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THE RECENT CHANGE OF ATTITUDE TOWARD THE LAW OF CAUSE AND EFFECT[1]

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NEARLY every educated person, brought up in present-day society and under the influence of the scientific ideas and spirit which pervade our intellectual life, prides himself in the belief that nothing happens without there being some cause for it. We may briefly characterize this attitude of ours by saying that we believe in the law of cause and effect or in the causality principle. To many of not too cynical a temperament this attitude will seem the most sweeping characteristic of the mental difference between the superstitious savage and the cultivated product of a hard-won civilization.

It is now becoming common knowledge that one of the most startling developments of the altogether surprising progress of physics in the last few years has been a weakening of the belief of the physicist, at least, in the validity of the causality principle. I want to examine with you this situation—to inquire in what sense we are losing our conviction of the validity of the causality principle, and to discover some of the implications. I want especially to emphasize that I am concerned only with the objective aspects of the situation. The idea of causality which we shall discuss is as remote as possible from the subjective questions of free will or determinism which are often associated with it, both in popular discussions and in a number of recent more technical discussions by scientific men. We shall be concerned only with the domain accessible to experiment, and the causality principle, in the sense in which I use the term, is a principle dealing with the findings of actual experiments.

[1] Address delivered at the University of Wisconsin, April 21, 1931.

We shall not be able to get very far without trying to make a little more precise what we mean by the causality principle. What do we mean in any concrete situation by saying that the event A is the cause of the event B? There is at least one obvious condition to be satisfied; B must occur later than A, for no one would think of saying that something happening now was the cause of something which had already occurred in the past. But mere sequence in time is not enough. For instance, in society as at present constructed one's education is almost always completed before embarking on the sea of matrimony. But do we therefore say that the cause of our getting married is that we are educated? That is, there may be sequences in time which we recognize as due to accidental and irrelevant associations. What more, then, is necessary than mere sequence in time? I believe that examination will show that we must at least have invariable sequence—the event B must always follow the event A under all sorts of conditions. But this involves being able to repeat the experiment, and it seems to me that the idea of invariable recurrence on repetition is indeed essentially involved in the notion of causality and perhaps comes closest to what is usually meant by causality. For example, suppose that I have a heavy weight attached to a support by a string; then I shall find that whenever I cut the string the weight falls, so that my definition in terms of invariable sequence would lead me to assert that the cause of the falling of the weight was the cutting of the string. But the situation is not quite as simple as this, for in the repetition of the experiment as ordinarily performed a number of the conditions are not varied, and who shall say that some one of these invariable conditions is not responsible, and therefore the true cause, of the weight falling? For instance, if I could by some heroic means remove the earth, then we are all convinced that the weight would no longer fall when the string is cut, and so we could justify the contention that the true cause of the falling of the weight was not the cutting of the string but the presence of the earth. But a still more critical analyst might contend that even the presence of the earth would not be sufficient to make the weight fall if it were not for the law of gravitation, and that therefore the true cause of the weight falling is the law of gravitation.

An impartial examination of the various arguments would convince us that there is much to be said for each of them; if we should decide that one of the claimants was correct and the others wrong, I think we should find it impossible to defend our decision against the objections of the losers. This sort of dilemma occurs very frequently. Sometimes one tries to save the day by the introduction of such ideas as that of immediate and remote or primary and secondary causes, but no such compromise ever works satisfactorily, as may be seen in our simple example by selecting some definite feature as the primary cause of the falling of the weight, to the exclusion of all others.

The conclusion to be drawn from these difficulties is, I think, that the notion of causality is not sharp and can not be made logically precise, but it is only a common-sense notion, used in describing many situations of daily life with sufficient accuracy for ordinary needs. I believe that analysis will show, however, that all situations which may be described from the common-sense point of view as those to which the law of cause and effect applies have certain more general properties. Whenever our knowledge of a situation has become sufficiently deep for us to attempt an analysis of its various events into cause and effect, we find that we have merely had sufficient experience with the situation to observe certain uniformities of behavior, certain regularities in the sequence of events, of the sort that when certain events occur other definite events follow. This characteristic of uniformity I take to be the fundamental feature in the situation. The analysis of events into sequences which are causally related is simply one of the ways of exhibiting certain uniformities and recurrences.

