

ON THE PRINCIPLE OF LEAST ACTION

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I

If the question were who among the philosophers was the first to realize that nature in all its operations follows the easiest path, or what amounts to the same thing, expends the least amount of effort, it would surely be ridiculous to suppose that any one modern philosopher could lay claim to this glorious discovery. The earliest philosophers had already recognized that nature does nothing in vain and this relates perfectly to [the question of] least expense. For if nature were to employ superfluous expense, it would clearly involve nature doing something in vain. Aristotle already makes references to this doctrine, and it seems that he has taken this idea from those who preceded him rather than envisioning it himself. The proposition has subsequently had such enormous influence in the Schools that it was regarded as one of the first precepts of philosophy until Descartes ultimately dared to reject it. So when Mr. Koenig objects that Malebranche, s'Gravesande, Wolff, along with others, have said that nature always follows the easiest route, or uses the least amount of effort in its processes, not only do we agree, but we also acknowledge that he could have named even more. Also our Illustrious President never claimed that no one before him had thought of this law and has willingly relinquished this glory, such as it is, to those whom Mr. Koenig deems worthy of receiving it.

II

Thus it is not a question of looking for who first said that such a law exists in nature, but who first made it known, and who determined the actual resources that nature conserves, not merely on occasion, but always, and in all of its operations. And what we deny in all fairness is that no one else had done this before our Illustrious President. Thus we would readily concede that others have known of this law in general, but they have understood it in such an obscure manner that they have entirely ignored that which nature conserves. We even concede that

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some authors have recognized what a *Minimum* is what in some of nature's operations, but it was only in cases so particular that one could never apply it to other cases, or at least no one saw any way of applying it. This initial understanding is worthy of praise, and must be regarded as having paved the way to a more extensive understanding, for our knowledge only advances in degrees, from the most particular to the most general. Yet as we are considering here the universal force of nature, which extends to all of its operations, we cannot attribute to it anything that only exists in particular cases. And it must be said that whoever determined that there is a *Minimum* in all of nature's operations is the one who discovered what nature intends. Knowing this is the highest degree of our understanding. Yet before Mr. de Maupertuis, no one had claimed this discovery. And as he clearly exposed this universal law, we see then that he deserves the glory for first discovering it. For how could we believe that he took from another, what no one before him is said to have known?

III

But there is no one with whom we would be less likely to have dispute than Professor Koenig, who boldly denies that there is in nature such a universal law and pushes this absurdity to the point of mocking the Principle of Conservation, which constitutes the *Minimum* that nature appoints. In addition, he introduces the great Leibniz as claiming and explaining that he was far from knowing such a principle. From this we see that Mr. Koenig cannot deny our President the discovery of the principle that he himself considers to be false. However, he is hardly consistent when he cites Malebranche, s'Gravesande, Wolff, and even Leibniz, as the authors from whom Mr. de Maupertuis has drawn his principle. Since Koenig does not accuse them of any errors, how can he accuse Mr. de Maupertuis of any if Mr. de Maupertuis has taken his principle from them? But he says that what Maupertuis has taken from these authors is true and what he has added is false. Koenig admits therefore that the principle of our Illustrious President contains something that has never before been claimed by these authors and he grants it to him. We concur with this admission. Thus, Mr. de Maupertuis' first principle differs from the views of the authors that we have just mentioned in its universality and it is this that Mr. Koenig opposes. From this he grants that these authors have been a long way away from an understanding of this universal law of nature and so positively leaves to our President alone the discovery of this law, and this is the principal state of the question. We will not trouble ourselves

with what Mr. Koenig opposes, namely that the principle is false. The truth of the matter will never depend on his opinion. We shall later show the miserable errors he has made in the proofs he has boasted about with such conceit. Having thus destroyed these objections, which he believed to be so unassailable, he shall be forced to admit that Mr. de Maupertuis' principle is not only quite beautiful and of the outmost importance to all of philosophy, but even more, that we must attribute the glory of this discovery to none other than Mr. de Maupertuis. Since he believed this discovery to be false and thus worthy of reproach, after having shown him its truth, it would be necessary that he regard it as worthy of praise.

