QUANTA IN CONTEXT

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The context of a scientific theory can be epistemological and methodological. Or it car be metaphysical, relating to the intellectual framework within which we cast it. Or it can be intertheoretical, both synchronically and diachronically. My concern here will be mainly diachronical -- the historical context of quantum theory, what is required of it vis-a-vis that context and how well it fulfills this requirement. But I shall come to this only at the later part of this essay. I shall have to clear the 3round by discussing the epistemic and metaphysical contexts first.

1. Epistemic

The claims for the foundation of a scientific theory are made on the basis of experience or intuition or both. Not only is there no foundation for any theory; also looking at anything as possible foundation distorts it.

When experience is looked at as foundation, only the success of a theory is considered; failure is overlooked. This is impossible to look at a theory in its historical context since the initial success of a theory is all too often the overcoming of the failure of its predecessor. It is often claimed that the predecessor was never meant to explain or predict data it failed to describe correctly. This is often historically false and heuristically confusing since the problem a new theory comes to solve often stems from false predictions based on old theories. To claim that old theories are not refuted since they are still in use is to confuse theory with practice. To claim that they are not refutable since they are only tools of predictions is to make science a part of technology devoid of any intellectual value, which is absurd. To claim that the old theory is still asserted with qualifications may be true, yet the knowledge of the qualifications when available, is the corollary of the new theory. This point will prove crucial in the later part of this essay, so let me elaborate with an example. Newtonian mechanics is putatively considered as still true within the limits of small velocities and weak gravitation if not tested too accurately. This is not true, since, however small the velocity of a nucleus is, if it disintegrates it violates both Newtonian conservation of mass and Newtonian conservation of energy, and the violation may be of as large an order of magnitude as that of a nuclear explosion. Nevertheless, suppose we have managed to specify the qualifications within which Newtonian mechanics is held to be true. Not only did the Newtonians deny any such qualification -- they explicitly denied all qualifications! -- they could not possibly know all those which we make, since this knowledge is part and parcel of later theories. Often the success of stating a qualification of a theory opens the road to its replacement. For example, after special relativity was established it became clear that Newtonian gravity that acts at a distance had to be replaced with a gravity that is propagated at the speed of light at most, thus showing the road to general relativity.

The same holds for intuition: the attempt to see it as foundations directs our attention to success, not to failure. Thus, those who attempted to found Newtonian mechanics on intuition had to declare it perfectly intuitive despite the Historical fact that when it appeared it was deemed highly counter-intuitive, as noticed by Imre Lakatos in his classic paper on infinite regress in the foundations of mathematics.

What is intuitive and what is not is hard to decide. We all agree that pre-Copernican astronomy is so very intuitive that even the most advanced astronomers still use it when they permit the sun to rise in the east and set in the west. It is, however, quite unproblematic because the newer theories explain the success of older theories, both empirical and intuitive. For many people the Newtonian replacement of Galilean gravity with the interaction of the falling apple with the earth is counter-intuitive. Consequently there is the opinion extant that those who find Newtonianism intuitive do so out of sheer habit. This is a typical foundationist attitude: either intuition is genuine or a mere habit and so fake. In truth, as Lakatos has indicated, we educate our intuitions.

The education of our intuitions need not progress on a unique path. Newton has suggested one possibility: extend Galilean gravity from the falling apple to the noon: make the moon fall. Make then the moon interact with the earth. Transfer then the interaction to the apple. Newton has also suggested that this is how he developed his own ideas; which may indeed be true. No doubt, the intuitions of discoverers are more developed than those of their contemporaries, and grow faster. But they are no guarantee, no proper foundations.

Niels Bohr's central contention was that classical physics is intuitive but not quantum mechanics. He concluded that classical physics is here to stay -- which, in the sense in which even pre-Copernican astronomy is here to stay, surely is true. He raised the question, how can classical and quantum theories coexist? The answer should be, by having quantum physics explain the success of classical physics. Barring this possibility, we may wish to have a theory that should explain the success of both classical and quantum physics. Bohr's answer was his principle of complementarity, which differs from the above two alternatives. Hence, whatever precisely it says, it will not be endorsed here. (See note 5 below.)

To conclude, both intuition and experiment are at times successful, at times they fail. Progress is achieved when a new step explains the success of the old and avoids its failures. This, however, need not always be so. Newtonian mechanics explained the success of both Kepler's theory and Galileo's. The special theory of relativity failed to explain the success of Newtonian gravity and so had to be replaced – by the general theory of relativity, as it turned out.

2. <u>Metaphysical</u>

By metaphysics I mean the general presuppositions, the intellectual framework, within which a scientific theory is or should he couched. When discussing quanta two interconnected metaphysical points are raised at once: realism and determinism. Einstein endorsed both; Bohr rejected both. It makes little sense to endorse determinism and reject realism, but the other way round is possible and proposed by Karl Popper, Alfred Landé, Mario Bunge, and many others. It will be endorsed here.

Realism is proposed here as naive, not in the sense of naive realism (that declares true all careful observations) but as the proposal to take scientific theories fairly literally. Also it may be deemed as a desideratum, as the demand of any theory to be an attempt to describe reality. Finally it may be deemed as the demand that science be truly explanatory. All this may prove excessive: we may fail in our attempt to describe reality, to offer true explanations, etc. The way to avoid the excess is to remember that what we want and what we get. are different things. We than can view Daltonian atomism, for example, realistically, and assess its success and failure, and explain them both by the high but not absolute stability of the nucleus. Tentative realism thus permits pictures of science that are more historically accurate, more heuristically satisfying, more dialectical and more logically coherent. Tentative realism, then, is the same as the attempt. to view a theory in its historical context.

