The British Society for the Philosophy of Science

ARE THERE QUANTUM JUMPS?

PART II *

E. Schrödinger

7 *The Cultural Background, Again*

It might be maintained that the case I am making in this essay is quite irrelevant. It might be said that the 'privilege' is not meant seriously, that I am naïvely taking it at its face value. We may, so my opponent might continue, use the pure energy states of *any* system; just take our choice so that they are convenient to handle analytically. Any state of any system may be regarded as some superposition of some or all of its proper modes (= pure energy states). One may deal with them separately, *as if* the system was in one of them; the several results, duly superposed, will then tell us what would come of their superposition, if we really want to know. Perhaps my supposed opponent would even grant that he is using just a convenient shorthand, the same as in chemistry and statistical thermodynamics; but he would maintain that this is perfectly admissible also for investigating a single event of interaction between micro-systems.

To this I have two answers, referring to two very different points. The first is this. Even if this shorthand were admissible for the micro-event, we have to keep in mind that physicists are not the only people interested in physics and in the outcome of theoretical research in physics. Those results that are tenable and will survive are destined, eventually, to be absorbed into our world picture and to become part and parcel thereof. A great many of our educated contemporaries, not equipped with the mathematical apparatus to follow our more technical deliveries, are yet deeply concerned with many general questions; one of the most stirring among them certainly is whether actually *natura facit saltus* or no. Whatever abbreviated language we physicists may find convenient to use among ourselves, we ought to be aware of the dilemmas that justly and duly interest others; we must be careful not to veil or distort them by indulging in loose speech. It is this point that I had in mind in my general historical introduction.

* Part I appeared in volume III, no. 10, of this *Journal.*

Science is not a soliloquy. It gains value only within its cultural milieu, only by having contact with all those who are now, and who in future will be, engaged in promoting spiritual culture and knowledge. The extant scientific papers of Archimedes, the dialogues and discourses of Galileo, are still of genuine interest in our day, and not only to philologists, but to many scientists. Would it mean setting ourselves too high and too proud a goal, if we occasionally thought of what will have become of our scientific papers 2,000 years hence? Science will have changed entirely. Will there be anybody to grasp our meaning, as we grasp the meaning of Archimedes?

8 *Details about Collisions*

This was the first part of my answer to the supposed opponent. The second part is more technical, and ought to be supported by a little mathematics which I must avoid here. I doubt whether in dealing with a micro-event (' collision problem ') one is allowed to pick one's ' initial state ' arbitrarily as composed of pure energy states of the two non-interacting systems, relying on the principle of un-tainted superposition. In point of fact, this procedure regularly leads to very acceptable results in the first meaningful approximation and to entirely unacceptable ones in the next step of approximation. Now it so happens that in the wave equation the term that depicts the interaction (engendered by that relatively small third part of the energy operator which we spoke of in Section 5), is, of necessity, a *non-linear* term ; it must contain the *product of the two wave-functions*, otherwise it could not entail any interaction between them. Thus by the very task it has to fulfil this term must infringe upon linearity, and thereby do away with clean and simple superposition. And it is, of course, from this term, and from this term alone, that the results are deduced. Now general mathematical considerations tell us in a case of this kind, three things, namely

(i) starting from any initial situation, the first small change caused by the perturbating term is correctly computed from the latter in spite of its non-linearity ; but

(ii) the changes computed in this way for two or more initial situations are not simply additive, when an initial situation, composed of the former, is contemplated ; there is a substantial mutual influence of the components ; moreover

(iii) to improve on the first approximation is a very intricate task, if the perturbing term is not linear ; the current routine follows, as far as I can see, the pattern introduced for *linear* perturbations. Thus it is insufficient to cover the actual situation.

