# HOW DOES A PARTICLE GET FROM *A* TO *B*?

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I think it is important to try to see quantum theory as one among different possible theories. I think we must make an effort to assess its successes and failures from a point of view which does not implicitly assume its essential correctness, while yet trying to give proper weight to the different reasons people have for thinking that the general picture presented by quantum theory is the only possible one.

Quantum theory was developed to take into account a certain class of experimental facts—namely those facts which forced on our attention that there exist discrete attributes of the physical world which cannot be incorporated within an essentially continuous classical theory. It seems reasonable to ask how far quantum theory has succeeded in this task.

Of course, the early forms of the theory never attempted to *explain* discreteness in the sense that they could be said to have incorporated both the discrete and the continuous within one theoretical structure. They simply imposed discreteness as a mathematical constraint on the range of values available as allowable experimental results of the measurement of certain physical quantities. (This description is directly applicable to the energies of atomic structures. It covers measurements involving free particles if we take the familiar probabilistic interpretation of the constant in the uncertainty relation between simultaneous momentum and position measurements.)

As quantum theory developed, however, attitudes towards the problem of explaining the discrete values seem gradually to have changed. At present a majority of physicists probably regard the modern form of the quantum theory as a coherent intellectual structure within which both discrete and continuous quantities appear properly related, and consider that modern quantum theory gives us an understanding of the intrusion of discreteness within continuum physics.

I regard Bohr's complementarity doctrine also as a theory whose first and essential function is to explain the existence of atomicity in circumstances where only concepts of continuity physics are considered operationally well-defined. The finite value of Planck's constant is the essential connecting link between the continuum concepts and the explanation of discreteness.

This account of Bohr's complementarity seems unacceptable to most: there is however, one problem, in the context of the explanation of the discrete, which no physicist would claim to have solved. This is the calculation of the values of the atomic and other basic physical constants. A quite elementary understanding of quantum theory may give us the idea that the uncertainty relation specifies a (statistically defined) lower bound on physical measurement to which the constant specifies a numerical value. It then turns out that to specify any such idea as this exactly one must derive absolute units from the familiar dimensionless constants which can be formed as ratios of the dimensional constants. These dimensionless constants can therefore be regarded as parameters which determine the scale of the microscopic phenomena in terms of the cosmological. This is how we see them if we think of a continuum physics with constants mathematically imposed from outside the theory. If we think in terms of a closed or self-sufficient continuum physics then they become parameters which can be interpreted as coupling constants or interaction constants which specify the relative strengths of the fundamental fields of physics. These constants have for a long time been a source of interest to those who would like to imagine the quantal situation from a starting point which does not presuppose the correctness of current quantum theory. The best-known contribution to the history of thinking about these constants was Eddington's conjecture that they are more fundamental than the dimensional constants ($\hbar$, $c$, and the like) of which they are usually written as ratios, and that they may well originate in hitherto obscure algebraic relationships like group structures. This contention was opposed by Dirac,[(2)] who argued that no importance need be attached to the values of the interaction constants since they might change with time, in which case these values at any particular epoch would not be significant.

The difference in outlook which underlies such different evaluations of the significance of the values of the interaction constants can again be reduced to a difference of opinion as to whether quantum theory really explains the existence of discrete magnitudes. If it does explain them then there remains a problem which may or may not be soluble within quantum theory itself—namely, the calculation of the actual numerical value of the constants though there can be different opinions as to the seriousness of this problem's being left unsolved. If, on the other hand, quantum theory has not given an adequate explanation of discreteness itself, then the constants constitute a much more pressing problem, for one has—if one is concerned at all with the generality in physical theory—to construct first an existence theorem:—namely that definite interaction constants exist (i.e. that they have some value rather than no value) and only then separately to calculate the values they actually have. An argument like Dirac's—accordingly—is only cogent at all if one is already independently assured theoretically of the existence of atomicity in the world.

