 degrees. In other places, this may not be till it is 25°, and in another, 15° may be e-

 nough.

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 ſyzigies 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 fo correfpond with the repetition of all the agitating forces, that the action of to-day may fo confpire with the remaining undulation of yefterday, as to increafe it by its reiterated impulfes, to a degree vaftly greater than its original quantity. By giving gentle impulfes in this way to a pendulum, in the direction of its motion, its vibrations may be increafed to fifty times their firft fize. It is not neceffary, for this effect, that the return of the luminary into the favourable fituation be juft at the interval of the undulation. It will do if it confpire with every fecond or third or fourth undulation; or, in general, if the amount of its confpiring actions exceeds confiderably, and at no great diftance of time, the amount of its oppofing actions. In many cafes this cooperation will produce periods of augmentation and diminution, and many feeming anomalies, which may greatly vary the phenomena.

704. A third obfervation that fhould be made here is, that as the obftructions to the motion of the ocean arifing from the mutual adhefion and action of the filaments are known to be fo very great, we have reafon 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 ofcillations 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 ftill water, we rarely fee a pure undulation repeated fo often. Even in a fyphon of glafs,

where all diffufions of the undulating power is prevented, they are rarely fenfible after the fifth or fixth. A gentle fmooth undulation on the furface of a very fhallow bafon, in the view of agitating the whole depth, will feldom be repeated thrice. This is the form which moft refembles a tide.

705. After this account of the many caufes of deviation from the motions affigned by our theory, many of which are local, and reducible to no rule, it would feem that this theory, which we have taken fo much pains to eftablifh, is of no ufe, except that of giving us a general and moft powerful argument for the univerfal gravitation of matter. But this would be too hafty a conclufion. We fhall find that a judicious confideration of the different claffes of the phenomena of the tides will fuggeft fuch relations among them, that by properly combining them, we fhall not only perceive a very fatisfactory agreement with the theory, but fhall alfo be able to deduce fome 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 fmall, and the time in which it is effected is confiderable, we may look for a confiderable 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 fmall, it is equally reafonable to expect fenfible

ſenſible deviations. If this conſideration be judiciouſly applied, we ſhall find a very ſatisfactory conformity.

707. Of all the modifications of a tide, the greateſt, and the moſt rapidly effected, is the difference between the ſuperior and inferior tides of the ſame day. When the Moon has great declination, the ſuperior tide at Breſt may be three times greater than the ſucceeding or inferior tide. But the fact is, that they differ very little. M. de la Place ſays that they do not differ at all. We cannot find out his authority. Having examined with the moſt ſcrupulous attention more than 200 of the obſervations at Breſt and Rochefort and Port l'Orient, and made the proper allowance for the diſtances of the luminaries, we can ſay with confidence that this general aſſertion of M. de la Place is not founded on the obſervations that have been publiſhed, and it does not agree with what is obſerved in the other ports of Europe. There is always obſerved a difference, agreeing with theory in the proportions, and in the order of their ſucceſſion, although much ſmaller. A very ſlight conſideration will give us the reaſon of the obſerved diſcrepancy. It is not poſſible to make two immediately ſucceeding undulations of inert water remarkably different from each other. The great undulation, in retiring, cauſes the water to heap up to a greater height in the offing; and this, in diffuſing itſelf, muſt make the next undulation greater on the ſhore. That this is the true account of the matter is fully proved by obſerving that when the

theoretic

theoretic difference between thofe two tides is very fmall, it is as diftinctly obferved in the harbours as when it is great. This is clearly feen in the Breft obfervations.

708. The abfolute magnitudes of the tides are greatly modified by local circumftances. In fome harbours there is but a fmall difference between the fpring and neap-tides, and in other harbours it is very great. But, in either cafe, the fmall daily changes are obferved to follow the proportion required by the theory with abundant precifion. Counted half way from the fpring to the neap-tides, the hourly fall of the tide is as the fquare of the time from fpring-tide, except fo far as this may be changed by the pofition of the Moon's perigee. In like manner, the hourly increafe of the tides after neap-tide is obferved to be as the fquares of the time from neap-tide.

