# HERTZ'S IDEAS ON MECHANICS

by
**Henri Poincaré**
(Translation and Foreword by Nicolae Mazilu)

## FOREWORD

Rarely, if ever, was the human spirit under a closer critical scrutiny than in the following masterpiece of the great scientist of the 19th and 20th centuries, Henri Poincaré. The work itself is seldom cited. Yet, the reader can find in it all the objections that can be raised against the main scientific inventions of the human spirit. They are still valid today, exactly as they were more than a century ago, or three centuries ago, for that matter.

It is, first and foremost, advisable to pay close attention to the definition of central forces as given by Poincaré. Like all of the classics of science, he understood them with a string attached: *their magnitude should depend only on the distance between points*. Einstein himself used the definition of central forces in that connotation when he judged the whole system of the classical mechanics and introduced the general relativity. However, the very first definition of the central forces, as it appears in Newton's *Principia*, doesn't ask anything of the kind. What can we say, but repeat with Nietzsche: the first reaction is usually the right one!

It is also advisable to pay attention to the critique of the concept of energy: it stands even today as it was then, in this work of Poincaré. Yet, in spite of the overwhelming cases against energy, the theoretical physics doesn't seem to stop speculating upon the kinds of energy that might exist in the world. Finally, it is worth paying attention to the criticism of the way in which Hertz assigns matter through a hypothesis: it seems like the hypothesis of missing mass of today.

It is our conviction that this masterpiece is not quite known to the English speaking readers. This is why we undertook here the burden of its translation. We hope to give it another chance, in order to have, at least nowadays, more than a century from its first publication, the impact it deserves on the human spirit.

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In the year 1890 the great electrician Hertz reached the apogee of his glory; all of the Academies of Europe rewarded him with the awards at their disposal. The entire world hoped that he would still have many years of life and that they should be just as brilliant as those of his debut.

Unfortunately, the malady he acquired so prematurely already got to him and started immediately to slow down, until almost complete halt, his experimental activity. He barely had time to install his new lab in Bonn; all kinds of ailing deprived him, and us, of the discoveries he promised.

However, he would still serve the physical sciences, by the enormous influence he exerted, by advices given to his students; nevertheless this period is only marked by a single personal discovery, of primeval importance, is true, namely that of the transparence of aluminum to cathodic rays.

But, if he was so brutally turned from the studies so precious to him, he didn't go however inactive; if the senses betrayed him, the intelligence remained and he would use it to profound reflections on the philosophy of Mechanics. The results of these reflections were published in a posthumous work, and I want to summarize and discuss them here briefly.

Hertz criticizes first the two principal systems proposed until now, which I'll call the classical system and the energetic system, and proposes a third system which I'll call the Hertzian system.

## I – THE CLASSICAL SYSTEM

1. *The definition of force* – The first tentative of coordination of the mechanical facts is that which I will call the *classical system*; this is, says Hertz, «the great royal way, whose main stations carry the names of Archimedes, Galilei, Newton and Lagrange.

»The fundamental notions we find as points of depart are those of *space*, *time*, *force* and *mass*. The force in this system is taken as a cause of motion; it exists before the motion and is independent of it».

I'll try to explain now why Hertz was unsatisfied with this manner of considering the things.

First, we have the difficulties met when we want to define the fundamental notions. What is *the mass*? It is the product between volume and density, says Newton. – It would be better to say that the density is the ratio between mass and volume, answer Thomson and Tait. – What is *the force*? It is, answers Lagrange, a cause producing or tending to produce the motion of a body. – It is, would Kirchhoff say, the product between mass and *acceleration*. But then why wouldn't we say that the mass is the ratio between force and acceleration?

These difficulties are inextricable.

When we say that the force is the cause of a motion, we do metaphysics, and if we should limit ourselves to this, the definition is absolutely sterile. In order that a definition could serve to something, it must teach us *to measure* the force; this would be entirely sufficient, we wouldn't need it to teach us what is the force *in itself*, nor that it is cause or effect of the motion.

Therefore, we first ought to define the equality of two forces. When will we say that two forces are equal? When applied to the same mass, one answers, they will imprint the same acceleration or, when opposed directly to one another, they are in equilibrium.

However, this definition is not here but only to bedazzle us. One cannot take a force, applied to a body, to attach it to another body, like one would take, for instance, the locomotive from a train in order to attach it to another train. It is therefore impossible to know what acceleration such and such force, applied to such and such body, would impart to another body, *if* applied to that body. It is impossible to know how two forces, which are not directly opposed to one another, would behave *in case* they are directly opposed.