There may, of course, be other reasons for the actual failure of the approximation method, a failure now usually referred to as 'the divergences.' We have loosely called the interaction operator 'relatively small.' Now this epithet cannot properly apply to an operator, only to the quantity which results from applying the operator to another quantity (in our case : to the wave-functions). This result is different in different parts of the field. There may be regions where the 'relative smallness' does not hold. It may be justifiable to disregard this fact, and the approximation method may none the less yield acceptable results. Even so one must be prepared to discover a fundamental shortcoming. This is well illustrated by the first application of this method of approximation, leading to the earliest quantitative achievement of wave mechanics, viz. my computation, in 1926, of the splitting of the spectral lines of atomic hydrogen by an external electric field (Stark effect). Both the frequencies and the intensities agreed very satisfactorily with observation. Yet a few years later Cornel Lanczos discovered a fundamental shortcoming in my statements. Compared with the field of the nucleus, the perturbing external field is weak only in the neighbourhood of the nucleus, where the internal field is strong ; the ratio is reversed at moderate distances, since the internal field fades away rapidly according to the inverse square law while the external field is constant. This has the consequence that none of the Stark effect lines is really sharp, an effect that is readily observed in the higher members of the spectral series when a fairly strong external field is applied.

There is another point that I consider relevant in collision problems. Towards the end of Section 6 I mentioned that two micro-systems which vibrate, each of them truly and exactly in one of its proper modes, cannot be conceived as approaching each other. This is not a far-fetched subtlety. Translatory motion with precisely fixed velocity is wave-mechanically represented by a plane sinusoidal wave filling the whole space. Two such plane waves, one pertaining to the first, the other to the second system, do not exhibit any feature representing 'distance from each other.' They describe a state that virtually

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includes all possible distances down to the smallest at which the inter-action already is ' in full swing.' To choose for the *initial state* one that includes these two plane waves seems hazardous. One thereby disregards the building up of the interaction during the gradual approach. I am more surprised at the acceptable results this pro-cedure yields in the first instance than at its ensuing failure. Some-times it is permissible to think of one system as localised. One would then at least have to picture the other as a plane wave *with a wave head* or wave front, approaching the point where the collision is to take place. It is known from pre-quantum physics that what happens at a wave front may differ widely from the stationary state that estab-lishes itself by and by in its rear. For example, it was shown by A. Sommerfeld that when a plane electromagnetic wave passes from vacuum into a dielectric (pictured as composed of molecules that contain electronic oscillators), the head of the wave is not refracted according to Snell's law, but continues indefinitely in the same direc-tion with unchanged velocity of propagation. The reason is that the electrons are initially at rest ; each of them is seized upon by the oncoming wave train and is gradually rocked into oscillations of in-creasing amplitude until an equilibrium state is reached after many waves have passed. True, this model is obsolete. But the means used now to depict the physical situation still follow the pattern to which it belonged. I do not believe that the intricacies involved in the ' classical ' problems have passed away. I find it hard to believe that quantum physics holds the master key for overcoming these intricacies without attacking them. In the above example, taken from Lorent-zian electronics, the initial state, which shows us an undisturbed incident wave and an electron at rest, differs very substantially from the final state, when the electron is in full swing and superposes its wavelet on the incident wave. (Indeed, all these wavelets together turn the original plane wave into one of entirely different velocity and entirely different direction.) In Lorentzian electronics this gradual transition cannot possibly be dealt with as a problem of ' perturbation.' One cannot hope that in quantum physics this method will yield an exhaustive answer, unless one maintains that according to quantum physics nothing of the sort happens at all, and that the whole analytical apparatus is set up only for telling us with what probability the system may choose to hop from one state into another, these ' states ' being patently selected so as to meet our analytical convenience and ability. Is that not wishful thinking ?

