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Hertz’s ideas on mechanics

By Henri Poincaré

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In 1890, the great electrician Hertz had arrived at the apogee of his glory. All of the European academies had lavished all the awards upon him that they had to give. The entire world hoped that there would be many years ahead for him and that they would be as brilliant as the ones that he began with.

Unfortunately, the illness that was to take him from us so prematurely had already been contracted, and his experimental activity soon slackened and almost stopped completely. He barely had enough time to install his new laboratory in Bonn. Various maladies impeded him, as well as depriving us of the discoveries that he promised to make.

He further contributed to the physical sciences by the enormous influence that he exerted and the advice that he gave to his students. However, it is true that that period was distinguished by only a single personal discovery of surpassing importance, namely, the transparency of aluminum to cathode rays.

However, although he was thus cruelly diverted from the studies that had been so dear to him, nonetheless, he did not remain inactive. Perhaps his senses betrayed him, but his intellect remained, and he applied it to some profound reflections on the philosophy of mechanics. The results of those reflections were published in a posthumous book, and I would like to summarize them and discuss them briefly here.

Hertz first criticized the two principal systems that had been proposed up to now, and that I shall call the *classical system* and the *energetic system*, and he proposed a third one that I shall call the *Hertzian system*.

I. – **THE CLASSICAL SYSTEM**.

§ 1. – Definition of force.

The first attempt at coordinating the facts of mechanics is the one that we shall call the *classical system.* According to Hertz, it is:

> “…the royal route whose principal stations bear the names of Archimedes, Galileo, Newton, and Lagrange.”
>
> “The fundamental notions that one finds at the point of departure are those of *space*, *time*, *force,* and *mass.* In that system, force is regarded as the cause of motion; it exists in advance of the motion and is independent of it.”

I would like to explain the reasons why Hertz was not satisfied with that way of considering things.

First, one has the difficulties that one encounters when one wishes to define the fundamental notions. What is *mass*? Newton responded, "it is the product of volume with density." – Thomson and Tait responded, "it would be better to say that density is the quotient of mass by volume." – What is *force*? Lagrange responded, "it is a cause that produces the motion of body or tends to produce it." – Kirchhoff said, "it is the product of mass times *acceleration*." But then, why does one not say that mass is the quotient of force by acceleration?

Those difficulties are inextricable.

When one says that force is the cause of a motion, one is dealing with metaphysics, and if one accepts that definition then it would be absolutely sterile. In order for a definition to be useful, it must tell one how to *measure* the force. If it can do that, moreover, then it would not at all be necessary for it to tell one what force is *intrinsically* nor whether it is the cause or the effect of motion.

One must first define the equality of two forces then. When does one say that two forces are equal? It is, one answers, when they are applied to the same mass and they impart the same acceleration upon it or when they are directly opposed to each other and it is found to be in equilibrium. That definition is only an illusion. One cannot unhook a force that is applied to one body in order to hook it up with another, as one unhooks a locomotive in order to couple it with another train. It is therefore impossible to know what acceleration a force that is applied to one body would impart upon another body *if* it were applied to it. It is impossible to know how two forces that are not directly opposed to each other would behave *if* they were directly opposed.

That is the definition that one seeks to materialize, so-to-speak, when one measures a force with a dynamometer or by equilibrating it with a weight. Two forces F and F', which I shall suppose to be vertical and point from down to up, for simplicity, are applied to two bodies C and C'. I first suspend the same heavy body P from the body C and then from the body C'. If there is equilibrium in both cases then I conclude that the two forces F and F' are equal to each other, since they are both equal to the weight of the body P.

But am I sure that the body P has kept the same weight when I transport it from the first body to the second? Far from that being true, *I am certain of the contrary*. I know that the intensity of gravity varies from one point to another, and that it is stronger, for example, at the poles than at the equator. Without a doubt, the difference is quite weak, and in practice, I would not take it into account. However, a well-constructed definition must be mathematically rigorous: That rigor does not exist here. That is why I say that weight obviously applies to the force of the spring in a dynamometer, but that temperature and a host of circumstances can make that vary.

But that is not all. One cannot say that the weight of the body P is applied to the body C and equilibrates the force F directly. What is applied to the body C is the action A of the body P on the body C. The body P is, in its own right, subject to, on the one hand, its weight, and on the other, the reaction R of the body C on P. By definition, the force F is equal to the force A, because it equilibrates it. The force A is equal to R by virtue of the principle of the equality of action and reaction. Finally the force R is equal to the weight of P, because it equilibrates it. It is from those three equalities that we deduce the equality of F and the weight of P as a consequence.

