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On the principles of mechanics and the mechanical explanation for natural phenomena ([1])

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I.

At the end of the Eighteenth Century, the principles of mechanics seemed to be above all criticism, and the work of the founders of the science of motion defined a body of work that one believed would survive the test of time. Since that era, a penetrating analysis has examined the foundations of that edifice with a magnifying glass. Indeed, whereas Lagrange and Laplace found all of the simple difficulties, today we have encountered some more serious ones. Everyone that has taught the elements of mechanics (to the extent that they could think for themselves) has sensed that many of the more or less traditional expositions of the principles are incoherent. Too often, they have been impregnated with the dualism between force and matter that was introduced into the old mechanics. Force seemed to be a special agency that was the cause of the motion of matter. Many illustrious physicists have wished to break from the old customs. While abandoning the historical viewpoint on the development of science completely, they have adopted a viewpoint that is analogous to that of geometers and constructed a geometry that starts from a certain number of axioms. Their method is therefore *deductive*. Such a way of proceeding has its advantages and disadvantages. The advantages are that the exposition will have perfect clarity and that the system will follow in logical sequence. One thus constructs a set of representations from all of its pieces *a priori* and infers all possible consequences from them. It is only when the exposition of the system is complete that one can compare the results with experiments. That manner of proceeding is obviously quite philosophical. It neatly asserts, in some way, from the outset that the only goal of science is the search for a system of images that are can make correspond to reality without making any pretense of having actually attained it. Moreover, one immediately understands that this system of images is not necessarily unique, and that one can adopt many others. However, here we shall also touch upon the disadvantages of that deductive path, at least, from the standpoint of teaching methodology. It does not show how one might be led to frame the construction, and in that sense, it is not satisfying to the mind. The same difficulty obviously does not present itself in

([1]) **Hertz**, *Die Prinzipien der Mechanik*, Leipzig, 1894. **Boltzmann**, *Vorlesungen ueber die Principe der Mechanik*, Leipzig, 1897. **Picard**, “Une première leçon de Dynamique,” Ens. math. (1900). **Boltzmann**, “Ueber die Grundprincipien und Grundgleichungen der Mechanik,” talk given at Clark University, 1900.

geometry, in which the postulates have a much more intuitive character and relate to common experience.

One way of constructing mechanics in the deductive manner was presented to us in Boltzmann's lectures on the principle of mechanics. One poses the existence of a certain number of material points and a series of postulates on the motion of those points is formulated. The acceleration at each point is the sum of $n - 1$ partial accelerations that point along the lines that connect the point in question to the other $n - 1$. Moreover, when one considers the various points, those partial accelerations have pair-wise opposite senses and constant ratios, and a unique system of ratios can be adopted for the various points. Finally, they depend upon simply the distance between the two corresponding points. One sees that one can even expand the scope of such a system of postulates somewhat, since the introduction of notions of mass and force presents no difficulty. A system of differential equations with a well-defined form is thus found to have been established. Various arbitrary functions will enter into it. One will have to see whether one can choose them for those categories of phenomena in such a way that the facts will agree with those differential equations, and whether one can then predict the motions that correspond to certain initial givens. If that is true then one will be in possession of a system of images like the ones that I spoke of above. One will have everything that one must seek for a mechanical explanation of phenomena of that kind that Kirchhoff demanded. Later on, we shall try to find out whether the latter viewpoint, which is excellent in its own right, must not nonetheless be stated with some reservations, and whether it would not be likely to obstruct the advancement of science if it were formulated in too absolute of a manner.

