aloof + skeptical 163, 170

all three were
about interaction
of matter + radiation
(following Planck)

2013·02·12

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EINSTEIN'S STATISTICAL THEORIES

ONE of the most remarkable volumes in the whole of scientific literature seems to me Vol. 17 (4th series) of *Annalen der Physik*, 1905. It contains three papers by Einstein, each dealing with a different subject, and each to-day acknowledged to be a masterpiece, the source of a new branch of physics. These three subjects, in order of pages, are: theory of photons, Brownian motion, and relativity.

Relativity is the last one, and this shows that Einstein's mind at that time was not completely absorbed by his ideas on space and time, simultaneity and electro-dynamics. In my opinion he would be one of the greatest theoretical physicists of all times even if he had not written a single line on relativity—an assumption for which I have to apologise, as it is rather absurd. For Einstein's conception of the physical world cannot be divided into watertight compartments, and it is impossible to imagine that he should have by-passed one of the fundamental problems of the time.

Here I propose to discuss Einstein's contributions to statistical ✓ methods in physics. His publications on this subject can be divided into two groups: an early set of papers deals with 1902-04 classical statistical mechanics, whereas the rest is connected with quantum theory. Both groups are intimately connected with Einstein's philosophy of science. He has seen more clearly than anyone before him the statistical background of the laws of physics, and he was a pioneer in the struggle for conquering the wilderness of quantum phenomena. Yet later, when out of his own work a synthesis of statistical and quantum principles emerged which seemed to be acceptable to almost all physicists, ✶ he kept himself aloof and sceptical. Many of us regard this as a tragedy—for him, as he gropes his way in loneliness, and for us who miss our leader and standard-bearer. I shall not try to suggest a resolution of this discord. We have to accept the fact that even in physics fundamental convictions are prior to reasoning, as in all other human activities. It is my task to give an account of Einstein's work and to discuss it from my own philosophical standpoint.

Einstein's first paper of 1902, "Kinetische Theorie des Wärmegleichgewichtes und des zweiten Hauptsatzes der Thermodynamik"[1] is a remarkable example of the fact that when the time is ripe important ideas are developed almost simultaneously by different men at distant places. Einstein says in his introduction that nobody has yet succeeded in deriving the conditions of thermal equilibrium and of the second law of thermodynamics from probability considerations, although Maxwell and Boltzmann came near to it. Willard Gibbs is not mentioned. In fact, Einstein's paper is a re-discovery of all essential features of statistical mechanics and obviously written in total ignorance of the fact that the whole matter had been thoroughly treated by Gibbs a year before (1901). The similarity is quite amazing. Like Gibbs, Einstein investigates the statistical behaviour of a virtual assembly of equal mechanical systems of a very general type. A state of the single system is described by a set of generalised co-ordinates and velocities, which can be represented as a point in a $2n$-dimensional "phase-space;" the energy is given as function of these variables. The only consequence of the dynamical laws used is the theorem of Liouville according to which any domain in the $2n$-dimensional phase-space of all co-ordinates and momenta preserves its volume in time. This law makes it possible to define regions of equal weight and to apply the laws of probability. In fact, Einstein's method is essentially identical with Gibb's theory of canonical assemblies. In a second paper, of the following year, entitled "Eine Theorie der Grundlagen der Thermodynamik,"[2] Einstein builds the theory on another basis not used by Gibbs, namely on the consideration of a single system in course of time (later called *"Zeit-Gesammtheit,"* time

[1] *Annalen der Physik* (4), *9*, p. 477, (1902).

[2] *Annalen der Physik* (4), *11*, p. 170, (1903).

assembly), and proves that this is equivalent to a certain virtual assembly of many systems, Gibb's micro-canonical assembly. Finally, he shows that the canonical and micro-canonical distribution lead to the same physical consequences.

