

RELATIONS BETWEEN EXPERIMENTAL PHYSICS AND MATHEMATICAL PHYSICS.¹

RÔLE OF EXPERIMENT AND OF GENERALISATION.

EXPERIMENT is the sole source of truth: this alone can teach us something new; this alone can give us certainty. These two points no one may question.

But if experiment is all, what place is there for mathematical physics? What has experimental physics to do with such an auxiliary which seems useless and even perhaps dangerous? Nevertheless mathematical physics exists, and has been of undeniable service; this fact needs explanation.

Observation is not sufficient; use must be made of our observations, and for that generalisation is necessary. This has always been done; but man profiting from past errors, has observed more and more and generalised less and less. Each century has scoffed at the preceding, accusing it of generalising too boldly and too naïvely. Descartes commiserated the Ionians; Descartes in his turn makes us smile; without doubt our sons will some day laugh at us. Is there no way to get at the gist of the matter at once and escape the raillery that we foresee? May we not be content with experiment alone?

No, that is impossible and would be misunderstanding completely the true character of science. The savant must work with

¹ Written in 1900 and delivered before the International Congress of Physics, in Paris. Translated by George K. Burgess, Docteur de l'Université de Paris, Instructor in Physics, University of California.

method; science is made of facts as a house of stones; but an accumulation of facts is no more a science than a pile of stones a house. Above all the scientist must foresee. Carlyle says: "The fact alone matters; John Lackland passed by here, that is what is admirable, here is a reality for the which I would give all the theories in the world." Carlyle was a compatriot of Bacon; like him he desired to proclaim the cult *for the God of Things as they are*, but Bacon would not have said that. It is the language of the historian. Most likely the physicist would have said: "John Lackland passed by here; never mind, for he will not pass this way again."

We all know that there are good experiments and poor ones. The latter accumulate in vain; whether there are a hundred or a thousand, a single piece of work by a real master, a Pasteur for instance, suffices to make them fall into obscurity. This, Bacon would have well understood; is it not he who invented the expression *experimentum crucis*? But Carlyle would not have understood it. A fact is a fact; a student has read a number on his thermometer, taking no precautions; no matter, he has read it, and if it is the fact only that counts, this is a reality of the same degree as the wanderings of John Lackland. What then is a good experiment? It is one which teaches us something more than an isolated fact; it aids us to predict, and enables us to generalise.

Without generalisation, prediction is impossible. The circumstances under which one has operated will never be simultaneously reproduced. The observed fact can never be realised again; the only thing that can be affirmed, is that under analogous circumstances, an analogous fact will be produced. In order to predict it is necessary to invoke analogy, that is, to generalise.

However timid one may be, it is necessary to interpolate; experiment gives us only a certain number of isolated points, which must be united by a continuous line; this is a true generalisation. But one does more, the curve so traced will pass between and near these points; but not through them. So that one is not limited to the generalisation of the experiment, he corrects it; and the physicist who would abstain from these corrections and content himself

solely with experiment would be forced to announce the most extraordinary laws. The detached facts are not enough; that is why we must have Science ordered, or better, organised.

It is often said that we must experiment with no preconceived idea. That is not possible; not only would this render sterile every experiment, but even if we wanted to do so, it could not be done. Every one has within him his idea of the world, which cannot be so easily put aside. For example, we have to make use of language, which is made up necessarily of preconceived ideas. Such ideas unconsciously held are the most dangerous of all.

Shall we say that if we cause to intervene others of which we have full consciousness, we shall but aggravate the evil? I do not think so; I believe rather that they will act as mutual counterweights, I was going to say antidotes, that in general will accord poorly and even conflict with each other forcing us to look at things from different aspects. This is enough to free us: he who can choose his master is no longer a slave.

Thus, thanks to generalisation, each observed fact enables us to predict a great number of others; but we must not forget that the first alone is certain and the others merely probable. However well founded a prediction may seem, we are never *absolutely* sure that experiment will not prove it false, if we undertake to verify it. But the probability of truth is often so great that practically we may be content with it. Better is it to predict without certainty than never to have predicted at all.

We should never disdain to make a verification when the occasion presents itself. But every experiment is long and difficult, the workers are few, and the quantity of facts that we need to predict is immense; beside this mass, the number of direct verifications that we can make will ever be a negligible quantity. Of this little that we may directly reach, we must select the better part; it is necessary that each experiment should allow the greatest possible number of predictions having the highest degree of probability. The problem is, so to speak, to increase the efficiency of the scientific machine.

Allow me to compare science to a library which must increase

indefinitely ; the librarian has at his disposal for purchases but limited funds, which must not be wasted. It is experimental physics that is charged with the buying ; she alone can enrich the library. As for mathematical physics, her mission is to make the catalogue ; if this is well made, the library will not be richer ; but it may aid the reader to make use of these riches. Also by showing the librarian the gaps in his collections, it will aid him to make judicious use of his funds ; which is the more important as the funds are quite inadequate.

Such is the rôle of mathematical physics ; she should direct generalisation so as to augment what I have just called the efficiency of Science. By what means she accomplishes this, and how she may do so without danger, that is what we shall examine.

THE UNITY OF NATURE.

We observe in the first place that every generalisation supposes in a certain measure the belief in the unity and in the simplicity of nature. In the case of unity there can be no difficulty. If the different parts of the universe were not as the organs of the same body, they would not react on each other, they would ignore each other mutually ; and we in particular could know but one part. Consequently we have not to ask ourselves if nature is one, but how she is one.

