On quantum mechanics

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Abstract — **All references are to my online bibliography of quantum stuff, on the course web page.**

1 A SKETCH OF QUANTUM FIELDS AND QUANTUM ELECTRODYNAMICS (QED)

When trying to extend Schrödinger's equation to make it relativistically invariant, it will not do to assume instantaneous action at a distance as is implied by the potentials in (EQ ??). For this reason, one must, instead of viewing particles as moving around in a potential, view them as interacting with an electromagnetic field. Assuming we are operating inside a large box with periodic boundary conditions (or any other nice compact space), one expresses the EM field as a infinite weighted sum of a complete set of eigenmodes indexed by integers which specify the wave vector and polarization. (E.g. see Morse and Feshbach 1953 13.3.45 page 1845 for explicit formulas for such a set.) The field is then described by the countably infinite set of real weights. One then uses the Lagrangian formulation of classical electrodynamics to write the total lagrangian as a quadratic function of the positions and momenta of the particles and the field-weights. Upon finding out what the "generalized momenta" corresponding to the field-weights (regarded as generalized position coordinates) are, one similarly writes the classical Hamiltonian function in terms of these. With this Hamiltonian in hand, one then simply writes down Schrödinger's equation (EQ ??) to describe the complete system. Unfortunately Schrödinger's equation now is a PDE in a real space of *countably infinite* dimensionality, but formally, everything is OK[1].

It is usually difficult to solve the resulting infinite dimensional PDE, but a useful calculational procedure has been worked out by **Feynman** based on combining his propagator formalism with a diagrammatic notational technique and a specific set of useful integrals. (See Feynman & Hibbs 1965 Feynman 1962, Jauch and Rohrlich 1976, Scadron 1991, etc.) These methods will be discussed rather vaguely here and in §??. They allow one to write down essentially any quantum mechanical quantity as a power series in the dimensionless fine structure constant α, where $\alpha^{-1} \approx 137.03599$. There is no assurance that this series will converge[2], but because α happens to be so small, the first few terms often give useful numbers.

Assuming we have a problem with n electrons, the coefficients of α^k in this series are expressible as sums of $(n+k)^{O(k)}$ integrals of products of known "propagator" and "interaction amplitude" functions over a space whose dimensionality is $O(k)$. These integrals arise from the "Feynman diagrams" with k interactions. The idea is that interactions between particles and the EM field occur only by emission or absorption of a "photon," and there are only a finite number of possible combinatorial ways in which n electrons can interact via at most k photons. Each one can be summarized with a diagram, and has a complex amplitude contribution expressible via integrals of products of free particle propagators (since the particles are free between the photon hits) and emission and absorption amplitudes. (One must integrate/sum over all possible positions of the electrons at each collision and all possible wavevectors and polarization states of the photons.)

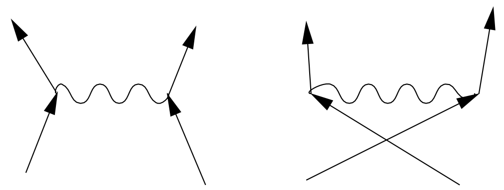

Figure 1: The two Feynman diagrams that arise in the analysis of electron electron scattering at "first order."

Now in Feynman's procedure, it can happen that certain of these integrals diverge. However, it has been

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[1] The recipe we have just described is in fact not relativistically invariant, if the pure-particle Hamiltonian is expressed using the nonrelativistic formula $2mE = p^2$. However, there is a relativistic version of Schrödinger's equation by Dirac, in which Ψ is no longer a complex number but instead is a $4N$ dimensional complex vector (if we are describing N fermions), and in which the operators (in addition to $\hat{x}$ and $\hat{p}$ there are new "spin" operators) are now $4N$ dimensional complex matrices whose elements are linear differential operators. If Dirac's recipe is used in place of Schrödinger's, the correct relativistically invariant quantum electrodynamics results... except that the relativistic possibility of particle-pair creation must also be taken into account...

[2] Dyson 1952 gives heuristic arguments that essentially all of them will diverge, and various numerical and analytical results collected in Le Guillou and Zinn-Justin 1990 suggest that the nth term of these series will behave like $(cn)^n$ for some complex constant c. Some proofs of Borel summability[3] for *some* such series, and numerical work suggesting Pade summability, have appeared.

shown that all the divergent integrals you can get may always be formally expressed in terms of two particular divergent integrals, representing the self-energy and "effective charge" of the electron. Whenever such integrals occur, Feynman *replaces* them by the experimental values $m_e c^2$ and e of the electron's self-energy and charge, causing everything to become finite!

This replacement is called **renormalization**. Its "justification" is that in some hypothetical more advanced theory that hopefully will appear someday, it will be shown that these integrals should be replaced by something that will be finite – and will in fact agree with the experimental values. Also, the integrals diverge only logarithmically, and physicists have somehow decided that this proves that QED is close enough to the hypothetical future theory that this procedure is acceptable (whereas, if they had diverged like a power law, then QED would have been "nonrenormalizable" and these physicists would not have accepted this trick). Another view of renormalization appeals to the commonplace miracle that very complex phenomena involving interactions with surrounding media can sometimes be approximated very well by simply altering some constants. For example, Maxwell's equations in many media are the same as Maxwell's equations in the vacuum and the entire effect of the medium is described by simply altering the values of some permittivity constants (Jackson 1975). As another example, Stokes 1843 discovered that the motions of small bodies in a nonviscous fluid may be described by Newton's laws but using an "effective mass" corresponding to the mass of the object plus the mass of the fluid that it "carries along with it." For a sphere of radius a in a fluid of density ρ, this mass increment is $2\pi a^3\rho/3$ (See Lamb[4] 1945, §92.) So, similarly, the vacuum in quantum electrodynamics – which is a very complicated nonlinear structure – hopefully has an effect which, to a good approximation, is simply to alter the effective mass and charge of electrons.

