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LOGIC AND PROBABILITY IN PHYSICS[1]

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THE history of the development of physics in the first quarter of the twentieth century will rank as one of the greatest in the advancement of knowledge, but it will also rank as one of the most curious in the history of human thought. In 1901 Planck started the quantum theory. Even this was curious. He was trying to find out the law of complete radiation by the use of ordinary statistical methods, and observed that he got his answer at what should have been the last stage but one of his work. The last stage would have involved proceeding to a limit, and he found that he got the experimental answer without doing so, and an absurd answer if he did. The work went rather deep into statistical theory and there were many for long afterwards who were not convinced of its compelling force, but it was the great merit of Planck that he knew that he had got something involving a quite revolutionary idea—the quantum. In succeeding years other phenomena were seen to involve the same revolutionary idea: Einstein's theory of the photoelectric effect and of the ionization produced by x-rays, his theory of specific heats, later improved by Debye, and Bohr's theory of spectra. All these things fitted in quite obviously with the quantum, but quite as obviously they violently contradicted the physics of the nineteenth century. What should a man think about a beam of light which according to Einstein had to be composed of arrows, whereas a hundred years earlier Fresnel had *proved* that it was a sys-

[1] Concluding portion of the address of the president of the section of mathematical and physical science of the British Association for the Advancement of Science, meeting at Cambridge from August 17 to 24.

tem of waves? What does a rational being do when faced with two mutually contradictory but both indubitable pieces of evidence? It was a nice test for the critical spirit, and it revealed a wide divergence of choices. In making a historical judgment long after the event, one of the hardest things to do is to recall the relative scale of importance which contemporaries were inclined to attach to the different branches of their subject.

The statistical theory of matter had already been well established by the work of Maxwell, Boltzmann and Gibbs, but it was not regarded as an essential part of a general mathematical-physical education. For example, in the various courses I was advised to undertake during my undergraduate career, no one at any stage ever suggested to me that I should learn anything about the kinetic theory of gases. I think that that period was one when the Cambridge mathematical school was not at its best, and very probably a little more was done at other places, but, to judge by the available text-books in any language, statistical theory was not regarded as one of the prime subjects of study, as it would be now. The period was essentially dynamic, and as such it was moderately easy for it to take in the new ideas of relativity, to which indeed the experimental work of the last century had been leading. But there was no common habit of thought on statistical lines, and so there was a sharp separation of opinion. The seniors, impressed with the vast mass of successful physics of the nineteenth century, with only a rather general knowledge of statistical theory but no facility of thought in it, found the new ideas completely contrary to their convictions. Such men would think that these ideas depended on the difficult and unfamiliar conceptions of statistics and would be inclined to judge that there must be a fallacy in the statistics which would be cleared up later. On the other hand, the laboratory workers, dealing with atoms and electrons from day to day, could not fail to be more impressed with the discontinuous phenomena and the beautiful way these could be explained by the quantum. Such men would cheerfully accept the Bohr orbits as a complete explanation of the hydrogen spectrum, and certainly in many cases would be actually ignorant of the difficulty, the monstrous absurdity, of supposing that a sharp jump from one orbit to another could be responsible for a train of waves shown by the spectroscope to be lasting for quite a long time. So the majority of rational beings behaved in the natural human way of managing to forget all the disagreeable facts. But not every one, for there were Bohr and other leaders who recognized the difficulties on both sides but could still maintain an attitude of balance and could believe that from somewhere there would come a higher synthesis by which everything would be fitted together.

As time went on the quantum got obviously stronger and stronger, and began to invade more fields. The nuclear atom in the hands of Bohr showed itself capable of giving all the broad details of the periodic table of chemistry, still with nothing done to meet the awful difficulties of optical theory. But about 1925, guided by the correspondence principle, things were moving towards a tentative theory of the refractive index, and it was this that finally suggested the break in the contradictions. Acting on a hint given by the theory of refraction, Heisenberg was led to the suggestion that the contradictions of atomic theory would disappear if one adopted the idea of non-commutative algebra in dealing with the motions of electrons in an atom. Then the floodgates broke and the whole new quantum theory burst forth. It would of course be an incomplete account of it not to mention the quite different approach made independently by de Broglie and Schrödinger. If we are to trace this to its origin we must go back a century to Hamilton, for it was his work in geometrical optics which showed how a wave of short wave-length could be treated as a ray. It was de Broglie who worked out the modern analogies, but it was Schrödinger who succeeded in giving its full form, and by the invention of the *wave-function* placed in the hands of the mathematicians the most powerful of weapons for the technical discussion of atomic problems.