Einstein's approach to the subject seems to me slightly less abstract than that of Gibbs. This is also confirmed by the fact that Gibbs made no striking application of his new method, while Einstein at once proceeded to apply his theorems to a case of utmost importance, namely to systems of a size suited for demonstrating the reality of molecules and the correctness of the kinetic theory of matter.

This was the theory of Brownian movement. Einstein's papers on this subject are now easily accessible in a little volume edited and supplied with notes by R. Fürth, and translated into English by A. D. Cowper.[3] In the first paper (1905) he sets out to show "that, according to the molecular-kinetic theory of heat, bodies of microscopically visible size suspended in a liquid will perform movements of such magnitude that they can be easily observed in a microscope, on account of the molecular motion of heat," and he adds that these movements are possibly identical with the "Brownian motion" though his information about the latter is too vague to form a definite judgment.

The fundamental step taken by Einstein was the idea of raising the kinetic theory of matter from a possible, plausible, useful hypothesis to a matter of observation, by pointing out cases where the molecular motion and its statistical character can be made visible. It was the first example of a phenomenon of thermal fluctuations, and his method is the classical paradigma for the treatment of all of them. He regards the movement of the suspended particles as a process of diffusion under the action of osmotic pressure and other forces, among which friction due to the viscosity of the liquid is the most important one. The logical clue to the understanding of the phenomenon consists in the statement that the actual velocity of the suspended particle, produced by the impacts of the molecules of the liquid on it, is unobservable; the visible effect in a finite interval of

ALUCINATIONS

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[3] *Investigations on the Theory of the Brownian Movement;* Methuen & Co., London, (1926).

time τ consists of irregular displacements, the probability of which satisfies a differential equation of the same type as the equation of diffusion. The diffusion coefficient is nothing but the mean square of the displacement divided by 2τ. In this way Einstein obtained his celebrated law expressing the mean square displacement for τ in terms of measurable quantities (temperature, radius of the particle, viscosity of the liquid) and of the number of molecules in a gramme-molecule (Avogadro's number N). By its simplicity and clarity this paper is a classic of our science.

In the second paper (1906) Einstein refers to the work of Siedentopf (Jena) and Gouy (Lyons) who convinced themselves by observations that the Brownian motion was in fact caused by the thermal agitation of the molecules of the liquid, and from this moment on he takes it for granted that the "irregular motion of suspended particles" predicted by him is identical with the Brownian motion. This and the following publications are devoted to the working out of details (e.g., rotatory Brownian motion) and presenting the theory in other forms; but they contain nothing essentially new.

I think that these investigations of Einstein have done more than any other work to convince physicists of the reality of atoms and molecules, of the kinetic theory of heat, and of the fundamental part of probability in the natural laws. Reading these papers one is inclined to believe that at that time the statistical aspect of physics was preponderant in Einstein's mind; yet at the same time he worked on relativity where rigorous causality reigns. His conviction seems always to have been, and still is to-day, that the ultimate laws of nature are causal and deterministic, that probability is used to cover our ignorance if we have to do with numerous particles, and that only the vastness of this ignorance pushes statistics into the fore-front.

Most physicists do not share this view to-day, and the reason for this is the development of quantum theory. Einstein's contribution to this development is great. His first paper of 1905, mentioned already, is usually quoted for the interpretation of the photo-electric effect and similar phenomena (Stokes law so far Schröd & Ehrenf Boltzmann

