G.W.L. A SHORT DEMONSTRATION OF A NOTEWORTHY ERROR of the Cartesians and Others about a Law of Nature, according to which the same Quantity of Motion is to be conserved by God always ; and where such Quantities are used up in Mechanical Devices.

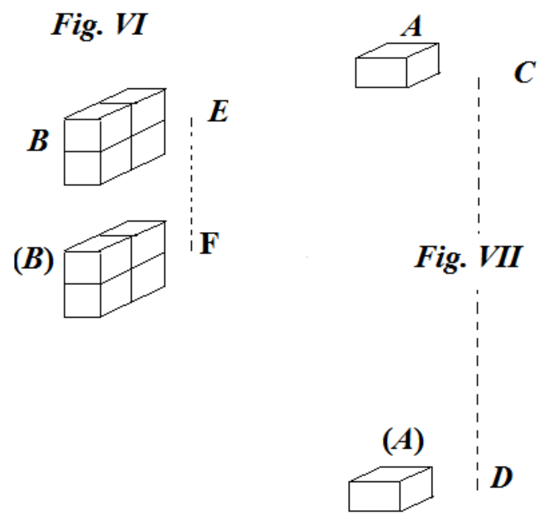
Communicated by letters given on the 6th Jan. 1686.

Many mathematicians, when they consider the speeds and the weights to balance each other in the five common [simple] machines, generally estimate the motive force from the quantity of motion, or from the product from multiplying [the weight of each body] by its speed. Or as may be said more geometrically, the forces of two fast moving bodies (of the same kind) in motion [in a machine such as a wedge, wheel and axle, lever, etc.], and interacting equally by their weights and motions, are said to be in the ratio composed of the bodies or the masses [at this time synonymous with weight], and of those velocities which they have. And thus when an agreement of the ratio shall be reached, the sum of all the same potentials of the motions in nature shall be conserved : and not to be diminished, because we may consider no force to be lost by one body, which shall not be transferred to the other body [i.e. no friction is involved] ; nor to be increased, because thus nowhere will there be a machine, where a mechanical perpetual motion shall be successful, and hence neither indeed can the whole world extend its [motive] force without a new external impulse. So that, according to Descartes [the world] had been made thence, who considered the motive force for all the matter in the world, and had announced that by God the equivalent quantity of motion in the whole world would be conserved.

[Thus verbally, Leibniz informs us that for a simple machine, as viewed by Descartes and his followers, the ratio Load to Effort is equal to the ratio Velocity of Effort to Velocity of Load, in which circumstances the corresponding weights or masses have been moved around by the machine, with no other effect and in the absence of frictional forces. At the time there was no conceptual difference between mass and weight, and thus Descartes could discuss for a given body, the motive force as being the weight times the speed, or the bulk or mass times the speed, as any simple ratio can be written equivalently in terms of weights or masses. This principle of conservation of 'motive force' in modern terms, also applies to the case of an isolated system of particles interacting amongst themselves where the total linear momentum of all the particles is conserved, and may be put equal to zero, if the centre of mass is taken at rest. In this case, Descartes' motive force corresponds to linear momentum and his principle to the conservation of linear momentum. The point made by Leibniz is simply that not all experimental devices involving physical interactions of moving masses operate according to this principle, involving steady motions, in which case another, or at least an extended principle must be adopted, which included accelerated motions.]

Truly I myself, so that I may show how great [a change in the motive force] may be present between these two [bodies], *supposition* 1*, in the first place* a body falling from a certain height to acquire a certain [motive] force as far as it will rise again, if its direction thus were considered [to be reversed], nor might it be impeded by something external :

for example, a soon returns to precisely the height from which it was sent, unless the resistance of the air and other similar small impediments may absorb some of its strength, from which indeed we ourselves can extract a certain concept. Likewise *supposition* 2, *in the second place*, just as much strength is required to raise body A of one pound all the way to a height CD of four ells, as is required to raise body B of four pounds, as far as to a height EF of one ell. All this is conceded equally by the Cartesians and by the Philosophers and Mathematicians of our time. Hence it follows body A falling from a height CD, to have acquired exactly as much strength, as the body B falling from the height EF. For body (A) after falling from C arrives at D, there it has the strength to be rising as far as to C, by *suppos.*1, that is the 'force' being required to raise the body of one pound (clearly a property of the body) to a height of four ells. And similarly the body (B) after falling from E arrives at F, there it has the strength to be rising as far as to E, by *suppos.*1, that is the 'force' of raising the body of four pounds (clearly a property of the body) to a height of one ell. Therefore by *suppose.* 2 the 'force' of the body (A) present at D, and the 'force' of the body (B) present at F, are equal.



And now we may see the quantity of motion shall be the same in both places. Truly there a hope besides may be found to discriminate the most. Because I show thus : it is been demonstrated by Galileo, the speed to be acquired by falling CD, to be double the speed by falling EF. Therefore we may multiply body A which is as 1, by its speed which is as 2, or the quantity of motion produced will be as 2, again we may multiply body B which is as 4. Therefore the quantity of motion of body (A) which is present at F, and yet at D, it is half of the quantity of motion of the body (B) which is present at F, and yet a little before the 'forces' in both places have been found to be equal. And thus there is a great distinction between the moving force and the quantity of motion, thus so that the one cannot be judged by the other, which we have undertaken to show. From these it is apparent, how the force shall be required to be judged from the size of the effect, which it is able to produce, for example from the height to which that heavy body of a given size and kind is able to be raised, truly not from the speed which can be impressed on the body. Indeed there is need not for twice the force but for a greater force required to be given to double the speed of the same body. Truly nobody wonders about simple machines, with the lever, wheel and axle, pulleys, wedge, screws and with the like to be in equilibrium, with the size of one body to be compensated by the speed of the other, which arises in the arrangement of the machine ; or when the magnitudes (for the same kind of bodies) are inversely as the speeds ; or when in some other way the same quantity of motion may appear. For there it comes about also that the same quantity is going to be effected on both sides, or the heights of the descent or ascent shall be the same; in whatever side of the motion you may wish to happen. And thus it happens there by accident, so that the size of the motive force can be judged. Truly other cases may be given, such as this one presented above, where they do not agree.