What now is the criterion that in a given physical system one has mastered the essential uniformities and has not merely stumbled on something accidental? I think that every one will admit that the supreme test is the ability to predict; if our neighbor is always able to accurately predict the future behavior of any physical system, I believe that we should all admit that he had completely mastered the uniformities of the system. Conversely, I think that we shall easily admit that without uniformities in the past behavior of a system no conceivable basis for predicting the future exists. The essential thing, however, is the ability to predict; granted this we have comparatively little concern with the language or system of philosophy in terms of which our neighbor may choose to describe his feelings in the matter. Furthermore, we shall all, I think, admit that in a system completely governed by the law of cause and effect the future should be predictable if we know the complete past history of the system, and conversely, if we know how to predict the future from the past, we should expect to be able to formulate some sort of statement as to the uniformities of the system which could be put into the form of a law of cause and effect.

It appears, then, that uniformity, causality and predictability all have certain common aspects, and for rough purposes may be treated as more or less equivalent. It will suit the purposes of this exposition to lay the emphasis on predictability and I shall in the future be concerned with this aspect of the situation. We may now reformulate our statement of the beginning that nearly every civilized person believes in the law of cause and effect by saying that nearly every civilized person believes that the future of a system is in principle predictable when we know all about its past behavior. To avoid argument, I am perfectly willing to make the qualification that this discussion shall be limited to the realm of purely inanimate things, leaving out altogether biological phenomena.

As a historical fact, classical physics has been committed to a much more restricted formulation of the possibilities of prediction in stating that the future could be predicted if the present position and velocity of each particle is known. This conviction arose from the belief that the laws of Newtonian mechanics, which can be formulated in terms of differential equations of the second order, completely govern the motion of the actual physical universe. This formulation, however, is much more restricted than we need to make it for our argument and in fact there are difficulties with the Newtonian conception of predictability when attention is paid to various propagation effects which are discussed in elementary expositions of relativity theory. The essence of the situation for our purposes is that we have become convinced that the behavior of nature exhibits regularities of such a kind that from observation of the events of the past we can predict those of the future.

What now is the basis of this belief of ours in the thorough-going uniformity of nature or in the essential predictability of future events? It must at once be admitted that this belief in its wide-spread acceptance is largely an outgrowth of the last few hundred years. Savage and superstitious man sees in nature nothing but the capricious; it is only after long experience and observation that the simple uniformities begin to emerge, first the regularities in the motions of the heavenly bodies and then the same regularities in the simpler terrestrial mechanical systems. It was a tremendously stimulating discovery to find that simple uniformities in the motions of the planets which could be formulated in simple mechanical laws recurred also in the motion of terrestrial systems, and to find that as we acquired skill in analyzing systems of increasing complication these same simple uniformities continued to describe the uniformities being newly discovered. It is no wonder that after physics had experienced success after success in mastering systems of increasing complication it came to look on this success as no accident but the expression of an underlying principle. In some such way arose the conviction of the essential uniformity and predictability of nature; it was of course recognized that in the walks of daily life, as distinguished from the artificial situations of the laboratory, the power to predict was more conspicuous by its absence than its presence, but this was ascribed merely to the enormous complication of actual physical systems, and the conviction became general that by sufficiently refining the accuracy of our measurements and increasing their scope we might some day hope to reduce the most complicated system to predictability with any desired degree of accuracy.

It is to be emphasized that although the justification for this conviction arose entirely from experience of the external world, it constituted, nevertheless, even in the simplest possible case, an idealization of that experience. For in any concrete physical situation a prediction about a future event could be verified only within the limits of experimental error. The conviction that the future is predictable down to the last detail arose entirely from the experience that whenever the precision of measurements was increased the predictions which it was possible to make on the basis of the measurements always became better. But the uncertainty arising from experimental error could never be eliminated, and even in the most favorable cases the jump from the actual experiment to the ideal formulation of it involved a process which the mathematician or the physicist would describe as a long range extrapolation.