of photo-luminescence, photo-ionisation) in terms of light-quanta (light-darts, photons). As a matter of fact, the main argument of Einstein is again of a statistical nature, and the phenomena just mentioned are used in the end for confirmation. This statistical reasoning is very characteristic of Einstein, and produces the impression that for him the laws of probability are central and by far more important than any other law. He starts with the fundamental difference between an ideal gas and a cavity filled with radiation: the gas consists of a finite number of particles, while radiation is described by a set of functions in space, hence by an infinite number of variables. This is the root of the difficulty of explaining the law of black body radiation; the monochromatic density of radiation turns out to be proportional to the absolute temperature (later known as the law of Rayleigh-Jeans) with a factor independent of frequency, and therefore the total density becomes infinite. In order to avoid this, Planck (1900) had introduced the hypothesis that radiation consists of quanta of finite size. Einstein, however, does not use Planck's radiation law, but the simpler law of Wien, which is the limiting case for low radiation density, expecting rightly that here the corpuscular character of the radiation will be more evident. He shows how one can obtain the entropy S of black body radiation from a given radiation law (monochromatic density as function of frequency) and applies then Boltzmann's fundamental relation between entropy S and thermodynamic probability W

$$S = k \log W$$

where k is the gas constant per molecule, for determining W. This formula was certainly meant by Boltzmann to express the physical quantity S in terms of the combinatory quantity W, obtained by counting all possible configurations of the atomistic elements of the statistical ensemble. Einstein inverts this process: he starts from the known function S in order to obtain an expression for the probability which can be used as a clue to the interpretation of the statistical elements. (The same trick has been applied by him later in his work on fluctuations;[4] although this is of considerable practical importance,

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[illegible]

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[4] *Annalen der Physik* (4), *19*, p. 373, (1906).

I shall only mention it, since it introduces no new fundamental concept apart from that "inversion.")

by chance

Substituting the entropy derived from Wien's law into Boltzmann's formula, Einstein obtains for the probability of finding the total energy E by chance compressed in a fraction αV of the total volume V

$$W = \alpha^{E/h\nu}$$

as if prob.

that means, the radiation behaves as if it consisted of independent quanta of energy of size and number $n = E/h\nu$. It is obvious from the text of the paper that this result had an overwhelming power of conviction for Einstein, and that it led him to search for confirmation of a more direct kind. This he found in the physical phenomena mentioned above (e.g., photoelectric effect) whose common feature is the exchange of energy between an electron and light. The impression produced on the experimentalists by these discoveries was very great. For the facts were known to many, but not correlated. At that time Einstein's gift for intuiting such correlations was almost uncanny. It was based on a thorough knowledge of experimental facts combined with a profound understanding of the present state of theory, which enabled him to see at once where something strange was happening. His work at that period was essentially empirical in method, though directed to building up a consistent theory—in contrast to his later work when he was more and more led by philosophical and mathematical ideas.

A second example of the application of this method is the work on specific heat.[5] It started again with a theoretical consideration of that type which provided the strongest evidence in Einstein's mind, namely on statistics. He remarks that Planck's radiation formula can be understood by giving up the continuous distribution of statistical weight in the phase-space which is a consequence of Liouville's theorem of dynamics; instead, for vibrating systems of the kind used as absorbers and emitters in the theory of radiation most states have a vanishing statistical weight and only a selected number (whose energies are multiples of a quantum) have finite weights.

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[5] "Die Planck'sche Theorie der Strahlung und die Theorie der specifischen Wärme," *Annalen der Physik* (4), 22, p. 180, (1907).

Now if this is so, the quantum is not a feature of radiation but of general physical statistics, and should therefore appear in other phenomena where vibrators are involved. This argument was obviously the moving force in Einstein's mind, and it became fertile by his knowledge of facts and his unfailing judgment of their bearing on the problem. I wonder whether he knew that there were solid elements for which the specific heat per mole was lower than its normal value 5.94 calories, given by the law of Dulong-Petit, or whether he first had the theory and then scanned the tables to find examples. The law of Dulong-Petit is a direct consequence of the law of equipartition of classical statistical mechanics, which states that each co-ordinate or momentum contributing a quadratic term to the energy should carry the same average energy, namely ½ RT per mole where R is the gas constant; as R is a little less than 2 calories per degree and an oscillator has 3 co-ordinates and 3 momenta, the energy of one mole of a solid element per degree of temperature should be $6 \times \frac{1}{2}RT$, or 5.94 calories. If there are substances for which the experimental value is essentially lower, as it actually is for carbon (diamond), boron, silicon, one has a contradiction between facts and classical theory. Another such contradiction is provided by some substances with poly-atomic molecules. Drude had proved by optical experiments that the atoms in these molecules were performing oscillations about each other; hence the number of vibrating units per molecule should be higher than 6 and therefore the specific heat higher than the Dulong-Petit value—but that is not always the case. Moreover Einstein could not help wondering about the contribution of the electrons to the specific heat. At that time vibrating electrons in the atom were assumed for explaining the ultra-violet absorption; they did apparently not contribute to the specific heat, in contradiction to the equipartition law.

