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THE DEBT OF PHYSICS TO METAPHYSICS[1]

IF I venture to address this society upon a subject where I am very liable, perhaps even likely, to be misunderstood, please bear in mind that I do so only in the belief that it is a matter of no small importance for workers in any one science to realize fully the limitations as well as the powers of their own science. It is hardly necessary to add, that while I shall consider a phase of physics which has little to do with experiment, I am not for an instant unmindful of the fact that ours *is* an experimental science, and that all the really great achievements in physics have been wrought through, or have led up to, or have been completed by, experiment and observation. This remark is doubtless true even of the supreme work of Newton, Fresnel and Maxwell. Nor am I forgetful that in days gone by the normal development of sound physics has been much retarded by metaphysics.

Second to none in my admiration of the man who has contributed even a single experimental fact to either the foundation or superstructure of the edifice which we call modern physics, I invite your attention for a few moments to the debit side of the account as it stands between the physicist and metaphysician. This I do with no little trepidation, remembering how easily one may, even with the utmost good will, go astray in a strange field of thought. Metaphysics is a term employed with such a variety of meanings that I must, at the very outset, explain the one sense in which

[1] Presidential address before the American Physical Society in New York, March 5, 1910.

I am using it. I am *not* employing it to indicate "the sum of all knowledge" (Paulsen), or as a synonym for the "science of the absolute" (Hegel), but rather as a branch of philosophy which is, in a certain sense, supplementary to all the individual sciences of phenomena. The metaphysician here in mind is a gleaner after physics and psychology, using these two words in their wide meaning so as to cover practically the whole of modern science. He it is who orients the sciences among themselves, criticizes their foundations, their methods and even their conclusions, in so far as these conclusions depend upon pure logic. He it is who rounds out and corrects the individual sciences. It will thus be seen that metaphysics, with these limitations, does not differ widely from the modern usage of the word philosophy; for the metaphysics I have in mind has been aptly characterized as "the supreme science of order."[2] There are those, and I myself am one of this class, who prefer to use the word "epistemology" to describe a metaphysics of this type. It is certainly not the type of metaphysics which allowed Kant to define matter in terms of force.[3] And, in any event, I trust we shall all agree that we are not getting into what Maxwell called "the den of the metaphysician, strewed with the remains of former explorers, and abhorred by every man of science."

But we must be careful to remember that the metaphysician which Maxwell here has in mind is not an epistemologist, but a man of the Hegelian type. Helmholtz[4] boasted that he never lost an opportunity to impress upon his students the principle that "a metaphysical conclusion is either a false conclusion or a concealed experimental conclusion." How far removed the more genuine metaphysics of to-day is from that which held sway during the first half of the nineteenth century and which exasperated men of the type of Maxwell and Helmholtz, may be indicated by the following paragraph from Professor A. E. Taylor,[5] of Aberdeen, himself a distinguished metaphysician. He says:

> Just because of the absence from metaphysics itself of all empirical premises, it can be no business of the metaphysician to determine what the course of events will be or to prescribe to the sciences what methods and hypotheses they shall employ in the work of such determination. Within these sciences any and every hypothesis is sufficiently justified, whatever its nature, so long as it enables us more efficiently than any other to perform the actual task of calculation and prediction. And it was owing to neglect of this caution that the *Naturphilosophie* of the early nineteenth century speedily fell into a disrepute fully merited by its ignorant presumption. As regards the physical sciences, the metaphysician has indeed by this time probably learned his lesson.

It is hardly necessary to add that the type of metaphysics here exposed is not one to which physics owes anything whatever, and is not the one I have in mind during these remarks.

I. THE MECHANICAL POSTULATE

The father of the present Duke of Argyll rendered marked service to science in pointing out how wide-spread is the use of physical and natural law. But nowhere in his notable volume, the "Reign of Law," does he indicate what may be called the most fundamental fact connected with the discovery and employment of such law, namely, that the very existence of laws governing natural phenomena is a postu-

[2] Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 236.

[3] Hoeffding, "History of Modern Philosophy," Vol. 2, p. 69.

[4] Vorträge, "Das Denken in der Medicin," p. 34.