IV

However this controversy, in which Mr. Koenig has involved the Academy, has given birth to the opportunity to raise the question of the *Minimum* that nature assumes. As Mr. Koenig has clearly shown, this is usually understood quite poorly and it will not be out of turn to state briefly what has been said and explain what has been done about this question before Mr. de Maupertuis.

V

First, although the earliest philosophers and the disciples of Aristotle established that nature does nothing in vain and that in all its processes chooses the shortest path, and although for them this principle is what constitutes the final cause that nature most intended, we do not however see them explaining any phenomena through this principle. If all the movements of nature followed a straight line, one could conclude that nature chooses straight lines because it is the shortest path between two points. In fact, one sees this with Ptolemy, for this is the cause that he gives for why rays of light come to us in straight lines. But as that only happens when the medium that these rays traverse is homogeneous, this explanation is too limited to merit any attention. With the exception of this case, there is barely any other type of motion produced in nature that follows a straight line, and so it is clear enough that, properly speaking, it is not the shortest route that nature assumes. Thus, there were philosophers who thought that one could also take the circular line to be the shortest route, perhaps because they had learned from the geometers that on the surface of a sphere the great arcs were the shortest lines between two points. Given this, and given that they believed that the celestial bodies moved in great circles, they do not hesitate to place the final cause of the movement

in this property of the circle. As we now know, however, the lines traversed by the celestial bodies are not great circles, but instead belong to a group of curves that are most transcendental. The view that nature follows straight or circular lines is entirely abandoned and in the same way, the view that nature always seeks a *Minimum* appears to be entirely reversed. There is no doubt that this was the reason Descartes and his disciples thought it necessary to abolish completely final causes from philosophy and the reason they claimed that in all of the operations of nature we observe extreme inconsistency rather than certain and universal law. Thus, as much as the renewal and progress of philosophy have made us more certain of this principle, it has also, to the contrary, appeared to have moved us farther away from understanding it.

VI

However in certain particular cases, there remained a shadow of this universal principle. One must above all count among these cases the reflection of light, as it does so in such a manner that the angle of reflection is equal to the angle of incidence. Ptolemy showed that the ray follows the shortest route and that if it is reflected in any other manner, the route it would follow would be longer. Thus it is clear that this explanation cannot be applied to refraction, where the route that the rays take does not correspond to the shortest path.

VII

Although it seems therefore that with the direct and reflected movement of rays, nature indeed chooses the shortest route, with refraction, in addition to an infinity of other phenomena, nature's law is not consistent with the shortest route. Thus as we do not find here a universal law, it would be necessary to have recourse to a *Minimum* other than the length of the route, as much in direct movement as in the reflected movements of rays, which in these cases have been confused with the shortest route; this confusion also happened in the refraction of rays. On this issue Fermat established that the rays of light did not seek in their movement the shortest route, but rather the path by which they could arrive at a point in the shortest time. So he posits that in the same medium rays moved with the same speed, in such a way that the times were proportional to the described routes. As such, the shortest route was, as much in direct as reflected movement, necessarily that which took shortest time. But in diaphanous media of different densities, such as air, water, and glass, the speed of light rays were also

different. It is the fastest in the least dense media such as air, slower in the densest like glass, a notion which appears to conform clearly enough to nature. And through this hypothesis, which Descartes attacked so vehemently, Fermat fortunately came, after the most difficult calculations, to explain the phenomenon of refraction and to find that the sine of the angles of incidence and refraction must have preserved between them the same relationship as the experiment has shown.