Determinism was, for Einstein, realism pushed all the way: the attempt to describe and explain everything is the attempt to have an all-embracing deterministic theory. This was his grand argument for determinism. He saw no need to elaborate on it. Many have attributed to him a deterministic scientific theory rather than a deterministic metaphysics and program. In his famous "Replies to Criticisms" he protested: he had no scientific theory, he stressed, only a program toward it. Those who did not see the difference he could not argue with.

One who did see the diffbetween having a scientific theory and the program to develop one was Wolfgang Pauli. The return to determinism, he said in a prophetic remark, was neither possible nor desirable.[1] He did not elaborate. The following elaboration is due to Alfred Landé and Karl Popper mainly.

The alleged determinism of classical mechanics was based on the supposition that particle dynamics fully determine trajectories out of any permissible initial conditions on any permissible equations of force and motion. Statistics, then, was deemed as rooted in ignorance or nescience. Einstein endorsed all this. He was doubtlessly in error: classical particle theory could not avoid singularities or discontinuities, and trying to handle these sooner or later led to quantum mechanics. History aside, ignorance cannot explain facts like the second law of thermodynamics. What Einstein assumed, of course, is not that ignorance gives us laws of distribution, but that we postulate laws of distribution in ignorance of the initial conditions of the system which include all details distributed.

Yet even if we count heads and observe the distribution, we cannot yet explain its stability without postulating it or some other stable distribution. The variational principle on probabilities says, no matter in what distribution a system is, it will get to the equilibrium distribution in the shortest way. This is postulated as a law of nature.

Suppose that we knew al. systems and in all desired detail and predicted them all to move along classical trajectories toward equilibrium. Would that make statistics redundant? Certainly not; it would make it a general fact, a law of nature. Hence statistics is not eliminable by knowledge. Nescience has nothing to do with it.

We have here a pressing question: the systems that are subject to strict particle mechanics and also obey laws of distribution of statistical mechanics, are they not over-determined? If they are not over-determined then, perhaps, the laws of distribution can be deduced from the equations of particle mechanics. Even so, the laws will be deduced for each system separately. We could not possibly deduce them for all systems, since it is not deducible from general particle mechanics. Therefore it needs an explanation which does not belong to particle mechanics. The fact of the logical independence of thermodynamics -- and hence of statistical mechanics -- from particle mechanics was discovered by Maxwell. He said, for thermodynamics to belong to particle mechanics, it should follow from a Hamiltonian. But Hamiltonians are time-reversible and the second law of thermodynamics is not. A more pictorial proof of the independence is Maxwell's demon, who violates thermodynamics without violating particle mechanics. A number of authors, especially Brillouin, have tried to outlaw Maxwell's demon, but he has already done his duty. One can argue against all this by saying, the systems that are permissible by general particle mechanics but not by statistical mechanics, e. g., one of particles all moving in parallel to and fro between two perfect mirrors, are of the measure zero. This is true, yet not deducible from Particle mechanics. To show this all we need is to view the improbable systems as models to establish independence in the classical Hilbertian way.

Thus, perfect knowledge of all the initial conditions (and computations) of all the systems in the universe will not yield the laws of statistical mechanics.

The question arises, at once, is not a system with both particle mechanics and statistical mechanics over-determined? Most likely it is. Does this make the system inconsistent? We do not know. Perhaps it simply excludes certain sets of initial conditions (like the above model of the two mirrors), perhaps it excludes full determinism in particle dynamics -- an exclusion which can be blamed on multiple collisions, discontinuities in elastic collisions of inelastic bodies (i. e., energy and momentum conserving collisions between absolutely rigid bodies), and other imperfections of classical particle dynamics.

My discussion of classical dynamics is not motivated by a desire to grant it any special position, intellectual or physical. I was questioning Einstein's claim that an attempt at a complete description and explanation of nature has to be deterministic. Perhaps so. Perhaps, however, far from having to be deterministic it simply cannot be.

So much for realism and determinism. Metaphysics also concerns relations between scientific theories, since we may be looking for a framework to accommodate diverse, coexistent, yet very different ones. I will not enter this discussion here: I have done so elsewhere. But at least one more topic is traditionally within the domain of metaphysics, the principles of the theory of the ultimate structure of matter. Evidently both the bootstrap theory and chromodynamics pertain to this topic. I will not enter this topic except for a brief casual remarks, and from want of proficiency.

3. Quantum mechanical

The two preceding discussions are preparatory to the discussion of the postion quantum theory has vis-a-vis its predecessors. To complete the preparation we may wart to present a coherent picture of quantum theory itself.

When we talk of quantum mechanics, then, what exactly do we have in mind? It is three quarters of a century since quantum mechanics was inaugurated, half a century since the debate about it began, and a quarter of a century since the end of the contributions of and debates between Bohr and Einstein. The debate was fluid, in content and even in reference: there was no canonical version of quantum theory. This did not trouble Bohr. He felt that as long as a theory includes the Heisenberg principle of uncertainty then what he had to say in this discussion of his own principle of complementarity stood fast. This did not trouble Einstein either. He liked fluidity since it stimulates heuristic. Canonical forms, he felt, are reached at the end of a road, perhaps after ages, he noticed, as was the case with Euclidean geometry. Without insisting on canonical forms, however, one may want to reach a narrowing down of the reference, since the very existence of variants, especially in time series, presents changing challenges to analysis.

