PHYSICAL REVIEW D **73,** 064025 (2006)

Why Eppley and Hannah's thought experiment fails

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(Received 31 January 2006; published 17 March 2006)

It is shown that Eppley and Hannah's thought experiment establishing that gravity must be quantized is fatally flawed. The device they propose, even if built, cannot establish their claims, nor is it plausible that it can be built with any materials compatible with the values of c, $\hbar$, G. Finally the device, and any reasonable modification of it, would be so massive as to be within its own Schwarzschild radius—a fatal flaw for any thought experiment.

DOI: 10.1103/PhysRevD.73.064025 PACS numbers: 04.20.Cv, 04.60.−m

I. INTRODUCTION

Physicists (and philosophers and other lay readers of physics) have by and large accepted claims to the effect that the gravitational field must be quantum mechanical in nature. Remarkably, the evidence for these claims is extremely thin, and indeed what there is is extremely dubious. For general analyses of the failure to establish the case for quantization, see the discussion in [1–3].

Even though there is little experimental verification that the gravitational field is quantized, it seems that both physicists and philosophers take very seriously two separate experiments that apparently establish that gravity is quantum mechanical. Widely cited in discussions of the necessity for quantizing gravity are Eppley and Hannah's [4] and Page and Geilker's [5] experiments. While Page and Geilker do carry out their experiment, the significance of their result was called into question even before the experiment was performed, by the very person (Kibble [6]) who suggested the experiment in the first place. I will not here discuss their experiment since its significance has been decisively undermined [1–3,6], and because it applies only to one specific version of semiclassical gravity, semiclassical general relativity.

While Eppley and Hannah's experiment has not been performed the issue of its significance is more delicate. They proposed a brilliant thought experiment to demonstrate the invalidity of *any* nonquantum mechanical version of gravitation theory that is general relativistic in the nonquantum limit. Their detailed analysis is meant to establish that *any* nonquantized gravitational theory is inconsistent with either the first signal principle of special relativity, or momentum conservation, or Heisenberg's uncertainty principle. This would be a profound result. I will show however that their experiment cannot be carried out. In particular, in any experimental situation suitable to producing the results they require, the device they use to measure these results cannot be built *in principle*. We might offer therefore, in analogy with the cosmic protection hypotheses that there are no naked singularities, the semiclassical protection hypothesis that possible inconsistencies in semiclassical gravity are hidden from observation.

The result here presented is important because Eppley and Hannah's paper, in particular, has had a significant impact on all future discussions of the question of quantizing the gravitational field. There are few discussions of the evidence that gravity is quantized that do not appeal to Eppley and Hannah's result, and many take their experiment to have settled the question. Here I will not directly challenge the view that the gravitational field really is quantum mechanical. Instead I will merely show that one important pillar supporting that view is without foundation. One important reason to reconsider this experiment is that it gives a misleading picture of what stands in the way of a nonquantized theory of gravity. Showing that Eppley and Hannah's method fails may prompt the development of a different, more successful experiment. And that experiment might itself be a useful pointer toward quantum gravity.

II. EPPLEY AND HANNAH'S THOUGHT EXPERIMENT

In 1977 Eppley and Hannah [4] proposed their thought experiment to show that the gravitational field must be quantized. The experiment uses a gravity wave to measure the position and momentum of a macroscopic body such that $\Delta p_x \Delta x < \hbar$ thus violating the Heisenberg uncertainty principle. The key idea is that a classical wave may have arbitrarily low momentum and, simultaneously, arbitrarily short wavelength. To find a conflict with quantum mechanics they couple a short wavelength/low momentum gravitational wave to a quantum system. The gravitational wave may then be used to localize a particle within one wavelength while introducing vanishingly small uncertainty into the particle's momentum.

Eppley and Hannah are well aware that a thought experiment must be in some sense realistic, and they adopt explicit standards: While the experiment may never be carried out—and indeed we may never even possess the engineering skill or the raw power to carry it out—it still must be physically possible to do so within the bounds of

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the theory employed for the description of the device—in this case the semiclassical theory of gravity. They say,

“we want to show that the experiment is possible in principle, in the sense that it does not require any masses, lengths, or times greater than those of the universe,”

and they conclude

“our experiment is fantastically difficult to perform, but nevertheless in principle possible.”

I agree with Eppley and Hannah that they need merely show that their experiment is possible in principle. I will adopt their standard—that they need to show that no “masses, lengths, or times greater than those of the universe” be required. Still I claim that their experiment *cannot* be performed even in principle.

We must also keep in mind that the burden of proof lies squarely on Eppley and Hannah. Proposing a thought experiment requires experimental controls at least as stringent as those on an ordinary experiment. It will not do to say what the experiment would have shown were it possible to construct in principle. It must be shown that the experiment really is possible in principle. For well-established physical principles such claims can be established quite casually. If, for example, a thought experiment requires that some structure be imagined to have been built according to standard construction techniques, then we need not demand an exhaustive demonstration that the structure is possible in principle. But for more exotic claims we do. If, for example, an experiment requires some exotic material, we must demand that it be demonstrated that such materials can be found given our current understanding of the laws of physics. Suppose we need a stable lump of some quantity of radioactive material. We must then show that that amount is less than the critical mass for that material.