We are then obliged to introduce the principle of the equality of action and reaction itself into the definition of the equality of two forces. *In that respect, that principle must not be regarded as an experimental law, but as a definition.*

We then come down to Kirchhoff's definition: *Force is equal to mass, multiplied by acceleration.* That "law of Newton," in turn, ceases to be regarded as an experimental law, since it is now just a definition. However, that definition is also insufficient, since we do not know what mass is. Without a doubt, it permits us to calculate the relationship between two forces that are applied to the same body at different instants, but it tells us nothing about the relationship between two forces that are applied to two different bodies.

In order to complete it, one must again resort to Newton's third law (viz., the equality of action and reaction), which is further regarded, not as an experimental law, but as a definition. Two bodies A and B act upon each other. The acceleration of A, multiplied by the mass of A, is equal to the action of B on A. Similarly, the product of the acceleration of B times its mass is equal to the reaction of A on B. Since the action is equal to the reaction, by definition, the masses of A and B are inversely proportional to the accelerations of those two bodies. The ratio of those two masses is then defined, and it is up to experiments to verify that the ratio is constant.

That will indeed be the case if the two bodies A and B are the only ones present and abstracted from the action of the rest of the world. Nothing of the sort is true: The acceleration of A is not due to solely the action of B, but to the action of a host of other bodies C, D… In order to apply the preceding rule, one must then decompose the acceleration of A into several components and discern which of those components is the one that is due to the action of B.

Furthermore, that decomposition will be possible if we *assume* that the action of C on A is simply added to that of B on A without the presence of the body C modifying the action of B on A or the presence of B modifying the action of C on A. Consequently, if we assume that two arbitrary bodies attract each other than their mutual action will point along the line that connects them and depend upon only their separation distance. In a word, if we assume *the hypothesis of central forces.*

One knows that in order to determine the mass of celestial bodies, one must appeal to an entirely different principle. The law of gravitation tells us that the attraction of two bodies is proportional to their masses. If r is their distance, m and m' are their masses, and k is a constant then their attraction will be:

$$\frac{k\,m\,m'}{r^2}$$

What one measures then is not the mass – i.e., the ratio of force to acceleration – but the mass of attraction. That is not the inertia of the body, but its attracting force.

That is an indirect procedure whose use is not theoretically indispensable. It might very well be the case that the attraction is inversely proportional to the square of the distance without being proportional to the masses, which would make it equal to:

$$\frac{f}{r^2}$$

but without one having:

$$f = k\, m\, m'.$$

If that were true then one could nonetheless measure the masses of those bodies by observing the *relative* motions of the celestial bodies.

But do we have the right to assume the hypothesis of central forces? Is that hypothesis rigorously exact? Is it certain that it will never be contradicted by experiments? Who dares to assert that? Moreover, if we must abandon that hypothesis then the entire edifice that was raised so laboriously would collapse.

We no longer have the right to speak of the component of the acceleration of *A* that is due to the action of *B*. We have no means for discerning what is due to the action of *C* or some other body. The rule for measuring the masses will then become inapplicable.

What does the principle of the equality of action and reaction then rest upon? If one rejects the hypothesis of central forces then that principle must obviously be stated thus: The geometric resultant of all of the forces that are applied to the various bodies of a system that is isolated from any external action will be zero, or, in other words, *the motion of the center of gravity of that system will be uniform and rectilinear.*

It seems that one has a means for defining mass in that: The position of the center of gravity obviously depends upon the values that one attributes to the masses. One must arrange those values in such a fashion that the motion of that center of gravity is uniform and rectilinear. That will always be possible if Newton's third law is true, and that will be possible in only one way, in general.

However, systems that are isolated from all external action do not exist. All of the parts of the universe exert a more or less strong effect on all of the other parts. *The law of motion of the center of gravity is rigorously true only if one applies it to the entire universe.*

However, one would then have to observe the motion of the center of gravity of the universe in order to infer the values of the masses. The absurdity of that conclusion is obvious. We only know about its relative motions. The motion of the center of gravity of the universe will remain eternally unknown to us.

