The British Journal for the Philosophy of Science

ARE THERE QUANTUM JUMPS ?

PART I*

E. SCHRÖDINGER

' . . cormnciai a credere, che uno, che lascia un'opinione imbevuta col latte, e seguita da infiniti, per venire in un' altra da pochissimi seguita, e negata da tutte le scuole, e che veramente sembra un paradosso grandissimo, bisognasse per necessità, che fusse mosso, per non dir forzato, da ragiom più enícaci.' Galileo, *Dialogue on the Two Greatest World Systems,* 2nd Day.

1 *The Cultural Background*

PHYSICAL science, which aims not only at devising fascinating new experiments, but at obtaining a rational understanding of the results of observations, incurs at present, so I believe, the grave danger ot getting severed from its historical background. The innovations of thought in the last 50 years, great and momentous and unavoidable as they were, are usually overrated compared with those of the preceding century ; and the disproportionate foreshortening by time-perspective, of previous achievements on which all our enlightenment m modern times depends, reaches a disconcerting degree according as earlier and earlier centuries are considered. Along with this disregard for histoncal linkage there is a tendency to forget that all science is bound up with human culture in general, and that scientific findings, even those which at the moment appear the most advanced and esoteric and difficult to grasp, are meaningless outside their cultural context. A theoretical science, unaware that those of its constructs considered relevant and momentous are destined eventually to be

H 109

framed in concepts and words that have a grip on the educated com-
munity and become part and parcel of the general world picture—
a theoretical science, I say, where this is forgotten, and where the
initiated continue musing to each other in terms that are, at best,
understood by a small group of close fellow travellers, will necessarily
be cut off from the rest of cultural mankind ; in the long run it is
bound to atrophy and ossify, however virulently esoteric chat may
continue within its joyfully isolated groups of experts. This has
happened before in similar circumstances. Benjamin Farnngton
puts it admirably in his *Greek Science,[1]* vol. 2, p. 173 :

> ' Perhaps the most decisive defeat of the scientific spirit in antiquity
> had been the loss of the sense of history. History is the most funda-
> mental science, for there is no human knowledge which cannot lose
> its scientific character when men forget the conditions under which it
> originated, the questions which it answered, and the functions it was
> created to serve. A great part of the mysticism and superstition of
> educated men consists of knowledge which has broken loose from its
> historical moorings.'

The disregard for historical connectedness, nay the pride of
embarking on new ways of thought, of production and of action,
the keen endeavour of shaking off, as it were, the indebtedness
to our predecessors, are no doubt a general trend of our time. In
the fine arts we notice strong currents quite obviously informed
by this vein ; we witness its results in modern painting, sculpture,
architecture, music and poetry. There are many who look upon
this as a new buoyant rise, while others regard it as a flaring up that
inaugurates decay. It is not here the place to dwell on this question,
and my personal views on it might interest nobody. But I may say
that whenever this trend enters science, it ought to be opposed.
There obviously is a certain danger of its intruding into science in
general, which is not an isolated enterprise of the human spirit, but
grows on the same historic soil as the others and participates in the
mood of the age. There is, however, so I believe, no other nearly
so blatant example of this happening as the theones of physical science
in our time. I believe that we are here facing a development which
is the precise counterpart of that in the fine arts alluded to above.
The most appropnate expression to use for it is one borrowed from
the history of poetry : Gongonsm. It refers to the poetry of the
Spaniard Luis de G6ngora (1561-1627), very fine poems, by the way,

[1] Pelican Books, Loi.m, 1949

especially the early ones.   Yet also his later poems (to which the term more particularly refers) are well sounding and *they all make sense.* But he uses all his acuity and skill on making it as difficult as possible to the reader to unravel the sense, so that even natives of Castile use extended commentaries to grasp the meaning safely.

One ought not, I think, to say that if, in this, physics is following a general trend of our time, we must not oppose it.   Though we are entirely the product of historical development, yet it is we who make its continuation and not history that drags us along a predestined trail. It depends entirely on us, on our stopping to think and acting according to reason, whether there will be decay or a new rise after the crisis. This is what Bertrand Russell in recent years has not tired to inculcate with regard to much more momentous questions than the fate of theoretical physics.   However, here we shall be concerned with the latter.