709. The priming and lagging of the tides correfponds with the theory with fuch accuracy, that they feem to be calculated from it, independent of obfervation. There is nothing that feems lefs likely to be deranged than this. Tides which differ very little from each other, either as to magnitude or time, fhould be expected to follow one another juft as the forces require. There is indeed a deviation, very general, and eafily accounted for. There is a fmall acceleration of the tides from fpring-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 compofers of tide tables. They give the hour of high water at full and change of the Moon, and this is confidered as fpring-tide. But perhaps there is no part in the world where that is the cafe. It is ufually the third tide after full or change that is the greateft of all, and the third tide after quadrature is, in moft places, the fmalleft tide. Now it is with the greateft tide that our monthly feries commences. Therefore, it is the hour of *this* tide that is to be taken for the hour of the harbour. But, as winds, frefhes, and other caufes, may affect any individual tide, we muft take the medium of many obfervations; and we muft take care that we do not confider as a fpring-tide one which is indeed the greateft, but chances to be enlarged by being a perigean tide.

When thefe precautions are taken, and the tides of one monthly feries marked, by applying the fame correction to the hours in the third column of Bernoulli's table (I.), it will be found to correfpond with obfervation with fufficient accuracy for all purpofes. In making the comparifon, it will be proper to take the medium between the fuperior and inferior tides of each day, both with refpect to time and height, becaufe the difference in thefe refpects between thofe two tides never entirely difappears.

712. The feries of changes which depend on the change of the Moon's declination are of more intricate comparifon, becaufe they are fo 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 Colepreſs ſpeak of it as quite uncertain, and Wallis and Flamſtead 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 ſeſquiplicate 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

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 perigee or apogee 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 firft or the fecond tide after; and that with refpect to the Atlantic Ocean, and all its coafts, it is very precifely the third tide. So that fhould we have high water in any port precifely at noon on the full or change of the Moon, and on the firft day of the month, the greateft tide happens at midnight on the fecond day of the month, or, expreffing it in the common way, it is the tide which happens when the Moon is a day and a half old. The fummit of the fpheroid is therefore 19 or 20 degrees to the eaftward of the Sun and Moon. At this diftance, the tendency of the accumulating forces of the Sun and Moon to complete the fpheroid, and to bring its pole precifely under them, is juft balanced by the tendency of the waters to fubfide. Therefore it is raifed no higher, nor can it come nearer to the Sun and Moon, becaufe then the obliquity of the force is dimifhed, on which the changing power depends. That this is the true caufe, appears from this, that it is, in like manner, on the third tide that all the changes are perceived which correfpond to the declination of the Moon, or her diftance from the Earth. Every thing falls out therefore as if the luminaries were 19 or 20 degrees eaftward of where they are, having the pole of the fpheroid in its theoretical fituation with refpect to this fictitious fituation of the luminaries. But, in fuch a cafe, were the Sun and Moon 20° farther eaftward, they would pafs the meridian 80 minutes, or one hour and 20 minutes later. Therefore $1^h\ 20'$ is added to the hours 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.

☾ α′ ☉	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo	☾ in Perigeo.	☾ in M. Dist.	☾ in Apogeo
0	−.18	−.22	−.27½	After. 18	After. 22	After. 27
10	−.49½	−.51½	−.54	9½	11½	14
20	1.20	1.20	1.20	—	—	—
30	1.50½	1.48½	1.46	9½ Before the Moon's Southing.	11½ Before the Moon's Southing.	14 Before the Moon's Southing.
40	2.22	2.18	2.12½	18	22	27½
50	2.54	2.48½	2.40½	26	31½	39½
60	3.27	3.20	3.10	33	40	50
70	4. 2½	3.55	3.44	37	45	56
80	4.41½	4.33½	4.22	38½	46½	58
90	5.26½	5.19½	5. 9½	33½	40½	50½
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 M's Southing. 21	After the M's Southing. 25	After the M's Southing. 31
130	9.13½	9.20½	9.30½	33	40	50
140	9.58½	10. 6½	10.18	38	46	58
150	10.37½	10.45	10.56	37	45	56
160	11.13	11.20	11.30	33	40	50
170	11.46	11.51½	11.59½	26	31	39
180	—.18	—.22	—.27½	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 fmall (never 12°) the error will be infenfible. For this reafon, the three other columns are added, expreffing 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 reft. We fhall come near enough to the truth, if we leffen the minutes in the three hour-columns $\frac{1}{10}$ when the Moon is in the equator, and increafe them as much when fhe is in the tropic, and if we ufe them as they ftand when fhe is in a middle fituation.