This definition is the one we try to materialize, so to speak, when we measure the force with a dynamometer or when we equilibrate it with a weight. Two forces F and F′, which for simplicity I assume vertical and oriented upward, are applied to two bodies C and C′ respectively; I suspend the same heavy body P, first to C, then to C′; if the equilibrium takes place in the two cases, I'll conclude that the two forces F and F′ are equal to each other, because they are equal with the weight of the body P.

But am I sure that the body P maintained its weight when moved from the first to the second body? Far from this, *I am rather sure of the contrary*; for I know that the intensity of weight varies from a point to another, and that it is for instance higher to the pole than to the equator. The difference is, obviously, very small and practically I wouldn't even care of it; but a good definition ought to have mathematical rigor, and this rigor doesn't exist here. What I just said about weight applies obviously to the force of the spring of a dynamometer, which the temperature and a host of other circumstances can prompt to vary.

And this is not all of it yet; one cannot say that the weight of body P was applied to body C and equilibrates the force F. What is actually applied to the body C is the action A of the body P on body C; the body P is in turn acted upon by its weight, and on the other hand by the reaction of body C on P. After all, the force F is equal to force A, for they are in equilibrium: the force A is equal to R by the principle of equality of action and reaction; finally, the force R is equal to the weight of body P, for they are in equilibrium. Only from these three equalities are we inferring the equality of F with the weight of P as a consequence.

We are therefore required to introduce in the definition of the equality of two forces the very principle of the equality of action and reaction; *based on this, that principle can no more be considered as an experimental law, but only as a definition.*

Here we are, therefore, in possession of two rules of recognizing the equality of two forces: the equality of two forces equilibrating each other; the equality of action and reaction. But, as we have just shown above, these two rules are insufficient; we are therefore compelled to appeal to a third rule, and admit that certain forces, like for instance the weight of a body, are constant in magnitude and direction. However, as we said, this third rule is an experimental law: it is only approximately true; *it is a bad definition*.

We are therefore down to the definition of Kirchhoff: *the force equals mass times acceleration*. This «law of Newton» ceases in turn to be considered as an experimental law, it is nothing more than a definition. But even this definition is still insufficient, for we don't know what's mass. It allows us to calculate the ratio of two forces applied to the same body at different times, no doubt; however, it doesn't teach us anything about the ratio of these forces in case they are applied to different bodies.

In order to round it up, we must appeal again to the third of Newton's laws (the equality of action and reaction), taken however not as an experimental law but as a definition. Two bodies A and B act on each other; the acceleration of A times mass of A is equal to the action of B on A; similarly, the product of the acceleration of B with its mass is equal with the reaction of A on B. As, by definition, the action is equal to the reaction, the masses of A and B are in inverse ratio with the accelerations of the two bodies. Here is, therefore, the ratio of the two masses defined, and it remains for experience to verify that this ratio is constant.

This would work very well, if the two bodies A and B would be alone present, and withdrawn to the action of the rest of the world. But it is not at all so; the acceleration of A is not exclusively due to the action of B, but to the action of a host of other bodies C, D… In order to apply the preceding law, we need to decompose the acceleration of A in many components and then find which one of these components is due to the action of B.

This decomposition would still be possible *should we admit* that the action of C on A simply adds to that of B on A, without the presence of C modifying the action of B on A, or the presence of B modifying the action of C on A; consequently, if we would admit that two arbitrary bodies attract each other, that their mutual action is oriented along the line joining them and does not depend but on the distance between them; in a word, if we admit *the hypothesis of central forces*.

It is well known that for the evaluation of masses of celestial bodies we make use of an entirely different principle. The law of gravitation teaches us that the attraction of two bodies is proportional to their masses; if r is the distance between them, m and m' are their masses and k a constant, their attraction will be $k \cdot m \cdot m'/r^2$.

What one measures then is not the mass, as ratio between force and acceleration, but the attracting mass; it is not the inertia of the body but its attractive power.

We have here an indirect procedure, whose use is not theoretically compulsory. It could very well be that the attraction is inversely proportional with the square of distance, without being proportional though with the product of masses, i.e. it could be equal to $f/r^2$, but without having though

$$f = k \cdot m \cdot m'$$

Even so, we would still be capable of measuring the masses of these bodies by observing their *relative* motions.