As to the second point, all is not so clear. It is not certain that nature is simple. Can we without danger act as if she were so?

There was a time when the simplicity of Mariotte's law was an argument presented in favor of its exactness, when Fresnel himself, after having said, in conversation with Laplace, that nature did not occupy herself with analytical difficulties, was obliged to explain his words, so as not to offend the current public opinion. To-day ideas have changed much ; nevertheless those who do not believe that natural laws must be simple, are still often obliged to act as if they so believed. They cannot separate themselves entirely from this appearance without rendering impossible all generalisation and consequently all science.

It is clear that any fact soever may be generalised in an infinite number of ways, and one must choose among them. The choice will be determined by considerations of simplicity. Take the case of interpolation. We draw a line as regularly as possible among the points given by observation. Why do we avoid the discordant points, the too sharp inflections? Why do we not describe a curve having the most capricious zigzags? It is because we know beforehand, or we think we know, that the law to be expressed cannot be as complicated as that. Jupiter's mass may be deduced either from the movements of his satellites, from the perturbations of the greater planets, or from those of the lesser planets. If the averages of the determinations obtained by these methods are taken, we find three numbers nearly but not quite identical. This result might be interpreted by supposing that the gravitation constant is not the same in the three cases; the observations would be certainly much better represented. Why do we reject this interpretation? Not because it is absurd but that it is uselessly complicated. It will not be accepted until it is forced upon us, and that day is not yet.

To resume, every law is reputed simple until proved otherwise.

This custom is forced upon physicists by the reasons that I have indicated; but how justify it in the presence of discoveries that daily show us new details richer and more complex? How reconcile it even with the unity of nature? For if all things are interdependent, the relations in which so many different objects intermingle cannot be simple.

If we study the history of science, we see produced two phenomena that are, so to speak, the inverse of each other: on the one hand there is a simplicity hidden under complex appearances, on the other hand an apparent simplicity conceals extremely complex realities.

What is more complicated than the troubled movements of the planets, what more simple than Newton's law? There, nature playing, as Fresnel said, with the analytical difficulties, employs but simple means and engenders by their combination I know not

what tangled snarl. Here is a case of hidden simplicity,—one which must be unravelled.

Examples of the other kind abound. In the kinetic theory of gases, we consider the molecules animated with great velocities, whose paths, deformed by incessant impacts, have the most capricious shapes, and cross space in all directions. The observable result is the simple law of Mariotte; each individual fact was complicated; the law of great numbers has re-established simplicity in the mean. Here the simplicity is only apparent, and the coarseness of our senses alone prevents us from perceiving the complexity.

Many phenomena obey a law of proportionality; but why? Because in these phenomena there is something which is very small. The simple law observed is then but a translation of this general analytical rule, according to which the infinitely small increment of a function is proportional to the increment of the variable. Since in reality the increments are not infinitely small, but very small, the proportionality law is but approximate and the simplicity is but apparent. What I have said applies to the law of the superposition of small movements, whose use is so fruitful and which is the basis of optics.

And Newton's law itself? Its simplicity, so long hidden, is perhaps only apparent. Who knows if it is not due to some complicated mechanism, to the impact of some subtle matter animated with irregular movements, and if it has not become simple merely by the play of averages and of large numbers? In any case it is difficult not to suppose that the true law contains supplementary terms, which may become sensible at small distances. If in astronomy they are negligible in comparison with Newton's expression, and if the law becomes thus simplified, this is merely on account of the enormity of the celestial distances.

Without doubt, if our means of investigation became more and more penetrating, we should discover the simple within the complex, then the complex from the simple, then again the simple within the complex, and so on, without being able to predict which would be the last term. It is necessary to stop somewhere, and

for science to be possible, we must stop where we have found simplicity. That is the only foundation upon which we can construct the edifice of our generalisations. But, the simplicity being only apparent, will this foundation be solid enough? That is what is to be studied.

For this, let us see what rôle our generalisations play in the belief in simplicity. We have verified a simple law in a considerable number of particular cases; we refuse to admit that this occurrence, so often repeated, is a result of mere chance, and we conclude that the law must be true in the general case.

Kepler finds that the positions of a planet observed by Tycho are all on the same ellipse. He has not for a single instant the thought that, by a singular chance, Tycho never regarded the heavens but at the moment when the true trajectory of the planet happened to cut this ellipse.

What does it matter then if the simplicity is real, or if it conceals a complex truth? Whether it be due to the influence of large numbers which level individual differences, or to the greatness or smallness of certain quantities which allow of neglecting certain terms, in no case is it due to chance. This simplicity, real or apparent, has always a cause. We may then reason in the same way at all times, and if a simple law has been observed in several particular cases, we may legitimately suppose that it will still be true in analogous cases. To refuse to so consider the matter would be to attribute an inadmissible rôle to chance.

Nevertheless there is a difference. If the simplicity was real and profound, it would bear the test of the increasing precision of our methods of measurement; if then we believe nature to be profoundly simple, we must conclude that it is an approximate and not a rigorous simplicity. This was formerly done; but this is what we no longer have the right to do.

The simplicity of Kepler's laws, for example, is only apparent. This does not prevent their being applied, almost exactly, to all systems analogous to the solar system, but it prevents their being rigorously exact.

THE RÔLE OF HYPOTHESIS.