At first the work was of a formal kind, obviously right, and a complete synthesis of the rival doctrines of particle and wave mechanics, but there is a very interesting point that has gradually emerged in connection with the discovery. In his first paper Heisenberg laid great stress on the idea of building theory only on directly observable quantities. It is not very clear how the distinction was drawn. The electron's orbit is certainly not observable, but is it less so than the electric force which is the amplitude in the light-wave emitted by the atom? It has seemed to me that it was not this idea of using the observable that was the merit of his work, but rather the contrary—the capacity for carrying through a formal mathematical analogy without ever asking what it all meant in terms of observable things. However that may be, it was only a year later that he remedied the defect by making a picture of his process by means of the uncertainty principle. I may remind you that the uncertainty principle asserts that it is impossible simultaneously to measure the position and velocity of any body, because the measurement of either inevitably produces a change of indeterminate amount in the other. The subject has been so often discussed that I am not going into it now, but as it concerns the center of my argument, I want to emphasize its negative side, which as I think is much the most important. In this rôle the uncertainty principle is to be regarded as the argument used to defeat the old-fashioned physicist who claims that there is at any rate ideally no limit to the accuracy with which both position and velocity can be simultaneously measured. He has to admit the correctness of experiments such as the Compton effect, and we show him that by his own admission he will be defeated. On the positive side the principle is not so useful, because once we have seen the reason for the failure of classical ideas, we had better take advantage of the full technique of the quantum mechanics. Here my point is that the uncertainty principle showed up a fallacy in the old arguments about causality, and it was a fallacy about which we were so unconscious that we did not even know we were making it. It is now easy to see that there was nothing wrong with the old inference that if I know all about the present I can forecast the future exactly; the trouble was the impossibility of knowing the present. Once this is seen the whole argument becomes obvious, but nobody saw it until Heisenberg. We had somehow to avoid the compulsory causality of the old mechanics, and there seemed no loophole allowing us to do so until the uncertainty principle. Knowing what we now know we may ask why no one discovered the loophole by applying a strict analysis, for example, by the use of symbolic logic. Such an analysis would presumably have revealed the fault, but the trouble is that it would also have revealed other unwarranted assumptions which we have made but which we do not in the least want to doubt, so that it would not really have helped in pinning down the exact point of error. It is invention, not criticism, that leads to the advance of knowledge.

Following up the later history of the subject, the success of Heisenberg in exploiting the idea of observables for atoms seemed to repeat the brilliant success of Einstein twenty years earlier in using the same idea over relativity. It seemed to imply that what was wanted in physics was to free ourselves of all abstractions and only make theories about real things. There grew up a great cult of doubting the reality of unobserved things, and then a curious thing was found; the charm did not work again, and only a few minor things have come out of it. The work of the new quantum theory has in fact run most surprisingly in the opposite direction. The technique is largely concerned with wave-functions, which are quantities much more abstract than anything in classical mechanics. There is certainly nothing observable, or even picturable, about waves propagating themselves in many-dimensional space with absolutely unknowable phase, and with intensity controlled by the curious extraneous rule of normalization. Largely by the use of these wave-functions the whole of atomic physics has been reduced to order, and so has molecular physics, except that it yields problems in which so many electrons are interacting that a full discussion is not feasible. So the doctrine of theorizing only about observables was not really a useful doctrine; it merely provided a germinating idea. In fact, we may well ask what an observable is, and if we go at all beyond direct sensations, which as physicists we certainly intend to do, the answer becomes perfectly indefinite. This opinion I heard admirably expressed a few years ago by the late Professor Ehrenfest. It was in a physics meeting in Copenhagen and some one was proposing a way out of certain difficulties which involved, as he maintained, a reversion to the cult of the observable. Professor Ehrenfest said: "To believe that one can make physical theories without metaphysics and without unobservable quantities, that is one of the diseases of childhood—*das ist eine kinderkrankheit.*"

I have dwelt at some length on the history of the quantum theory because I think it serves as an analogy to the deeper question of what is wrong with the old logical processes. Just as we used to feel the all-pervading compulsive force of causality, so we feel the all-pervading force of pure logic. Just as we felt that classical mechanics provided no room for anything beyond itself, so we feel that the old logic is the only admissible kind of reasoning. We know that certain things led to the old quantum theory and obstinately refused to fit into mechanics, and we know that the principle of probability can cover many things outside the old logic. Many men tried to force the quantum theory into the classical system, and many are still trying to bring probability within the fold of the old logic. I do not believe it can be done. This is not the occasion, nor have I the capacity, for a deep argument on the place of probability in logic, but one of the most convincing ways of seeing it may be found in the consideration of another branch of physical theory, the kinetic theory of gases.

