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Gravity and quantum mechanics

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0 Introduction

At a conference in honour of John S. Bell, held in October 1991, the following quotation, from a paper by Bell and Nauenberg (1966), was presented (in a talk by Dipankar Home):

> "... the quantum mechanical description will be superseded. In this it is like all theories made by man. But to an unusual extent its ultimate fate is apparent in its internal structure. It carries in itself the seeds of its own destruction."

I was struck by a similarity of sentiment, as expressed here, with one that I have myself often expressed, but now in relation to general relativity; e.g. (Penrose 1991):

> "... in a clear sense, general relativity predicts its own downfall as a complete description of the structure of space-time"

There is, indeed, a remarkable parallel, in this regard, between these two great physical theories. Both theories are now known to be exceptionally accurate, within the range of phenomena to which they are applied; yet both present us with profound difficulties. In the case of general relativity, the profound problem is that of *space-time singularities*, whose presence in Einstein's theory is an implication of the theory itself (cf. e.g. Hawking and Penrose 1970). In the case of quantum theory, the difficulty is the so-called *measurement problem*, which still has no really satisfactory solution. Many different viewpoints are expressed in relation to the measurement problem, often accompanied by claims of some kind of solution - but where the "solution" proposed satisfies none of those holding to an opposing viewpoint.

Despite the fact that *both* theories have their profound difficulties, the normal attitude to them, amongst contemporary physicists, is very different in the two cases. The standard reply to the question of the space-time singularities of classical general relativity is that that theory should be modified by applying to it, in some appropriate way, the rules of standard quantum theory - or, if this fails to work, then the classical Einstein theory itself should be changed (as would be the case with Kaluza-Klein-type theories, supergravity, and the classical limit of superstring theory) so as to force it into a more amenable shape for the consistent application of quantum procedures. When it comes to quantum theory's own problems, on the other hand, the normal attitude among contemporary physicists seems to be that the problem ought just to go away - if we only understood the theory itself properly. There is little suggestion that the very rules of quantum mechanics might in any way be in need of modification, or that quantum mechanics might itself derive any benefit from general relativity. (Isolated expressions of dissent from this general viewpoint have, however, been put forward from time to time; cf. Károlyházy 1966, 1974, Károlyházy, Frenkel, and Lukács 1986, Komar 1969, Diosi 1989, Penrose 1981, 1989.)

My own attitude to these problems is a much more even-handed one than that normally adopted. I believe that the sought-for union between general relativity and quantum theory will involve as much change in the structure of quantum theory as in general relativity. I believe that *both* quantum theory and general relativity are (superb) approximations to some hitherto undiscovered new theory, where each would be valid in some appropriate limiting sense. The solution to the singularity problem and to the measurement problem would then both find their resolutions within this new theory; indeed, the solutions to these two problems should arise, accordingly, as two sides of but a single coin.

1 Quantum theory's fundamental problem

Since the singularity problem of general relativity is now generally accepted as providing a limit to that theory's classical applicability, I shall concentrate here on the central problem of quantum theory. I think that a very profound remark concerning different people's attitudes to quantum theory was made to me some years ago in a dinner-table comment by Bob Wald:

"If you really believe in quantum mechanics, then you can't take it seriously."

This expresses the fact that it is only those who dissent from the standard "Copenhagen" view who are prepared to regard the state-vector $\mid \psi >$ as actually representing (even an approximation to) physical *reality*. Niels Bohr, on the other hand, was one of the strongest proponents of the idea that $\mid \psi >$ was to be regarded merely as a calculational tool, an expression only of our knowledge about a physical system, and to be used simply for the mathematical calculation of probabilities with regard to the various results of "measurements" that might be performed on a system. Bohr, indeed, was someone who really did believe in quantum mechanics, and so was unable to take $\mid \psi >$ seriously as a description of actual physical reality.