My friend and scientific colleague Professor Hans Thirnng, in his book *Homo Sapiens,*[1] in which he conducts an elaborate and very creditable campaign against War, and for Universal Peace, incidentally opines that in antiquity everybody except a few men of genius considered the earth to be a flat disk.   Professor E. P. Wigner, in an article on ' The Limits of Science '[2] is in doubt whether to date the ' birthyear' of chemistry around 1780 (Lavoisier) or at 1808 (Dalton's law).   Physics, he says, is somewhat older, since Newton's *Principia* became available m 1687.   He grants that ' Archimedes discovered laws of physics around 250 B.C. but his discoveries can hardly be called the real beginning of physics.'   I must not take up space by refuting these strange views, but refer the reader to Professor Benjamin Farnngton's two excellent Pelican books on *Greek Science.*   Still I would mention that among ˎ the ' insignificant' discoveries of that period was the mference, drawn (probably by Archimedes) from the heliocentric system of Aristarchus, that the fixed stars must be at least at a distance of, in our units, about two light years ; and the further conclusion that from there the sun would appear as a faint star, and therefore, mversely, many of those stars must equal and even exceed the sun in size—or luminosity, as we would call it today.   Of course scientific knowledge takes some time to get a grip on the cultured community.   Charles Darwin tells us in the *Voyage of a Naturalist* of the sensation he caused in 1833 among the

[1] Wien, 1948

[2] *Proc. Am. Philosoph. Soc.* 1950, 94, *422*

'educated' society in Argentine by telling them that the earth is a sphere. This knowledge was then more than 2,300 years old.

What has all this to do with quantum jumps ? I have been trying to produce a mood that makes one wonder what parts of contemporary science will still be of interest to others than historians 2,000 years hence. There have been ingenious constructs of the human mind that gave an exceedingly accurate description of observed facts and have yet lost all interest except to historians. I am thinking of the theory of epicycles. I confess to the heretical view that their modern counterpart in physical theory are the quantum jumps. Or rather these correspond to the *circles* which the sun, the moon and the stars were thought to describe around the earth in 24 hours after earlier and better- knowledge had been condemned. I am . reminded of *epicycles* of vanous orders when I am told of the hierarchy of *virtual* quantum transitions. But let these rude remarks not deter you. We shall now come to grips with the subject proper.

2 *The Discontinuous States as Proper Modes*

Max Planck's essential step in 1900, amounted, as we say now, to laying the foundation of quantum theory; it was his discovery, by abstract thought, of a *discontinuity* where it was least expected, namely in the exchange of energy between an elementary material system (atom or molecule) and the radiation of light and heat. He was at first very reluctant to draw the much more incisive conclusion that each atom or molecule had only to choose between a *discrete* set of 'states'; that it could normally only harbour certain discrete amounts of energy, sharply defined and characteristic of its nature ; that it would normally find itself on one of these 'energy levels' (as the modern expression runs)—except when it changes over more or less abruptly from one to another, radiating its surplus energy to the surrounding, or absorbing the required amount from there, as the case may be. Planck was even more hesitant to adopt the view that radiation itself be divided up into portions or light-quanta or 'photons,' to use the present terminology. In all this his hesitance had good reasons. Yet only a few years later (1905) Einstein advanced the hypothesis of light-quanta, clinching it with irresistible arguments; and in 1913 Niels Bohr, by taking the discrete states of the atoms seriously and extending Planck's assumptions in two directions with great ingenuity, but irrefutable consistency, could explain quantita-

tavely some of the atomic line spectra, which are all patently *discrete,* and which had in their entirety formed a great conundrum up to then : .Bohr's theory turned them into the ultimate and irrevocable direct evidence, that the discrete states are a genuine and real fact. Bohr's theory held the ground for about a dozen of years, scoring a grand series of so marvellous and genuine successes, that we may well claim excuses for having shut our eyes to its one great deficiency : while describing minutely the so-called ' stationary ' states which the atom had normally, i.e. m the comparatively uninteresting periods when *nothing happens,* the theory was silent about the periods of transition or ' quantum jumps ' (as one then began to call them). Since intermediary states had to remain disallowed, one could not but regard the transition as instantaneous ; but on the other hand, the radiating of a coherent wave train of 3 or 4 feet length, as it can be observed m an interferometer, would use up just about the average interval between two transitions, leaving the atom no time to ' be' in those stationary states, the only ones of which the theory gave a description.

