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Statistical Interpretation of Quantum Mechanics

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The published work for which the honor of the Nobel prize for the year 1954 has been accorded to me does not contain the discovery of a new phenomenon of nature but, rather, the foundations of a new way of thinking about the phenomena of nature. This way of thinking has permeated experimental and theoretical physics to such an extent that it seems scarcely possible to say anything more about it that has not often been said already. Yet there are some special aspects that I should like to discuss.

The first point is this: The work of Göttingen school, of which I was at that time the director, during the years 1926 and 1927, contributed to the solution of an intellectual crisis into which our science had fallen through Planck's discovery of the quantum of action in the year 1900. Today physics is in a similar crisis—I do not refer to its implication in politics and economics consequent on the mastery of a new and terrible force of nature, but I am thinking of the logical and epistemological problems posed by nuclear physics. Perhaps it is a good thing to remind oneself at such a time of what happened earlier in a similar situation, especially since these events are not without a certain element of drama. In the second place, when I say that physicists had accepted the way of thinking developed by us at that time, I am not quite correct. There are a few most noteworthy exceptions—namely, among those very workers who have contributed most to the building up of quantum theory. Planck himself belonged to the skeptics until his death. Einstein, de Broglie, and Schrödinger have not ceased to emphasize the unsatisfactory features of quantum mechanics, and to demand a return to the concepts of classical, Newtonian physics, and to propose ways in which this could be done without contradicting experimental facts. One cannot leave such weighty views unheard. Niels Bohr has gone to much trouble to refute the objections. I have myself pondered on them and believe I can contribute something to the clarification of the situation. We are concerned with the borderland between physics and philosophy, and so my physical lecture will be partly historically and partly philosophically colored, for which I ask indulgence.

Roots of Quantum Mechanics

First of all, let me relate how quantum mechanics and its statistical interpretation arose. At the beginning of the 1920's every physicist, I imagine, was convinced that Planck's hypothesis was correct, according to which the energy in oscillations of definite frequency ν (for example, in light waves) occurs in finite quanta of size $h\nu$. Innumerable experiments could be explained in this manner and always gave the same value of Planck's constant h. Furthermore, Einstein's assertion that light quanta carry momentum $h\nu/c$ (where c is the velocity of light) was well supported by experiment. This meant a new lease on life for the corpuscular theory of light for a certain complex of phenomena. For other processes, the wave theory was appropriate. Physicists accustomed themselves to this duality and learned to handle it to a certain extent.

In 1913 Niels Bohr had solved the riddle of line spectra by using quantum theory and at the same time had explained, in their main features, the wonderful stability of atoms, the structure of their electronic shells, and the periodic system of the elements. For the sequel the most important assumption of his teaching was this: an atomic system cannot exist in all mechanically possible states, which form a continuum, but in a series of "discrete stationary" states; in a transition from one to another the difference in energy $E_m - E_n$ is emitted or absorbed as a light quantum $h\nu_{mn}$ (according as E_m is greater or less than E_n). This is an interpretation, in terms of energy, of the fundamental law of spectroscopy discovered some years previously by W. Ritz. The situation can be pictured by writing the energy levels of the stationary states twice over, horizontally and vertically; a rectangular array results

	E_1	E_2	E_3 . . .
E_1	11	12	13
E_2	21	22	23

in which positions on the diagonal correspond to the states and off-diagonal positions correspond to the transitions.

Bohr was fully aware that the law thus formulated is in conflict with mechanics and that, therefore, even the use of the concept of energy in this context is problematical. He based this bold fusion of the old with the new on his principle of correspondence. This consists in the obvious requirement that ordinary classical mechanics must hold to a high degree of approximation in the limit, when the numbers attached to the stationary states, the quantum numbers, are very large—that is, far to the right and low down in the foregoing array—so that the energy changes relatively little from place to place—that is, practically continuously.

Theoretical physics lived on this idea for the next 10 years. The problem was

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that a harmonic oscillator possesses not only frequency but intensity as well. For each transition in the scheme there must be a corresponding intensity. How is the latter to be found by considerations of correspondence? It was a question of guessing the unknown from a knowledge of a limiting case. Considerable success was achieved by Bohr himself, by Kramers, by Sommerfeld, by Epstein, and by many others. But the decisive step was again taken by Einstein, who, by a new derivation of Planck's radiation formula, made it evident that the classical concept of intensity of emission must be replaced by the statistical idea of transition probability. To each position in our scheme there belongs, besides the frequency $\nu_{mn} = (E_m - E_n)/h$, a certain probability for the transition accompanied by emission or absorption of radiation.