Incidentally, some words are in order regarding the "nonlinearity" of QED. Two effects which in classical and lower quantum theories would be forbidden, due to linearity of the field equations, are scattering of light by light, and scattering of light by a static electromagnetic potential. Both of these effects in fact can occur in QED, due to Feynman diagrams involving virtual pairs. The latter effect has been observed ("Delbrück scattering," see Wilson 1953), and since generally QED is supported extremely well by experiment[5] and these nonlinear effects influence virtually every calculation, one has to take it seriously. Thus, exact linear superpositions no longer are[6]. Furthermore, the noisy vacuum in QED seems to remove the possibility of regarding any quantum system as "closed," since there are an infinite number of degrees of freedom in the quantized fields and virtual particles, all of which can interact with it.

Part I
More detailed examination

We will now examine each of the four "problems" of §?? in separate sections, one for each problem. The conclusions of eachg section are summarized concisely at its end, and these section conclusions, when combined, will hopefully form a coherent picture which shows how to resolve all the problems satisfactorily... see the overall conclusion §5.

2 Nonlocality and hidden variables

Linear Quantum mechanics under the Copenhagen interpretation is *nonlocal.* The difficulty is not in the Schrödinger equation, which (being a partial differential equation) is inherently local. (It is true that there is an instantaneous "action at a distance" implied by the potential function in the Hamiltonian, but that is repaired in relativistic formulations.) The difficulty lies in the mysterious measurement process – which instantaneously collapses the wave function, even though this wave function may be describing a physical system spread over a large region of space.

Einstein, Podolsky, and Rosen 1935 pointed this out and concluded that quantum mechanics was not "complete."

They imagined preparing a system of two particles A and B, speeding away from each other, in which the center of mass and the momentum difference of the two particles were initially known, exactly. When A and B become very far (spacelike) separated, one suddenly measures the momentum of A (or the position of A; we

[4]Effective mass problems with more than one sphere are considered in Lamb §93, 98, and 99, but for small a all these formulas reduce to the one from §92.

[5]For example, heroic computations by Kinoshita and Lindquist 1990, see also Sommerfield 1958, show that the magnetic moment of the electron is predicted by fourth order QED to be $1+\frac{1}{2}(\alpha/\pi)-0.32847896557919(\alpha/\pi)^2+1.17611(42)(\alpha/\pi)^3-1.434(138)(\alpha/\pi)^4$ Bohr magnetons. [Here the exact expression for the coefficient $-0.328...$ is $197/144+\pi^2/12-\pi^2\ln\sqrt{2}+3\zeta(3)/4$.] By use of the value $\alpha^{-1}=137.0359979(32)$ from a quantized Hall effect measurement, and also adding in corrections of 2.8, 0.1, 1.6(2), and 0.05 pico magnetons from muon, tauon, hadron, and weak effects respectively, this yields 1.001159652141(28) Bohr magnetons, whereas the latest experiment gives 1.0011596521884(43) for the electron and 1.0011596521879(43) for the positron. The experimental and theoretical values thus agree to 11 significant figures!

[6]Although if enough dimensions beyond those usually treated in lower quantum are put in, then linearity returns, in the sense that the propagators arising in the Feynman diagrams at any *fixed* order of QED all obey the linear superposition principle. But of course QED at any fixed order is inexact and the discrepancy between it and reality would seem to an observer to be caused by nonlinear effects. Does linearity also hold at infinite order? Not clear to me, since the Feynman series are probably divergent and the infinite dimensional PDE picture is muddied by renormalization. However, the fact that Feynman's renormalization trick of "subtracting out" infinite quantities, seems to work, perhaps might be taken as experimental evidence for linearity.

may freely choose which measurement to make). The momentum of B (or the position of B) is then instantly deducible[7].

EPR rest their case on two axioms:

1. "If, without in any way disturbing a system, we can predict with certainty the value of a physical quantity, then there exists an element of physical reality corresponding to that quantity."
2. Any element of physical reality must have a counterpart in a "complete" physical theory.

Now the position or momentum of particle B is deducible without disturbing it (by looking merely at A, which is spacelike separated from B) it follows from these two axioms that any complete physical theory must "know" both the momentum and position of B – and quantum mechanics does not. Hence, at least if you accept the EPR "axioms," quantum mechanics is incomplete, and a complete theory would have to admit extra information carried along by particles. This extra information was called "elements of physical reality" by EPR, but is called "local hidden variables" by everybody else.

Bohm 1952 (page 166):

> ...from the point of view of macroscopic physics, the coordinates and momenta of individual atoms are hidden variables, which in a large scale system manifest themselves only as statistical averages. Perhaps then [quantum mechanics is] similarly a manifestation of hidden variables which have not, however, yet been detected directly.

EPR suspected that a local hidden variable theory could exist whose predictions would always be compatible, at least in a statistical sense, with those of quantum mechanics. However, this was disproven by various authors.

The most powerful disproof I know of is due to Kochen and Specker in 1967 The reason I say that this is the most powerful one, is that it not only rules out local hidden variable theories, but in fact rules out any hidden variable theory, local or not, in which at any time the values of all co-measurable observables are determined by the hidden variables (or any statistical ensemble of such scenarios). Sketch of the proof: Kochen and Specker constructed a set of 117 points on the unit sphere $x^2 + y^2 + z^2 = 1$ such that for *any* boolean function f on the 117 points, there are always three points A,B,C which are mutually perpendicular vectors and such that $f(A) + f(B) + f(C) \neq 2$. Hence, in a spin 2 system ($S_x^2 + S_y^2 + S_z^2 = 2$; note that these S_j^2 commute with each other and are simultaneously measurable) and with $S_j^2 \in \{0, 1\}$, any hidden prediction of all 117 possible measurements is self-contradictory. Nor can the prediction be a probability distribution over $(\mathbf{Z}_2)^{117}$.