Another type of construction of mechanics in the deductive manner was presented by Hertz's treatise. One knows that the great physicist, who was taken too soon from science following his immortal discoveries on the propagation of waves, dedicated the last year of his life to collecting his thoughts on mechanics. Hertz's system departed considerably from the traditional conventions of the preceding systems. Some purely kinematical notion must first be recalled. If one considers a system of points that are affected with certain coefficients that will become masses later on then it will be easy to define what one means by the *length* of an elementary displacement of that system of points, as well as by the *direction* and *curvature* of that displacement. One might have certain constraints between the points of the system. When they are independent of time and refer to only the relative positions of the different points, one says that the system is *free*. With Hertz, we shall once more call an elementary displacement *the shortest line* when it is a displacement that has a curvature that is less than any other possible elementary displacement in the same direction, and finally call a path whose elements are all the straightest elements *the straightest path*. With those definitions, we can state the fundamental postulate upon which all of Hertz's mechanics is based: A free system will remain at rest or describe a uniform trajectory that is the straightest path. One can give another form to that principle by saying that during a real motion, at each moment, the sum of the accelerations of the points of the system, multiplied by their masses, will be a minimum among all possible motions that start from the same position with the same velocity, and that recalls a famous theorem by Gauss. For Hertz, any system in nature is a free system or part of a free system to which one can apply the fundamental principle. Meanwhile, there are systems that appear free to us, even though we can certainly not apply the principle to them. In regard to them, Hertz responded that other than the visible motion, there are hidden motions in such a way that the combination of the visible motion and the hidden motions actually forms a free system. The necessity of introducing hidden masses in certain cases seems singular at first. In reality, that introduction is very familiar to the physicist. The ether, which plays such a significant role in physics, is a hidden mass, and certain vibratory motions of ponderable matter are themselves hidden motions. One nevertheless understands that the indeterminacy that persists in the introduction of hidden masses must make the application of Hertz's ideas singularly difficult, and even in some very simple cases. That is a serious reproach against them, but if one overlooks it then one can only admire the beauty of what that great physicist has constructed. It is largely suggestive and *constitutes a vast program* for mechanics and physics to implement. One must not be surprised that force plays no role in all of that. Things could not be otherwise in the deductive method where only the laws of motion are posed. Force can appear only in the form of a certain analytical expression. That is what happens when one considers a free system that can decompose into two parts. One will then be led to imagine the action of one of the parts on the other and conversely. One will then obtain directly-opposite actions and reactions. That recalls Newton's classical postulate of the equality of action and reaction, but it is important to remark that the law that Newton stated has a more general character than the one that is deduced from Hertz's principles. Any comparison would be somewhat questionable, and the fact that it would not be entirely exact would make that point sufficiently clear. For Hertz, the only forces are, in some way, contact actions. Therefore, in mechanics, there are only actions and reactions that are applied to the same points. On the contrary, when Newton considered the action and reaction of the Sun and a planet, the two forces are applied to two different points. The mechanics of the physicist from Bonn knows nothing of such forces, and his principles can apply to that case only when some hypothesis has been made regarding the nature of constraint between two stars. Furthermore, Newton's principle, in its absolutely general form, is less than clear, and its meaning is very doubtful in more than one example that touches upon magnetism and electricity.

The principle of the conservation of energy that is defined by the sum of the products of the masses with the squares of the velocity is an immediate consequence of Hertz's fundamental postulate for free systems, and one will also recover it in its conventional form for a part of a free system that is regarded as subject to certain forces. Hertz made a deep study of cyclic motions and conservative motions that was inspired by the earlier research of Helmholtz. Consider a system that is composed of visible and hidden masses. Under the condition that the latter must form what Hertz called an *adiabatic cyclic system*, the original system is called *conservative*. One knows that one often distinguishes between kinetic energy and potential energy. For a conservative system, kinetic energy is the energy of the visible masses that is defined as it was above, and potential energy is nothing but the kinetic energy of the hidden masses. Those two energies are no different in their nature. The basic distinction is artificial and depends upon the extent of our knowledge. One then sees how profound Hertz's speculations are then. Those general viewpoints can be regarded as defining what one must mean by a mechanical explanation. As for the delicate question of knowing whether any phenomenon is susceptible to a mechanical explanation, I will soon say a word about that.

II.

The *deductive* ways of presenting things, as they are presented in one form or another, are quite seductive initially. They condense the results of the succession of efforts and fumbling attempts on the part of the creators of the science of motion into a few postulates that one formulates from the outset. Those postulates have an extremely general character, and one resorts to experiments in order to verify their more or less distant consequences. Nonetheless, one must recognize that, apart from them, some other ways of presenting things have undoubtedly been preferred for some time now that are closer to the historical order of events. It is not that they do not present great difficulties or that the traditional form must be modified considerably. However, what one fears is that they will preserve a character that is half-inductive and half-deductive that gives little unity to them. The beautiful exposition that Boussinesq made in 1889 in his remarkable *Leçons synthétiques de Mécanique générale* deserves to be cited. However, it seems to me that it was written for people who already had some knowledge of mechanics. Of course, it is hard to speak about mechanics to listeners that are completely ignorant of that science; one must cheat a little (if I dare say so). Any exposition will attract criticism to itself at some point. Please permit me to say a few words about a manner of presentation that I have had occasion to point out without trying to create any illusions regarding its weak points.