electron degree of freedom
frozen out

All these difficulties were at once swept away by Einstein's suggestion that the atomic oscillators do not follow the equipartition law, but the same law which leads to Planck's radiation formula. Then the mean energy would not be proportional to the absolute temperature but decrease more quickly with falling temperature in a way which still depends on the frequencies of the oscillators. High frequency oscillators like the electrons would at ordinary temperature contribute nothing to the specific heat, atoms only if they were not too light and not too strongly bound. Einstein confirmed that these conditions were satisfied for the cases of poly-atomic molecules for which Drude had estimated the frequencies, and he showed that the measurements of the specific heat of diamond agreed fairly well with his calculation.

But this is not the place to enter into a discussion of the physical details of Einstein's discovery. The consequences with regard to the principles of scientific knowledge were far reaching. It was now proved that the quantum effects were not a specific property of radiation but a general feature of physical systems. The old rule *"natura non facit saltus"* was disproved: there are fundamental discontinuities, quanta of energy, not only in radiation but in ordinary matter.

JUMPS ✓

In Einstein's model of a molecule or a solid these quanta are still closely connected with the motion of single vibrating particles. But soon it became clear that a considerable generalisation was necessary. The atoms in molecules and crystals are not independent but coupled by strong forces. Therefore the motion of an individual particle is not that of a single harmonic oscillator, but the superposition of many harmonic vibrations. The carrier of a simple harmonic motion is nothing material at all; it is the abstract "normal mode," well known from ordinary mechanics. For crystals in particular each normal mode is a standing wave. The introduction of this idea opened the way to a quantitative theory of thermodynamics of molecules and crystals and demonstrated the abstract character of the new quantum physics which began to emerge from this work. It became clear that the laws of micro-physics differed fundamentally from those of matter in bulk. Nobody has done more to elucidate this than Einstein. I cannot report all his contributions, but shall confine myself to two outstanding investigations which paved the way for the new micro-mechanics which physics at large has accepted to-day—while Einstein himself stands aloof, critical, sceptical, and hoping that this episode may pass by and physics return to classical principles.

solid state

BORN lattice waves

EXACTLY!

The first of these two investigations has again to do with the law of radiation and statistics.[6] There are two ways of tackling problems of statistical equilibrium. The first is a direct one, which one may call the combinatory method: After having established the weights of elementary cases one calculates the number of combinations of these elements which correspond to an observable state; this number is the statistical probability *W*, from which all physical properties can be obtained (e.g. the entropy by Boltzmann's formula). The second method consists in determining the rates of all competing elementary processes, which lead to the equilibrium in question. This is, of course, much more difficult; for it demands not only the counting of equally probable cases but a real knowledge of the mechanism involved. But, on the other hand, it carries much further, providing not only the conditions of equilibrium but also of the time-rate of processes starting from non-equilibrium configurations. A classical example of this second method is Boltzmann's and Maxwell's formulation of the kinetic theory of gases; here the elementary mechanism is given by binary encounters of molecules, the rate of which is proportional to the number-density of both partners. From the "collision equation" the distribution function of the molecules can be determined not only in statistical equilibrium, but also for the case of motion in bulk, flow of heat, diffusion etc. Another example is the law of mass-action in chemistry, established by Guldberg and Waage; here again the elementary mechanism is provided by multiple collisions of groups of molecules which combine, split, or exchange atoms at a rate proportional to the number-density of the partners. A special case of these elementary processes is the monatomic reaction, where the molecules of one type spontaneously explode with a rate proportional to their number-density. This case has a tremendous importance in nuclear physics: it is the law of radio-active decay. Whereas in the few examples of ordinary chemistry, where monatomic reaction has been observed, a dependence of reaction velocity on the physical conditions (e.g. temperature) could be assumed or even observed, this was not the case for radio-activity:

weight

rate

Stoßzahlansatz

RUTHERFORD
RANDOM

BOLTZMANN EQ !?

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[6] "Zur Quantentheorie der Strahlung," *Phys. Z. 18*, p. 121, (1917).

Did Rutherford deny causality ?

RADIOACTIVE DECAY !

NO CAUSALITY !

The decay constant seemed to be an invariable property of the nucleus, unchangeable by any external influences. Each individual nucleus explodes at an unpredictable moment; yet if a great number of nuclei are observed, the average rate of disintegration is proportional to the total number present. It looks as if the law of causality is put out of action for these processes.

NON CAUSAL

Now what Einstein did was to show that Planck's law of radiation can just be reduced to processes of a similar type, of a more or less non-causal character. Consider two stationary states of an atom, say the lowest state 1 and an excited state 2. Einstein assumes that if an atom is found to be in the state 2 it has a certain probability of returning to the ground state 1, emitting a photon of a frequency which, according to the quantum law, corresponds to the energy difference between the two states; i.e. in a big assembly of such atoms the number of atoms in state 2 returning to the ground state 1 per unit time is proportional to their initial number—exactly as for radio-active disintegration. The radiation, on the other hand, produces a certain probability for the reverse process 1 → 2 which represents absorption of a photon of frequency ν_{12} and is proportional to the radiation density for the frequency.

Now these two processes alone balancing one another would not lead to Planck's formula; Einstein is compelled to introduce a third one, namely an influence of the radiation on the emission process 2 → 1, "induced emission," which again has a probability proportional to the radiation density for ν_{12}.

MODEL

This extremely simple argument together with the most elementary principle of Boltzmann's statistics leads at once to Planck's formula without any specification of the magnitude of the transition probabilities. Einstein has connected it with a consideration of the transfer of momentum between atom and radiation, showing that the mechanism proposed by him is not consistent with the classical idea of spherical waves but only with a dart-like behaviour of the quanta. Here we are not concerned with this side of Einstein's work, but with its bearing on his attitude to the fundamental question of causal and statistical laws in physics. From this point of view this paper is of particular interest. For it meant a decisive step in the direction of non-

EPISTEMIC!

causal, indeterministic reasoning. Of course, I am sure that Einstein himself was—and is still—convinced that there are structural properties in the excited atom which determine the exact moment of emission, and that probability is called in only because of our incomplete knowledge of the pre-history of the atom. Yet the fact remains that he has initiated the spreading of indeterministic statistical reasoning from its original source, radio-activity, into other domains of physics.

ad

- Bohr

Still another feature of Einstein's work must be mentioned which was also of considerable assistance to the formulation of indeterministic physics in quantum mechanics. It is the fact that it follows from the validity of Planck's law of radiation that the probabilities of absorption (1 → 2) and induced emission (2 → 1) are equal. This was the first indication that interaction of atomic systems always involves two states in a symmetrical way. In classical mechanics an external agent like radiation acts on one definite state, and the result of the action can be calculated from the properties of this state and the external agent. In quantum mechanics each process is a transition between two states which enter symmetrically into the laws of interaction with an external agent. This symmetrical property was one of the deciding clues which led to the formulation of matrix mechanics, the earliest form of modern quantum mechanics. The first indication of this symmetry was provided by Einstein's discovery of the equality of up- and down-ward transition probabilities.