9 *On Neo-Machian Purism*

There is, of course, among physicists a widely popular tenet, informed by the philosophy of Ernst Mach, to the effect that the only task of experimental science is to give definite *prescriptions* for success-fully foretelling the results of any future observations from the known results of previous observations. If this contention is taken at its face value, then *not only* is it irrelevant whether the prescription makes use of a visualisable model or only consists in definitely prescribed mathe-matical operations, to be performed with the previously observed values in order to obtain predicted values ; *but also* the said operations would apparently not even have to be mathematically correct, as long as they are precise and enable us to prophesy correctly. Indeed the lack of rigour in a theoretical deduction is nowadays sometimes followed by the remark that what vindicates the procedure is its success, its leading to results that agree with observation. The neo-Machian principle covers this argument not only pending the proof that the theoretical deduction is correct, *but even if it should prove to be wrong.*

Nobody will agree. But why? If our task is only to predict precisely and correctly by any means whatsoever, why not by false mathematics ? Because, as little as in any other science, does the scientific method in physics consist of a vast number of juxtaposed independent prescriptions for the purpose of prophecy. Whether or no they are based on a visualisable model-picture, those prescrip-tions, if one wants to give them this abstract name, form an intricately connected and interwoven scheme. Even in the simplest quantitative experiment half a dozen of them must be relied upon for applying corrections to the rough numbers read on the scales, before you obtain what you call the result of your measurement. Moreover, there are basic statements or assumptions, from which a thousand branches of inference issue, which coalescing in bundles with many others eventually lead off and on to a relevant new ' prediction.' And in every single experiment, though it be not of a new type, this network of inferences has to be produced afresh, adapted to the special data and circumstances. The ' scheme of prescriptions ' is not satisfactory unless we trust it to embrace, suitably applied, *any* future observation. We have no other means for rendering tolerably precise the application to new cases that were not foreseen, nay for drawing any conclusions at all with respect to them, but to decide once and for

all to use the self-consistent scheme of correct mathematics. Mathematics is not a very special thing. It is the most general way of contemplating the possible relationships between numbers without contradiction. A single 'formula,' provided it contains at least one undetermined symbol, (and does not exhibit an explicit zero as a denominator), cannot be 'mathematically wrong.' Those 'prescriptions' of physical science contain at least two undetermined symbols, viz. at least one representing a previous observation, and one that is to be predicted. So no single prescription can be mathematically wrong. But we have never to do with one alone. We have to combine them in rational thought. Without a guide we should be groping in the dark. Correct mathematics is the indispensable guide even to the hardiest Machian purist. Most physicists, whether or no they confess to it, are using some kind of model-picture besides.

So much for vindicating my clamour for mathematical consistency in handling collision problems.

10 Numbers

The states of sharp energy (whose privilege of being *the* states of a system, with abrupt transitions between them, I contest) include the characteristic that the total number of particles of every kind contained in the system be an integer. According to Einstein's famous discovery (1905) that mass and energy are the same thing, a particle of rest-mass m, when at rest, represents an amount of energy mc^2 (c = velocity of light in vacuo). Broadly speaking, n equal particles contained in a system contribute the amount nmc^2 to any of the sharp energy levels of the whole system, which differ therefore on this account by integral multiples of mc^2. This is an entirely sound, nay, an unavoidable assumption, even though we have not yet succeeded in accounting satisfactorily for the various numerical values of m, actually observed in the various elementary particles, as electrons, protons, the various mesons, etc., from which the masses of atoms and molecules are composed.

From the point of view taken here we must consider it inadequate to think, e.g. a body of nitrogen gas in front of us as consisting of a definite integral number of nitrogen molecules that could be indicated. It has to be represented by a superposition of proper modes with a considerable spread over many integers. We should wish to estimate the extension of this spread. This, it must be granted, is not so easy.

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Though it is *not* a question of fluctuations, one naturally thinks that considerations of the kind used in thermodynamics might come to our rescue. However, the thermal energy of one degree of freedom ($\frac{1}{2}kT$) is—except perhaps at the temperature T of the interior of a star —so small compared with the rest energies mc^2, that in ordinary circumstances no system containing ponderable mass must be considered in *true* thermodynamic equilibrium.