Suppose we take this quotation/boast of Bohm (1965, page 475) at face value:

> In our theory, every (quantum) proposition is either true or false with certainty, if the values of the hidden variables are known. However, these propositions refer to the results of experiments on the system... and to *realize* the truth or falsity of experimental propositions representing projection operators which do not commute involves *incompatible processes* [where processes are "incompatible" if they correspond to two quantities which cannot be simultaneously measured, in particular those whose quantum operator versions do not commute, and compatible otherwise].

It then follows from Kochen and Specker's theorem that there cannot be any extension of Bohm's hidden variable theory to allow spin (and hence any relativistic extension of the Bohm theory), or at least, there cannot be any extension having the properties (or even much weaker ones) that Bohm considers so desirable in his quotes above.

Another disproof, this one much more susceptible to experimental check, was devised by J.S. Bell in 1966 and was extended by Clausen et al. in 1969.

The Clausen et al. scenario was subjected to experimental tests by A.Aspect et al. in 1982. We will now discuss this.

In the Clausen scenario, a source emits pairs of spin-1/2 particles with total spin 0. One may deduce from the EPR axioms (since the result of a spin measurement in some direction on particle B is deducible from a measurement in the same direction on particle A) that any "complete" theory must involve hidden state information predetermining the results of a spin measurement in any possible direction on any particle. So, after the particles have separated, one performs measurements of their spin components along directions $\vec{a}$ and $\vec{b}$ respectively. The possible results are $\{++, +-, -+, --\}$. Let these have probabilities $P_{++}(\vec{a}, \vec{b})$, etc. Define

$$E(\vec{a}, \vec{b}) = P_{++}(\vec{a}, \vec{b}) + P_{--}(\vec{a}, \vec{b}) - P_{+-}(\vec{a}, \vec{b}) - P_{-+}(\vec{a}, \vec{b}). \quad (1)$$

The statistical quantities $E(\vec{a}, \vec{b})$ are susceptible to experimental measurement. Any local hidden variable theory must obey

$$-2 \leq B = E(\vec{a}, \vec{b}) - E(\vec{a}, \vec{b'}) + E(\vec{a'}, \vec{b'}) + E(\vec{a'}, \vec{b'}) \leq 2. \quad (2)$$

[7] Meanwhile a different observer could be measuring the momentum or position of B (he freely chooses which). If the choices made by the two observers were different, apparently we are enabled to measure both the momentum and position of particle B and thus violate the uncertainty principle?! Bohr argues that one actually cannot get around the uncertainty principle in this way because the observation of A's position will cause an uncontrollable shift in the momentum of A's observer! The two observers eventually have to meet and measure their own relative momenta and positions and then the uncertainty principle will play its part. This being the case, Bohr concluded by waving his hands that the faster than light collapse of the wave function did not actually represent the transmission of *usable* information and was hence allowable.

However, when $\angle\vec{a}\vec{b} = \angle\vec{b}\vec{a'} = \angle\vec{a'}\vec{b'} = 22.5°$ and $\angle\vec{a}\vec{b'} = 67.5°$, quantum mechanics predicts the value $B = 2\sqrt{2} \approx 2.828$. These inequalities and predictions can be modified to consider the effects of imperfect polarizers; with the polarizers used in the experiments of Aspect et al., the prediction of quantum mechanics was $B = 2.70 \pm 0.05$, and the prediction of any hidden local variables theory was $|S| < 2.08$. The result of the experiment was $B = 2.697 \pm 0.015$. Hence hidden local variables theories of the sort that EPR would consider to be "complete," have been ruled out by 41 standard deviations. It would still be possible for hidden local variables theories to exist with smart adversarial hidden variables which somehow "knew" which of the 4 possible orientations the experimenter was about to choose for his next spin measurements... but in a later experiment in which these angles were chosen by oscillators operating at incommensurate frequencies (and physically separated by 13 meters – which was spacelike, considering each spin measurement took ≈ 10 nanoseconds), Aspect et al. still rule out hidden local variables by 5 standard deviations. The hidden variables would indeed have to have been rather cleverly adversarial to outwit this.

Summary To summarize this section, we've seen that hidden variable theories seem to have no future:

1. All local H.V. theories have been ruled out experimentally,

2. All H.V. theories satisfying Bohm's desiderata have been ruled out in the sense that they disagree with the quantum theory of spin, which is generally highly supported by evidence (although I know of no experimental evidence pertaining to the particular scenario in Kochen and Specker's disproof).

At which point we are stuck with quantum mechanics and the nonlocality that lies at the root of E, P, & R's complaints – very unpalatable! However there is a way to salvation. Consider these three facts:

1. EPR's scenarios do not allow one to defeat the uncertainty principle, that is just an illusion which vanishes on deeper examination.

2. In the Dirac (relativistic version) of Schrödinger's time evolution, information is not transmitted faster than light (and we are not weaseling by defining some notion of "usable" information such that superluminal changes in the wave function are unusable – in fact, *all* changes in the wave function propagate at speeds $\leq c$)

3. The only superluminal process in the Copenhagen interpretation of Dirac's quantum mechanics is thus wave function collapse during measurement, which is *not* governed by the Dirac equation.

