

Might perhaps Energy be a merely Statistical Concept? (*)

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Summary. — Arguments are given in favour of the opinion that the quantum-mechanical frequency, multiplied by Planck's constant, has for microscopic systems *not* the meaning of energy. It is suggested, that the concept of energy and its conservation, just like that of entropy and its increase, has merely a statistical meaning, the energy of a macroscopic system being the product of Planck's constant and a weighted average of the frequencies in question. The wide-spread attitude that the claim for an objective description of physical reality must be given up, is rejected on the ground that the so-called external world is built up exclusively of elements of the single minds, and is characterized as what is common to all, recognized by every healthy and sane person. Hence the demand for a non-subjective description is inevitable, of course without prejudice whether it be deterministic or otherwise.

1. — The prevailing opinion is to the contrary. It is claimed, that the conservation of energy and momentum holds for single collisions or similar events in all cases that have been observed. The earliest and indeed very difficult investigations were about the Compton effect. A vast number of single processes have since been fixed and analysed in the Wilson cloud chamber, in photographic emulsions etc. However, I believe that in all cases the kind of interpretation suffices that I put forward thirty years ago (1927) for the Compton effect, *viz.* reflexion of one progressive wave by another one; or to be quite accurate: the interference pattern formed by *one* wave and its reflected wave serves as a sort of (moving) Bragg crystal mirror for the *other*

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wave, and *vice-versa*. To this there is, by the way, a close analogy in the reflexion of monochromatic light by the waves of heat motion or by ultrasonic waves in a fluid (LÉON BRILLOUIN and others). It is not my objective to go into details about these things here. But allow me to mention: it is usually believed, that the current orthodox theory actually accounts for the « nice linear traces » observed in the Wilson chamber etc. I think this is a mistake, it does not.

The first to raise serious and well argued doubts as to the validity of the conservation laws on the small scale was FRANZ EXNER in his *Vorlesungen über die physikalischen Grundlagen der Naturwissenschaften* (Wien, Deuticke, 1919). This was six or seven years before the advent of quantum- or wave-mechanics. After the latter had been developed and adopted, it became extremely improbable that the conservation of energy retain its meaning as an exact and sharp law. For the concept of energy and its conservation stems from classical mechanics (GALILEO, NEWTON), namely from an integration constant (« constant of the motion ») which in that theory plays a fundamental part. However, the mathematical scheme and the mental image of classical mechanics have turned out to be only an approximation, that holds on a large scale but breaks down entirely in spatial dimensions of about the order of an Å. It cannot even approximately comprehend the details of the motion of elementary particles (supposing that this concept is at all going to survive in the new setting of ideas). That is why I consider it *prima facie* rather improbable, that the notion of energy conservation should hold good even in the domain where the theory from which it hails is no longer competent.

We are reminded of the Second Law, which according to *phenomenological* thermodynamics says that in an isolated system the entropy never decreases and as a rule increases. *Statistical* thermodynamics, by revealing an insight into the true nature of this theorem, at the same time annuls it as a strict law; and that in two ways. *First* it is truly invalidated for systems with a small number of degrees of freedom, and for any system in the neighbourhood of thermodynamic equilibrium; nay, in these cases it is hardly possible to offer a sound definition of entropy or to give it with sufficient precision. *Secondly*, and even more relevantly, the statistical theorem seems *prima facie* to imply a logical contradiction, because it makes bold to deduce from reversible models the unidirectional running down which is the gist of the Second Law. As everybody knows, this dangerous hitch can only be overcome by allowing the « arrow of time »—the direction from past to future—to be defined by the very law of increasing entropy itself. Thus we eventually hit on the problematics of the concept of time.

According to the new physics energy too has something to do with time, though not with time's arrow. There is an uncertainty relation stating, for simultaneous determinations of energy and of time, a lower boundary for the

product of their respective lacks of precision. Moreover some difficulties arise for the concept of an *isolated* system (whose energy might prove to be constant), not only because a system under observation is by principle not isolated, but also because the interaction with the general heat radiation can in actual fact never be excluded.