Those who *do* take $\mid \psi >$ seriously as an objective description of the physical world - although possibly only a provisional or approximate one, perhaps to be superseded if quantum theory is someday replaced by an even more accurate theory - seem to me to fall into two camps. There are those who believe that the present theory of the way that $\mid \psi >$ evolves, namely *unitary evolution* **U**, must be preserved at all costs, and that the phenomenon of *state-vector reduction* **R** is some kind of illusion; on the other hand there are those who believe that **U** applies only to an approximate (though highly accurate degree) and that **R** represents a real physical phenomenon that effectively interrupts, from time to time, the continuous evolution according to **U** (such as might be entailed by some modification of the Schrödinger equation). In my own view, it is those in this latter camp (and I would count myself among their number) who are taking the formalism of quantum mechanics most "seriously" of all, because they believe that *both* the main ingredients of the theory, indeed **R** as well as **U**, are to be taken seriously as describing something objectively real about the evolution of the world. Among those in the latter camp would be such as Károlyházy 1974, 1986 and his co-workers, Pearle 1985, 1989, Ghirardi, Rimini, and Weber 1986, Diosi 1989 moreover, I would count John Bell as essentially being in this camp.

Belonging to the former camp within the group who "take $\mid \psi >$ seriously" would be those who follow a "many-worlds" viewpoint with regard to the quantum state. Accordingly, $\mid \psi >$ always evolves according to **U**, but all the different outcomes of a "measurement" must co-exist in different "worlds", each being perceived by a separate copy of any observer. The difficulties with this viewpoint lie, to my own mind, not so much in its lack of economy and in its extreme stretching of one's notions as to what "reality" should encompass, but in its incompleteness with regard to its descriptions as to what a conscious observer would actually perceive about the world and about the probability values that such an observer would assign to different perceived events. In short, in itself, it provides no solution to the "Hilbert space basis problem" or to the problem of why squared moduli of amplitudes should actually become probabilities. Also in this former camp might be those who hold to some form of "decoherence" explanation of R, although the standard explanations of this kind, described by John Bell as FAPP ("for all practical purposes") explanations, could really be satisfying only to those who do *not* take $\mid \psi >$ seriously as providing an actual description of the physical world. With regard to those "decoherence" viewpoints, such as those put forward by Griffiths (1984), Gell-Mann, and Hartle (1990), which adopt a path-integral-type picture of reality, I would regard them as being to some extent *modifications* of standard quantum theory (which would place them in the latter camp), but in any case, as not providing a real resolution of the measurement problem. For they accept their own versions of **U** and **R** as things with distinct mathematical descriptions, and they do not tell us under what actual physical circumstances a "measurement" would be deemed as taking place.

2 Two sides to the state-reduction phenomenon

Consider a simple (thought) experiment, where a photon source (the lamp L) and a detector (the photo-cell P) are placed at opposite ends of a hall, with suitable paraboloidal or ellipsoidal mirrors placed so as to ensure that virtually every photon emitted by the source would reach the detector, provided that there is no obstruction on the line joining them (fig. 1). Let us now imagine that there is a half-silvered mirror M placed mid-way between them on this line, tilted at 45° to the line. We are to take it that any photon moving along this line in *either* direction, when reflected off the mirror, would be absorbed at a point of the hall wall (at B if the photon comes from the direction of L, and at A if the photon were to come from P). Suppose that from time to time photons are emitted by L and that from time to time P (assumed to be a 100% efficient detector) registers the reception of a photon. Assume, also, that every time L emits a photon, it registers this fact (again with 100% efficiency), so that all the emission and reception events are clear-cut *measurements*.

Suppose, now, that it is given that L has emitted a photon. We ask: what is the probability that P receives one? Clearly the answer is $\frac{1}{2}$. This is a consequence of the standard quantum rule whereby probabilities are obtained by squaring the moduli of complex amplitudes. The photon wave function splits into two components, each of which has an amplitude $\frac{1}{\sqrt{2}}$ (times a possible phase factor). One component reaches the detector at P, and the other reaches the wall at B. The squared modulus of each is $\frac{1}{2}$, so the respective probabilities of reaching P or B are each $\frac{1}{2}$.

