

SCIENCE

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THE BRITISH ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

ADDRESS TO THE MATHEMATICAL AND PHYSICAL SECTION.*

THE members of this Section will, I am sure, desire me to give expression to the gratification that we all feel in the realization of the scheme first proposed from this chair by Dr. Lodge, the scheme for the establishment of a national Physical Laboratory. It would be useless here to attempt to point out the importance of the step taken in the definite foundation of the Laboratory, for we all recognize that it was absolutely necessary for the due progress of physical research in this country. It is matter for congratulation that the initial guidance of the work of the Laboratory has been placed in such able hands.

While the investigation of nature is ever increasing our knowledge, and while each new discovery is a positive addition never again to be lost, the range of the investigation and the nature of the knowledge gained form the theme of endless discussion. And in this discussion, so different are the views of different schools of thought, that it might appear hopeless to look for general agreement, or to attempt to mark progress.

Nevertheless, I believe that in some directions there has been real progress,

* MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

* Given at the Dover meeting on September 14, 1899, by the President of the Section.

and that physicists, at least, are tending towards a general agreement as to the nature of the laws in which they embody their discoveries, of the explanations which they seek to give, and of the hypotheses they make in their search for explanations.

I propose to ask you to consider the terms of this agreement, and the form in which, as it appears to me, they should be drawn up.

The range of the physicist's study consists in the visible motions and other sensible changes of matter. The experiences with which he deals are the impressions on his senses, and his aim is to describe in the shortest possible way how his various senses have been, will be, or would be affected.

His method consists in finding out all likenesses, in classing together all similar events, and so giving an account as concise as possible of the motions and changes observed. His success in the search for likenesses and his striving after conciseness of description lead him to imagine such a constitution of things that likenesses exist even where they elude his observation, and he is thus enabled to simplify his classification on the assumption that the constitution thus imagined is a reality. He is enabled to predict on the assumption that the likenesses of the future will be the likenesses of the past.

His account of Nature, then, is, as it is often termed, a descriptive account.

Were there no similarities in events, our account of them could not rise above a mere directory, with each individual event entered up separately with its address. But the similarities observed enable us to class large numbers of events together, to give general descriptions, and indeed to make, instead of a directory, a readable book of science, with laws as the headings of the chapters.

These laws are, I believe, in all cases brief descriptions of observed similarities.

By way of illustration let us take two or three examples.

The law of gravitation states that to each portion of matter we can assign a constant—its mass—such that there is an acceleration towards it of other matter proportional to that mass divided by the square of its distance away. Or all bodies resemble each other in having this acceleration towards each other.

Hooke's law for the case of a stretched wire states that each successive equal small load produces an equal stretch, or states that the behavior of the wire is similar for all equal small pulls.

Joule's law for the heat appearing when a current flows in a wire states that the rate of heat development is proportional to the square of the current multiplied by the resistance, or states that all the different cases resemble each other in having $H \div C^2 R t$ constant.

And, generally, when a law is expressed by an equation, that equation is a statement that two different sets of measurements are made, represented by the terms on the two sides of the equation, and that all the different cases resemble each other in that the two sets have the constant relation expressed by the equation. Accurate prediction is based on the assumption that when we have made the measurements on the one side of the equation we can tell the result of the measurements implied on the other side.

If this is a true account of the nature of physical laws, they have, we must confess, greatly fallen off in dignity. No long time ago they were quite commonly described as the Fixed Laws of Nature, and were supposed sufficient in themselves to govern the universe. Now we can only assign to them the humble rank of mere descriptions, often tentative, often erroneous, of similarities which we believe we have observed.

The old conception of laws as self-sufficient governors of Nature was, no doubt, a

survival of a much older conception of the scope of physical science, a mode of regarding physical phenomena which has itself passed away.

