neglect the opportunities that practice yields for accurate, original investigation or for the deliberate, logical reflection that helps to expand the confines of knowledge. Moreover, minute accretions to the store of knowledge are not to be despised; indeed, were it not for the continuous collection of such small increments fewer of the so-called great discoveries would be made, since for the most part great discoveries are nothing but the summits of structures composed of a mass of minor investigative results.

Men who continuously insist upon the cross-examination of the evidences of their senses, men who, when they observe an object or an event, try to consider also all the conditions in which that object or event is situated, men who examine into the variations, the concurrences, the sequences and the mutual relations of phenomena, men who are cautious not to insert their own beliefs and expectations and wishes into their observations, men who are so distrustful of their memories that they record accurately what they observe at the time and place of observation, men who are not satisfied merely with thinking of explanations but insist upon testing them in order to see whether or not they are true, are the men who discover new things, who detect errors in conceptions that are supposed to be well founded, and who, in general, do most to contribute to the advance of our knowledge of health and disease.

The growth of knowledge has often been compared to that of an avalanche; its magnitude and its velocity must steadily increase. Every single advance prepares the way for a whole series of other advances. Every new method devised makes it easier to invent still other modes of extending observation. The physician who died in 1900, could he come to life again, would be astonished and perhaps confused by the medicine of 1925; if any one of us were to stop his medical work to-day and were to try to resume it ten years from now he would doubtless find himself almost as disoriented in special medical fields as is a patient in his general surroundings when he suffers from Korsakoff's psychosis!

You who are members of this Undergraduate Medical Association enter upon your medical activities at the most favorable period the world has ever known for the enlargement of medical knowledge and for the advancement of human welfare. Just what part each of you who will graduate here (from an institution made famous by Cope, Leidy and a long series of original investigators) will play in this enlargement and this advancement remains to be seen. There will be many difficulties in the way; for an exaggerated account of them consult the recent clever novel "Arrowsmith" by Sinclair Lewis. But difficulties are only additional spurs to determined men. Some of you will have better brains than others but each of us has to do the best he can with the brain that he possesses. Some of you will be able to command more leisure for scientific studies than will others, but again every one can secure some leisure for scientific work if he will make his life orderly. Some of you may be privileged to work in institutes devoted entirely to original research; some of you will find it advantageous to combine research with teaching; the majority, perhaps, will engage in medical practice of one sort or another and will there find the special opportunities for certain kinds of scientific work that can not be found apart from such practice. No matter where your lot may be cast the main thing is that you utilize every opportunity that comes to you to observe accurately, to record carefully, to imagine vividly, to reason logically, to experiment cautiously and to test rigidly the validity of the explanatory ideas that occur to you. For these are not only pleasures that you may enjoy; they are also obligations and responsibilities that fall upon you as medical men. May I extend to each and every one of you my very best wishes for happiness, success and duty fulfilment in your work during the coming years.

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THE TREND OF THOUGHT IN PHYSICS. II

STATIONARY STATES AND THE LAWS DESCRIBING ELECTRONIC MOTION

APART from the nature of emission of quanta during the transitions between the stationary states, there yet remains the problem of accounting for the stationary states themselves. Now the mind is content that the electron should not be allowed to move in any manner it pleases. It is content to have an equation of motion of the Newtonian form, for example, a simplification of the force equation of electrodynamics, for this case; but, having become accustomed to this, it resents any further conditions of constraint upon the motion, such as are involved in the Wilson-Sommerfeld conditions that the quantities $\int pdq$ shall be integral multiples of h, where the p's and q's are suitably chosen momenta and coordinates.

Now the question arises as to what attitude of mind it is necessary for us to get into in order that the existence of stationary states shall seem reasonable to us in terms of the criteria for reasonableness to which we adapt ourselves. This may well be considered as part of the whole question of the description of motion.

Suppose we return for a moment to the circuital relations of electrodynamics with the ρ and $\rho\mathbf{u}$ terms absent, *i.e.*, suppose we return to the equations for free space. The electron, in this scheme of relations, is the place where $\mathbf{E}$ and $\mathbf{H}$ cease to be analytic functions of x, y, z and t—crudely speaking—the places where $\mathbf{E}$ and $\mathbf{H}$ become infinite.