This difficulty was overcome by quantum mechanics, more especially by wave mechanics, which furnished a new description of the *states;* this was precisely what was still missing in the earliest version of the new theory which had preceded wave mechanics by about one year. The previously· admitted discontinuity was not abandoned, but it shifted from the *states* to something else, which is most easily grasped by the simile of a vibrating string or drumhead or metal plate, or of a bell that is tolling. If such a body is struck, it is set vibrating, that is to say it is slightly deformed and then runs in rapid succession through a continuous series of slight deformations again and again. There is, of course, an infinite vanety of ways of striking a given body, say a bell, by a hard or soft, sharp or blunt instrument, at different points or at several points at a time. This produces an infinite vanety of initial deformations and accordingly a truly infinite vanety of shapes of the ensuing vibration : the rapid ' succession of cinema pictures,' so we might call it, which describes the vibration following on a particular initial deformation is infinitely manifold. But m every case, however complicated the actual motion is, it can be mathematically analysed as being the *superposition* of a discrete series of comparatively simple ' proper vibrations,' each of which goes on with a quite definite frequency. This discrete senes of frequencies depends on the shape and on the matenal of the body, its density and elastic properties. It can be computed from the theory of elasticity,

113

from which the existence and the discreteness of proper modes and proper frequencies, and the fact that any possible vibration of that body can be analysed into a superposition of them, are very easily deduced quite generally, i.e. for an elastic body of any shape whatsoever.

The achievement of wave mechanics was, that it found a general model picture in which the ' stationary ' states of Bohr's theory take the rôle of proper vibrations, and their discrete ' energy levels' the rôle of the proper frequencies of these proper vibrations ; and all this follows from the new theory, once it is accepted, as simply and neatly as in the theory of elastic bodies, which we mentioned as a simile. Moreover, the radiated frequencies, observed in the line spectra, are in the new model, equal to the *differences* of the proper frequencies ; and this is easily understood, when two of them are acting simultaneously, on simple assumptions about the nature of the vibrating ' something.'

3 *The Alleged Energy Balance—a Resonance Phenomenon*

But to me the following point has always seemed the most relevant, and it is the one I wish to stress here, because it has been almost obliterated—if words mean something, and if certain words now in general use are taken to mean what they say. The principle of super-position not only bridges the gaps between the ' stationary' states, and allows, nay compels us, to admit intermediate states without removing the discreteness of the ' energy levels ' (because they have become proper frequencies) ; but it completely *does away with the prerogative of the stationary states.* The epithet stationary has become obsolete. Nobody who would get acquainted with wave mechanics without knowing its predecessor (the Planck-Einstein-Bohr-theory) would be inclined to think that a wave-mechanical system has a pre-dilection for being affected by only one of its proper modes at a time. Yet this is implied by the continued use of the words ' energy levels,' ' transitions,' ' transition probabilities.'

The perseverance in this way of thinking is understandable, because the great and genuine successes of the idea of energy parcels has made it an ingrained habit to regard the product of Planck's constant h and a frequency as a bundle of energy, lost by one system and gained by another. How else should one understand the exact dove-tailing in the great ' double-entry' book-keeping in nature ? I maintain that it can in all cases be understood as a resonance pheno-

menon.   One ought at least to try, and look upon atomic frequencies just as frequencies and drop the idea of energy-parcels. I submit that the word 'energy' is at present used with two entirely different meanings, macroscopic and microscopic.   Macroscopic energy is a 'quantity-concept' (Quantitätsgrösse).   Microscopic energy meaning $h>$) is a 'quality-cojncept' or 'intensity-concept' (Intensitätsgrösse) ; it is quite proper to speak of high-grade and low-grade energy according to the value of the frequency v.   True, the macroscopic energy is, strangely enough, obtained by a certain weighted summation over the frequencies, and in this relation the constant h is operative. But this does not necessarily entail that in every single case of micro-scopic interaction a whole portion $h>$ of *macroscopic* energy is ex-changed.   I believe one is allowed to regard microscopic interaction as a continuous phenomenon without losing either the precious results of Planck and Einstein on the equilibrium of (macroscopic) energy between radiation and matter, or any other understanding of pheno-mena that the parcel-theory affords.