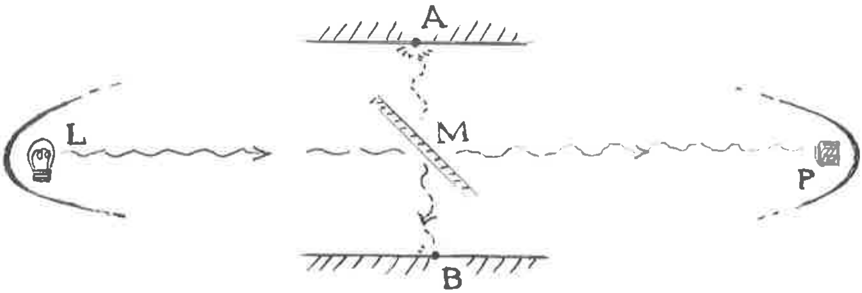

Fig.1 Photons, aimed towards the detector P, are emitted from time to time by the source L. Between the two is a half-silvered mirror M which partially deflects the photons to the absorbing wall at B. A photon ejected from the wall at A also could reach P. The probability that P receives a photon, given that L emits one, is governed by the quantum-mechanical squared-modulus rule, whereas the probability that L has emitted one given that P receives one is determined by the second law of thermodynamics.

Let us now ask a time-reversed kind of question. Suppose that we are given that P has received a photon. We now ask: what is the probability that L had emitted one? Now the answer is certainly not $\frac{1}{2}$, but we have a probability of essentially 1 that the photon came from L and a probability of essentially 0 that it came from A, which is the only other possibility. It is virtually certain that the photon came from the lamp L rather than from the wall at A. However, had we tried to use the "squared modulus" rule for calculating these probabilities, we should have indeed obtained $\frac{1}{2}$ for each of them. This discrepancy has nothing to do with the fact that it is usual to evolve wave functions into the future rather than into the past. Precisely the same answers are obtained whichever way we evolve. The conclusion is that the "squared modulus" rule simply does not work if we try to apply it to obtain retrospective probabilities. There is no real reason why it should. The miracle is that such a simple and elegant rule indeed works in the future time-direction!

If we were to ask what rules indeed govern probabilities in the past time direction, then we are forced to consider such matters as the second law of thermodynamics. This law is certainly playing a role here, because for a photon to jump out of the wall at A in order to be reflected off the mirror and arrive at the detector P, a severe violation of the second law would be needed. Basically it is the second law that is responsible for a virtual vanishing of the probability of a photon emerging from the wall at A.

It is clear from all this that there is no necessity for the probabilities in the past and future directions to match up in a particular experimental situation, such as this. However, there are strong reasons for expecting that if we were to consider, in some appropriate sense, the totality of *all* possible "experimental situations", then there would indeed be a past-future balance for the probabilities, taken as a whole. Basically, we must balance the physics responsible for the second law of thermodynamics against the physics responsible for the quantum-mechanical probabilities - in order to show that these two areas of physics must actually be two different aspects of the *same* physics.

3 The role of the Weyl curvature hypothesis

I do not wish to repeat the entire argument here, since it is one that I have given many times before (see, particularly, Penrose 1981, 1989). The essential point is that the second law arises ultimately from the enormous constraint on the space-time geometry that was operative at the big bang singularity. This initial constraint (which for a 10^{80}-baryon universe would amount to a restriction of the phase-space down to a region whose volume is about exp(-10^{123}) of that of the entire phase space) starts the universe off with a very tiny entropy, as compared with what it "might have been", and the entropy has been rising ever since, in accordance with the second law. The simplest way to impose such a constraint is to take it as a geometrical restriction: the initial Weyl curvature is to be zero - or at least to be much smaller, in some appropriate sense, than it would have been for a generic big bang. This restriction, which is taken to apply only to *initial*, and not to final space-time singularities (so that we can derive the time-asymmetric second law), is referred to as the *Weyl curvature hypothesis* (WCH). (In recent work, R.P.A.C.Newman has shown that a form of WCH due to K.P.Tod can be used to derive an initial Friedmann-Robertson-Walker structure for the early universe, assuming an appropriate perfect-fluid state.)

Thus it is WCH (or something closely of this nature) that we seem to have to balance against the "squared modulus" rule of the **R** part of quantum mechanics. Moreover, if we take it that this singularity structure, as implied by WCH, is a feature of the correct quantum gravity theory - or rather, of the putative correct *new* theory out of which classical general relativity and standard quantum mechanics must both emerge as appropriate limiting cases - then we appear to conclude that it is this new theory that must also be responsible for the probabilities involved in **R**. In other words, the modification of quantum theory that would be needed in order for us to be able to understand **R** as a real physical process must be a modification that operates at the level where gravitational effects begin to become quantum-mechanically important.