However, do we have the right to admit the hypothesis of central forces? Is this hypothesis rigorously exact? Is it sure that it will not be contradicted by experience? Who would venture to answer in the affirmative? And yet, if we are to abandon this hypothesis, the whole edifice, so laboriously erected, will collapse.

We have therefore no right to talk of the component of acceleration of A due to the action of B. There is no mean to discern it from that due to the action of C or of any other body. The rule of measuring the masses becomes inapplicable.

What is left then from the principle of equality of action and reaction? If the hypothesis of central forces is rejected, this principle must then be expressed in the following way: the geometrical resultant of all forces applied to different bodies of a system withdrawn from any external action will be zero. In other words, *the motion of the center of gravity of this system will be rectilinear and uniform*.

Here is, apparently, a means to define the mass; the position of the center of gravity depends obviously on the values attributed to the masses; we should dispose of those values in such a way that the motion of the center of gravity is rectilinear and uniform; this will always be possible if the third law of Newton is true, and will not be possible but in one single way.

Nevertheless, there is no system withdrawn to any external action; all the parts of the Universe suffer more or less the action of all the other parts. *The law of the motion of the center of gravity is not rigorously valid but when applied to the entire Universe*.

But then, in order to be capable to extract the value of the masses, we ought to be able to observe the motion of the center of gravity of the Universe. The absurdity of this conclusion is obvious; we don't know but relative motions; the motion of the center of gravity of the Universe remains for us an eternal unknown.

We are left therefore with nothing, and our efforts were unfruitful; we are compelled to adopt the following definition, which is nothing else but a confession of incapacity: *the masses are coefficients convenient to introduce in calculations*.

We will be able to redo the whole Mechanics, by attributing to all the masses different values. This new Mechanics will not be in contradiction with experience nor will it be contradicting the general principles of Dynamics (the principle of inertia, the proportionality of the forces with masses and with accelerations, equality of action and reaction, rectilinear and uniform motion of the center of gravity, the principle of areas).

Only, the equations of this new Mechanics will be *less simple*. Let's understand this well: only the first terms will be simpler, i.e. the ones we already know from experience; it would be possible that, by altering the masses by small quantities, the complete equation neither gain nor drop anything from their simplicity.

I insisted on this discussion longer than Hertz himself; I meant to show though that Hertz didn't simply look for quarrel with Galilei and Newton; we must agree to the conclusion that in the framework of the classical system *it is impossible to give a satisfactory idea for force and mass*.

2. *Different objections*. – Hertz asks himself, further, if the principles of Mechanics are rigorously true. «In the opinion of many physicists, he says, will appear as inconceivable that the remotest experience could ever change something from the undestructible principle of Mechanics; yet, what comes out of experience can always be rectified by experience.»

After what we just have discussed such a fear seems superfluous. The principles of Dynamics appear to us first as experimental truths; nevertheless we were compelled to use them as of definitions. *By definition* the force is equal with the product of the mass and acceleration; here is, therefore, a principle that from this moment on is placed out of reach of any upcoming experience. Similarly, only *by definition* the action is equal to the reaction.

But, can one say, these unverifiable principles are absolutely empty of any meaning; the experience cannot contradict them; and they cannot teach us anything beneficial; why then study the Dynamics?

This expedite condemnation would be unjust. There is in Nature no system *perfectly* isolated, perfectly withdrawn to any external action; there are *nearly* isolated systems though.

If one observes such a system, then one can study not only the relative motion of its different parts one with respect to another, but also the motion of its center of gravity with respect to other parts of the Universe. One can ascertain then that the motion of this center of gravity is almost rectilinear and uniform, according to the the third law of Newton.

We have here an experimental truth, but it will not be possible to be invalidated by experience; for, what could tell us a more precise experience, indeed? It could tell us that the law is almost exact; but this we already knew.

*Now it is obviously explainable why the experience could serve as the basis of the principles of Mechanics, and yet it will never be capable of contradicting them.*

But let's come back to Hertz's argument. The classical system is incomplete because all of the motions compatible with the principles of Mechanics are not materialized in Nature, they are not even realizable. Indeed, it is obvious that the principles of areas and of the motion of center of gravity are not the only laws that regulate the natural phenomena. No doubt, it would be unreasonable to ask Dynamics to comprise in the same formula all the laws that Physics ever discovered or will be capable of discovering. Nevertheless, it is no less true the fact that we will need to consider as incomplete and insufficient a system of Mechanics in which the principle of conservation of energy is passed by in silence.