Now it is easy to see that if the values of $\mathbf{E}$ and $\mathbf{H}$ be assigned at all parts of space at any one instant, the equations determine their values everywhere for all time, and with these values the motions of the singularities themselves. No law other than the circuital relations is necessary to determine the complete subsequent history of the system, once the initial conditions have been stated. This point was emphasized by Sir Joseph Larmor many years ago. However, corresponding to any motion we like to assign to a singularity, we can always determine fields $\mathbf{E}$ and $\mathbf{H}$ to correspond. But the equations are linear so that the addition of two such solutions results in a field which is itself a solution. In other words, as far as these equations are concerned, there is no mutual influence between the electrons. An initial assignment of the fields will control the motions of the singularities for all time, and so their mutual relations; but there is nothing in the equations themselves to prohibit any motion whatever. In fact, the scheme is too wide for our universe. The real function of such an equation as the force equation of electrodynamics is to restrict matters so that, of all the fields which might be possible with a sufficiently arbitrary choice of initial conditions, only those to be found in our universe are the subjects of discussion. In this sense, the force equation and even the law of gravitation constitute what, many years ago[10] I termed subsidiary laws, *i.e.*, laws which would be unnecessary, and indeed superfluous if we knew the initial conditions, but which serve to fill the gap caused by our ignorance of those conditions, by restricting the cases we are to regard as possible to those which could have evolved out of the actual initial conditions inherent in our universe.

Now it is possible to utilize this principle of adding additional conditions to still further restrict the atomic orbits beyond the restrictions implied in the laws of classical dynamics. To be explicit, suppose that, confining ourselves for the time being to circular orbits, we write down the ordinary equations of classical dynamics, and work out the radius, a, of the orbit in terms of the angular momentum p, for an electron of mass m revolving about a nucleus of charge E. It is easy to show that

$$a=\frac{p^2}{meE} \qquad (13)$$

[10] *Phil. Mag.*, S-6, Vol. 23, pp. 86–94, 1912.

Now suppose that I should write down some differential equation other than that of classical dynamics, and solve for the relation between the radius of the orbit and the quantity p which appeared as the angular momentum in the classical theory. I should obtain

$$a=f\ (p) \qquad (14)$$

where $f\ (p)$ is different from the right-hand side of (13), so that, in general, equations (13) and (14) would not be consistent. There might be certain values of p for which they were consistent, however, and if we take the classical law plus this new law as the expression of the laws of motion, they will together restrict the motion to these orbits. Thus suppose we consider the equation

$$\left(p-\frac{h}{2\pi}\right)\left(p-\frac{2h}{2\pi}\right)\ \cdots\ \left(p-\frac{nh}{2\pi}\right)=0 \qquad (15)$$

expand the left-hand side and, having picked out the term in the first power of p, equate p to the remainder of the expression, or rather its negative, say $\varphi\ (p)$, to which this equation makes p equal.

Suppose now we take as our dynamical laws the classical dynamical laws, whose solution is expressed in

$$a=\frac{p^2}{meE}$$

and the additional equation

$$a=\frac{[\varphi\ (p)]^2}{meE} \qquad (16)$$

These equations will only be consistent for the values of p given by

$$p=\frac{h}{2\pi},\frac{2h}{2\pi},\ \cdots\ \frac{nh}{2\pi}$$

which make p equal to $\varphi\ (p)$.

Thus if classical dynamics and equation (16) (which is itself a differential equation) be taken together as constituting the laws of dynamics, they will, for circular orbits, restrict the motion to the various orbits corresponding to the angular momenta

$$\frac{h}{2\pi},\frac{2h}{2\pi}\ \cdots\ \frac{nh}{2\pi}$$

By taking $\varphi\ (p)=p+A\ \sin\dfrac{2\pi^2 p}{h}$

where A is a constant, we obtain an expression which gives $\varphi\ (p)=p$ for $p=\dfrac{nh}{2\pi}$ where n has all values up to infinity. Of course, in a sense, the equations $\int pdq=nh$ provided by the Wilson-Sommerfeld theory constitute supplementary conditions analogous to equation (16), but the advantage of the latter is that it is in the form of a differential equation. It will of course be understood that the foregoing is only intended as a brief sketch illustrating the general idea of the process. It is a process in which the mind has to accustom itself to accept as fundamental a more restricting relation between the function of the position of the electron and the various time derivatives of its motion than can be expressed in a single vectorial differential equation. It must remain incomplete as does the restriction involving $\int pdq$ until formulated in such a way as to permit of the departure from one stationary state and the entry into another as determined by those phenomena, emission of unifrequentic radiation, emission of quanta, etc., which we associate with the transition.