In fact the connection can be made more explicit if we consider the "Hawking box" thought experiment (described in Penrose 1981, 1989). In this situation, the phase-space volume gain that occurs with experiments like that discussed in the previous section, where in effect there are more possible outputs than there are inputs (here: P and B allowed as outputs, whereas L is allowed as an imput but A is effectively forbidden), is balanced against a phase-space loss that occurs in black holes. There is a net loss because black holes are allowed by WCH, but their time-reverses - the white holes - are forbidden by it. The black hole singularity is a future singularity which absorbs information, whereas a white hole's singularity would be a past singularity, like the big bang, but with an infinite rather than zero Weyl curvature. The absorbing of information by a black hole's singularity is what is responsible for the Hawking effect, according to Hawking's original derivation (fig. 2). It should be mentioned, however, that there are some opposing points of view, according to which it is argued that if the entire history of a black hole's formation and ultimate disappearance (or possible non-disappearance) due to its Hawking evaporation is taken into account, then information is not actually lost. My own considered opinion is that such information restoration is not plausible, particularly if one is to believe that something of the nature of WCH must hold, so that white-hole singularities, with their potential for creating new phase-space volume, are not permitted.

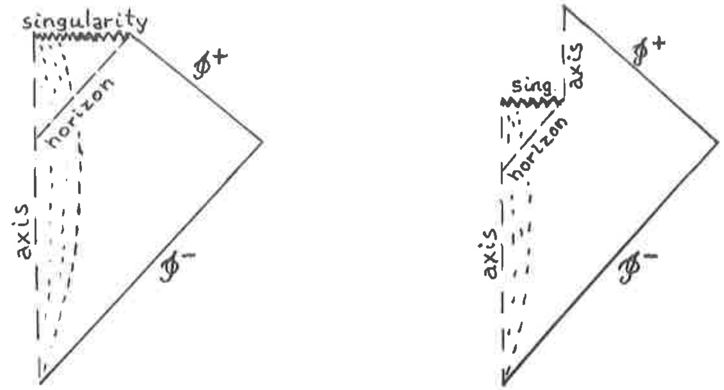

Fig.2 Hawking evaporation. The conformal diagram on the left gives the geometrical background for Hawking's original derivation of the presence of Hawking radiation - as an effect of a loss of information in the Black hole's singularity. The diagram on the right takes into account the final disappearance of the hole from the back-reaction of this radiation on the space-time geometry.

4 A gravitational origin for R?

An implication of the preceding discussion is that we should look for some criterion for the onset of quantum state-vector reduction which is of a gravitational character. There are, indeed, other reasons for suspecting that the standard quantum formalism might not apply without change in situations where the curved-space features of general relativity begin to become significant. For example, the normal ideas of energy, momentum, and angular momentum, in a quantum context, relate these physical concepts directly to symmetries of the space-time manifold, whereas in a general-relativistic setting, such symmetries would normally be absent. This leads to certain severe difficulties with regard to quantization, since it is indeed the quantum rules for energy, momentum, and angular momentum that provide the initial guide to quantization in a normal flat-space setting. (We recall that the passage from a classical to a quantum description - i.e. from a symplectic manifold to Hilbert space - is not a well-defined mathematical procedure, except in the presence of further structure such as that supplied by space-time symmetries; cf. Woodhouse 1980.) Even the notion of positive/negative energy (i.e. frequency) splitting, vital for the setting up of quantum field theory, is not well-defined in a general curved space-time.

These difficulties occur even for the problem of quantizing within a curved space-time background. As is well known, the problems that arise when it is the curvature of space-time itself that has to be subjected to the laws of quantum mechanics are of a much more serious nature. One must ask, in particular, how it is possible to interpret a physical system in which different space-time geometries are being subjected to quantum linear superposition; indeed, even worse than that, one must ask how is it possible to interpret *other* physical objects that try to inhabit such a curious quantum-superposed background? When there is no natural correspondence even between the individual points in the different classical space-times that are to be superposed, then it is hard to see how interference between the different physical states associated with the different geometries can be understood.