[5] Congress of Arts and Science, St. Louis, 1904, Vol. 1, p. 240.

late laid down, consciously or unconsciously, by the investigator. No one, except the later metaphysicians, has convinced us that however tangled the knot of physical facts which we are called upon to explain, the first thing we *assume* is that these phenomena are subject to law. We assume that we are studying a machine which behaves in a definite manner.

This assumption—which we may call the mechanical postulate—is not something to be discovered or verified by experiment, not something whose adoption stamps a man as a materialist, not something which sane men consider, in order to accept or refuse, but something which *all* men adopt as a laboratory convenience, one might better say, a laboratory essential.

Nor is the mechanical postulate one which is confined to physics; but is employed in all the sciences where men are attempting to bring order out of chaos. It is not, therefore, something to be charged up against one in the sense employed when modern physics is said to rob the world of all spontaneity and sentiment, or when science is said to be devoid of poetry. While we treat nature as a machine and while we adopt the mechanical hypothesis as a necessity of productive scholarship let us be very careful however not to allow ourselves to dogmatize to the extent of saying that a machine is all we have.

Is not the physicist under obligations to the philosopher for making this matter perfectly clear?

Apparent deviations from mechanical law lead to some of the most important biological problems. Animate and inanimate matter may appear, at first glance, to belong in two different categories; and so they undoubtedly do as regards many of the superficial phenomena. But conversation with some of the most productive scholars of our country in zoölogical and botanical lines has convinced me that they are practically all working on the assumption that biological phenomena are physical phenomena. These investigators assure us, moreover, that the introduction of an *enteliche* here and there, wherever convenient, would be sufficient to discourage all serious research on life problems. The same point of view is expressed by Münsterberg when, in his classification of knowledge, he places physics and biology together at the very bottom of the group called "physical sciences."

The hatching of an egg is apparently a different process from that of melting ice, although both are accomplished by the application of heat. But to assume anything else than that they are both mechanical processes is merely to erect a barrier which shall delay the discovery of truth. The study of cytology and artificial parthenogenesis have already gone so far that the discovery of a much more definite connection between life and mechanics would shock the world perhaps even less than did Wöhler's synthesis of urea in 1828.

UNIFORMITY POSTULATE

There is of late a very distinct change of feeling in regard to the principle of the Uniformity of Nature—a principle which was widely circulated, a generation ago, as an experimental fact but which is now properly regarded as another formulation of the mechanical postulate. But, thanks to the metaphysician, this principle is now, so far as I know, regarded by us all, neither as an axiom nor as an empirical fact, but as a fundamental hypothesis which we may call the "uniformity postulate."

This assumption is practically equivalent to considering matter, energy and electrification to have no personal or individual traits which we need take into account. Without this postulate we should be unable to generalize our physical laws so as to include many new phenomena—phenomena unknown at the time of the formulation of the law. The tenacity with which the experimentalist holds to his assumption of a simple law is well illustrated by two papers read before the last meeting of this society: papers which illustrate how complex nature is becoming as research goes on. I refer to the work of Professor H. W. Morse and Professor E. B. Rosa on electrolysis. Each investigation dealt with slight deviations from one of Faraday's fundamental laws: and each investigation apparently assumed its truth: in any event assumed an equally simple law. Thousands of engineering results obtained each day in the week convince us that there are no accidents in history and allow us to believe that no postulate was ever better justified by its success.

The behavior of nature in this respect always reminds me of a remark, really a new formulation, once made by Professor Michelson in describing the labor of several years in locating and eliminating the errors in a certain steel rod upon which he was cutting an accurate screw. "I felt," he said, "as if matched in a game against an opponent: *but my antagonist always played fair.*"

II. ENERGY POSTULATE

Passing now to the consideration of energy, it is not yet three score years and ten, since Poggendorff and Magnus refused space, in the *Ann. d. Physik.*, to Helmholtz's little tract, "Die Erhaltung der Kraft," on the ground that it was too metaphysical. But thanks partly to the clear vision of Helmholtz, partly to the clever analysis of H. Poincaré, and largely to the experimental success of the principle, the time has now come, I believe, when we can say that the conservation of energy is so useful, *as a postulate*, that present-day science can not successfully accomplish its work without it. Experiment has been able to demonstrate it as a law only for particular cases and only approximately: but experiments have been so numerous and compelling, as to have created a new attitude of mind in the present generation, leading us to believe that everywhere in the physical universe there is *some* constant quantity, corresponding to a certain constant of integration, called "energy."