«Our system, concludes Hertz, comprises indeed *all* of the natural motions, but at the same time it comprises still many others that are not natural. A system that will exclude a part of these motions will be much more in agreement with the nature of things, and consequently will represent a progress». Thus, for instance, will be the energetic system, to be discussed hereinafter, in which the fundamental principle of conservation of energy is introduced quite naturally.

Perhaps it is not well understood what specifically deters the sheer annexation of this principle to the other principles of the classical system.

But Hertz asks himself still another question:

The classical system gives us an image of the external world. Is this a *simple* image? have we gotten rid in it of the spurious features, arbitrarily introduced along with the essential ones? Aren't the forces we are compelled to introduce genuine fruitless gears, gyrating in vain?

On this table there is a bar of iron; an uninfluenced observer will think that because there is no motion there is no force. How deceived will he be! The physics teaches us that every atom of iron is attracted by all the other atoms in the Universe. Moreover, every atom of iron is magnetic, and consequently is submitted to the action of all the magnets in the Universe. All of the electrical currents in the world act upon this atom. I leave aside the electrostatic forces, the molecular forces, etc.

If just a few of these forces are the only ones acting, their action will be enormous; the iron will fly apart into pieces. Fortunately they all act and counterbalance, so that nothing happens. Our uninfluenced observer, who doesn't see but only a thing, an iron bar at rest, will obviously conclude that all these forces do not exist but in our imagination.

No doubt, these assumptions don't have anything of absurd in themselves, but a system that makes a clean sweep of them will be, by this very thing, better than our system.

It's impossible that this objection doesn't strike us. Besides, in order to show that it is not merely artificial, it is sufficient for me to recall a polemics that took place a few years ago between two eminent scientists, von Helmholtz and Mr. Bertrand, in connection with the mutual actions of the currents. Trying to translate in a classical language the theory of von Helmholtz, Mr. Bertrand encountered unfathomable contradictions. Every element of current would need to be submitted to a couple of forces; but a couple of forces is composed of two parallel, equal and opposite forces. Mr. Bertrand has calculated that every one of those two component forces must be considerable, big enough to be able to destroy the wire, and concluded that the theory must be rejected. On the contrary von Helmholtz, partisan of the energetic system, would not see here any difficulty.

Thus, according to Hertz, the classical system should be abandoned:

1º because a good definition of force is impossible;

2º because it is incomplete;

3º because it introduces spurious assumptions and these can often times generate difficulties, purely artificial, but big enough though, in order to stop even the most exquisite spirits.

## II – The Energetic System

1. *Different objections* – The energetic system was born after the discovery of the principle of conservation of energy. Von Helmholtz gave it the definitive form.

One starts by definition of two quantities playing the fundamental part in this theory. These are: on one hand, the *kinetic energy* or the living force; on the other hand the *potential energy*.

All the changes that bodies from nature can undergo are governed by two fundamental laws.

1º The sum of kinetic energy and potential energy is a constant. This is the principle of conservation of energy.

2º If a system of bodies is in the situation A at the time $t_0$ and in situation B at the time $t_1$, it passes always from the first situation to the second in a way for which the mean value of the difference between the two kinds of energies in the time interval separating the moments $t_0$ and $t_1$, is the least possible.

This is the Hamilton principle, one of the forms of the principle of the least action.

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

1º It is less incomplete; i.e. the principles of the conservation of energy and of Hamilton teach us the fundamental principles of the classical theory, and additionally they exclude certain motions which are not materialized in Nature, but are nevertheless compatible with the classical theory;

2º It releases us from the hypothesis of atoms, almost impossible to avoid with the classical theory.

But it brings out in turn new difficulties; before discussing the objections of Hertz I will make reference to two of these that come now to my mind:

The definitions of the two kinds of energy uncover difficulties almost as great as those of the force and mass from the first system. However, we get on with it, at least in the simplest of the cases.

Let's admit an isolated system formed out of a certain number of material points; assume that these material points are submitted to some forces that don't depend but on their relative position and the mutual distances, but are independent of their velocities. By virtue of the principle of conservation of energy, there must exist here a function of the forces.

In this case the statement of the principle of conservation of energy is of an extreme simplicity. A certain quantity, accessible to experiment, must be constant. This quantity is the sum of two terms: the first one depends only on the positions of the material points but is independent of their velocities; the second one is proportional to the square of these velocities. This decomposition cannot be done but in one way

The first of these terms, which I'll call U, will be the potential energy; the second one, which I'll call T, will be the kinetic energy.