In the early days of kinetic theory the central problem was the law of distribution of velocities of the molecules and attempts were made to prove the law absolutely from dynamics, but the process always failed. Maxwell made the assumption that with the lapse of time a system of molecules would pass through all possible phases. There are technical difficulties in the discussion of this assumption which have never been overcome, and it is quite uncertain if it is even true. Indeed Kelvin, who disliked the whole kinetic theory, argued with some force that the only examples any one could give contradicted the principle—for example, the motion of the planets. The greatest contribution to the subject was that of Gibbs, who recognized that there had to be a big assumption somewhere and made it quite frankly and without attempt at justification. The works of Gibbs are not easy reading; in both his great works he attends to every detail with a particularity that is really rather tedious, whereas his basic ideas are thrown at the reader almost without explanation. The idea of a canonical ensemble is a really beautiful idea once you understand it, but where does it come from? An ensemble is an idea which will be unfamiliar to many, so I had better explain it. We want to know something about the behavior of a complicated system composed of a great many parts; say we want to know the pressure of the gas in some vessel. If we tried to attack the question by pure mechanics, we should be faced with an enormous number of mechanical equations for the motions of the molecules, and even if these could be solved the solution would be of no use, because it would depend on the initial positions and velocities of the molecules, and these we should not know. Instead of trying this impossible and useless task, Gibbs considers a very large number of possible states of motion of the set of molecules, which have some character in common, such as their total energy, but which are otherwise unrelated. Though each specimen of the motions is quite independent of all the others, he looks at them all together; this explains the word *ensemble*—I do not know why he had to take a French word—and makes the assumption that the pressure of the gas is correctly given by the average of all the specimens. The actual gas in the vessel at any instant is one of the specimens; in its motion it passes into configurations corresponding to others, but only after a fantastically long time would it go through even a perceptible fraction of the whole ensemble. Gibbs is assuming that the behavior of the actual gas will be determined by the average of the uncountable millions of specimens in the ensemble. Almost at the start one finds oneself presented with the ensemble with hardly an attempt to explain where it comes from or why it is right, and the beginner is usually troubled by the fact that, though the subject is obviously mechanical, all the mechanics he laboriously learned in his youth seem to have faded into comparative unimportance. There are various kinds of ensemble, the chief of which is the canonical, corresponding to all the possible motions of the gas which would have the same temperature. Later, almost as a concession to human frailty, Gibbs introduces the micro-canonical ensemble, composed of much fewer specimens becausee they all have exactly the same energy. This is usually welcomed by the beginner because it seems closer to his familiar mechanics, but with more experience he will realize that the gap is still so great that he is really no better off, and he may as well accept the more general idea at once.

With the old mechanics all this involved ideas which for many readers were distinctly hard to accept. The principle of probability, embodied in the averaging over the ensemble, was frankly laid on top of the logical principles of Newtonian mechanics, and to any one believing that probability would ultimately be brought down to the old logic the association was most repellent. But we can now see that Gibbs was a prophet far ahead of his time—and indeed, to be frank, far ahead of his own knowledge—for the new mechanics accommodates the ensemble very much more easily than did the old. The new mechanics has shown us that it is impossible to know how the individual molecules are moving, because when one undertakes an experiment to see, that experiment automatically alters the condition of the gas and so fails to tell what was wanted, the state of the molecules without the experiment. In the old days one used to feel that the validity of Gibbs's idea would be spoiled by some skilful experimenter who would really observe the motions of the individual molecules and would therefore rule out the legitimacy of averaging over the *whole* ensemble, but we now know that there is no danger of this. The real gas in the vessel is not merely one specimen of the ensemble, unrecognizable only because of our clumsiness; it is itself the whole of the ensemble. We used to think of the gas as *either* in the state A, *or* in the state B, *or* in C, but according to the new physics we have to think of it as in *all* the states A *and* B *and* C. The distinction is typical of the change we must make in our habits of thought, and most of us resist this change strongly, for we find we can hardly help asking: "But which state was it really in?" As I have said, we used to be ashamed of ignorance, but we must now realize that *this* ignorance is one of the things that makes the world possible. The principle of probability, which used to be loosely superposed on the old logical principle, is now with the new mechanics fully united with it in a higher synthesis.