I imagine that originally man looked on himself and the result of his action in the motions and changes which he produced in matter, as the one type in terms of which he should seek to describe all motions and changes. Knowing that his purpose and will were followed by motions and changes in the matter about him, he thought of similar purpose and will behind all the motions and changes which he observed, however they occurred; and he believed, too, that it was necessary to think thus in giving any consistent account of his observations. Taking this anthropomorphic—or, shall we say, psychical—view, the laws he formulated were not merely descriptions of similarities of behavior, but they were also expressions of fixed purpose and the resulting constancy of action. They were commands given to matter which it must obey.

The psychical method, the introduction of purpose and will, is still appropriate when we are concerned with living beings. Indeed, it is the only method which we attempt to follow when we are describing the motions of our fellow-creatures. No one seeks to describe the motions and actions of himself and of his fellow-men, and to classify them without any reference to the similarity of purpose when the actions are similar. But as the study of Nature progressed, it was found to be quite futile to bring in the ideas of purpose and will when merely describing and classifying the motions and changes of non-living matter. Purpose and will could be entirely left out of sight, and yet the observed motions and changes could be described, and predictions could be made as to future motions and changes. Limiting the aim of physical science to such description and prediction, it gradually became clear that the method was adequate

for the purpose, and over the range of non-living matter, at least, the psychical yielded to the physical. Laws ceased to be commands analogous to legal enactments, and became mere descriptions. But during the passage from one position to the other, by a confusion of thought which may appear strange to us now that we have finished the journey, though no doubt it was inevitable, the purpose and will of which the laws had been the expression were put into the laws themselves; they were personified and made to will and act.

Even now these early stages in the history of thought can be traced by survivals in our language, survivals due to the ascription of moral qualities to matter. Thus gases are still sometimes said to obey or to disobey Boyle's law as if it were an enactment for their guidance, and as if it set forth an ideal, the perfect gas, for their imitation. We still hear language which seems to imply that real gases are wanting in perfection, in that they fail to observe the exact letter of the law. I suppose on this view we should have to say that hydrogen is nearest to perfection; but then we should have to regard it as righteous overmuch, a sort of Pharisee among gases which overshoots the mark in its endeavor to obey the law. Oxygen and nitrogen we may regard as good enough in the affairs of everyday life. But carbon dioxide and chlorine and the like are poor sinners which yield to temptation and liquefy whenever circumstances press at all hardly on them.

There is a similar ascription of moral qualities when we judge bodies according to their fulfillment of the purpose for which we use them, when we describe them as good or bad radiators, good or bad insulators, as if it were a duty on their part to radiate well, or insulate well, and as if there were failures on the part of Nature to come up to the proper standard.

These are of course mere trivialities, but

the reaction of language on thought is so subtle and far-reaching that, risking the accusation of pedantry, I would urge the abolition of all such picturesque terms. In our quantitative estimates let us be content with 'high' or 'low,' 'great' or 'small,' and let us remember that there is no such thing as a failure to obey a physical law. A broken law is merely a false description.

Concurrently with the change in our conception of physical law has come a change in our conception of physical explanation. We have not to go very far back to find such a statement as this—that we have explained anything when we know the cause of it, or when we have found out the reason why—a statement which is only appropriate on the psychical view. Without entering into any discussion of the meaning of cause, we can at least assert that that meaning will only have true content when it is concerned with purpose and will. On the purely physical or descriptive view, the idea of cause is quite out of place. In description we are solely concerned with the 'how' of things, and their 'why' we purposely leave out of account. We explain an event not when we know 'why' it happened, but when we show 'how' it is like something else happening elsewhere or otherwhen—when, in fact, we can include it as a case described by some law already set forth. In explanation, we do not account *for* the event, but we improve our account *of* it by likening it to what we already knew.

For instance, Newton explained the falling of a stone when he showed that its acceleration towards the earth was similar to and could be expressed by the same law as the acceleration of the moon towards the earth.

He explained the air disturbance we call 'sound' when he showed that the motions and forces in the pressure waves were like motions and forces already studied.

Franklin explained lightning when and

so far as he showed that it was similar in its behavior to other electric discharges.