Therefore: if we deny the existence of the Copenhagen instant nonunitary collapse process during measurement, and somehow deepen the theory to find a replacement for it which is local, then we will be in clover – all EPR's nonlocality objections will have vanished.

3 NonHamiltonian systems

The formulation of quantum mechanics in §?? is only capable of treating "totally isolated," "closed," "ballistic," or purely Hamiltonian (all four words mean the same thing) systems.

However, a little reflection convinces us that no system[8] is ever closed. One can often shield off a system fairly well from the rest of the universe – well enough that the effects of the rest of the universe represent only a very small perturbation on it – but this shielding can never be perfect. For one thing, even inside a perfectly evacuated chamber surrounded by a thick cosmic ray shield, superconductive EM shield, etc, and even supposing we could somehow shield out gravitational and neutrino radiation (although there is no known way to do so) there will still always be a blackbody radiation bath at some temperature T, which can be reduced by refrigerating the shield, but never to zero. (Even a perfect Carnot refrigerator has efficiency at most T_1/T_2, where T_1 is the internal temperature and T_2 is the external temperature.)

3.1 Open classical systems

What effect will such a blackbody radiation bath have on a *classical* system? Consider a stationary perfect mirror sphere of radius a (or some other sort of body of rough size a) immerse in a blackbody radiation bath which is moving, relative to the sphere, at a speed v. We assume that $a \gg \frac{hc}{k_B T} = \lambda_T$, i.e. the sphere is much larger than the typical wavelength of a blackbody-bath photon; otherwise the radius a would have to be replaced by some other "effective radius" in all the below developments[9] We also assume we are dealing with time scales much larger than the typical interval between photon-sphere collisions, namely, much larger than $k_B/(4\pi a^2 \sigma T^3)$. The blackbody bath may be modeled as exerting a uniform radiation pressure $2\sigma T^4/(3c)$ on all stationary surfaces.

[8] Even the entire universe – if we realize from QED that there are always more dimensions out there to leak into

[9] Let the "momentum cross section" $\sigma_{\rm mom}$ of an object be defined so that the drag the object would experience in a light beam, is $\sigma_{\rm mom}$ times the momentum flux in the beam. Let the wavelength of the light in the beam be λ. Then for a large ($a \gg \lambda$) massive mirrored sphere, $\sigma_{\rm mom} = 2\pi a^2/3$. The appropriate integral of the Thomson differential cross section for nonrelativistic scattering of light at bending angle θ by an electron, yields $\sigma_{\rm mom} = 8\pi a^2/3$, if a is the classical electron radius $e^2/(m_e c^2)$. (If we use a charge q with mass m in place of the electron, the same formula would apply but with $a = q^2/(mc^2)$.) If instead of the Thomson differential cross section, we were to use the (relativistically valid) Klein-Nishina QED formula, one would get

$$\sigma_{\rm mom} = \frac{\pi\lambda(\lambda+a)}{3(\lambda+2a)^3 a^2}[18a\lambda^4 + 102a^2\lambda^3 + 186a^3\lambda^2 + 102a^4\lambda - 20a^5 \tag{3}$$
$$+(9\lambda^5 + 60a\lambda^4 + 141a^2\lambda^3 + 126a^3\lambda^2 + 12a^4\lambda - 24a^5)\ln\frac{\lambda}{\lambda+2a}].$$

which reduces to the old λ-independent formula in the limit $\lambda \gg a$. Thus the formulas for a large massive sphere and an electron are the same, except for a factor of 4. However, for a small ($a \ll \lambda$) but still massive perfectly conducting sphere, one may show by performing the appropriate integral of Mie and Debye's differential cross section (Jackson 1975 §9.6c) that $\sigma_{\rm mom} = 14\pi a^6/(3\lambda^4)$.

However, since moving surfaces will see a slightly blue shifted (or red shifted) blackbody bath, our sphere will experience a drag force. The magnitude of this force under the assumptions above is

$$\text{Drag} = \kappa \frac{4\pi a^2 v \sigma T^4}{c^2 - v^2} \tag{4}$$

where $\kappa = 2$ for a mirror and 1 for a black sphere. The force is expressed in the rest frame of the sphere.

This is an instance in which the total effect of the rest of the universe can be modeled very simply as a linear frictional force just as in §**??**.

More generally, it is usually the case that the effect of rest of the universe on classical systems which are moving *sufficiently slowly*, compared with external thermal velocities, may be modeled[10] as a linear frictional force. It is also usually the case that small perturbations to a system may be treated linearly.

Hence most such systems feature a characteristic "relaxation time" τ during which any small perturbation will shrink in energy to a factor $\approx 1/e$ times its original size, due to frictional dissipation. If the system has a characteristic frequency f, then the dimensionless quantity $Q = \tau f$ measures the "quality" of the isolation of the system.

For example, consider the Earth-moon system. The main frictional force on the moon is caused by the tides it induces on the earth. If the moon were orbiting sufficiently slowly, then the distortion of the earth's atmosphere, oceans, and crust would exactly track the moving (gravitational and elastic) equipotential at whatever the frequency of the moon's orbit was. The viscous dissipation for slowly moving fluids is linear, so that dissipated energy per period would be constant, and the frictional force experienced by the moon would indeed be proportional to its orbital velocity. However, for large orbital speeds, the law of friction would not be linear (in fact, since we know that tides are higher in some parts of the ocean than others, we know that the moon's actual period is small enough that we are slightly outside the linear regime). It has been estimated that tidal forces at their present intensities cause the moon to lose 10^{-11} of an orbital period, per orbit, for a "Q" value of $\approx 10^{11}$. If there were no tidal forces and the drag on the moon were entirely due to the blackbody formula (EQ 4), then (assuming $T = 300$ Kelvin) we would have $Q \approx 2 \times 10^{17}$, and at 3 Kelvin it would be $\approx 2 \times 10^{25}$. The highest-Q systems that have been constructed in the laboratory tend to have $Q \approx 10^8$???