But now in the last few years all these expectations have changed, and the change has arisen primarily from the discovery of new experimental facts. Nothing previously found by experiment ceases to be true. Physics never has to retract statements about experimental facts when these statements are made with sufficient care to reproduce the physical situation with fidelity, that is, when due regard is paid to the limits of experimental error. Thus the law of gravitation should not be formulated baldly as the law of the inverse square, but rather that the attraction varies as the inverse square within certain limits of error, perhaps one part in a million, or whatever the greatest precision happens to be at present. The only genuine retractions which physics or other science has to make are in its statements about what it anticipates may in the future be found to be true about experimental realms not yet entered. So in the present situation, physics finds that it must retract a hope or expectation which it had based on previous experience, but it has not had to retract any statement about actual experiment. The expectation was that by increasing the accuracy of measurement indefinitely we would be able to make predictions about the future with indefinite precision. This turns out not to be true, for we have recently found that when we increase the refinement of our measurements beyond a certain point and enter a domain of small things not before accessible, the new domain is full of the most capricious irregularities, unlike the regularities in the domain of ordinary experience, so that in the new domain no refinement of measurement enables us to predict the future. The new domain in which this disturbing state of affairs holds is the domain in which the motions of single electrons or atoms are concerned, and is, of course, enormously remote from the domain of everyday affairs, in fact, so remote that only within the last few years have physical methods been sufficiently refined to enable us to enter this domain at all. The situation is not unlike that presented by the semi-convergent series of the mathematician. Situations are not uncommon in mathematics in which the goodness of the answer to a problem may be improved up to a certain point by increasing the labor of computation, but if labor is put into the calculation beyond this point the answer becomes poorer instead of better.

Our new understanding of the experimental situation can be made in the following bald statement: "As a matter of fact, events are not predictable in the realm of small things." This is practically equivalent to saying that in the realm of small things the law of cause and effect does not operate.

Some little experience has proved to me that this bald statement is likely to awaken in many persons the most active hostility, and this audience would indeed be unique if it did not contain an appreciable number of persons who positively bristle with animosity at such a statement. The reaction which this statement is most likely to produce is this: "You have not proved that in the realm of small things the future is not predictable, but all you are justified in saying is that you have not yet found how to predict the future. In fact, judging by past experience, there is every reason to think that if we keep on trying we shall eventually discover how to predict in this domain which at present seems so hopeless." This objection, I am sure, will appeal to many as entirely sound, but I believe that nevertheless it can not be maintained, and one of the points which I am most anxious to make this afternoon is my reason for thinking this position not to be sound. I hope that the positiveness of the assurance of the physicist in this matter will not give the unpleasant impression of mental arrogance which it easily might. I believe that every physicist recognizes that one can never say with complete assurance that his present theory is correct. There is nevertheless at least one statement which, when it can be made at all, can be made with absolute assurance, and that is that we are now taking into consideration ideas which had not previously occurred to us. All that the physicist is maintaining is that we have now at our command new experimental facts and new ideas and that in the light of them our former ideas must be modified.

It must be conceded that certain parts of the objection of our bristly critic are well taken. We have not, of course, proved that the future is not predictable, and we can say only that we are not at present able to predict and do not believe that we shall ever be able to. In fact, in the very nature of logic, it can never be proved that an entirely unexpected discovery may not be made some day which will enable us to predict the apparently unpredictable. We shall have to admit that from this point of view the tactics of the objection is very clever. From another point of view also the tactics is clever in that it puts the burden of proof on the advocate of the new point of view in effectively asking him "by what right do you expect that no one ever will find out how to do what to-day we do not know how to do?" I believe that nevertheless, in spite of the superficial strength of the objection, it is fallacious.