Since nothing other shall be simpler than from our proof, it is a wonder the idea did not come to mind for either Descartes or the Cartesians, the most learned men. But indeed he acted with too much confidence in his ability to change his point of view to these foreign matters. For Descartes, more by a common fault of great men, later became a little overconfident. Moreover a good many of the Cartesians I fear gradually began to resemble the Peripatetics [*i.e.* followers of Aristotle] whom they derided; that is they were not going about the business of reasoning correctly about the nature of things, but rather adopting the attitude of those accustomed to consult the books of the master.

Therefore it is required to be said that forces are in a composite ratio of the bodies (of the same specific weight or solidity) and of the speed producing heights, from which evidently by slipping such speeds might be able to be acquired ; or more generally (because sometimes at this point no speed has been produced) of the heights about to be produced : truly not generally of the speeds themselves, in whatever manner that first kind may appear plausible, and several shall be seen; from which many errors have arisen, by those who have written on the mathematics of mechanics. *The Reverend Fathers Honore Fabri and C. des Chales,* and likewise *G.A.Borelli,* and other men, who have been caught unawares while in other respects being outstanding in these studies. And hence I have thought : why not go and do this, because the Huygens rule about the centre of oscillation of the pendulum, which certainly is most true, has been called into doubt by some learned men.

No. XVII. (Dutens Book III)

A SHORT REMARK BY THE Abbé Catelan,
where the paralogism contained in the preceding objection of *M. G. W. Leibniz* is shown.

Extract from the Nouvelles de la Republique des Lettres, September 1686.

Mr. *Leibniz* is surprised that his proof, which he believes to be the simplest in the world, cannot be presented according to the spirit of *Descartes*, or of the Cartesians. But it would be even more surprising if a Philosopher & a Geometer of such depth had given such a thought in an oversight, and so many clever people rush off there with him. Only the learned can judge whether it is he or Mr. *Leibniz*, who has become overconfident in his thinking, the usual fault of great men. Mr. *Leibniz* cites a concern which is about the truth a good soul, but a little too counter-productive, when he states that he is afraid the disciples of Mr. *Descartes* mimic the Peripatetics they mock.

Let us look at this considerable error, by which he claims to destroy Descartes reasoning about motive forces.

He says :

1. Mr. *Descartes* assures that God maintains the same quantity of motion in the Universe;
2. The same Philosopher accounts the equal things to be the motive force, and the quantity of motion ;

3. Many Mathematicians generally estimate the moving force by the amount of movement, or the product of the multiplication of the body by its velocity. Now he claims that these things are in disagreement with each other, thus the moving force, and the amount of movement differ greatly, and that this rule of *Descartes* is wrong: *the same amount of motion is always conserved in nature*.

For from the last part of that consequence, it is for the Cartesian readers to consider how it can be linked up with his premises. For the first, he proves it thus.

"According to the *Descartes* & the other Mathematicians there is no less force required to raise a body of one pound to a height of 4 yards than to raise a body of 4 pounds to a height of one yard : from which it follows that the simple fall from a height of four acquires exactly the same force as the four falling the simple height; because the one and the other would acquire such force that the external obstacles being removed it would be able to climb back up to where it has fallen from. Moreover *Galileo* had shown that the body acquires a speed falling from the height of 4 yards double the rate it acquires by falling from the height of a yard. Thus multiplying the body of one pound by its speed, that is to say, 1 by 2 product, or the quantity of motion will be like 2, and multiplying the body of 4 pounds per speed, that is, the product 4 by 1, wherein the amount of movement will be as 4, so one of these quantities is half of the other, though previously the forces having been found to be equal, I say that *Descartes* cannot distinguish the quantities of motion. Etc."

I wonder that Mr. *Leibniz* had not seen the paralogism of this evidence, because where is a man a little skilled in mechanics who does not understand the principle of the Cartesian concerning the 5 simple machines, regarding the isochronous powers, or motions expressed in equal times, when comparing two weights together? Because it is shown in the Elements 2. that two moving masses unequal in volume such as 1 and 4 but equal in quantity of movement as 4, have speeds proportional in the inverse ratio of their masses, as 4 to 1 & therefore they always travel distances proportional to these speeds in same time. Besides that *Galileo* shows that the distances described by the falling bodies are in the same ratio between them as the squares of the time. Thus in the example of Mr. *Leibniz* the body of one pound would rise up to 4 yards in a time as 2, and the body of 4 pounds would rise to the height of one yard in a time as 1. Since these the times are unequal, it is not strange that in this case there are unequal amounts of the movement, though they had been found in a drop equal amounts in an equality from time rendered all the difference to that case. Let us suppose now that these two bodies move only at the same times, that is to say, they are suspended from the same balance & at distances reciprocal to their size, we find the equal and opposite amounts of their motions or strengths of their weight, or the forces of their weights, to be either as their masses multiplied by their distances, or by their speeds in the same manner. The matter turns out differently when the times are unequal. Hence it appears, that neither *Descartes* nor any other is wrong here, and I doubt any of these learned men, who have recently challenged the rule of Mr. *Huygens* concerning the center of oscillation, will change their opinion s because of this objection of Mr. *Leibniz*.