Before leaving Gibbs I would like to refer to one thing in his book, where I think he has not even yet come into his own. He considers various types of ensemble of increasing generality. In the micro-canonical the members all have the same energy. Now we never know the exact energy of the gas in a vessel, so that a better idea is the wider one of a gas at a given temperature, which therefore has a certain range of admissible energies. This is represented by Gibbs's canonical ensemble, and it is the main one that he uses. In both these the number of atoms in the ensemble is constant. But in the last chapter of his book Gibbs introduces a still wider ensemble. He calls the ones with a constant number of atoms *petits ensembles,* which I shall translate as petty ensembles, and regards them as parts of a grand ensemble in which the total number of atoms is not fixed. He uses the idea to some extent in connection with semi-permeable membranes, but on the whole does not get far with it. As in much of Gibbs's work, it is the idea itself, rather than what he does with it, that is important. This idea of the grand ensemble is not yet incorporated in the new physics. In the quantum theory we take a number of electrons and nuclei and, allowing for their interactions, we construct something that is practically the canonical ensemble. But we take fixed numbers of them—this is partly reflected in the technical process of using normalized wave-functions. Now in an experiment dealing with a large number of particles we are never really sure exactly how many there are, and to assume this number is much like assuming a constant energy for them. If the canonical ensemble is a better idea than the micro-canonical, then the grand ensemble is superior to the petty ensemble. In the new mechanics nobody has yet succeeded in making anything of it, or has made any proposal how to do so, but I will venture to forecast that when some of our present difficulties in the quantum theory are cleared up, it will be found that we shall be using the grand ensemble with its indefinite number of atoms.

Reverting to my main theme, what is the moral of all this? It is that the new physics has definitely shown that nature has no sharp edges, and if there is a slight fuzziness inherent in absolutely all the facts of the world, then we must be wrong if we attempt to draw a picture in hard outline. In the old days it looked as if the world had hard outlines, and the old logic was the appropriate machinery for its discussion. Things went wrong when it was found necessary to call in the help of the principle of probability; this appeared first as an alien, but there was hope in the old days that the alien might be naturalized. It has resisted the process and we now recognize that it can not be assimilated, because it provides the necessary step to a wider reason, that of the new fuzzy world of the quantum theory, a world which is not contained in the old. How far it will be possible to make a full synthesis of the new and the old I do not know, but I like to think there is something in my analogy from the history of the quantum theory, and to suppose that we are still in the condition corresponding to the old quantum theory, and that some day a real synthesis will be made like that of the new quantum theory, so that there will be only one thing in the world that has not indefinite outlines, and that will be a new reformed principle of reasoning.

I may fitly conclude this part of my subject by returning to the point from which I started. As an example of what the ordinary man regards as correct reasoning I quoted some words of Sherlock Holmes. I must now confess that I was not quite sincere in my quotation; the impression I gave was the impression the reader carries away, but on examining the text I was interested to find that the great detective had himself arrived at the ideas I have been putting forward. In the sentence before he said "No, no; I never guess. It is a shocking habit destructive of the logical faculty," he had said: "I could only say what was the balance of probability—I did not expect to be at all accurate." The master-mind uses the word logic in its modern sense.

There may be a feeling among some that the very general suggestions I have been making are open to every sort of criticism. Perhaps they are right; as I have said, it is part of my doctrine that the details of a physicist's philosophy do not matter much. But whether it is wrong or right, my next point is one on which I do very much hope that there may be a consensus of agreement. This is that the subject of probability ought to play an enormously greater part in our mathematical-physical education. I do not merely mean that every one should attend a course on the subject at the university, but that it should be made to permeate the whole of the mathematical and scientific teaching not only at the university but also at school. To the best of my recollection in my own education I first met the subject of probability at about the age of thirteen in connection with problems of drawing black and white balls out of bags, and my next encounter was not till the age of twenty-three, when I read a book—I think it was on the advice of Rutherford—on the kinetic theory of gases. Things are better now, but mathematicians are still so interested in the study of rigorous proof that all the emphasis goes against the study of probability.

Its elements should be part of a general education also, as may be illustrated by an example. Every month the Ministry of Transport publishes a report giving the number of fatal road accidents. Whenever the number goes up there is an outcry against the motorists, and whenever down, of congratulation for the increased efficiency of the police. No journalist ever seems to consider what should be the natural fluctuations of this number. A statistician answers at once that the natural fluctuation will be the square root of the total number, and apart from obvious seasonal effects that is in fact about what the accidents show; the number is roughly 500 ± 25. The proof of this does not call for any difficult mathematics, neither the error function nor even Stirling's formula, but can be done completely by the simple use of the binomial theorem. There is no mathematical difficulty that should trouble a clever boy of 15; it is only the train of thought that is unfamiliar, and it is just this unfamiliarity that is the fault of our education. The ideas and processes connected with the inaccuracy of all physical quantities are much easier to understand than many ideas that a boy has to acquire in the course of his studies; it is only that at present they are not taught, and so when met they are found difficult.