No participant in the debate makes much of the absence of a canonical definitive version of quantum mechanics but the absence still is something adumbrated in the literature, perhaps as a serious problem, at least as an added technical difficulty. Nearest comes, I feel, Carl Friedrich von Weizsäcker, who is, by the way, as establishment as they come, these days. His essay "Probability and Quantum Mechanics", in the British Journal of the Philosophy of Science of 1973, presents as a problem for discussion the problem, what in quantum mechanics is meant as a universal theory, what as specific to quanta? Clearly, the logic, the mathematics, the probability, all these are general. "Bohr's concept of complementarity was never understood", he says (p. 323), "because it was misinterpreted as a generalization of a particular empirical concept of physics while Bohr intended it to indicate a universal structure of all human experience which could be particularly well exemplified only in quantum theory." Similarly, he says of quantum logic (loc. cit.), that though its discovery "was induced by experience", once it was discovered "it could be understood without reference to experience". Let me overlook the unfortunate foundationist expression "induced from experience" as well as the historical fact that the principle of complementarity and quantum logic came to overcome difficulties within quantum mechanics. My aim is to reproduce the problem-situation as presented by Weizsäcker: wanting to distinguish within quantum theory what is specific to it and what not he found the need to be specific about the theory: the desire to be at times context-independent made him want to specify the context exactly first.

Weizsäcker chooses as his context second quantization. It, too, was traditionally misconstrued, he notices: "it was never quite clear", he tells us (p.334), "whatthe iteration of the quantization process really meant." It is, he adds, quite generally, "a process of ensemble building according to the peculiar rules of probability that are characteristic of quantum theory. And this is exactly the thesis of the present paper: quantum theory is nothing but a general 'theory of probability, i. e., of expectation values of relative frequencies in ensembles." This last sentence is crucial.

There is a program here, and it is executed by Weizsäcker at once in two quick steps. First the formulation of quantum theory in general, and second the manifestation of its character as a theory of expectation values of relative frequencies. The formulation is Feynman's formulation of quantum field theory, so called. I think things can be made more specific: not merely Feynman's theory, but in the presentation of Julian Schwinger as it is sketched, say, in his Particles and Sources of 1969. As Schwinger explains in his preface there, his formulation overcomes traditional difficulties and is more parsimonious in its assumptions; he calls it phenomenological in the sense that it avoids speculations not intrinsic to the more formal part of the theory. It is not phenomenological in any sense used in philosophy; for example it employs Feynman's virtual particles. [2]

The second step, then, exhibiting the phenomenological part-. of the theory as that of distributions of expected values, is also a matter of a careful development of the formal theory and also already executed by Schwinger. What Weizsäcker adds is precisely what we are looking for: the inter-theoretical context: classical mechanics, he says, is the limiting and special case of quantum mechanics. It obtains for classical ensembles when the quantum phases are not crucial so that quantum probabilities can be replaced by classical probabilities. And it obtains for classical single particles when many quantum states come close enough to look like a single and highly probable state. This is Weizsäcker's point, his addition to the picture, and the reason I refer to his paper here.

Before coming to examine the truth or falsity of Weizsäcker's point we may want to know more about the content and the meaning of the diverse parts of the theory. It is not a question of sympathies but of reading. For, some of the physicists who share Weizsäcker's reading but not his sympathies, conclude that anyone who is not pleased with the present situation must get out: of Hilbert's space. A few people undertook such adventurous journeys, including heretics like Mendel Sachs and orthodox like Leon Rosenfeld, but this is a different matter. The matter at hand is quantum theory as understood in the reading mentioned here, and the classical difficulties it encountered, the so-called quantum paradoxes. What has happened to them? What can one expect from a statistical reading of quantum mechanics about the quantum paradoxes?

The less one can expect from the theory, the less paradoxical it should seem, and quite a priori. For example, in the case of the two slit experiment, this supports (a priori) authors like Niels Bohr and P. A. M. Dirac, who have declared the crucial central bothersome question quite meaningless, on the simple ground that the question is quite unrelated to the job at hand. The question, we remember, is, through which slit did the particle go, aid how did it decide that the other slit is open or closed, as the case may be? To begin with, does quantum theory cover the two slit experiment? Let us take this question a bit slowly. Do we have a quantum equation for the experiment whose solution fits the observed facts of the matter? Of course, not. But, quantum physicists hasten to add, this is only small technicality. Maybe; why are they so convinced that in principle there is no problem here? Which principle applies here? The answer is very simple and in two steps. First, the classical equation, the variation principle, classically applies just to a case like this. And when we take it as the limiting case of some unknown quantum equation such that should take the phases as probabilities and replace them with some vector out of Hilbert space, then, without deciding which equation it be, the facts as observed make sense and only the facts unobserved become paradoxical![3] Query: can we have a quantum theory that should offer expected values of quanta passing through this slit of that? Yes. Could these be compared with observations? Yes. Could the observations of the slits and the observations of the screen be superimposed? Yes, but this would not have <u>any</u> specific physical meaning. For this we can take the superposed calculated results -- meaning, not really but presumably calculated, we may remember -- and compare them with superposed observed results. Now, says Bohr, there is no such physical superposition to compare with any theoretical superposition of this sort. This, in essence, is Bohr's reply to Einstein, Podolsky and Rosen. It always strikes me as odd that Bohr has elucidated his reply to the old objection of the two slit experiment by discussing a new one. But these things happen.