Nothing remains then, and our efforts have been fruitless. We are then forced to make the following definition, which is only a confession of our powerlessness: *Masses are convenient coefficients to introduce into the calculations.*

We can reconstruct all of mechanics by attributing different values to all masses. That new mechanics will not contradict either experiments or the general principles of dynamics (e.g., the principle of inertia, proportionality of masses and accelerations, equality of action and reaction, uniform, rectilinear motion of the center of gravity, principle of areas).

However, the equations of that new mechanics will be *less simple.* Note well: It will be only the first terms that will be less simple – i.e., the ones that experiment has already made known to us. Perhaps one can alter the masses of small quantities without the *complete* equations gaining or losing any simplicity.

I have insisted upon discussing that point much longer than Hertz himself. However, I wanted to show that Hertz did not seek to have simply a German quarrel with Galileo and Newton. We must conclude that with the classical system, *it is impossible to give a satisfactory conception to force and mass.*

§ 2. Various objections.

Hertz then demanded to know whether the principles of mechanics are rigorously true. He said:

> "In the opinion of many physicists, it would seem inconceivable that even the most extensive experiments could ever change anything about the unwavering principles of mechanics, and anyway that type of experiment can always be rectified by experiments."

From what we said, those fears seem superfluous. The principles of dynamics initially seem to be experimental truths to us. However, we have been obliged to appeal to appeal to them as definitions. It is *by definition* that force is equal to the product of mass times acceleration. That is a principle that is henceforth placed beyond the reach of any ultimate experiment. Similarly, it is by definition that action is equal to reaction.

But then, one says, those unverifiable principles will be absolutely devoid of any significance. Experiments cannot contradict them, but they can give us nothing useful. Why should one study dynamics then?

That condemnation very rapidly proves to be unjustified. There are no *perfectly* isolated systems in nature, namely, ones that are perfectly abstracted from any external action. However, there are *almost* isolated systems.

If one observes such a system then one can study not only the relative motion of its various parts with respect to each other, but the motion of its center of gravity with respect to the other parts of the universe. One then confirms that the motion of that center of gravity is *almost* uniform and rectilinear, which conforms to Newton's third law.

That is an experimental truth, but it could be invalidated by experiment. What would we learn from a more precise experiment? It would tell us that the law was only approximately true; however, we knew that already.

One now explains how experiments can serve as the basis for the principles of mechanics and still never contradict them.

But let us return to Hertz's argument. The classical system is incomplete, because not all of the motions that are compatible with the principles of dynamics are realized in nature, or even realizable. Indeed, is it not obvious that the principle of areas and the motion of the center of gravity are not the only laws that govern natural phenomena? Undoubtedly, it would be unreasonable to demand that dynamics should embrace all of the laws of physics that were discovered or could be discovered in the same formula. However, it is no less true that one must regard a system of mechanics in which the principle of the conservation of energy is passed over in silence as incomplete and insufficient.

Hertz concluded:

> "It is true that our system embraces *all* natural motions, but at the same time, it embraces many *other ones* that are not natural. A system that excludes some of those motions would better conform to the nature of things and would consequently constitute an advance."

Such a thing would be, for example, the energetic system that we shall speak of later on, in which the fundamental principle of the conservation of energy is introduced quite naturally.

Perhaps one can very well understand what prevents one from quite simply adding that fundamental principle to the other principles of the classical system.

However, Hertz posed another question:

The classical system gives us an image of the external world. Is that image *simple*? Is one spared the existence of parasitic traits that are introduced arbitrarily along with the essential traits? Are the forces that we are led to introduce not truly useless gears that turn in a vacuum?

A piece of iron rests upon a table. An observer is not prevented from believing that since there is no motion, there is no force. How wrong he would be! Physics teaches us that every atom of iron is attracted by all of the other atoms of the universe. Moreover, each atom of iron is magnetic, and consequently subject to the action of all the magnets in the universe. All of the electric currents in the world also act upon that atom. (I shall overlook the electrostatic forces, molecular forces, etc.)

If one of those forces were to act alone then their action would be enormous; the piece of iron would shatter. Fortunately, they act together, and they counterbalance in such a way that nothing of sort happens. Our observer who sees only a piece of iron at rest will obviously conclude that those forces exist only in our imagination.

Undoubtedly, there is nothing absurd about any of those suppositions, but a system that eliminates them will be better than ours, by that fact alone.