VIII

But Descartes, indubitably Fermat's enemy, unequivocally banished final causes and so explained refraction in an entirely different manner. Applying the rules of collision of bodies, he showed that a spherical body thrown obliquely into a fluid must veer from its path. And as he established, light rays are just a sequence of small spherical objects. If a ray passed obliquely from one diaphanous medium to another, it must change direction, and from this he also draws out the same rules of refraction indicated by the experiments. But Descartes veered from Fermat in that he believed that light rays move more quickly in a denser medium such as glass than they do in a less dense material such as air. Fermat drew the opposite conclusion. Descartes thought that the cause of the faster speed in glass than in air was that glass offered less resistance than air, and he sought to find reasons for this in the *Principles of Philosophy*. That this caused such intense controversy at the time must seem all the more surprising as Descartes established that light traveled in an instant over the greatest of distances and this is not in accord with the idea of speed. So to pose the question of whether rays travel faster in air or in glass is quite ridiculous.

IX

Although Fermat's view was received mostly by the philosophers and geometers who did not adhere to Descartes' doctrine, he often made claims to the discovery of a general law that nature follows in all of its operations. This subtle man had remarked that the principle of least time only occurred within the movement of light and could not be extended to other phenomena. He was even further from thinking that a rock tossed in the air or the planets traveling through the heavens moved according to the law of least time. Thus even when his view were true, it did not help with the present question, where it was not about some particular principle, but a universal principle which extends to all operations of nature. Moreover, as a result, he had Descartes as an

adversary, and as he could not refute Descartes, he is less harmful to our cause.

X

Leibniz also endeavored to reverse Fermat's explanation. In the Acts of Leipzig, 1682, he proposed that the refraction of light reintroduce to philosophy the final causes that had been banished by Descartes and to reestablish the explanation that Descartes had deduced from the collision of bodies, a view opposed to that of Fermat. He therefore begins by denying that nature either assumed the shortest route or took the least time, but claims that it chooses the easiest route, which must not be confused with these other two. So to assess the easiest route he considers the resistance with which light rays traversed the diaphanous medium and he assumes this different resistance in different media. He even establishes what appears to favor Fermat's opinion, namely, that in the densest media, such as water or glass, resistance is greater than in air and other media that are less dense. Granting this, he considers the difficulty a ray has when traversing some medium and determines the difficulty by multiplying the path by the resistance. He claims that rays always follow the route in which the sum of difficulties calculated in this way is the smallest. And by the method of *Maximums* and *Minimums*, he finds the rule that experiments have revealed. Although at first glance this explanation seems to agree with that of Fermat, it is however subsequently interpreted with such marvelous subtlety that it turns out to be diametrically opposed to it and is in agreement with Descartes. For even though Leibniz has supposed that the resistance of glass is greater than that of air, he nevertheless claims that rays move faster in glass than in air, which is certainly a paradoxical sign as the resistance of glass is greater. Here is how he thinks he supports it: he says that a greater resistance prevents the diffusion of rays, as the rays disperse more when resistance is less, and, since the diffusion has been hindered, the rays narrow in their passage, just as a river that flows on a narrower bed acquires greater speed. So Leibniz' explanation agrees with that of Descartes, in that they both grant to rays a greater speed in a denser medium, but they differ significantly in what they think causes this greater speed. Descartes believed that rays moved with greater speed through a denser medium because there is less resistance, while Leibniz, on the contrary, attributes this greater speed to greater resistance. I am not examining here whether or not Leibniz' view can be accepted, but what I must note is that he seems to want to regard this principle of the easiest route as universal, yet he

never applied it to any other case, nor taught how in other cases this difficulty, which must make a *Minimum*, must be determined. If he says here that it is through the product of the described route and the resistance, in most cases it will be absolutely impossible to define what we mean by resistance, for it is such a vague term. And when there is no resistance, as in the movement of celestial bodies, how is this difficulty to be assessed? This will be by the only described route since there is no resistance we could regard it as the same throughout. But then it follows that, contrary to experience, these movements, and the described route itself, must be a *Minimum* and consequently a straight line. If, on the contrary, movement is made in a resisting medium, will he say that this movement will be such that the product of the described route and the resistance would be a *Minimum*? One would draw from this the most absurd conclusions. Thus we clearly see that the principle of the easiest route, like the one Leibniz proposed and explained, could not be applied to any phenomenon other than the movement of light.

