



Principles of Mathematical Physics

Author(s): Henri Poincaré

Source: *The Scientific Monthly*, Vol. 82, No. 4 (Apr., 1956), pp. 165-175

Published by: American Association for the Advancement of Science

Stable URL: <http://www.jstor.org/stable/21942>

Accessed: 25/07/2009 09:50

Your use of the JSTOR archive indicates your acceptance of JSTOR's Terms and Conditions of Use, available at <http://www.jstor.org/page/info/about/policies/terms.jsp>. JSTOR's Terms and Conditions of Use provides, in part, that unless you have obtained prior permission, you may not download an entire issue of a journal or multiple copies of articles, and you may use content in the JSTOR archive only for your personal, non-commercial use.

Please contact the publisher regarding any further use of this work. Publisher contact information may be obtained at <http://www.jstor.org/action/showPublisher?publisherCode=aaas>.

Each copy of any part of a JSTOR transmission must contain the same copyright notice that appears on the screen or printed page of such transmission.

JSTOR is a not-for-profit organization founded in 1995 to build trusted digital archives for scholarship. We work with the scholarly community to preserve their work and the materials they rely upon, and to build a common research platform that promotes the discovery and use of these resources. For more information about JSTOR, please contact support@jstor.org.



American Association for the Advancement of Science is collaborating with JSTOR to digitize, preserve and extend access to *The Scientific Monthly*.

<http://www.jstor.org>

THE SCIENTIFIC MONTHLY

APRIL 1956

Principles of Mathematical Physics

HENRI POINCARÉ

At the beginning of the present century could anyone have predicted the striking developments in physics soon to take place? One man came surprisingly close. He was Henri Poincaré (1854–1912), professor at the Sorbonne in Paris. In 1904, a year before the publication by Einstein of his fundamental papers on special relativity, quantum theory, and the Brownian movement, Poincaré examined the state of physical research and offered to the International Congress of Arts and Sciences in St. Louis what he jokingly termed his diagnosis. Foresight is a matter of genius, hindsight is something all can enjoy. On the following pages, available for comparison with what has actually happened, are Poincaré's remarkable predictions of more than half a century ago. Poincaré's own contribution to science was broad and deep, enriching pure mathematics, mathematical physics, and astronomy. His writings on the meaning and methods of science are now accepted as classics of philosophy. The St. Louis address on "The principles of mathematical physics," translated by George Bruce Halsted, was published in The Monist, January 1905; it served subsequently as the basis for several chapters in Poincaré's The Value of Science.

WHAT is the actual state of mathematical physics? What are the problems it is led to set itself? What is its future?

Is its orientation on the point of modifying itself?

Will the aim and the methods of this science appear in ten years to our immediate successors in the same light as to ourselves; or, on the contrary, are we about to witness a profound transformation? Such are the questions we are forced to raise in entering today upon our investigation.

If it is easy to propound them, to answer is difficult.

If we feel ourselves tempted to risk a prognostication, we have, to resist this temptation, only to think of all the stupidities the most eminent savants of a hundred years ago would have uttered, if one had asked them what the science of the nineteenth century would be. They would have believed themselves bold in their predictions, and after the event, how very timid we should have found them.

Do not, therefore, expect of me any prophecy; if I had known what one will discover tomorrow,

I would long ago have published it to secure me the priority.

But if, like all prudent physicians, I shun giving a prognosis, nevertheless I cannot dispense with a little diagnostic; well, yes, there are indications of a serious crisis, as if we should expect an approaching transformation.

We are assured that the patient will not die of it, and even we can hope that this crisis will be salutary, that it was even necessary for his development. This the history of the past seems to guarantee us.

This crisis in fact is not the first, and for its comprehension it is important to recall those which have preceded it.

Mathematical physics, we know, was born of celestial mechanics, which engendered it at the end of the eighteenth century, at the moment when it itself attained its complete development. During its first years especially, the infant resembled in a striking way its mother.

The astronomic universe is formed of masses,

very great without doubt, but separated by intervals so immense, that they appear to us only as material points. These points attract each other in the inverse ratio of the square of the distances, and this attraction is the sole force which influences their movements. But if our senses were sufficiently subtle to show us all the details of the bodies which the physicist studies, the spectacle we should there discover would scarcely differ from what the astronomer contemplates. There also we should see material points, separated one from another by intervals, enormous in relation to their dimensions, and describing orbits following regular laws.

These infinitesimal stars are the atoms. Like the stars properly so called, they attract or repel each other, and this attraction or this repulsion, directed following the straight line which joins them, depends only on the distance. The law according to which this force varies as a function of the distance is perhaps not the law of Newton, but it is an analogous law; in place of the exponent -2 , we probably have a different exponent, and it is from this change of exponent that springs all the diversity of physical phenomena, the variety of qualities and of sensations, all the world colored and sonorous which surrounds us—in a word, all nature.

Such is the primitive conception in all its purity. It only remains to seek in the different cases what value should be given to this exponent in order to explain all the facts. It is on this model that Laplace, for example, constructed his beautiful theory of capillarity; he regards it only as a particular case of attraction, or as he says of universal gravitation, and no one is astonished to find it in the middle of one of the five volumes of the *Mécanique céleste*.