The Description of Motion in the Case of Non-Linear Equations

An interesting situation would have arisen had the circuital relations of electromagnetic theory been nonlinear. It would then have been impossible to have added two solutions and obtained as a result a field which was itself a solution. The story of two moving singularities would have to be sought as a solution of the field equations of such a nature that it contained two singularities, and even the most general solution would not permit of any motions we chose to assign. The field equations themselves would have a power to restrict the motion of one singularity in relation to another, a power which is denied to the electromagnetic field equations on account of their linearity, a linearity which thus gives mathematical simplicity at the expense of content, and necessitates the addition of a force equation to make up for the deficiency.

In a sense, the determination of the interactions between electrons by regarding them as singularities in the solution of non-linear equations is philosophically very satisfying. The process consists in setting up differential equations in which the quantities talked about are singularities of the solutions, and then seeking a one-one correspondence between these singularities and the observable entities of nature. The whole story is told in one set of equations. The reason for the existence of stationary states of quantum theory would then find itself in the fact that there would be no solutions of the equations corresponding to motions of the singularities other than the motions desired. The transition to classical theory would find its way by virtue of the ordinary force equation of electrodynamics being a close approximation to the law of the singularities for the cases where it was applicable. Tempting as the possibilities seem from the philosophical standpoint, it would appear that the mathematical difficulties incidental to handling the situation would be very great. However, these difficulties are apt to demand our attention whether we like it or not. Our natural inclination is to describe the relative motions of particles by force equations, or at any rate by attempting to write a differential equation which describes the motion of one particle considered as a thing by itself in the field of the others. If, however, the nature of the complete theory is such that the particles form singularities in a set of non-linear equations, it is necessary to ascertain whether any additional law which may be imposed is consistent with the restrictions on the motion imposed by this condition.

I place some emphasis on this matter because it appears to be very pertinent in connection with the description of motion as given by Einstein's General Theory of Relativity. Much as I should like to speak at some length of this great revolution of our method of looking upon nature which has engaged so much of our attention during recent years, it is of course impossible adequately to deal with it in the few minutes I can devote to it. If, however, you will pardon the use of statements in a somewhat elastic form, I should like to sketch a few of the elements involved particularly in relation to the point I am discussing at present, the modes which we adopt for the description of motion.

General Relativity and the Description of Motion; the General Invariance of Laws

The mind has decided that it likes the idea of a body moving in a straight line with constant velocity, and it would like to see everything going in that way. The sense in which we use the term straight line is that of a linear relation between the coordinates of space and time.

We can avoid the twofold ideas—straight line, constant velocity, by saying that we should like the body to move so that $\int ds$ is a minimum, *i.e.*, $\delta \int ds = 0$ where[11]

[11] I have avoided the introduction of the velocity of light, *c*, in the line element at this stage owing to a preference for having it make its first appearance as a constant in the expression for the line element which has to be chosen in order to obtain a law of motion approximating to the Newtonian law and with g's satisfying $G_{\mu\nu} = 0$. In this capacity c serves to determine a limiting velocity for planetary motion, and the choice of c necessary to explain the anomalies in the perihelion motion of Mercury is consistent with its being equal to the velocity of light. The nature of the occurrence of c in the line element is such that far removed from matter we have $ds^2 = dx^2 + dy^2 + dz^2 - c^2dt^2$. It is now a matter of mere algebra to show that this expression is invariant under the Lorentzian transformation, although it must be remarked that this does not imply the restricted theory of relativity in its entirety as a necessary consequence, for there is nothing to show that observers moving relatively to each other with constant rectilinear velocity will automatically adopt the relation of measures given by the Lorentzian transformation.

$$ds^2 = dx^2 + dy^2 + dz^2 + dt^2 \qquad (17)$$

Now if we imagine any arbitrary point transformation of coordinates,

$$x = f_1(x_1, y_1, z_1, t_1),\ y = f_2(x_1, y_1, z_1, t_1),$$
$$z = f_3(x_1, y_1, z_1, t_1),\ t = f_4(x_1, y_1, z_1, t_1)$$

the most general value assumed by ds is

$$ds^2 = g_{11}\, dx_1{}^2 + g_{12}\, dx_1 dy_1 + \ . \ . \ .$$

where the g's are functions of the new coordinates.