I have now made a case for thinking that quantum theory has not explained the most basic fact it set out to deal with. Adopting this view, accordingly, for the sake of argument, I must first discuss the nature of its success. I shall assume—to put a complex position in a sharp, unambiguous, if crude way—that quantum theory has its main area of undisputed success in the theory of atomic spectra and in solving problems (such as those of the theory of solids) which arise as fairly natural extensions of that theory.† I argue, moreover, that in this area the discoveries made by quantum theory have been discoveries of *combinatorial relations*, and of predictive schemes expressed in terms of such relations. Where there has been dynamics, in the strict sense, it has been imported from classical ways of thinking, and a way of working has been established in which a rather uneasy association of combinatorial scheme with classical type dynamics has become the rule, with a generalized faith in the unitary nature of physical explanation to serve in place of any real synthesis of these two fundamentally different kinds of thinking. Hence we think in classical concepts which presuppose indefinite divisibility of material, and express the presupposition in the mathematical representation of the space and time continua, and yet we work formally with a discrete theory. The difficulties produced by this situation were discussed, in one way or another, in most of the papers in the colloquium. I shall discuss the situation as it concerns the concept of *particle path* or *trajectory*, because this fundamental classical notion—though simple—presents the characteristic difficulties that will be encountered by any combinatorial theory devoid of dynamics.

In a standard text—Chapter 8 of their *Quantum Mechanics—Non-Relativistic Theory*—Landau and Lifschitz [(1)] use the mechanics of a particle moving in a straight line to introduce and demonstrate the essential difference between quantum theory and classical dynamics. In the former, the path of the particle becomes progressively smoother as more observations are made of the particle. In the

† C. W. Kilmister's paper 'Beyond what?' in this book, takes a similar line.

quantum-theoretical case things are different; the observations generate a swarm of points rather than a progressively more determinate line which could be interpreted as 'the path of the particle' (fig. 1). In this text, this difference is made the experimental foundation upon which a mathematical superstructure is built. Quantum

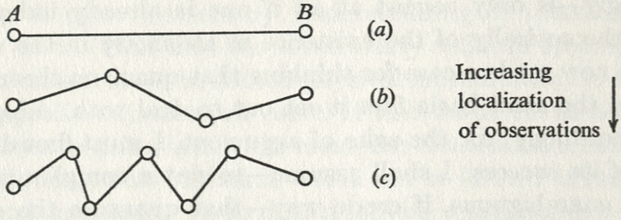


Fig. 1

theory has to kick off somewhere and this is quite a good place; it is not as neat as Dirac's [2] basing the mathematical superposition principle on the behaviour of a composite photon path in a Jamin interferometer, but it is a considerably more useful approach when we are up against problems raised by high energy particles.

When we think about the concept of the path of a quantum particle in the way suggested by Landau and Lifschitz, it is very tempting to idealize the phenomenon by insisting that except where we already have an observation of the position of the particle we can have no knowledge of any sort of the whereabouts of that particle. A given particle process—in this idealized way of looking at things—is that process and no other. Thus, for example, to obtain a 'repetition' of that process with one additional piece of information added (say another collision or disintegration) may require a quite prohibitive increase in the difficulty of the experimental arrangements that are necessary to give a reasonable chance of securing the phenomenon, as well as a quite different mathematical procedure for handling the resulting experimental information. This idealization is one that is becoming more familiar as the operationally realistic approach in the case of high energy particles.

This aspect of the quantum picture is also stressed by Feynman[3] in his presentation of fundamentals, even for low energies. Discussing the two-slit experiment, Feynman observes: 'Now we are not allowed to ask which slit the electron went through unless we actually set up a device to determine whether or not it did. *But then we would be considering a different process.*' (Feynman's italics).

I shall call the idealization that interpolation or extrapolation of points can only have significance on the basis of new experiments being conducted to define each new point, *the quantum idealization.* It is tempting to maintain that the quantum idealization is the whole story. However no physicist ever behaves as though it really is. The physicist always in fact relies on there being a ghostly form of the old classical notion of continuity of path somewhere in the background. For example, suppose we have three cloud chambers *A*, *B*, *C*,

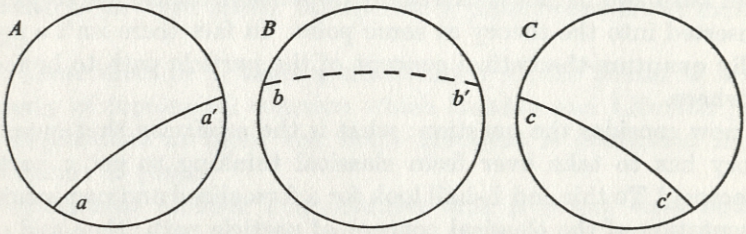


Fig. 2

arranged as shown in fig. 2, and suppose that photographs are taken simultaneously of *A* and *C* but not of *B*. And suppose that the 'path of a particle' is observed at *aa'* and at *cc'*. The quantum idealization would require us to say that we had no justification whatever for asserting that if we had photographed *B* at the same time as we photographed *A* and *C*, then we should have got a track *bb'* (shown dotted). Obviously what the quantum idealization dictates in this sort of case is unreasonable, and physicists are right in their general practice, but to understand how there can be a *limited degree of applicability* of the classical picture of the continuous path is very difficult.

