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PRESENT TENDENCIES IN THEORETICAL PHYSICS¹

At a time like the present, when the minds of all of us are intent upon the war and the great issues which depend upon it, it seems almost an affectation to discuss before you a subject so remote from "the instant need of things" as the methods and outlook of theoretical physics. The custom of many years, however, constrains the sectional vice-president to deliver an address. The many questions raised by the war and the relation of science to war have been so thoroughly discussed that I should certainly not be justified in inflicting upon you at great length my own views. The only alternative, therefore, to an appearance of detachment, which I am far from feeling, would have been the abolition for this year of the vice-presidential address before Section B—a measure of war-economy which would have commanded my hearty and unqualified support.

When, however, we turn our minds to a consideration of the recent development of our science, we are confronted at once with the unmistakable fact that there has been little progress since August, 1914, in either theoretical or experimental physics. We had become accustomed to a steady succession, year by year, of important experimental discoveries and of ingenious and original theoretical discussions; we need mention only a few—the Stark Effect, the crystalline diffraction of X-rays, Onnes's

¹ Address of the vice-president and chairman of Section B—Physics—American Association for the Advancement of Science, Pittsburgh, December, 1917.

superconductivity, Debye's theory of specific heats, the Rutherford nucleus atom, the existence of chemical isotopes, Bohr's theory, Moseley's law, Einstein's theory of gravitation. I do not recall anything comparable with these in interest and importance which has appeared during the past three years. Whatever services science may render to war, it is plain that a state of war is not favorable to the progress of science. Accordingly, the word "present" in my title must be interpreted with some latitude; it really applies to the state of things before the peaceful labor of physicists was interrupted by the duty of turning their attention to problems in applied science whose solution is of immediate urgency.

No one can doubt that there has been something very like a revolution in the ideas and methods of theoretical physics since the beginning of the twentieth century. Much recent work of undoubted significance would seem very strange to Helmholtz and Lord Kelvin; and even in some of our own contemporaries whose tastes are conservative, it excites feelings similar to those experienced by a Royal Academician before a cubist painting. On the other hand, some of our younger and more enthusiastic colleagues are inclined to be impatient of what they call "classical" theories (some of which were perfected in the 1890's), and to regard them as examples of superstition and logical punctilio from which they have been happily freed.

The truth is of course to be found between the two extreme views. We must recognize that this is not the first change in physical science which has seemed at the time to be revolutionary. In the past, these changes have never been so complete and overwhelming as was expected by their supporters, nor so abortive as hoped by their opponents. In science, as in art, pol-

itics and religion, the radicals are always partly right and the conservatives never wholly wrong; and the interplay and conflict between the two is of the very essence of progress.

One of the most striking things about the modern beginnings of our science—the preliminary formulation of the principles of mechanics by Galileo, and their more complete development by Newton—was their almost immediate acceptance by all who were not blinded by theological prejudices. This can not have been because they were simple or easy to formulate, or the world would not have had to wait so many centuries for them. But the phenomena of mechanics are directly and explicitly presented to us from our earliest childhood, and have been so presented to our long line of ancestors, human and pre-human. Under given conditions, certain mechanical actions are almost as confidently expected (even by quite uninstructed persons) as if their knowledge was of the *a priori* character that is attributed by many philosophers to our mathematical and spatial concepts. Even animals share this mechanical knowledge. The instinctive movements of a cat, which enable it to land upon its feet, could scarcely be improved upon if it possessed a satisfactory knowledge of the conservation of angular momentum. The difficulty of formulation was doubtless due to the lack of recognition of the true character of frictional and dissipative forces, and to the obscuring of the idea of mass by the more conspicuous property of weight. At all events, when the principles are once presented to the normal, intelligent, observant mind, they are quickly recognized, and soon come to seem almost as axiomatic as the attributes of space and number. There can be little doubt of the reality of this mechanical "intuition," be its origin what it may. Whatever the philosophers

may think of it in their moments of sophisticated philosophizing, there can be no doubt that they, in common with less instructed people, have a feeling of satisfaction and intellectual rest when an adequate mechanical "explanation" is given of some natural phenomenon.