The one thing which one has to accept and which is the inalien-able consequence of the wave-equation as it is used in every problem, under the most various forms, is this : that the interaction between two microscopic physical systems is controlled by a peculiar law of resonance.   This law requires that the *difference* of two proper fre-quencies of the one system be equal to the difference of two proper frequencies of the other :

$$"i \ - \ v| \ = \ v_2' \ - \ v_2. \qquad\qquad\qquad .\text{(i)}$$

The interaction is appropriately described as a gradual change of the amplitudes of the four proper vibrations in question.   People have kept to the habit of multiplying this equation by h and saying it means, that the first system (index i) has dropped from the energy level /ivi to the level hv_1, the balance being transferred to the second system, enabling it to rise from hv_t to hv_2'.   This interpretation is obsolete. There is nothing to recommend it, and it bars the understanding of what is actually going on.   It obstinately refuses to take stock of the principle of superposition, which enables us to envisage simultaneous gradual changes of any and all amplitudes without surrendering the essential discontinuity, if any, namely that of the frequencies: To be accurate we must add, that the condition of resonance, equation (i), may include three or more interacting systems.   It may for example

read $$v_l - v_t' = v_t' - v_2 + v_3' - v_s. \qquad . \qquad . \qquad (2)$$

" 5

9

Moreover we may adopt the view that the two or more interacting systems are regarded as *one* system.   One is then inclined to write equations (i) and (2), respectively, as follows

$$v_l +_v, = v_l' +_v,'. \qquad . \qquad . \qquad . \qquad . \qquad (1')$$

$$"1 + "i + "3 = "1' + ",' + \bullet ',',. \qquad \bullet \qquad \bullet \qquad \bullet \qquad (2')$$

and to state the resonance condition thus : the interaction is restricted to constituent vibrations of the *same* frequency.   This is a familiar state of affairs, of old.   Unfamiliar is the tacit admission that frequencies are *additive,* when two or more systems are considered as forming *one* system.   It is an inevitable consequence of wave mechanics.   Is it so very repugnant to common sense ? If I smoke 25 cigarettes per day, and my wife smokes 10, and my daughter 12—is not the family consumption 47 per day—on the average ?

4 *A Typical Experiment*

Jokes aside, I wish to consider some typical experiments that ostensibly force the energy parcel view upon us, and I wish to show that this is an illusion.   A beam of cathode rays of uniform velocity, which can be gradually increased, is passed through sodium vapour. Behind the vessel containing the vapour the beam passes an electnc field which deflects it and tells us the velocity of the particles after the passage.   At the same tune a spectrometer inspects the light, if any, emitted by the vapour.   For small initial velocity nothing happens : no light, no change of velocity in the cathode beam.   But when the initial velocity is increased beyond a sharply defined limit, two things happen.   The vapour begins to glow, radiating the frequency of the first line of the ' principal senes' ; and the beam of cathode rays emerging from the vapour is split into two by the deflecting electnc field, one indicating the initial velocity unchanged, and another slow one has ' lost an amount of energy ' equal to the frequency of the said spectral line multiplied by Planck's constant *h.*   If the velocity is further increased the story repeats itself when the incident cathode ray energy increases beyond the ' energy level' that is responsible for the second line (or rather the ' level-difference' in question) ; this line appears and a third beam of cathode rays with correspondingly reduced speed occurs; and so on.   This was, and still is, • regarded as blatant evidence of the energy parcel view.

But it is just as easily understood from the resonance point of view.   A cathode ray of particles with uniform velocity is a mono-

chromatic beam of de Broglie waves.   Only when its frequency
(v_x) surpasses the frequency difference $(v_s' — v_2)$ between the lowest
(v_2) and the second (v_2') proper frequencies of the sodium atom is there
a de Broglie frequency $v^{\wedge'} > 0$ that fulfils the resonance demand,
equation (i).   Then the vibration $v^{\wedge}$ appears in the de Broglie wave
and vt' among the atoms which begin to glow with frequency
•V —"«» since Maxwell's ' electromagnetic vacuum' is prepared
for resonance with-anything.   The splitting of the cathode ray beam
in the deviating electric field, after passing the vapour, is accounted
for by de Broglie's wave equation.   An electric field has for de
Broglie waves an ' index of refraction ' that *depends* on their frequency
(' dispersion') and has a gradient in the direction of the field (which
thus acts as an ' inhomogeneous medium').   Any further events
that might happen, for instance a transfer of some of the ' energy
quanta' $h(v_2' — v_2)$ from the sodium atoms to other gas molecules
by ' impacts of the second kind,' are just as easily understood as
.esonance phenomena, provided only one keeps to the wave picture
throughout and for all particles involved.