What has this to do with state-vector reduction **R**? Let us consider a type of situation like a "Schrödinger's cat", in which one strives to produce a state in which a pair of macroscopically distinguishable alternatives are linearly superposed. For example, in

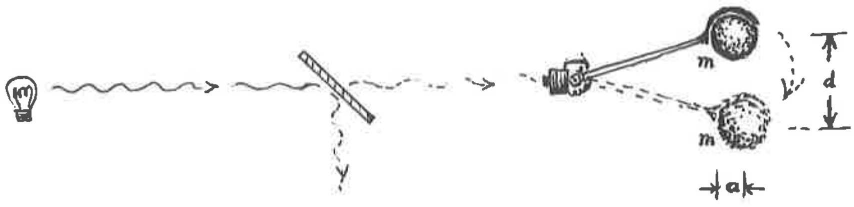

Fig.3 A photon impinges on a half-silvered mirror, so that only the transmitted part of its wave function is received by a device which if activated would move a macroscopic spherical lump of mass m and radius a through a distance d. Is there stage at which linear superposition between the two possible positions of the lump fails, and the lump actually becomes localized in one position or the other?

Fig. 3 we have a situation in which a photon impinges upon a half-silvered mirror, and the photon state becomes a linear superposition of being transmitted through it and reflected by it. The transmitted part of the photon's wave function activates (or would activate) a device which moves a macroscopic spherical lump from one location to another. So long as Schrödinger evolution **U** holds good, the "location" of the lump becomes a quantum superposition of its being in the original position with its being in the displaced position. As soon as **R** comes into effect, we are allowed to consider that the lump is in either one position or the other - and a "measurement" has been performed. The idea here is that this is an entirely objective physical process which occurs whenever the mass of the lump is large enough or the distance it moves is far enough. In particular, it has nothing to do with whether or not a conscious observer may happen to have actually "observed" the movement or otherwise of the lump. (In this, I am imagining that the device that detects the photon and moves the lump is itself "small" enough that it can be treated entirely quantum mechanically, and it is only the lump that registers the measurement. For example, in an extreme case, we might simply imagine that the lump is poised sufficiently unstably that the mere impact of the photon would be adequate to cause it to move off significantly.)

How would such a situation be treated according to the standard **U** procedure of quantum mechanics? After the photon has encountered the mirror its state would have to be considered as a non-local system, with the two parts of its wave function in two very different locations. One of these parts then becomes entangled with the device and finally with the lump. We thus have a quantum state which involves a linear superposition of two quite dfferent positions for the lump. Now the lump will have its gravitational field, which must also be involved in this superposition. Thus, the state also involves a superposition of two different gravitational fields - i.e., according to Einstein's theory, with two different space-time geometries! The question is: is there a point at which the two geometries become sufficiently different from each other that the rules of quantum mechanics must change, and rather than forcing the different geometries into superposition, Nature chooses between one or the other of them to effect the reduction procedure **R**?

5 The weak-field gravitational symplectic integral

As a guide to establishing a plausibe criterion, in accordance with the foregoing general ideas, we consider a certain integral (Fierz 1940), over a spacelike Cauchy hypersurface Σ, which determines the symplectic structure on the function-space of weak vacuum gravitational fields (i.e. massless fields of spin 2 in flat space). This integral (up to a real factor) is:

$$\{K_{(1)}, K_{(2)}\} = \int_\Sigma (\Psi_{(1)ABCD}\bar{\eta}_{(2)A'}^{BCD} + \bar{\Psi}_{(1)A'B'C'D'}\eta_{(2)A}^{B'C'D'})d^3x^{AA'},$$

where the fields (labeled by (1) and (2), respectively) are described by linearized curvatures in Minkowski space

$$K_{abcd} = \Psi_{ABCD}\varepsilon_{A'B'}\varepsilon_{C'D'} + \varepsilon_{AB}\varepsilon_{CD}\bar{\Psi}_{A'B'C'D'},$$