Newton, with the characteristic boldness of genius, extended the Galilean mechanics of earthly matter to the heavenly bodies, and (as often happens) found in the remoter phenomena better and more complete confirmation of his theory than in the nearer and more obvious manifestations. With the single additional assumption of the gravitational force, all the intricate wanderings of sun, moon and planets in the celestial sphere fell into a system—simple, orderly and in accord with our commonest experiences of every-day life. It is not surprising that to all minds capable of understanding it, Newton's theory carried instant conviction.

Nature and Nature's Laws were hid in night,
God said, "Let Newton be," and all was light.

But the law of gravitation did not enjoy the same independent status in the minds of natural philosophers; from that day to this they have been under temptation to find what we all call an "explanation" of it, while few if any have ever felt the necessity for an explanation of the laws of motion. Newton himself, in the "Opticks," speculates as to a possible ethereal explanation of gravitation; and even in the celebrated passage at the end of the "Principia," in which he renounces hypotheses, the context shows, I think, that he felt strongly the desire for an explanation, but was compelled to forego it because "hitherto I have not been able to discover the causes of those properties of gravity from phenomena."

The century following Newton was de-

voted to the development of mechanics and of gravitational astronomy and culminated in the great achievements of Lagrange and Laplace. There was some discussion as to the relative merits of action at a distance and *vis a tergo*, and some direct attempts to account for gravitation on the latter basis—notably that of LeSage early in the nineteenth century. But, on the whole, the opinion gained strength that Newton had been right in his view that there was little hope of being able to test such theories by comparison with "phenomena."

The discovery by Coulomb that magnetic and electric forces conformed to the Newtonian law gave strength to the prevalent opinion that this law was fundamental in the constitution of the physical universe. The mathematical technique of the subject was highly developed and there was a growing tendency to explain observed phenomena by distance-forces between particles, rather than to seek a more strictly dynamical theory to account for such forces. This procedure was certainly defensible upon philosophical grounds, and proved its utility in many problems of mathematical physics. It was the prevailing fashion in the early part of the nineteenth century.

Thus it was entirely natural that Ampère, when he heard in 1820 of Oersted's discovery, should have based his investigation of electrodynamics upon the Newtonian model, by using current-elements acting upon each other by forces in the line joining them. Again the law proved to be that of the inverse square; but the fact that the attracting elements were directed quantities added many difficulties which, in the state of mathematical science at that time, gave ample scope to the "Newton of electricity" for the display of his genius. These vector relations involve an indeterminateness which later gave rise to many rivals to Ampère's theory; other expres-

sions for the forces between elements gave the same results when integrated around closed circuits, and no one succeeded in devising experiments which would discriminate between them. Even Ampère, however (like the great predecessor whose name Maxwell connected with his), was not immune from the inherent desire of the physicist for "explanations" of distance-forces, though he was compelled to forego them because no way appeared for putting them to an experimental test. At the beginning of the memoir,² in which he sums up his electrodynamic researches, after declaring his adherence to the Newtonian procedure and renouncing anything in the nature of Cartesian vortices which Oersted's discovery had in a measure revived, he says:

I have made no attempt to find the cause of these forces, well persuaded that any attempt of this kind ought to be preceded by the purely experimental knowledge of the laws and by the determination from these laws alone of the value of the elementary forces, whose direction is necessarily that of the straight line drawn through the material points between which the forces act.

Later in the same memoir³ he disclaims any intention to assert that his forces are to be regarded as "truly elementary" and calls attention to previous attempts of his own⁴ to "assign a cause for these forces in the reactions of the fluid filling all space whose vibrations produce the phenomena of light."