Every generalisation is a hypothesis ; the hypothesis has then a necessary rôle that no one has ever contested. But it should always, as soon and as often as possible, be submitted to verification. It is evident, that if it does not stand this test, it must be thrown aside without regret. This is what is usually done, but sometimes with impatience.

This impatience, however, is not justifiable ; the physicist who has just renounced one of his hypotheses should be glad, on the contrary, for he has just found an unhopèd-for occasion of discovery. His hypothesis, I imagine, had not been lightly adopted ; it took account of all the known factors which seemed to be able to intervene in the phenomenon. If the verification is not made, it is because there is something unexpected, something extraordinary ; we are on the point of finding something unknown.

Has the hypothesis so rejected been sterile ? Far from it. One may even say that it has rendered more service than a true hypothesis ; not only has it been the occasion of a decisive experiment, but if the experiment had been made by chance, without the existence of the hypothesis, nothing would have been inferred ; nothing extraordinary would have been seen ; merely one fact more would have been catalogued without deducing the least consequence.

Now under what conditions is the use of hypothesis without danger ?

The firm purpose to submit all to experiment does not suffice ; there are still hypotheses that are dangerous ; they are in the first place and above all those that are tacit and unconscious. Since we make them without knowing it, we are powerless to abandon them. Here again is a service that mathematical physics may render. By the precision proper to it, we are obliged to formulate all the hypotheses that we should make without this aid, but without being aware of their existence.

Note, besides, that it is important not to multiply our hypotheses too fast, but to make them only one after another. If we con-

struct a theory founded on multiple hypotheses, and if experiment condemns it, which among our premises must we change? It is impossible to know. And conversely, if the experiment succeeds, are we to think all the hypotheses verified at once? Have several unknowns been determined with a single equation?

Care must also be taken to distinguish between the several kinds of hypotheses. First there are those that are quite natural and without which we could hardly do. It is difficult not to suppose that the influence of very distant bodies is quite negligible, that small movements obey a linear law, that the effect is a continuous function of the cause. I will say as much for the conditions imposed by symmetry. All these hypotheses form, so to speak, the common foundation of all theories in mathematical physics. They are the last that should be abandoned.

There is a second category of hypotheses that I will qualify as indifferent. In the greater number of questions, the analyst supposes at the outset of his calculations, either that matter is continuous, or inversely that it is made up of atoms. By either method his results will be the same. If he chooses the latter, and experiment confirms his results, will he think he has demonstrated, for example, the real existence of atoms?

Into optical theories two vectors are introduced, which are regarded, the one as a velocity, the other as a vortex. This is again an indifferent hypothesis, since the same conclusions would have been reached with contrary suppositions; the success of the experiment cannot prove that the first vector is a velocity; it proves but one thing, namely that it is a vector; this is really the only hypothesis that was introduced in the premises. To give it that concrete appearance that the weakness of our intellects requires, it was necessary to consider it either as a velocity or as a vortex; likewise it was necessary to represent it by a letter, as x or y ; but the result, whatever it be, will not prove that we were right or wrong to regard it as a velocity; no more can it be proved correct or not to call it x and not y .

These indifferent hypotheses are never dangerous, provided their character is not misunderstood. They may be useful, either

as artifices for calculation, or to sustain our comprehension by concrete images, to fix our ideas, as we say. There is then no reason to proscribe them.

The hypotheses of the third category are veritable generalisations. They are the ones that experiment will confirm or prove false. Verified or condemned, they will always be fruitful. But, for the reasons that I have given, this holds only if they are not too numerous.

ORIGIN OF MATHEMATICAL PHYSICS.

Let us go farther and study at close range the conditions which have brought about the development of mathematical physics. We recognise at once that savants have always tried to resolve the complex phenomenon given directly by experiment into a very great number of elementary phenomena. And this in three different ways :

First, with respect to time, instead of embracing in its entirety the progressive development of a phenomenon, we seek simply to join each instant to the one immediately preceding ; it is admitted that the actual state of the world depends only on the immediate past, without being influenced by the memory of a more remote past. Thanks to this postulate, instead of studying directly the whole succession of phenomena, it is possible to write its *differential equation* representing a single epoch ; for Newton's laws Kepler's are substituted.

Next, we seek to decompose the phenomenon in space. What experiment gives us, is a confused collection of facts spread over a field of considerable extent ; the task is to discern the elementary phenomenon, which is localised in a very small region of space.

A few examples will perhaps make my meaning clearer. If one wished to study in all its complexity the distribution of temperature in a solid which is cooling, it would be impossible to do so. All becomes simple if we reflect that a point in the solid cannot impart heat to a distant point, but only to the nearest, and it is only gradually that the flow of heat will be able to reach other portions of the solid. The elementary phenomenon is the exchange of

heat between two contiguous points; it is strictly localised, and it is relatively simple, if it be admitted, as is natural, that it is not influenced by the temperature of molecules whose distance is sensible.

I bend a rod; it will assume a very complicated form whose direct study would be impossible; but I shall be able to attack the problem, if I observe that the flexure is only the resultant of the deformations of the very small elements of the rod, and that the deformation of each of these elements depends only on the forces which are directly applied to it and in nowise on those which may act upon the other elements.