It is possible to take a rigid 'ensemble' view of particle path, according to which the 'particle' is completely undefined for single (or even few) observations. A logically consistent picture can be achieved this way but then all interest shifts to the question why ensembles of single observations so cohere as to provide the appearance of a particle trajectory. I should regard the ensemble *façon de parler* as pointless in the absence of a detailed theory which accounted for this coherence.

Moreover, that Landau and Lifschitz present a *sequence* of cases, makes no difference to my contention that there is more justification for the classical picture than the quantum idealization can provide. The particle path may become progressively fuzzier as greater localization is demanded, but even with high localization a sort of Milky Way of points is left, which has to be explained *somehow*. If one takes the quantum-theoretical position literally then the surprising thing is not that 'particle path' becomes an indefinite concept but that there is any sense at all to be attached to it. Presumably quantum theorists believe that if the solution of the relevant equations could be pursued in sufficient detail then the trajectory-like distribution of points would emerge. This belief however is a pure act of faith and in fact is extremely implausible unless the trajectory is inserted into the theory at some point. In fact there isn't a specifically quantum-theoretical concept of the particle path to be found anywhere.

I now consider the question: what is the *minimum* that quantum theory has to take over from classical thinking to get a particle trajectory? To this end I shall look for a formalized and non-intuitive presentation of the classical concept of particle path; then and only then, can we decide whether what we take over is consistent with the principles of quantum theory or not. Our first essay in this direction might naturally be to consider the definitions of the straight line that are provided by classical mathematical analysis. Does, for example, the Dedekind cut satisfy our requirements? It isn't much good. The operations that are imagined in defining continuity in the Dedekind manner don't seem to have anything to do with the considerations that the classical physicist invokes when he wants to say what he means and what he does not mean by a particle having moved from $A$ to $B$.

An account of continuity that is operationally more adequate to the physicist's underlying problem is that used by L. E. J. Brouwer, for whom the continuity of a line required in the first place the possibility of indefinite interpolation of points. (A view which was part of the general constructivist philosophy of Brouwer in which one could significantly speak of a mathematical entity if and only if one had specified an algorithm for constructing that entity.) In this case new points must be generated by some finite process, and what is needed for continuity is to know that there is no limit to the process of selecting two points and interpolating a new one between them (i.e. adding a new point in a given order to the existing set).

We can now list the main requirements necessary to define a sufficient degree of classical continuity in a quantum-theoretical set in order to provide a realistic particle path.

1. The points must be *discriminable*. One must be able to know one from another. (In the classical idea of a line, of course, one would always be able to make measurements independently of the definition of the points which would settle at once the discriminability of the points.)

2. The points will have to be *ordered*. That is to say each point will have to have a definite successor and a definite predecessor at any given stage of building up of the line in order that meaning can be attached to the instruction 'interpolate a point between two existing ones'.

3. There must be a 'topological cohesion' of the points. It is this property of topological cohesion which Landau and Lifschitz point out degenerates as more and more precision is demanded in the localization of points.

Requirement number 3 is intuitive and it is not clear how to express it exactly in general. However, there exists one special case in which it can be given a clear meaning. This is the case of a 'space of potential infinity' in which a construction rule is defined for new points and in which experimental discovery or 'observation' of the new points at this abstract level is identical with the construction process. In this special case the smoothness of the curve or 'tendency of the points to keep together' is not something over and above the discriminating and ordering of the points, but is the same process. It is not to be deduced from these remarks that all trajectories will be smooth—only that the concept of cohesion has been defined and further discussion of detailed cases (like the sequence I have quoted from Landau and Lifschitz) will be possible to describe different sorts of cohesion.

My paper in Part v is an application of a theory which uses a 'space of potential infinity' of precisely the sort that I have just described and the foregoing note really provides the general arguments to justify such a radical attempt.

## REFERENCES

(1) Landau, L. D. and Lifschitz, E. M. *Quantum Mechanics—Non Relativistic Theory* (London: Pergamon, 1958).

(2) Dirac, P. A. M. *Quantum Mechanics* (Oxford, any edition).

(3) Feynman, R. P. *Theory of Fundamental Processes* (Benjamin, 1961).