It is impossible to not be struck by the scope of that objection. Moreover, in order to show that is it not purely artificial, it will suffice for me to recall the memory of a polemic that has existed for some years between two entirely eminent scholars – namely, Helmholtz and Bertrand – in regard to the mutual actions of currents. Bertrand, who sought to translate Helmholtz's theory into classical language, collided with some insoluble contradictions. Each element of current must be subject to a couple. However, a couple is composed of two parallel forces that are equal and oppositely directed. Bertrand calculated that each of those two components must be considerable and large enough to lead to the destruction of the wire, so he concluded that one must reject the theory. On the contrary, Helmholtz, who was an advocate of the energetic system, did not see any difficulty.

Therefore, according to Hertz, the classical system must be abandoned, because:

1. A good definition of force is impossible.

2. It is incomplete.

3. It introduces parasitic hypotheses, and those hypotheses can often generate purely artificial difficulties that are meanwhile large enough to impede even the best minds.

II. – THE ENERGETIC SYSTEM.

§ 1. – Various objections.

The energetic system was born as a result of the discovery of the principle of the conservation of energy. It was Helmholtz who gave it its definitive form.

One begins by defining two quantities that play the fundamental roles in that theory. Those two quantities are: On the one hand, the *kinetic energy*, or *vis viva*, and on the other hand, the *potential energy*.

All of the changes that bodies in nature can submit to are governed by two experimental laws:

1. The sum of the kinetic energy and the potential energy is constant. That is the principle of the conservation of energy.

2. If a system of bodies is in the situation *A* at the instant t_0 and in the situation *B* at the instant t_1 then it will always go from the first situation to the second one by a path such that the *mean* value of the difference between those two types of energy over the time interval that separates the two instants t_0 and t_1 is as small as possible.

That is Hamilton's principle, which is one form of the principle of least action.

The energetic theory presents the following advantages over the classical theory:

1. It is less incomplete: i.e., the principles of the conservation of energy and Hamilton's principle tell us more than the fundamental principles of the classical theory and exclude certain motions that nature does not realize that would be compatible with the classical theory.

2. It allows us to dispense with the hypothesis of atoms, which was almost impossible to avoid with the classical theory.

However, it raises some new difficulties, in turn. Before speaking of Hertz's objections, I would like to point out two that come to my mind:

The definitions of the two types of energy raise difficulties that are almost as great as the ones that are raised by force and mass in the former system. Meanwhile, one can infer those definitions more easily, at least in the simplest cases.

Suppose that an isolated system is composed of a certain number of material points. Suppose that those points are subject to forces that depend upon only their relative positions and the mutual separation distances but are independent of their velocities. By virtue of the principle of the conservation of energy, one must have a mass function.

In that simple case, the statement of the principle of conservation of energy is one of extreme simplicity. A certain quantity that is accessible to experiment must remain constant. That quantity is the sum of two terms: The first one depends upon only the positions of the material points and is independent of their velocities. The second one is proportional to the square of those velocities. That decomposition can be accomplished in only one way.

The first of those terms, which I will call U, will be the potential energy. The second one, which I will call T, will be the kinetic energy.

It is true that if $T + U$ is constant then the same thing will be true for an arbitrary function of $T + U$:

$$q\,(T + U)\,.$$

However, that function $q\,(T + U)$ will not be the sum of two terms, one of which is independent of velocities, and the other of which is proportional to the square of those velocities. Among the functions that remain constant, there is only one that enjoys that property: namely, $T + U$ (or a linear function of $T + U$, which will change nothing, since that linear function can always be reduced to $T + U$ by a change of unit and origin). That is what we shall call the energy. It is the first term that we shall call the kinetic energy and the second one that we shall call the potential energy. The definition of those two types of energy can them be pushed up to the limit with no ambiguity.

The same thing is true for the definition of mass. The kinetic energy – or *vis viva* – is expressed very simply with the aid of the masses and relative velocities of all material points with respect to each other. Those relative velocities are accessible to observation, and when we have an expression for the kinetic energy as a function of those relative velocities, the coefficients of that expression will give us the masses.

Hence, in that simple cases, one can define the fundamental notions with no difficulty. However, the difficulties will reappear in the most complicated cases, if, for example, the forces depend upon the velocities, instead of upon only the distances. For example, Weber supposed that the mutual action of two electric molecules depends upon not only the distance between them, but their velocities and accelerations. If the material points attract each other according to an analogous law then U will depend upon the velocity, and it can contain a term that is proportional to the square of the velocity.