The said uncertainty relation is usually taken to mean that in principle an infinite time is required for finding out the exact value of the energy. It is difficult to see how «after» doing so we should still manage to ascertain that the value we have found does not change with time. In addition, within a comparatively short span of time, an appreciable interaction with the radiation is to be expected, and thus—if we keep to the idea of energy conservation—some change of the energy of the system under consideration. It may seem that in these remarks we are maliciously pushing things to extremes (which one ought never to do), while in actual fact the situation is not as bad as all that. Please wait, I hope to show that it is even worse.

In the old mechanics the energy was a function in phase-space (p_k, q_k). In the new mechanics this cannot be, since from the general uncertainty relations no point in phase space can be exactly determined. How does the quantum statistician meet this predicament? He writes out what is often called the energy-eigenvalue-equation (or time-independent Schrödinger equation, or most aptly the amplitude equation). It determines the eigenvalues of the frequency. These, multiplied by h , are declared to be the *only admissible* values of the energy; which means falling back into the foot steps of NIELS BOHR's pioneer work of 1913, duly hailed then and for ever, for the immense advancement of physics that it brought about, but now (1958) after all superseded for more than thirty years. Anyhow, this «energy spectrum» replaces the phase space of Boltzmann and Gibbs. The weight to allot to every «level» is easily made out from analogy with Liouville's theorem, *viz.* the same for every single eigenvalue, and for degenerate ones according to their multiplicity. Very satisfactory agreement with observations is obtained by this procedure, which might pass for impeccable, were it not in glaring contradiction to the foundations of quantum mechanics. This heretic statement and the following argument refers in the first place to systems of few degrees of freedom, when the multiplicities too are not very great; these are the cases of primary interest.

Then, since every privileged quantum level is associated with a volume h^f of phase space when single, or a small multiple thereof when degenerate (f being the number of pairs of canonically conjugate variables) it is easily seen that the levels are just so densely packed as to disallow one to distinguish unequivocally between neighbouring levels, on account of the uncertainty relations that hold between the pairs of conjugate variables. Hence it does not seem to me consistent to declare those levels as the only admissible ones, since in

doing so one tacitly admits anything between them. I am sure that if an advocate of the orthodox view cares to argue the case with me, the first thing he is going to tell me is, would I please have a look at a line spectrum and *see* that the levels are not blurred but very sharply distinguished and privileged. But this argument is based on the idea that an observed spectral frequency is emitted by single atoms jumping from a certain higher level to a certain lower level, each atom producing in this process a photon with energy equal to the difference of the two levels. This, of course, presupposes the detailed validity of the conservation law, which is just the point under discussion that I do not take for granted.

For macroscopic systems with a very large number of degrees of freedom the multiplicities may, and will as a rule, be very large numbers, so that our above argument breaks down. Still it is known that in this case the eigenvalues are so closely packed, that their discreteness is practically unobservable, whether you regard them as frequencies or as energy levels. They do have an important say in determining the statistical thermodynamics of the system in question. In this the customary relation between the frequency (of the whole macroscopic system!) and its energy must of course be admitted, but the relation may quite conceivably be itself only of a statistical nature. Anyhow the situation is not quite as simple as it might seem. The following discussion is mainly aimed at macroscopic systems.

Must we, in view of the thermodynamical application, regard an eigenfunction of a sharply determined «energy»-eigenvalue as representing a state in which our system has this sharply determined energy?—Certainly not. Why not?