Here I do not fear any accusation of pedantry in joining those who urge that we should adapt our language to the modern view. It would be a very real gain, a great assistance to clear thinking, if we could entirely abolish the word 'cause' in physical description, cease to say 'why' things happen unless we wish to signify an antecedent purpose, and be content to own that our laws are but expressions of 'how' they occur.

The aim of explanation, then, is to reduce the number of laws as far as possible, by showing that laws, at first separated, may be merged in one; to reduce the number of chapters in the book of science by showing that some are truly mere sub-sections of chapters already written.

To take an old but never-worn-out metaphor, the physicist is examining the garment of Nature, learning of how many, or rather of how few different kinds of thread it is woven, finding how each separate thread enters into the pattern, and seeking from the pattern woven in the past to know the pattern yet to come.

How many different kinds of thread does Nature use?

So far, we have recognized some eight or nine, the number of different forms of energy which we are still obliged to count as distinct. But this distinction we cannot believe to be real. The relations between the different forms of energy, and the fixed rate of exchange when one form gives place to another, encourage us to suppose that if we could only sharpen our senses, or change our point of view, we could effect a still further reduction. We stand in front of Nature's loom as we watch the weaving of the garment; while we follow a particular thread in the pattern it suddenly disappears, and a thread of another color takes its place. Is this a new thread, or is

it merely the old thread turned round and presenting a new face to us? We can do little more than guess. We cannot get to the other side of the pattern, and our minutest watching will not tell us all the working of the loom.

Leaving the metaphor, were we true physicists, and physicists alone, we should, I suppose, be content to describe merely what we observe in the changes of energy. We should say, for instance, that so much kinetic energy ceases, and that so much heat appears, or that so much light comes to a surface, and that so much chemical energy takes its place. But we have to take ourselves as we are, and reckon with the fact that though our material is physical, we ourselves are psychical. And, as a mere matter of fact, we are not content with such discontinuous descriptions. We dislike the discontinuity and we think of an underlying identity. We think of the heat as being that which a moment before was energy of a visible motion, we think of the light as changing its form alone and becoming itself the chemical energy. Then to our passive dislike to discontinuity we join our active desire to form a mental picture of what may be going on, a picture like something which we already know. Coming on these discontinuities our ordinary method of explanation fails, for they are not obviously like those series of events in which we can trace every step. We then imagine a constitution of matter and modifications of it corresponding to the different kinds of energy, such that the discontinuities vanish, and such that we can picture one form of energy passing into another and yet keeping the same in kind throughout. We are no longer content to describe what we actually see or feel, but we describe what we imagine we should see or feel if our senses were on quite another scale of magnitude and sensibility. We cease to be physicists of the real and become physicists of the ideal.

To form such mental pictures we naturally choose the sense which makes such pictures most definite, the sense of sight, and think of a constitution of matter which shall enable us to explain all the various changes in terms of visible motions and accelerations. We imagine a mechanical constitution of the universe.

We are encouraged in this attempt by the fact that the relations in this mechanical conception can be so exactly stated, that the equations of motion are so very definite. We have, too, examples of mechanical systems, of which we can give accounts far exceeding in accuracy the accounts of other physical systems. Compare, for instance, the accuracy with which we can describe and foretell the path of a planet with our ignorance of the movements of the atmosphere as dependent on the heat of the sun. The planet keeps to the astronomer's time table, but the wind still bloweth almost where it listeth.

The only foundation which has yet been imagined for this mechanical explanation—if we may use 'explanation' to denote the likening of our imaginings to that which we actually observe—is the atomic and molecular hypothesis of matter. This hypothesis arose so early in the history of science that we are almost tempted to suppose that it is a necessity of thought, and that it has a warrant of some higher order than any other hypothesis which could be imagined. But I suspect that if we could trace its early development we should find that it arose in an attempt to explain the phenomena of expansion and contraction, evaporation and solution. Were matter a continuum we should have to admit all these as simple facts, inexplicable in that they are like nothing else. But imagine matter to consist of a crowd of separate particles with interspaces. Contraction and expansion are then merely a drawing in and a widening out of the crowd. Solu-

tion is merely the mingling of two crowds, and evaporation merely a dispersal from the outskirts. The most evident properties of matter are then similar to what may be observed in any public meeting.