It is true that T + U is a constant, but the same we can say about some function of T + U, φ(T + U).

But this function φ(T + U) will be no more the sum of two terms, one independent of velocities, the other proportional with the square of these velocities. Among the functions that remain constants there is but one that bears this property, i.e. T + U (or a linear function of T + U, which does not change anything inasmuch as it can be reduced to T + U by a change of units and origin). This is then what we call energy; its first term will be called potential energy and the second will be called kinetic energy. The definition of the two kinds of energy can then be pushed through without any ambiguity.

The same goes for the definition of masses. The kinetic energy, or the living force, is formulated very simply with the help of masses and the velocities of all material points with respect to one of them. These relative velocities are accessible to observation, and when we have the expression of kinetic energy as a function of them, the coefficients of this expression will give us the masses.

Therefore, in this simple case one can define the fundamental notions without any difficulty. But the difficulties come back in more complicated cases, for instance if forces, instead of depending only on distances, depend also on velocities. For example, Weber assumes that the reciprocal action of two electric molecules depends not only on the distance between them, but also on their velocity and acceleration. Should the material points attract each other by a similar law, then U would depend on velocity, and could even contain a term proportional to the square of velocity.

Then how are we to discern, among the terms proportional with the square of velocity, those of T from those of U? Therefore, how are we to distinguish the two parts of the energy?

Even more, how do we define the energy itself? We have no reason to take as definition T + U instead of any other function of T + U, when the property characterizing T + U of being the sum of two terms of particular form disappeared.

And this is not even all of it, because we will have to account not only for the mechanical energy proper, but also for some other forms of energy, heat, chemical energy, electric energy etc. The principle of conservation of energy must then be written

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

where T would represent the sensible kinetic energy, U the potential energy, depending only on the positions of the bodies, Q the internal molecular energy in thermal, chemical or electric forms.

Everything would go just fine should these terms be perfectly distinct, i.e. should T be proportional to the square of velocities, should U be independent of these velocities and of the state of bodies, should Q be independent of the velocities and positions of the bodies and depend only on their internal state.

The expression of energy could then be decomposed in just one single way into three terms of this form.

But it is not so; consider some electrified bodies: the electrostatic energy due to their reciprocal action will obviously depend on their charge, i.e. on their state; but it will depend equally well on positions. If these bodies are in motion, they will interact electrodynamically and the electrodynamic energy will depend not only on their state and positions, but also on their velocities.

We have therefore no means to select the terms which belong to T, to U and to Q, in order to separate the three parts of the energy.

If (T + U + Q) is constant, so is some function

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

Should (T + U + Q) be of the particular form mentioned above, we would have no ambiguity; among the functions φ(T + U + Q) which are constants wouldn't exist but only one of this particular form, and that I would agree to call energy.

But, as I said, it is not rigorously so; among the constant functions there is none which can be rigorously put in this particular form; besides, how are we to choose from among them the one that we must call energy? We have nothing that could guide us in our choice.

We are left with only one statement for the principle of conservation of energy: *there is something that remains constant*. In this form it can find itself, in turn, outside experience and reduces to some kind of tautology. For, it is clear that if the world is governed by laws, there will be quantities that will remain constants. Like Newton's principles, and for a similar reason, the principle of the conservation of energy, based on experience, cannot be invalidated by it.

This discussion shows that passing from the classical system to the energetic one, a progress is indeed achieved; but at the same time it shows that this progress is quite insufficient.

Another objection seems to me even more severe; the principle of the least action is applicable to reversible phenomena; but it is no more satisfactory when it comes to irreversible phenomena; the tentative of von Helmholtz of extending it to this kind of phenomena has not succeeded, and it couldn't succeed; in this respect everything remains yet to be done.

There are still other objections, of almost metaphysical order, on which Hertz expounds most.

If the energy *is materialized*, so to speak, it should remain always positive. However, there are cases where it is difficult to avoid contemplation of the negative energy. Consider, for instance, Jupiter revolving around the Sun; its energy has as expression $av^2 - b/r + c$, where a, b, c are three positive constants, v is the velocity of Jupiter and r is its distance from the Sun.

Because we dispose of the constant c, we can assume that it is big enough so that the energy is positive; we already have here an arbitrary that shocks the spirit.