Simultaneously with these developments and partly in consequence of some of them, the employment of imponderable fluids became very general in theoretical physics. In electrostatics and magnetism, the gravitational analogy required some sort of attracting or repelling substance;

in the theory of heat, the calorimetrical experiments of Black and his clear discrimination between temperature and quantity of heat, led directly to a substantial theory of heat. There was no great encouragement for the attempt to apply the principles of mechanics to these imponderables; so far as experiment showed they lacked not only the conspicuous property of weight, but also the most essential dynamical characteristic of ordinary matter, viz., inertia. The natural and fertile method of dealing with them was to take some empirical relations, as simple and fundamental as possible, as postulates for the mathematical development of the subject. Some of the most epoch-making advances in theoretical physics are instances of this method; as examples one needs only to recall Fourier's theory of heat conduction (afterwards applied by Ohm to the conduction of electricity), and Carnot's deduction of the theory of heat engines from the empirical principle which we now call the second law of thermodynamics. In fact, the great physicists who flourished during the first three or four decades of the nineteenth century seem to have felt that there was little hope of giving dynamical explanations of all physical phenomena. Thus Fourier, in the introduction of his great work, recounting the glorious achievements of Newton and his successors, says:

It is recognized that the same principles regulate all the movements of the stars, their form, the inequalities of their courses, the equilibrium and oscillations of the seas, the harmonic vibrations of air and sonorous bodies, the transmission of light, capillary action, the undulations of fluids, in fine the most complex effects of all the natural forces; and thus has the thought of Newton been confirmed; *quod tam paucis tam multa præstet geometria gloriatur.*

"But," continues Fourier, "whatever may be the range of mechanical theories, they do not apply to the effects of heat.

² *Mem. de l'Acad.*, VI., p. 177 (1825).

³ P. 294.

⁴ "Recueil d'observations electro-dynamiques," p. 215.

These make up a special order of phenomena which can not be explained by the principles of motion and equilibrium." His attitude may fairly be taken as, in general, characteristic of his time; there were sharp lines of demarkation between the different departments of natural philosophy which would doubtless cause feelings of surprise and discomfort to a modern physicist if he could suddenly find himself at a meeting of the Royal Society or of the Paris Academy in the year 1822. These barriers were to a considerable extent broken down in the forties by the discovery and development of the principle of the conservation of energy. It was not simply the quantitative relation which excited the enthusiasm of the men of that time, but the knowledge that there was a "correlation of Physical Forces"; their reception of the discovery shows how much such a relation had been wanted.

The psychology of physicists made it inevitable that energy should be regarded as something more real than a mathematical expression which remains constant during various processes. It was given a quasi-substantial interpretation and localized in space; and it was most natural that its newly recognized forms should be identified as nearly as possible with the familiar energy of ordinary mechanics. Thus we had at once a *mechanical* theory of heat which led to a great extension of molecular hypotheses; and the desire to deduce the empirical second law from dynamical principles was the motive for the development of statistical mechanics through the successive stages shown in the work of Maxwell, Boltzmann and Gibbs.

This tendency toward dynamical explanations was strengthened by the progress of the wave-theory of light. After the brilliant experiments and interpretations

of Young and Fresnel it was impossible to doubt the kinematical similarity of light to a transverse wave motion. This made it necessary to postulate an ether and to give it suitable properties; the theory of waves in an ordinary material elastic solid was developed by Green, Cauchy, Thomson and others and compared with the phenomena of light. The lack of complete agreement was a stimulus to the investigation of other possible types of elastic substances conforming to the general laws of mechanics. In the hands of MacCullagh, Stokes, and especially of Kelvin, these investigations led to great advances in our knowledge of the properties of continuous media, and showed the dynamical possibility of the existence of media which were quite different in their elastic properties from ordinary matter.

Another current of thought which influenced profoundly the complex development of theoretical physics in the nineteenth century was the strong prejudice of Faraday against action at a distance and his instinctive preference for a mode of representation which involved the transfer of forces from point to point by the interaction of contiguous parts of a continuous medium. The fertility and usefulness of this method in electromagnetism is attested not only by Faraday's unparalleled success as a discoverer (for genius chooses the method best suited to itself), but also by the fact that it has held the field in elementary instruction as well as in the most complicated applications of electrical engineering. We all know how Maxwell deliberately submitted himself to the influence of this prejudice, and the epoch-making result which followed from its union with his mathematical skill. The inclusion in a single theory of two great bodies of phenomena, those of light and those of electricity, was an achievement of