Among the terms that are proportional to the squares of the velocities, how can one discern the ones that are provided by T or U ? How does one consequently distinguish the two types of energy?

But there is more: How does one define energy itself? We no longer have any reason to take $T + U$ to be the definition, rather than any other function of $T + U$, when the property that characterizes $T + U$ disappears, namely, that it is the sum of two terms of a particular form.

But that is not all: One must take into account not just the mechanical energy, properly speaking, but the other forms of energy, such as heat, chemical energy, electric energy, etc. The principle of the conservation of energy is then written:

$$T + U + Q = \text{const.}$$

in which T represents the observable kinetic energy, U represents the potential energy of position, which depends upon only the positions of the bodies, and Q is the internal molecular energy, which can take the form of thermal, chemical, or electrical energy.

Everything would be fine if those three terms were absolutely distinct, namely, if T were proportional to the square of the velocities, U were independent of those velocities and the state of the body, and Q were independent of the velocities and positions of the bodies, but dependent upon only their internal states.

The expression for energy could be decomposed into three terms of that form in only one way.

However, that is not the case. Consider the charged bodies: The electrostatic energy that is due to their mutual action will obviously depend upon their charges – i.e., upon their states. If those bodies are in motion then they will act upon each other electrodynamically, and the electrodynamical energy will depend upon not only their states and positions, but upon their velocities.

We would no longer have any means of sorting out the terms that must belong to T, U, and Q then, and thus to separate the three types of energy.

If $(T + U + Q)$ is constant then the same thing will be true for an arbitrary function:

$$\varphi(T + U + Q) .$$

If $T + U + Q$ has the special form that I envisioned above then no ambiguity will result. Among the functions $\varphi(T + U + Q)$ that remain constant, there will be only one of them that has the special form, and that will be the one that I agree to call energy.

However, as I said, that is not rigorously true. Among the functions that remain constant, there are none that can be put into that special form rigorously. Moreover, how does one choose the one that must be called energy? We no longer have anything that can guide us in our choice.

It only remains for us to state the principle of the conservation of energy: *There is something that remains constant.* In that form, it is, in turn, found to be beyond the scope of experiments and reduces to a sort of tautology. It is clear that if the world is governed by laws then there will have to be quantities that remain constant. Things like Newton's principles, and for an analogous reason, the principle of the conservation of energy, which are based upon experiments, can no longer be confirmed by them.

That discussion shows that one has made some progress upon passing from a classical system to an energetic system. However, at the same time, it shows that this progress is insufficient.

Another objection seems much more serious to me: The principle of least action applies to reversible phenomena. However, it is not remotely satisfied as far as irreversible phenomena are concerned. Helmholtz's attempt to extend to that class of phenomena did not succeed, nor can it succeed. Everything remains to be done in that respect.

There are other objections of an almost metaphysical order that Hertz developed at length.

If the energy is *materialized* – so to speak – then it must always be positive. Now, there are cases in which it difficult to avoid considering negative energy. For example, consider Jupiter orbiting around the Sun. The total energy will have the expression:

$$a\,v^2 - \frac{b}{r} + c,$$

in which a, b, c are three positive constants, v is Jupiter's velocity, and r is its distance to the Sun.

Since we can choose the constant c, we can suppose that it is large enough to make the energy positive. That is already something arbitrary that should come as a shock.

But there is more: Now, imagine that a celestial body of an enormous mass and an enormous velocity traverses the solar system. When it has passed through and once more gone out to an immense distance, the orbits of the planets will have been subjected to considerable perturbations. For example, we can imagine that the major axis of Jupiter has become very small, but that its orbit remains reasonably circular. No matter how big the constant *c* might be, if the new major axis is very small then the expression:

$$a\,v^2 - \frac{b}{r} + c$$

will become negative, and one will see the difficulty reappear that we believe to be obvious by giving a large value to *c*.

In summary, we cannot insure that energy will always remain positive.

On the other hand, in order to *materialize* the energy, one must *localize* it. For the kinetic energy, that is easy, but the same thing is not true for the potential energy. Where does one localize the potential energy that is due to the attraction of two stars? Is it in one of the two stars? Is it in both of them? Is it in the intermediate medium?

The statement of the principle of least action itself includes something that should come as a shock the senses. In order to go from one point to another, a material molecule that is free from the action of any force, but subject to move on a surface, must take a geodesic line; i.e., the shortest path.