In Göttingen we also took part in the attempts to distill the unknown mechanics of the atom out of the experimental results. The logical difficulty became ever more acute. Investigations on scattering and dispersion of light showed that Einstein's conception of transition probability as a measure of the strength of an oscillation was not adequate, and the idea of an oscillation amplitude associated with each transition could not be dispensed with. In this connection work by Ladenburg (*1*), Kramers (*2*), Heisenberg (*3*), Jordan and I (*4*) may be mentioned. The art of guessing correct formulas, which depart from the classical formulas but pass over into them in the sense of the correspondence principle, was brought to considerable perfection. A paper of mine, which introduced in its title the expression "quantum mechanics," probably for the first time, contains a very involved formula—still valid at the present time—for the mutual disturbance of atomic systems.

Heisenberg's Theory

This period was brought to a sudden end by Heisenberg (*5*), who was my assistant at that time. He cut the Gordian knot by a philosophic principle and replaced guesswork by a mathematical rule. The principle asserts that concepts and pictures that do not correspond to physically observable facts should not be used in theoretical description. When Einstein, in setting up his theory of relativity, eliminated the concepts of the absolute velocity of a body and of the absolute simultaneity of two events at different places, he was making use of the same principle. Heisenberg banished the picture of electron orbits with definite radii and periods of rotation, because these quantities are not observable; he demanded that the theory should be built up by means of quadratic arrays of the kind suggested in a preceding paragraph. Instead of describing the motion by giving a coordinate as a function of time $x(t)$, one ought to determine an array of transition probabilities x_{mn}. To me the decisive part in his work is the requirement that one must find a rule whereby from a given array

$$\begin{matrix} x_{11} & x_{12} & \cdots \\ x_{21} & x_{22} & \\ \vdots & & \end{matrix}$$

the array for the square,

$$\begin{matrix} (x^2)_{11} & (x^2)_{12} & \cdots \\ (x^2)_{21} & (x^2)_{22} & \\ \vdots & & \end{matrix}$$

may be found (or, in general, the multiplication law of such arrays).

By consideration of known examples discovered by guesswork he found this rule and applied it with success to simple examples such as the harmonic and anharmonic oscillator. This was in the summer of 1925. Heisenberg, suffering from a severe attack of hay fever, took leave of absence for a course of treatment at the seaside and handed over his paper to me for publication, if I thought I could do anything about it.

The significance of the idea was immediately clear to me, and I sent the manuscript to the *Zeitschrift für Physik*. Heisenberg's rule of multiplication left me no peace, and after a week of intensive thought and trial, I suddenly remembered an algebraic theory that I had learned from my teacher, Rosanes, in Breslau. Such quadratic arrays are quite familiar to mathematicians and are called matrices, in association with a definite rule of multiplication. I applied this rule to Heisenberg's quantum condition and found that it agreed for the diagonal elements. It was easy to guess what the remaining elements must be, namely, null; and immediately there stood before me the strange formula

$$pq - qp = h/2\pi.$$

This meant that coordinates q and momenta p are not to be represented by the values of numbers but by symbols whose product depends on the order of multiplication—which do not "commute," as we say.

My excitement over this result was like that of the mariner who, after long voyaging, sees the desired land from afar, and my only regret was that Heisenberg was not with me. I was convinced from the first that we had stumbled on the truth. Yet again a large part was only guesswork, in particular the vanishing of the nondiagonal elements in the foregoing expression. For this problem I secured the collaboration of my pupil Pascual Jordan, and in a few days we succeeded in showing that I had guessed correctly. The joint paper by Jordan and myself (*6*) contains the most important principles of quantum mechanics, including its extension to electrodynamics.

There followed a hectic period of collaboration among the three of us, rendered difficult by Heisenberg's absence. There was a lively interchange of letters, my contribution to which unfortunately went amiss in the political disorders. The result was a three-man paper (*7*), which brought the formal side of the investigation to a certain degree of completeness. Before this paper appeared, the first dramatic surprise occurred: Paul Dirac's paper (*8*) on the same subject. The stimulus received through a lecture by Heisenberg in Cambridge led him to results similar to ours in Göttingen, with the difference that he did not have recourse to the known matrix theory of the mathematician but discovered for himself and elaborated the doctrine of such noncommuting symbols.