But this is not all of it. Imagine now that a celestial body of an enormous mass and with enormous speed passes through the solar system; after it would have been passed and would have departed again to an immense distance, the orbits of the planets would have suffered considerable perturbations. We can imagine, for instance, that the major axis of the Jupiter orbit became much smaller, but that this orbit would have remained sensibly circular. No matter how big the constant c, if the new axis is very small, the expression $av^2 - b/r + c$ would have become negative, and we will have the occasion to see resurfacing the difficulty we thought to avoid by giving c a great value.

Summarizing, we cannot ensure that the energy will be always positive.

On the other hand, in order to materialize the energy, we need to localize it; as concerns the kinetic energy this is easy to do, but it is not quite so easy for the potential energy. Where do you localize the potential energy due to the attraction of two heavenly bodies? In one of the two? In both of them? In the intermediate medium?

The statement of the principle of minimum action itself has in it something that shocks the spirit. In order to reach from one point to another, a material molecule, withdrawn to the action of any force but compelled to move on a surface, will follow the geodesic line, i.e. the shortest path.

This molecule seems to know the point in which we want to bring it, to predict the time it will take to arrival following such and such paths, and then to choose the most convenient path. The statement presents it as a being alive and free, so to speak. It is clear that we would want to replace it by something rather less shocking, in which, like the philosophers would say, the final caused don't seem to substitute themselves for the efficient causes.

2. *The objection of ball* ([1]) – The last objection, which seems to be the one that impressed Hertz most, is of a little different nature.

It is known what a system with constraints is; let's imagine first two points connected by a rigid triangle, in such a way that their distance apart is maintained invariable; or, more general, let's assume that some mechanism maintains a relation among the coordinates of two or more points of a system. We have here a first kind of constraint, called «solid constraint».

Assume now, that a sphere would be constrained to roll on a plane. The speed of the contact point must be zero; we have therefore a second kind of constraints, expressed by a relationship not only between the coordinates of the different points of the system, but between these coordinates and the velocities of points.

The systems in which there are constraints of this second kind have a queer property, which I'll try to explain through the simple example I just cited above, that of a ball rolling over a horizontal plane.

Let O be a point of the horizontal plane and C the center of the ball.

In order to better define the situation of the mobile sphere, I'll take three fixed coordinates, Ox, Oy and Oz, the first two of them being situated in the horizontal plane over which the ball rolls; I'll take also three coordinate axes invariable connected to the sphere, $C\xi$, $C\eta$ and $C\zeta$.

The situation of sphere will thus be completely defined when one will give the coordinates of the contact point and the nine direction cosines of the mobile axes with

[1] *See Notes to the Principles of Analytical Mechanics*

respect to the fixed axes. Let A be a position of the sphere for which the point of contact is in origin and the mobile axes are parallel with the fixed axes.

The coordinates of the contact point are

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

and the nine direction cosines are:

$$\begin{matrix} 1, & 0, & 0; \\ 0, & 1, & 0; \\ 0, & 0, & 1. \end{matrix}$$

Let's give now to sphere an infinitely small rotation ε around the axis Cξ; it will come now in a position B, in which the coordinates of the contact point are

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

and the nine direction cosines become

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

However, this rotation is impossible, because it would make the sphere glide on the plane without rolling. This rotation is therefore impossible, because it would make the sphere slip without rolling. It is therefore impossible to pass from the position A to the infinitely neighboring position B directly, i.e. by an infinitely small motion.

Let's show that this passage can be achieved *indirectly*, i.e. by a finite motion.

Let's start from the position A. Make the sphere roll on the plane in such a way that the instantaneous rotation axis is situated in the horizontal plane, is at all times parallel to Oy, and stop when the axis Cξ would become vertical and parallel to Oz. We will reach a position D, where the coordinates of the contact point are

$$x = \pi/2\cdot R,\ \ y = 0.$$

and the nine cosines will be

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

In position D, the point of contact is at the extremity of axis Cξ which is now vertical.

Let's give sphere a rotation ε around the axis Cξ; this rotation is a swing around the vertical axis passing through the contact point, it doesn't use any skidding and is therefore compatible with the constraints. The sphere has come then in a position E where the coordinates of the contact are

$$x = \pi/2\cdot R,\ \ y = 0$$

and the cosines are

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

Let's make now the sphere roll in such a way that the instantaneous rotation axis remains constantly parallel to Oy, and consequently the contact takes always place along the axis Ox. Stop then when the contact point reached again the origin. It is easy to see that we have arrived in position B.