where we have a (Dirac) chain of potentials

$$\begin{aligned} \nabla_{BB'}\eta_A^{B'C'D'} &= \chi_{AB}^{C'D'}, & \nabla^{AA'}\eta_A^{B'C'D'} &= 0,\\ \nabla_{BB'}\chi_{AB}^{C'D'} &= \gamma_{ABC}^{D'}, & \nabla^{AA'}\chi_{AB}^{C'D'} &= 0,\\ \nabla_{BB'}\gamma_{ABC}^{D'} &= \Psi_{ABCD}, & \nabla^{AA'}\gamma_{ABC}^{D'} &= 0,\\ & & \nabla^{AA'}\Psi_{ABCD} &= 0, \end{aligned}$$

all of Ψ_{ABCD}, $\gamma_{ABC}^{D'}$, $\chi_{AB}^{C'D'}$, $\eta_A^{B'C'D'}$ being symmetric in their unprimed and primed indices separately. (See Penrose and Rindler 1984 for the relevant spinor notation.) In vacuum, the divergence of the integrand vanishes, showing that (with suitable fall-off at infinity), the integral is independent of the choice of Σ. The symplectic form $\{,\}$ is closely related to the scalar product $<|>$. We have $< K \mid K >= i\{K, \mathbf{J}K\}$, where **J** multiplies the positive-frequency part of K by i and the negative frequency part by -i. Integrating by parts, we find, schematically, (for fields falling off suitably at infinity):

$$\int \Psi_{(1)}\bar{\eta}_{(2)} = -\int \gamma_{(1)}\bar{\chi}_{(2)} = \int \chi_{(1)}\bar{\gamma}_{(2)} = -\int \eta_{(1)}\bar{\Psi}_{(2)},$$

whence

$$\{K_{(1)}, K_{(2)}\} = -\{K_{(2)}, K_{(1)}\}.$$

The quantity $\gamma_{\dots}$ describes the linearized spin-coefficients or, equivalently, the linearized Christoffel symbols; moreover, the Hermitian part of $\chi_{\dots}$ describes the linearized metric.

We can use this symplectic integral as a measure of the *difference* between two weak (vacuum) gravitational fields. When this difference reaches order unity, all quantities being measured in *absolute units* ($G = c = \hbar = 1$), we can say that the two fields are "significantly different". The idea is, roughly, that when two (weak) gravitational fields are judged as being significantly different according to this criterion, then quantum linear superposition between them cannot be maintained, and the state reduces to one or the other of them - so **R** has been effected!

6 A gravitational criterion for the onset of R

How can we apply this kind of idea to the situation considered in **4.**, as illustrated in Fig. 3? A difficulty is that the symplectic integral of **5.** applies (and is divergence-free) only in the vacuum region, whereas we need it in situations where the lumps are actually present. Any spacelike Cauchy hypersurface Σ must intersect the lump (in both its alternative locations). One point to note, however, is that there is actually no need for Σ' to be spacelike everywhere. It just has to be topologically deformable to a Cauchy hypersurface. As it stands, this is no direct help, but it indicates that we need not feel restricted to spacelike hypersurfaces in applying the integral expression.

What seems suggestive, however, is to perform the integral over a *timelike* hypersurface Σ' in a region between the two locations of the lump after they have separated from one another, as indicated

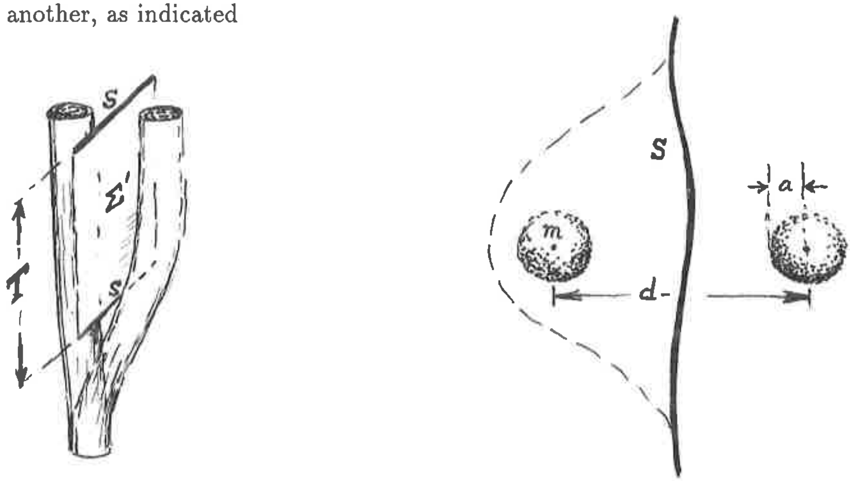

Fig.4 The two alternative locations of the lump, as depicted in Fig.3, are indicated, on the left, in a space-time diagram. The symplectic integral is performed over a portion of timelike hypersurface, suggesting that a criterion for reduction to take place is when this integral becomes of order unity. The spatial configuration is depicted on the right.