the first magnitude and an immense stride in the direction of the unification of natural causes. But it did not satisfy the thoroughgoing dynamical prejudices of Lord Kelvin, who insisted to the end of his life that he did not "understand" the electromagnetic theory and that it "has not helped us hitherto." Maxwell himself was scarcely less desirous of finding a dynamical foundation for his theory. In fact, its first form was a detailed mechanical model of vortices and idle wheels; in the final form details were avoided by the use of the generalized dynamics of Lagrange and Hamilton, and Maxwell succeeded in showing that certain parts at least of his theory could be based upon dynamical principles.

This use of Lagrangian and Hamiltonian methods in the investigation of physical phenomena was a new weapon in the hands of those who sought to reduce them all to a dynamical basis. It has been used with effect by J. J. Thomson, Larmor, and (in application to statistical mechanics) by Gibbs. It makes feasible the ultimate refinement and completeness of dynamical explanation; in place of the potential energy in the Lagrangian function we may substitute the kinetic energy of concealed motions and thus the last vestige of unexplained distance-forces may be swept away.

The most thoroughgoing and successful example of this method is the very comprehensive theory of the physical universe contained in Larmor's "*Æther and Matter*" published in the last year of the nineteenth century. His ether is identical with MacCullagh's rotationally elastic medium; it has imbedded in it centers of rotational strain (the electrons), out of which the atoms of matter may be built up. The only assumptions are that the positive and negative electrons are somehow prevented from destroying each other and that they,

with their fields of strain, are capable of motion through the fixed medium. From Hamilton's principle, the Maxwellian equations for the free ether are deduced and, in the presence of matter (electrons), whether at rest or in motion, the same relations hold as those found experimentally. The rotational elasticity of the medium may be produced gyrostatically, so that the potential energy may, if one chooses, be replaced by kinetic. It is interesting to observe that the position, velocity and momentum of a material particle, in this theory, are really Lagrangian, generalized values. The motion of the centers of strain (*e. g.* in a straight line) cause a slight twisting and untwisting motion of the ether where the true mass and momentum reside. Thus the apparent mass of Larmor's electron varies with its speed as that of cathode rays was afterward found to do; but its dynamical orthodoxy is as sound as that of a steam-engine governor, whose moment of inertia varies with its angular velocity.

Notwithstanding the triumphs of the dynamical school of thought, its assumptions and methods were subjected to searching criticism on philosophical grounds particularly by Kirchhoff and Mach. In Kirchhoff's "*Lectures on Mechanics*," published in 1876, he explicitly renounces the attempt to find the causes of natural phenomena or to "explain" them in the traditional sense; the purpose of mechanics itself (to say nothing of the parts of physics more remote from common observation) is simply the description of phenomena. Forces as *causes* of motion are rejected; they are merely convenient abbreviations for certain functions of observed motions. In the first lecture he points out that Newton by no means discovered that the force of gravitation was the cause of the motion of the planets which Kepler had described; he only

showed that the description was simpler and briefer if expressed in terms of the second differential coefficients instead of the first. Similar ideas have been developed with greater generality by Mach, who finds the final purpose of scientific theories to be economy of thought, and classes the search for causes, explanations and dynamical theories, among the metaphysical prejudices which hinder the progress of science.

The criticism of Kirchhoff and Mach is logical and convincing. No unprejudiced person can doubt that, after a discovery is made, it may be interpreted in their way and that, on the whole, this interpretation is the cleanest, most rational and most free from human weakness. But from the pragmatic point of view, and in the light of experience of the course of science in the past, it may well be doubted if their attitude of mind is a useful one in the work of investigation and discovery as distinguished from subsequent criticism and clarification. A somewhat extreme example of militant advocacy of the descriptive method was furnished about twenty years ago by the school of energetics under the leadership of Ostwald. Any use of atomic hypotheses was by them regarded as evidence of feebleness of intellect and slavery to metaphysical prejudices. Their opinions were based upon an incomplete acquaintance with the state of physical knowledge even at that time; they were vigorously opposed in numerous papers by Boltzmann who demonstrated "the indispensability of atomistics in natural philosophy" in a most convincing manner. As we all know, the progress of experimental discovery has long since convinced the energeticians that no adequate description of material phenomena can be given without the use of atomic theories.