The first nontrivial and physically important application of quantum mechanics was made soon afterward by W. Pauli (*9*), who calculated the stationary energy values of the hydrogen atom by the matrix method and found complete agreement with Bohr's formulas. From this moment there was no longer any doubt about the correctness of the theory.

Wave Mechanics

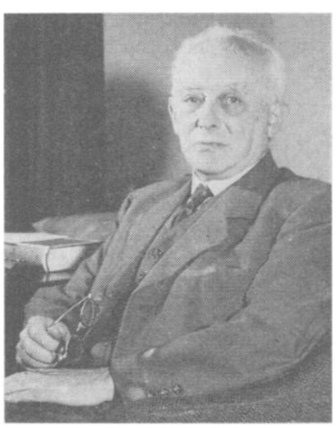
What the real significance of this formalism might be was, however, by no means clear. Mathematics, as often happens, was wiser than interpretative thought. While we were still discussing the point, there occurred the second dramatic surprise: the appearance of Schrödinger's celebrated papers (*10*). He followed quite a different line of thought, which derived from Louis de Broglie (*11*). The latter had a few years previously made the bold assertion, supported by brilliant theoretical considerations, that wave-corpuscle dualism, familiar to physicists in the case of light, must also be exhibited by electrons; to each freely movable electron there belongs, according to these ideas, a plane wave of perfectly definite wavelength, determined by Planck's constant and the mass. This exciting essay by de Broglie was well known to us in Göttingen.

One day in 1925 I received a letter from C. J. Davisson containing singular results on the reflection of electrons from metallic surfaces. My colleague on the experimental side, James Franck, and I at once conjectured that these curves of Davisson's were crystal-lattice spectra of de Broglie's electron waves, and we arranged for one of our pupils, W. Elsasser (*12*) to investigate the matter. His result provided the first quantitative proof of de Broglie's idea, a proof independently given later by Davisson and Germer (*13*) and by G. P. Thomson (*14*), by systematic experiments.

But this familiarity with de Broglie's line of thought did not lead on further toward an application to the electronic structure of atoms. This was reserved for Schrödinger. He extended de Broglie's wave equation, which applied to free motion, to the case in which forces act and gave an exact formulation of the additional conditions, already hinted at by de Broglie, to which the wave function ψ must be subjected—namely, that it should be single-valued and finite in space and time—and he succeeded in deriving the stationary states of the hydrogen atom as monochromatic solutions of his wave equation not extending to infinity. For a short while, at the beginning of 1926, it looked as if suddenly there were two self-contained but entirely distinct systems of explanation in the field—matrix mechanics and wave mechanics. But Schrödinger himself soon demonstrated their complete equivalence.

Wave mechanics enjoyed much greater popularity than the Göttingen or Cambridge version of quantum mechanics. Wave mechanics operates with a wave function ψ, which—at least in the case of one particle—can be pictured in space, and it employs the mathematical methods of partial differential equations familiar to every physicist. Schrödinger also believed that his wave theory made possible a return to deterministic classical physics; he proposed (and has emphatically renewed this suggestion quite recently, *15*) to abandon the particle picture entirely and to speak of electrons not as particles but as a continuous density distribution $|\psi|^2$, or electric density $e|\psi|^2$.

To us in Göttingen this interpretation appeared unacceptable in the face of the experimental facts. At that time it was already possible to count particles by means of scintillations or with the Geiger counter and to photograph their tracks with the help of the Wilson cloud chamber.



Max Born

Psi-function

It appeared to me that it was not possible to arrive at a clear interpretation of the ψ-function by considering bound electrons. I had therefore been at pains, as early as the end of 1925, to extend the matrix method, which obviously covered only oscillatory processes, in such a way as to be applicable to aperiodic processes. I was at that time the guest of the Massachusetts Institute of Technology in the U.S.A., and there I found in Norbert Wiener a distinguished collaborator. In our joint paper (*16*) we replaced the matrix by the general concept of an operator and, in this way, made possible the description of aperiodic processes. Yet we missed the true approach, which was reserved for Schrödinger; and I immediately took up his method, since it promised to lead to an interpretation of the ψ-function. Once more an idea of Einstein's gave the lead. He had sought to make the duality of particles (light quanta or photons) and waves comprehensible by interpreting the square of the optical wave amplitudes as probability density for the occurrence of photons. This idea could at once be extended to the ψ-function: $|\psi|^2$ must represent the probability density for electrons (or other particles). To assert this was easy; but how was it to be proved?