For ages the molecular hypothesis hardly went further than this. The first step onward was the ascription of vibratory motion to the atoms to explain heat. Then definite qualities were ascribed, definite mutual forces were called into play to explain elasticity and other properties or qualities of matter. But I imagine its first really great achievement was its success in explaining the law of combining proportions, and next to that we should put its success in explaining many of the properties of gases.

While light was regarded as corpuscular—in fact molecular, and while direct action at a distance presented no difficulty, the molecular hypothesis served as the one foundation for the mechanical representation of phenomena. But when it was shown that infinitely the best account of the phenomena of light could be given on the supposition that it consisted of waves, something was needed, as Lord Salisbury has said, to wave, both in the interstellar and in the intermolecular spaces. So the hypothesis of an ether was developed, a necessary complement of that form of the molecular hypothesis in which matter consists of discrete particles with matter-free intervening spaces.

Then Faraday's discovery of the influence of the dielectric medium in electric actions led to the general abandonment of the idea of action at a distance, and the ether was called in to aid matter in the explanation of electric and magnetic phenomena. The discovery that the velocity of electro-magnetic waves is the same as that of light waves is at least circumstantial evidence that the same medium transmits both.

I suppose we all hope that some time we shall succeed in attributing to this medium such further qualities that it will be able to enlarge its scope and take in the work of gravitation.

The mechanical hypothesis has not always taken this dualistic form of material atoms and molecules, floating in a quite distinct ether. I think we may regard Boscovich's theory of point-centers surrounded by infinitely extending atmospheres of force as really an attempt to get rid of the dualism, and Faraday's theory of point-centers with radiating lines of force is only Boscovich's theory in another form. But Lord Kelvin's vortex-atom theory gives us a simplification more easily thought of. Here all space is filled with continuous fluid—shall we say a fluid ether—and the atoms are mere loci of a particular type of motion of this frictionless fluid. The sole differences in the atoms are differences of position and motion. Where there are whirls, we call the fluid matter; where there are no whirls we call it ether. All energy is energy of motion. Our visible kinetic energy, $MV^2/2$, is energy in and around the central whirls; our visible energy of position, our potential energy, is energy of motion in the outlying regions.

A similar simplification is given by Dr. Larmor's hypothesis, in which, again, all space is filled with continuous substance all of one kind, but this time solid rather than fluid. The atoms are loci of strain instead of whirls, and the ether is that which is strained.

So, as we watch the weaving of the garment of Nature, we resolve it in imagination into threads of ether spangled over with beads of matter. We look still closer, and the beads of matter vanish; they are mere knots and loops in the threads of ether.

The question now faces us—How are we to regard these hypotheses as to the consti-

tution of matter and the connecting ether? How are we to look upon the explanations they afford? Are we to put atoms and ether on an equal footing with the phenomena observed by our senses, as truths to be investigated for their own sake? Or are they mere tools in the search for truth, liable to be worn out or superseded?

That matter is grained in structure is hardly more than the expression of the fact that in very thin layers it ceases to behave as in thicker layers. But when we pass on from this general statement and give definite form to the granules or assume definite qualities to the intergranular cement, we are dealing with pure hypotheses.

It is hardly possible to think that we shall ever see an atom or handle the ether. We make no attempt whatever to render them evident to the senses. We connect observed conditions and changes in gross visible matter by invisible molecular and ethereal machinery. The changes at each end of the machinery of which we seek to give an account are in gross matter, and this gross matter is our only instrument of detection, and we never receive direct sense impressions of the imagined atoms or the intervening ether. To a strictly descriptive physicist their only use and interest would lie in their service in prediction of the changes which are to take place in gross matter.