The energy alone or together with a few other macroscopic parameters as volume etc., does not determine the state of the system uniquely, but at best the state of thermodynamic equilibrium that it is going to reach eventually when left to itself. In other words the system can harbour the same amount of energy in very many different ways, among which there are states of equilibrium, but also many far away from equilibrium, *e.g.* with considerable temperature gradients, arbitrary distributions of pressure, concentration, density etc. At first sight it may seem that this variety is fully accounted for by the high degree of multiplicity (degeneracy) of the eigenvalue in question; it ought to be possible to associate the eigenfunctions with the empirical states in such a way that each of the latter is represented by at least one of the former. But this is not so. A non-equilibrium state cannot be represented by an energy eigenfunction. For it is well known and easily seen that when the wave function depends on the time just by one imaginary exponential (one frequency) the system is «completely dead». No change takes place, nothing depends on time. The proposed association would therefore in every case reduce the system in question to the «sleeping beauty» of the fairy tale:

gradients of temperature or concentration frozen in, chemical reactions stopped midway, a falling brick hung midway in the open air by witchcraft, a radio wave emitted from London not reaching Cambridge etc.—including the irate chef de cuisine, whose hand remains brandished «for a hundred years», threatening to box the ears of the negligent lad, who is petrified in motionless fright, trying to ward off the blow.—But jokes apart, there is another more serious aspect of the same thing, namely that, according to the accepted view an absolutely precise measurement of total energy of a physical system would bring the system into the state of thermodynamic equilibrium or maximum entropy, however far away from this state it may have been when the measuring device was applied. Just imagine your walking into a pharmacist's shop and asking him to make out your *weight* quite exactly. Could he comply with your request, which of course he cannot, he would become guilty of murder.

Thermodynamic equilibrium is only an abstraction, a limiting case that in actual fact is never met with. No system that we observe has a sharply determined energy value, nay we must not even admit this in the mental images we invent in order to describe what is *going on*. For nothing that takes part in what is *going on* has a well defined energy. Is that not rather in favour of the view I am advocating, that energy, just like entropy, is a statistical concept? Any display of physical events, while to the classical view it was taking place within or between systems of well defined energies, is quantummechanically represented by state functions that do *not* depend on time just by one imaginary exponential factor with one single frequency, but by a superposition of several, as a rule a great many such terms, covering a discrete or continuous range of frequencies, though it may be restricted to a narrow domain of the spectrum. Only in this way can one obtain a representation of something *happening*, an evolution in time. (The situation is mathematically analogous to the well known, not to say ill-famed (because wrongly used), wave parcel.) These considerations fortify my conviction that for small systems—with few degrees of freedom—one ought not to consider the product of Planck's constant and the frequency as meaning a definite amount of energy, while for macroscopic systems this relation is, of course, indispensable for the theory of thermodynamics and ought itself to be given a statistical foundation.

The theoretical facts adduced in the preceding paragraphs have been familiar for thirty years. Let me still recall a theoretical discovery that at the time aroused keen interest and is closely related to the same order of ideas. In the case of some small and comparatively simple systems the states which the classical physicist without hesitation deems states of equilibrium, are from the quantummechanical point of view not equilibria. Take for instance the molecule of ammonia (NH_3). It is pictured as an equilateral pyramid with the nitrogen atom at the apex (N), the basis being an equilateral triangle

formed by the three hydrogens. But since the nitrogen might as well be situated on the other side of the said triangle at the mirror point (say at N') one might expect that in the quantummechanical description this gives rise to a twofold degeneracy, two eigenfunctions belonging to the same eigenvalue. However from very general and very fundamental considerations this is not so. Neither the position at N , nor that at N' is associated with an eigenfunction. There are indeed two of them, but their eigenvalues differ slightly, moreover, both are quite «impartial» as regards the positions N and N' , they leave this alternative in abeyance. The essential difference between them is that one is symmetric with respect to the plane of the three hydrogens, the other anti-symmetric. Suitable linear aggregates of the two functions represent the N -configuration or the N' -configuration respectively, according as the phase difference is adjusted. Naturally none of them is permanent, since the difference in phase is subject to slow secular change on account of the slightly different frequencies of the two proper modes.