Concerning the Isochronous Curve, along which a Weight may fall without an Acceleration downwards, and the Dispute with the Abbé Catelan.

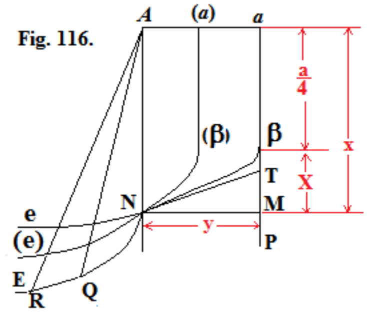
Acta Erud. April, 1689.

Since a demonstration was published by me in the March 1686 edition of the *Acta* against the *Cartesians,* where the true evaluation of the measure of force was examined, [The Latin words *vis* and *potentia* are synonyms with military or political strength, force or power; here and elsewhere these words are used to express vaguely *force, work, energy,* and *power* in the modern sense, as these concepts had not yet been fully established.] and where it was shown [when unbalanced forces act on a body] the quantity of motion was not to be conserved, but rather by the force providing a difference in the quantity of motion : a certain learned man in France, the Abbé Catelan, replied on behalf of the Cartesians, but, as it was apparent later, he had not understood the strength of my argument well enough. Indeed it is to be believed, he was attacking me about a certain other principle due to me which he had encountered, which is to be found in the *Nouvelles de la Republique des lettres* for the month of June 1687 p. 579, and he himself denied not knowing about that contradiction, that I myself had found in his response from p. 579 onwards : moreover at no time did I have reason to doubt these things, such as I had mentioned in *Nouvelles de la Republique des letters* for September 1687. Likewise so that he could elude my objection, he himself had conjectured at different times other matters along the way, as in that way, that to me appear clearly to be of no consequence. For with the same height maintained, the same strength is acquired or impeded by some weight allowed in a given time, which may be increased or diminished according to the inclination of the descent. In that circumstance, so that it may become more evident, the time and thus the distinction between isochronous and an-isochronous strengths makes no difference to the matter, and so that from our dispute some increased understanding might be grasped, and such a *problem*, as it might not seem to be inelegant, I had proposed in the said *Nouvelles* of September 1687 while writing out a response [in this on-going dispute]:
" To find the isochronous line, along which a weight may fall uniformly, or in equal times to approach to the horizontal in equal amounts, and thus without an acceleration, and always to be carried downwards with an equal velocity."
But the Abbé Catelan said nothing in return, either because he did not wish to become involved with the problem, or he himself was satisfied to judge that at last he had understood my thinking. But the most celebrated of men *Christian Huygens* had judged the problem worthy of a place in his own work [*Horologium Oscillatorium*, III, Prop. 9], and whose solution in harmony with mine appeared in the *Nouvelles* for October 1687, but with the demonstration and exposition suppressed and without an explanation how to choose between the different lines of the same kind, as he says : For besides this line BC, there will be an infinitude of lines of the same kind, which are easy to find. Therefore I wanted to supply this [explanation] here, to come about more quickly, as perhaps I might have expected something here from the industry of the Abbé.

Problem. To find the plane curve, along which a weight may fall without a [vertical] acceleration.

Solution. The curve (fig.116) shall be some quadrato-cubic [*i.e.* semi-cubic] parabola βNe put in place thus (evidently where the volume under the square of the base NM and with the parameter aP , is equal to a cube of the height βM), [*i.e.* the equation of the curve is hence $y^2 aP = X^3$] so that from the vertex β [*i.e.* the original origin] the tangent βM shall be perpendicular to the horizontal, at some point N of which curve, there a weight may be placed to be falling down further with the aforesaid speed, that it must acquire by falling from the horizontal Aa, the elevation of which $a\beta$ above the vertex β shall be $\frac{4}{9}$ th of the parameter of the curve, [*i.e.* $\frac{a}{4} = \frac{4}{9}\text{aP}$ or $\text{aP} = \frac{9}{16}a$, for some constant *a*. In this case, also, $\sqrt{a} = \frac{4}{3}\sqrt{\text{aP}} \propto$ velocity on falling freely from rest the distance $a\beta$] then likewise the weight will fall further uniformly along the curve Ne, however far it may continue, as it may be desired.

Fig. 116.



[The curve thus becomes conveniently $9ay^2 = 16X^3$, and taking *y* as the abscissa :

$3\sqrt{a}\,y = \pm 4X^{3/2}$; also, $\frac{\text{TM}}{\text{MN}} = \frac{dX}{dy} = \frac{dX/dt}{dy/dt} = \frac{v_X}{v_y} = \frac{3\sqrt{\text{a}}}{6\sqrt{\text{X}}} = \frac{\sqrt{\text{a}}}{2\sqrt{\text{X}}} = \frac{\sqrt{\frac{\text{a}}{4}}}{\sqrt{\text{X}}} = \frac{\sqrt{\text{a}\beta}}{\sqrt{\beta\text{M}}}$, in general;

from which we can conclude the vertical velocity v_X is constant during the descent; however, Leibniz adopts a non-differential approach.]