In all these examples, which may be increased indefinitely, it is admitted that there is no action at a distance or at great distances. This is a hypothesis; it is not always true, as the law of gravitation proves; it must then be submitted to verification. If it is confirmed, even approximately, it is precious, for it is going to permit the use of mathematical physics by successive approximations at least. If it does not stand the test, something analogous must be sought, for there are still other ways to reach the elementary phenomenon. If several bodies act simultaneously, it may happen that their actions are independent and may be added together, either as vectors or as scalar quantities. The elementary phenomenon is then the action of an isolated body. Or perhaps one has to do with small movements, or more generally with small variations, which obey the well-known law of superposition. The observed movement will then be decomposed into simple movements; for example, a sound into its harmonics, white light into its monochromatic components.

When we have discerned in what direction to seek the elementary phenomenon, by what means may we reach it?

It will often happen that to predict it, or rather to predict what is useful for us, it will not be necessary to know the mechanism; the law of great numbers will suffice. Consider the example of the propagation of heat; each molecule radiates towards its neighbors, according to a law which we have no need of knowing; if we make any supposition in this regard it will be an indifferent

hypothesis and consequently useless and unverifiable. And, indeed, by the action of averages and thanks to the symmetry of the medium, all differences are razed, and whatever the hypothesis, the result is always the same.

The same circumstances are present in the theories of elasticity and capillarity; the neighboring molecules attract and repel each other, we have no need to know according to what law; it suffices that this attraction is sensible at small distances only, that the molecules are very numerous, that the medium is symmetrical, and we have but to let the law of great numbers act.

Here again the simplicity of the elementary phenomenon was hidden beneath the complexity of the observable resultant phenomenon; but in its turn, this simplicity was only apparent and concealed a very complex mechanism.

The best way to reach the elementary phenomenon would be evidently by experiment. It would be necessary by experimental artifices, to dissociate the complex beam that nature offers to our researches and study with care its elements as purified as possible; for example, natural white light can be decomposed into monochromatic lights by means of a prism and into polarised lights by means of a polariser.

Unfortunately this is neither always possible nor sufficient, and it is sometimes necessary for the mind to anticipate the experiment. I will cite but a single example which has always appealed to me.

If I decompose white light, I can isolate a small portion of the spectrum, but however small it may be, it always conserves a certain width. Similarly, the natural lights called *monochromatic* give us a very fine line, although not infinitely fine. One might suppose that in studying experimentally the properties of these natural lights, operating with finer and finer spectral beams, and passing at last to the limit, one would come to know the properties of a light rigorously monochromatic. This would not be so. Imagine that two beams start from the same source, that they are polarised in two planes at right angles, afterwards brought into the same plane of polarisation, and that one tries to make them inter-

fere. If the light were *rigorously* monochromatic they would interfere, but with our nearly monochromatic lights there would be no interference, and this however narrow the beam; it would be necessary in order to have it otherwise that the beam be several million times narrower than the finest known. Here then the passage to the limit would have deceived us; the intellect has outstripped experiment, and if this has been successfully done, it is because the former was guided by the instinct of simplicity.

A knowledge of the elementary fact permits us to put the problem into the form of an equation; it only remains to deduce from this by combination the complex observable and verifiable fact. This is what is called *integration*; it is the mathematician's affair.

It may be asked why, in the physical sciences, a generalisation readily takes the mathematical form. The reason is now easy to see; it is not merely that one has to express numerical laws; it is because the observable phenomenon is due to the superposition of a great number of elementary phenomena *all similar to each other*; in this way the differential equations are quite readily introduced.

It is not sufficient that each elementary phenomenon obeys simple laws, it is necessary that all to be combined obey the same law. It is only then that the intervention of mathematics may be useful; mathematics teaches us, in fact, to combine like with like. Its goal is to divine the result of a combination, without passing through all the intermediate steps each time. If we have to repeat several times the same operation, it enables us to avoid this repetition by informing us beforehand of the result by a sort of induction. In such cases all these operations must be similar to each other, otherwise we should have to go step by step, and mathematics would become useless.

It is thus of the approximate homogeneity of matter studied by the physicist that mathematical physics could be born. In the natural sciences, we do not find these conditions: homogeneity, relative independence of distant parts, simplicity of the elementary part; and that is why naturalists are obliged to make use of other modes of generalisation.

SIGNIFICATION OF PHYSICAL THEORIES.

Men of the world are struck to see how transient are scientific theories. After several years of prosperity, they see them successively abandoned; they see ruins pile on ruins; they predict that the theories current to-day will, after a brief delay, in their turn succumb, and they conclude that such theories are absolutely in vain. It is what they call the *bankruptcy of science*.

Their scepticism is superficial; they take no account whatever of the object and rôle of scientific theories, otherwise they would understand that the ruins are still good for something. No theory seemed so well established as Fresnel's which attributed light to movements of the ether. However, that of Maxwell is to-day preferred. Does this mean that the work of Fresnel has been in vain? No, for Fresnel's goal was not to know whether there really is an ether, whether or not it is formed of atoms, whether these atoms move in such or such a way; it was to predict optical phenomena. As for that, Fresnel's theory enables us to do this to-day as well as it did before Maxwell. The differential equations are always true; they may always be integrated by the same methods and the results of this integration ever preserve their value.

Let no one say that we thus reduce physical theories to simple practical recipes; these equations express actual relations, and if the equations remain true, it is because these relations preserve their reality. They teach us, now as before, that there is such and such a relation between this thing and that; only, something which we called *movement* before, we now call *electric current*. But these names were only images substituted for the real objects that nature will forever hide from us. The true relations between these real objects are the only reality that we can reach, and the sole condition is that the same relations shall exist between these objects as between the images we are forced to put in their place. If these relations are known to us, what matters it if we judge it convenient to replace one image by another?