Let us in the first place examine what the experimental situation is which leads us to say that events in the realm of small things are unpredictable. When, in the domain of large things, we fail to predict, it is usually because of the extreme complication of the physical system, as, for example, in our endeavors to predict the weather or mob psychology. In the domain of small things, however, the element of complication is lacking and our failure arises from another reason. The reason we fail is because those regularities which are the basis on which we are able to make predictions in the simpler situations of large scale experience are entirely lacking in the domain of small things. Let us imagine the simplest possible large scale situation—a billiard ball rolling without friction or other interference on a table top. Let us imagine the table top marked with lines a foot apart. Then we all know that if we observe the ball to be at the zero mark when the second hand of our watch points to zero, and to be at the one foot mark when the second hand points to one second, when the second hand points to two seconds we shall find the ball at the two-foot mark. This is the simplest example well conceivable in which we project into the future a uniformity of behavior in the past, and, parenthetically, is doubtless the origin of the ordinary concept of velocity. But experiment shows that if we were dealing with an electron instead of a billiard ball the experiment would entirely fail, and if the electron had been observed at zero at zero seconds and at one foot at one second, at two seconds we might sometimes find it at seven feet and sometimes at five feet, or sometimes at minus one foot, and indeed sometimes at two feet, like the billiard ball. As a matter of fact this ideally simple experiment probably has not been performed, but inference from the results in less simple cases leads us to be convinced that such would be found to be the state of affairs if the experiment were made.

If capricious results like those just described for the electron happened in the experiment with the billiard ball we would almost certainly say that the initial conditions had not *all* been the same the time the ball appeared at the seven foot mark as when it appeared at five feet, and we should endeavor to find something that we had failed to take proper account of in specifying the initial conditions. Furthermore, we are convinced that this procedure would be successful in the case of the billiard ball, and that search would disclose the missing feature. We are also convinced that it would need some rather striking feature to account for the billiard ball sometimes turning up at seven feet and sometimes at five feet. But in the case of the electron, all our experience indicates that the missing feature does not exist, but that in systems in which the initial conditions are completely identical the electron will sometimes appear in one place and sometimes in another. Anything more unlike ordinary experience would be difficult to conceive, and the consequence is that there is very little basis indeed for making a successful prediction of the future position of an individual electron.

The experimental evidence, then, apparently forces us to the conclusion that if some basis for predicting the behavior of a single electron is to be found, it must be entirely different from the basis of prediction for ordinary events. Why is it that the majority of physicists at present believe that there is good reason to think that this basis for prediction will never be found, but that on the contrary we shall always have to treat the motions of single electrons as beyond the reach of prediction, that is, beyond the law of cause and effect? The reason is not that the physicist is either lazy or a quitter. Part of the reason is rather to be found in the quite surprising success achieved within the last few years by that body of physical theory variously described as "quantum theory" or "wave mechanics." The very foundations of this theory contain as an integral part the hypothesis that the individual electrons, as also the indivisible units of radiation, have the fundamental property that in any specific situation their behavior as individuals can not be predicted, but only the average behavior of large numbers. One explicit deduction from the theory which is directly concerned with predictability has been much discussed, namely the Heisenberg Principle of Uncertainty. This is usually formulated as a statement about the accuracy of measurement, the fundamental idea being that if we strive to increase the accuracy with which we make one kind of measurement we must pay a price in a necessary decrease in the accuracy of some other kind of measurement. Specifically, I can not measure the position and the simultaneous velocity of the electron with any desired accuracy, but if I increase the accuracy of my measurement of position, my measurement of velocity becomes less accurate in such a way that the probable value of the product of the two inaccuracies is of the order of magnitude of Planck's constant, h, divided by the mass of the electron. The principle applies equally well to the measurement of the position and velocity of an ordinary body. The reason why the principle is important for the electron and not for ordinary bodies is that the mass of the electron is so very much less than that of any ordinary body that when I divide Planck's constant, h, by the mass of the electron I get a comparatively large number, that is, a comparatively large uncertainty, whereas the quotient of h by the mass of an ordinary body is so much smaller as to represent an uncertainty below ordinary methods of detection.