This is not the place to describe a revised scheme of education. I would only say that it is not special new courses that are needed, but rather a change in the spirit of our old courses. When a boy learns about the weighing machine, emphasize its sensitivity and consider the length of time that must be taken for the weighing. When he has a problem on projectiles, make him consider the zone of danger and not merely the point of fall. At a rather higher level, but still I should hope at school, introduce the idea of a distribution law; for example, in doing central orbits work out Rutherford's law of scattering. Calculate the fluctuations of density of a gas, or the groupings in time of the scintillations of α-particles. All these things ought to be examples of a familiar train of thought, and not merely a highly specialized side branch of mathematics first met at the university. It is the incorporation of probability in the other subjects on which I want to insist, but there will of course remain some higher aspects—things like least squares or significance tests—which are still to be treated in separate university courses. Even these I should hope would come to be recognized as subjects of central interest and not, as they are at present, relegated to a remote corner of specialized study.

If these reforms are carried out I shall hope that generations will grow up which have a facility that few of us at present possess in thinking about the world in the way which the quantum theory has shown to be the true one. The inaccuracies and uncertainties of the world will be recognized as one of its essential features. Inaccuracy in the world will not be associated with inaccuracy of thought, and the result will be not only a more sensible view about the things of ordinary life, but ultimately, as I hope, a fuller and better understanding of the basis of natural philosophy.

SCIENTIFIC EVENTS

NATIONAL PARKS

PRESIDENT ROOSEVELT has approved a recent act of Congress, marking an important step towards the final establishment of the proposed Isle Royale National Park in the State of Michigan. This act provides that all lands purchased by the Federal Government for conservation or forestation purposes within the authorized park boundaries, with funds heretofore allocated and made available by executive order, or otherwise, shall be made a part of the park as fully as if originally acquired for that purpose.

The establishment of the Isle Royale National Park was authorized by the act of Congress approved on March 3, 1931. Isle Royale, the largest island in Lake Superior, is rich in wildlife and is famous for its copper mines worked by Indians before the advent of white men. It is situated just within the international boundary separating Canada and the United States, being 50 miles northwest of Keweenaw Point, Michigan, and 20 miles southeast of the nearest Canadian mainland at Thunder Cape. Isle Royale measures 44 miles in length and 9 miles in width, including an area of 205 square miles. To date, 102,000 acres of land have been acquired under the executive order, leaving approximately 19,000 acres under contract to be purchased or in condemnation. The State of Michigan, which has appropriated $100,000 toward the acquisition of private rights on the island, must also cede exclusive jurisdiction to the United States over the lands acquired directly by the Federal Government before the park will be fully established.

Two days before his term as Chief Executive expired on March 4, 1909, Theodore Roosevelt by executive order established the Mount Olympus National Monument. An act was passed during the closing hours of the last session of Congress creating the Olympic National Park with the Mount Olympus National Monument as a nucleus. It provides for the immediate inclusion of 634,000 acres, nearly twice the area of the Mount Olympus National Monument, and in addition authorizes the President to add to this acreage lands from the Olympic National Forest and any lands that may be acquired by gift or purchase up to 898,292 acres. The region comprising the park is one of rugged ice-capped peaks and dark but vividly green "rain forests" of giant moss-festooned spruce and fir; of lake-studded flowering meadows forming natural gardens. Through its deep canyons streams fed by the waters of melting glaciers above find their outlet in the Pacific.

Purchase by the Federal Government of the last remaining land needed to complete the Great Smoky Mountains National Park in the wilderness of North Carolina and Tennessee has been announced. The act provides an appropriation for the National Park Service with which it will be possible to acquire 26,000 acres of land in Tennessee. All but the relatively small amount of land remaining to be bought gradually has been acquired since 1926 by the states with private donations and with state and federal funds. Private funds were matched, dollar for dollar, up to $5,000,000 by the Rockefeller Foundation as a memorial to Laura Spelman Rockefeller, mother of John D. Rockefeller, Jr. The Great Smoky Mountains National Park area so far acquired has been under the jurisdiction of the National Park Service since 1930. With the money now available, the final steps can be taken toward completion and formal