That a given periodic phenomenon (an electrical oscillation for instance) is really due to the vibration of a given atom which,

behaving like a pendulum, is displaced in such or such a way, all this is neither certain nor interesting. But that there is between the electrical oscillation, the movement of the pendulum, and all periodic movements an intimate relationship which corresponds to a profound reality; that this relationship, this similitude, or better this parallelism is continued in the details; that it is a consequence of more general principles, as the conservation of energy and least action,—this we may affirm; this is the truth that will remain forever the same in all the guises in which we may see fit to dress it.

Numerous theories of dispersion have been proposed. The first were imperfect and contained but little truth. Then came Helmholtz's, which was modified in various ways; and its author himself has imagined another based on Maxwell's principles. But the remarkable thing is, that all the scientists who have followed Helmholtz reach the same equations, from seemingly widely separated starting-points. I venture to say that these theories are all true at once, not merely because they allow us to predict the same phenomena, but because they express a true relation, that between absorption and anomalous dispersion. In the premises of these theories, that which is true is common to all; it is the affirmation of such or such a relation between certain things that some call by one name some by another.

The kinetic theory of gases has given rise to many objections, to which reply would be difficult, if there had been any claim that it contained absolute truth. But all these objections cannot refute its past usefulness, particularly in revealing to us the one true relation, otherwise profoundly hidden, between gaseous and osmotic pressures. In this sense it may be said to be true.

When a physicist finds a contradiction between two theories which are equally dear to him, he sometimes says: Let us not be troubled but let us hold fast to the two ends of the chain that the intermediate links be not lost to us. This argument of the embarrassed theologian would be ridiculous if we are to attribute to physical theories the sense given them by men of the world. In case of contradiction, one of them at least should then be considered false. It is no longer so if we will seek in them what is

to be sought. It may be they both express true relations and that there is contradiction only in the images with which we have dressed reality.

To those who find that we restrict too much the domain accessible to the scientist, I reply: These questions which we prohibit you from studying and which you so regret, are not only insoluble, they are also illusory and void of sense.

Your philosopher claims that all physics can be explained by the mutual impact of atoms. If he means that the same relations obtain among physical phenomena as among the mutual impacts of a great number of billiard-balls, nothing better, this is verifiable, it is perhaps true. But he means to say something more; and we think we understand him because we think we know what an impact is in itself. Why? simply because we have often seen a game of billiards. Are we to understand that God, in contemplating his work, feels the same sensations as we in the presence of a billiard match? If we do not wish to give to his assertion this fantastic meaning, if also we do not wish to give it the one I previously mentioned, then it has no meaning whatever.

Hypotheses of this nature have only a symbolic sense. The scientist should not banish them any more than a poet banishes metaphor; but he should know what they are worth. They may be useful to give satisfaction to the mind, and they will not be harmful provided they are but indifferent hypotheses.

These considerations show us why certain theories that were thought to be abandoned and definitely condemned by experiment, are suddenly revived from their ashes and recommence a new life. It is because they express true relations, and had not ceased to do so, when for some reason or other we thought it necessary to enunciate the same relations in another language. They had thus kept a sort of latent life.

Hardly fifteen years ago, was there anything more ridiculous, more quaintly old-fashioned, than the fluids of Coulomb? But nevertheless here they reappear under the name *electrons*. In what do these molecules electrified in a permanent way differ from the electric molecules of Coulomb? True, in the electrons the elec-

tricity is supported by a little, though very little, matter; in other words, they have mass. But Coulomb did not gainsay mass to his fluids; or if he did, it was reluctantly. It would be rash to affirm that the belief in electrons will not also undergo its eclipse; but it was not less curious to remark this unexpected renaissance.

But the most striking example is Carnot's principle. Carnot established it, starting from false hypotheses. When it was perceived that heat is not indestructible but may be converted into work, his ideas were completely abandoned; later Clausius returned to them and caused them to triumph definitively. Carnot's theory, in its primitive form, expressed, besides true relations, other inexact relations, *débris* of old ideas; but the presence of the latter did not alter the reality of the others. Clausius had but to separate them as one cuts away dead branches.

The result was the second law of thermodynamics. The relations were always the same, although these relations did not hold, in appearance at least, between the same objects. This sufficed to preserve for the principle its value. Nor have the reasonings of Carnot perished by reason of this; they were applied to matter infected with error; but their form (that is to say, their essential part) remained correct.

What I have said throws light at the same time on the rôle of general principles like the principles of least action and the conservation of energy. These principles have a very great value; they were obtained in seeking what was common in the statements of numerous physical laws; they thus represent the quintessence of innumerable observations. However, from their very generality results a consequence to which I have called attention in the preface to my *Course on Thermodynamics*; it is that they are of necessity verified. Since we cannot give energy a general definition, the principle of the conservation of energy signifies simply that there is a *something* that remains constant. Whatever new notions of the world future experiments may give us, we are certain beforehand that there is something which will remain constant, and which we may call *energy*.

Does this mean that the principle has no sense and vanishes

into a tautology? Not at all; it means that the different things we call *energy* are joined by a true relationship. But even if this principle has a meaning, it may be false; perhaps we have no right to deduce applications from it indefinitely, and yet it is sure beforehand to be verified in the strict sense of the word. How then shall we be warned when it has reached the full development that we may legitimately give it? Simply when it ceases to be useful, or when we may no longer use it to correctly predict new phenomena. We shall be sure in such cases that the relation affirmed is no longer true; for otherwise it would be fruitful; experiment, without directly contradicting a new extension of the principle, will nevertheless have condemned it.