Why should it be presumed impossible to observe a particle twice without interfering with its business, which is the business of contributing to an interference pattern? The answer was, this will violate the Heisenberg principle. This answer may be true, but it does not belong to quantum mechanics: Bohr and Dirac declared the question outside quantum mechanics, and all those who read it statistically must do so too. And if the question does not belong, then the answer to it does not belong either. And the Heisenberg formula, being statistical, cannot stop us from observing one particle's trajectory, even if Nature does stop us from doing so. But suppose we cannot observe a particle's trajectory. Why can we not envisage it? After all, we cannot see a classical wave front or wave train, yet we can and do envisage it. The quantum paradoxes derive, as Bohr was the one to tirelessly emphasize, from our wish to envisage electrons either as particles or as waves, which can be done only partially and complementarily. No one contests this point of Bohr. The question is, what is the proper quantum description or envision of the electron's trajectory? Bohr says, it is complementary. Others say, it does not exist. Here is the dispute. If quantum theory is statistical and the quantum paradoxes not, then the two do not impinge on each other, and complementarity is redundant at best.

Quantum mechanics has no trajectories of individual electrons. This is by no means paradoxical or disastrous, nor even unusual. New theories often replace answers given by older theories, but often they only reject old answers without replacing them. Spontaneous emissions, quantum jumps, relativistic simultaneity, even action-at-a-distance, all these leave <u>gaps</u> in previously filled cases. The gaps are at times permitted to be filled by additional assumption, at times not – e. g., yes in the case of action-at-a-distance and no in the case of simultaneity. In 1927 Heisenberg tried to show the impossibility of quantum trajectory by the use of his microscope thought experiment. And in 1928 Bohr added that what can never be empirically decided should be left outside science for good. Yet Heisenberg's microscope thought experiment employed not quantum electrons but the arbitrary mixture of classical wave and particle presentations of it. And Bohr confused what is outside the domain of quantum mechanics with what is outside science at large -- a confusion known in the philosophical jargon as hypostatization. Clearly von Neumann tried to prove the impossibility of hidden variables in 1932 because he was not fully satisfied with Heisenberg's and Bohr's discussions. Yet hidden variables proved possible even if admittedly hideous. Meanwhile, heavy beam microscopes either had the Heisenberg-Bohr claim that we can never see atoms refuted, or shown it too vague for a proper debate. With this, much of the force of Bohr's thought experiments was gone.[4]

The question is, then, what is the proper domain of the quantum paradoxes? Whom should they worry and why? But first we may better ask, do they exist at all, and what makes them problematic? This, I contend, is a problem for the inter-theoretical considerations.

4. <u>Inter-theoretical</u>

Let me explain briefly how a theory should relate to its predecessor. I speak of the general requirement, due to Einstein and well formulated by Popper, that a new theory replacing a once-successful theory should yield its predecessor as an approximation or as a special case and preferably both. The domain of approximation is that where the new theory refines results of the old theory. The domain where the old is a special case only is that where the new theory introduces new parameters, and new domains of facts to explain and predict. Once a theory does that, a crucial experiment between the old and the new takes place and if the new succeeds it is here to stay even though we expect it to be superseded.

The previous paragraph included claims that will be taken as self-understood by some people, such as Einstein or Schrödinger, Popper or Bunge, and as obviously unsatisfactory and misleading at best by others, such as Poincaré and Duhem, Heisenberg, Weisskopf, perhaps also Bohr.[5] Many say there is no crucial experiment in science; yet the presence of crucial experiments is an obvious fact that invites no discussion here. It is discussed in other works of mine and I always find the discussion on whether crucial experiments exist rather tedious: what else was Eddington's observation? Let me repeat: the idea is of a requirement from a theory; not a matter of historical fact. This means that when the theory in question does not fulfill the requirement we may well be dissatisfied with it. How then, if at all, does quantum mechanics conform to this requirement?

Historically, relating quanta to classical matters always was a troublesome affair. In 1900 Planck postulated that when his constant was viewed as zero classical theory came back intact. Einstein in 1905 saw this as defective; he offered a different rule of approximation: when a field is very weak it appears quantized, but when strong enough it is classical. In 1913 Bohr came with a different rule: distances from the nucleus that are small enough (compared with Bohr's radius) are the domain of quantum orbits; larger distances permit electrons to behave classically. In 1918 his celebrated correspondence principle closed the gap a bit: even for quantum orbits classical calculations for the intensities of spectral lines may be good approximations. It was clearly all in a fluid state. In 1924 Bohr, Kramers, and Slater published a paper offering almost nothing but a new revolutionary rule of approximation: energy is conserved only statistically. And they referred to Einstein as their inspiration. (The rule was refuted at once in one of the best known experiments. The importance of this episode for the history of physics is thus very great and is constantly underestimated because foundationist bias[6] make us overlook refuted ideas.) With the advent of classical wave and matrix mechanics things became less clear, and even Schrödinger's proof of their overlap did not help. Soon Dirac showed that quantum fields and double quantization were the same. The situation became increasingly exciting and people awaited a picture to emerge. After about fifty years it is still open enough to be debated.