More recently Briot believed he had penetrated the final secret of optics in demonstrating that the atoms of ether attract each other in the inverse ratio of the sixth power of the distance; and Maxwell, Maxwell himself, does he not say somewhere that the atoms of gases repel each other in the inverse ratio of the fifth power of the distance? We have the exponent -6 , or -5 in place of the exponent -2 , but it is always an exponent.

Among the theories of this epoch, one alone is an exception, that of Fourier; in it are indeed atoms, acting at a distance one upon the other; they mutually transmit heat, but they do not attract, they never budge. From this point of view, the theory of Fourier must have appeared to the eyes of his contemporaries, to those of Fourier himself, as imperfect and provisional.

This conception was not without grandeur; it was seductive, and many among us have not finally

renounced it; they know that one will attain the ultimate elements of things only by patiently disentangling the complicated skein that our senses give us; that it is necessary to advance step by step, neglecting no intermediary; that our fathers were wrong in wishing to skip stations; but they believe that when one shall have arrived at these ultimate elements, there again will be found the majestic simplicity of celestial mechanics.

Neither has this conception been useless; it has rendered us an inestimable service, since it has contributed to make precise in us the fundamental notion of the physical law.

I will explain myself; how did the ancients understand law? It was for them an internal harmony, static, so to say, and immutable; or it was like a model that nature constrained herself to imitate. A law for us is no more that at all; it is a constant relation between the phenomenon of to-day and that of tomorrow; in a word, it is a differential equation.

Behold the ideal form of physical law; well, it is the law of Newton which first covered it; and then how has one acclimated this form in physics; precisely in copying as much as possible this law of Newton—that is, in imitating celestial mechanics.

Nevertheless, a day arrived when the conception of central forces no longer appeared sufficient, and this is the first of those crises of which I just now spoke.

What did one do then? One gave up trying to penetrate into the detail of the structure of the universe, to isolate the pieces of this vast mechanism, to analyze one by one the forces which put them in motion, and was content to take as guides certain general principles which have precisely for object to spare us this minute study.

How so? Suppose that we have before us any machine; the initial wheel work and the final wheel work alone are visible, but the transmission, the intermediary wheels by which the movement is communicated from one to the other are hidden in the interior and escape our view; we do not know whether the communication is made by gearing or by belts, by connecting rods or by other dispositives.

Do we say that it is impossible for us to understand anything about this machine so long as we are not permitted to take it to pieces? You know well we do not, and that the principle of the conservation of energy suffices to determine for us the most interesting point. We easily ascertain that the final wheel turns ten times less quickly than the initial wheel, since these two wheels are visible; we are able thence to conclude that a couple ap-

plied to the one will be balanced by a couple ten times greater applied to the other. For that there is no need to penetrate the mechanism of this equilibrium and to know how the forces compensate each other in the interior of the machine; it suffices to be assured that this compensation cannot fail to occur.

Well, in regard to the universe, the principle of the conservation of energy is able to render us the same service. This is also a machine, much more complicated than all those of industry, and of which almost all the parts are profoundly hidden from us; but in observing the movement of those that we can see, we are able, aiding ourselves by this principle, to draw conclusions which remain true whatever may be the details of the invisible mechanism which animates them.

The principle of the conservation of energy, or the principle of Mayer, is certainly the most important, but it is not the only one; there are others from which we are able to draw the same advantage. These are the principle of Carnot, or the principle of the degradation of energy; the principle of Newton, or the principle of the equality of action and reaction; the principle of relativity, according to which the laws of physical phenomena should be the same, whether for an observer fixed, or for an observer carried along in a uniform movement of translation; so that we have not and could not have any means of discerning whether or not we are carried along in such a motion; the principle of the conservation of mass, or principle of Lavoisier. I would add the principle of least action.

The application of these five or six general principles to the different physical phenomena is sufficient for our learning of them what we could reasonably hope to know of them.

The most remarkable example of this new mathematical physics is, beyond contradiction, Maxwell's electromagnetic theory of light.

We know nothing as to what is the ether, how its molecules are disposed, whether they attract or repel each other; but we know that this medium transmits at the same time the optical perturbations and the electrical perturbations; we know that this transmission should be made conformable to the general principles of mechanics and that suffices us for the establishment of the equations of the electromagnetic field.

These principles are results of experiments boldly generalized; but they seem to derive from their generality itself an eminent degree of certitude.

In fact, the more general they are, the more frequently one has the occasion to check them, and the verifications, in multiplying themselves, in

taking forms the most varied and the most unexpected, finish by leaving no longer place for doubt.

Such is the second phase of the history of mathematical physics and we have not yet emerged from it.

Do we say that the first has been useless? that during fifty years science went the wrong way, and that there is nothing left but to forget so many accumulated efforts that a vicious conception condemned in advance to nonsuccess?

Not the least in the world.

Do you believe that the second phase could have come into existence without the first?

The hypothesis of central forces contained all the principles; it involved them as necessary consequences; it involved both the conservation of energy and that of masses, and the equality of action and reaction; and the law of least action, which would appear, it is true, not as experimental verities, but as theorems and of which the enunciation would have at the same time a something more precise and less general than under their actual form.