One can thus arrive from position A to position B, passing through the positions D and E.

Hertz calls holonomic the systems for which, if the constraints do not allow a direct passage from a position to another neighboring one, they don't allow an indirect passage either. These are the systems for which there are but solid constraints.

One can see therefore that our sphere is not a holonomic system.

One can reach this way the conclusion that the principle of minimum action is not applicable to unholonomic systems.

One can indeed pass from the position A to position B the way we just indicated, and without any doubt many other ways; among these there is one corresponding evidently to an action smaller than the other ones; the sphere would therefore be capable to follow it in order to reach from A to B; but it is not so; no matter of the initial conditions of the motion, the sphere would never go from A to B.

Moreover, if the sphere goes effectively from position A in other position A', it will never take the way that corresponds to the minimum action.

The principle of minimum action is not true anymore.

«In this case, says Hertz, a sphere which would obey this principle, would resemble a living being following conscientiously a designated purpose, while a sphere following the law of Nature offers the image of an inanimate mass rolling uniformly… But, one would reply, that such constraints don't exist in the Nature; this alleged rolling without slipping is but a rolling with small slipping. This phenomenon falls among the irreversible phenomena, like the friction, still less known, and to which we don't know yet to apply the true principles of Mechanics».

«A rolling without skidding, we answer, is neither contrary to the principle of energy, nor to any other known law of Physics; this phenomenon can be materialized in the visible world with such a great approximation, that we can use it in order to build integration machines among the most delicate ones (planimeters, harmonic analyzers, etc.) We have no right to exclude it as impossible; however, being approximate and accomplished but only approximately, still doesn't solve the difficulties. In order to adopt a principle we must ask that, when applied to a problem whose data are approximately exact, give results that are approximately exact too. As a matter of fact, the other constraints, the solid ones, are also only approximately accomplished in Nature; however, we don't exclude them… »

## III – The Hertzian System

Here is now the system that Hertz proposes to substitute for the two theories he criticizes. This system is based on the following hypotheses:

1° There are not in Nature but only systems with constraints, withdrawn to the action of any external force;

2° If certain bodies seem to be submitted to forces, is because they are *connected* to other bodies which, for us, are invisible.

A material point which seems free does not describe however a rectilinear trajectory; the ancient mechanicians would say that it departs from rectilinear because it is submitted to a force; Hertz says that it departs because it is not free, but connected to other invisible points.

This hypothesis seems strange at the first sight: why introduce besides visible bodies also hypothetical invisible bodies? But, answers Hertz, the two theories are equally compelled to assume besides visible bodies, some invisible entities; The classical theory introduces forces, the energetic theory introduces energy; and these invisible entities, force and energy are of an unknown and mysterious nature: the hypothetical entities which I imagine here are, on the contrary, of the same nature as the visible bodies.

Isn't this simpler and more natural?

One can discuss over this point and uphold that the entities of the old theories must be retained especially because of their mysterious nature. Respecting this myster appears as a confession of ignorance; and because our ignorance isn't sure, isn't it better to admitting it rather than to hiding it?

But let's get over this, and proceed to see what conclusion draws Hertz from his hypotheses.

The motions of the systems with constraints, without external force, are governed by a unique law.

Among the motions compatible with the constraints, the one which will be materialized is that for which the sum of the masses multiplied by the square of the accelerations is minimal.

This principle is equivalent to that of the minimum action when the system is holonomic, but is more general, inasmuch as it also applies to the nonholonomic systems.

In order to realize the impact of this principle, let's take a simple example: that of a point compelled to move on a surface. The acceleration must therefore be minimum; this acceleration is equal to $dv/dt$, $v$ being the velocity and $t$ the time; therefore $v$ is constant and the motion of the point is uniform; more than this, it is necessary that the normal acceleration be minimum; but this is equal to $v^2/\rho$, $\rho$ being the radius of curvature of the trajectory, or to $v^2/(R\cdot\cos\varphi)$, $R$ being the radius of curvature of the normal section of the surface, and $\varphi$ the angle between the osculating plane of the trajectory and the normal to surface.

But the velocity is assumed known, both in magnitude and direction. Therefore $v$ and $R$ are known.

Therefore we must have $\cos\varphi = 1$, i.e. the osculating plane must be normal to surface; in other words, the point in motion describes a geodesic line.