It appears quite possible that various types of machinery might be devised to produce the known effects. The type we have adopted is undergoing constant minor changes, as new discoveries suggest new arrangements of the parts. Is it utterly beyond possibility that the type itself should change?

The special molecular and ethereal machinery which we have designed, and which we now generally use, has been designed because our most highly developed sense is our sense of sight. Were we otherwise,

had we a sense more delicate than sight, one affording us material for more definite mental presentation, we might quite possibly have constructed very different hypotheses. Though, as we are, we cannot conceive any higher type than that founded on the sense of sight, we can imagine a lower type, and by way of illustration of the point let us take the sense of which my predecessor spoke last year—the sense of smell. In us it is very undeveloped. But let us image a being in whom it is highly cultivated, say, a very intellectual and very hypothetical dog. Let us suppose that he tries to frame an hypothesis as to light. Having found that his sense of smell is excited by surface exhalations, will he not naturally make and be content with a corpuscular theory of light? When he has discovered the facts of dispersion, will he not think of the different colors as different kinds of smell—insensible, perhaps, to him, but sensible to a still more highly gifted, still more hypothetical dog?

Of course, with our superior intellect and sensibility, we can see where his hypothesis would break down; but unless we are to assume that we have reached finality in sense development, the illustration, grotesque as it may be, will serve to show that our hypotheses are in terms of ourselves rather than in terms of Nature itself, they are ejective rather than objective, and so they are to be regarded as instruments, tools, apparatus only to aid us in the search for truth.

To use an old analogy—and here we can hardly go except upon analogy—while the building of Nature is growing spontaneously from within, the model of it, which we seek to construct in our descriptive science, can only be constructed by means of scaffolding from without, a scaffolding of hypotheses. While in the real building all is continuous, in our model there are detached parts which must be connected with the rest by tem-

porary ladders and passages, or which must be supported till we can see how to fill in the understructure. To give the hypotheses equal validity with facts is to confuse the temporary scaffolding with the building itself.

But even if we take this view of the temporary nature of our molecular and ethereal imaginings, it does not lessen their value, their necessity to us.

It is merely a true description of ourselves to say that we must believe in the continuity of physical processes, and that we must attempt to form mental pictures of those processes, the details of which elude our observation. For such pictures we must frame hypotheses, and we have to use the best material at command in framing them. At present there is only one fundamental hypothesis—the molecular and ethereal hypothesis—in some such form as is generally accepted.

Even if we take the position that the form of the hypothesis may change as our knowledge extends, that we may be able to devise new machinery—nay, even that we may be able to design some quite new type to bring about the same ends—that does not appear to me to lessen the present value of the hypothesis. We can recognize to the full how well it enables us to group together large masses of facts which, without it, would be scattered apart, how it serves to give working explanations, and continually enables investigators to think out new questions for research. We can recognize that it is the symbolical form in which much actual knowledge is cast. We might almost as well quarrel with the use of the letters of the alphabet, inasmuch as they are not the sounds themselves, but mere arbitrary symbols of the sounds.

In this country there is no need for any defence of the use of the molecular hypothesis. But abroad the movement from the position in which hypothesis is confounded

with observed truth has carried many through the position of equilibrium equally far on the other side, and a party has been formed which totally abstains from molecules as a protest against immoderate indulgence in their use. Time will show whether these protesters can do without any hypothesis, whether they can build without scaffolding or ladders. I fear that it is only an attempt to build from balloons.

But the protest will have value if it will put us on our guard against using molecules and the ether everywhere and everywhen. There is, I think, some danger that we may get so accustomed to picturing everything in terms of these hypotheses that we may come to suppose that we may have no firm basis for the facts of observation until we have given a molecular account of them, that a molecular basis is a firmer foundation than direct experience.