The most recent illustration of the manner in which the physicist assumes this constancy is, of course, the case of the steady heat production in radium. No sooner had Curie and Laborde made this remarkable discovery, in 1903, than men began, *not* to doubt the validity of the law of the conservation of energy, but to look about for the energy which was thus being transformed into heat. Accordingly Rutherford and Barnes succeeded, in the following year, in showing that 23 per cent. of this intra-atomic energy was due to radium itself, 32 per cent. to radium C and 45 per cent. to the emanation and radium A together. In saying that the time has come when the Law of the Conservation of Energy may properly be regarded as one of the presuppositions of physics, it is to be carefully noticed that this statement does not include the Law of the Dissipation of Energy.

III. CAUSAL POSTULATE

The infinite regress involved in the search after causes and the vanity of attempting to follow a series of causes to its end are, at least, as old as the Greeks.

The postulate which the philosopher here shows us to be one of our presuppositions is as follows: events in physical science depend upon a few antecedents, knowing which we may successfully predict the immediate consequence, and may safely disregard all other circumstances. The brevity of the sequence which really determines phenomena in physics is a matter of continual surprise—while the length and complexity of the sequence in the case of ordinary human actions is a matter of equal astonishment.

But it is very easy to forget what a powerful influence this postulate has at times exerted in almost all departments of science. Few physicists, and still fewer engineers, of the present seem to realize that some of the most fundamental conceptions of our science have been introduced directly through the adoption of this postulate.

Take, for instance, what is perhaps the central idea of modern dynamics—the idea of force—an idea which is older than either that of mass or of energy. When viewed in the light of the causal postulate, *i. e.*, in the light of history, the definition of force becomes a matter of the utmost simplicity and perfect clarity. From many other points of view it is one of the most complex and puzzling of physical quantities. Sir Oliver Lodge says:

> We are chiefly familiar, from our youth up, with two apparently simple things, *motion* and *force*. We have a direct sense for both of these things. We do not understand them in any deep way, probably do not understand them at all, but we are accustomed to them. Motion and force are our primary objects of experience and consciousness; and, in terms of them, all other less familiar occurrences may be stated and grasped.

To identify "force" in this manner with the "muscular sensation" of tension or pressure, which we feel when giving an accelerated motion to a body or when equilibrating by muscular effort the pull of the earth upon a body, seems to me dangerously near darkening counsel with words, and quite contrary to the spirit of the modern mathematician and physicist who are mending their fences at every possible point to keep out ideas which are not clear, sharp and definite.

The standard definition of the engineer, and, I fear, of not a few students of physics, is set forth by Professor William Kent in his article on the teaching of dynamics which appeared in SCIENCE[6] a few weeks ago, namely, "Force is defined as a pull or push, something that causes or tends to cause either motion or a change in the velocity or direction of motion."

Now considering both of these points of view, which I believe are widespread, every one is willing to admit at once the existence of certain elastic, and gravitational, and muscular, and electric, and cohesive, stresses which none of us understand: but the historical, or, if you please, the metaphysical, point of view would appear to be something like the following.

So far from our possessing any direct muscular sense of force, in the physical meaning of the word as distinguished from muscular tension, with which we are all familiar, the idea is one which was introduced by an Italian professor of mathematics, but a comparatively short time ago. How short may be illustrated by the following circumstances:

My grandmother, who lived in my own home for a number of years, was born on the banks of the Brandywine in 1789. She was therefore a contemporary as well as a neighbor of Benjamin Franklin. When Franklin was a printer's lad in London he had a promise from a friend that he should be taken to visit Sir Isaac Newton. Sir Isaac Newton was born within the same week in which Galileo died. Two human

[6] SCIENCE, Vol. 30, p. 919, 1909.

lives suffice therefore to bridge the gap between Galileo and our contemporaries. Back to Galileo is not therefore a far cry.