Of even greater interest, is the case when the two configurations have not, as with NH_3 , exactly the same physical properties (since they differ only by orientation), but can be distinguished by observation, though from the point of view of classical physics we should have to allot them exactly the same energy. I am alluding to stereoisomeres, *i.e.* molecules that are mirror images of one another, but cannot be made to coincide by a mere movement in space. In every other respect the state of affairs is exactly the same. Neither the R -situation nor the L -situation is represented by a genuine eigenfunction. Both the latter are «razemic», that is impartial with regard to R and L , but again in two different manners (and therefore slightly out of tune with one another), one being symmetric, the other antisymmetric with respect to the operation of space reflexion. Since many stereoisomeres can be kept for almost indefinitely long periods without razemisation, the frequency differences must in these cases be exceedingly small. The remarkable thing is that here we meet with states that are ostensible equilibria, but «sub specie aeternitatis» they are not, because they cannot be represented by one eigenfunction with one definite frequency.

2. – Let me join here some remarks which, though not covered by the title of this paper, have to do with our enquiry. The opponents of the Copenhagen view about *complementarity*, the relation between *object* and *subject* etc., are regularly reproached with wrongly clamouring for a picture of reality, of the real world around us, without reference to the observer, the subject of cognizance. We are blamed for shutting our eyes to the fact (allegedly only discovered in this century by quantum physicists) that the description of objective reality is impossible, because our knowledge about things is based upon our interaction with them, which is essentially mutual, that is to say not only

do the objects make impressions on us but also we on them, and that in an uncontrollable fashion etc. etc.

I cannot share this attitude (which I hope to have summarized impartially), indeed this kind of discrimination between the ego and the world outside appears to me to be based on an epistemology out of date for some time. Naturally our urge to form a picture, valid for all of us, of the world in space and time—which includes of course our own bodies—must not be framed ontologically; this would be rather naive science and ignore philosophical achievements very much older than quantum mechanics. From Democritus to Bertrand Russell there have been thinkers who became aware of the obvious fact that our sensible, perceiving, feeling, thinking ego, and the so called external world consist of the same elements, only comprehended in different arrangements. The elements in themselves (I am following Bertrand Russell) may not be called psychic or physical, mental or material; only after arranging themselves in various patterns do they in conventional language acquire this or that characteristic. In any case the so called external world is built up exclusively of constituents of the ego. It is characterized as what is common to all, recognized by every healthy and sane person. That is what distinguishes it from dreams and hallucinations, also from joy or pain, tooth-ache, sorrow, depression etc.

This « being-shared-by-everybody », this community (1) is the one and only hall-mark of physical reality. Not unfrequently do we resort to this criterion in daily life: I hear a humming; do you hear it too? Is it perhaps the stove or something out in the street or is it only in my ear? Well it seems to me that the Kopenhagen epistemology does not acknowledge this criterion, pays no attention to it. In being satisfied with describing the material world for one observer (while for another one a different description may hold) it leads to the physics of solipsism. This bears even on the linguistic expression used in the analysis of experiment; instead of « we find » or « we measure » (plural) the singular is usually preferred: « I find » or « the observer states ». This is not astonishing. Indeed the one lightquant which *e.g.* in the gamma-ray microscope is supposed to reveal to *me* the place of an electron hardly suffices for poor me, let alone for others.

But jokes apart, I shall not waste the time by tritely ridiculing the attitude that the state-vector (or wave function) undergoes an abrupt change, when « I » choose to inspect a registering tape. (Another person does not inspect it, hence for him no change occurs.) The orthodox school wards off such insulting smiles by calling us to order: would we *at last* take notice of

(1) This is in itself the most remarkable trait of our general experience, considering the complete privacy of an individual's sensations, the absolute seclusion of every mind from every other mind.

the fact that according to them the wave function does not indicate the state of the physical object but its relation to the subject; this relation depends on the knowledge the subject has acquired, which may differ for different subjects, and so must the wave function.—Very well, but this situation is by no means novel. Think of entropy. The entropy of, let me say, a given body of gas has a certain value for him who only knows the energy and the volume—he may take for it the logarithm of the phase volume up to that energy; the entropy of the same body has a different, indeed a smaller value for one who is informed of the inhomogeneous distribution of density and temperature in the gas. Yet I do not think that it has ever occurred to anyone to declare that entropy is *not* a property of a physical system *per se*, but only expresses « my » knowledge about that system. In a certain way of course one may, if one likes, say this about *all* the numerical results of physical measurements, either about all of them or none; however this is an old yarn, an entirely irrelevant matter of taste.