XI

It seems however that one could extend this principle further depending upon the interpretation that gives the following remarks. Supposing that rays move so much more quickly that they encounter greater resistance. In this case, for Leibniz, speed would be proportional to the resistance and could be taken for its measure, and the calculation of the difficulty, as Leibniz has made it, reduces to the product of the described route and the speed. What is being taken as a *Minimum* would agree with the principle of Mr. de Maupertuis, who measures the quantity of action by the same product of the space traversed and the speed. As this product becomes, in effect, the smallest possible, not only in the movement of rays, but also in all of nature's movements and operations, and as the principle of least action consists in this, we might initially think that Leibniz had this principle in mind, which agrees with his principle of the easiest path. But if we were to accept without exception Leibniz's reasoning that attempts to prove that a higher resistance raises the speed, no one would ever believe that in all movement, speed rises with resistance. There is in nature an infinite number of examples where the contrary is so obviously the case and resistance diminishes with the speed. It is therefore by pure chance that here the principle of the easiest path agrees with that of least action. Similarly, Ptolemy's principle of the shortest path in optics and catoptrics agrees with this same principle, although we must not look

for the reason for these phenomena in this principle. Thus, when Leibniz gives his principle of the easiest as a universal law of nature and makes the difficulty proportional to the product of the path and the resistance, he can only reconcile this with the principle of least action in those cases where the speed rises in proportion with resistance, that is, in cases which are quite rare, if not, dare we say, entirely non-existent.

XII

In all other cases therefore, the principle of easiest path will differ significantly from the principle of least action. Leibniz would contradict himself if he were ever to claim that in nature's operations, the product of the described path multiplied by the speed was a *Minimum*, except in the sole cases where the speed was proportional to resistance. From this we conclude with certainty that the principle of least action was not only entirely unknown to Leibniz, but also that he employed a very different principle. This only agrees with the principle of least action in a very few number of extremely unique cases, while in a vast majority of other cases, it was manifestly opposed to it. Moreover Leibniz' principle, however general it may seem, is only in use in a few cases and perhaps only in those about which we have spoken. We cannot even apply it in all other cases because we do not even know how to measure resistance, and in whatever manner one measures it, it would always produce substantial errors. Leibniz never held the principle of least action, far from it. On the contrary, he had an entirely opposite principle, the use of which, except in a single case, was never applicable or led to error. And we also do not see that Leibniz had not wanted to apply this principle in any other way.

XIII

One could not therefore imagine anything more ridiculous than relying upon the fragment of this letter which attributes to Leibniz a principle opposed to the one that he publicly adopted. And one could not redeem this absurdity by supposing that he had held these different principles at different times. For Leibniz explained refraction through an entirely different principle from that of least action, so since he had reached an understanding of this universal principle, which was applicable in this case, the first thing he would have done without a doubt was to apply it to the phenomenon of light, which he had explain using a very different principle.

XIV

It is surely a matter worth noting that a partisan of Leibniz had put to us at the same time the double obligation of proving that the principle of least action is true and that it is not a principle held by Leibniz. It was the singular work of Koenig. He wanted to have us believe that the principle of Mr. de Maupertuis was a chimera. To those whom he could not persuade, he wanted them to believe that the principle was that of Leibniz. He was no more successful doing one than the other.

XV

Yet Leibniz' disciples are accustomed, with good reason, to make a strong case for all of his writings, including the one we are addressing, which is found in the Acts of Leipzig. It is surprising enough that the Illustrious Baron de Wolff who is so attached to all of Leibniz' views relating to the explanation of the refraction of light, has strayed so far from his master and rejected his subtle explanation. Instead he reports word for word in his Elements of Dioptrics the explanation of Fermat, which was rejected by Leibniz. For in his Problem II, §35, this great man, having supposed that the speed of light was different in different media (lower in denser medium, higher in less dense medium), looks for the time that a ray will take to arrive along the route it takes from a given point to another point located in another medium. From this he concludes that *since nature always acts through the shortest path*, the amount of time must be the least. One surely does not see here how he concludes that the shortest route takes place in the least amount of time. Moreover while he neither produces nor invokes any proof for this proposition, everywhere else he can hardly utter a word without invoking the axiom that *the whole is bigger than its part*. Because of this, the greatest disciple of Leibniz not only omitted his explanation of refraction, but still preferred that of Fermat. We can conclude with certainty that Leibniz' explanation appeared quite dubious to such an enlightened man and that it is not in this source that one must search for the principle which governs nature.