PHYSICS AND MECHANISM.

Most theorists have a constant predilection for explanations borrowed from mechanics or dynamics. Some would be satisfied if they could account for all phenomena by the movement of molecules attracting one another according to certain laws. Others are more exacting, they would suppress attractions at a distance; their molecules would follow rectilinear paths from which they could only be deviated by impacts. Still others, as Hertz, suppress also the forces, but suppose their molecules submitted to geometrical connections analogous, for example, to those of articulated systems; they thus wish to reduce dynamics to a sort of kinematics. All, in a word, wish to bend nature into a certain form, lacking which their minds cannot be satisfied. Is nature flexible enough for this?

I have already put the question in the preface to my work: *Electricity and Optics*. I have shown that every time the principles of energy and of least action are satisfied, not only is there always a mechanical explanation possible, but there is always an infinity of them. Thanks to a well-known theorem on articulated systems due to Koenigs, it may be shown that everything may be explained in an infinite number of ways by connections after the manner of Hertz, or else by central forces. Without doubt, it might be just

as easily demonstrated that everything may be explained by simple impacts.

For this, bear in mind, it is not sufficient to be content with ordinary matter, which comes in contact with our senses and whose movements we observe directly. Ordinary matter may be conceived either as formed of atoms whose inner movements escape us, the displacement of the whole being alone accessible to our senses, or one of those subtle fluids may be imagined which, under the name *ether* or other names, have always played such an important rôle in physical theories.

Often one goes farther and regards the ether as the only primitive matter, or as the only true matter. The more moderate consider ordinary matter as condensed ether, which is in no way startling; but others reduce still further its importance and see in matter only the geometrical locus of the singularities in the ether. Thus, for Kelvin, what we call *matter* is but the locus of the points at which the ether is animated by vortex motions; for Riemann, it was the locus of the points at which ether is constantly destroyed; for more recent writers, Wiechert or Larmor, it is the locus of the points at which the ether has undergone a sort of torsion of a very particular kind. Taking any one of these points of view, the question arises in my mind, by what right do we apply to the ether, under pretext that it is true matter, the mechanical properties observed in ordinary matter, which is but false matter?

The ancient fluids, caloric, electricity, etc., were abandoned when it was seen that heat is not indestructible. But they were abandoned also for another reason. In materialising them, their individuality, so to speak, was emphasised, gaps were opened between them. It was necessary to fill in these gaps when the sentiment of the unity of nature became stronger, and when the intimate relations binding all parts were perceived. In multiplying the fluids, not only did the ancient physicists create unnecessary entities, but they broke down real ties. It is not sufficient that a theory does not affirm false relations, neither must it hide true relations.

Does our ether actually exist?

We know whence comes our belief in the ether. If light takes several years to reach us from a star, it is no longer upon the star nor yet upon the earth; but it must be somewhere, and supported by some material agency.

The same idea can be expressed in a more mathematical and abstract form. What we note are changes undergone by material molecules; we see, for example, that our photographic plate experiences the consequences of phenomena of which the incandescent mass of a star was the theatre several years ago. Now, in ordinary mechanics, the state of the system studied depends only on its state at the moment immediately preceding; the system satisfies certain differential equations. On the other hand, if we did not believe in the ether, the state of the material universe would depend not only upon the state immediately preceding, but also upon much more ancient states; the system would satisfy equations of finite differences. It is to obviate this transgression of the general mechanical laws that we have invented the ether.

This would oblige us to fill the interplanetary space with ether, but not to make it penetrate into the midst of material media. Fizeau's experiment goes farther. By the interference of rays that have passed through water or air in motion, it seems to show us two different media penetrating each other and yet moving with respect to each other. We all but touch the ether.

Situations may be conceived in which we can touch it closer still. Suppose Newton's principle of the equality of action and reaction is not true if applied to matter *only* and that this is demonstrated. The geometrical sum of all the forces applied to all the material molecules would no longer be zero. It would be necessary, if we did not wish to change the whole science of mechanics, to introduce the ether, in order that the action that matter here apparently undergoes should be counterbalanced by the reaction of matter on something.

Or again, suppose we discover that optical and electrical phenomena are influenced by the movement of the earth. It would follow that these phenomena could reveal to us not only the relative movements of material bodies, but also what would seem to be

their absolute movements. It would again be necessary to have an ether, in order that these so-called absolute movements should not take place with respect to empty space, but with respect to something concrete.

Will this ever be accomplished? I do not cherish the hope, and I will say shortly why. And yet, it is not so absurd since others have entertained it. For example, if the theory of Lorentz were true, Newton's principle would not apply to matter *alone*, and the difference would not be very far from being accessible to experiment. On the other hand, many experiments have been made on the influence of the earth's movement. The results have always been negative. But if these experiments have been undertaken, it is because we were not sure beforehand, and indeed according to the reigning theories, the compensation should be only approximate, and we should expect to see improved methods give positive results.

I think that such an experiment is illusory; it was none the less interesting to show that a success of this kind would open in a certain sense a new world.

And now allow me to digress slightly; I must explain why I do not believe, in spite of Lorentz, that more exact observations will ever make evident anything else than relative displacements of material bodies. Experiments have been made that should have disclosed the terms of the first order; the results were negative; can that have been by chance? No one has admitted it; a general explanation was sought, and Lorentz found it; he showed that the first order terms should cancel each other, but not the second order terms. Then more precise experiments were made, which were also negative; neither could this be a result of chance; an explanation was necessary and was found; they are always found; hypotheses are what we lack the least.