Recognizing the limitations of his science, and seeing that the search after causes was futile, Galileo adopted the causal postulate and prepared to confess his ignorance of gravitation, cohesion, muscular tension, and to say that, when we see a body changing its momentum, there is a "force" at work upon it. Following is the sentence, from his "Dialogues"[7] in which he introduces force as a synonym for any of these unknown influences which produce acceleration:

It does not appear to me worth while to investigate the *causes* of natural motion concerning which there are as many different opinions as there are different philosophers. Some refer them to an attraction towards the center; others assign them to repulsion between the small particles of a body, while still others would introduce a certain stress in the surrounding medium which closes in behind the falling body and drives it from one of its positions to another. Now all these fantasies, and others too, must be examined; but it is not really worth while. For all that is needful is to see just how one investigates the properties of accelerated motion and how these are defined, *without consideration of their cause*, in such a way that the momentum (of the body) increases uniformly from the initial condition of rest in simple proportionality to the time.

The paragraph which I have just quoted is, so far as I am able to learn, the earliest expression and definition of that central physical quantity which we now call "force." Observe first of all the modesty of the man; twice within this definition he inserts a distinct disavowal of any consideration of the cause of motion. So far is he in advance of our modern text-books, that he declines to define force as a "cause of motion" or a "tendency to produce motion," but says it is not even worth while to consider the question from that point of view.

[7] Ostwald's "Klassiker der Exakten Wissenschaften," No. 24, p. 15.

How clear these same ideas were to Newton will be evident from the following two sentences from the first book of the "Principia." He says:

For I here design only to give a mathematical notion of those forces without considering their physical causes or seats.

And again:

Wherefore the reader is not to imagine that by those words I anywhere take upon me to define the kind or the manner of any action, the cause or the physical reason thereof.

Having thus abandoned all consideration of cause and having assigned ourselves the simpler task of describing the motions of bodies, we come back to the definition of Galileo and Newton, namely, the rate of change of momentum—as the one perfectly *correct, competent* and *complete* description of force.

It remains only to show that Galileo had a clear and modern conception of momentum. This is sufficiently evident from the following paragraph in the "Dialogues."[8] He says:

It is clear that an impulse is not a *simple* matter, seeing that it depends upon *two* important factors, namely, the weights (il peso) of the colliding bodies and their velocities.

And again on the same page he says

It is customary to say that the "momentum" of a light body is equal to the "momentum" of a heavy body when the velocity of the former bears to the velocity of the latter the inverse ratio of their weights.

If then I have correctly stated the facts of the case, force would appear to be a pure concept of the intellect: but a precious concept; one which is well understood, clear, definite, quantitative, and one whose extraordinary usefulness has made

[8] Ostwald's "Klasiker der Exakten Wissenschaften," No. 25, p. 44.

it survive through the entire history of physics.

The paradoxy of this dominant idea of modern physics being a mere picture created by the human mind, disappears when we consider how the same method is employed in subjects other than physics.

In history, for example, we have important culminating events which we ascribe to "certain influences," while as a matter of fact the most that we actually know and observe in history is a series of *individual* acts, prompted, we suppose, by certain purposes.

The Franco-Prussian war came when the German Kaiser decided to send the telegram from Ems, when Prince Bismarck decided to publish certain parts of this telegram, when Von Moltke decided that the army was ready, when Napoleon III. decided to emulate the military career of his uncle, when the Congress of Vienna decided, in 1815, to give Prussia additional Rhenish territory, when in 868 the father of Lothar gave to his son the middle kingdom, the modern Lorraine, between France and Germany.

In practise we find it more convenient to say that "certain influences" had been at work for a full thousand years which culminated in the victory of Prussia over France. In physics, we give to the corresponding "influences" the name forces. That's the whole story! We *measure* these influences by the mass-acceleration of the body under consideration.