Boltzmann has also pointed out⁵ that even the most elaborate and detailed mechanical theories of Kelvin or Maxwell, for example, are regarded by their authors themselves merely as models; that description by means of models, if accurate and convenient, is quite as legitimate as description by means of differential equations; and that the method could be thus amply justified even on the most sophisticated philosophical principles.

It may, I think, safely be said that the most remarkable example in physical science of the purely descriptive theory—the one with the least taint of the fallacy of cause and effect—is Einstein's theory of relativity. All of us who studied our Maxwell in the early nineties or previous to that time, and who have kept an interested eye upon the progress of electrodynamics in the intervening years, are aware of the great difficulties which were encountered in the attempt to extend the Maxwellian electrodynamics to moving bodies. Maxwell and Hertz both went astray in that portion of the subject. We all remember how these difficulties were slowly cleared up, step by step, especially by the masterly work of Lorentz, but with important contributions by J. J. Thomson, Heaviside, Larmor, FitzGerald, Max Abraham and others. What we now call the electron theory had its origin in this attack upon the electrodynamics of moving matter, and was not the result of any prevision that within a few years we should be able to handle, and experiment with, the disembodied electrons themselves. The final puzzle was the reconciliation of the result of the Michelson-Morley experiment with the facts of aberration, the Fresnel "coefficient of entrainment" and other optical knowledge. Most of us can remember the great perplexity which this caused; and it

⁵ "Populäre Schriften," p. 1.

did not at first sight appear to be helped very much by FitzGerald's suggestion, contained in a memoir by Lodge⁶ "that the cohesive force between molecules, and, therefore, the size of bodies, may be a function of their direction of motion through the ether; and accordingly that the length and breadth of Michelson's stone-supporting block were differently affected, in what happened to be, either accidentally or for some unknown reason, a compensatory manner. This seemed a rather desperate dodge; and the impression was not removed until Lorentz (who had independently made the same suggestion) showed⁷ that just the right alteration of dimensions would take place if the intermolecular forces were of electrical origin. Later the experimental results of Rayleigh and of Brace forced him to the conclusion that the electron itself must be similarly contracted, and one of the consequences of his hypothesis was brilliantly verified by Bucherer.

Upon one who had followed step by step this slow and laborious, but highly interesting, course of development, with its constant action and reaction of theory and experiment upon each other, the impression of directness and simplicity made by Einstein's papers of 1905 can scarcely be exaggerated. The difficult and (at first sight) irreconcilable results of experiment, which the older theory had conscientiously "explained," were taken by Einstein as his postulates. There remained only to describe the world as it appears to an observer limited by these restrictive postulates; this proved to be (for Einstein) an apparently easy task and resulted in the Lorentz equations for bodies in motion, slightly improved, in that some relations which Lorentz had obtained only approxi-

mately were now exact. Since description and not mechanism is the essence of the method, it is unnecessary to postulate an ether; and since an observer at rest with reference to the ether would have no detectable advantage over one who was in motion, the assumption of an ether was not only useless, but actually in the way of clear description. This rejection of the ether has made Einstein's theory unpalatable to many physicists, while others (as well as many mathematicians) have been so carried away with its beauty and elegance that the use of the word ether is to them distinctly offensive. A simple rule, however, enables one to converse peaceably with either group separately; the same statements and arguments may be addressed to both, provided the word "observer" is substituted for "ether," or *vice versa*.

If we consider Einstein's theory from the pragmatic point of view we cannot fail to recognize that no new discoveries in electrodynamics have resulted from its suggestions. In this fact there appears to be support for the opinion that a theory of this type is not valuable as an instrument of research, but finds its proper place as a succinct summary of a body of knowledge after that knowledge has been acquired by other means. There are a number of considerations, however, which serve as a warning against this generalization, of which I will mention but two.