Demonstration. The right line NT is a tangent to the curve βNe at N and shall meet the line βM itself at T. Certainly (from the above noted property of the tangents of this curve) TM will be to NM in the square root ratio aβ to βM. Therefore TM will be to TN, in the square root ratio aβ to aβ+βM , or to aM.

[For $\text{TN}^2 = \text{TM}^2 + \text{MN}^2$ and $\frac{\text{TM}^2}{\text{MN}^2} = \frac{\text{a}\beta}{\beta\text{M}}$, from which

$\text{TN}^2 = \text{TM}^2 + \text{MN}^2 = \text{TM}^2\left(1 + \frac{\beta\text{M}}{\text{a}\beta}\right) \therefore \frac{\text{TM}^2}{\text{TN}^2} = \frac{\text{a}\beta}{\text{a}\beta+\beta\text{M}} = \frac{\text{a}\beta}{\text{aM}}$, and $\frac{\text{TM}}{\text{TN}} = \frac{\sqrt{\text{a}\beta}}{\sqrt{\text{aM}}}$]

Now the ratio TM to TN is likewise the ratio, of the velocity of [vertical] descend which the weight has along the curve at the position N (or again from the above horizontal line as it approaches the curve), to the velocity by which the same weight arrives at N again, not along the curve but by falling freely, if that could happen (so that it agrees with the nature of the inclined motion).

[*i.e.* $\frac{v_X}{v} = \frac{\text{TM}}{\text{TN}} = \frac{\sqrt{\text{a}\beta}}{\sqrt{\text{a}\beta+\beta\text{M}}} = \frac{\sqrt{\text{a}\beta}}{\sqrt{\text{aM}}}$].

But this free velocity again is to a certain constant [velocity] in the square root ratio aM to aβ ; for the free fall velocities (as agreed from the motion of the weight) at the height

aM are in the square root ratio (from which they are sought by falling): therefore the velocity of the descent along the curve Ne, which the weight has put in place at any point N of the curve, is to the constant velocity by the composite square root ratio $\mathrm{a\beta}$ to aM and aM to $\mathrm{a\beta}$, which is the ratio of equality.
[For if v_1 is the velocity acquired after falling through $\mathrm{a\beta}$, while v is the velocity after falling freely aM, following Galileo we have independently $\frac{v}{v_1} = \frac{\sqrt{\mathrm{aM}}}{\sqrt{\mathrm{a\beta}}}$. Consequently from these two ratios we have $\frac{v_X}{v} \times \frac{v}{v_1} = \frac{\sqrt{\mathrm{aM}}}{\sqrt{\mathrm{a\beta}}} \times \frac{\sqrt{\mathrm{a\beta}}}{\sqrt{\mathrm{aM}}} = 1$; hence $v_X = v_1$. Physically, this means of course that the extra velocity is all acquired by the horizontal component as the mass slides down the curve, while the vertical component maintains its initial value.]

Therefore according to this, the velocity of descent along the curve is constant, or the same everywhere on the curve Ne. Q.P.E.

Conclusions: 1) A weight having acquired a certain speed in falling from some height Aa, can keep on falling along an infinite isochronous curve from the same point N, either of the same kind, or only with a different size of parameter, so that Ne, N(e), NE, are all semi-cubic parabolas, and thus similar between themselves. Indeed any of these parabolas may be inserted here, but they must be arranged thus, so that $\mathrm{a\beta}$ or $(\mathrm{a})(\beta)$, the distance of the vertices from the horizontal line a(a), from which the weight begins to fall, shall be $\frac{4}{9}$ of the parameter of the curve $\beta\mathrm{e}$ or $(\beta)(\mathrm{e})$: nor does it matter whether the weight falling along the isochronous curve N(e) may arrive at N from a(a) along the path $(\mathrm{a})(\beta)\mathrm{N}$, or some other, or without falling along any on account of another reason by which it may have acquired the same speed and direction. Yet from the infinitude of isochronous lines, along which a weight again can fall from N without accelerating, the descent provides that speed itself, the vertex of which is the point N itself, such as NE, which is a vertical tangent AN to the horizontal.

2) The descent time along the right line $\mathrm{a\beta}$ is to the descent time along the curve $\beta\mathrm{N}$, as half the height $\beta\mathrm{M}$ to $\mathrm{a\beta}$ itself, and therefore if $\beta\mathrm{M}$ shall be twice $\mathrm{a\beta}$, the descent times along $\mathrm{a\beta}$ and $\beta\mathrm{N}$ will be equal. [*i.e.* the average speed v/2 along $\mathrm{a\beta}$ is used in determining the first time, while the speed v at β is used in determining the second time.] It is evident of which ratio: for uniform descent times are as the heights themselves, and from Galileo's demonstration, the time in which the moving weight traverses the height $\mathrm{a\beta}$ in an accelerated motion, is the double of that, in which it traverses an equal height $\beta\mathrm{M}$ (as it happens here, it is allowed to travel with a uniform motion along the curve $\beta\mathrm{N}$ with a constant motion of descent, which has a speed equal to the final speed acquired during the acceleration at β.

I acknowledge this problem was not proposed by me for Geometers of the first order, who are skilled in certain depths of analysis, but rather for these, who think with that learned Frenchman, who seemed to have taken offence at a complaint by me concerning most present day Cartesians (by paraphrasing rather than emulating the master). Indeed both with other such received dogmas among the Cartesians, as well also as they attribute excessively the proclaimed analysis amongst themselves, to the extend that with the help of that they are able to consider any outstanding matter in mathematics (but only if they might wish to take the labour of doing the calculation), but not without being to the detriment of the sciences, which now are developed more needlessly by a misplaced trust. For these I have decided to present the material in this problem in order to exercise their analysis, so that it does not need an involved calculation, but rather an art.