A path which was a straight line in the old set of coordinates will not be a straight line in the new set, although both paths would be recognized in their respective coordinates as the paths which made $\int sp$ a minimum. The question then arises as to whether any arbitrary motion which might be assigned could be described as a straight line with constant velocity in some other system of coordinates. The answer to the question is No. In spite of the infinite number of different kinds of motion describable in this way, all can not be so described. There is something peculiar about all the g's which have come from the g's 1, 1, 1, 1, of (17) by transformation. That something can be expressed in necessary and sufficient form as a set of differential equations for the g's with the coordinates as variables. This set of differential equations has in it nothing to indicate what particular set of coordinates is used. If the answer to our question of whether all paths could be reduced to rectilinear constant velocity paths by suitable transformation had been Yes, the whole law of motion under any circumstances whatever could have been expressed in astonishingly elegant form in the statement that

A particle moves so that, with zero variations at the limits,

$$\delta \int (g_{11}\, dx_1{}^2 + g_{12}\, dx_1 dy_1 \ . \ . \ . \ )^{\frac{1}{2}} = 0$$

where the g's are solutions of the set of differential equations to which I have referred, particular solutions depending on the coordinates used, each solution being determined in its own set of coordinates by specification of the appropriate initial conditions. The general law would not involve the specification of any particular set of coordinates, and would therefore be of a form to satisfy the wish, which many people feel, to be able to express nature's laws in a form invariant under any transformation of coordinates. If, as I have said to be the case, such a law is not wide enough to cover the circumstances of nature, what then? Well, it may be that the laws of nature are expressed under

$$\delta \int (g_{11}\, dx_1{}^2 + g_{12}\, dx_1 dy_1 + \ . \ . \ . \ )^{\frac{1}{2}} = 0$$

where the g's are solutions of some set of differential equations other than those which I referred to, a set of differential equations which is satisfied when the other set is satisfied, since the cases comprised under that heading must constitute a special case. The law in this widened form will still be the same in all systems of coordinates. Now what Einstein did was to suggest an appropriate differential equation for the g's such that the law

$$\delta \int (g_{11}\, dx_1{}^2 + g_{12}\, dx_1 dy_1 \ . \ . \ . \ )^{\frac{1}{2}} = 0$$

with the g's solutions of that set of equations, comprised the motions of astronomy, the understanding being that matter was to be regarded as existing at the places where the equations for the g's possessed singularities. All garnishing of this question by discussions of non-euclidean geometry of space is unnecessary for the statement of the law, and constitutes an elaboration clarifying to some minds and confusing to others, because the said others have formed about ordinary geometry intuitions which are stronger than they had any right to form. For here, as in many cases, the secret of understanding a subject which is abstract is a more clear realization of the fact that we ought not to have thought we understood the part which we regarded as non-abstract.

Now the main reason why I have dragged in this discussion of the Einstein theory is this. The motion of the particle has been described by the geodesic law, and the problem of the motion of one particle about another particle, regarded as fixed, was of course solved by Einstein. The g's which occur in that calculation are the g's as calculated for the fixed particle alone, and indeed the process of finding them was to find a solution of the differential equations which was symmetrical about the point which the particle was to occupy. Now, in electromagnetic theory, the linearity of the equations makes it possible to define what we mean by the field of one electron as distinct from that of another. But, the equations of the Einstein theory are non-linear, and this circumstance renders such a definition impracticable. We might still impart definiteness of statement into the geodesic law were we to say that the particle B moves in a geodesic in the field which the particle A would produce if alone in space. The non-linearity of the equations will not let us escape from the difficulty so easily, however, for, as I have already remarked, that very non-linearity itself insures a relation between the motions of the singularities from the mere fact that they are singularities in a solution of non-linear equations. Neither can we avoid the difficulty by diminishing very greatly the strength of the singularity whose motion we are discussing, with the idea of thereby diminishing its contribution to the g's; for, the *definiteness* with which the motion of a singularity is determined by the non-linearity of the equations does not necessarily diminish with the strength of the singularity. The question then remains as to whether the law of the geodesic is less stringent, more stringent or the analytical equivalent of the law imposed by the non-linearity. Until this matter is cleared up, there is a danger that the law of the geodesic may say too much.

At first sight, it may appear that the difficulty may be avoided by that line of attack in which the theory is thrown into a form in which the Matter-Energy Tensor is used to define the presence of a matter, and a set of equations of motion are apparently built up without the use of the geodesic law, to which, however, an attempt is made to show them equivalent in a special case. A careful examination of the procedure will, I think, reveal however that, in the case of the motion of a body for example, the quantity which figures in the theory as a velocity is not necessarily the space-time velocity of the region of discontinuity between what is defined as matter and what is defined as empty space, and it remains as an additional postulate that solutions of the equations which Einstein uses for his g's can be found for which the two are consistent.

The sort of difficulty here discussed must arise in any scheme in which matter is defined as present at points where there exist singularities of solutions of non-linear systems of differential equations, and in which an attempt is made to describe the motion other than through the laws which the differential equations themselves impose on that motion.