That molecule seems to know the point where it must go to, predict the time that it will take to reach it by following this or that path, and then choose the most convenient path. That statement makes it sound, so to speak, like a free and animate being. It is clear that one would do better to replace it with a statement that is less shocking, and in which, as the philosophers would say, the final causes do not seem to substitute for the effective causes.

§ 2. – Boule's objection.

The final objection, which seems to be the one that is most striking to Hertz, is of a slightly different nature.

One knows what one calls a system with constraints. First imagine two points that are connected by a rigid link in such a fashion that their separation distance is always kept invariable, or, more generally, suppose that an arbitrary mechanism maintains a relation between the coordinates of two or more points of the system. That is the first type of constraint, which one calls a "solid constraint."

Now suppose that a sphere is constrained to roll on a plane. The velocity of the point of contact must be zero. We then have a second kind of constraint that is expressed by a relation that is no longer just between the coordinates of the various points of the system, but between their coordinates and velocities.

The systems in which there are constraints of the second kind enjoy a curious property that I would like to explain in the simple example that I just cited, namely, that of a ball that rolls on a horizontal plane.

Let *O* be a point in the horizontal plane, and let *C* be the center of the sphere.

In order to define the location of the moving sphere, I will take three fixed coordinate axes *Ox*, *Oy*, *Oz*, the first two of which are located in the horizontal plane upon which the sphere rolls, and three coordinate axes that are invariably coupled with the sphere $C\xi$, $C\eta$, and $C\zeta$.

The location of the sphere will be defined completely when one is given the two coordinates of the contact point and the nine direction cosines of the moving axes with respect to the fixed axes. Let *A* be a position of the sphere where the contact point is at *O*, the origin, and the moving axes are parallel to the fixed axes.

The coordinates of the contact point are:

$$x = 0, \quad y = 0,$$

and the nine direction cosines are:

$$\begin{matrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{matrix}$$

Give the sphere an infinitely small rotation ε around the axis $C\xi$. It will go to a position *B* where the contact point will become:

$$x = 0, \quad y = 0,$$

and the nine cosines will become:

$$\begin{matrix} 1 & 0 & 0 \\ 0 & \cos\varepsilon & \sin\varepsilon \\ 0 & -\sin\varepsilon & \cos\varepsilon \end{matrix}$$

However, that rotation is impossible, since it will make the sphere slide on the plane, not roll. It will then be impossible to pass from the position *A* to the infinitely close position *B directly*; i.e., by an infinitely small motion.

Nonetheless, we shall see that this passage can be made *indirectly;* i.e., by a finite motion.

Start from the position *A*. Roll the sphere on the plane in such a way that the instantaneous axis is situated in the horizontal plane and is parallel to the axis *Oy* at each instant, and stop when the axis $C\xi$ becomes vertical and parallel to *Oz*. One will arrive at a position *D* where the coordinates of the contact point have become:

$$x = \frac{\pi}{2} R, \qquad y = 0,$$

in which *R* is the radius of the sphere, and the nine cosines are:

$$\begin{matrix} 0 & 0 & -1 \\ 0 & 1 & 0 \\ +1 & 0 & 0 \end{matrix}$$

In the position *D*, the contact point is at the extremity of the axis $C\xi$, which is vertical.

Impart a rotation ε around the axis $C\xi$ to the sphere. That rotation is a pivoting around the vertical axis that passes through the contact point, so it will not include any sliding, and it will therefore be compatible with the constraints.

The sphere then goes to a position E where the coordinates of its contact point are:

$$x = \frac{\pi}{2} R, \qquad y = 0,$$

and the nine cosines are:

$$\begin{matrix} 0 & 0 & -1 \\ \sin \varepsilon & \cos \varepsilon & 0 \\ \cos \varepsilon & -\sin \varepsilon & 0 \end{matrix}$$

Now, roll the sphere in such a fashion that the instantaneous axis of rotation remains constantly parallel to Oy, and consequently the contact will always be along the axis Ox. Stop when the contact point has returned to the origin O. It is easy to see that we have arrived at the position B.

One can then go from the position A to the position B by passing through the intermediate positions D and E.

Hertz called the systems such that if the constraints did not permit one to pass directly from a certain position to another infinitely close position then they would no longer permit one to pass from one to the other indirectly *holonomic*. Those are the systems where there are only solid constraints.

One sees that our sphere is not a holonomic system.

Now, it can happen that the principle of least action is not applicable to the non-holonomic systems.