The following attempt to answer the question suggests itself. Our isolated body of nitrogen gas has the same properties as one that forms part of a much larger body from which it is only severed by fictitious boundaries ; indeed our body might have been obtained from a much larger one, the fictitious boundaries being replaced by real ones. It is universally agreed that the number N of molecules contained within a volume delimited only in thought exhibits fluctuations of the order of $\sqrt{N}$. I consider it reasonable to assume that the wave function of the isolated system would in general be composed of eigenfunctions whose N-values spread over a region of order of magnitude $\sqrt{N}$.

It may be asked : what difference does this make ? Could it be tested by experiment ?—The main difference is that it is reasonable, while it is unreasonable to admit a sharp count N of objects that avowedly lack individuality.[1] Secondly, this is the obvious way of registering the fact, that we *never* experiment with just *one* electron or atom or (small) molecule. In thought-experiments we sometimes assume that we do ; this invariably entails ridiculous consequences as, e.g. that a spherical de Broglie wave, which is supposed to represent ' one ' electron, moving in an ' unknown ' direction, suddenly collapses into a small wave parcel, when ' that ' electron is detected at a definite spot. Nothing of the sort happens if the number ' one ' is not ascertained, but may as well be zero or two or three. Even better : the certain detection of one does not reduce the expectation of a second or third, it leaves it unchanged, according to generally accepted principles of statistics. And, I do not regret to say, we sorely need those spherical waves as realities (not merely as expressing our lack of knowledge), if we wish to account, e.g. for G. P. Thomson's beautiful experiments on the interference patterns of de Broglie waves, diffracted by crystals ; and the same in many, many other cases.

[1] The lack of individuality of micro-systems is argued in my article in *Endeavour*, July 1950, **9**, No. 35 ; reprinted in the Smithsonian Report for 1950, pp. 183-196 ; in German, *Die Pyramide*, January and February, 1951.

11 *Observing Single Particles*

In writing this I have before me the lovely records of single particle tracks, of 'stars' (nuclear disintegrations), of 'showers,' of broken tracks showing successive decays of one particle $(\pi \to \mu \to e)$, the photographic emulsion records that C. F. Powell selected to illustrate his Nobel lecture on cosmic radiation in 1950. I am asking myself whether here as well as in the tracks of particles produced in the Wilson cloud chamber we are not actually experimenting with a quite definite number of particles, in some cases really with just one particle, and whether we are not presented with records of quantum transitions, which I am so anxious to deny.

In the first place it is fair to state that we are not *experimenting* with single particles, any more than we can raise Ichthyosauria in the zoo. We are scrutinising records of events long after they have happened. That the interval of time is shorter and that we have ourselves produced favourable conditions for the records to produce themselves, makes no difference. We can never reproduce the same single-particle-event under planned varied conditions ; and this is the typical procedure of the experimenter. But now to the other question : are these not records of quantum transitions ?

They certainly do *not* show us the transformation of a couple of plane waves (representing the colliding particles before the collision) into a number of other plane waves (representing the particles that emerge after the collision). Here my opponent interrupts me : ' Well, that's just the point. That is why your waves must not be regarded themselves as a real observable phenomenon, but only as indicating the probability of particle-phenomena.' I mentioned just before that there are many experiments which we simply cannot account for without taking the wave to be a wave, acting simultaneously throughout the region over which it spreads, not ' perhaps here ' or ' perhaps there,' as the probability view would have it. I shall come back to this point.