Let me illustrate this kind of danger. The phenomena of capillarity can, for the most part, be explained on the assumption of a liquid surface tension. But if the subject is treated merely from this point of view it stands alone—it is a portion of the building of science hanging in the air. The molecular hypothesis then comes in to give some explanation of the surface tension, gives, as it were, a supporting understructure connecting capillarity with other classes of phenomena. But here, I think, the hypothesis should stop, and such phenomena as can be explained by the surface tension should be so explained without reference to molecules. They should not be brought in again till the surface-tension explanation fails. It is necessary to bear in mind what part is scaffolding, and what is the building itself, already firm and complete.

Or, as another illustration, take the Second Law of Thermodynamics. I suspect that it is sometimes supposed that a molecular theory from which the Second Law

could be deduced would be a better basis for it than the direct experience on which it was founded by Clausius and Kelvin or, that the mere imagining of a Maxwell's sorting demon has already disproved the universality of the law; whereas he is a mere hypothesis grafted on a hypothesis, and nothing corresponding to his action has yet been found.

There is more serious danger of confusion of hypothesis with fact in the use of the ether; more risk of failure to see what is accomplished by its aid. In giving an account of light, for instance, the right course, it appears to me, is to describe the phenomena and lay down the laws under which they are grouped, leaving it an open question what it is that waves, until the phenomena oblige us to introduce something more than matter, until we see what properties we must assign to the ether, properties not possessed by matter, in order that it may be competent to afford the explanations we seek. We should then realize more clearly that it is the constitution of matter which we have imagined, the hypothesis of discrete particles, which obliges us to assume an intervening medium to carry on the disturbance from particle to particle. But the vortex-atom hypothesis and Dr. Larmor's strain-atom hypothesis both seem to indicate that we are moving in the direction of the abolition of the distinction between matter and ether, that we shall come to regard the luminiferous medium, not as an attenuated substance here and there encumbered with detached blocks—the molecules of matter—but as something which in certain places exhibits modifications which we term matter. Or starting rather from matter, we may come to think of matter as no longer consisting of separated granules, but as a continuum with properties grouped round the centers, which we regard as atoms or molecules.

Perhaps I may illustrate the danger in the use of the conception of the ether by considering the common way of describing the electro-magnetic waves, which are all about us here, as ether waves. Now in all cases with which we are acquainted, these waves start from matter; their energy before starting was, as far as we can guess, energy of the matter between the different parts of the source, and they manifest themselves in the receiver as energy of matter. As they travel through the air, I believe that it is quite possible that the electric energy can be expressed in terms of the molecules of air in their path, that they are effecting atomic separations as they go. If so, then the air is quite as much concerned in their propagation as the ether between its molecules. In any case, to term them ether waves is to prejudice the question before we have sufficient evidence.

Unless we bear in mind the hypothetical character of our mechanical conception of things, we may run some risk of another danger—the danger of supposing that we have something more real in mechanical than in other measurements. For instance, there is some risk that the work measure of specific heat should be regarded as more fundamental than the heat measure, in that heat is truly a 'mode of motion.' On the molecular hypothesis, heat is no doubt a mixture of kinetic energy and potential energy of the molecules and their constituents, and may even be entirely kinetic energy; and we may conceivably in the future make the hypothesis so definite that, when we heat a gramme of water 1° , we can assign such fraction of an erg to each atom. But look how much pure hypothesis is here. The real superiority of the work measure of specific heat lies in the fact that it is independent of any particular substance, and there is nothing whatever hypothetical about it.*

*This risk of imagining one particular kind of

Another illustration of the illegitimate use of our hypothesis, as it appears to me, is in the attempt to find in the ether a fixed datum for the measurement of material velocities and accelerations, a something in which we can draw our coördinate axes so that they will never turn or bend. But this is as if, discontented with the movement of the earth's pole, we should seek to find our zero lines of latitude and longitude in the Atlantic Ocean. Leaving out of sight the possibility of ethereal currents which we cannot detect, and the motions due to every ray of light which traverses space, we could only fix positions and directions in the ether by buoying them with matter. We know nothing of the ether, except by its effects on matter, and, after all, it would be the material buoys which would fix the positions and not the ether in which they float.