It is the mathematical physics of our fathers which has familiarized us little with these divers principles; which has habituated us to recognize them under the different vestments in which they disguise themselves. One has compared them to the data of experience, or has seen how it was necessary to modify their enunciation to adapt them to these data; thereby they have been enlarged and consolidated.

So one has been led to regard them as experimental verities; the conception of central forces became then a useless support, or rather an embarrassment, since it made the principles partake of its hypothetical character.

The frames have not therefore broken, because they were elastic; but they have enlarged; our fathers, who established them, did not work in vain, and we recognize in the science of today the general traits of the sketch which they traced.

Are we about to enter now upon the eve of a second crisis? These principles on which we have built all—are they about to crumble away in their turn? Since some time, this may well be asked.

In hearing me speak thus, you think without doubt of radium, that grand revolutionist of the present time, and in fact I will come back to it presently; but there is something else.

It is not alone the conservation of energy which is in question; all the other principles are equally in danger, as we shall see in passing them successively in review.

Let us commence with the principle of Carnot.

This is the only one which does not present itself as an immediate consequence of the hypothesis of central forces; more than that, it seems, if not to directly contradict that hypothesis, at least not to be reconciled with it without a certain effort.

If physical phenomena were due exclusively to the movements of atoms whose mutual attraction depended only on the distance, it seems that all these phenomena should be reversible; if all the initial velocities were reversed, these atoms, always subjected to the same forces, ought to go over their trajectories in the contrary sense, just as the earth would describe in the retrograde sense this same elliptic orbit which it describes in the direct sense, if the initial conditions of its movement had been reversed. On this account, if a physical phenomenon is possible, the inverse phenomenon should be equally so, and one should be able to reascend the course of time.

But it is not so in nature, and this is precisely what the principle of Carnot teaches us; heat can pass from the warm body to the cold body; it is impossible afterwards to make it reascend the inverse way and reestablish differences of temperature which have been effaced.

Motion can be wholly dissipated and transformed into heat by friction; the contrary transformation can never be made except in a partial manner.

We have striven to reconcile this apparent contradiction. If the world tends toward uniformity, this is not because its ultimate parts, at first unlike, tend to become less and less different; it is because, shifting at hazard, they end by blending. For an eye which should distinguish all the elements, the variety would remain always as great, each grain of this dust preserves its originality and does not model itself on its neighbors; but as the blend becomes more and more intimate, our gross senses perceive no more than the uniformity. Behold why, for example, temperatures tend to a level, without the possibility of turning backwards.

A drop of wine falls into a glass of water; whatever may be the law of the internal movements of the liquid, we soon see it colored of a uniform rosy tint and from this moment, one may well shake the vase, the wine and the water do not seem able any more to separate. See, thus, what would be the type of the reversible physical phenomenon: to hide a grain of barley in a cup of wheat, this is easy; afterwards to find it again and get it out, this is practically impossible.

All this Maxwell and Boltzmann have explained; the one who has seen it most clearly, in a book too little read because it is a little difficult to

read, is Gibbs, in his *Elementary Principles of Statistical Mechanics*.

For those who take this point of view, the principle of Carnot is only an imperfect principle, a sort of concession to the infirmity of our senses; it is because our eyes are too gross that we do not distinguish the elements of the blend; it is because our hands are too gross that we cannot force them to separate; the imaginary demon of Maxwell, who is able to sort the molecules one by one, could well constrain the world to return backward. Can it return of itself? That is not impossible; that is only infinitely improbable.

The chances are that we should long await the concurrence of circumstances which would permit a retrogradation, but soon or late, they would be realized, after years whose number it would take millions of figures to write.

These reservations, however, all remained theoretic and were not very disquieting, and the principle of Carnot retained all its practical value.

But here the scene changes.

The biologist, armed with his microscope, long ago noticed in his preparations disorderly movements of little particles in suspension; this is the Brownian movement; he first thought this was a vital phenomenon, but soon he saw that the inanimate bodies danced with no less ardor than the others; then he turned the matter over to the physicists. Unhappily, the physicists remained long uninterested in this question; one concentrates the light to illuminate the microscopic preparation, thought they; with light goes heat; thence inequalities of temperature and in the liquid interior currents which produce the movements of which we speak.

M. Gouy had the idea to look more closely, and he saw or thought he saw that this explanation is untenable, that the movements become more brisk as the particles are smaller, but that they are not influenced by the mode of illumination.

If then these movements never cease, or rather are reborn without cease, without borrowing anything from an external source of energy, what ought we to believe? To be sure, we should not renounce our belief in the conservation of energy, but we see under our eyes now motion transformed into heat by friction, now heat changed inversely into motion, and that without loss since the movement lasts forever. This is the contrary of the principle of Carnot.

If this be so, to see the world return backward, we no longer have need of the infinitely subtle eye of Maxwell's demon; our microscope suffices us. Bodies too large—those, for example, which are a

tenth of a millimeter—are hit from all sides by moving atoms, but they do not budge, because these shocks are very numerous and the law of chance makes them compensate each other; but the smaller particles receive too few shocks for this compensation to take place with certainty and are incessantly knocked about. And behold already one of our principles in peril.