We do feel the yearning for a complete description of the material world in space and time, and we consider far from proven, that this aim cannot be reached. This does not mean that we wish to outwit the uncertainty relation. Yet it ought to be possible, so we believe, to form in our mind of the physical object an idea (*Vorstellung*) that contains in some way everything that *could be* observed in some way or other by any observer, and not only the record of what *has been* observed simultaneously in a particular case. I mean precisely what someone (was it not Ernst Mach?) has called the completion of facts in thought (*Ergänzung der Tatsachen in Gedanken*). We prefer to grasp the shape of a solid by visualizing it in three-dimensional space instead of by a set of perspective drawings, even though the eye can at any moment only perceive one perspective view. Everyday life is based every minute on « completion in thought » since we rely on the continued existence of objects while they are not observed by anyone; *e.g.* we surmise the nocturnal preservation of our portfolio and its contents, locked up in a drawer at night and taken out in the morning.

It is not now the question whether the wave function (or state vector) yields the desired complete description. I remember reading the other day the very apt statement that with a physical theory one ought never to ask whether it would still have to undergo some change, but in what direction. My point is that at the present stage and as long as the state vector plays the role it does it must be taken to represent « the real world in space and time », it ought not to be sublimed into a probability function for the purpose of making forecasts, depending therefore on the momentary state of our knowledge and changing abruptly when somebody (who?) cares to inspect a photograph or a registering tape; it must not be regarded as « hovering in empty space » between subject and object; the question what *is* now the wave

function (meaning, what is now the actual state of the physical system?) must be regarded as meaningful, even though it can hardly ever be answered exhaustively.

In the first part of this paper I have given reasons for doubting, in the case of energy, the current association between wave function and observation. Here I feel induced to contradict emphatically an opinion that Professor L. ROSENFELD ⁽²⁾ has recently uttered in a meeting at Bristol, to the effect that a mathematically fully developed, good and self-consistent physical theory carries its interpretation in itself, there can be no question of changing the latter, of shuffling about the concepts and formulae.—This does not make much sense to me. I recall a brief paradoxical remark that Einstein made, half in joke, while we were strolling *Unter den Linden*: Of course every theory is true provided that you suitably associate its symbols with observed quantities.

Rosenfeld's statement is a danger signal. What is at stake today—far more important than the eventual decision for this or that view—is the peril of a progressive narrowing of our field of vision, a mental glaucoma as it were.

⁽²⁾ *Proc. of the Ninth Symposium of the Colston Research Society, April 1957.*

RIASSUNTO (*)

Si danno argomenti in favore dell'opinione che in meccanica quantistica la frequenza moltiplicata per la costante di Planck *non* ha per i sistemi microscopici il significato di energia. Si esprime l'opinione che i concetti di energia e della sua conservazione, al pari di quelli di entropia e del suo aumento, hanno solo un significato statistico, l'energia di un sistema macroscopico essendo il prodotto della costante di Planck per una media ponderata delle frequenze in questione. L'opinione diffusa che il proposito di dare una descrizione obiettiva della realtà fisica debba essere abbandonato è respinta basandosi sul fatto che il cosiddetto mondo esterno è costituito soltanto di elementi delle singole menti ed è caratterizzato come ciò che è comune a tutti, e riconosciuto da ogni persona sana e ragionevole. Donde è inevitabile la richiesta di una descrizione non soggettiva, naturalmente senza pregiudizio del fatto che essa sia deterministica o di altra natura.

(*) *Traduzione a cura della Redazione.*