But friction is not the only way in which the universe interacts with a classical system. There is also "Brownian motion." Recall that, when deriving (EQ 4), we had to assume that the time scale was much larger than the typical interval between collisions. Looked at on a shorter time scale than that, we do not have a smooth frictional drag force, but rather, a series of collision-induced impulses, in random directions! The frictional drag really results merely from a statistical bias in these random impulses induced by the sphere's motion. And in general, we see that there are always going to be such "Brownian kicks," which tend to lead to random diffusion away from the classical trajectory (as calculated with friction).

These Brownian kicks may be modeled as a random noise external driving force $\vec{F^{\text{fluct}}}(t)$. A classical system subject to frictional drag and this fluctuating (zero mean) force then obeys the Langevin equation

$$m_r \ddot{x}_r = -\sum_b \mu_{rb} \dot{x}_b + F_r^{\text{fluct}}(t). \tag{5}$$

(For a sphere of small radius a in a Stokes fluid of viscosity η, $\mu = 6\pi\eta a$.) If we suppose that the force F is supposed to be the same as the force that would be exerted on the mass were it nearly stationary and in thermal equilibrium with the medium at temperature T, then the requirement that the mass lose energy to friction at the same rate at which it gains energy from F^{fluct} when its mean kinetic energy $\overline{\sum_r m_r \dot{x}_r^2}$ is $k_B T$ yields the classical *fluctuation dissipation theorem* (Pathria 1977, or Callen and Welton 1952, 1957; Chandrasekhar 1943)

$$2 k_B T \mu_{ab} = \int_{-\infty}^{\infty} \overline{F_a^{\text{fluct}}(s) F_b^{\text{fluct}}(t)} dt \tag{6}$$

where the overline denotes thermal averaging[11].

The time spectrum of F_{fluct} should feature a cutoff at frequencies larger than those arising from the typical time scale of the collision process. Assuming Poisson statistics, it is roughly white below this cutoff, but if we assume that the likelihood of especially large gaps between collisions is smaller than that entailed by Poisson statistics (certainly the case in a nonideal gas), then there would also be a cutoff at very low frequencies. For many purposes, though, it suffices to ignore these cutoffs and model the driving force as, in fact, the differential of a Wiener process (i.e. gaussian noise) with an autocorrelation proportional to a delta function.

In the case of our mirror sphere in a blackbody bath,

$$\int \overline{\vec{F_r}(s) \cdot \vec{F_r}(t)} dt = \kappa \frac{8\pi a^2 v \sigma k_B T^5}{c^2 - v^2} \tag{8}$$

So, to conclude.

1. No system can be totally isolated from its environment.

[10]There is another interesting paper by J.B. Pendry: Shearing the Vacuum – Quantum Friction, J. Physics Condensed Matter 9 (1997) 10301- in which as QED analysis suggests that "QED vacuum effects" will cause a drag force between two nearby moving objects of nonzero electrical resistivity.

[11]Callen and Welton 1952, 1957 later showed that under quite general *quantum* statistical mechanical definitions and assumptions, this should be replaced with

$$\pi \overline{\tilde{F}_a^{\text{fluct}}(\omega) = \tilde{F}_b^{\text{fluct}}(\omega)} = 2 \int_0^{\infty} \frac{\hbar\omega}{2} \coth \frac{\hbar\omega}{2 k_B T} \tilde{\mu}_{ab}(\omega) d\omega \tag{7}$$

where the tilde is the Fourier transform to ω-space, and the friction coefficient μ is now allowed to have a spectral dependence, if desired.

2. Although it is impossible to produce a model of every possible interaction with every possible kind of environment, a model which is often useful (especially for fairly well isolated systems) is the Langevin equation (EQ 5), which is Newton's laws with a friction term linear in the velocities and a fluctuating force term whose magnitude is related to the friction term and the environment's temperature by the fluctuation dissipation theorem (EQ 6).

So, now that we have some understanding of how to model these effects in classical mechanics, we ask how they should affect quantum mechanics.

3.2 Open quantum systems

Perhaps the most obvious way to proceed is to try to produce a quantum version of the Langevin equation. The direct attempt to do this was made by Kostin 1972. We will describe his simple argument in a moment. The Kostin equation was rederived later by other authors; in particular Yasue 1978 derived it by applying Nelson's interpretation of quantum mechanics to the Langevin equation.

Kostin wanted an equation of form

$$i\hbar\frac{\partial\Psi}{\partial t} = -\frac{\hbar^2\nabla^2}{2m}\Psi + U(x,t)\Psi + U_{\text{fluct}}(x,t)\Psi + U_{\text{frict}}(x,\Psi)\Psi \tag{9}$$

and he wanted the expectation value of the momentum p (by Ehrenfest's theorem) to obey the classical Langevin equation

$$\dot{\bar{p}} = -\frac{\mu}{m}\bar{p} + F_{\text{fluct}} + F \tag{10}$$

where $F = -\vec{\nabla}U$. He first set $U_{\text{fluct}} = -xF_{\text{fluct}}$, and then noticed that for all wavefunctions Ψ, the choice

$$U_{\text{frict}} = \frac{\hbar\mu}{m}\arg\Psi + W(t) \tag{11}$$

causes (EQ 10) to be satisfied (for a certain function $W(t)$). Kostin argued that since $W(t)$ is merely a constant added to a potential, it makes no difference to the dynamics, or alternatively by multiplying Ψ by a certain phase factor depending on t only, the $W(t)$ term vanishes.