We come to the principle of relativity: this not only is confirmed by daily experience, not only is it a necessary consequence of the hypothesis of central forces, but it is imposed in an irresistible way upon our good sense, and yet it also is battered.

Consider two electrified bodies; though they seem to us at rest, they are both carried along by the motion of the earth; an electric charge in motion, Rowland has taught us, is equivalent to a current; these two charged bodies are, therefore, equivalent to two parallel currents of the same sense and these two currents should attract each other. In measuring this attraction, we measure the velocity of the earth—not its velocity in relation to the sun or the fixed stars, but its absolute velocity.

I well know what one will say—it is not its absolute velocity that is measured, it is its velocity in relation to the ether. How unsatisfactory that is! Is it not evident that from the principle so understood we could no longer get anything? It could no longer tell us anything just because it would no longer fear any contradiction.

If we succeed in measuring anything, we would always be free to say that this is not the absolute velocity in relation to the ether, it might always be the velocity in relation to some new unknown fluid with which we might fill space.

Indeed, experience has taken on itself to ruin this interpretation of the principle of relativity; all attempts to measure the velocity of the earth in relation to the ether have led to negative results. This time experimental physics has been more faithful to the principle than mathematical physics; the theorists, to put in accord their other general views, would not have spared it; but experiment has been stubborn in confirming it.

The means have been varied in a thousand ways and finally Michelson has pushed precision to its last limits; nothing has come of it. It is precisely to explain this obstinacy that the mathematicians are forced today to employ all their ingenuity.

Their task was not easy, and if Lorentz has gotten through it, it is only by accumulating hypotheses. The most ingenious idea has been that of local time.

Imagine two observers who wish to adjust their

watches by optical signals; they exchange signals, but as they know that the transmission of light is not instantaneous, they take care to cross them.

When the station B perceives the signal from station A, its clock should not mark the same hour as that of the station A at the moment of sending the signal, but this hour augmented by a constant representing the duration of the transmission. Suppose, for example, that the station A sends its signal when its clock marks the hour o , and that the station B perceives it when its clock marks the hour t . The clocks are adjusted if the slowness equal to t represents the duration of the transmission, and to verify it, the station B sends in its turn a signal when its clock marks o ; then the station A should perceive it when its clock marks t . The timepieces are then adjusted. And in fact, they mark the same hour at the same physical instant, but on one condition, which is that the two stations are fixed. In the contrary case, the duration of the transmission will not be the same in the two senses, since the station A, for example, moves forward to meet the optical perturbation emanating from B, while the station B flies away before the perturbation emanating from A. The watches adjusted in that manner do not mark, therefore, the true time—they mark what one may call the *local time*, so that one of them goes slow on the other. It matters little since we have no means of perceiving it. All the phenomena which happen at A, for example, will be late, but all will be equally so, and the observer who ascertains them will not perceive it since his watch is slow; so as the principle of relativity would have it, he will have no means of knowing whether he is at rest or in absolute motion.

Unhappily, that does not suffice, and complementary hypotheses are necessary; it is necessary to admit that bodies in motion undergo a uniform contraction in the sense of the motion. One of the diameters of the earth, for example, is shrunk by $1/200,000,000$ in consequence of the motion of our planet, while the other diameter retains its normal length. Thus, the last little differences find themselves compensated. And, then, there still is the hypothesis about forces. Forces, whatever be their origin, gravity as well as elasticity, would be reduced in a certain proportion in a world animated by a uniform translation; or, rather, this would happen for the components perpendicular to the translation; the components parallel would not change.

Resume, then, our example of two electrified bodies; these bodies repel each other, but at the same time if all is carried along in a uniform trans-

lation, they are equivalent to two parallel currents of the same sense which attract each other. This electrodynamic attraction diminishes, therefore, the electrostatic repulsion, and the total repulsion is more feeble than if the two bodies were at rest. But since to measure this repulsion we must balance it by another force, and all these other forces are reduced in the same proportion, we perceive nothing.

Thus, all is arranged, but are all the doubts dissipated?

What would happen if one could communicate by nonluminous signals whose velocity of propagation differed from that of light? If, after having adjusted the watches by the optical procedure, one wished to verify the adjustment by the aid of these new signals, then would appear divergences which would render evident the common translation of the two stations. And are such signals inconceivable, if we admit with Laplace that universal gravitation is transmitted a million times more rapidly than light?

Thus, the principle of relativity has been valiantly defended in these latter times, but the very energy of the defense proves how serious was the attack.

Let us speak now of the principle of Newton, on the equality of action and reaction.

This is intimately bound up with the preceding, and it seems indeed that the fall of the one would involve that of the other. Thus we should not be astonished to find here the same difficulties.

Electrical phenomena, we think, are due to the displacements of little charged particles, called electrons, immersed in the medium that we call ether. The movements of these electrons produce perturbations in the neighboring ether; these perturbations propagate themselves in every direction with the velocity of light, and in turn other electrons, originally at rest, are made to vibrate when the perturbation reaches the parts of the ether which touch them.