The Heisenberg principle, as I have just formulated it, does not seem to make statements about predictability, but that it really does may be seen by considering the significance of the velocity about which we are talking. To make the problem concrete, go back to the moving electron. If I observe it at the one-foot mark at a certain time and then one second later at the two-foot mark, I know that its velocity during this second was exactly one foot per second. It is not this velocity with which the Heisenberg principle is concerned, and indeed the Heisenberg principle sets no limit to the accuracy with which this velocity may be determined. The velocity with which the Heisenberg principle is concerned is the velocity to be ascribed to the electron after, not before, the second observation. Now the only way I have of checking whether any statement about this second velocity is correct is to predict where I shall find the electron at a later instant of time. If the Heisenberg principle is correct, this prediction can not be made with precision; we thus see that the Heisenberg principle is really a statement about the impossibility of accurately predicting the course of the electron.

There is a point here which it will pay to emphasize because there has been considerable misconception about it. The *modus operandi* by which the uncertainty gets into the situation is through the act of observation—the electron can not be observed without bouncing an atom of radiation from it or doing something equivalent, and whatever the process of observation, the motion is interfered with. The essential fact is not that the act of observation interferes with the motion; if this were the only effect we could allow for the amount of interference by calculation. The essential fact is that the act of observation interferes with the motion by an unpredictable and incalculable amount. The fact that the amount of interference is unpredictable is an integral part of the theory.

Those persons who for one reason or another are anxious to save the face of the causality principle have often stated the conclusion from the Heisenberg principle in another way. They say that the causality principle is still valid, only it turns out that we are unable to make the measurements which are demanded in applying the causality principle, that is, in making a prediction. This contention it seems to me may easily degenerate into a mere matter of words, and become highly unprofitable. The essential fact is that it appears to be due to a law of nature and not to any temporary failure of ours that we can not make the measurements that we demand for our attempted predictions. In this situation it seems to me that we are keeping as close as possible to the actual facts in making the bald statement that experiment now makes it highly improbable that the future is predictable.

Not only is the Heisenberg principle checked by experiment, but apparently all other deductions from the wave mechanics theory are also checked with equal success. In fact, the success of the theory has been so great that the statement is often made by its enthusiastic advocates that in no case where it has been correctly applied, that is, without blunders in the calculation, has it failed, and that no experimental facts are known which are in contradiction with it. The average physicist now takes the next step, and draws the conclusion that because of the success of the theory the fundamental hypotheses on which it rests must also be very probably correct. It must be admitted that this last step is rather dubious from the logical point of view, because it does not follow at all that because our conclusions are correct our reasoning or our premises must be correct, and in fact there are a number of instances in physics in which the fundamental hypotheses have been changed without changing at all the superstructure, as shown, for example, by the change in attitude toward the physical reality of the ether.

I think we would have to admit that if the sole argument of the physicist were the success of the theory as at present formulated we would have some ground for scepticism as to how long the present attitude would last. But the physicist has other reasons for his attitude. Along with his experimental activity he has been active in critically examining the fundamental concepts of physics. Among other things he has examined the grounds on which rest our conviction that nature is uniform or predictable and has come to the conclusion that at least the burden of proof is now on the side of those who maintain that nature is uniform and can be described in terms of a causality principle. The reasons for this conclusion I have already intimated, but they are worth repeating. The physicist recognizes that belief in uniformity or predictability is a belief compelled by no inner necessity, but is a belief that has gradually grown up as a generalization from large scale experiences; that as long as experience was confined to the large scale things of daily life an ever increasing number of phenomena could be brought under the approximate sway of the principle, but that as soon as physical methods became sufficiently refined so that we could deal with small scale phenomena, uniformities became less and less conspicuous, until we finally arrived at electrons and photons, the ultimate structural elements of the physical world as we know it, where we would expect the utmost in the way of simplicity and uniformity but where, on the contrary, experiment shows that uniformity in its original sense has entirely vanished.

The situation thus contains two elements: There is in the first place the recognition that the notion of causality, in the sense in which I am using it, was an outgrowth of experience, and that the extent to which the causality principle is valid is solely for experiment to decide. This attitude I believe must now be accepted by every one who will take the pains to examine the argument. In the second place there is the conclusion from experiment that as a matter of fact nature is very far from predictable or that the causality principle fails by large amounts to be valid for small-scale events. If the first point is accepted, then the second may be accepted as a summary of the best experimental knowledge at present, without resentment or antagonism or rebelliousness. This willingness to accept the findings of experiment must remain permanently a part of our attitude, whether or not future experiment justifies present optimism about the complete adequacy of wave mechanics. This, then, is the chief idea that I hope you will carry away from this talk; that it is purely a matter for experiment to decide whether nature is predictable in the domain of small things or not; that until some at present totally unlooked-for development turns up to prove predictability or to make it plausible, we must assume that the causality principle does not apply in this domain, and that this conclusion is to be accepted without prejudice or passion just as any other experimental result is accepted.