For this purpose atomic scattering processes suggested themselves. A shower of electrons coming from an infinite distance, represented by an incident wave of known intensity (that is, $|\psi|^2$) impinge on an obstacle, say a heavy atom. In the same way that the water wave caused by a steamer excites secondary circular waves in striking a pile, the incident electron wave is partly transformed by the atom into a secondary spherical wave, whose amplitude of oscillation ψ is different in different directions. The square of the amplitude of this wave at a great distance from the scattering center then determines the relative probability of scattering in its dependence on direction. If, in addition, the scattering atom is itself capable of existing in different stationary states, one also obtains quite automatically from Schrödinger's wave equation the probabilities of excitation of these states, the electron being scattered with loss of energy, or inelastically, as it is termed. In this way it was possible to give the assumptions of Bohr's theory, first verified experimentally by Franck and Hertz, a theoretical basis (*17*). Soon Wentzel (*18*) succeeded in deriving Rutherford's celebrated formula for the scattering of α-particles from my theory.

But the factor that contributed more than these successes to the speedy acceptance of the statistical interpretation of the ψ-function was a paper by Heisenberg (*19*) that contained his celebrated uncertainty relationship, through which the revolutionary character of the new conception was first made clear. It appeared that it was necessary to abandon not only classical physics but also the naive conception of reality that thought of the particles of atomic physics as if they were exceedingly small grains of sand. A grain of sand has at each instant a definite position and velocity. For an electron this is not the case; if one determines the position with increasing accuracy, the possibility of determining the velocity becomes less, and vice versa. I shall return to these questions in a more general connection, but before doing so would like to say a few words about the theory of collisions.

The mathematical techniques of approximation I used were somewhat primitive and were soon improved. Out of the literature, which has grown to unmanageable proportions, I can name only a few of the earliest authors, to whom the theory is indebted for considerable progress: Holtsmark in Norway, Faxén in Sweden, Bethe in Germany, Mott and Massey in Great Britain.

Today collision theory is a special science, with its own voluminous textbooks, and has grown completely over my head. Of course, in the last resort all the modern branches of physics, quantum electrodynamics, the theory of mesons, nuclei, cosmic rays, elementary particles and their transformations, all belong to this range of ideas, to a discussion of which no bounds could be set.

Probability of Transitions

I should also like to state that during the years 1926 and 1927 I tried another way of justifying the statistical conception of quantum mechanics, partly in collaboration with the Russian physicist Fock (*20*). In the afore-mentioned three-man paper there is a chapter in which the Schrödinger function is really anticipated; only it is not thought of as a function ψ of space, but as function ψ_n of the discrete index $n = 1, 2, \ldots$ which enumerates the stationary states. If the system under consideration is subject to a force that is variable in time, ψ_n also becomes time-dependent, and $|\psi_n(t)|^2$ denotes the probability for the existence of that state n at time t.

Starting from an initial distribution in which only one state is present, we obtain in this manner transition probabilities, and we can investigate their properties. In particular, what interested me most at the time was what happens in the adiabatic limiting case, that is, in the case of very slowly variable external action; it was possible to show that, as might have been expected, the probability of transitions became ever smaller. The theory of transition probabilities was developed independently by Dirac and made to yield results. It may be said that the whole of atomic and nuclear physics works with this system of concepts, especially in the extremely elegant form given to them by Dirac (*21*); almost all experiments lead to statements about relative probabilities of events, even if they appear concealed under the name cross section or the like.

How then does it come about that great discoverers such as Einstein, Schrödinger, and de Broglie are not satisfied with the situation? As a matter of fact, all these objections are directed not against the correctness of the formulas but against their interpretation. Two closely interwoven points of view must be distinguished: the question of determinism and the question of reality.

Newtonian mechanics is deterministic in the following sense. If the initial state (positions and velocities of all particles) of a system is accurately given, the state at any other time (earlier or later) may be calculated from the laws of mechanics. All the other branches of classical physics have been built up in accordance with this pattern. Mechanical determinism gradually became an article of faith—the universe as a machine, an automaton. As far as I can see, this idea has no precursors in ancient or medieval philosophy; it is a product of the immense success of Newtonian mechanics, especially in astronomy. In the 19th century it became a fundamental philosophic principle for the whole of exact science. I asked myself whether this was really justified. Can we really make absolute predictions for all time on the basis of the classical equations of motion? It is easily seen, by simple examples, that this is the case only if we assume the possibility of absolutely accurate measurement (of the position, velocity, or other quantities). Let us consider a particle moving without friction on a straight line between two endpoints (walls) at which it suffers perfectly elastic recoil, The particle moves backward and forward with constant speed equal to its initial speed v_0, and one can say exactly where it will be at a stated time provided that v_0 is accurately known.