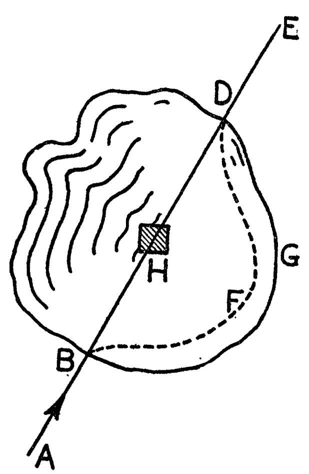
A good deal might be said as to the causes which have been responsible for the desire to express nature's laws in a form invariant under any transformation of coordinates, and for this I have no time. I should like, however, to make one remark on the meaning of the invariance of physical laws. For simplicity, let me for the moment confine my remarks to invariance in the sense of the restricted theory of relativity.

Suppose that we modify the equations of the Newtonian theory into a form consistent with the restricted theory, as in fact Poincaré did. Of course they will differ but very slightly from the Newtonian equations. Suppose the planets actually follow such a law. A particular solution of the equations will still correspond to one body, say the sun, with all the other planets moving in exact circles around it, apart, of course, from the mutual influence of the planets. Suppose I then made careful measurements and actually give ϑ and r as functions of t. The former would of course be proportional to t and the latter would be a constant, its magnitude being determined by the constant value of $\frac{d\vartheta}{dt}$. Suppose, now, I assert that the equations I have written down represent a law of nature since they are true for every planet I observe. Will my law be invariant under the transformation of the restricted theory? Certainly not. Is it inconsistent with the restricted theory? Certainly not. In fact, we started out with the supposition that the law I had found experimentally satisfies equations of motion consistent with the restricted theory, although I was not supposed to know that. Then here is a dilemma. The thing is true because I observed it. It is even a solution of equations consistent with relativity, but I must not call it a law because it itself is not invariant under the restricted theory. When is a thing which is law to be called a law and when is it not to be called a law?

If I should take my results concerning the planets' motions to a relativist he will say: "No, that is not the law; you have said too much. You have given an integration of the differential equations which express the law, and that contains much that is peculiar to your coordinates. The thing is not as invariant as all that." I shall ask him how much less I am supposed to say before he will permit my statement to pass as a law. He will show me those differential equations of the second order which, as we stated at the beginning, were supposed to be invariant, and of which my empirically found result constituted a solution. The relativist will say: "Now these differential equations say less than you have said, because, on integrating them there will be certain arbitrary constants which the equations themselves will not determine. These constants will be different for different systems of coordinates, so that what you will write down as the solution of these equations for that particular set of planetary motions which you have described will not appear the same in other sets of coordinates. Of course the most general solution of these equations, with the constants left arbitrary, will be of the same form in all systems of coordinates." Since, in order to say something which is invariant, I have been condemned to speak in differential equations of order no lower than the second, I now begin to wonder why I have been allowed even that amount of liberty. For I might have said less still by speaking in equations of higher order, since, in solving these equations it would have been necessary to specify more constants in order to make my solution definite. With equations of very high order, it would be necessary to add almost the whole story by the insertion of the various constants (and, in the case of partial differential equations, arbitrary functions), necessary to proceed from the differential equations to the solution.

Thus, if any one should ask me to state the general spirit of the statement of the general theory of relativity, I do not think that I should say that it implies that the laws of nature are invariant under any transformation of coordinates, for that is not fair to statements of truths which would appear to have every right to be called laws, but would not be invariant. I should feel inclined to say that the spirit of the principle is that it is possible to say *something* in a form invariant under any transformation of coordinates, even though, in the limit, when the equations are of very high order, that something might amount to nothing. An important feature of Einstein's theory is that it speaks in terms of equations of only the second order.

Apart from its relativistic bearing, the most revolutionary aspect of Einstein's gravitational theory is its substitution in place of force acting in opposition to kinetic reaction as a starting point in our thinking, the properties of a mathematical curve as a more fit thing in which to find a representation of nature's laws. It is not a question of one method being right and the other wrong, but a clear realization that neither has any claim to ultimate fundamentality other than that which its simplicity implies. A very crude analogy will serve to illustrate the nature of the situation.