There are several flaws in this derivation. First, Kostin never justified his assumption that the SL equation had to be of the form (EQ 9), and second, Kostin never proved that (EQ 11) was the only possible choice for U_{frict}; perhaps some other choice would also satisfy (EQ 10) for all wavefunctions Ψ. And in fact, were we to allow U_{frict} to be an arbitrary differential operator instead of a scalar, there would certainly be a vast number of possible choices for it, all of which would obey (EQ 10) (for example, one could add any outer product of wavefunctions orthogonal to Ψ, to this operator).

However, as Yasue showed, if we apply Nelson's procedure for converting any classical system described by Newton's laws to a quantum system, we get Kostin's equation (EQ ?? with the "external" driving force being repalced by the "fluctuating" force) as the unique result. The only way to attack the Kostin-Yasue equation, then, is either to argue that Nelson's procedure is wrong, or to argue that the Langevin equation, although it may be a good model for open classical systems, is not a good model for open quantum systems. Since Nelson's procedure has been proven to yield the same result as the usual quantization procedure for all Hamiltonian systems, the first counterargument is never going to get experimental support. The second argument, I believe, homes in on the final weak point of Kostin's derivation. *The problem is that the fluctuating force has been assumed to be a function of t only, i.e. the same at all positions. This is not realistic,* since there is no reason the environment should be having the same effect at all points of space. Yasue fell into the same trap. Nor is this easy for Kostin or Yasue to fix. If we instead assume that F_{fluct} is gaussian noise with fixed spatial and time autocorrelation values, we will be able to write down the "Nelson-Langevin" equations. But since F_{fluct} will not be the gradient of a potential $U_{\text{fluct}}(x,t)$ (except if we are in one dimension, in which case $U_{\text{fluct}}(x,t) = \int_0^x F_{\text{fluct}}(y,t)dy$) it may not be possible to convert these from (u_r, v_r) land to Ψ land. The Nelson-Langevin equations have not been studied, and the whole approach of using Nelson's equations to do nonHamiltonian quantum mechanics has never really been used.

In the event that $F_{\text{fluct}} = 0$ but $\mu \neq 0$ (friction, but no fluctuating force), however, the Kostin-Yasue equation (EQ ??) should still be still valid. We now enquire what effects the small μ term in it has. First, notice that the μ term is *nonlinear.* This is a qualitatively drastic change. It wrecks the superposition principle.

In the event that that Schrödinger's equation were applied to any Hamiltonian system, but only a subsystem (a subset of the complete Hilbert space basis) was of interest, so we integrate over the probability distribution of possible states of the environment... considering that Schrödinger and integration are both linear, we see that the time-evolution of the subsystem would still be *linear.* Hence, Kostin's equation with $F_{\text{fluct}} = 0$ *cannot* be derived by assuming the interaction of a subsystem with *any* environment and taking expectations over the environment states! This disturbs us. The explanation is presumably that it is impossible to get friction without any fluctuating force.

We may nevertheless enquire what sort of behavior the solutions of Kostin's equation would have in the presence of friction but the absence of a fluctuating force. First, consider a wavefunction Ψ which is everywhere real. (Or of constant phase.) Such a wavefunction (well, its square) obeys the classical laws of probability and does not obey the quantum laws of complex superposition. Even though a wavefunction may have two humps, and thus represent a state of a particle "in two locations" (rather difficult for humans to comprehend), still if Ψ is real, at least we are capable of interpreting this as "with probability p, the particle is in the left hump, and with probability $1-p$, it is in the right hump," which is still pretty comprehensible. The point is that all-real wavefunctions are somehow more "classical." Now, considering the phase version (EQ ??) of the SL equation, we see that friction is causing a restoring effect which tends to move the phase angle toward 0. Indeed, in regions in which the potential U and $|\Psi|^2$ are flat, S will asymptote as $t \to \infty$ to a constant value. This is quite in contrast to the usual Schrödinger equation, where the phase angle S will continually move, with asymptotically constant angular *velocity* in such regions. Hence, we see that friction would cause quantum mechanics to behave more like the classical laws of probability and less like quantum laws of complex amplitudes, and the time scale of this collapse is the same as the classical damping time τ_{class} of the system, at least in the semiclassical limit $\hbar \to 0$.

But in view of the flaws in the derivation of the Kostin-Yasue equation, a different approach to describing the effect of the external environment on quantum systems, is desired[12]. Such an approach, which fits entirely within the framework of orthodox quantum mechanics, was described by Feynman and Vernon 1963.

It is as follows. *First*, a general expression (as a path integral) is derived that allows one to express the effect of the external universe ("environment") on the time evolution of a "test system" in terms of a linear "influence functional" which depends purely on the interaction Hamiltonian and the Hamiltonian of the environment. *Second*, in the event that the coupling between the test system and the environment is weak, it makes sense to approximate the interaction Hamiltonian as a quadratic function which is linear in the position variables of the test and environment systems alone. Also, in the event that the environment is only slightly perturbed by the test system, it makes sense to linearize these perturbations. Thus we model the enviroment as a linear system (i.e. whose Hamiltonian is quadratic) coupled to the test system by a bilinear quadratic term. *Third*, it turns out that in that model, all the path integrals in the expression for the influence functional are of complex Gaussian form, and hence may be integrated in closed form. *Fourth*, we also integrate over all possible initial states of the environment, each one weighted according to a Gibbs distribution at temperature T, to find the thermal average time evolution of the test system. This integration turns out also to be a path integral of Gaussian form, also soluble in closed form.