Perhaps a word of caution is in order here. The question, what are the rules of approximation between quanta and classic theories are fundamentally different from the question, by what rules classic equation become quantized. The question how to quantize refers to future new theories and their expected explanatory function. The question of approximation pertains to the old theories and their explanatory success. The question was, as with many cases, publicly suppressed yet privately studied. One root of the trouble is the lack of a clear identification of the canonical theory. There are many specific theories with quantum characteristics, yielding bits and pieces of classical theories as approximations. Now that a canonical version seems to be emerging, the question can be repeated. Taking quantum mechanics to be quantum field theory (including a variant of Dirac's equation, quantum statistics, weak and strong interaction), how much of the success of the classical theories is explained? There is no need to go into the failures of classical theories here, especially since there is no question about the superiority of quantum mechanics each time it competes with classical mechanics. The question is not, what is included, but rather, is anything left out?

Quantum field theory is a theory of both scatter and interactions by creation and annihilation of particles and virtual particles. It includes the theory of electrons in their orbits, and of the propagation of free particles, including photons and electrons. As to scatter, clearly it is a peculiarly quantum mechanical effect, and, considering Compton's effect, it is naturally relativistic -- it belongs to quantum field theory. The only scatter that may be left out -- I cannot say -- is the Rayleigh dispersion so-called, which is classical, and so should follow from a quantum formula. The picture is much more problematic with the accelerated propagation of classical particles in accord with Lorentz force. Here we have classical trajectories of electrons and if there is no quantum trajectory at all, precise or not, then the classical electron trajectory is not covered by quantum theory. Yet quantum theoreticians have no hesitance in using Lorentz force, in J. J. Thomson or cathode-ray tubes such as oscillographs and boob tubes, in accelerators, such as cyclotrones and linear accelerators, in tracking down charged particles in Wilson chambers and in bubble chambers, and, most disturbingly, in plasma physics in general.

We may remember that Weizsäcker has declared classical particle states to be derivative of quantum statistics. Does this include trajectories? if yes, how? Nor is it hard to see the connection between this and the quantum paradoxes: the J. J. Thomson electron has a path, and the electron's path leads to the quantum paradoxes. Heisenberg, in his debate on his uncertainty principle, tried to soften the path by making it imprecise. This very imprecision may very well do the job of covering the J. J. Thomson path as a good approximation. Yet it is precisely this kind of Heisenbergian fuzzy path that is hit by the Einstein, Podolsky, and Rosen paradox. If quantum theory is statistical it is not hit by the paradox and it fails to account for the J. J. Thomson path. If it does account for it then it grants the electron its path and then it is possibly hit by that special paradox. It is a clear choice; and both options seem unpleasant.

This seems to me to be the best way to present the quantum paradoxes, in the abstract and pertaining to approximation rules. Thus, the two slit experiment is not a matter to envisage intuitively but the claim that there is a possible experimental arrangement with conflicting results: by Einstein's approximation rule (weak fields are quantum mechanical, strong ones are classical) it is quantal but by Bohr's rule (small distances are quantal, large distances classical) it is classical. The Heisenberg microscope, too, uses two different approximations, the one to wave representation the one to particle representation, of the same case. Schwinger claims, perhaps, that slower electrons follow classical orbits, but his presentation admittedly fails because of the classical difficulty: the accelerated electron should radiate. In classical considerations of an electron we overlook the problem of radiation. In quantum field theory, where emission and absorption are the means of describing interactions, the situation seems to me to be seriously troublesome.[7] This is another way to say, what could be said with no reference to any special version of quantum mechanics: returning to Weizsäcker's point, we can say, there is no difficulty to imagine a quantum wave looking fairly much like a classical wave in limiting cases or a cluster of quantum s looking like a classical particle state in limiting cases. This will not do: we have a classical path but not a cluster of quantum paths. Why? Can these be supplemented?

I shall turn to this question presently. Let me notice, however, that quantum theory does become much easier to comprehend -- even to intuit -- and much simpler, once we reject both the wave and the particle presentation. This does not mean that the two presentations are taboo: even the pre-Copernican sunrise is not taboo, we remember, much less classical waves and particles. But the quantum thing, the quanton, as Bunge calls it, is simply neither.

And thus the proposal of the present essay is to reverse the concern about relations between classical and quantum mechanics: a part of the old theory which covers a part of the new theory may thereby win survival value, but this is unproblematic. The problematic is whether the new theory covers all the valuable parts of the old theory, so that the replacement incurs no loss. Yet to this effect we should be able to consider the new theory on its own. And here is another problem lurking: what do the Planck and de Broglie formulas say? Seemingly they are translation rules between the wave presentation and the particle presentation. But admitting this takes us back to the wave-particle duality of the old quantum theory, and to the demand that the (semi-) classical theories cover quantum theory and to the recognition of the partiality of this coverage and the complementarity principle as an attempt to make the best of a bad job. What else then can the Planck and de Broglie formulas mean?

De Broglie insisted all his life: these formulas quantize wave packets. Schrödinger insisted, they are derivative from quantum resonance laws which say that energy exchanges are in whole multiples of Planck's constant. Schrödinger could not account for the seeming localization of the quanton and was always bothered by it. He tended to accept de Broglie's view of it as a wave packet. But the wave packet should be stable. De Broglie hoped to discover stable wave packets, perhaps as solutions to nonlinear equations, preferably derived from equations linear in the space of general relativity and made nonlinear by transfer to a localized space of special relativity. And, no doubt, had there been any equations offering as solutions in addition to the current solutions, also wave packets, some stable some not, in agreement with known facts, what a joy that would be. Moreover, we do have the phonon, which is a bundle of elastic energy that fails to dissipate due to quantum restrictions that prevent it from splitting. It is possible to look at the rules for quarks as such restrictions thereby explaining their containment and perhaps also allowing them to appear as series analogous to the spectral series of the early quantum theory that were explained by the early quantum restrictions. If quantons were quantum wave packets, then their interference and localization would be intuitively comprehended with ease, though they would not be anything like classical waves since the cause of their stability would be quantum selection or exclusion rules. Not only that. Classical optics postulates ad hoc the requirement that diffraction grids be of the order of magnitude of the wavelength of the diffracting waves. Were quantons wave packets, this would then be a most obvious requirement and the causal anomaly debate about the particle moving through one slit but being influenced by another would then be seen as a gross exaggeration.