I would first call your attention to the fact that the development of thermodynamics, as based upon the two empirical laws, exemplifies a method which is very similar to that of Einstein; and we must all recognize its enormous services in the advancement of science. It has constantly served as the guide in important experimental investigations, and has predicted results which could scarcely have been foreseen on the basis of the more detailed

⁶ *Phil. Trans. R. S.*, 184, p. 749 (1893).

⁷ "Versuch einer Theorie," etc., § 92.

molecular and statistical theories. The converse is also true, as Boltzmann so stoutly maintained; and I think we must recognize that the progress of thermodynamics has been greatly facilitated by the interplay and mutual reaction of both types of theory.

The second example is a more direct one; it is the remarkable theory of gravitation in which the highly individual genius of Einstein has again manifested itself. It is too early to come to a definite conclusion as to its validity. It has had one striking verification in the deduction of the correct value for the unexplained motion of Mercury's perihelion; but this agreement may conceivably be due to accident and, in any case, its evidence is too slender to be regarded as establishing the theory. But we must face the distinct possibility of its ultimate success; and, in that case, we can not fail to recognize it as a brilliant triumph of the descriptive method. It is difficult to believe that any living physicist except Einstein could have constructed this theory even with the help of Minkowski's highly simplified method of description by means of four-dimensional geometry; but it is quite beyond belief that such a theory could have arisen at the present time by the use of any of the more usual methods of theoretical physics.

There is one further matter in this connection to which I should like to invite your attention. It is the question of the complete validity of Einstein's original postulate of relativity. There can be little doubt of its correctness when applied to motions of translation; speaking in terms of the ether, we may be reasonably confident that it is impossible to detect the effects of uniform translation relative to the ether. But little has been accomplished in extending the theory to motions of rotation; indeed, rotation has always been a

stumbling-block to a purely relative theory of motion, as soon as dynamical considerations are introduced. As Maxwell says:⁸

So far as regards the geometrical configuration of the earth and the heavenly bodies, it is evidently all the same

"Whether the sun predominant in heaven
Rise on the earth, or earth rise on the sun;
He from the east his flaming road begin,
Or she from west her silent course advance
With inoffensive pace that spinning sleeps
On her soft axle, while she paces even,
And bears thee soft with the smooth air along."⁹

But, as we all know, the plane of Foucault's pendulum remains fixed with reference to the stars, and this has usually been interpreted as proving by dynamical means the absolute rotation of the earth. The thoroughgoing relativist replies, however, that the contrary supposition is equally possible; it would merely require a restatement of the principles of mechanics which happen (for some unknown reason) to take on their simplest form when referred to axes fixed with respect to the stars. The new statement of the laws of motion would seem to us very unnatural, but the essential point is not their strangeness, but that they would be *different*. To cause them to transform into themselves, as Maxwell's equations do when subjected to the Lorentz-Einstein transformation, would apparently require curious assumptions of curved space, and of time recurrent after twenty-four-hour periods, which would certainly be very foreign to the ordinary habits and preferences of the human mind, whether we assume that these habits are inherent or acquired. Even from the point of view of convenient description it seems likely that we shall do better by adhering to the belief that the stars are fixed and that the earth rotates. We must, however,

⁸ "Matter and Motion," p. 154 (Van Nostrand, 1878).

⁹ "Paradise Lost," Book 8, ll. 160 et seq.

admit that relativists are quite within their rights when they demand an answer to the question, "Fixed with reference to what; rotates relative to what?" Here, it seems to me, is a possible field of usefulness for the ether in addition to its original function of serving as nominative case to the verb "to undulate." This appears the more likely when we consider that the earth's magnetism has never received an explanation—or, if one chooses, a description which connects it with other physical phenomena.