The electrons, therefore, act on one another, but this action is not direct—it is accomplished through the ether as intermediary.

Under these conditions can there be compensation between action and reaction, at least for an observer who should take account only of the movements of matter—that is to say, of the electrons—and who should be ignorant of those of the ether that he could not see? Evidently not. Even if the compensation should be exact, it could not be simultaneous. The perturbation is propagated with a finite velocity; it therefore reaches the second electron only when the first has long ago entered upon its rest.

This second electron, therefore, will undergo, after a delay, the action of the first, but certainly it will not react on this, since around this first electron nothing any longer budes.

The analysis of the facts permits us to be still more precise. Imagine, for example, a Hertzian generator, like those employed in wireless telegraphy; it sends out energy in every direction; but we can provide it with a parabolic mirror, as Hertz did with his smallest generators, so as to send all the energy produced in a single direction.

What happens then according to the theory? It is that the apparatus recoils as if it were a gun and as if the energy it has projected were a bullet; and that is contrary to the principle of Newton, since our projectile here has no mass, it is not matter, it is energy.

It is still the same, moreover, with a beacon light provided with a reflector, since light is nothing but a perturbation of the electromagnetic field. This beacon light should recoil as if the light it sends out were a projectile. What is the force that this recoil should produce? It is what one has called the Maxwell-Bartholdi pressure. It is very minute, and it has been difficult to put it into evidence even with the most sensitive radiometers; but it suffices that it exists.

If all the energy issuing from our generator falls on a receiver, this will act as if it had received a mechanical shock, which will represent in a sense the compensation of the recoil of the generator: the reaction will be equal to the action, but it will not be simultaneous; the receiver will move on but not at the moment when the generator recoils. If the energy propagates itself indefinitely without encountering a receiver, the compensation will never be made.

Does one say that the space which separates the generator from the receiver and which the perturbation must pass over in going from the one to the other is not void, that it is full not only of ether, but of air; or even in the interplanetary spaces of some fluid subtle but still ponderable; that this matter undergoes the shock like the receiver at the moment when the energy reaches it, and recoils in its turn when the perturbation quits it? That would save the principle of Newton, but that is not true.

If energy in its diffusion remained always attached to some material substratum, then matter in motion would carry along light with it, and Fizeau has demonstrated that it does nothing of the sort, at least for air. This is what Michelson and Morley have since confirmed.

One may suppose also that the movements of matter, properly so called, are exactly compensated

by those of the ether; but that would lead us to the same reflections as just now. The principle so extended would explain everything, since whatever might be the visible movements, we would always have the power of imagining hypothetical movements which compensated them.

But if it is able to explain everything, this is because it does not permit us to foresee anything; it does not enable us to decide between different possible hypotheses, since it explains everything beforehand. It therefore becomes useless.

And then the suppositions that it would be necessary to make on the movements of the ether are not very satisfactory.

If the electric charges double, it would be natural to imagine that the velocities of the divers atoms of ether double also, and for the compensation, it would be necessary that the mean velocity of the ether quadruple.

This is why I have long thought that these consequences of theory, contrary to the principle of Newton, would end some day by being abandoned, and yet the recent experiments on the movements of the electrons issuing from radium seem rather to confirm them.

I arrive at the principle of Lavoisier on the conservation of masses: certainly, this is one not to be touched without unsettling all mechanics.

And now certain persons think that it seems true to us only because one considers in mechanics merely moderate velocities, but that it would cease to be true for bodies animated by velocities comparable to that of light. Now these velocities, it is believed at present, they have been realized; the cathode rays or those of radium may be formed of very minute particles or of electrons which are displaced with velocities smaller no doubt than that of light, but which might be its one-tenth or one-third.

These rays can be deflected, whether by an electric field, or by a magnetic field, and we are able, by comparing these deflections, to measure at the same time the velocity of the electrons and their mass (or rather the relation of their mass to their charge). But when it was seen that these velocities approached that of light, it was decided that a correction was necessary.

These molecules, being electrified, could not be displaced without agitating the ether; to put them in motion it is necessary to overcome a double inertia, that of the molecule itself and that of the ether. The total or apparent mass that one measures is composed, therefore, of two parts: the real or mechanical mass of the molecule and the electrodynamic mass representing the inertia of the ether.

The calculations of Abraham and the experiments of Kaufmann have then shown that the mechanical mass, properly so called, is null, and that the mass of the electrons, or, at least, of the negative electrons, is of exclusively electrodynamic origin. This forces us to change the definition of mass; we cannot any longer distinguish mechanical mass and electrodynamic mass, since then the first would vanish; there is no mass other than electrodynamic inertia. But, in this case the mass can no longer be constant, it augments with the velocity, and it even depends on the direction, and a body animated by a notable velocity will not oppose the same inertia to the forces which tend to deflect it from its route, as to those which tend to accelerate or to retard its progress.

There is still a resource; the ultimate elements of bodies are electrons, some charged negatively, the others charged positively. The negative electrons have no mass, this is understood; but the positive electrons, from the little we know of them, seem much greater. Perhaps, they have, besides their electrodynamic mass, a true mechanical mass. The veritable mass of a body would, then, be the sum of the mechanical masses of its positive electrons, the negative electrons not counting; mass so defined could still be constant.