A principle like the principle of inertia is, in reality, only a definition, and when one statically measures force with a spring or dynamometer, one must also regard Newton's principle of the equality of action and reaction as a definition. Galileo's experiments related to dynamics in a constant field. Moreover, they postulated absolute time and space, and if the Earth turned around its axis under some other conditions then it would be much more difficult to construct a theory of dynamics. One can conclude from Galileo's experimental results that the principle of the independence of the effect of the field on the motion that had been previously acquired is true for a constant field. As for the general principle of the independence of the effects of the forces, one can state it only with certain reservations because, as Poincaré remarked, the fields of forces can sometimes influence each other. Under a succession of constant fields and for the same material point, forces can be defined by accelerations from the dynamical viewpoint and with a dynamometer statically. It is only experiments that can teach us whether those two definitions will give proportional numbers. A comparison of material elements, i.e., a definition of mass, can be deduced from the fundamental experiment that showed that all bodies fall in the same way under the same constant field and using static measurements of force. Finally, one passes on to variable forces by the usual limiting process in mathematics, and one will then get the fundamental equation of dynamics. One must nonetheless understand its precise significance quite well and not waste words: That equation is interesting in each case only to the extent that preliminary observations or experiments, whose results one generalizes by a type of induction, have given some information about the nature of force.

Without a doubt, when the fundamentals of mechanics are presented in that way or something similar to it, they will present a mixture of postulates and experiments that are more or less precise, along with a bit of anthropomorphism. However, there are some inconveniences that we can hardly avoid if we would like to account for the main points in the historical path of science. Let us not pretend that this is pointless. In geometry, as we say, we do not begin by describing the observations and experiments that were made by prehistoric human beings and that this was the origin of the postulates of geometry. We might respond that in the science of space, those experiments are, for one reason or another, so simple that each of them can be done without thinking today, whereas the same thing is not true in mechanics, where things are otherwise complex. It is obviously possible to proceed in mechanics as one does in geometry. We have seen that in the expositions that I have called *deductive*, like Hertz's system. However, some difficulties of a different nature will present themselves, and the fundamental postulates that are posed at the outset will seem singular in comparison to the ones that one is stating for the first time. I do not know what the future holds. Perhaps science will go down some paths that we cannot predict, but there is reason to hope that the study of some of the principles that are getting so much attention today will arrive at some important results. Some scholars think that the first chapter of dynamics, as we presently construe it, and I speak of the dynamics of material points, must probably disappear. That is certainly possible, but I do not believe that will happen in the near future because the atomic hypotheses will still (and perhaps always) play a predominant role in many branches of science. However, as we know, several viewpoints that are quite different can be maintained simultaneously, but I would depart from my present topic by belaboring that thought.

III.

There was an idea that was dear to the Cartesians that all of the transformations of the physical world take place according to the laws of mechanics. What is the exact meaning of that statement, assuming that it even has one? The answer to that question is not simple and can present some degree of indeterminacy. What does one mean by a mechanical explanation for a phenomenon? For Hertz, a phenomenon that is exhibited by a system will be susceptible to a mechanical explanation if the system belongs to a conveniently-chosen free system and its motion can be deduced from the fundamental postulates that were indicated above. Helmholtz and Poincaré ([1]) adopted a slightly different form. It relates to the classical system of Lagrange equations in rational mechanics. That system includes independent functions of the parameters and their derivatives. If one can choose them in such a fashion that the Lagrange differential equations will then correspond to the motions of the system then one will have a mechanical explanation for those motions. Such responses will remain quite abstract and vague if one does not make them a bit more precise. Indeed, it is impossible to obtain indeterminate functions and to form differential equations as a result of a succession of inductions that are based upon some more or less plausible generalizations of simple experiments that do not ultimately provide any indispensable information. To what extent is it currently precise to say, as one sometimes does, that a mechanical explanation is nothing but a system of differential equations? Once one has obtained them, one can abandon the framework that allowed one to construct the system and employ the resources of mathematical analysis in order to infer from that system how one might coordinate the known facts and make some predictions, which is the supreme goal of the theory and the hallmark of its fecundity. However, it often happens that some new fact will show the inadequacy of the explanation that was adopted.