Many similar cases of apparent transfer of energy-parcels can
be reduced to resonance—for instance photochemical action.   The
pattern is always the same : you may either take equations like (i)
or (2) as they stand (resonance), or multiply them by *h* and think they
express an energy balance of every single micro-transition.   In the
preceding example one point is of particular interest.   One is able
by an external agent (the electric field) to *separate in space* the two or
more frequencies which have arisen in the cathode ray by the inter-
action ; for they behave differently towards this agent and the different
behaviour is completely understood from de Broglie's wave equation ;
one thus obtains two or more beams of homogeneous frequency (or
veloaty).   It is extremely valuable that there are simple cases of this
kind in which the separation into two ' phases' has nothing enigmatic;
it is an immediate consequence of the principles laid down in L. de
Broglie's earliest work on material waves.   I say, this is fortunate ;
for there is a vast domain of phenomena in which the separation in
space either takes place in the natural conditions of observation,
or can easily be brought about by simple appliances; but it is not
as easily explained on first principles.   This might dishearten one in
accepting the view of gradually changing amplitudes, that I put
forward here ; for the separation into different phases that produces
itself before our eyes seems to confirm the belief that a discontinuous

117

abrupt and *complete* transition occurs in every single microscopic interaction.

5 *Chemistry, Photochemistry, and the Photoelectric Effect*

The vast amount of phenomena I am alluding to is m the first place ordinary chemistry. Two or more constituents, mixed in a solution or m a gaseous phase, begin to react with each other, under the influence of light or otherwise ; the portions that have reacted and have formed a new chemical compound may separate themselves almost entirely from the rest and form a new phase, say because the product is almost insoluble m the liquid, or (in the case of a gaseous mixture) by its being a liquid or solid with a low vapour pressure at the temperature in question. Almost any chemical reaction may serve as an example, but let us take a slow one to facilitate speech and thought. If a suitable mixture of hydrogen gas (H_2) and oxygen gas (O_2) is illuminated by ultraviolet light, the following slow reaction is induced

$$2H_2 + O_2 \rightarrow 2H_2O \quad \bullet \qquad . \quad . \quad . \qquad (3)$$

As the concentration of water vapour (H_2O) increases, part of it separates off into liquid droplets.

The actual process is not as simple as the balance (3) indicates, it is a chain reaction. But we need pay no attention to this, and contemplate only the initial state and the end-product. Wave-mechanically the gaseous mixture is represented by a vibration of the combined system, and, by the way, not by *one* proper vibration since there is anyhow the vast variety of translational and rotational modes, and, of course, the electronic modes. The gaseous compound, H_2O, is represented by an entirely different vibration of the *same* system. The modes composing it, absent at first, are gradually chiming in as the reaction proceeds. But then there is a *third* group of vibrations representing the liquid H_2O ; they gradually build up where they are facilitated by dust nuclei, and are observed as droplets. It is, of course, deplorable that wave mechanics does not allow us to follow this *observed* process analytically, while, in the now current inter-pretation, ample information is forthcoming about a host of experi-ments that nobody has ever been or ever will be able to perform (for instance we are told, what is the probability of our finding at a definite spot inside a given hydrogen atom an electron, if we look for one). But there is no reason to suspect that the separation of phases is funda-mentally different from the spectroscopic resolution of a beam of

light or of cathode rays into its monochromatic constituents. One need not be afraid that the formation of spatial boundaries, separating coherent regions of chemically or physically distinct properties, cannot possibly be controlled by the wave equation, but must necessarily be accounted for by the picturesque pageantry of individual molecules swallowing or re-spewing whole energy parcels, being disrupted and re-combined, until they eventually go to form one or two molecules of a new type.