Indeed, one can pass from the position A to the position B by the path that I just described, and without a doubt by many other paths. Among all of those paths there is obviously one of them that corresponds to an action that is smaller than all of the other ones. The sphere must then be able to follow it in order to go from A to B. Nothing of the sort is true. No matter what the initial conditions of motion are, the sphere will never go from A to B.

There is more: If the sphere effectively goes from the position A to another position A' then it does not always take the path that corresponds to the minimum action.

The principle of least action is no longer true.

As Hertz said:

> "In this case, a sphere that obeys that principle would seem to be a living being that consciously pursues a definite goal, while a sphere that follows the laws of nature will present the impression of being an inanimate mass that rolls uniformly… However, one says, such constraints do not exist in nature. The so-called rolling without slipping is only rolling with a small amount of slipping. That phenomenon enters the realm of irreversible phenomena such as friction, which are still poorly understood, and to which we further *do not know* how to apply the principles of mechanics."
>
> "I respond: Rolling without slipping is not contrary to either the principle of energy or any of the known laws of physics. That phenomenon

> can be realized in the observable world to such an approximation that one can appeal to it in order to construct the most delicate integration machines (e.g., planimeters, harmonic analyzers, etc.) We have no right to exclude it as impossible. However, it will be and it can be realized only to a degree of approximation such that the difficulties still do not disappear. In order to adopt a principle, we must demand that when it is applied to a problem whose givens are approximately exact, it should also give results that are approximately exact. Furthermore, the other constraints – viz., the solid constraints – are also realized only approximately in nature; nonetheless, one does not exclude them…"

III. – THE HERTZIAN SYSTEM.

Now here is the system that Hertz proposed to substitute for the two theories that he criticized. That system is based upon the following hypotheses:

1. There are only systems with constraints in nature, which are free from the action of any external force.

2. If certain bodies seem to obey some forces then that is because they are *coupled* by other bodies that are invisible to us.

Meanwhile, a material point that seems free to us does not described a rectilinear trajectory. The old mechanicians said that it deviated from such a trajectory because it was subject to a force. Hertz said that it deviated because it was not free but was coupled to other invisible points.

That hypothesis seems strange at first: Why introduce hypothetical invisible bodies, along with the visible ones? However, Hertz responded, the two old theories were likewise obliged to suppose who-knows-what sort of invisible entities, along with the visible ones. The classical theory introduced forces, and the energetic theory introduced energy, but those invisible entities of force and energy have an unknown and mysterious nature. On the contrary, the hypothetical entities that I imagine have entirely same nature as the visible bodies.

Is that not simpler and more natural?

We can argue that point and maintain that the entities in the old theories must be retained precisely because of their mysterious nature. To respect that mystery is to confess our ignorance, and since the fact of our ignorance is certain, would it not be better to acknowledge it than to deny it?

But let us move on, and see what Hertz inferred from his hypotheses.

The motions of systems with constraints in the absence of external forces are governed by a unique law.

Among the motions that are compatible with the constraints, the one that is realized will be the one that is such that the sum of the masses times the square of their accelerations is a minimum.

That principle is equivalent to that of least action when the system is holonomic, but it is more general, because it also applies to non-holonomic systems.

In order to better explain the scope of that principle, take a simple example: namely, that of a point that is constrained to move on a surface. Here, we have only one material point. The acceleration must then be a minimum. In order for that to be true, it is necessary for the tangential acceleration to be zero. Now, that acceleration is equal to dv / dt, where v is the velocity, and t is the time, so v is a constant, and the motion of the point is uniform. Moreover, it is necessary that the normal acceleration should be a minimum. Now, it is equal to v^2 / ρ, where ρ is the radius of curvature of the trajectory, or to $v^2 / (R \cos \varphi)$, where R is the radius of curvature of the normal section to the surface, and φ is the angle between the osculating plane to the trajectory and the normal to the surface.

Now, the magnitude and direction of the velocity is supposed to be known. Therefore, v and R will be known.

It will then be necessary to have $\cos \varphi = 1$; i.e., the osculating plane must be normal to the surface. That is, the moving point must describe a geodesic line.