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IS THIS THE "1"

[illegible]

The last of Einstein's investigations which I wish to discuss in this report is his work on the quantum theory of monatomic ideal gases.[7] In this case the original idea was not his but came from an Indian physicist, S. N. Bose; his paper appeared in a translation by Einstein[8] himself who added a remark that he regarded this work as an important progress. The essential point in Bose's procedure is that he treats photons like particles of a gas with the method of statistical mechanics but with the difference that these particles are not distinguishable. He does not distribute individual particles over a set of states, but counts

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[7] *Berl. Ber.* 1924, p. 261, 1925, p. 318.

[8] S. N. Bose, *Zeitschrift für Physik*, 26, 178, (1924).

the number of states which contain a given number of particles. This combinatory process together with the physical conditions (given number of states and total energy) leads at once to Planck's radiation law. Einstein added to this idea the suggestion that the same process ought to be applied to material atoms in order to obtain the quantum theory of a monatomic gas. The deviation from the ordinary gas laws derived from this theory is called "gas degeneracy." Einstein's papers appeared just a year before the discovery of quantum mechanics; one of them contains moreover (p. 9 of the second paper) a reference to de Broglie's celebrated thesis, and the remark that a scalar wave field can be associated with a gas. These papers of de Broglie and Einstein stimulated Schroedinger to develop his wave mechanics, as he himself confessed at the end of his famous paper.[9] It was the same remark of Einstein's which a year or two later formed the link between de Broglie's theory and the experimental discovery of electron diffraction; for, when Davisson sent me his results on the strange maxima found in the reflexion of electrons by crystals, I remembered Einstein's hint and directed Elsasser to investigate whether those maxima could be interpreted as interference fringes of de Broglie waves. Einstein is therefore clearly involved in the foundation of wave mechanics, and no alibi can disprove it.

refs?

I cannot see how the Bose-Einstein counting of equally probable cases can be justified without the conceptions of quantum mechanics. There a state of equal particles is described not by noting their individual positions and momenta, but by a symmetric wave function containing the co-ordinates as arguments; this represents clearly only one state and has to be counted once. A group of equal particles, even if they are perfectly alike, can still be distributed between two boxes in many ways—you may not be able to distinguish them individually but that does not affect their being individuals. Although arguments of this kind are more metaphysical than physical, the use of a symmetric wave function as representation of a state seems to me preferable. This way of thinking has morover led to the other case of

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[9] "Quantisierung als Eigenwertproblem," *Annalen der Physik* (4), 70, p. 361, (1926); s. p. 373.

gas degeneracy, discovered by Fermi and Dirac, where the wave function is skew, and to a host of physical consequences confirmed by experiment.

The Bose-Einstein statistics was, to my knowledge, Einstein's last decisive positive contribution to physical statistics. His following work in this line, though of great importance by stimulating thought and discussion, was essentially critical. SAD. He refused to acknowledge the claim of quantum mechanics to have reconciled the particle and wave aspects of radiation. This claim is based on a complete re-orientation of physical principles: causal laws are replaced by statistical ones, determinism by indeterminism. I have tried to show that Einstein himself has paved the way for this attitude. Yet some principle ? of his philosophy forbids him to follow it to the end. What is this principle?

Einstein's philosophy is not a system which you can read in a book; you have to take the trouble to abstract it from his papers on physics and from a few more general articles and pamphlets. I have found no definite statement of his about the question "What is Probability?"; nor has he taken part in the discussions going on about von Mises' definition and other such endeavours. I suppose he would have dismissed them as metaphysical speculation, or even joked about them. From the beginning he has used probability as a tool for dealing with nature just like any scientific device. He has certainly very strong convictions about the value of these tools. His attitude toward philosophy and epistemology is well described in his obituary article on Ernst Mach:[10]

> Nobody who devotes himself to science from other reasons than superficial ones, like ambition, money making, or the pleasure of brain-sport, can neglect the questions, what are the aims of science, how far are its general results true, what is essential and what based on accidental features of the development?