([1]) **H. Poincaré**, *Thermodynamique*, 1892 (in the Preface).

One then completes it by adding differential relations to some term, and it is more often necessary to inspect the original framework once more in order to make the corrections to the defining construction useful. If one can then agree that the final form of a theory consists of a system of differential equations then it will nonetheless be indispensable for one to not forget the ideas that served to define it.

Let us finally return to the question: Does every phenomenon admit a mechanical explanation? Answering a question that is posed in such a general manner will be complicated, if not impossible. Opinions on it are divided, and it is above all in the study of caloric phenomena that opinions seem to diverge the most. As one knows, the mechanical explanation for Carnot's principle presents great difficulties. Clausius was the first to attempt such an explanation, and then Helmholtz thought that he had succeeded in that attempt in his memorable research into the principle of least action. After him, Boltzmann sought to eliminate certain objections that Helmholtz had raised. Helmholtz's essential idea consisted of the hypothesis of hidden motions. According to him, the variables could be divided into two categories: The ones that are accessible and the other ones that are unknown to us and correspond to hidden motions. Upon making certain hypotheses, one will arrive at some differential relations for the accessible variables that have a completely different form from the equations of classical mechanics and that is how one can account for the dissipation of energy. Boltzmann, who seemed to have posed the question much more precisely than Helmholtz, made a distinction between ordered and disordered motion. For him, an increase in entropy corresponded to an increase in the disordered motion in comparison to the ordered motions. One knows that Poincaré, at the end of his *Thermodynamique*, considered Helmholtz's attempts to be insufficient and preferred to think that there are some phenomena that do not admit mechanical explanations. For my own part, I am hesitant to formulate a response to a question that seems too vague to me. Why must one suppose that there is any other mechanical explanation than one that falls within the purview of the Lagrange equations? Cannot one adopt a more comprehensive viewpoint, and are we not in danger of degenerating into a war of words then?

We have just assumed a viewpoint that is analytical and abstract. When dealing with the same class of ideas, one can sometimes give those considerations a more concrete form. Suppose that two different phenomena lead to the same system of differential relations. They will both be models then, and there can be several models for the same category of phenomena. We point out, in a general manner, that the images of things that our mind forms are models for those things. Therefore, in a system where there are hidden masses, i.e., ones that are inaccessible to observation, we can do nothing but create models for them that are not essentially capable of achieving a statement of reality. To that way of thinking, the agreement between mind and nature is comparable to the agreement between two systems that are models for each other.

It then seems that everyone is free to look for different models. It is even true that there is a certain degree of variety in the models by reason of the indeterminacy in the problem itself, but the history of science has nonetheless shown that this variety is quite limited. Indeed, it is necessary that our models should be *simple*, and while remaining within the realm of pure mechanisms, we will always have a tendency to return to those atomic and molecular concepts that have played a fundamental role in Nineteenth-Century physics. I shall leave the circle of ideas that Hertz developed in his so curious and so suggestive book and whose analysis has been my principal objective by extending some mechanical representations to that question. I will say only a few words in conclusion about a very special representation that is very dear to the English school ([1]), in which the model is constructed from the most commonplace mechanisms. Maxwell constructed an ingenious apparatus that exhibited various analogies with electrical phenomena, and in which, for example, induction seems to be due to the inertia of certain masses. Above all, Lord Kelvin went very far down that path. As one knows, with the use of rigid solids, he succeeded in realizing some elastic effects with the help of rotational motions and constructing some singular representations of the ether. The extreme complexity of some of those models shocked the minds that were accustomed to seeing things from an analytical viewpoint. It is clear that if one were to make any pretense of knowing about reality in that way then it would have to be something very strange. From the moment that one deals with only images, it will come as no surprise that the various opinions will differ in regard to the degree of simplicity of this order that representation. Lord Kelvin's bell ringers (*les renvois de sonnette*) have their own philosophy.

([1]) On this topic, *see* an extremely interesting article by Duhem, "L'École anglaise et les theories physiques," in the Revue de questions scientifiques (1893).