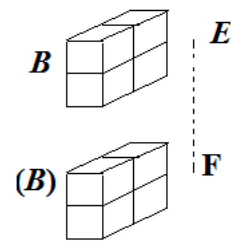
Yet now if anyone may complain the solution has been snatched away from him, he can look for another neighbouring isochronous curve, in which not as hitherto does a weight recede uniformly from the horizontal (or may approach towards that), but from a certain point. From which problem he will go to is thus : *to find the curve in which a weight falling may descend uniformly, not from a line, but from a given point, or towards that same point.*

Such will be the curve NQR, if its nature should be, so that from a give or fixed point A, with whatever right lines drawn to the curve so that for AN, AQ, AR, there shall be the excess of AR over AQ, to the excess of AQ over AN, in the ratio of the time in which the weight descends through the arc QR, to that in which it descends through the arc NQ.

G.G.L. BREVIS DEMONSTRATIO ERRORIS
memoribilis Cartesii & Aliorum circa legem natura, secundum quam volunt a Deo eandem semper quantitatem motus conservari ; qua & in re mechanica abutuntur.

Comunicata in litteris d. 6. Jan.I686. datis.

Fig. VI



Complures Mathematici cum videant in quinque machinis vulgaribus celeritatem & molem inter se compensari, generaliter vim motricem aestimant a quantitate motus, sive producto ex multiplicatione corporis in celeritatem suam. Vel ut magis geometrice loquar , vires, duorum corporum (ejusdem speciei) in motum concitatorum, ac sua mole pariter ac motu agentium, esse dicunt ratione composita corporum seu molium, & earum quas habent velocitatum. Itaque cum rationi consentaneum sit, eandem motricis potentiae summam in natura ,conservari : & neque imminui, quoniam videmus nullam vim ab uno corpore amitti, quin in aliud transferatur; neque augeri, quia vel ideo motus perpetuus mechanicus nuspiam succedit, quod nulla machina, ac proinde ne integer quidem mundus suam vim intendere potest sine novo externo impulsu; inde factum est ut Cartesius, qui vim motricem, & quantitatem motus pro re aequivalente habebat, pronunciaverat eandem quantitatem motus a Deo in mundo conservari.

Ego vero, ut ostendam quantum inter haec duo intersit, suppono, *primo* corpus cadens ex certa altitudine acquirere vim eousque rursus assurgendi, si directio ejus ita ferat, nec quicquam externorum impediat: exempli causa, pendulum ad altitudinem ex qua dimissum est praecise rediturum esse, nisi aeris resistentia similiaque impedimenta exigua alia nonnihil de vi ejus absorberent a quibus nos quidem nunc animum abstrahimus. Suppono item *secundo*, tanta vi opus esse ad elevandum corpus A unius librae usque ad altitudinem CD quatuor ulnarum, quanta opus est ad elevandum corpus B quatuor librarum, usque ad altitudinem EF unius ulna. Omnia haec a Cartesianis pariter ac caeteris Philosophis & Mathematicis nostri temporis conceduntur. Hinc sequitur corpus A delapsum ex altitudine CD, praecise tantum acquisivisse virium, quantum corpus B lapsum ex altitudine EF. Nam corpus (A) postquam lapsu ex C pervenit ad D, ibi habet vim reassurgendi usque ad C, per *suppos.*1, hoc est vim elevandi corpus unius librae (corpus scilicet proprium) ad altitudinem quatuor ulnarum. Et similiter corpus (B) postquam lapsu ex E pervenit ad F, ibi habet vim reassurgendi usque ad E, *per suppos.* 1, hoc est vim elevandi corpus quatuor librarum (corpus scilicet proprium) ad altitudinem unius ulnae. Ergo per *suppose.* 2 vis corporis (A) existentis in D, & vis corporis (B) existentis in F, sunt aequales.

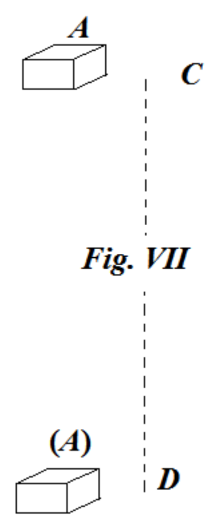


Fig. VII

Videamus jamen & quantitas motus utrobique eadem sit. Verum ibi praeter spem discrimen maximun reperietur. Quod ita ostendo. Demonstratum est a Galilaeo, celeritatem acquisitam lapsu CD, esse duplum celeritatis acquisitae lapsu EF. Multiplicemus ergo corpus A quod est ut 1, per celeritatem suam que est ut 2, productum seu quantus motus erit ut 2, rursus multiplicemus corpus B quod est ut 4. Ergo quantitas motus quae est corporis (A) existentis in F, & tamen in D, est dimidia quantitatis motus quae est corporis (B) existentis in F, & tamen paulo ante vires utrobique inventae sunt aequales. ltaque magnum est discrimen inter vim motricem & quantitatem motus, ita ut unum per alterum aestimari non possit, quod ostendendum susceperamus. Ex his apparet, quomodo vis aestimanda sit a quantitate effectus, quem producere potest. exempli gratia ab altitudine ad quam ipsa corpus grave datae magnitudinis & speciei potest elevare, non vero a celeritate quam corpori potest imprimere. Non enim dupla sed majore vi opus est ad duplam eidem corpori dandam celeritatem. Nemo vero miretur in vulgaribus machinis , vecte, axe in peritrochio, trochlea, cuneo, cochlea & similibus aequilibrium esse, cum magnitude unius corporis celeritate alterius, quae ex dispositione machinae oritura esset, compensatur ; seu cum magnitudines (posita eadem corporum specie) sunt reciproce ut celeritates; seu cum eadem alterutro modo prodiret quantitas motus. Ibi enim evenit etiam eandem utrobique futuram esse quantitatem effectus, seu altitudinem descensus aut ascensus; in quodcunque aequilibrii latus motum fieri velis. Itaque per accidens ibi contingit, ut vis motus quantitate possit aestimari. Alii vero casus dantur, qualis is est quem supra attulimus, ubi non coincidunt.