Later in the same article he formulates *his empirical creed* in these words:

> Concepts which have been proved to be useful in ordering things easily acquire such an authority over us that we forget their human origin

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[10] *Phys. Zeitschr.* 17, p. 101, (1916).

and accept them as invariable. Then they become "necessities of thought," "given *a priori,*" etc. The path of scientific progress is then, by such errors, barred for a long time. It is therefore no useless game if we are practising to analyse current notions and to point out on what conditions their justification and usefulness depends, how they have grown especially from the data of experience. In this way their exaggerated authority is broken. They are removed, if they cannot properly legitimate themselves; corrected, if their correspondence to the given things was too negligently established; replaced by others, if a new system can be developed that we prefer for good reasons.

refs?

That is the core of the young Einstein, thirty years ago. I am sure the principles of probability were then for him of the same kind as all other concepts used for describing nature, so impressively formulated in the lines above. The Einstein of to-day is changed. I translate here a passage of a letter from him which I received about four years ago (7th November, 1944): "In our scientific expectation we have grown antipodes. You believe in God playing dice and I in perfect laws in the world of things existing as real objects, which I try to grasp in a wildly speculative way." These speculations distinguish indeed his present work from his earlier writing. But if any man has the right to speculate it is he whose fundamental results stand like rock. What he is aiming at is a general field-theory ☆ which preserves the rigid causality of classical physics and restricts probability to masking our ignorance of the initial conditions or, if you prefer, of the pre-history, of all details of the system considered. This is not the place to argue about the possibility of achieving this. Yet I wish to make one remark, using Einstein's own picturesque language: If God has made the world a perfect mechanism, he has at least conceded so much to our imperfect intellect that, in order to predict little parts of it, we need not solve innumerable differential equations but can use dice with fair success. That this is so I have learned, with many of my contemporaries, from Einstein himself. I think, this situation has not changed much by the introduction of quantum statistics; it is still we mortals who are playing dice for our little purposes of prognosis—God's actions are as mysterious in classical Brownian motion as in radio-activity and quantum radiation, or in life at large.

✓ D

MONTE CARLO - ULAM

Born does not reply to EPR.

Einstein's dislike of modern physics has not only been expressed in general terms, which can be answered in a similarly general and vague way, but also in very substantial papers in which he has formulated objections against definite statements of wave mechanics. The best known paper of this kind is one published in collaboration with Podolsky and Rosen.[11] That it goes very deep into the logical foundations of quantum mechanics is apparent from the reactions it has evoked. Niels Bohr has answered in detail; Schroedinger has published his own sceptical views on the interpretation of quantum mechanics; Reichenbach deals with this problem in the last chapter of his excellent book, *Philosophic Foundations of Quantum Mechanics,* and shows that a complete treatment of the difficulties pointed out by Einstein, Podolsky, and Rosen needs an overhaul of logic itself. He introduces a three-valued logic, in which apart from the truth-values "true" and "false," there is an intermediate one, called "indeterminate," or, in other words, he rejects the old principle of "*tertium non datur,*" as has been proposed long before, from purely mathematical reasons, by Brouwer and other mathematicians. I am not a logician, and in such disputes always trust that expert who last talked to me. My attitude to statistics in quantum mechanics is hardly affected by formal logics, and I venture to say that the same holds for Einstein. That his opinion in this matter differs from mine is regrettable, but it is no object of logical dispute between us. It is based on different experience in our work and life. But in spite of this, he remains my beloved master.

q.v.

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[11] A. Einstein, B. Podolsky, N. Rosen: "Can Quantum Mechanical Description of Physical Reality be Considered Complete?" *Phys. Rev.* 47, p. 777, (1935).

not of 'statistical'.