What difference is the recognition of this situation now going to make to us? As far as actual action in most concrete situations of daily life go, it will make practically no difference at all. Large-scale phenomena will remain for all practical purposes just as predictable as they ever were. The reason for this is that in no case have we ever been able to predict large-scale phenomena with more than a certain degree of approximation; the goodness of the approximation has been fixed by the accuracy of the measurements. Furthermore, in nearly all cases, the inaccuracies of our measurements arise from the ordinary imperfections of our instruments, recognized and well understood long before the Heisenberg principle was formulated, and these inaccuracies are so great that the uncertainty in our predictions arising from them is much greater than the uncertainty arising from the action of the Heisenberg principle. In most practical situations the Heisenberg uncertainty is so very far beyond the reach of detection by ordinary means that from the practical point of view its effects will in nearly all situations remain forever of purely academic interest.

There is a difficult point here which we may stop to examine for a moment. At first sight it is not easy to see how it is that if large-scale phenomena are built up from small-scale phenomena and if the small-scale phenomena are essentially unpredictable, the large-scale phenomena acquire approximate predictability. The reason is that although any single small-scale phenomenon is unpredictable, experiment shows that nevertheless there is a sort of regularity in large numbers of them which permits combinations of them to be predicted, approximately. This sort of regularity is of the sort that may be described as statistical. Let us go back to the illustration of the electron moving over the marks on a table, and let us suppose that the electron has been observed at the zero mark at the zero of time and at the one-foot mark at one second. Then it is a matter of experiment that if I attempt to predict where the electron will be found at two seconds I shall make a great many mistakes, but it is also a matter of experiment, neglecting a consideration which is not important for this argument, that if I always guess that it will be at the two-foot mark, I shall in the long run make fewer mistakes and obtain a better score than if I make any other guess. There is thus a certain regularity in the aggregate results of a great many experiments, although the results of individual experiments may fluctuate widely, being now greater than the average and now less. If I could make a great many experiments simultaneously, it is evident that I could make a good prediction about the average result of all of them taken together, because in a great many experiments those individual results which are too high will cancel with those which are low, leaving outstanding merely the average. Something very much like this is involved when experiments are made on ordinary matter, for even the smallest bits of matter that can be distinctly seen in the microscope still contain a very large number of electrons, and the behavior of the whole bit of matter is merely the average behavior of all its electrons, which can therefore be predicted with much success. In fact, as already stated, the mean fluctuations arising from the uncompensated fluctuations of the individual electrons are less than the uncertainties arising from other and more ordinary sorts of imprecision of measurement.

It might seem, therefore, as though there could never be any practical effects arising from such small-scale uncertainties. This, however, would be too hasty a conclusion. If one makes the deliberate attempt, it is possible to magnify such small-scale events sufficiently to bring them into the range of everyday experience. An example of this sort of thing is known to every physicist in the Geiger counter. This apparatus is so constructed that the effect of the entrance of even a single electron into the sensitive part of the apparatus is amplified with vacuum tubes to such an extent as to give a crack of sound in a loud speaker, or to perform other functions, such, for example, as starting or stopping a piece of machinery. Now the electron which enters the apparatus may be the result, for example, of the radio-active disintegration of a single atom. The disintegration of such an atom is the sort of thing that experiment and theory both show is essentially unpredictable. It would therefore be possible, by utilizing an arrangement of this kind, to make all the lights of a great city flicker up and down, and it would be absolutely impossible to predict when the next eclipse would occur.