The aim of the last paragraph is not to advocate these speculations. Nor is it to lull the sense of discomfort by reference to possible speculative resolutions of a difficulty. My aim in presenting these possible resolutions is precisely the opposite. By showing such speculations as appealing we also indicate the current troubles, the difficulties that these speculative possible solutions might repair, though not very likely. By showing what speculation may be effective we learn what it is that we are after. And once we notice what is our program we may also rehabilitate Einstein's program, though only as a program which one may but need not endorse, and with no deterministic basis or pretension to it.

5. Einstein's Program

Einstein never objected to quantum mechanics, contrary to almost every physicist’s understanding of what he said: quantum theory in Born's interpretation, as a statistical theory, is quite satisfactory. Yet, he added, as a statistical theory it wants supplementation of a particle mechanics. Briefly he wanted an X that would stand to quantum mechanics roughly as classical particle dynamics stands to classical statistical mechanics. I say roughly, because Einstein did not want total differential equations, only partial ones with proper boundary conditions and quasi-singularities, i. e., field theory with particle-like parts having proper trajectories. Obviously, what formal apparatus should be involved should not be prejudged. And, obviously, contrary to Einstein's (deterministic) view, we need not insist on a classical trajectory: all we can suggest is the program of seeking quantons whose propagation should be so described or envisaged as to yield classical – forced and free -- propagation of particles as special cases; and as approximations to the propagation of quantons. It is easy to see that classical propagation cannot be the general case, since quantum scatter and creation-annihilation processes are there. But we may want to overcome the absence of the special case if it is indeed the case that there are no quantum mechanical formulas yielding classical accelerated charges particles as approximate.

All this is different from the way Einstein discussed the situation, but only because his discussions were couched in a deterministic framework while arguing against opponents who rejected realism. He could not, then, go into further detail, particularly because his idea of the limits of quanta as he held in 1905 was erroneous and yet not replaced to his satisfaction by any other. And for Einstein, clearly, size was of no import. His astonishing ideas about statistics were the application of statistics to visible particles that exhibit Brownian motion and to galaxies. His induced radiation theory has as its major step increasing temperatures and radiation intensity beyond any limits.[8] The only experiment carrying his name, the Einstein-de-Haas effect, exhibits the micro-particle spin as a macro-phenomenon. His drawing attention to Bose's statistic and to the Davisson and Germer experiment with material waves, again, have to do with the recognition of the applicability of quantum ideas to broader domains of heavy particles. And he noticed at once that Bose's statistics deprive the proton of its sharp edges. In his "Replies to Criticisms" he noticed that were it possible to slow down billiard balls far enough to make their de Broglie wavelengths comparable to their radii and hit a grid they would exhibit an interference pattern. The experiment would last as long a time as the age of the universe, but it proves that the two-slit experiment holds for billiard balls no less than for photons. This is a logical fact, and so the unperformability of the experiment it describes is immaterial.

It seems that Einstein conceded here too much to Bohr. It is clear that there are two different aspects of quanta here that Bohr took together and Einstein conceded. The Einstein-Podolsky-Rosen paradox is not a quantum paradox. It does not put into question quantum mechanics proper, whether as understood by Einstein or by Weizsäcker: it was meant as a paradox for those discussing quantum mechanics for individual particles, not quantum mechanics as statistics. Not so with the soft edge of the proton, with the phenomena that exhibit properties that are neither wave-like nor corpuscle-like (since the Bose-Einstein condensation is a statistical matter). There is little difficulty having something that is not quite classical, and the difficulty is of the sort of having a simultaneity not quite classical, gravity not quite classical, e., etc. The difficulty with quanta is greater: we know when relativistic simultaneity looks Galilean, when -Einsteinian gravity looks Galilean or even Aristotelian. We do not quite know when quantum electrons become classical Lorentz or J. J. Thomson ones. If they do not, then we want a theory unifying the two. If they do, then all we need notice, as Weizsäcker rightly points out, is that in the paradoxically seeming experiments the paradox vanishes upon the observation that the electron is not supposed to act classic ally but quantum mechanically. But we do not even know what exactly this is. Einstein's program of having a quantum theory of single particle propagation should be useful, and allowing the theory to be statistical rather than deterministic may be the modification it wants, and is what followers of the statistical reading of quantum theory, Weizsäcker for example, should support. But it seems to be more of a program then physicists are willing to admit.

Notes

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1. Pauli was not interested in the question whether classical physics is or is not deterministic or even causal, since he was convinced that the future of physics lies in a still less deterministic region. He ended his editorial introduction to the 1948 Dialaectica issue devoted to the philosophy of quantum mechanics saying (p. 331), "We are here in the very beginning of a new development of physics which will certainly lead to still further generalizing revisions of the ideals underlying the particular description of nature which we today call the classical one." By contradistinction, Einstein and Bohr both appreciated the importance for their debate of the question, is classical physics deterministic? This was shown in their discussion with Karl Popper, in Princeton, after his lecture there on the topic, whose content was published in 1950. (See Bibliography.) I have refuted Popper's argument to my own satisfaction in a paper read in the Fifth International Conference on Logic, Methodology, and Philosophy of Science in London, Ontario. This, however, does not detract from the importance of his claim and his approach the argument was simply replaced by one developed by Landé and Popper later on.