The extension which this idea of force has received in later times is known to us all. Huygens was the first to show that Galileo's fundamental variable, linear momentum, might change in two ways, namely, in direction and amount; and he gives us for the first time a method of computing the force when the momentum varies in direction only—a force which we now call "centrifugal." Later, in the case of rigid bodies the conception of "angular momentum" was introduced; its time variation we now call either "torque" or "precessional couple" according as the angular momentum varies in amount only or direction only.

This definition is identical in form and meaning with that of Galileo.

The essential step made by Lagrange, in his treatment of the simplest possible case, namely, a single particle, is to derive both the time variation of momentum and the rate of directional change of momentum, each by differentiation of a single function.

Momentum for him is the velocity-variation of kinetic energy, a quantity whose time-variation is the tangential force; and centrifugal force is the space-variation of kinetic energy: but each of these is still a time-variation of momentum, agreeing perfectly with Galileo's original definition.

The space-variation of *potential* energy is the measure of stress—or more properly a stress integral—which we do not understand—but which nevertheless can be evaluated in terms of force.

I shall detain you for only one more illustration.

Faraday had discovered a quantity—the "electrotonic state," he called it—electrokinetic momentum, we call it—whose variations through any closed circuit, were always accompanied by an electric current in that circuit. Not knowing the cause of this current, physicists agreed to say that an "electromotive force" was at work whenever the electro-kinetic momentum changed, and to define this electromotive force as the time rate of change of electrokinetic momentum (Neumann). Here again we have a generalized force introduced as a synonym for an unknown cause; exactly as was done by Galileo in the first instance.

Let us distinguish carefully between the observed facts of nature and those tempting pictures of the human mind which we only too easily create and are only too apt to worship.

Among the realities of mechanics are to be mentioned bodies in motion, liquids flowing, springs changing length; among the abstractions of the subject—helpful and needful abstractions—but abstractions nevertheless—are to be numbered the forces, velocities and accelerations of these bodies. Only by understanding these matters and by drawing a sharp line here shall we avoid Maxwell's "den of the metaphysician."

It is not infrequently that one finds a clever metaphysician in the orthodox man of empirical science; and I am free to confess myself unable to say whether the majority of the criticisms of the foundations of our science are due to the physicist or the philosopher; but in either case the critic speaks *as a metaphysician.* As an illustration consider the penetrating criticisms of the foundations of rational dynamics recently given by Mr. Norman Campbell,[9] who shows that the science of mechanics is so loaded with assumptions that the experimental verification of its laws is utterly hopeless.

IV. PRELIMINARY DISCUSSIONS

Fourthly, metaphysics has, I believe, rendered distinct service in giving us certain helpful preliminary discussions. Indeed, it is the history of many of the special sciences, such as psychology and sociology, that they were at one time departments of philosophy—but now, having shown themselves amenable to experiment or observation and subject to the "reign of law," are established as kingdoms of their own. The very notion of mechanical law is at least as old as Thales—600 B.C.—whose idea it was, in common with Anaximander, Anaximenes and Heraclitus, that the variety of things is due to "a single material cause, corporeal, endowed with qualities and capable of self-transformation."[10] Ridiculous and absurd as this sounds to us, it nevertheless contains the fundamental conception of mechanical law, and made it easier for later men to adopt more useful hypotheses.

The history of the atomic theory illustrates well the value of this contribution. The atom of Democritus—a purely metaphysical structure—differs in no essential respect from the modern atom up to the year 1738 when Daniel Bernoulli initiated the kinetic theory of gases.

The contention of Anaxagoras that all bodies are really continuous has also been of the utmost help: Poisson adopted it *in toto* in his mechanics; it was employed in electrical science up to the date of Helmholtz's Faraday lecture, 1881, and it is to-day practically adopted in all discussions of hydrodynamics.

Maxwell[11] goes so far as to say:

> In the earliest times the most ancient philosophers whose speculations are known to us seem to have discussed the ideas of number and of continuous magnitude, of space and time, of matter and motion with a native power of thought which has probably never been surpassed.

It was a really profound insight into the nature of pure mathematics that led certain participants in the relativity discussion, at the last meeting of this society, to place in the same class the metaphysician and the mathematician; the new grouping of studies at Harvard College does the same; each of these subjects is concerned neither with phenomena of any kind, nor with individual purposes, but with those

[9] *Phil. Mag.*, January, 1910.