But this is not enough; who does not think this leaves too important a rôle to chance? Would it not be also a chance that this singular concurrence would cause a certain circumstance to destroy the terms of the first order, and that a totally different circumstance should cause those of the second order to vanish? No, it is

necessary to find the same explanation for the two cases, and everything tends to show that this explanation would serve just as well for the higher order terms, and that the mutual destruction of these terms will be rigorous and absolute.

ACTUAL STATE OF THE SCIENCE.

In the history of the development of physics two opposite tendencies are to be distinguished. On the one hand, at each instant new relations are discovered between objects which seemed destined to remain forever separated; scattered facts cease to be strangers to each other; they tend to arrange themselves into an imposing synthesis. Science marches towards unity and simplicity.

On the other hand, observation reveals every day new phenomena; they must wait for their place a long time; and sometimes to make one, a corner of the edifice must be demolished. In the known phenomena themselves, where our crude senses indicate unity, we perceive details more varied from day to day; what we thought to be simple becomes complex and science seems to march towards diversity and complication.

Of these two opposite tendencies each of which seems to triumph in turn, which will win? If the first, science is possible; but nothing proves this *a priori*, and possibly after vain efforts to bend nature in spite of herself to our ideal of unity, submerged by the ever-mounting flood of our new riches, we shall be compelled to renounce classifying them, abandon our ideal, and reduce science to the recording of innumerable recipes.

We cannot reply to this question. All that we can do is to observe the science of to-day and to compare it with that of yesterday. From this examination we may doubtless draw some conjectures.

A half century ago, hopes were high. The discovery of the conservation of energy and of its transformations had just revealed the unity of force. It showed also that the phenomena of heat could be explained by molecular movements. The nature of these movements was not exactly known, yet no one doubted but that it soon would be. For light, the work seemed completely done. As

concerns electricity, the advancement was less great. Electricity had just annexed magnetism. This was a considerable step towards unity, and a definite one. But in what way was electricity to enter in its turn into the general unity, how was it to be included in the universal mechanism? No one had any idea. The possibility of this reduction was not doubted by any one; they had faith. Finally, as to what concerns the molecular properties of material bodies, the reduction seemed still easier; but all the details were hazy. In a word, the hopes were vast, they were strong, but they were vague.

To-day what do we see? In the first place, a step in advance, an immense progress. The relations between electricity and light are now known; the three domains of light, electricity, and magnetism, formerly separated, are but one now; and this combination seems definite. This conquest, nevertheless, has cost us some sacrifices. Optical phenomena enter as particular cases in electrical phenomena; as long as the former remained isolated, it was easy to explain them by movements thought to be known in all their details; that was easy. But now an explanation, to be acceptable, must be readily applicable to the whole electrical domain. This often causes difficulty.

The most satisfactory theory we have, is that of Lorentz; it is unquestionably the one that best explains the known facts, the one that sheds light on the greatest number of true relations, the one in which are to be found the most traces of definite construction. Nevertheless, it still possesses a serious fault, as I have above shown; it is in contradiction with Newton's principle of the equality of action and reaction; or rather, in the eyes of Lorentz, this principle is not applicable to matter alone; in order to be true, it must take account of the actions exerted by the ether on matter, and of the reaction of matter upon the ether. At present it seems most probable that things do not happen in this way.

However this may be, thanks to Lorentz, the results of Fizeau on the optics of moving bodies, the laws of normal and anomalous dispersion and of absorption have been connected together and with the other properties of the ether by bonds that doubtless will

not break. Look at the ease with which the Zeeman effect found its place, and even helped to classify the magnetic rotation of Faraday which had remained rebellious to Maxwell's efforts. This facility proves that Lorentz's theory is not an artificial assemblage destined to give way. Probably it should be modified, but not destroyed.

Lorentz had no other ambition than to include in a single whole all the optics and electrodynamics of moving bodies; he made no pretense to give a mechanical explanation. Larmor goes farther; keeping of Lorentz's theory what is essential, he grafts on it MacCullagh's ideas on the direction of the movement of the ether. However ingenious this effort may be, the fault in Lorentz's theory remains, and is even aggravated. According to Lorentz, we do not know what the movements of the ether are; thanks to this ignorance, we might suppose them such as compensated those of matter and re-established the equality of action and reaction. With Larmor, we know the movements of the ether, and we can demonstrate that the compensation does not take place.

If Larmor has to my mind failed, does that mean that a mechanical explanation is impossible? Far from it: I said above that as long as a phenomenon obeys the two principles of energy and least action, it permits of an infinite number of mechanical explanations. It is the same for optical and electrical phenomena.

But that does not suffice: for a mechanical explanation to be good, it must be simple; in order to choose it from among all those that are possible, there must be other reasons than the necessity to make a choice. Well, a theory which satisfies this condition and which consequently might be useful, we do not possess as yet. Are we to complain? That would be to forget the end sought, which is not the mechanism, but the true and sole aim of unity.

We should then bridle our ambition; let us not seek to formulate a mechanical explanation; let us be content to show that we may always find one if we so wish. In this we have succeeded; the principle of the conservation of energy has always been confirmed; a second principle has been joined to this, that of least action, put in the form appropriate to physics. This also has always been

verified, at least as far as concerns reversible phenomena, which obey Lagrange's equations, that is to say, the most general laws of physics.