2. The ontological status of Feynmann's virtual particles is contested among physicists. The naive scientific realism as advocated here should take their existence as a matter of course. Yet it is far from clear what real existence is there to virtual existence. That a proton is virtually a neutron plus is a fact, yet there is no virtual neutron analogous to a virtual pion in the theory; but then, had one found use for it, perhaps it would have been brought into action. What virtual particles do is reduce interaction between particle and field into that between particle and virtual particle. Hence virtual particle is field action under quantum constraints, which, the theory postulates, may be released as a particle proper. The fact that the virtual particle has this dual role all the way is what distinguishes it, and gives it more reality, than the virtual neutron that the proton contains in it. All this is acceptable to all parties within quantum field theory and needs further separate elaboration within each of the different sub-theories.

3. Hans Reichenbach, in his Philosophical Foundations of Quantum Mechanics, presents the quantum paradoxes as the wave-particle duality discussed by Einstein early in the days of quanta, and observes that Heisenberg's principle stops the electron for wave or particle nature. He concluded that we should confine the theory to the observed facts and thus have no problem left. And his proposal does work. He views the problem, especially of the two-slit experiment, as that of a causal anomaly. And he abolishes all causality. Quite generally, Popper has observed (see his 1963 book), depriving a theory of its realistic pretense solves all its problems. It is like the use of strong poison as medicine on the true ground that it stops all complaint and all ailment. All that remains, then, is a rigid corpse instead of live science. Yet some do like their science dead. Strange but true.
As Bunge has noticed, the positivistic fashions of the day left their impact on the early literature of quantum mechanics. Even the vague and useless term "observable" testifies to that. Clearly, not all observables are observable -- not even in principle, e. g., ground levels, which the new quantum theory but not the old one makes different from zero. Nor are all quantum transitions observable -- at least not adiabatic ones, i. *e.,* those between states of equal energy levels. This may be dismissed as irrelevant on the ground that quantum statistics does not distinguish two such states and Pauli's principle may even identify them. And this seems a victory for positivism, a profit accrued from the use of

Occum's razor. Yet the inapplicability of Pauli's principle to bosons may suffice to cheat positivism of its victory: why are there more bosons but not more fermions with the same quantum characteristics? Positivists may say, this is an empirical matter: we can count quantum particles with the same characteristics but not distinguish between them, and so quantum statistics takes account of the number but not of the combination of bosons in the same state, whereas we can distinguish large particles and so classical statistics does take account of their combinations. if this were true, then the limit between classical and quantum statistics will depend on our tools of observation! Moreover, Pauli's principle identifies two particles with the same quantum characteristics, not two with the same energy level, such as the two electrons in an orthohelium atom which can exchange spins with no loss or gain of energy; the two states, the one before and the one after the isoenergetic transitions are quantum statistically indistinguishable yet quantum mechanically not identical. Hence quantum theory does not endorse the Leibnizian principle of identity of indescernibles, contrary to what many textbooks say in the twilight of positivistic fervor.

There is still the question, why is quantum statistics so different from classical statistics? This question is very different from the question handled in the present essay since, no doubt, quantum statistics does yield classical statistics as a good approximation. Hence the question is not a matter of methodology but of metaphysics and pertains to the fact that, precise numerical values aside, we prefer to stick to classical statistics on the macro level. Schrödinger has claimed that on the macro level we would not distinguish between combinations of states, only of things, and hence bosons, and more so fermions, are not things but states. This idea goes well with the speculations presented here in the wake of de Broglie and Schrödinger. But I am not here advocating these speculations and there may be other explanations for the difference between classical and quantum statistics. Let me only mention, however, that Schrödinger used his idea to solve Gibbs' paradox -- see his 1946 book -- namely the fact that uniting two containers of equal pressure and volume of gas will or will not alter the entropy level depending on whether the qases in the two are the same or not. What this means is that only quantum statistics, not Boltzmannian one should account for ordinary thermodynamics! This is a far-reaching claim. It also amounts to saying that any two quantum statistically indistinguishable states are indeed identical (through not for positivistic reasons but for realistic ones). I cannot judge all this, except that it sounds suspicious, especially since the two vessels in Gibbs' paradox are large enough to count as things.

4. The fact that the same quantum thought experiments were viewed sovery differently by different people is disturbing: it *shows* that the game is played without prerules. It seems to me now, in retrospect, that there is no harm in this, on condition that it is made clear. Yet it was not made clear, chiefly since sycophants of the Copenhagen school both repressed differences of opinions within the school -- and even Bohr confessed disagreement with Heisenberg only in private conversations and claimed utter victory over Einstein -- though he never did and always stayed troubled, as it is well-known.