I deem the latter simply •wrong ; it is not in accordance with our present state of knowledge, whose further progress is hampered if these easy pictures, that are in common use, are taken literally.   And we are encouraged to take them literally not only by text-books and popular essays but also by the language used in very high-browed technical treatises.   By this I will not deny that this imagery is a very useful, nay indispensable, conceptual shorthand in chemical research.   One cannot see how to avoid it when, for example, a complicated chain reaction is to be unravelled.   And, of course, the chemical equation for describing a reaction will never be ousted, though it ostensibly describes the single micro-event and is wrong in this.   It is an instance of the famous ' as if.' It is not the first instance of this land in the relation of chemistry and physics.   The chemist used the valency stroke for building models of complicated molecules.   It represented very real facts of observation. For a long time the physicist could not afford any explanation of the mechanism of the chemical bond. Then, in brief succession, *two* were given : there is a heteropolar bond (Kossel, 1916) and a homopolar bond (London-Heitler, 1926).   The discoveries were illuminating to the chemist, indeed they removed some difficulties caused by interpreting the valency strokes too naively. But, of course, the valency strokes were retained as an extremely convenient shorthand.   They could be retained because they were based on carefully pondered observation.

As one of the simplest photochemical reactions we may regard the photoelectric effect, which was one of the main incentives for Einstein in 1905 to launch the hypothesis of light quanta.   When a metal plate is illuminated by light of sufficiently high *frequency,* elearons emerge from it forthwith with an energy corresponding to this frequency. There is no tune delay, even when the *intensity* of the incident light is so weak that according to the electron theory of H. A. Lorentz, which was at the tune in full swing, an electron would need half an hour to be sped up to the velocity in question. This was—and,

I am afraid, still is—regarded as convincing evidence of the instantaneous transfer of whole quanta of energy from the light to the electron. I understand the present orthodox interpretation to be as follows. The incident light beam produces at once in each of tens of thousands of electrons an exceedingly small *probability* of taking within the next split second a leap into a state of higher translational energy ; a correspondingly small fraction of those tens of thousands do so and emerge from the metal, and that is why the game starts without delay.

But according to wave mechanics, as put forward by de Broglie and myself and generally accepted, the interpretation does produce without delay electronic wave trains of the higher frequency that we observe emerging from the metal. (For to observe the *frequency* of an electron or its *velocity* means the same thing:) After this has been recognised, is the probability scheme any longer needed ? Has the idea of the mysterious sudden leaps of single electrons not become gratituitous ? Is it expedient ? The waves are there anyhow, and v/e are not at a loss to prove it. We need only put a tube of crystal powder in the way of the emerging beam and produce an interference pattern of the type first achieved by G. P. Thomson (it might not be as beautiful as Thomson's, but it would vouch for the waves all the same).

6 *Single Interaction Processes between*

Micro-Systems (' *Collisions')*

There are besides chemistry several other domains of theoretical investigation m which the simplifying scheme of individual constituent micro-systems on sharp energy levels, with abrupt transitions between them, affords a very convenient shorthand. Nearly all thermodynamical considerations are greatly facilitated by adopting this scheme in speech and thought, which makes very little difference, if-any, in the results. This constitutes a certain danger. In the inseparable union of speech and thought the primacy, rather paradoxically, rests with speech. When we hear the same words again and again pronounced with authority, we are apt to forget that they were originally meant as an abbreviation ; we are induced to believe that they describe a reality.

If the simplified scheme of sharp energy states and abrupt transitions between them was workable throughout in all instances (which

I do not think is the case), one would have to try and cast it into a consistent theory. At the moment no such theory exists and I can see no prospect of obtaining one, nor any inducement to try, for the reason just mentioned in parenthesis. At present the scheme is inconsistent, not only because of the mystery the *transitions* continue to present from their first appearance in the theories of Planck (1900) and Bohr (1913), but also for another reason, intimately connected with the former. In application to two individual micro-systems which interact, it is not at all clear which are the pure energy levels distinguished, to which the scheme shall apply. The choice rests with the mathematical technique. The usual procedure is as follows. The total energy (which enters the mathematical analysis as the ' operator' of the wave equation) is regarded as made up of three additive parts; the two mam contributions are said to pertain to the single systems and are said to control their behaviours, respectively, if they did *not* interact; the third is said to be their energy (or' operator ') of interaction. But this partition is rather artificial, at any rate whilst the interaction takes place. It is largely guided by the requirement that the main parts should be comparatively simple and easy to deal with analytically, the whole complication of the problem bemg shoved into the interaction, which is called a perturbation and dealt with by methods of approximation. Even so, it is hardly ever amenable to a true solution (albeit an approximate one) ; one has to content oneself with finding out what happens in a small interval of time. One computes the very small changes of amplitudes that occur during this short interval; and one is pleased to call the time-rate of change the *probability of transition.* By calling it so one expresses the belief that after the interaction has taken place and the two systems have separated again each of them will find itself in a pure sharp energy state. The computation does *not* give this result. The computation tells us, that in either system a host of pure energy-states will be super-posed—with a certain dependence between the partial amplitudes in the one system and those in the other. But one chooses to interpret this result as meaning that there will be *complete* exchange between only *one* pair of proper modes, one of the many for which the resonance condition holds.