716. All that remains now, is to adjuft this general table to the peculiar fituation of the port. Therefore, collect a great number of obfervations of the hour of high water at full or change of the Moon. In making this collection, note particularly the hour on thofe days where the Moon is new or full precifely at noon; for this is the circumftance neceffary for the truth of the elongations in the firft column of the table. A fmall equation is neceffary for correcting the obferved hour of high water, when the fyzigy is not at noon, becaufe in this fituation of the luminaries, the tide lags 35′ behind the Sun in a day, as has been already fhewn. Suppofe the lagging to be 36′, this will make the equation 1½ 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 syzigy 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 syzigy is before noon, and subtract it if the syzigy 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

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 $S = \frac{A - B}{2}$.

Although

Although this is undoubtedly an approximation, and perhaps all the accuracy that is attainable, it is not founded on exact phyfical principles. The local proportion of A to B depends on circumftances peculiar to the place; and we have no affurance that the changes of the lunar force will operate in the fame manner and proportion on thefe two quantities, however different. We are certain that it will not; otherwife the proportion of fpring and neap-tides would be the fame in all harbours, however much the fprings may differ in different harbours. I compared Bernoulli's apogean and perigean tides, in about twenty inftances, felected from the obfervations at Breft and St Malo, where the abfolute quantities differ very widely. I was furprifed, but not convinced, by the agreement. I am however perfuaded that the table is of great ufe, and have therefore inferted it, as a model by which a table may eafily be computed for any harbour, employing the fpring-tide and neap-tide heights obferved in that harbour as the A and B for that place. The table is, like the laft, accommodated to the eafterly deviation of the pole of the fpheroid from its theoretical place.

It appears from this table, and alfo from the laft, that the neap-tides are much more affected by the inequalities of the forces than the fpring-tides are. The neap-tides vary from 70 to 128, and the fprings from 90 to 114. The firft is almoft doubled, the laft 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° 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, becaufe by this means the connexion with the operation of univerfal gravitation is much better kept in fight. At the fame time La Place's method allows us, in fome cafes, to ftate the individual fact more nearly as it occurs, without confidering 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 furnifh the arbitrary quantities in his equations. Thefe being wholly taken from the obfervations in the ports of France and Spain, it may be queftioned whether the famenefs, arifing from the latitude being fo near 45°, may not have made the ingenious author fimplify too much his theory. He confiders every clafs of phenomena as operations completely accomplifhed, and the ocean at the end of the action of any one of the forces as in a ftate of indifference, ready for the free operation of the next. For example, the equality of the fuperior and inferior tides of one day is deduced by La Place immediately from the circumftance of the ocean being of nearly an uniform depth, faying that the fmall inferior tide is not affected by the greatnefs of the preceding fuperior tide, becaufe the obftructions are fuch that all motions ceafe very foon, almoft immediately after the force has ceafed to act. We doubt the truth of the near uniformity of the fea's depth. The unequal tides are confeffedly moft remarkable on the coafts, where the depth is the moft 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.

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 baſon, which communicated with a long and narrow horizontal channel, ſhut at the far end, the baſon 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 comparifon of the fhip's reckoning with lunar obfervations and the arrivals on known coafts, from which we cannot help inferring the fame general current in the Indian and Ethiopic feas. It feems therefore to obtain over the whole. The part of this current which diffufes itfelf into the Atlantic is but fmall, it having a freer paffage ftraight forward. But the part thus diffufed produces the gulf ftream, in its way along the American coafts, and efcapes round the north capes of Europe and America. In all probability, a foutherly current may be obferved in the ftraits which feparate America from the Afiatic continent. The whole amount of this motion cannot be confiderable, but there muft be fome, if there be two circumpolar communications between the great eaftern and weftern divifions of the ocean. Without this, it muft be reduced to a reciprocating motion too intricate for inveftigation.