The real difficulty for the wave aspect is the exact balance of energy (and momentum) *in the single event*, for which the said records (in the photographic plate and in the Wilson cloud chamber) are full evidence. Take the simplest case : an α-particle of given initial velocity in the cloud-chamber. It has a well defined range ; it is always stopped after travelling a distance of, say, 5 cms., and after producing a certain number of ionisations, which number is practically

always the same and corresponds to the particle spending its kinetic energy and coming to rest. How are we to understand this, if ionisation is to mean *not* the transfer of an energy parcel, but only a resonance phenomenon ? Well, there would be no difficulty in understanding it, if the slowing down were due to a static gravitational or electric field against which the particle runs up. In this case, if we picture the particle as a composite wave-phenomenon (usually called a ' wave-parcel '), this would, according to the theory, actually be slowed down as a whole and come to rest as a whole, always at the same distance, when its ' kinetic energy is spent.' What happens is that all the constituents have their wave-length slowly, ' adiabatically,' increased so that the group keeps together. To meet the actual case we are speaking of one has to adopt the view that the ionisable medium influences the particle that passes through it in the same adiabatic fashion as a field would. I cannot see that this view is quite unacceptable once we have shaken off the nightmare that physical events consist in continual sequences of little fits and jerks, the handing over of energy parcels from one particle (or group of particles) to another.

I believe that not even the apparently catastrophic events oblige us to accept the ' fit and jerk view '; I mean the events recorded as ' stars ' showing a nucleus shelled to pieces by the impact of a swift particle. They only occur with *exceedingly* fast particles of *exceedingly* high wave frequency. This encourages one to think that they too may be slow, adiabatic processes ; for the criterion of ' slowness ' always is : slow compared with the rapidly oscillating wave-field. One has, of course, to use the auxiliary concept, familiar to quantum physicists, of wave parcels in more than three dimensions, actually three times the number of particles that come into play. To enlarge on this here in general terms would have little value. Besides I am quite aware that eventually there remains the momentous question : what are these apparent particles anyhow? Can it be understood that they turn up within continuous wave trains—somewhat like the white crests in a choppy sea—and that in some cases they constitute the only observable features of these wave trains ?

I am aware of these questions. They are no longer as embarrassing as they were, before we had gained the insight we have now gained into what a particle certainly is *not;* it is *not* a durable little thing with individuality.[1] However, let me conceive of an opponent to

[1] See my article in *Endeavour,* July 1950, **9**, No. 35.

whom the preceding paragraphs of this Section 11 seem lame excuses and loose talk. To him I should still plead as follows.

The cloud chamber and emulsion phenomena, though they are at the moment in the focus of interest, represent after all only a small section of all that we know about nature. In their apparent simplicity they appeal to the vivid imagination of an intelligent child. They would have set any of the old warriors for the cause of atomism, from Democritus down to Dalton and Boltzmann, gasping with joyful excitement. Yet they are not as simple as they look. This is witnessed by the pages and pages of intricate formalism that is often devoted to account for even the simplest of them. We must not be so rash as to remove the obvious difficulties presented by these new and unprecedented observations, by remodelling our picture of the physical world in a manner that makes it unfit to give us an understanding of a great many other things that are at the moment not fashionable.

To give an example : if one scans the introductory chapters of Max von Laue's standard works on the diffraction of X-rays [1] and on the diffraction of material waves,[2] one finds that no pattern of thought has yet been discovered to get on in these matters even one step without regarding the wave functions—Maxwell's field and the de Broglie amplitudes of both the incident wave and those it encounters in the diffracting body—as describing something real. Here ' real ' is not a controversial philosophical term. It means that the wave acts simultaneously throughout the whole region it covers, not either here or there. That would fail to account for the interference phenomena. So the epithet ' real ' means the momentous difference between ' both-and ' (et-et) and ' either-or ' (aut-aut). I challenge anyone to dispute this discrepancy away, if he can. I do not think there is any escape from this : if you accept the current probability view (aut-aut) in quantum mechanics, the single-event observation becomes comparatively easy to tackle, but all the rest of physics (unfashionable at the moment) is lost to sight.

[1] *Roentgenstrahlinterferenzen*, Berlin, Akad. Verl. Ges., 1941
[2] *Materiewellen und ihre Interferenzen,* ibid. 1948

School of Theoretical Physics
Dublin Institute for Advanced Studies
64-66 Merrion Square, Dublin