Yet there must be a limit to the looseness of any rules or else the game becomes pointless fast. It seems to me that the proper rule is, idealization is either a part of the theory, or a supposition that opponents should be invited to contest, or concessions to opponents. And possible deviations from such rules are better noted during the debate. I think Heisenberg violated the rules most, especially when he said, having observed one particle's position precisely twice may give it a trajectory but <u>only</u> in the past, which is uninteresting. In essence thought experiments do not sit well with such a cavalier attitude to all that is not predictive. And he said, Bohm's hidden variables are untestable and so do not count. This is cheating: the game was logical, not empirical, as understood by von Neumann and as is still understood by all students of hidden variables today, including those who claim to

have tested and refuted the assumption of hidden variables. Their claim, incidentally, is much more restrictive than it sounds.
Bohr's use of general relativity to neutralize Einstein's argument from the weighing of a photon was deemed a tour de force. I never understood why. On the contrary, I found it logically fantastic that such a remote theory should be dragged into the debate. I have discussed this with a number of physicists and found little sensitivity to this. I was fortunate in having an occasion to discuss this with Schrödinger, but he too was unimpressed, saying, if Einstein brought in gravity, Bohr was at liberty to bring in the best theory of gravity available. To my surprise Schrödinger lost patience and would not have my response to this. Karl Popper did me the honor of publishing my views on the matter. (See Bibliography for his 1959 publication.) Max Jammer criticizes my point while implicitly conceding it. (See Bibliography, his 1976 publication.) He says, Einstein's argument can be neutralized without the use of general relativity. Jammer is more concerned with the outcome neutralizing Einstein's argument than with the rule. For me, however, it remains the case that it is not who wins but how the game is played. It is no accident that the game has lost popularity; it can only regain it by making it better played, i. e., played more in accord with the rules.
5. It is hard to judge what was Bohr's methodological position, on account of its idiosyncrasy, fluidity and notorious difficulty to comprehend. In his contribution to the <u>Dialectica</u> issue of 1948 edited by Pauli, "Causality and Complementarity", he said (P. 316), "In presenting a generalization of classical mechanics suited to allow for the existence of the quantum of action quantum mechanics offers a frame sufficiently wide to account [also] for empirical regularities which cannot be comprised in the classical way of description." Putting aside the fact that he viewed the rule of approximation to be, quantum mechanics has classical mechanics for the limiting and special case when Planck's constant is equated with zero, Bohr's view expressed here is the one endorsed in the present essay. Pauli's understanding of Bohr, cited in Note 1 above, is more pronounced an expression of the same view, as he speaks there of "generalizing revisions". Yet, Pauli also endorses there Heisenberg's theory of science as of "closed theories", adding that it accords well with the dialectical view of science. It does not, though it may be viewed, dialectically, as an approximation and a special case, perhaps.
6. I may be overestimating the importance of foundationism. The apologetic oversight of the Bohr-Kramers-Slater theory (observed by B. L. van der Waerden) may be closer to contemporary physics. The denial of strict conservation laws had its import in its glorious denial with the history of the theory of the neutrino: though every conjecture about the neutrino was refuted, the rationale of introducing it, namely the defense of strict conservation laws in the face of evidence from beta decay, was amply empirically vindicated. Nevertheless, and protestations to the contrary notwithstanding, the fact remains: almost all physicists reject strict conservation, and even high-handedly. Mario Bunge is almost the only one explicitly and systematically endorsing it. Others often enough declare the tunnel effect to be an empirical refutation of strict conservation. The view that the law of strict conservation of energy is decidedly violated, but for periods of time short enough to guard the violation against detection by the Heisenberg time-energy uncertainty, this view is metaphysical and irrefutable and unempirical in the extreme, yet it is endorsed unhesitatingly by most physicists, including those who viciously ridicule much lesser violations of empiricism. Bunge denies even the validity of the Heisenberg formula for time and energy. Also Bohr was consistent here. He said, since energy and momentum conserve strictly, once a particle is permitted to have precise initial conditions (regardless of our knowledge or ignorance of them) it is thereby doomed to a precise path all the way. At heart, it seems, Bohr was committed to classical physics in its classical interpretation, and he thus found a most important function for the uncertainty principle: it had to make

room for quanta! But this is no longer the only view open to us. Once we recognize that both Bohr and Einstein were too impressed with classical arguments, once we see the tunnel effect as a violation of strict conservation (regardless of our assuming that strict conservation holds for energy transfer), then we have to decide again on the large issues, and in a manner that will decidedly put the heroic Bohr-Einstein debate well into the background. What stops physicists from this move is their apologetic mood. And the louder one criticizes them the more apologetic they become. Pity.

7. Julian Schwinger's Particles, Sources and Fields, 1970, introduces Lorentz force (p. 11) under the strange title of Galilean relativity, commenting (p. 12) that the systems described there "give a simple description of the behavior of a particle that is influenced by a macroscopic, controllable environment." Next comes a crucial sentence, quite out of tune with the whole volume and its tenor: "Since a classical theory of such interactions underlies the measurement of free particle properties, a test of self-consistency is also involved." What is bothersome is that the classical theory underlies the measurements without quantum theory underlying it. Schwinger's presentation is not clear to me. He derives the Coulomb and Ampère energies for the charge and current interactions for very slowly moving photon source (p. 77); the emission of the slow electron is taken up again later on and the radiation proves to be infinite (p. 274), and this impediment is then removed. The overall resultant picture is not clear to me.

8. See B. L. van der Waerden's thoughtful book or the sources of quantum mechanics for the fact that Einstein's radiation theory embarrassed the establishment. It is a historical fact that soon after the new quantum theory and quantum field theory were established this theory was neglected and not even mentioned in many textbooks, not even in those which introduced the topic in a historical manner, as is quite usual. The advent of lasers sent many a physicist back to school to study Einstein's A's and B's.

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