I have left to the end the consideration of the most revolutionary change which the twentieth century has brought about in the outlook and methods of theoretical physics—the rapid development and great successes of the quantum hypothesis of Planck. As we have seen, the fifty years following the discovery of the conservation of energy were marked by the steady progress of dynamical theories and the conquest by them of one disputed position after another. It is true that the victory was never quite complete, that the models were always in some degree imperfect and approximate; but the success was, on the whole so great that it seemed to justify the hope that only time and labor were necessary to clear away present difficulties as so many had been overcome in the past. It had not been easy to bring thermodynamics and irreversible processes into the dynamical system, but so far as material systems were concerned, most physicists were in agreement that it had been successfully done. It is true that a violation of the second law of thermodynamics could not be shown to be impossible; but its improbability was so great that there was no reasonable expectation of its ever being observed by finite human beings. The most complete and general exposition of this great

triumph of the dynamical hypothesis is contained in the "Statistical Mechanics" of Willard Gibbs, which was published in 1902, but which had been completed and given in the form of academic lectures by the author for some years previous to that date. As in all of Gibbs's work the assumptions and the results were of a very general character; but he was quite aware that at one point they were too restricted. He says:¹⁰

Although our only assumption is that we are considering conservative systems of a finite number of degrees of freedom, it would seem that this is assuming far too much, so far as the bodies of nature are concerned. The phenomena of radiant heat, which certainly should not be neglected in any complete system of thermodynamics, and the electrical phenomena associated with the combination of atoms, seem to show that the hypothesis of systems of a finite number of degrees of freedom is inadequate for the explanation of the properties of bodies.

The difficulties involved in the possession by the continuous ether of an infinite number of degrees of freedom were brought more clearly to light in 1900 by Lord Rayleigh's formula for black body radiation. It was quite irreconcilable with the measurements of Paschen and, moreover, it led to a kind of superdissipation of energy into high frequency vibrations of the ether which appeared entirely out of accord with the facts of empirical thermodynamics. Paschen's observations were well represented by the formula which had been obtained by Wien, who assumed the Maxwellian distribution of velocities among the molecules of the black radiator, and also that the wave-length radiated by any molecule was a function of its velocity. Later experiments by Lummer and Pringsheim and by Rubens and Kurlbaum, with longer wave-lengths and higher temperatures, approximated to the Rayleigh formula.

¹⁰ "Statistical Mechanics," p. 167.

Planck endeavored to find a mathematical compromise which should reduce to Wien's formula when λT was small and to that of Rayleigh when λT was great. In this way¹¹ he was led to the celebrated formula which has proved to be of such unexpected importance in the development of theoretical physics. In its original publication, however, the formula was otherwise deduced.¹² Planck had previously calculated the entropy of a system of linear resonators and believed that he had proved Wien's formula to be a necessary consequence of the second law.¹³ To obtain the new formula (by a process similar to that of Boltzmann in the kinetic theory of gases), he found it necessary to assume that energy was absorbed and radiated discontinuously. To satisfy Wien's displacement law these discrete energy quanta must be proportional to the frequency of the radiation, and thus the constant, h , came into existence.

The process was not very convincing and I suppose that, if nothing else had come of it, Planck's result would have been regarded as an empirical formula for which a satisfactory theoretical basis was lacking. But there were other puzzles which were, at nearly the same time, troubling the minds of physicists. One was the curious relation between X-rays of a certain hardness and the speed of the secondary electrons which they caused to be emitted from a metal. We all remember how Bragg was led by these difficulties to support a corpuscular theory of X-rays. The same difficulties existed in the case of photo-electrons and the ultra-violet light which liberated them. Einstein also proposed a quasi-corpuscular theory in which, however, instead of actual corpuscles, he substituted light-quanta whose energy was

equal to Planck's $h\nu$. It was not difficult to show, as Lorentz did, that Einstein's quanta were quite irreconcilable with the phenomena of diffraction; but the fact remains that the quantitative predictions of his theory have been verified in the case of both X-rays and light, in the latter instance with great accuracy by Professor Millikan and his pupils.