Caeterum cum nihil sit probatione nostra simplicius, mirum est vel Cartesio vel Cartesianis, viris doctissimis, in mentem non venisse. Sed illum quidem nimia fiducia sui ingenii in transversum egit, hos alieni. Nam Cartesius, solito magis viris vitio, postremo factus est paulo praefidentior. Cartesiani autem non pauci vereor ne paulatim Peripateticos complures imitari incipient, quos irrident, hoc est ne pro recta ratione natura .rerum, consulendis magistri libris assuefiant.

Dicendum est ergo vires esse in composite ratione corporum (ejusdem gravitatis specificae seu soliditatis) & altitudinun celeritatis productricium, ex quibus scilicet labendo tales celeritates acquiri potuissent; vel generalius (quia interdum nulla adhuc celeritas producta est) altitudinum prodituraram : non vero generaliter ipsarum celeritatum, utcunque id plausibile prima specie videatur, & plerisque sit visum ; ex quo complures errores nati sunt, qui scriptis mathematico-mechanicis. *RR. PP Honarati Fabry & Claudii des Chales,* itemque *Joh. Alph. Borelli &* aliorum virorum, caeteroqui in his studiis praestantium, deprehenduntur. Quin & hinc factum puto, quod nuper Regula Hugeniana circa centrum oscillationis pendulorum, quae verissima est, a nonnullis viris doctis in dubium fuit vocata.

No. XVII. (Dutens Book III)

COURTE REMARQUE DE M. L'ABBE' DE CONTI,
où l'on montre à M. G. G. *Leibniz* (a) le paralogisme contenu dans l'objection precedente.

Extraite des Nouvelles de la Republique des Lettres Ju mois de Septembre 1686.

Monsieur *Leibniz s'*étonne que sa preuve , qu'il croit la plus simple du monde, ne se soit pas présentée à l'esprit de M. Descartes , ni à celui des Cartesiens. Mais il faudroit bien plus s' étonner si un Philosophe & un Géomètre de tant de pénétration avoit pû donner par mégarde dans une telle pensée , & y précipiter avec lui tant d'habiles gens. Que les savans jugent si c'est lui, ou M. *Leibniz* , qui est allé de travers par une trop grande confiance en son esprit, le defaut ordinaire des grands hommes. M. *Leibniz* se donne un souci qui est à la vérité d'une bonne ame , mais un peu trop à contre-tems , lorsqu'il a peur que les Disciples de M. *Descartes* n'imitent les Péripateticiens dont ils se moquent. Voyons un peu cette erreur considerable , qu'il prétend détruire.

Il dit 1°. que M. *Descartes* assûre que Dieu conserve dans l'Univers la mêne quantité de movement ; 2°. que ce mêne Philosophe compte pour choses équivalentes la force motrice , & la quantité du movement ; 3°. que plusieurs Mathematiciens font en général l'estime de la force mouvante par la quantite du mouvement , ou par le produit de la multiplication du corps par sa vitesse. Or il prétend que ces choses répugnent entre elles, qu'ainsi la force mouvante, & la quantité du mouvement different beaucoup, & que cette régle de M. *Descartes* est fausse :
la même quantite du mouvement eft toujours conservée dans la nature.

Pour ce qui est de la derniére partie de sa consequence , c'est aux lecteurs Cartésiens à examiner comment elle peut être liée avec *ses prémisses.* Pour la premiere, il la prouve ainsi. Selon M. *Descartes* & les
" autres Mathématiciens il ne faut pas mains de force pour élever un corps d'une livre á la hauteur de 4 aunes que pour elever un corps de 4 livres à la hauteur d'une aune : d'où il s'ensuit que le simple tombant de la hauteur quadruple acquiert précisément la même , force que le quadruple tombant de la hauteur simple; car l'un & l'autre acquerroit une telle force, que les obstacles externes étant ôtés il pourroit remonter d'où il seroit descendu. De plus *Galileo* a démontré que la vitesse qu'un corps acquiert en tombant de la hauteur de 4 aunes est le double de la vitesse qu'il acquiert en tombant de la hauteur d'une aune. Multipliant donc le corps d'une livre par sa vitesse, c'est-à-dire , 1 par 2 le produit , ou la quantité du mouvernent sera comme 2 , & multipliant le corps de 4 livres par sa vitesse , c'est-à-dire, 4 par 1, le produit, où la quantité du mouvement sera comme 4, donc l'une de ces quantites est la moitié de l'autre , quoique peu auparavant les forces ayent été trouvées égales, les forces, dis je , que M. *Descartes* ne distingue point des quantités du mouvement. Donc &c. "