In order to understand now how the motion of the systems *that seem* to be submitted to forces can be explained, I'll take again a simple example, the one of the regulator with balls. This well known device consists of a hinged parallelogram ABCD: the vertices B and D of this parallelogram carry balls of a significant mass; the superior vertex A is fixed; the inferior vertex C carries a ring which can glide along a fixed vertical rod AX; the whole device is driven by a rapid rotation around the rod AX. Of the ring C a control lever T is hung.

The centrifugal force tends to pull the balls off and consequently to raise the ring C and the control lever T. This lever is therefore subjected to a traction which is harder the faster the rotation.

Assume now, an observer who sees only this lever, and imagine that the balls, the rod AX, the parallelogram are made of a matter which is invisible for him. That observer will notice the traction exerted upon lever; but, as he will not see the organs producing it, he will attribute it to a mysterious cause, to a «force», to an attraction exerted by the point A upon lever.

Well, according to Hertz, any time we imagine a force we are duped by an analogous illusion.

Then a question is raised: can we imagine a hinged system imitating a system of forces, defined by a certain law or approximating it as close as we need? The answer must be affirmative; I limit myself to recalling a theorem of Mr. Kœnigs that could serve as basis to a demonstration. Here is the theorem: *One can always imagine a hinged system, such that a point of this system describes a curve or an arbitrary algebraical surface; or, more generally, one can imagine a hinged system such that, by the virtue of its constraints, the coordinates of the different points of the system are submitted to some arbitrary given algebraical relations*.

Only, the hypotheses to which we are led could be very complicated.

As a matter of fact, this would not be the first tentative made along these lines. It's impossible not to see here closeness between the hypotheses of Hertz and the theory of Lord Kelvin on the gyrostatic elasticity.

It is known that Lord Kelvin tried to explain the properties of ether without making any force intervene. He even gave a definitive form to his hypothesis, representing the ether by one of these mechanical models, as the Englishmen call them. Satisfied if they materialized their ideas, if they made them palpable, the English scientists are not afraid of the complication of these models whereby they multiply the levers, the tillers, the guideways as in a mechanical shop.

In order to give an idea along these lines, let's describe the model representing gyrostatic ether. The ether would be formed of a kind of lattice. Each cell of this lattice is a tetrahedron. Each one of the edges of this tetrahedron is made of two rods, one solid and one hollow, gliding into one another; this edge is therefore extensible but not flexible.

In each cell there is a device formed of three lines fixed with respect to one another and forming a right trihedron. Any one of these rods rests upon two of the opposite edges of the tetrahedron; finally, each one of them carries four gyroscopes.

In the system we just described there is no potential energy, but only kinetic energy, the one of tetrahedrons and the one of gyroscopes. However, a medium thus constituted will behave like an elastic medium; it will transmit the transversal oscillations exactly like the ether.

I shall add one more thing: with hinged systems of this kind, containing gyroscopes, one can not only imitate all the forces we find in Nature, but even other which Nature will not know to materialize; this is precisely the aim that lord Kelvin took; he wanted to explain certain properties of ether, which the common hypotheses seemed incapable to explain.

It is known that the gyroscope axis tends to preserve a fixed direction in space; when pulled away it tends to come back to it as if it is acted on by a directing force. This apparent force, tending to maintain the direction of gyroscope, is not counterbalanced by a reaction equal and contrary like the real forces. It is therefore liberated from the law of action and reaction, as well as from its consequences, like the law of areas, to which the natural forces are submitted.

We can think therefore that, only to the extent to which it is liberated from this restrictive rule, the gyrostatic hypothesis explained facts impossible to explain by the usual hypotheses obeying that rule.

Taking all in all, what are we to think about the theory of Hertz? Interesting, to be sure, but it doesn't satisfy me completely, because it leaves too much room for hypothesis.

Hertz took shelter from a few of the objections that worn him out; but he doesn't seem to have removed them all.

The difficulties discussed at length at the beginning of this article could be summarized the following way:

The principles of Dynamics have been expounded in many ways; but never was it sufficiently distinguished what is definition, what is experimental truth, what is mathematical theorem. In the Hertzian system the distinction is not yet perfect, and moreover, a fourth element is introduced: the hypothesis. However, by the sheer fact that it is new, this manner of presentation is useful: it compels us to reflect, to break loose from the old associations of ideas. We cannot see the entire monument yet: this would mean a new perspective, from an entirely new point of view.