[10] "Encyclopedia Britannica," 23, 219.

[11] "Encyclopedia Britannica," art. Atom.

over-individual purposes, with those universal agreements, with that world-wide consensus of opinion, in which all sane men unite; in brief, mathematics and metaphysics each belong in the group which Münsterberg calls the "normative sciences." There is therefore a certain sense, which in passing I merely mention, but do not urge, in which all consideration of number and quantity and limits which the mathematical philosophers have handed down, increases the debt of physics to metaphysics.

Sound method in drawing inferences is a branch of science to which the physicist owns no copyright, but one in which he may claim to be fairly well versed. For this method he is indebted in no small degree to the development of logic in the hands of the metaphysician. In brief, modern physics, at its very inception in the seventeenth century, found that the schoolmen had already furnished it with a set of beautiful tools in the shape of fundamental logical ideas, including "precise definition," "classification," and "fallacies." Even Bacon when "preaching the funeral sermon of scholasticism," used the accurate methods of the schoolmen.

Space and time, as continuous quantities and as limiting conditions for all phenomena, is another conception of no small value which we have inherited from the Greeks. The critical examination of our conception of time, which was given by Einstein[12] some five years ago, and perhaps even earlier by Lorentz, had, among other interesting and more valuable features, the following: He showed clearly —and, so far as I am aware, for the first time—just what kind of "time" we have been and are still using in ordinary Newtonian mechanics, namely, time such as would result from having all our clocks controlled by a single central time-keeper *which would transmit its controlling signals with absolute instantaneity.*

The clear definitions of synchronous clocks and simultaneity—in brief the idea of local time—may be considered as belonging either to physics or to mathematics —but surely the exposition in which Einstein has taught us just what kind of time we have been unconsciously using for more than two centuries is a metaphysical contribution of high order.

The dangers of mere nominalism, or, if you prefer, extrapolation, by which I mean the danger of ascribing to any physical system a set of properties which we have merely learned to associate with its name, has been clearly pointed out in the history of philosophy. Due regard for this warning would, I believe, have saved many pages that have been written concerning the ether—especially those devoted to a determination of its inertia, its weight, and its place in the periodic table of Mendelejeff.

V. LIMITATIONS OF SCIENCE

Fifthly and lastly the metaphysician has rendered the inestimable service of pointing out to the experimental investigator the paradox that his greatest strength lies in his confessed limitations. Each of the particular sciences views phenomena from its own particular angle; but there is, I fear, sometimes—often, indeed—a tendency for the student of physics to think that in measuring, say, the inertia of a body, he is in some sense getting at the "quantity of matter" in it; or to put it in another way, there is often a tendency to think that in determining the mass, on a beam balance, he is perhaps doing something more fundamental than merely determining inferentially the ratio of the

[12] *Ann. der Physik* (4), 17, 891–921 (1905).

inertia of this body to the inertia of some body selected as a standard; for which purpose he has abstracted the inertia from all other properties of body and is really no nearer the nature of the ultimate "substance" of the body than if he had measured its temperature or its color.

A most important limitation which might have been entirely forgotten were it not for the metaphysician, is the fact that *phenomena* do not constitute the *entire* subject matter of science. Indeed it is only the mental and physical sciences which deal with phenomena. Human purposes and acts of the human will are quite as much subjects of scientific study, whether we consider the individual, the group or the entire race of sane men, as are any of the phenomena of physics. It includes such branches as history, politics, language and literature. Not only so, but if we define the real as that with which we must reckon in the accomplishment of our purposes, this second group of sciences deals with subject matter which is quite as real as anything we consider in physics.

It will perhaps not be out of place here to repeat the warning given by President Maclaurin[13] to the American Chemical Society on the occasion of the recent Boston Meeting of the American Association for the Advancement of Science. He says:

> We should pay more serious attention than we usually do to the logic of science and have as clear ideas as possible as to what we are really aiming at, as to what we can really expect to do and not to do. A little artificial stimulus toward philosophy might accelerate the process. It seems to me extremely unfortunate that men of science are still so much scared by the bogey of metaphysics. . . . We should realize, perhaps, that a science such as chemistry is above all else a work of art, and that concepts like atoms, energy and the like are not much more than pigments with which we paint our pictures.