Suppose that the figure represents a crater with a house H in the middle, and that a traveler sets out to go from A to E by the shortest path. He will not necessarily pursue the path $ABHDE$ leading down to the bottom of the crater, and through the house, because that may be too long. Nor will he necessarily go by the path $ABGDE$, because that may be too long. By taking some such course as $ABFDE$ crossing the crater part of the way down, it is possible that he will find a path shorter than any other, and this is the path he will take. Suppose now that while this is true, we know nothing about it, and that we find ourselves seated high up in an airplane watching the spectacle. Of course I shall not see the crater as such, everything will appear flat. I shall see the traveler going from A to E, and shall wonder why he does not go straight across and through the house. If I have been taught in my youth that a body moves in a straight line unless a force acts on it I shall conclude that the house repels him. Having come to this interesting conclusion, I shall ponder over the reason why the house repels him. Possibly I shall receive a sudden inspiration which will lead me to believe that the house contains a man, who is provided with a hose which he plays on the traveler, compelling him to keep away from his property. I may be able to describe the traveler's path very accurately in terms of the hose. Now if you are philosophically inclined, you may doubt the reality of this explanation, and ask me whether I am certain that there really is a hose in that house. Of course, if you press me hard enough, I shall find difficulty in proving that there is. If you worry me enough, however, I shall get very angry with you and call you an impracticable philosopher; but, feeling the need of saying something to your arguments, I may finally talk to you in this way: "I care not whether there is or is not a hose in that house. So long as by picturing one there, and expressing the traveler's motion in terms of its influence I can predict results which are true, I am on perfectly firm ground. I am responsible to no man for how I think so long as my conclusions are correct." Having said this, I shall feel that I have justified my attitude, and confounded you completely. I shall begin to feel a bit of a philosopher myself; but I shall continue to enjoy secretly the picture of this hose and the various details of its action. I shall think all sorts of things about it which I shall never dare to tell you lest you should laugh at me. I shall wonder what the density of the liquid composing the hose stream may be, what its boiling point may be, and so forth, and I shall thank heaven that I am not like you who have no hose to aid you; for I shall wonder how your mind can think at all unless it has something to think about. Now suppose that while I am doing all this you should bring me some observations which show that the motion of the traveler was not exactly what we thought it was. The difference might be very slight, but it might be of such a nature as to upset completely the simplicity of the action which I had imagined as going on by virtue of the hose. Of course, I shall first cling to the hose, but shall modify it slightly. I shall say "Naturally this is no ordinary kind of a hose. Possibly it does not push entirely in the direction in which the stream of liquid travels," but I shall have to face the situation that while the discrepancies may be small in amount they may be large in principle, and involve such radical alterations in my notions of the mechanism of the process that the hose which I shall have to picture will be radically different from any hose which I have ever seen. I shall go on in this way, modifying and adjusting the hose, making it more and more difficult to understand; and, forgetting that the original justification for its introduction was its apparent power to explain what was observed in terms of something which I thought I knew all about, I shall soon be in the position of expending 99 per cent. of my ingenuity in trying to understand the hose, leaving only one per cent. for the law of the traveler. Now suppose that, while I am doing this, and am feeling rather disheartened with my success, you should come to me and say, "I have made a discovery. I do not know why the traveler moves as he does, neither, I think, do you, but I have found exactly how he does move. He is moving from A to E by a path which is the shortest distance between those two points, not as the crow flies, but across a crater whose form I can describe to you in a very elegant way, and which appears to play a very important part in a lot of other phenomena going on down there, the direction which that little stream takes for example." Suppose you should say this, and should add: "Now, I am going to take this statement of the law as my starting point. If there is to be any hose in the matter, it is the hose which is going to be explained in terms of this fundamental law, and not the fundamental law in terms of the hose." I think I should have to admit that your attitude was at least reasonable. It is a change of viewpoint of this kind to which we have to adapt ourselves in passing over from the Newtonian description of motion to that adopted in Einstein's theory of gravitation.

Conclusion

In its fundamental aspects, practically the whole of modern physics is concerned with the discussion of the relations which exist between the motions of two sets of points in such a way as to establish a sort of one-one correspondence between the things which we do, and the behavior of one set of points on the one hand, and the things which we observe and the behavior of the other set of points on the other hand. We send a beam of electrons through an X-ray tube, and certain dark lines appear on photographic plates elsewhere, or electrons are emitted with certain velocities which we measure indirectly. As regards the electrons which are emitted into our apparatus, we can almost say that we observe them directly. As regards the blackening of the photographic plate, we are content if we can account for certain electronic emissions or motions to which we can attribute it. We do not, however, try to establish a direct relation between the original beam of electrons and the photographic plate or photoelectric cell, because we find that certain other apparatus was necessary for the experiment, a calcite grating and X-ray target, and so forth. These pieces of apparatus are replaced in the mind's eye by other sets of points grouped into atoms and molecules, in a manner characteristic of the substances in such a way that we may hope to be able to establish a relation between the first set of points, those in the target, those in the calcite and those in the final photographic plate or photoelectric cell. The whole problem is to discover how the points must be assigned and what function their mutual motions are of each other in order that the correlation may be satisfactorily made.