in Fig. 4. The hypersurface Σ' is bounded between two times, and let us call the time-interval between them T. A tentative suggestion might now be that the system reduces in a timescale T such that the symplectic integral, applied between the gravitational fields of the two possible positions of the lump, is of order unity.

The situation is basically a Newtonian one, assuming that, compared with the speed of light, the lump moves slowly, and the gravitational escape velocity at its surface is also tiny. In the Newtonian limit, for an essentially static situation, and taking a hypersurface Σ' that is also static - i.e. the product of a spacelike 2- surface S with an interval of the time axis (of duration T) - our symplectic integral becomes (some simple constant multiple of)

$$T \int_S (\phi_{(2)} \vec{\nabla} \phi_{(1)} - \phi_{(1)} \vec{\nabla} \phi_{(2)}) \bullet d\vec{x}.$$

Here ϕ denotes the Newtonian gravitational potential due to the lump. The parenthetic suffix specifies the lump location. We note that the integrand is divergence-free in vacuum, so the integral remains unchanged under topological deformations of S through regions of matter-free space. Where lump matter is present (density ρ), we make use of

$$\vec{\nabla} \bullet (\phi_{(2)}\vec{\nabla}\phi_{(1)} - \phi_{(1)}\vec{\nabla}\phi_{(2)}) = -4\pi\phi_{(2)}\rho_{(1)} + 4\pi\phi_{(1)}\rho_{(2)};$$

thus, moving S across one of the two possible locations for the lump, we find that the value of our integral is $4\pi T$ times the gravitational energy of one instance of the lump in the gravitational field of the other. This would suggest that the time that it takes for the state to reduce is a simple multiple of the reciprocal of this energy.

This energy is

$$m^2/d,$$

where the lump has mass m, and the spatial displacement between the centres of its two alternative positions is d. However, the reciprocal of such a measure is not really a satisfactory suggestion as to the time it takes for reduction to occur in this situation, because for large displacements the measure gets smaller and tends to zero. Thus the suggested "time" would get longer, making it *less* likely for **R** to occur, the greater the displacement, rather than *more* likely as one would expect. Moreover, when the two instances of the lump overlap, there would be no way of locating Σ' so that it lies entirely in the vacuum region.

This suggests that the reduction time should be something a little different from this, but perhaps belonging to the same order of ideas. In fact a slight modification of this expression (arrived at some ten weeks following the Cordoba meeting) does give something reasonable. The expression m^2/d is the gravitational energy gained (in absolute units) if one moves one instance of the lump in from infinity, in the gravitational field of the other, until it reaches the required separation d. If we consider, instead, the more relevant energy of moving one instance of the lump away from the other, starting from coincidence, until they reach the required separation, then, taking the lump to have radius a, we obtain something of the order of m^2/a for this energy. Although the energy now indeed increases as the separation increases, the additional energy in moving from the contact position to all the way out to infinity is of the same order as that in moving from coincidence to the contact position. Thus, as far as orders of magnitude are concerned, one can ignore the contribution due to the displacement d, and take the reduction time to be of the order of

$$a/m^2.$$

It is reassuring that this gives very "reasonable" answers in certain simple situations. For example, in the case of a nucleon, where we take a to be 10^{-13}cm, which in absolute units is about 10^{20}, and m to be about 10^{-20}, we get a reduction time of 10^{60}, which is about a Hubble time. If we consider a droplet of water of radius 10^{-5}cm, we get a reduction time of about a day; if of radius 10^{-4}cm, the reduction time, according to this scheme is roughly a second; if of radius 10^{-3}cm, then about 10^{-5} of a second. So far, this seems to be quite plausible, but clearly more work is needed to see whether the idea will survive more stringent examination.

I am grateful to many colleagues for valuable comments, most particularly Abhay Ashtekar and Ted Newman.

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