The discussion of the physical method, with its descriptive laws and explanations, and its hypothetical extension of description, leads us on to the consideration of the limitation of its range. The method was developed in the study of matter which we describe as non-living, and with non-living measure more real than another, more in accordance with the truth of things, may be further illustrated by the common idea that mass-acceleration is the only way to measure a force. We stand apart from our mechanical system and watch the motions and the accelerations of the various parts, and we find that mass-accelerations have a certain significance in our system. If we keep ourselves outside the system and only use our sense of sight, then mass-acceleration is the only way of describing that behavior of one body in the presence of others which we term force on it. But if we go about in the system and pull and push bodies, we find that there is another conception of force, in which another sense than sight is concerned—another mode of measurement much more ancient and still far more extensively used—the measurement by weight supported. Each method has its own range; each is fundamental in that range. It is one of the great practical problems in physics to make the pendulum give us the exact ratio of the units in the two systems.

matter the method has sufficed for the particular purposes of the physicist. Of course only a little corner of the universe has been explored, but in the study of non-living matter we have come to no impassable gulfs, no chasms across which we cannot throw bridges of hypothesis. Does the method equally suffice when it is applied to living matter? Can we give a purely physical account of such matter, likening its motions and changes to other motions and changes already observed, and so explaining them? Can we group them in laws which will enable us to predict future conditions and positions?—The ancient question never answered, but never ceasing to press for an answer.

Having faith in our descriptive method, let us use it to describe our real attitude on the question. Do we, or do we not, as a matter of fact, make any attempt to apply the physical method to describe and explain those motions of matter which on the psychological view we term voluntary?

Any commonplace example, and the more commonplace the more it is to the point, will at once tell us our practice, whatever may be our theory. For instance, a steamer is going across the Channel. We can give a fairly good physical account of the motion of the steamer. We can describe how the energy stored in the coal passes out through the boiler into the machinery, and how it is ultimately absorbed by the sea. And the machinery once started, we can give an account of the actions and reactions between its various parts and the water, and if only the crew will not interfere, we can predict with some approach to correctness how the vessel will run. All these processes can be likened to processes already studied—perhaps on another scale—in our laboratories, and from the similarities prediction is possible. But now think of a passenger on board who has received an invitation to take the journey. It is simply a matter of

fact that we make no attempt at a complete physical account and explanation of those actions which he takes to accomplish his purpose. We trace no lines of induction in the ether connecting him with his friends across the Channel, we seek no law of force under which he moves. In practice the strictest physicist abandons the physical view, and replaces it by the psychical. He admits the study of purpose as well as the study of motion.

He has to admit that here his physical method of prediction fails. In physical observations one set of measurements may lead to the prediction of the results of another set of measurements. The equations expressing the laws imply different observations with some definite relation between their results, and if we know one set of observations and that definite relation we can predict the result of the other set. But if we take the psychical view of actions, we can only measure the actions. We have no independent means of studying and measuring the motions which preceded the actions, we can only estimate their value by the consequent actions. If we formed equations they would be mere identities with the same terms on either side.

The consistent and persistent physicist, finding the door closed against him, finding that he has hardly a sphere of influence left to him in the psychical region, seeks to apply his methods in another way, by assuming that if he knew all about the molecular positions and motions in the living matter, then the ordinary physical laws could be applied, and the physical conditions at any future time could be predicted. He would say, I suppose, with regard to the Channel passenger, that it is absurd to begin with the most complicated mechanism, and seek to give a physical account of that. He would urge that we should take some lower form of life where the structure and motions are simpler, and apply the physical methods to that.

Well, then, let us look for the physical explanation of any motion which we are entitled from its likeness to our own action to call a voluntary motion. Must we not own that even the very beginning of such explanation is as yet non-existent? It appears to me that the assumption that our methods do apply, and that purely physical explanation will suffice to predict all motions and changes, voluntary and involuntary, is at present simply a gigantic extrapolation, which we should unhesitatingly reject if it were merely a case of ordinary physical investigation. The physicist when thus extending his range is ceasing to be a physicist, ceasing to be content with his descriptive methods in his intense desire to show that he is a physicist throughout.