Alas, this resource also evades us. Recall what we have said of the principle of relativity and of the efforts made to save it. And it is not merely a principle which it is a question of saving, such are the indubitable results of the experiments of Michelson.

Lorentz has been obliged to suppose that all the forces, whatever be their origin, were affected with a coefficient in a medium animated by a uniform translation; this is not sufficient, it is still necessary, says he, that *the masses of all the particles be influenced by a translation to the same degree as the electromagnetic masses of the electrons*.

So the mechanical masses will vary in accordance with the same laws as the electrodynamic masses; they cannot, therefore, be constant.

Need I point out that the fall of the principle of Lavoisier involves that of the principle of Newton? This latter signifies that the center of gravity of an isolated system moves in a straight line; but if there is no longer a constant mass, there is no longer a center of gravity, we no longer know even what this is. This is why I just said that the experiments on the cathode rays appeared to justify the doubts of Lorentz on the subject of the principle of Newton.

From all these results, if they are confirmed, would arise an entirely new mechanics, which would be, above all, characterized by this fact, that

no velocity could surpass that of light, any more than any temperature could fall below the zero absolute, because bodies would oppose an increasing inertia to the causes, which would tend to accelerate their motion; and this inertia would become infinite when one approached the velocity of light.

No more for an observer carried along himself in a translation that he did not suspect could any apparent velocity surpass that of light; and this would be then a contradiction, if we recall that this observer would not use the same clocks as a fixed observer, but, indeed, clocks marking "local time."

Here we are then facing a question I content myself with stating. If there is no longer any mass, what becomes of the law of Newton?

Mass has two aspects, it is at the same time a coefficient of inertia and an attracting mass entering as factor into Newtonian attraction. If the coefficient of inertia is not constant, can the attracting mass be? That is the question.

At least, the principle of the conservation of energy yet remains to us, and this seems more solid. Shall I recall to you how it was in its turn thrown into discredit? This event has made more noise than the preceding and it is in all the memoirs.

From the first works of Becquerel, and, above all, when the Curies had discovered radium, one saw that every radioactive body was an inexhaustible source of radiations. Its activity would seem to subsist without alteration throughout the months and the years. This was already a strain on the principles; these radiations were in fact energy, and from the same morsel of radium this issued and forever issued. But these quantities of energy were too slight to be measured; at least one believed so and was not much disquieted.

The scene changed when Curie bethought himself to put radium in a calorimeter; one saw, then, that the quantity of heat incessantly created was very notable.

The explanations proposed were numerous; but in such case we cannot say, "store is no sore."

Insofar as no one of them has prevailed over the others, we cannot be sure there is a good one among them.

William Ramsay has striven to show that radium is in process of transformation, that it contains a store of energy enormous but not inexhaustible.

The transformation of radium then would produce a million times more of heat than all known transformations; radium would wear itself out in 1250 years; you see that we are at least certain to be settled on this point some hundreds of years from now. While waiting our doubts remain.

In the midst of so many ruins what remains

standing? The principle of least action is hitherto intact, and Larmor appears to believe that it will long survive the others; in reality, it is still more vague and more general.

In presence of this general ruin of the principles, what attitude will mathematical physics take?

And first, before too much excitement, it is proper to ask if all that is really true. All these derogations to the principles are encountered only among infinitesimals; the microscope is necessary to see the Brownian movement; electrons are very light; radium is very rare, and one never has more than some milligrams of it at a time.

And, then, it may be asked if, beside the infinitesimal seen, there be not another infinitesimal unseen counterpoise to the first.

So, there is an interlocutory question, and, as it seems, only experiment can solve it. We have, therefore, only to hand over the matter to the experimenters, and while waiting for them to finally decide the debate, not to preoccupy ourselves with these disquieting problems, and to tranquilly continue our work, as if the principles were still untested. Certainly, we have much to do without leaving the domain where they may be applied in all security; we have enough to employ our activity during this period of doubts.

And as to these doubts, is it indeed true that we can do nothing to disembarass science of them? It may be said, it is not alone experimental physics that has given birth to them; mathematical physics has well contributed. It is the experimenters who have seen radium throw out energy, but it is the theorists who have put in evidence all the difficulties raised by the propagation of light across a medium in motion; but for these it is probable we should not have become conscious of them. Well, then, if they have done their best to put us into this embarrassment, it is proper also that they help us to get out of it.

They must subject to critical examination all these new views I have just outlined before you, and abandon the principles only after having made a loyal effort to save them.

What can they do in this sense? That is what I will try to explain.

Among the most interesting problems of mathematical physics, it is proper to give a special place to those relating to the kinetic theory of gases. Much has already been done in this direction, but much still remains to be done. This theory is an eternal paradox. We have reversibility in the premises and irreversibility in the conclusions; and between the two an abyss. Statistic considerations, the law of great numbers, do they suffice to fill it? Many points still remain obscure to which it is

necessary to return, and doubtless many times. In clearing them up, we will understand better the sense of the principle of Carnot and its place in the ensemble of dynamics, and we will be better armed to properly interpret the curious experiment of Gouy, of which I have spoken.