XVI

But other than the *Minimum* that nature allocates in the movement of light, the philosophers, and above all the geometers, have searched for a *Minimum* in the other operations of nature. For we must here primarily consult the geometers who are not only able to define in exact terms what a *Minimum* is, but can also demonstrate how this *Minimum*

is achieved. The philosophers differ from the geometers in that they are usually content to use vague terms which signify nothing with precision, and so do not explain what the *Minimum* is, and even less how a *Minimum* is achieved. In the same way they say, in general terms, that nature operates through the shortest or easiest route without explaining what the shortest route is in each case or that it is the easiest. They also fail to prove how in each case this route becomes either the easiest or the shortest. But while the geometers have treated this matter with more exactitude, they have only examined certain particular phenomena deduced from a law of nature that the Ancients accepted only in a confused way, and have searched for that which in the phenomena became, in effect, a *Minimum*. And we find in this sort of thing nothing more than what was observed by some regarding the rules of the collision of bodies, and this involves only an extremely particular case. However Mr. Koenig has brazenly accused our Illustrious President of having extracted his universal principle from it and he has taken the concealing of the names of the authors as a sign of plagiarism. It is an accusation that is even more absurd given the admission of Mr. Koenig himself that this observation of the *Minimum*, which takes place in his appeal to bodies, is extremely limited and only touches upon a certain case of this collision. But the principle produced by Mr. de Maupertuis is universal and all of its force consists in its universality. It cannot be deduced in any manner out of this particular observation. Mr. Koenig names above all s'Gravesande and Englehard as those with whom our Illustrious President has collaborated, and as those who have for a long time acknowledged that which he has provided. Given this, one sees as clear as day how much Mr. Koenig contradicts himself. Since he entirely approves of what these other two authors have said, how can he take Mr. de Maupertuis to be in error, if he simply said the same thing? And as Mr. Koenig declares the principle to be false, how could it be that Mr. de Maupertuis had taken it from these two heroes? Mr. Englehard will certainly not thank Mr. Koenig for having introduced his name into this dispute. It seems in line with the truth that he taught twenty years ago what Mr. de Maupertuis has only proposed recently as an important discovery. As if to confuse things even more, however, Mr. Koenig adds soon afterward that this discovery had been published thirty years ago by s'Gravesande and was known to geometers at least. Therefore, he imputes to Mr. Englehard, who he had just cited favorably, the most shameful plagiarism by making him speak as if he had discovered what could be found in the books of s'Gravesande ten years earlier. Given how injuriously Mr. Koenig treats his friends, it is hardly surprising that he does not blush when behaving with such

iniquity toward his adversaries by accusing them in cases where there is not a trace of plausibility.

XVII

But let us consider what Mr. Englehard and Mr. s'Gravesande claim. Since both said the same thing as our Illustrious President, what one said the other must have said as well. According to the exposition that even Mr. Koenig made, s'Gravesande's discovery consists in this: if two rigid bodies meet in such a manner that after the impact both remain at rest, the sum of the live forces [forces vives] before the impact was weaker, provided that one conceives the relative speed as staying the same. From this one can derive the following proposition: in the collision of rigid bodies, the quantity of live force that dissipates is equal to the smallest amount of live force that the same bodies are able to receive, insofar as their respective speed before the impact stays the same. This is a proposition of no importance and has nothing whatsoever to do with the principle of least action, since it only concerns what dissipates, and this is not even the least amount of living force, but something that reduces to another living force that can only be taken for a *Minimum* under certain, specific considerations. Instead, here, it is about what is truly produced: there is such a difference between the two that it is impossible to imagine a greater one. That s'Gravesande adds the impact of several bodies, coming from the same principle, does nothing for our inquiry. Finally, the force of this proposition is so limited that it only applies to rigid bodies, while the principle of least action has greater range of application and is not subject to any restrictions. Given this, will there be anyone of sound mind who accuses the one who has discovered this most expansive truth of taking it through such a particular case as well? Surely we would not expect such an accusation if we did not know the fury of the blind, quibbling Mr. Koenig. He is so transported by his fury that wherever he finds the word *Minimum*, he believes he has found the source of the principle of least action.