Of course we may describe the motions and changes of any type of matter after the event, and in a purely physical manner. And as Professor Ward has suggested, in a most important contribution to this subject which he has made in his recently published *Gifford Lectures*,* where ordinary physical explanations fail to give an account of the motions, we might imagine some structure in the ether, and such stresses between the ether and matter that our physical explanations should still hold. But, as Professor Ward says, such ethereal constructions would present no warrant for their reality or consistency. Indeed they would be mere images in the surface of things to account for what goes on in front of the surface, and would have no more reality than the images of objects in a glass.

If we have full confidence in the descriptive method, as applied to living and non-living matter, it appears to me that up to the present it teaches us that while in non-living matter we can always find similarities, that, while each event is like other events, actual or imagined, in a living being there

* 'Naturalism and Agnosticism,' *The Gifford Lectures*, 1896-98, Vol II., p. 71.

are always dissimilarities. Taking the psychical view—the only view which we really do at present take—in the living being there is always some individuality, something different from any other living being, and full prediction in the physical sense, and by physical methods is impossible. If this be true, the loom of Nature is weaving a pattern with no mere geometrical design. The threads of life, coming in we know not where, now twining together, now dividing, are weaving patterns of their own, ever increasing in intricacy, ever gaining in beauty.

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*THE WORK OF THE INTERNATIONAL ASSOCIATION FOR TESTING MATERIALS.**

PRIOR to the year 1800 little was known of the properties of the materials of construction. Gallileo had shown in 1638 that the strength of a rectangular beam varied with the square of its depth; Hooke in 1678 had announced the law that the stretch of a spring was proportional to the stress upon it; various authors had discussed the forms of beams of uniform strength, and Euler, in 1744, had enunciated his formula for the resistance of columns under compression. The theory was far in advance of practice, for experiments had been so few and so imperfect that the elastic limit was scarcely recognized.

During the years from 1800 to 1850 great progress was made in the theory of elasticity, and a slow growth took place in knowledge of the properties of materials under stress. The introduction of railways and the consequent necessity of providing a firm roadbed and safe bridge structures gave a powerful stimulus to the investigation of metals, in order that ample security might be afforded with the greatest degree of economy. The methods of testing were,

however, so imperfect that progress was slow, and, with the exception of the classic researches of Hodgkinson, the work of this period was mostly of value as a preparation for that of the future.

After 1850 large testing machines for special purposes began to be built, elongation and ductility began to be carefully studied, and soon after 1870, it was recognized by many manufacturers that physical tests of metals were imperatively necessary in order to secure uniformity of product. As these tests were multiplied and the records subjected to investigation, the knowledge was gained that the strength of a specimen depended upon its size and proportions and also upon the manner in which the load was applied. The term elastic limit assumed a new significance when it became recognized that it could be defined and measured in different ways. In short, it was found that tests of materials must be made in a similar manner in order to render the results comparable. This idea, although long recognized, has proved a difficult one to realize. It has been discussed by many engineering societies, some of which have attempted to formulate standard methods. Finally the International Association for Testing Materials was formed in order to study the whole subject and endeavor to arrive at conclusions that should be authoritative.

In 1882, through the influence of John Bauschinger, a number of German experimenters met at Munich and discussed the question as to how uniformity in the methods of testing materials could be promoted. As a result of this meeting, formal conferences were held at Dresden in 1884, at Berlin in 1886, at Munich in 1888, and at Vienna in 1893, delegates from other European countries being often present. The reports of the proceedings of these conferences, published in Bauschinger's *Mittheilungen*, attracted wide attention, and the

* An address by the Chairman of the American Section of the Association, at the second annual meeting held in Pittsburg, Pa., August 15-16, 1899.