But if we allow a small inaccuracy Δv_0, the inaccuracy of the prediction of position at time t is $t\Delta v_0$; that is, it increases with t. If we wait long enough, until time $t_c = l/\Delta v_0$, where c is the distance between the elastic walls, the inaccuracy Δx will have become equal to the whole interval l. Thus it is possible to say absolutely nothing about the position at a time later than t_c. Determinism becomes complete indeterminism if one admits even the smallest inaccuracy in the velocity datum. Is there any sense—I mean physical, not metaphysical, sense—in which one can speak of absolute data? Is it justifiable to say that the coordinate x is π cm, where $\pi = 3.1415\ldots$ is the familiar transcendental number that determines the ratio of the circumference of a circle to its diameter? As an instrument of mathematics, the concept of a real number represented by a nonterminating decimal is extremely important and fruitful. As a measure of a physical quantity, the concept is nonsensical. If the decimal for π is interrupted at the 20th or 25th place, two numbers are obtained which cannot be distinguished by any measurement from each other and from the true value. According to the heuristic principle employed by Einstein in the theory of relativity and by Heisenberg in quantum theory, concepts that correspond to no conceivable observation ought to be eliminated from physics. This is possible without difficulty in the present case also; we have only to replace statements like $x = \pi$ cm by: the probability of the distribution of values of x has a sharp maximum at $x = \pi$ cm; and (if we wish to be more accurate) we can add: of such and such a breadth. In short, ordinary mechanics must be formulated statistically. I have occupied myself with this formulation a little recently and have seen that it is possible without difficulty. This is not the place to go into the matter more closely. I only wish to emphasize the point that the determinism of classical physics turns out to be a false appearance, produced by ascribing too much weight to mathematicological conceptual structures. It is an *idol,* not an *ideal,* in the investigation of nature and, therefore, cannot be used as an objection to the essentially indeterministic, statistical interpretation of quantum mechanics.

Much more difficult is the objection concerned with reality. The concept of a particle, for example, a grain of sand, contains implicitly the notion that it is at a definite position and has a definite motion. But according to quantum mechanics it is impossible to determine simultaneously with arbitrary accuracy position and motion (more correctly momentum, that is, mass times velocity). Thus two questions arise. First, what is there to prevent us from measuring both quantities with arbitrary accuracy by refined experiments, in spite of the theoretical assertion? Second, if it should really turn out that this is not feasible, are we still justified in applying to the electron the concept of particle and the ideas associated with it?

With regard to the first question, it is clear that if the theory is correct—and we have sufficient grounds for believing this—the obstacle to simultaneous measurability of position and motion (and of other similar pairs of so-called "conjugate" quantities) must lie in the laws of quantum mechanics itself. This is indeed the case, but it is not at all obvious. Niels Bohr himself has devoted much labor and ingenuity to developing a theory of measurements to clear up this situation and to meet the most subtle considerations of Einstein, who repeatedly tried to think out measuring devices by means of which position and motion could be measured simultaneously and exactly. The conclusion is as follows. In order to measure space coordinates and instants of time rigid measuring rods and clocks are required. On the other hand to measure momenta and energies arrangements with movable parts are needed to take up and indicate the impact of the object to be measured. If we take into consideration the fact that quantum mechanics is appropriate for dealing with the interaction of object and apparatus, we see that no arrangement is possible that satisfies both conditions at the same time. There exist, therefore, mutually exclusive but complementary experiments, which only in combination with each other disclose all that can be learned about an object. This idea of complementarity in physics is generally regarded as the key to the intuitive understanding of quantum processes. Bohr has transferred the idea in an ingenious manner to completely different fields—for example, to the relationship between consciousness and brain, to the problem of free will, and to other fundamental problems of philosophy.

Now to come to the final point—can we still call something with which the concepts of position and motion cannot be associated in the usual way a *thing,* a *particle*? And if not, what is the reality that our theory has been invented to describe?