Some of you may have read a recent story by Lord Dunsany in which a crazy power magnate wards off the vengeance of the powers above by gigantic prayer wheels driven by 10,000 horse power steam turbines. We may similarly romance about the future religion of a superstitious race by imagining in the inmost shrine of their temple a speck of radio-active salt in process of disintegration, and attached to this a train of vacuum tube amplifiers, which shall ever and anon flood the temple with light, or beat a tom-tom, or perhaps sacrifice a victim. A rather good argument might be made for this sort of thing, and it really appeals to the imagination in many ways, for we have here the possibility of a spectacular projection into the realm of ordinary sense of the eternally inscrutable foundations of our physical world.

In the realm of ordinary physical objects, however, this sort of unpredictability probably seldom occurs unless it is the result of deliberate design. It is not quite so evident, however, what the true state of affairs is in biological systems. I gather the impression that at the present time a number of biologists are prepared to admit that not infrequently the adjustment of a single cell may be so delicate as to be thrown out of balance and a reaction started by the entrance of a single free electron or light corpuscle into the cell. In such cases the behavior of the individual cell must be admitted to be unpredictable. In one simple field of biological experiment the facts have already been definitely established. It has been shown that when certain unicellular organisms are radiated with alpha particles, the death of the organism results if a single alpha particle makes a direct hit on the nucleus of the cell. The death of any single organism is therefore an absolutely unpredictable event, although in a large colony of such organisms the average number of deaths may be predicted by the methods of life insurance statistics. The important question now is whether in large-scale organisms consisting of many cells the adjustment is ever so delicate as to be thrown one way or the other by the unpredictable action of only a few cells, or whether so large a number of cells must cooperate in any large-scale movement as to make all large-scale movements approximately predictable by the methods of statistics. This question can be answered only by experiment, and it is certainly one of the most important questions to which biology can address itself. At the same time it is obviously one of enormous experimental difficulty, for no experiment can be repeated under identical conditions on an organism complicated enough to have memory.

A word of warning may be interjected here. Many will be tempted to see a connection between the question of the predictability of the behavior of organic systems and those questions which have always exercised the human race, determinism and free will. It seems to me that there is no connection. The former is primarily a question of physical fact, while the latter are predominantly questions of a subjective character which involve those emotional experiences which the subject goes through when on the point of making a decision.

The concrete physical changes which are likely to arise from our modified attitude toward the causality principle are therefore small. I believe, however, that there will be very important effects on our methods and habits of thought and our entire outlook. One of the most obvious effects of these discoveries is in prescribing the program for future scientific investigation. One such possibility, I have just indicated, namely, the examination of the question as to whether the behavior of complex biological organisms is ever initiated by unpredictable small-scale events. A similar question arises with respect to other sorts of physical happenings; are there anywhere in nature mechanical systems of such a high degree of instability, as for example in the turbulent motion of a liquid, that an unpredictable small-scale event initiates a large-scale event, which thereby itself becomes unpredictable. Another possible program for future scientific investigation is in devising simple experiments which shall demonstrate less indirectly than we now can some of the statistical effects of small-scale events. For example, the invention of a photographic plate such that the impact of a single photon is sufficient to make a single grain of the plate developable would be an enormous assistance.

Apart from these concrete effects on our scientific program there will be many other less tangible results of our changed mental outlook, which I have not time to elaborate in detail here, but I shall try to give a few brief indications of what may be expected. A parallel may be drawn from the theory of relativity. Although the theory of relativity deals with phenomena which are so difficult to detect as to require instruments of the highest delicacy, nevertheless it has made exceedingly important changes in our attitude toward the fundamental concepts of space and time. I believe that in the same way the clear recognition that causality can not function in detail in small things, as has been supposed, but can have only a statistical meaning, must have important repercussions on our thinking. In fact, activity is already beginning in the ranks of the philosophers which bears out this contention. Many articles and even books have already appeared in which the endeavor is made either to explain away the importance of the new findings, or else to discover how we may adjust ourselves to them. The bearings on epistemology are apparently considered especially important. A single possibility may be mentioned. At a recent discussion between philosophers I heard it argued that the principle of sufficient reason is an absolute *sine qua non* of thought, that the human reason must by its very nature refuse to accept the possibility, to go back to the well-worn example of the billiard ball and the electron, that in one experiment the electron should appear at the seven-foot mark and in another at the five-foot mark, without there being some reason for the difference. To which I am afraid that the brutal physicist would be tempted to rejoin that if the human reason is incapable of accepting the situation, so much the worse for the human reason. However, these are questions of technical philosophy which are entirely beyond my sphere, and I shall not say anything more about them for fear of making even more egregious blunders than I may have already.