Time permits only the barest mention of Debye's daring application of Planck's formula to the elastic vibrations of solid bodies, his calculations of their specific heats upon this basis, and the remarkable agreement of the calculated values with the experimental results of Nernst and his collaborators. I must be equally brief in referring to Bohr's theory of line spectra in which the *form* of the Balmer progression is undoubtedly introduced in the assumptions; but the numerical value of Rydberg's constant is accurately calculated from the mass and charge of the electron and the inevitable h . In all these applications the same characteristics are observable: the fundamental ideas are not clear and precise, except arithmetically; if we try to make them so, we encounter apparently insuperable contradictions with some of the most firmly established experimental facts; the deductions from the premises do not follow inevitably, but must be helped out by special hypotheses in each different application; but numerical relations of surprising exactness are obtained, and an account is given of whole classes of phenomena which seem to be quite beyond the scope of the "classical" methods of twenty years ago. We do not know whether Planck's constant is an atom of Hamiltonian action, or of angular momentum, or of something quite different from either; but we can not doubt that it is a physical constant comparable in importance with the

¹¹ Planck, "Wärmestrahlung," 1te Aufl., p. 219.

¹² Planck, *Ann. d. Phys.*, 4, p. 553 (1901).

¹³ *Ann. d. Phys.*, 1, p. 118 (1900).

velocity of light and the electronic charge.

Poincaré's demonstration of the necessity for discontinuities in atomic processes if the total black radiation is to remain finite has not yet been successfully questioned. If it stands, we must not only give up the hope of bringing the phenomena of physics under the sway of generalized dynamics—we must renounce even the humbler ambition of describing them, in all their details, by means of differential equations. It will certainly be a triumph of the atomistic method—though unexpected and somewhat embarrassing to its most ardent supporters—if our very mathematics must become atomic.

The present state of theoretical physics is obviously one of transition, with all the discomfort that such a state involves. We are waiting for a synthesis of elements which are apparently discordant and mutually contradictory. The experience of the past forbids us to doubt that the necessary reconciliation will come in time; and we can foresee that it will be comparable with the greatest generalizations in the history of science. It may be that we must await the appearance of another Newton; or it may be that the result will be achieved in a more democratic manner by the co-operation of many lesser men.

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CYRILLE GRAND'EURY

THE writer has waited some months in the hope that some one whose acquaintance was not limited to an occasional interchange of letters might publish a note of appreciation of the life and work of this savant—the last of the illustrious trio of paleobotanist, Renault, Zeiller, Grand'Eury—who made the French Carboniferous and Permian floras classic and a standard for the whole world.

François Cyrille Grand'Eury was born at Houdreville (Meurthe) on March 9, 1839. He

was a mining engineer by profession and early in his career he became interested in the fossil plants of the Carboniferous, publishing a paper on the St. Étienne flora as early as 1869. His large work on the Loire flora, a folio monograph of 624 pages and 27 plates, was published as a memoir of the French Academy in 1877 and is one of the most comprehensive works of its kind ever printed. The only other large systematic work from his pen was that on the geology and paleontology of the coalfield of the Gard published in 1890.

Grand'Eury was always much interested in the stratigraphic applications of his subject, in the conditions of growth of the coal plants, and the origin of coal—subjects upon which he repeatedly published. He may be said to have established the chronologic succession of floras for the coal seams of the Stephanian, named from the typical development of this stage at St. Étienne. Probably no other student of Carboniferous floras had so thorough a field experience or saw one tenth the amount of material in place in the rocks as did Grand'Eury. Consequently his observations on the habit, sizes and positions of growth of the various Cordaites, Lepidophytes and Calamites are especially trustworthy. His name is inseparably associated with the elucidation of the habit and morphology of Cordaites and his restorations of these and other coal plants are to be found in every text-book.

He published a memoir upon the formation of coal in the *Annales des Mines* in 1882, a subject to which he returned in his paper before the International Geological Congress in 1901, and in his last large work commenced in 1912. He was not a voluminous writer and with the exception of his work on the Carboniferous plants of the Spanish peninsula, embodied in lists of species, all of his work was centered on the French floras. Nor did he, so far as I know, publish anything in the fields of Mesozoic and Cenozoic paleobotany, unless his paper of 1902 on the formation of stipite, brown coal and lignite can be so considered.

He did, however, contribute a very large