The result is that the test system's time evolution in the presence of the environment is described via a path integral – just as in Feynman's usual formulation of closed quantum mechanics – except that the integrand is altered by the influence functional of the environment. In cases in which the test system is especially simple, namely the cases of a rigid rotator or a harmonic oscillator, it is possible to proceed further and do even this path integration, thus obtaining a closed form solution for the entire problem of the time evolution of the open system. Usually, however, this final step cannot be done in closed form, and so we seek to derive approximate time-evolution equations for open quantum systems. Often these approximate equations can be written in such a way that they are exact for the rigid rotator and harmonic oscillator.

We now go through this approach in more detail. We divide the Hamiltonian into three parts, $H = H^{test} + H^{int} + H^{envir}$. First, the Feynman-Vernon theorem expresses the propagator for the combined test and environment system, but restricted to the subspace of the test system only, as

$$\begin{aligned} K^{\text{test}}(2,1) &= \int\int e^{iS^{\text{test}}(x)/\hbar} F(x,y) \mathcal{D}\frac{x(t)}{A}\mathcal{D}\frac{y(t)}{A} \\ &= \int e^{iS^{\text{test}}(x)/\hbar} F(x,y) \mathcal{D}\frac{x(t)}{A} \end{aligned} \tag{12}$$

where

$$F(x,y) = \exp\left(\frac{i}{\hbar}[S^{\text{env}}(y) + S^{\text{int}}(x,y)]\right) \tag{13}$$

and

$$F(x) = \int F(x,y)\mathcal{D}\frac{y(t)}{A}. \tag{14}$$

Here $F(x)$ is the "influence functional" (note, our formalism differs from Feynman and Vernon) and its effect on the time evolution of the test system is linear.

Next, in the weak-coupling approximation, H^{envir} is a quadratic function of of the environmental generalized positions and momenta. By a linear change of variables, this quadratic may be expressed in the diagonalized form $H^{\text{envir}}(y_r, p_r) = \frac{1}{2}\sum_r \omega_r^2 y_r^2 + p_r^2$. The interaction, we will assume, is bilinear: $H^{\text{int}} = \sum_a \sum_b I_{ab} x_a q_b$ where x_a are the position coordinates in the test system and the I_{ab} are constants. Letting $I_b(x) = \sum_a I_{ab} x_a$, we have

$$\begin{aligned} H^{\text{envir}} + H^{\text{int}} &= \frac{1}{2}\sum_r \omega_r^2 y_r^2 + p_r^2 + I_r(x) y_r \\ &= \frac{1}{2}\sum_r \omega_r^2 (y_r')^2 + p_r^2 - \frac{I_r(x)^2}{4\omega_r^4} \end{aligned} \tag{15}$$

where $y_r' = y_r + \frac{I_r(x)}{2\omega_r^2}$, so that the environment in the presence of an interaction with a test system (whose x

[12] Nelson's stochastic quantization is still one of the most natural, maybe the most natural, way to put nonHamiltonian forces into quantum mechanics, and the wrong physical assumption made by Kostin, Yasue and other derivers does not affect the validity of Nelson's approach. One should be able to derive a Schrödinger-Langevin equation via Nelson's approach but this time with the correct model of the fluctuating force, e.g. Gaussian noise with both space and time autocorrelations. It appears, though, that one has to use Nelson's equations (EQ ??) and (EQ ??) for u_r and v_r and abandon attempts to get back to the Ψ world. This is because, if you follow Nelson's 1966 derivation of the Schrödinger equation, you will observe that at a crucial step, gradients are used, and this is impossible if the force field is not a gradient. We no longer require that the drift momentum be a gradient, but u_r/θ_r is still a gradient (of $\ln\rho$). The logical replacement for the condition that the drift momentum be a gradient, may be to require that $(\vec{v}\cdot\vec{\nabla})\vec{v} = \frac{1}{2}\vec{\nabla}(v^2) + \frac{\vec{F}}{m}$???

is fixed) is, if we switch to y' coordinates, still mathematically the same as it was before except for an overall energy shift $-\sum_r \frac{I_r(x)^2}{4\omega_r^4}$.

The heat bath is entirely specified by its temperature T, the density of modes function $\rho(\vec{\omega})$ and the coupling constant $a(\vec{\omega})$.

4 Measurement

The Copenhagen view of measurement (sudden, instantaneous, nonlocal nonunitary collapse of a wave function down to an eigenfunction) is clearly an untrue idealization. What is really going on is just unitary Schrödinger time evolution of the combined measuring and measured system.

How can a unitary transformation possibly look extremely nonunitary? This is simply explained – see figure reffig:rotateproj.

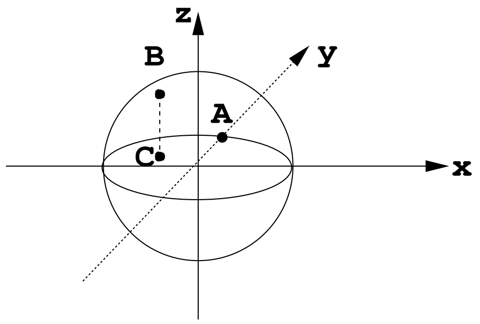

Figure 2: Let the xy plane represent the measured system. The full system, including both the measured and measuring systems (or the system and its environment) is represented by the xyz 3-space. Actually the picture is misleading in the sense that the environment usually has a far huger number of dimensions than the measured system. After a unitary transformation (i.e. 3D rotation) of the initial state A, we reach B. *Projected down* into the measured system degrees of freedom, however, this transformation looks very *nonunitary.* For huge dimensional environments, this kind of thing is very likely to happen.

In the Von Neumann density matrix view, we saw that the Copenhagen idealization of the collapse was equivalent either to zeroing all the off-diagonal terms, or to multiplying them by random phase factors The latter is the more correct view (since it is unitary)– later averaging over that randomness is not unitary, but really is performed by the averaging (or anyhow probability sampling, which is assumed to have a very similar effect of converting a pure state into a mixture) over the initial unknown thermal ensemble of environment states.