[13] *Boston Herald*, December 31, 1909.

Ether.—One other illustration must serve to complete this ungracious paragraph on limitations. I shall not weary you with citations from Lord Kelvin, telling us how much more we know about the ether than about ordinary matter, but I shall trouble you with a single sentence from that skilled expositor, Sir Oliver Lodge,[14] whose latest pronouncement upon this subject, omitting, however, the suppositions with which the entire argument is honeycombed, is as follows:

> The estimates of this book and of "Modern Views of Electricity" are that the ether of space is a continuous, incompressible, stationary fundamental substance or perfect fluid, with what is equivalent to an inertia-coefficient of 10^{12} grams per c.c.: that *matter* is composed of modified and electrified specks or minute structures of ether which are amenable to mechanical as well as electrical force and add to the optical or electric density of the medium: and that elastic rigidity and all potential energy are due to an excessively fine-grained etherial circulation with an intrinsic kinetic energy of the order of 10^{33} ergs per cubic centimeter.

Suffice it to say that I am second to no man in this society in my admiration for that group of men whose names are associated with the following dates—1676, 1728, 1820, 1831, 1845, 1864, 1888, Römer, Bradley, Oersted, Faraday, Neumann, Maxwell, Hertz; names and dates which mark the discovery of the finite speed of light, the discovery of aberration, the discovery of the magnetic field produced by an electric current, the discovery of the electromotive force produced by magnetic displacement, the mathematical formulation of this result by Neumann, the combination of these two results by Maxwell and the prediction from them of electric waves, the experimental realization of these waves by Hertz. For brilliancy of achievement this series has certainly sel-

[14] "Ether of Space," p. 151, Harper, 1909.

dom, if ever, been surpassed in the history of physics.

But leaving matter aside, and considering only the ether, what is the net result? Practically this, that *electromagnetic disturbances, including light waves, are propagated through space with a speed of 300 million meters per second.* This, I conceive to be the criticism which *every* sound metaphysician, but only *some* sound physicists, would pass upon our present knowledge of the ether. This is the one fact concerning the ether which we know in the same sense in which we are said to "know" the ordinary everyday facts of physics.

In conclusion, and still dealing with limitations, I beg to offer for your consideration a definition (*i. e., a* delimitation) of physics recently given to me by an eminent metaphysician.

Last summer I had the pleasure of several times meeting Professor Münsterberg; and on one of these occasions I took the liberty of submitting to him, for criticism, a definition of physics, which I myself had formulated. Following is his definition of the physics of to-day which he, in return, submitted to me and which is, I am inclined to think, unsurpassed in point of accuracy, clearness and completeness:

Physics deals with changes in the world of over-individual objects, in so far as they are not changes of composition. It consists of those judgments which have proved themselves by trial to determine most accurately our justified expectations concerning these changes. In dealing with *objects* it separates itself from the knowledge of will-acts; in dealing with *over-individual* objects it separates itself from psychology; in abstracting from changes of composition, it separates itself from chemistry. The over-individual objects may be *matter* or *ether* or *electrons.*

CONCLUSION

The view of physics here presented is that of a half truth or partial truth. But this is very far from saying it is an untruth. The essential point—the only essential point—is for us to recognize the facts; to know ourselves; to admit our limitations. Then the more nearly we remain inside these limitations, and avoid "the den of the metaphysician," the better.

That flexibility of mind which it is desirable to secure *by not* translating every temporary opinion into a hard-and-fast fact of nature is well illustrated by a recent remark of Professor Schuster[15] who is himself one of the small group of men who have established the pulse theory of white light. "These two representations of white light (by homogeneous waves and by impulses) are," he says, "not mutually exclusive: They represent two points of view, and we may adopt either one or the other in different problems according to our convenience."

Less fixity and more flexibility in our views concerning the ether might, for instance, permit a more cordial consideration of Professor Osborne Reynolds's theory of gravitation which, so far as I understand it, has much to recommend it.