Apart from philosophical questions, however, it seems to me the realization that it is possible to exemplify on a large scale such things as our capriciously disintegrating radio-active material, which may serve as the nucleus of a superstitious religion, or may equally well serve as a most excellent gambling device, can not fail to get under the skin of the man in the street. This objectifies in the most striking way the limitations of the human intellect, and I believe that the greatest changes in our mental outlook will come as a consequence of the realization of just these human limitations—we had thought the human reason capable of conquering all things, we now find it subject to very definite limitations. We can definitely conjure up physical situations in which the human reason is powerless to satisfy itself, but must passively be content to accept phenomena as they occur, which constitutes in fact a reversion to the mental attitude of primitive man, which is purely receptive. What is more, the strictly scientific attitude recognizes no escape from the situation, but it must be accepted as inherent in the nature of things, and no way out attempted by such inventions, material or conceptual, as primitive man makes.

The realization of human mental limitations will, I believe, have the greatest effect, and the process of adjustment will be slowest, in such non-scientific activities as philosophy, religion, as already suggested, and very probably education, for some just apprehension of the possibilities of the human intellect should be imparted in any satisfactory educational program. The adjustment in scientific activity I believe will be made more rapidly, and in fact it is possible to see even now in what the adjustment will consist.

A formulation of the purpose of scientific activity which appeals to me as rather exhaustive is the understanding, prediction and control of events. It might be thought that the discovery that there are aspects of nature which are not understandable, predictable or controllable would work havoc with this scientific program. But the way out is already obvious. If it is true that there are certain aspects of nature which are neither controllable or predictable, then the obvious course is to avoid these aspects of nature. This may sound like a flippant suggestion, but the matter is really to a large extent in our own power. We have seen that although single small-scale events are unpredictable, the statistical average of large numbers of them is highly regular and predictable. The obvious course of action, then, whenever we want to be sure of the result, is to so arrange the apparatus or machine as to respond only to statistical averages, and not to function like a Geiger counter in response to single small-scale happenings. If it should prove that the large-scale behavior of biological organisms is unpredictable, then we shall take pains never to depend on the behavior of a single such organism whenever we have to be sure of our results, but this is hardly more than we do already.

The situation with regard to *understanding* small-scale events will probably take a little more adjustment, because it involves giving up an ideal which we had set ourselves. But even here the adjustment can hardly take more than one generation, and in science generations are short. Analysis will show, I believe, that what we call understanding consists in picking out from a situation elements with which we are already familiar. The difficulty in the present situation is that we are not familiar with systems in which individual events occur with no close connection with past events, so that naturally we are confused and seek for a hidden connection. But as our familiarity increases and the strangeness gradually wears off, we shall come to feel that it is natural and proper that small-scale events show only statistical regularities, and we shall come to be satisfied with our understanding of a situation when we have analyzed it sufficiently to show, if we are dealing with large numbers, the statistical regularities to be expected, or, if we are dealing with small numbers, the corresponding capricious variations. In fact, a number of the younger generation have already achieved this degree of emancipation, and the rest of us, by deliberate effort, may hope to attain it.

PALEONTOLOGY VERSUS DEVRIESIANISM AND GENETICS IN THE FACTORS OF THE EVOLUTION PROBLEM[1]

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THIS is a study in what may be called *progressive or adaptive heredity;* it is the second of a series of communications I plan to give to the National Academy on the factors of evolution; as implied in the title, this study is directly opposed to all accidental, discontinuous or mutational hypotheses of the causes of the bio-mechanical evolution of the germ plasm.

Last year I summarized results obtained through

[1] Address before the National Academy of Sciences, Washington, April 28, 1931.