But what could be causing the appearance of all these random complex phase factors? Well, suppose you are measuring the position of a particle. That means you somehow set up an interaction between the particle and some very complicated high dimensional measurement system (ultimately, the brain of the human supervising the measurement) such that Schrödinger time evolution will cause a particle in a position eigenstate at $x = 57$ to cause your brain to read "57," which means at least 10^{10} atoms in your brain change around. This means that some very high dimensional unitary transformation is going on, in which as the particle's x degree of freedom changes, the state of your brain follows some exceedingly bizarre curve in 10^{10}-dimensional space, and this curve depends what the initial state of your brain was (which we can only model as a thermal ensemble we have to take samples from). So if the particle were initially in a superposition of two x eigenstates (say at $x = 57$ and $x = 32$) then the effect of the time evolution will be to multiply each of them by, for all practical purposes, exceedingly random phase factors. (It could also alter the eigenstates and their relative probability masses, but if we assume we have a "good" measurement device designed to prevent that, the only effect will be phase angle changes.) Hence the off-diagonal terms of the density matrix, e.g. at $(32, 57)$, will get "dephased" (randomized in phase angle). However, the *diagonal* terms $(32, 32)$ and $(57, 57)$ will be multiplied by both the phase factor and its complex conjugate for no net effect.

Now really, there is no way a particle knows it is being "measured" ("oops! the experimenter just opened his eyes – I'll collapse now!") so it can decide to collapse... interactions between a system and its environment are really acting like "measurements" of something (causing nonunitary-looking dephasing effects) all the time.

Joos and Zeh 1985 had a particularly simple way to model that idea: their environment model consisted of particles (electrons, photons, neutrinos, gravitons) randomly swooping by all the time, causing slight, time dependent interaction potentials to appear. Specifically, an interaction energy ΔE "turned on" during a swoopby time interval Δt would lead to a phase-angle shift of order $\Delta\theta \approx (\Delta E)(\Delta t)/\hbar$. After enough such random interactions, the off-diagonal terms of the density matrix would be totally randomized, but if the swoopbys were rare enough and the coupling energies small enough, then a "diffusion" of the phase angles of the off-diagonal terms would gradually occur. This effect is called "decoherence" and it is an *extremely powerful effect.* Even things like gravitons, which are so weak they have never been detected, ought to result in very significant rates of decoherence. Decoherence occurs more quickly in the Joos-Zeh picture the further (in the position basis) off the diagonal of the density matrix we are, because a particle "superposed with a further away version of itself" will experience "more significantly different" interactions with swooping-by particles. (If a particle and its alter-ego experience the same swoopby interaction potential, then the phase shifts for that off-diagonal term would exactly cancel out.) You can estimate just how powerful it is, by estimating particle fluxes, scattering cross sections, and interaction energies caused by various kinds of assumed environments.

We append Joos & Zeh's table of estimates for the "decoherence time scale" (rough amount of time needed before complete phase angle randomization occurs) in various scenarios.

Different approaches to modeling decoherence has been taken by Unruh and Zurek 1989, by Dekker 1985, Ullersma 1966, and Caldiera and Leggett 1981 1983 1984 1985. Dekker and Ullersma and Caldiera and Leggett all modeled the environment as a huge bath of harmonic oscillators weakly coupled to degrees of freedom in our system. They then attempted to exactly solve (or at least take as far as they could, then resort to various questionable approximations) the solution of the Feynman Vernon picture. In the Dekker and Ullersma cases, the system was also a harmonic oscillator, so that the combined system+environment was a quadratic multivariate potential. By immense labor, the exact eigenfunctions of such potentials can be found under various assumptions about the probability density of various oscillator eigenfrequencies and coupling constants, and then you can try to see what is going on back in the system degrees of freedom. Unruh and Zurek modeled the environment as a classical "stretched string" (really a continuum of harmonic oscillator wave-modes) and under certain rather strange assumptions about the form of the coupling between the test system and the string they were able to take the exact solution rather far before resorting to questionable approximations. Again in the Unruh-Zurek model, decoherence happens faster the more off-diagonal one is, because longer wavelength modes on the string (which is assumed to be a blackbody thermal bath of wave modes) can now exert a decohering effect.

Zurek 1991 in fact derived (under dubious assumptions) a "master equation"

$$\frac{\mathrm{d}\rho}{\mathrm{d}t} = -\frac{i}{\hbar}[H,\rho] - \frac{x-x'}{\tau_R}\left(\frac{\partial\rho}{\partial x} - \frac{\partial\rho}{\partial x'}\right) - \frac{2mk_BT}{\tau_R\hbar^2}(x-x')^2\rho \tag{16}$$

intended to replace the Von Neumann equation. This includes nonunitary dephasing effects caused by the environment. Here T is the temperature of the environment "string." There is a semiempirical "relaxation time" τ_R parameter. Here the first term on the right is the usual Von Neumann term, the second is a dissipative "friction" term (frictional energy loss happens on the τ_R time scale), and the third is a "Brownian fluctuation" term and causes off-diagonal terms to die exponentially on a time scale

$$\tau_D \approx tau_R \frac{\hbar^2}{2mk_BT\,(\Delta x)^2} = \tau_R\left(\frac{\lambda_T}{\Delta x}\right)^2 \tag{17}$$

where $\lambda_T = \hbar/\sqrt{2mk_BT}$ is the thermal de Broglie wavelength. This decoherence time scale can be *much* shorter than the relaxation time.

5 Conclusion

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