723. There is another circumftance which feems to ftrengthen our confidence in the reality of this wefterly current of the ocean. The gravity of the waters being more diminifhed in conjunction and oppofition than it is augmented in quadrature with the acting luminary, each particle tends to recede from the centre, and to defcribe a larger circle, employing a longer time. Here is a tendency or *nifus* to a relative motion wefterly. Water, being almoft perfectly fluid, will obey this tendency, and in time acquire fuch a motion, were it not obftructed by

folid obftacles. But fome effect muft remain, too intricate to admit any calculation, and perhaps not ultimately fenfible.

724. If the height of the atmofphere be equal to the radius of the Earth, we fhall have a tide in the air double of that in the ocean. When all the affecting circumftances are confidered, it appears that an ebb and flood of the atmofphere may differ in elevation about 120 feet. This might be fenfible by affecting the barometer. True, the gravity of the mercury is alfo diminifhed, but not fo much as that of the more diftant air. But the height of the atmofphere is too fmall to give rife to any fuch tides. They cannot fenfibly exceed thofe of the ocean, and this cannot change the height of the mercury in the barometer $\frac{1}{100}$ of an inch. Profeffor Toaldo at Padua kept a regifter of the barometer for more than thirty years. He has added into one fum all the mercurial heights obferved at new Moon. Another fum was made of all the heights obferved in the quadratures; another of the perigean; and another of the apogean heights, &c. &c. He thinks that differences were obferved in thofe fums fufficient for proving the accumulation and compreffion of the air by its unequal gravitation to the Moon. Thus, the apogean heights exceeded the perigean by 14 inches. The heights in fyzigy exceeded thofe in quadrature by 11 inches. (See Mem. Berlin 1777, and a book exprefsly on the fubject).

But there is another effect of this difturbing force which

which may be much more fenfible, namely, the general wefterly current of the air. M. D'Alembert has inveftigated this with great care, and fingular addrefs, and has proved that there muft be a wefterly current in the tropical regions, at the rate of eight feet nearly in a fecond. This is a very adequate caufe of the trade winds which are obferved between the tropics. It is indeed increafed by the rarefaction of the air occafioned by the heat of the Sun, which expands the air heated by the ground, and it is both raifed and diffufed laterally. When the Sun has paffed the meridian a proper number of degrees, the air muft now cool, and in cooling contract behind the Sun. Air from the eaft comes in greater abundance than from any other quarter to fupply the vacancy.

725. The difk of Jupiter, when viewed through a good telefcope, is diftinguifhable into zones, like a bit of ftriped fatin. Thefe zones, or belts, are of changeable breadth and pofition, but all parallel to his equator. Therefore they are not attached to his furface, but float on it, as clouds float in our atmofphere. This Earth will have fomewhat of this appearance, if viewed from the Moon. For each climate has a ftate of the fky peculiar in fome degree to itfelf in this refpect, and there muft be a fort of famenefs in one climate all round the globe. A feries of obfervations on a particular fpot of Jupiter's furface demonftrate his rotation in $9^h\ 56'$. Spots have been obferved in the belts, which have lafted fo long as to make feveral revolutions before they were effaced. They

They appear to require a minute or two more for their rotation, and therefore have a wefterly motion relative to the firm furface of the planet. This however cannot be depended on from the time of their rotation. But a few obfervations have been had of fpots in the vicinity of the fixed fpot of his furface, and here the relative motion weftward was diftinctly obferved. M. Schroeter at Manheim has obferved the atmofphere of Jupiter with great care, and finds it exceedingly variable; and fpots are obferved to change their fituations with amazing rapidity, with great irregularity, but moft commonly eaftward. The motions and changes are fo rapid, and fo extenfive, that we can fcarcely confider them as the transference of matter from one place to another. They more refemble the changes which happen in our atmofphere, which are fometimes progreffive, over a great tract of the country. The ftorm in 1772 was felt from Siberia to America in fucceffion. The gale blew from the weft, but the chemical operation which produced it was in the oppofite direction, being firft obferved in Siberia; three days afterward, it was felt at St Peterfburg; two days after this, at Berlin; two days more, it was in Britain; and feven days after, it was felt in North America. Here then, while a fpectator on the Earth faw the clouds moving to the eaftward, a fpectator in the Moon would fee the change of appearance proceed from eaft to weft. The motions in the atmofphere of Jupiter muft be very complicated, becaufe they are the joint operation of four fatellites. The inequality of gravitation to the firft firſt ſatellite muſt be very great. And as each ſatellite 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 effectally 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.