The answer to this question is no longer physics, but philosophy, and to deal with it completely would overstep the bounds of this lecture. I have expounded my views on it fully elsewhere (*23*). Here I will only say that I am emphatically for the retention of the particle idea. Naturally it is necessary to redefine what is meant. For this purpose well-developed concepts are available, which are familiar in mathematics under the name of invariants with respect to transformations. Every object that we perceive appears in innumerable aspects.

The concept of the object is the invariant of all these aspects. From this point of view, the present universally used conceptual system, in which particles and waves occur at the same time, can be completely justified.

The most recent research on nuclei and elementary particles has, however, led us to limits beyond which this conceptual system in its turn does not appear to suffice. The lesson to be learned from the story I have told of the origin of quantum mechanics is that, presumably, a refinement of mathematical methods will not suffice to produce a satisfactory theory, but that somewhere in our doctrine there lurks a conception not justified by any experience, which will have to be eliminated in order to clear the way.

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C. T. Brues, Zoologist

Charles Thomas Brues, professor emeritus of entomology at Harvard University, died in Crescent City, Florida, on 22 July 1955. He was born in Wheeling, West Virginia, on 20 June 1879. The family moved to Chicago in 1893, and the following year Brues, with a fellow-student, Axel Leonard Melander, attended the North Division High School in Chicago. This was a significant event, for under the tutelage of the principal, Oliver S. Wescott, and the biology teacher at the school, Herbert Eugene Walter, the boys were inspired to undertake a serious study of insects.

On graduation from high school Brues and Melander entered the University of Texas to study under W. M. Wheeler, who had just been appointed there. After taking his A.B. degree in 1901 and his M.S. degree in 1902, Brues went to Columbia University for 2 years, subsequently returning to Texas as a special field agent in entomology for the U.S. Department of Agriculture. It was at this time that he married Beirne Barrett, a former biology major at the University of Texas.

In 1905 he was appointed curator of invertebrate zoology at the Milwaukee Public Museum but left there in 1909 to join Wheeler, who was then professor of entomology and dean of the Bussey Institution at Harvard University. Brues was appointed instructor in economic entomology and advanced through the several ranks, becoming professor of entomology in 1935. Just prior to this, in 1932, the Bussey Institution was abolished as a separate graduate school, and Brues and Wheeler moved their offices to the Biological Laboratories in Cambridge, the headquarters of the department of biology. In 1946 Brues was appointed professor emeritus and honorary curator of parasitic hymenoptera in the Museum of Comparative Zoology.

Brues was broadly interested in all aspects of insects and, indeed, in all biological phenomena. Although most of his research was of a taxonomic nature, his investigations also included such diverse subjects as the ecology of thermophilous animals, the food and feeding habits of insects, insect paleontology, medical entomology, fluorescent staining of insect tissues, and intracellular bacteroids of insects. His early publications were devoted mainly to the taxonomy and biology of myrmecophilous insects, especially phorid flies; later papers also dealt with taxonomic studies on parasitic Hymenoptera, including the fossil forms in Baltic amber and in the Florissant shales of Colorado. His bibliography contains 280 titles. Several of his publications appeared in book form: *A Key to the Families of North American Insects* (with A. L. Melander), 1915; *Insects and Human Welfare* (1921 and 1947); *Insect Dietary* (1946); and the *Classification of Insects* (with A. L. Melander), which went through three printings in the first edition. The revised and enlarged edition of the latter (1954), with F. M. Carpenter as a third author, was the last of Brues' publications.

In connection with his investigations, Brues made a number of field trips; on these he was usually accompanied by Mrs. Brues, a biologist in her own right and the author of several botanical papers. In addition to many collecting expeditions in this country, he went to Jamaica in 1911–12, Peru and Ecuador in 1913 (Harvard Medical Expedition), Cuba in 1926–27, Hudson Bay in 1936 (amber insect collecting), Dutch East Indies, Sumatra, Java, Celebes, and Bali in 1937, and the Philippines in 1949.

Brues took great interest in the Cambridge Entomological Club and was the editor of *Psyche,* the club's journal, from 1910 to 1947. He took an active part in other scientific societies and served as president of the Entomological Society of America in 1929.

His teaching at Harvard was very effective, especially at the graduate level. He was unusually close to his students and was always available to them for friendly and informal discussions. He was a wise counselor whose greatest strength was in his humility and in his devotion to truth.

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