Should we not also endeavor to obtain a more satisfactory theory of the electrodynamics of bodies in motion? It is there especially, as I have sufficiently shown, that difficulties accumulate. Evidently we must heap up hypotheses, we cannot satisfy all the principles at once; heretofore, one has succeeded in safeguarding some only on condition of sacrificing the others; but all hope of obtaining better results is not yet lost. Let us take, therefore, the theory of Lorentz, turn it in all senses, modify it little by little, and perhaps everything will arrange itself.

Thus in place of supposing that bodies in motion undergo a contraction in the sense of the motion, and that this contraction is the same whatever be the nature of these bodies and the forces to which they are otherwise submitted, could we not make a hypothesis more simple and more natural?

We might imagine, for example, that it is the ether which is modified when it is in relative motion in reference to the material medium which it penetrates, that when it is thus modified, it no longer transmits perturbations with the same velocity in every direction. It might transmit more rapidly those which are propagated parallel to the medium, whether in the same sense or in the opposite sense, and less rapidly those which are propagated perpendicularly. The wave surfaces would no longer be spheres, but ellipsoids, and we could dispense with that extraordinary contraction of all bodies.

I cite that only as an example, since the modifications, one might essay, would be evidently susceptible of infinite variation.

It is possible also that astronomy may some day furnish us data on this point; she it was in the main who raised the question in making us acquainted with the phenomenon of the aberration of light. If we make crudely the theory of aberration, we reach a very curious result. The apparent positions of the stars differ from their real positions because of the motion of the earth, and as this motion is variable, these apparent positions vary. The real position we cannot know, but we can observe the variations of the apparent position. The observations of the aberration show us, therefore, not the movement of the earth, but the variations of this movement; they cannot, therefore, give us information about the absolute motion of the earth. At least this is true in first approximation, but it would be no longer the same if we could appreciate the

thousandths of a second. Then it would be seen that the amplitude of the oscillation depends not alone on the variation of the motion, variation which is well known, since it is the motion of our globe on its elliptic orbit, but on the mean value of this motion; so that the constant of aberration would not be altogether the same for all the stars, and the differences would tell us the absolute motion of the earth in space.

This, then, would be, under another form, the ruin of the principle of relativity. We are far, it is true, from appreciating the thousandths of a second, but after all, say some, the total absolute velocity of the earth may be much greater than its relative velocity with respect to the sun. If, for example, it were 300 kilometers per second in place of 30, this would suffice to make the phenomena observable.

I believe that in reasoning thus one admits a too simple theory of aberration. Michelson has shown us, I have told you, that the physical procedures are powerless to put in evidence absolute motion; I am persuaded that the same will be true of the astronomic procedures, however far one pushes precision.

However that may be, the data astronomy will furnish us in this regard will some day be precious to the physicist. While waiting, I believe, the theorists, recalling the experience of Michelson, may anticipate a negative result, and that they would accomplish a useful work in constructing a theory of aberration which would explain this in advance.

But let us come back to the earth. There also we may aid the experimenters. We can, for example, prepare the ground by studying profoundly the dynamics of electrons; not be it understood in starting from a single hypothesis, but in multiplying hypotheses as much as possible. It will be then for the physicists to utilize our work in seeking the crucial experiment to decide between these different hypotheses.

This dynamics of electrons can be approached from many sides, but among the ways leading thither is one which has been somewhat neglected, and yet this is one of those which promise us most of surprises. It is the movements of the electrons which produce the line of the emission spectra; this is proved by the phenomenon of Zeemann; in an incandescent body, what vibrates is sensitive to the magnet, therefore electrified. This is a very important first point, but no one has gone farther; why are the lines of the spectrum distributed in accordance with a regular law?

These laws have been studied by the experimenters in their least details; they are very precise and relatively simple. The first study of these dis-

tributions recalled the harmonics encountered in acoustics; but the difference is great. Not only the numbers of vibrations are not the successive multiples of one same number, but even we do not find anything analogous to the roots of those transcendental equations to which so many problems of mathematical physics conduct us: that of the vibrations of an elastic body of any form, that of the Hertzian oscillations in a generator of any form, the problem of Fourier for the cooling of a solid body.

The laws are simpler, but they are of wholly other nature, and to cite only one of these differences, for the harmonics of high order the number of vibrations tends toward a finite limit, instead of increasing indefinitely.

That has not yet been accounted for, and I believe that there we have one of the most important secrets of nature. Lindemann has made a praiseworthy attempt, but, to my mind, without success; this attempt should be renewed. Thus we will penetrate, so to say, into the inmost recess of matter. And from the particular point of view which we today occupy, when we know why the vibrations of incandescent bodies differ from ordinary elastic vibrations, why the electrons do not behave themselves like the matter which is familiar to us, we will better comprehend the dynamics of electrons and it will be perhaps more easy for us to reconcile it with the principles.

Suppose, now, that all these efforts fail, and after all I do not believe they will, what must be done? Will it be necessary to seek to mend the broken principles in giving what we French call a *coup de pinceau*? That is evidently always possible, and I retract nothing I have formerly said.