XVIII

As Mr. Koenig himself was unable to find phenomena of movement in which one had observed any *Minimum*, one will be forced to recognize that before Mr. de Maupertuis, there were only a few very limited cases in which one found some reason for the *Minimum*; and there was absolutely no one who has attributed to himself the discovery of a general principle.

XIX

I am not reporting here on my observation that for the movement of celestial bodies, and more generally for the movement of all attracting bodies drawn toward centers of force, if at each instance one multiplies the mass of the body by the distance traveled and by the speed, the sum of all these products is always least. For even though this discovery is certainly much more preferable to those that we have mentioned, and the product that I consider presents the action itself as Mr. de Maupertuis defines it, one must note that having appeared only after Mr. de Maupertuis had exposed his principle, it could not draw into question its originality. Moreover I had not discovered this beautiful property *a priori*, but (to use logical terms) *a posteriori*, deducing the formula after several attempts, which in this movement became a Minimum. I am also not claiming to give it more importance than in the case that I have treated. I had never believed myself to have found a principle that is more all-encompassing, but am content with having found this beautiful property in the motion that occurs around the centers of forces. Mr. Koenig does not seem to pay great attention to this discovery either, since after my proofs, which are not metaphysical, but geometric, he still doubts whether my formulas become *Maximums* or *Minimums*? I would thus have wished that such a great master had examined my proofs and had indicated to us the errors that he believed were hidden in them, because I would have liked to have acquired some insight from such a sublime doctor.

XX

We had also remarked in relation to the stability of bodies certain cases where we evidently found some *Minimum*. It had been easy to see that short bodies could remain in equilibrium if their centers of gravity were not as low as possible. Given this, we have attributed to the stability of short bodies this property, that the distance from their center of gravity to the center of the earth was the shortest. From this principle the geometers have drawn through the isoperimetric method several curves such as the "catenary", formed by a chain that hangs freely attached at both ends, as well as the "*linteaire*", which is the form taken by cloth filled with liquor and other things of that sort, where the common center of gravity occupies the lowest part. But if these bodies are close enough to the center of the earth, or some other center of force, so that the direction of the forces that they seek could not be taken as parallel, the consideration of the center of gravity ceases entirely, because now in these bodies there is no longer a point

that has the property of the center of gravity. Also the principle of greatest descent from the center of gravity no longer applies, It could not therefore pass for general even in the unique state of equilibrium, much less in the state of motion. However we have noticed in some of these cases a sort of center of gravity, through the greatest descent from which one could determine the state of equilibrium, but no one has boasted of having achieved the universal principle that took place in all of these states. Mr. Daniel Bernouilli, one of the most subtle men in these sorts of speculations, has in truth given us something of remarkable beauty for an extremely specific case, when he assigned *a priori* the quantity which, in elastic curves, was a *Minimum*, a proposition that I subsequently proved to be true. If we compare it to other particular principles that we have previously discovered, this discovery, must certainly stand as one of the most sublime. But Mr. Koenig clearly showed that he has not even understood it, as he remains so stubbornly attached to the error of believing that the formulation that Mr. Bernouilli held to be a *Minimum* in the curvature of elastic bodies reduces to zero. We will subsequently show how egregiously false reasoning has thrown him into such an enormous error. Thus all that we have presented until now on the *Minimum* that nature affects in all its processes, as much in the state of movement as in the state of rest, only applies to particular cases. It does not have the connection from which one can draw a more general principle, which leads to cases that we have considered. From this we can see what Mr. de Maupertuis has done in regards to this matter and how little he has to fear the suspicion, which Mr. Koenig wanted to arouse, that he had taken these principles from others.