One might say, why not, if this interpretation works and if it is consistent ? I maintain that u is inconsistent. The reason is the following. Assume each of the two systems found itself in a pure energy state, when they were isolated, before the interaction started.

Now let the interaction set in ; take it to be very weak. It is certainly legitimate to adopt the attitude that we are now faced with *one* system ; nay, this is the fundamentally correct attitude to take ; the splitting of the wave-operator *(* energy') into two private parts plus a part depicting the interaction is only a mathematical artifice. But however weak the interaction be, it has the *immediate* consequence that the combined system is now very far from any one of *its* pure energy states. This is not the result of a very strong mutual physical influence. It obtains prior to any physical change. It results from a slight dis-tuning of the proper modes by the perturbation. What were clear ' one tone' proper modes in the isolated systems no longer are in the combined ' dis-tuned' systems—*not nearly.* You have to re-shuffle them and combine (superpose them) *in your mind* in an intriguing fashion to find the proper modes of the combined system. I say ' in your mind '—there is, of course, no immediate physical re-shuffling ; you just state that your combined system is very far from rinding itself in one of its proper modes. And that is the very reason why, as time goes on, anything will happen at all, and why, in fact, even a weak interaction, given time, will produce substantial changes of the amplitudes. For it is a simple elementary and universally recognised statement of wave mechanics that an isolated system that vibrates exactly in one of its proper modes undergoes no change whatsoever.

This has, by the "way, a consequence worth mentioning. When we spoke just before of a weak interaction setting in between the previously isolated systems, the reader may very naturally have pictured to himself the two systems being first at great distance and then approaching each other and getting in contact. I avoided this description on purpose, because it would flatly contradict the assump-tion that the isolated systems were in pure energy states. *If so they cannot be said to approach each other.* To think of atoms and molecules *in pure energy states,* moving hither and thither, colliding and rebounding contradicts the fundamental concepts of the theory. Where anything *happens,* we are not facing pure energy states. So obviously we never are.

Let me return for a moment to our two micro-systems in weak interaction or, as I prefer to say, to a system consisting of two parts in slight coupling. The state of affairs is simply this : if this system as a whole settled down in one of its exact proper modes, this would *not* be a state which *the current view* interprets as indicating a definite partition of the total energy between the two parts—*not nearly;*

ARE THERE QUANTUM JUMPS'

I mean to say, it is not a question of slight fluctuation or uncertainty, but of many widely different partitions. If you abolished the coupling at this moment, the now isolated parts would vibrate, each of them, in a superposition of *widely* different proper modes.

Summarising • the current view, which privileges the ' sharp energy states,' is self-contradictory, anyhow in the language it uses (what people mean, when they say something else than they mean, is difficult to guess).   We found it self-contradictory in that it cannot be maintained for both the whole and the parts ; we are left to choose and to apply the privilege the way it is most convement.   We found a minor inconsistency in the apparently innocent statement that two systems (both of sharply defined energy) *approach* each other and collide.   This seems a little less obnoxious, because it can be evaded by saying : Oh well, we do not mean really quite sharp.   Some may consider this point a rather gratuitous nagging.   I wonder whether in actual collision problems it is entirely irrelevant.

(The concluding part of this article will be published in November.)

NOTE

Professor Michael Polanyi drew my attention to a mistake. My description of the Franck-Hertz experiment in the first paragraph of Section 4 is, to put it mildly, oversimplified.   The cathode ray beam is appreciably scattered in the vapour.   The two or more electronic frequencies that emerge could therefore hardly be separated by the simple transversal field method.   But any two of them can be separated by a potential barrier which the one can penetrate, while the other is turned back, being totally reflected.   Since this is also completely understood by de Broglie's wave equation, the main argument is not impaired.

E. S.

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