Have you not written, you might say if you wished to seek a quarrel with me, have you not written that the principles, though of experimental origin, are now unassailable by experiment because they have become conventions? And now you have just told us the most recent conquests of experiment put these principles in danger. Well, formerly I was right and today I am not wrong.

Formerly I was right, and what is now happening is a new proof of it. Take for example the calorimeter experiment of Curie on radium. Is it possible to reconcile that with the principle of the conservation of energy?

It has been attempted in many ways; but there is among them one I should like you to notice.

It has been conjectured that radium was only an intermediary, that it only stored radiations of unknown nature which flashed through space in every direction, traversing all bodies, save radium, without being altered by this passage and without

exercising any action upon them. Radium alone took from them a little of their energy and afterward gave it out to us in divers forms.

What an advantageous explanation, and how convenient! First, it is unverifiable and thus irrefutable. Then again it will serve to account for any derogation whatever to the principle of Mayer; it responds in advance not only to the objection of Curie, but to all the objections that future experimenters might accumulate. This energy new and unknown would serve for everything. This is just what I have said, and therewith we are shown that our principle is unassailable by experiment.

And after all, what have we gained by this *coup de pinceau*?

The principle is intact, but thenceforth of what use is it?

It permitted us to foresee that in such or such circumstance we could count on such a total quantity of energy; it limited us; but now that one puts at our disposition this indefinite provision of new energy, we are limited by nothing; and, as I have written also, if a principle ceases to be fecund, experiment, without contradicting it directly, will however have condemned it.

This, therefore, is not what would have to be done, it would be necessary to rebuild anew.

If we were cornered down to this necessity, we should moreover console ourselves. It would not be necessary thence to conclude that science can weave only a Penelope's web, that it can build only ephemeral constructions, which it is soon forced to demolish from top to bottom with its own hands.

As I have said, we have already passed through a like crisis. I have shown you that in the second mathematical physics, that of the principles, we find traces of the first, that of the central forces; it will be just the same if we must learn a third.

Of such an animal as exuviates, as breaks its too narrow carapace and makes itself a fresh one, under the new envelope we easily recognize the essential traits of the organism which have subsisted.

We cannot foresee in what way we are about to expand; perhaps it is the kinetic theory of gases which is about to undergo development and serve as model to the others. Then, the facts which first appeared to us as simple, thereafter will be merely results of a very great number of elementary facts which only the laws of chance make cooperate for a common end. Physical law will then take an entirely new aspect; it will no longer be solely a differential equation, it will take the character of a statistical law.

Perhaps likewise, we should construct a whole new mechanics, that we only succeed in catching a glimpse of, where inertia increasing with the

velocity, the velocity of light would become an impassable limit.

The ordinary mechanics, more simple, would remain a first approximation, since it would be true for velocities not too great, so that one would still find the old dynamics under the new.

We should not have to regret having believed in the principles, and even, since velocities too great for the old formulas would always be only excep-

tional, the surest way in practice would be still to act as if we continued to believe in them. They are so useful, it would be necessary to keep a place for them. To determine to exclude them altogether would be to deprive oneself of a precious weapon. I hasten to say in conclusion we are not yet there, and as yet nothing proves that the principles will not come forth from the combat victorious and intact.

A Neurosurgeon Looks at Sculpture

This month's cover is a photograph of a portrait in bronze of Albert Einstein by Emil Seletz, which will be unveiled in the rotunda of the Jewish Community Building in Los Angeles, California, on 18 April, the first anniversary of Dr. Einstein's death. Dr. Seletz is author of *Surgery of Peripheral Nerves*. At present he is assistant professor of neurologic surgery at the University of Southern California School of Medicine, chief of neurologic surgery at Cedars of Lebanon Hospital, and senior attending neurologic surgeon to the Los Angeles County General Hospital. Describing his approach, as a physician, to sculpture, Dr. Seletz says:

"A portrait bust is a biographic sketch of an individual, recorded for posterity, in bronze or stone. But if a piece of sculpture is to have enduring value, it must possess much more than physical likeness. It must possess personality, true character, a suggestion of the subject's life, aims, purpose, state of mind, scope of thinking, strength, weaknesses and possibly, the field of his endeavor.

"The neurologic surgeon's entire life is spent in studying intimately the face of the patient, minutely observing facial expression. For while there are innumerable tests to aid in the diagnosis of some diseases of the brain and nervous system, there are still a greater number of brain affections and mental aberrations for which there are no diagnostic tests, save the physician's keen sense of observation. The sick face, surely, teaches much, for it is a face stripped of vanity, devoid of subterfuge and sham. It is a face starkly real with pain, apprehension, and the desperate need for help. These faces, more than any reading of textbooks or study of great works of art, have taught me to read character and motive in the face of my sitter. So, as my patients' faces became my textbook in medicine, with time, my sitter became my teacher in art.

"Einstein's face was both a lure and an enigma to me. I saw in that wonderful face much to record. Yet, how can a mere mortal hope, in static form, to reveal the unfathomable genius of Einstein? I worked with humility, intent more upon the grave nobility of the man than his vast mentality."