Lest what I said at the outset concerning the experimental side of physics should be forgotten, let me, in justice to myself, remind you once more of my attitude toward the experimentalist, towards that group which in Italy includes Galileo, Volta, Melloni and Righi; the skillful group which includes Oersted, Kirchhoff, Hertz, Roentgen; the French group of laboratory workers, Mersenne, Fresnel, Regnault, the Curies; in England, Gilbert, Boyle, Joule, Rayleigh; and those dextrous men, our own countrymen, Franklin, Henry, Rowland, Michelson. Toward the experimentalist as compared with the friendly critic and reviewer, my feeling is precisely that of Lincoln toward the soldiers who fought at Gettysburg. You all remember his sen-

[15] *Phil. Mag.*, (6), 18, 767 (1909).

tence—"The world will little note nor long remember what *we say* here; but it can never forget what *they did* here."

HENRY CREW

NORTHWESTERN UNIVERSITY

CHARLES REID BARNES

CHARLES REID BARNES was born at Madison, Ind., September 7, 1858, and died at Chicago, Thursday, February 24, 1910. He attended Hanover College, where he graduated with the degree of A.B. in 1877, being the valedictorian of his class. He was a student of Professor Coulter, with whom he was henceforth intimately associated professionally and otherwise until his death. After graduation he studied at Harvard University with Professor Gray, who regarded him as a man of great promise. In 1880 Barnes returned to Hanover College, where he was given the degree A.M. That same year he entered upon an instructorship of natural science at the high school, Lafayette, Ind., and later at Purdue University, where he was promoted to a professorship in 1882. In 1885 his chair was changed from natural science to botany and geology. In the year 1885–6 Professor Barnes again spent some time at Harvard University, and his *alma mater* in 1886 conferred upon him the degree Ph.D. In 1887 he was called to the chair of botany at the University of Wisconsin, whence in 1898 he was called by the University of Chicago to occupy its newly created chair of plant physiology, and here he remained until his death. From 1883 until his death he was associated with Professor Coulter in the editorship of *The Botanical Gazette.*

Professor Barnes was always prominently connected with the various scientific societies, having become a member of the American Association for the Advancement of Science in 1884 and a fellow in 1885. In 1890 he was secretary of the Botanical Club of the American Association for the Advancement of Science, and was secretary of the Botanical Society of America from its inception at Brooklyn from 1894 until 1898. In 1894 he served as secretary of Section G, in 1895 as secretary of the council of the American Association for the Advancement of Science, and in 1896 as general secretary of the American Association for the Advancement of Science. In 1898 he served as vice-president for Section G, American Association for the Advancement of Science, giving his retiring address at Columbus in 1899 on "The Progress and Problems of Plant Physiology." In 1903 he served as president of the Botanical Society of America, giving his retiring address at Philadelphia in 1904 on "The Theory of Respiration." In 1905 Professor Barnes served as a delegate from Section G, American Association for the Advancement of Science, to the international Botanical congress at Vienna. He was also a member of the American Society of Naturalists and of the Botanists of the Central States, and was in turn a member of influence in the state scientific academies of Indiana, Wisconsin and Illinois.

As a botanical contributor Professor Barnes began his career in a modest way in *The Botanical Gazette* in 1877, his first contributions, entitled "Notes," having to do chiefly with annotated lists of plants and additions to county floras, quite in the manner of the time. As early as 1879, however, some of his contributions reveal a strong physiological bent, the necessity of devices for accurate experimentation appealing to him then and ever afterward with unusual force. From 1883, when he became editorially connected with *The Botanical Gazette,* he gave freely of his time and energy to that journal. Much of the remarkable success of this periodical is due to his editorial genius; his trenchant English, and his insistence on accurate statement and mechanical perfection have for many years been reflected on almost every page. Perhaps no botanical reviewer has been so fearless as was Professor Barnes; frank but friendly disapproval of all that seemed bad, whether in fundamental principles, in statement of fact, or in mechanical alignment, was as natural to him as is fulsome praise to most reviewers. Possibly his greatest service to American botany was in his many-sided work on *The Botanical Gazette.*

Professor Barnes was first generally known