XXI

Since 1740, in the *Mémoires de l'Académie Royale des Sciences de Paris*, Mr. de Maupertuis has explained the Universal Principle of Rest and Equilibrium, which contains through a marvelous synthesis all the particular principles that we have discussed. It brings together those principles which are drawn from the nature of the center of gravity as much as those which are appropriate to non-rigid bodies, however different they may seem. It also extends with still greater universality to all cases of equilibrium in such a way that they relate to opposing bodies or forces. Through this sole principle, I have not only explained in their entirety all the cases where bodies, either rigid or flexible, elastic or fluid, can never be in equilibrium, but also these cases can be determined with marvelous ease in such a way that this principle

must be considered as an important discovery in mechanics. For having asserted this principle, all that has until now been treated more in dynamics than in hydrodynamics follows so easily from it that even in the most complicated cases, those demanding the most tedious studies through the direct method, one arrives at a very elegant and very simple calculation. The state of equilibrium, especially in machines of all kinds, regardless of their material composition, determines itself with such ease that we do not even need to pay attention to their construction, which often lends itself to a more difficult calculation. And as the first elements of this science follow naturally from this very same principle, one must consider it to be the most convenient foundation, and the most fortunate, in dynamics as much as in hydrodynamics. Indeed, the veracity of this principle can be demonstrated by the most obvious reasoning and does not require the consideration of any kind of motion that would upset the order of the different sciences. For it is only necessary to examine how each particle of the body is affected by the opposing forces to draw from each application a quantity that we can call the *efficiency* of each force. To assure that there will be equilibrium, as soon as the sum of all these *efficiencies* is the least, so that by the unique method of *Maximums* and *Minimums*, one can execute with unbelievable ease all that concerns dynamics and hydrodynamics.

XXII

It would therefore be thoroughly ridiculous to compare such a principle with the most sterile and disagreeable principle that Mr. Koenig has tried to produce. The latter so confuses dynamics with phoronomics that it would be impossible to come to an understanding of any state of equilibrium for someone who did not previously have perfect knowledge of motion: It is not only impossible simply to suppose this in dynamics, but it usually requires the most sublime research and is only able to be applied in a very few number of cases. As we shall show in what follows, the principle of Mr. Koenig is only applicable in one or two cases and only with the most egregious confusion of the different sciences.

XXIII

The principle that Mr. de Maupertuis discovered is therefore worthy of the greatest of praise; and without a doubt it is far superior to all discoveries that have been made in dynamics up until now. Its utility does not only touch all of dynamics, which would already be of great universality, but with a minor and natural addition, it stretches

with the greatest success to all of the science of motion. Since for each proposed motion one can easily understand that what we have called *efficiency*, having been taken at each instant, the sum must be a *Minimum*. By including this condition of motion, one sees the birth of the other universal principle of our Illustrious President, what he calls the *Principle of Least Action*. For one can easily demonstrate, as I did in a particular treatise, that if all the efficiencies of which we have spoken above are multiplied by the element of time, one uncovers the product of the mass by the speed and by the small distance covered, a product that contains the idea of action.

XXIV

These two principles are so intimately connected to one another that one can consider them to be one and the same. And as the principle of movement follows clearly from the principle of equilibrium, the principle of movement, or of least action, is applicable to all cases of equilibrium. Thus all of the sciences that we are accustomed to grasping together under the title of Mechanics, whether considering equilibrium or movement, are so thoroughly founded in this principle that one can deduce them quite productively and perfectly from it. One also sees through this that anyone who has accepted one of these principles can no longer doubt the other. Since the principle of equilibrium is the most rigorously proven, one must count on the principle of movement with the same certitude. Therefore the combination of these two principles, or rather each taken separately, since they are most closely linked to one another, declares this law to be the most universal in nature. Through it we finally know distinctly what we had previously only suspected, namely that nature in all of its operations assumes a *Minimum*, and that this *Minimum* is certainly contained in the idea of action, as it is defined by Mr. de Maupertuis in such a way that there is nothing left to object.