It

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 diſſimilar. 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 circúmſtances 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

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*, Παντοκρατωρ, *dici ſolet. Deitas eſt dominatio Dei,* ' *non in corpus proprium, uti ſentiunt quibus Deus eſt ani-* ' *ma 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,

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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 anato-

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mift never imagines that what he has difcovered is of no ufe. *

732. So far therefore from banifhing the confideration of final caufes from our difcuffions, it would look more like philofophy, more like the love of true wifdom, and it would tafte lefs of an idle curiofity, were we to multiply our refearches in thofe departments of nature where final caufes are the chief objects of our attention—the ftructure and œconomy of organifed bodies in the animal and vegetable kingdoms. I cannot help remarking, with regret, that of late years, the tafte of naturalifts has greatly changed, and, in my humble opinion, for the worfe. The ftudy of inert matter has fupplanted that of animal life. Chemiftry and mineralogy are almoft the fole objects of attention. Nay, the *ruins* of nature, the fhattered relicks of a former world, feems a more engaging object than the numberlefs beauties that now adorn the prefent furface of our globe. I acknowledge that, even in thofe inanimate works, God has not left himfelf without a witnefs. Yet furely

* I would earneftly recommend to my young readers fome excellent remarks on the argument of final caufes (without which Cicero thought that there is no philofophy) in the preface by the editor of Derham's Phyfico-Theology, publifhed at London in 1798. He there confiders the proper province of this argument, its ufe, and incautious abufe, with the greateft perfpicuity 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 Haüy, ' 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. Haüy 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 *Syſtême du Monde.* In the whole of this work, the author miſſes no opportunity of leſſening the impreſſion that might be made by the peculiar ſuitableneſs 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 pow-'ers 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 accommodation much has been done, as if we were objects of peculiar care. But we have been punifhed, fays he, for thefe miftaken notions of felf-importance, by the foolifh anxieties to which they have given rife, and by the fubjugation to which we have fubmitted, while under the influence of thefe fuperftitious terrors. Miftaking our relations to the reft of the univerfe, focial order has been fuppofed to have other foundations than juftice and truth, and an abominable maxim has been admitted, that it was fometimes ufeful to deceive and to fubdue mankind, in order to fecure the happinefs of fociety. But nature refumes her rights, and cruel experience has fhewn that fhe will not allow thofe facred laws to be broken with impunity.

734. I think it will require fome inveftigation before we can find out what connexion there is between the difcoveries of Sir Ifaac Newton and this myfterious detection that M. de la Place has at laft deduced from the furvey. It is communicated in the dark words of an oracle, and we are left to interpret for ourfelves. I can affix no meaning but this, that ignorance and felf-conceit have made us imagine that this Earth is the centre, and the principal object of the univerfe, and that all that we fee derives its value from its fubferviency 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 fpectacle is difplayed,—and that our fortunes are to be read in the ftarry heavens. But it

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is now demonftrated that this Earth, when compared, even with fome fingle objects of our fyftem, is but like a peppercorn. The whole fyftem is but as a point in the univerfe. How infignificant then are we! But we have been juftly punifhed for our felf-conceit, by imagining that the ftars influence our fortunes, and have made ourfelves the willing dupes of aftrologers and soothfayers.

Thus far I think that M. de la Place's words have fome meaning, but, furely, very little importance; nor did it call for any congratulatory addrefs to his contemporaries on their emancipation from fuch fears. It is more than a century fince all thoughts of the central fituation and great bulk of the Earth, and of the influence of the ftars on human affairs, have been exploded and forgotten.