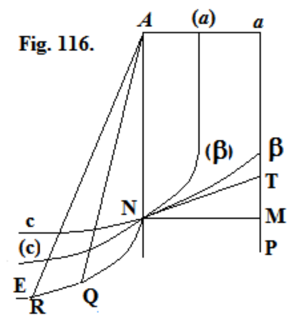
J'admire que M. *Leibniz* n'ait pas aperçu le paralogime de cette preuve, car où est l'homme un peu habile dans les méchaniques qui n'entende que le principe des Cartésiens touchant les 5 machines vulgaires regarde les puissances *isochrones,* ou les mouvemens imprimés en tems égaux, lorsque l'on compare deux poids ensemble? Car on démontre dans les Elémens que 2. mobiles inégaux en volume comme 1. & 4. mais égaux en quantité de mouvement comme 4 ont des vitesses proportionelles en raison reciproque de leurs masses, comme 4 à 1 & par consequent qu'ils parcourent toujours in même tems des espaces proportionnels à ces vitesses. Outre cela *Galilée* montre que les espaces décris par les corps qui tombent, sont en même raison entr'eux que les quarres des temps. Ainsi dans l'exemple de M. *Leibniz* le corps d'une livre monteroit à la hauteur de 4 aunes dans un tems comme 2, & le corps de 4 livres monteroit à la hauteur d'une aune dans un tems comme 1. Puis done que les tems sont inegaux, il n'est pas étrange qu'il trouve inégales dans cette chûte les quantites du mouvement, quoiqu'elles eussent été trouvées égales dans une chûte que l'égalite de tems rendoit tout-à-fait différente de celle- ci. Supposons presentement que ces deux corps ne se meuvent qu'en même tems, c'est-a-dire, qu'ils sont suspendus a une même balance & à des distances reciproques à leur grosseur, nous trouverons égales les quantites opposées de leurs mouvemens, ou les forces de leurs poids, soit que nous multipliions leurs mases par leurs distances, soit que nous le fassions par leurs vitesses. La chose arrive autrement lorsque les tems sont inégaux. D'où il paroit que ni M. *Descartes* ni aucun autre ne se trompe ici , & je doute fort qu'aucun de ces hommes doctes, qui ont depuis peu contesté la regle de M. *Huygens* touchant le centre d'Oscillation, change de sentiment à cause de cette objection de M. *Leibniz.*

III.

DE LINEA ISOCHRONA, IN QUA GRAVE SINE ACCELERATIONE DESCENDIT, ET DE CONTROVERSIA CUM DN. ABBATE DE CONTI.

Cum a me in his Actis Martio 1686 editis publicata esset demonstratio contra *Cartesianos,* qua vera virium aestimatio traditur, ostenditurque non quantitatem motus, sed potentiae, a quantitate motus differentem servari, Vir quidam doctus in Gallia, Dn. Abbas *De Conti,* pro Cartesianis respondit, sed, ut post apparuit, vi mei argumenti non satis perspecta. Credidit enim, recepta quaedam alia principia a me impugnari, quae in Novellis Reip. Litterar. mens. Jun. 1687 p. 579 enumerat, et negat p. 579 seq. se agnoscere contradictionem, quam ego in illis invenire mihi videar: cum tamen nunquam mihi de illis dubitare in mentem venerit, quemadmodum ipsum admonui Novell. Reip. Lit. Septemb. 1687. Idem ut eluderet objectionem meam, conjecerat se in diverticulum temporis, quod eo modo, quo conceptus a me erat status controversiae, plane est incidentale. Eadem enim manente altitudine, eadem vis acquiritur aut impenditur a gravibus quocunque tempore indulto, quod pro inclinatione descensus majore minoreve augetur aut minuitur. Ea occasione, quo magis appareret, tempus atque adeo distinctionem inter potentias isochronas ad anisochronas hoc loco nihil ad rem facere, et ut ex disputatione nostra aliquid incrementi scientia caperet, *problema* tale, a me inter scribendum solutum, et, ut videtur, non inelegans, ipsi proposui in dictis Novellis Septembr. 1687: " Invenire lineam isochronam, in qua grave, descendat uniformiter, sive aequalibus temporibus aequaliter accedat ad horizontem, atque adeo sine acceleratione et aequali semper velocitate deorsum feratur." Sed Dn. Abbas *De Conti* nihil ultra reposuit, sive quod problema attingere nollet, sive quod agnita tandem mente mea, satisfactum sibi judicaret. Sed ejus loco problema hoc sua opera dignum judicavit Vir celeberrimus *Christianus Hugenius*, cujus solutio mea prorsus consona extat in Novellis Reip. Lit. Octbr. 1687, sed suppressa demonstratione et explicatione Haec igitur ego supplere hoc loco volui, facturus citius, nisi aliquid hic a Dn. Abbatis industria exspectavissem.

Fig. 116.



Problema. Invenire lineam planam, in qua grave sine acceleratione descendit.

Solutio. Sit (fig. 116) linea parabolocides quadrato- cubica quaecunque βNe (nempe ubi solidum sub quadrato basis NM et parametro aP aequale est cubo altitudinis βM) ita sita, ut verticis β tangens βM sit perpendicularis horizonti, in cujus lineae puncto quocunque N si ponatur grave ea descendendi ulterius celeritate praeditum, quam potuit acquirere descendendo ex horizonte Aa, cujus elevatio $a\beta$ supra verticem β sit $\frac{4}{9}$ parametri curvae, tunc idem grave descendet porro uniformiter per lineam Ne, utcunque
continuatam, ut desiderabatur.