So far it has been possible to get along with points of two classes, the positive and negative electrons, and it would be considered artificial if we had to introduce anything else. The reason for this lies in the fact that only these two kinds of points occur in the specification of what we do and what we observe. In a certain sense we have isolated them, and the desire is to introduce into the atom nothing other than what we have actually observed outside. Apart from this, there would appear to be no logical objection to the introduction of other classes of points as intermediaries if they would prove helpful. Bohr's virtual oscillators and the quanta which we have discussed at some length in the foregoing are cases in point.

Now, it is in the problem of how the points control each other's motions that there appears that deference to traditions in our thinking which is the source of comfort on the one hand and trouble on the other. Our ideas of force and kinetic reactions which had their birth in an age which dealt only with the motions of the heavenly bodies, combined with a repugnance to anything savoring of action at a distance, has led us to describe the motions of any one of these points in terms of fields produced by the others. This description of the motion has invited many other ideas. We have felt the desire to define in terms of this field something whose radiation from one of the points is synonymous with decrease in the velocity of that point and whose influx into a region containing another point is synonymous with increase of motion of the point there in such a way as to maintain what we speak of as conservation of energy. The necessity of providing for the smooth working of these ideas has suggested extending the points into finite sizes, with the attachments of numbers (the volume density) to the volume elements to which they are extended, in such a manner as to retain the total charge constant. It is in the mechanism which we have provided for interrelating the motions of the points that most of our difficulties have occurred. The mere description of that interrelation is not difficult, and could readily be written down. Such a description as a complete statement would, however, be regarded as artificial without an interlocking mechanism built up in accordance with the ideas which we have inherited from our studies of matter in bulk. It is only little by little that we will allow these modes of expression to depart from us. Each step must be allowed time to grow; and not until it is firmly entrenched are we willing to admit a further extension. Classical electrodynamics itself affords a good illustration of a stage in such developments. I suppose it would be fair to say that, to-day, the average physicist would be very content if everything could be correlated on the basis of classical electrodynamics. Yet, at the time when it was born, electrodynamics was as incomprehensible to most people as the quantum theory is to-day. The desire to interpret it in terms of the properties of ordinary matter was compelling, and afforded difficulties of primary moment. To-day we would be satisfied with the theory itself if it were complete, and would be glad of it, not because we have any more of a picture in the sense in which Maxwell or Kelvin looked for a picture, but because we have developed a state of mind in which we have widened our criteria for reasonableness, the change having come about through continual contact with the phenomena correlated by that theory which was at first incomprehensible. It is only by close familiarity with the phenomena that radical changes in physical assumptions, which they suggest, and the corresponding moulding of our thoughts become part of our make-up in the sense that when something is explained in terms of them we say we understand the matter. This development of mental attitude takes place naturally with greatest intensity in the minds of those radicals in physical thought who are responsible for the new points of view, so that it is quite natural to find, as we do find, that radicals in physics are apt to become extremely conservative in their own radicalism.

It is a point which has been mooted before that there may be many ways of understanding the universe in terms of different fundamental starting points; and, once one has become imbued with this view, he is not averse to adopting different methods for the discussion of different classes of phenomena; for it is not unnatural to find that correlation in a limited field of nature's laws may be made in terms which are simpler than those which would aim to correlate all the phenomena of the universe. I sometimes picture the state of business of an optician whose activity is devoted to the manufacture of lenses, but, hearing of the unsatisfactory state of the quantum theory of dispersion, shuts up his shop until the matter is settled or even attempts to use the theory when it is settled.

How frequently somebody puts forth a theory founded on concepts which seem to have no place in a complete logical scheme, which even imply a misunderstanding of the fundamentals and which the philosopher could knock into a cocked hat in a minute if he so chose. How frequently he uses these concepts in a manner which tempts the mathematical physicist to tell him that he does not know what he is doing. Yet, how aggravatingly frequently he deduces from the illogical mess some new consequence which is found to be right and which may even have no place in the more profound philosopher's scheme of things. There is something in the fact that if we write down any correlation of a group of n phenomena where n is large, the chances are that, no matter what we may be thinking about, or what our picture of the processes is, if that scheme of correlation predicts a new phenomenon, that phenomenon will be found in nature. Even in its full generality, the scheme of nature's laws is not so elusive and fickle but that if we fit some scheme of regularity to it at one place we may, with reasonable chances of success, extrapolate that scheme of regularity beyond the place where we made the fit and predict new results. And it would seem that there need be no fundamental objection to the process, provided that one does not trouble himself about non-essentials —provided that if he uses an electron with mass, energy and so forth, he does not wonder what its color is—provided that, if he uses an ether for the purpose of visualizing the propagation of effects in terms of elasticity and inertia, he does not trouble himself as to why it does not freeze out in space where it is so cold, and why he does not therefore get light reflected from the boundary, and why the ether does not boil as the stars pass through it.