In order to now understand how one can explain the motion of systems that *seem* to be subject to forces to us, I shall once more take a simple example, namely, that of the governor. That apparatus is known to consist of an articulated parallelogram *ABCD*. The opposite angles *B* and *D* of that parallelogram carry balls whose mass is appreciable. The upper angle *A* is fixed. The lower angle *C* carries a ring that can slide along a fixed vertical rod *AX*. The entire apparatus is animated with a rapid rotational motion around the rod *AX*. A link *T* is suspended from the ring *C*.

The centrifugal force tends to move the balls apart, and consequently to raise the ring *C* and the link *T*. The link *T* is then subject to a traction that becomes greater as the rotation becomes more rapid.

Now suppose that an observes sees only that link and imagines that the balls, the rod *AX*, and the parallelogram are made of a material that is invisible to him. That observer will confirm that traction is exerted upon the link *T*. However, since he will not see the organs that produced it, he will attribute a mysterious cause – say, a "force" – to the attraction that is experienced by the point *A* on the link.

Indeed, according to Hertz, whenever we imagine a force, we are being duped by an analogous illusion.

That raises the question: Can one imagine an articulated system that imitates a system of forces that is defined by an arbitrary law or approaches one as close as one desires? The response must be in the affirmative. I shall be content to recall a theorem of Koenigs that can serve as the basis for a proof. This is the theorem: One can always imagine an articulated system such that a point of that system describes a curve or an arbitrary algebraic surface, or more generally, one can imagine an articulated system such that by virtue of its constraints, the coordinates of the various points of the system are subject to some given, but arbitrary, algebraic relations.

Nonetheless, the hypotheses to which one will be led can be very complicated.

That was not the first attempt that was made along those lines, moreover. It is impossible to not compare Hertz's hypotheses with Lord Kelvin's theory of gyrostatic elasticity.

As one knows, Lord Kelvin sought to explain the properties of the ether without introducing any forces. He even gave a definitive form to his hypothesis and represented the ether by a mechanical model that was like that of the English magnet. The English scholars, who were satisfied to give a body to their ideas in order to make it tangible, were not frightened by the complexity of models in which one has a multiplicity of links, rods, and slides, as in the mechanic's workshop.

To give some idea of that, let me describe the model that represented the gyrostatic ether. The ether is composed of a sort of mesh. Each intersection in that mesh is a tetrahedron. Each of the edges of the tetrahedron is composed of two rods, one of which is solid and the other of which is hollow, and the former slides inside the latter. That edge is then extensible, but not flexible.

At each intersection, one finds an apparatus that is composed of three rods that are coupled to each other invariably and form a tri-rectangular trihedron. Each of those three rods is supported by two opposite edges of the tetrahedron. Finally, each of them carries four gyroscopes.

In the system that I just described, there is no potential energy, but only kinetic energy, namely, that of the tetrahedra and the gyroscopes. Meanwhile, a medium thus constituted will behave like an elastic medium. It will transmit transverse undulations absolutely like the ether.

I shall add something more: One can not only imitate all forces that are found in nature with articulated systems of that type that contain gyroscopes, but also imitate some other ones that nature has not realized. That was precisely the goal that Lord Kelvin proposed to attain. He wished to explain certain properties of the ether that the usual hypotheses seemed incapable of accounting for.

One knows that the axis of the gyroscope tends to preserve a fixed direction in space. When it deviates from it, it tends to return as if it were acted upon by a guiding force. Unlike the real forces, the apparent force that tends to maintain the direction of the gyroscope is not counterbalanced by an equal and opposite reaction. It is thus liberated from the law of action and reaction and from its consequences, such as the law of areas, to which the natural forces are subject.

One then agrees that the gyrostatic hypothesis, in which one is freed from that restrictive rule, has accounted for some fact that could not be explained by the usual hypotheses upon which it rests.

What must one ultimately think of Hertz's theory? It is certainly interesting, but I do not find it entirely satisfying, because it places too much weight upon hypothesis.

Hertz is protected from some of the objections that have tormented us; he does not seem to have dismissed all of them.

The difficulties that we discussed at length at the beginning of this article can be summarized as follows:

One can present the principles of dynamics in two ways. However, one can never sufficiently distinguish what is a definition, what is an experimental truth, and what is a mathematical theorem. In the Hertzian system, the distinction is still not perfectly clear, and a fourth element is introduced, moreover: viz., hypothesis.

Nevertheless, that mode of exposition is useful due to the fact that it is new: It forces us to reflect and to free ourselves from the old ways of associating ideas. We still cannot see the monument in its entirety, but it is worth something to have a new perspective and to take a new viewpoint.

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