But the remaining part of the remarks, about focial order, and truth, and juftice, and about deceiving and enflaving mankind, in order to fecure their happinefs, is more myfterious. 'More is meant than meets the ear.' M. de la Place carefully abftains, through the whole of this performance, from all reference to a Contriver, Creator, or Governor of the univerfe, particularly in the prefent reflections, *which are fo pointedly contrafted* with the concluding reflections of the great Newton. The oppofition is fo remarkable, that it ftartles every reader who has perufed the Principia. I cannot but fufpect that M. de la Place would here infinuate that the doctrine of a Deity, the Maker and Governor of this World, and of

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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 B C D E, 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.

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737. But there is not the leaſt ſhadow of proof here, nor any ſimilarity, 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,

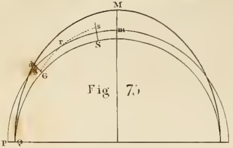

Fig. 75

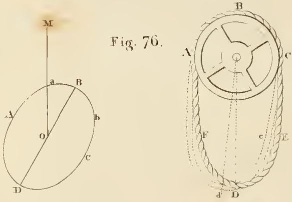

Fig. 76.

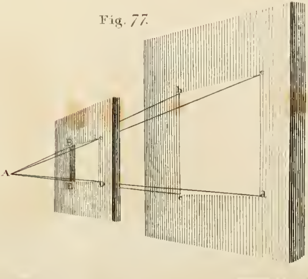

Fig. 77.

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-

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.

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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 obferve certain relations, which have hitherto been conftant, and I am led by the conftitution of my mind to expect the continuation of thefe relations. My collection of fuch obfervations is my knowledge of its nature. This gravitation is one of them, and this is all that I know about it.

740. The obferved relations may be fuch that they involve certain confequences. This, in particular, has confequences that cannot be difputed. If gravitation in the ratio of $\frac{1}{x^2}$ be the primordial relation of all matter, and the fource of all others (which is a part of La Place's fyftem), it is impoffible that a particle compofed of fuch atoms can act with a force which decreafes more rapidly by an increafe of diftance. But there are many phenomena which indicate a much more rapid decreafe of force. Simple cohefion of folid bodies is one of thefe. The expanfion of fome exploding compofitions fhew the fame thing. We may add, that no compofition of fuch atoms can form repelling particles, nor give rife to many expanfive fluids, or indeed to any of the ordinary phenomena of elaftic bodies. But thefe things are not immediately before us, and we fhall have another and a better opportunity of confidering many things connected with this great queftion.

741. De la Place is not the firft perfon who has attempted a demonftration of this propofition. 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 fee natural philofophy calumniated, and accufed of lending her fupport to doctrines which are the abhorrence of all the wife and good. I cannot better difcharge this duty than by wiping off this ftain, with which carelefs ignorance, or atheiftical perverfion, has disfigured the fair features of philofophy. I was grieved when I firft faw M. de la Place, after having fo beautifully epitomifed the philofophy of Sir Ifaac Newton, conclude his performance with fuch a marked and ungraceful parody on the clofing reflections of our illuftrious mafter; and, as I warmly recommend this epitome to my pupils, it became the more neceffary to take notice of the reprehenfible peculiarities which occur in different parts of the work; and particularly of this propofition, from which the materialifts feem to entertain fuch hopes. Nor am I yet done with it. A demonftration has been recently offered, in a work which profeffes to explain *the intimate conftitution of matter*, and to account for *all* the phenomena of the univerfe. This will come in my way when we fhall be employed in confidering the force of cohefion. Till then, *requiefcat in pace*.

It is fomewhat amufing to remark how the authority of Sir Ifaac Newton has been eagerly catched at by the atheiftical fophifts to fupport their abject doctrines. While fome hankering remained in France for the Atomiftic philofophy, and there was any chance of bewildering the imaginations, and mifleading the underftandings, of fuch as wifhed 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 *Syſtéme 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 altæ,*
'*Finxit in effigiem moderantûm cuncta Deorum,*
'*Pronaque cum ſpectent animalia cætera terram,*
'*Os homini ſublime dedit, cœlumque tueri*
'*Juſſit, et erectos ad ſidera tollere vultus.*'

OVID.

Allow

Allow me to conclude in the words of Dr Halley.

‘ *Talia monſtrantem mecum celebrate Camœnis,*
‘ *Vos, ô cœlicolûm gaudentes nectare veſci,*
‘ NEWTONUM, *clauſi reſerantem 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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