A power of critical insight which will enable us to show that, in the last analysis, nothing is real and most things are meaningless is all very well in its way; but it will not always carry its owner very far, and may frequently lead him into pessimism. Perhaps the most hopeful condition is a combination of critical insight with a none too delicate conscience when smelling out the truth. The owner of this combination may be subject to the criticism of being a particularly vicious sinner since he knows better, but his philosophy will save him the retribution of his misdeeds; for he will know not to concern himself with the color of the electron unless the matter has a bearing. He will be apt to reap only the wheat from his harvest and leave the tares.

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THE WHITE INDIANS OF THE SAN BLAS AND DARIEN

As a result of recent studies carried on in the Darien region of Panama, more specifically on the San Blas coast, certain facts concerning the reported White Indians have been brought to light.

The White Indians obviously express a form of albinism which has been termed imperfect or partial albinism by Geoffroy Saint Hilaire, Pearson and others. These terms signify that either the skin, hair or eyes, any two or all three may fail to express the full albinotic condition, but that one or more are, partially at least, relatively free from pigment.

Such a condition actually applies to the Indians in question. Many, if not all, of the White Indian males have frecklelike copper colored pigment spots of varying size, location and number, which evince an imperfect condition of albinism in the skin. Again the hair is not necessarily devoid of pigment, but in some cases shows traces of brown and in other cases is clearly auburn. Finally the iris is hazel (blue with brown spots), or dark blue, or dark violet. These observations clearly establish the fact that, if any classification of different degrees of albinism is valuable, as no doubt it is, the White Indians of Darien and the San Blas must be considered imperfect or partial albinos.

This contention, as opposed to the classification of these Indians as persons exhibiting idiopathic leucoderma, is supported by the fact that the condition obviously has a genetic basis, and that it is not the result of progressive development but is apparent at birth. Indeed its hereditary nature is demonstrated clearly in the hundred or more matings, the history of which I obtained during my residence in San Blas. If idiopathic leucoderma be considered as an inherited trait, then the terms imperfect and partial albinism and idiopathic leucoderma obviously approach a synonymous significance.

The origin of the condition expressed in the White Indians would seem to be most satisfactorily placed in the mutation theory. I saw no evidence of their origin being traceable to previous miscegenation with Caucasians.

The White Indians appear frequently from matings of brown San Blas Indians, resident on the islands just off the mainland, and on the mainland itself. Indeed this is the principal source of White Indians in the region, as whites are not permitted to mate with whites, and browns only very rarely mate with whites.

Do the White Indians form a race? If for the existence of a race one demands geographical segregation and permanency through the demonstrated production of likes by likes the White Indians can not be said to form a race, for they neither occur by themselves in segregated geographical areas nor are they permitted to reproduce their kind. But if in a definition of race one includes any group exhibiting strikingly differential characteristics which are insured of permanency by virtue of possessing a genetical basis, then the White Indians of the San Blas may be said to form at present a race which, due to artificial restrictions, is dependent for its continued appearance upon another race, carrying factors for the former's inherited differential characteristics in its germplasm. It is obvious that whatever definition is applied the difference is not qualitative but rather quantitative; the qualitative basis, genetical nature of differential characteristics, being present in either case. Whether or not it is admitted that the White Indians now form a distinct race there can be no question that, at least, they hold potentialities for race production.

Actually, it may be repeated, the whites do not occur by themselves in a segregated geographical area but are to be found in varying numbers in practically all the "brown" villages of the region, where they appear, from time to time, from matings of recessive carrying browns with recessive carrying browns. The whites form about 0.7 per cent. of the total population: an exceedingly high proportion for any form of albinism. This high proportion I attribute to the fact that intense inbreeding has occurred for some time in the Indian villages. Thus conditions are highly favorable for the frequent expression of any recessive traits occurring in the strain. Since all known forms of albinism are apparently recessive to the normal condition, and as albinism, like all recessive traits, occurs more frequently in consanguineous marriages than in the population at large, the observed high proportion of whites among the San Blas Indians is not astounding, in view of the constant inbreeding which occurs.

The exact method of inheritance functioning in the present instance is not yet established. The condition is apparently recessive to the normal, but it is not clear whether it is due primarily to the action of a single gene, in its interrelation to the whole chromosomal content, or whether the genetic composition