The irreversible phenomena are much more rebellious. They also, however, are being arranged and tend to enter the unity: the light which illuminates them has come from Carnot's principle. For a long time thermodynamics was confined to the study of the dilatation of bodies and their change of state. Later it became bolder and enlarged its domain considerably. We owe to it the theories of the voltaic cell and thermo-electric phenomena; there is not in all physics a corner that it has not explored, and it has even attacked chemistry. Everywhere the same laws reign; everywhere under a diversity of appearances Carnot's principle reappears; everywhere also appears that eminently abstract concept of entropy, which is as universal as energy, and like it seems to conceal a reality. Radiant heat seemed to escape it; but recently that too has been brought under the same laws.

In this way new analogies are revealed, which may often be pursued in detail; electric resistance resembles the viscosity of liquids; hysteresis resembles rather the friction of solids. In all cases, friction appears to be the type imitated by the most diverse irreversible phenomena, and this relationship is real and profound.

A strictly mechanical explanation of these phenomena has also been sought. Such is hardly possible. To find it, it has been necessary to suppose that the irreversibility is but an appearance, that the elementary phenomena are reversible and obey the known laws of dynamics. But the elements are extremely numerous and blend more and more, so that to our crude eyes all appears to tend towards uniformity, that is to say, all seems to march in the same direction without hope of return. The apparent irreversibility is thus but an effect of the law of great numbers. Only a being of infinitely subtle senses, as the imaginary demon of Maxwell, could untangle this snarl and turn the world about.

This conception, which is connected with the kinetic theory of gases, has cost great effort, and has been on the whole not very fruitful; it may become so. This is not the place to examine if it

leads to contradictions, and if it conforms well to the true nature of things.

Let us notice, however, the original ideas of Gouy on the Brownian movement. According to this *savant*, this singular movement does not obey Carnot's principle. The particles that it sets moving about are smaller than the meshes of this tightly drawn net; they should then be ready to unravel them and in that way turn the world about. One may imagine he sees Maxwell's demon at work.

To resume, phenomena long known are better and better classified; but new phenomena come to claim their place; and most of them, as the Zeemann effect, find it at once.

But we have the cathode rays, the X-rays, the uranium and radium radiations. There is a whole world that none suspect. How many unexpected guests to find a place for! No one can yet predict the place that they will occupy. But I do not think they will destroy the general unity, I think rather they will complete it. On the one hand, indeed, the new radiations seem to be connected with the phenomena of luminescence; not only do they excite fluorescence, but they arise sometimes under the same conditions as it. Neither are they without relationship with the causes producing the spark discharge under the action of ultra-violet light.

Finally, and above all, it is believed that in all these phenomena there exist ions,—animated, it is true, with far greater velocities than in electrolytes.

All this is very vague, but it will become clearer.

Phosphorescence and the action of light on a spark were regions quite isolated and consequently somewhat neglected by investigators. It is to be hoped that now a new path may be made which will facilitate their communication with the rest of science.

Not only do we discover new phenomena, but in those that we think we know, unlooked-for aspects are revealed. In the free ether, the laws preserve their majestic simplicity; but matter, properly so called, seems more and more complex; all that is said of it is but approximate and at each instant our formulæ require new terms.

Nevertheless the ranks are not broken ; the relations that we have recognised between objects that we believed simple, still remain between the same objects when recognised in their complexity, and that alone is important. Our equations become more and more complicated, it is true, so as to embrace more closely the complexities of nature ; but nothing is changed in the relations which permit these equations to be derived from one another. In a word, the *form* of these equations persists.

Take for example the laws of reflexion ; Fresnel established them by a simple and attractive theory, which experiment seemed to confirm. Subsequently, more precise researches proved that this verification was only approximate ; they showed everywhere traces of elliptical polarisation. But, thanks to the aid given us by the first approximation, the cause of these anomalies was soon found in the presence of a transition layer ; and Fresnel's theory has remained in all its essentials.

It would seem, nevertheless, that all these relations would never have been noted if the complexity of the objects they joined had been known beforehand. Long ago it was said : If Tycho had had instruments ten times as precise, we should never have had either Kepler, Newton, or Astronomy. It is a misfortune for a science to be born too late, when the means of observation have become too perfect. This is what is happening to-day with physical chemistry ; the founders are hampered in their estimates by the third and fourth decimals ; happily they are men of robust faith.

As the properties of matter are better known, we see that continuity reigns. From the work of Andrews and Van der Waals, we see how the transition from the liquid to the gaseous state is made, and that it is not brusque. Similarly there is no gap between the liquid and solid states, and we note in their work by the side of articles on the rigidity of liquids memoirs on the flow of solids.

With this tendency simplicity without doubt is lost ; such and such an effect was represented by several straight lines ; it is necessary now to join these lines by curves more or less complicated. In return unity is gained. These separated categories quiet the mind but do not satisfy it.

Finally the methods of physics have invaded a new domain, that of chemistry; physical chemistry is born. It is still quite young, but we see that already it has allowed us to connect such phenomena as electrolysis, osmosis, and the movements of ions.

From this rapid exposition, what do we conclude?

Taking all things into account, unity has become more nearly realised; this has not been as quickly done as was hoped fifty years ago, and the way predicted has not always been followed; but, on the whole, much ground has been gained. •

H. POINCARÉ.

PARIS, 1900.