Demonstratio. Recta NT curvam βNe tangat in N et ipsi βM occurrat in T. Utique (ex nota proprietate tangentium hujus curvae) erit TM ad NM in subduplicata ratione $a\beta$ ad βM. Ergo TM ad TN erit in subduplicata ratione $a\beta+\beta$M seu ad aM. Jam ratio TM ad TN eadem est, quae velocitatis per curvam dcscendendi (seu horizonti porro in curva appropinquandi), quam grave habet positum in N ad velocitatem, qua idem ex N porro, non per curvam, sed libere descenderet, si posset (ut constat ex natura motus inclinati). Sed velocitas haec libera porro est ad constantem quandam in subduplicata ratione aM ad aβ*;* sunt enim (ut ex motu gravium constat) velocitates liberae in altitudinum (unde descendendo quaesitae sunt) aM subduplicata ratione: ergo velocitas descendendi per curvam Ne, quam grave habet in quocunque curvae puncto N positum, est ad velocitatem constantem in composita subduplicata ratione aβ ad aM et aM ad aβ, quae est ratio aequalitatis. Ipsamet igitur velocitas illa per curvam descendendi est constans, seu ubique in curva Ne eadem. Quod praestandum erat.

Consectaria: 1) Grave celeritatem habens tamquam lapsum ab altitudine aliqua Aa, descendere potest ex eodem puncto N per curvas isochronas infinitas, sed ejusdem speciei, seu sola magnitudine parametri differentes, ut Ne, N(e), NE, quae omnes sunt paraboloeides quadrato-cubicae, adeoque similes inter se. Imo quaelibet hujusmodi paraboloeidum hic inservit, modo ita collocetur, ut aβ vel $(\mathrm{a})(\beta)$ distantia verticis ab horizontali a(a), unde descendere incepit grave, sit $\frac{4}{9}$ parametri curvae βe vel $(\beta)(\mathrm{e})$: nec refert, an grave isochrone descensurum in curva N(e) pervenerit ad N ex a(a) per viam $(\mathrm{a})(\beta)$N, an per aliquam aliam, aut sine descensu ullo ob aliam causam eandem celeritatem atque directionem acquisiverit. Ex infinitis tamen istis lineis isochronis, in quibus grave ex N porro sine acceleratione descendere potest, ea celerrimum ipsi descensum praebet, cujus vertex est ipsum punctum N, qualis est NE, quam recta AN horizonti perpendicularis tangit.

2) Tempus descensus per rectam aβ est ad tempus descensus per curvam βN, ut dimidia altitudo βM ad ipsam aβ ac proinde si βM sit dupla aβ, aequalia erunt tempora descensuum per aβ et per βN. Quorum ratio manifesta est: nam tempora descensus uniformis sunt inter se ut altitudines, et ex demonstratis a *Galilaeo,* tempus quo mobile percurrit altitudinem aβ motu accelerato, est duplum ejus, quo percurrit aequalem altitudinem βM (ut hoc loco fit, licet per curvam βN motu uniformi, qui celeritatem habet aequalem ultimae per accelerationem acquisitae in β.

Hoc autem problema fateor me non Geometris primariis proposuisse, qui interiorem quandam Analysin callent, sed his potius, qui cum *erudito illo Gallo* sentiunt, quem mea de *Cartesianis* plerisque hodiernis (Magisti paraphrastis potius, quam aemulatoribus) querela suboffendisse videbatur. Tales enim cum alias receptis inter Cartesianos dogmatibus, tum etiam analysi inter ipsos pervulgatae nimium tribuunt, adeo ut se ipsius ope quidvis in Mathesi (si modo velint scilicet calculandi laborem sumere) praestare posse arbitrentur, non sine detrimento scientiarum, quae falsa jam inventorum fiducia negligentius excoluntur. His materiam exercendae suae Analyseos praebere volueram in hoc problemate, quod non prolixo calculo, sed arte indiget.

Si quis tamen praereptam sibi jam solutionem queratur, poterit *aliam isochronam* huic vicinam quaerere, in qua non, ut hactenus, grave uniformiter recedat ab horizontali (vel ad eam accedat), sed a certo puncto. Unde problema irit tale, *invenire lineam, in qua descendens grave recedat uniformiter a puncto dato, vel ad ipsam accedat.*

Talis foret linea NQR, si ejus esset naturae, ut ex puncto dato seu fixo A, ductis rectis quibuscunque ad curvam ut AN, AQ, AR, esset excessus AR super AQ, ad excessum AQ super AN, ut tempus quo descenditur per arcum QR, ad tempus quo descenditur per arcum NQ.

In der Beilage zu dieser Nummer folgt Hugens' Lösung des in Rede stehenden Problems, auf die hier Leibniz Bezug nimmt. Derselbe hatte bereits seine grosse Reise nach ltalien angetreten (Herbst 1687), als die Nummer der Novelles de la Republique des lettres, welche die Lösung von Hugens enthält, zu seiner Kenntuiss gelangte. Voll Freude, dass sein vochverehrter Lehrer und Freund das Problem der Beachtung für werth gehalten, entwarf Leibniz zu Pilsen_in Böhmen Zusätze, die er nach dem Vermerk auf dem Manuscripte dem Herausgeber der Nouvelles de la Repub. des lettres übersandte. Es lässt sich nicht ermitteln, ob die Ahsendung wirklich erfolgte; ich habe in dem auf der Königlichen Bibliothek zu Hannover befindlichen Exemplar des genannien Joumals diese Zusätze Leibnizens vergeblich gesucht.