It is not necessary to show that no thought experiment could ever demonstrate that gravity must be quantized. Instead we need only show that Eppley and Hannah have not delivered on their promise to produce such an experiment, or even one that requires only trivial and easily generated modifications. What follows will establish that Eppley and Hannah have not made good on their claim that their experiment can be performed. Thus one of the best arguments for the quantization of gravity fails.

A. Their argument

I will consider the construction of the device shortly. First I lay out their argument and describe an auxiliary experiment that must be performed in order for them to make their case.

Beginning with a poorly localized particle of sharply defined momentum Eppley and Hannah consider a gravitational wave scattering from the particle. They claim that there are two exhaustive and mutually exclusive cases.

Either the scattering event constitutes a position measurement of the particle with a consequent collapse of its position wave function to a smaller region of localization, or else it is not a position measurement and does not result in collapse.

They argue that each of these cases will conflict with well-entrenched physical principles, and hence that the coupling of a classical field to a quantum field is impossible. They take it for granted that the “first signal” principle of special relativity holds. That is, they claim that signaling “across arbitrary spacelike distances … clearly violates relativity.” Likewise they appeal to conservation of momentum and to the Heisenberg uncertainty principle. There are two cases to consider. I consider them in reverse order.

1. Case 2

Suppose that, rather than collapsing the particle’s wave function, the gravitational wave merely registers the presence of the particle’s wave function. A measurement of a particle’s wave function in this way would then allow superluminal signaling. For before measurement one can split the position-space wave function of the particle into two spacelike separated parts. Then one can measure the shape of the wave function in one region. Depending upon whether one finds no wave function, the full wave function, or half of the wave function, one can tell whether someone else has either measured the other part of the wave function (by normal quantum mechanical means) and found the particle, measured and not found the particle, or not made a measurement at all, respectively. Thus there is a prescription for superluminal causation. One can (since we are assuming here that collapse is instantaneous) tell instantly the state of the wave function in a spacelike separated region. So on a collapse interpretation of quantum mechanics we must reject this possibility.

2. Case 1

Suppose instead that the gravitational wave does collapse the particle’s wave function. Then, Eppley and Hannah argue, the particle is now localized within one wavelength of the gravitational wave. By determining the point of interaction, we can attribute a sharply defined position to the particle and, since we introduced arbitrarily little momentum disturbance into the system, it should still have a well-defined momentum. We are presented with two subcases: either the interaction conserves momentum but the uncertainty principle is violated (since now the particle has well-defined momentum and position), or if the uncertainty principle is not violated then the interaction does not conserve momentum.

Of the latter subcase they say, “If the classical probe gives the particle a very good position localization, then quantum mechanics implies that the particle is now in a state of very high momentum. If the quantum description of the particle is valid, then momentum is not conserved, since the momentum of the initial quantum state was very well defined and the classical probe imparted negligible momentum." This argument has been challenged in a number of places [1–3], and I will not pursue it here. I will instead grant for argument's sake that if the gravitational wave does collapse the particle's wave function it localizes it while preserving its sharply defined momentum state, in apparent violation of the uncertainty principle.

3. A necessary auxiliary experiment

Even here, though, establishing a violation of the uncertainty principle would require the possibility of performing an interference experiment of some kind—like the two-slit experiment. Eppley and Hannah implicitly agree with this view, claiming that the uncertainty relations are violated because "a beam of such particles sent through an arbitrarily narrow slit would show no diffraction effects, contrary to fact." But their "particles" have such small wavelengths with respect to their size, that no interference experiment of that kind is possible. Their "arbitrarily narrow slit" would have to be much narrower than the dimensions of the bodies, and therefore there could be no transmission through the device, much less scattering effects. Perhaps it will be suggested that this experiment could be done with smaller bodies that can, in principle, be diffracted. As far as I know the largest bodies for which a diffraction measurement is possible have masses on the order of 100 000 neutrons (a very complicated mesoscopic molecule). Whether this is a matter of principle is unclear, but at least for the devices considered by Eppley and Hannah, and for bodies of this mass, the radius of the Eppley and Hannah interference device needs to be roughly 10^{59} cm. On the other hand, the radius of the universe is of order 10^{28} cm. So even in principle we could not detect just one, let alone an entire ensemble of such particles. One might consider other ways to measure interference effects on macroscopic bodies. Is that possible? Without giving a detailed method that is possible in principle—the whole point of their article—we cannot conclude that it is. I believe most would assume that, given the small value of $\hbar$, such experiments are impossible in principle. In any case Eppley and Hannah have not *shown* that the uncertainty principle would be violated.

Even being able to interfere such macroscopic bodies is not enough though. For the experiment to show a violation of the uncertainty principle it is necessary that the position-space wave function not spread between the time that the particle is localized and the time it is sent through the interference device. That is to say that, if indeed we have produced particles with $\Delta p \Delta x \leq \hbar$, there is every reason to suppose that the wave function will spread again. This spreading occurs rapidly enough that to ensure experimental violation of the uncertainty principle would require an additional measurement protocol unaddressed by Eppley and Hannah, in order to assure that the spreading had no effect on their experiment. The possibility of such an auxiliary protocol simply has not been established. Normally of course one does not need to worry about such issues, but if we are assuming that the experiment only functions when the gravity wave collapses the wave function of the particle, then we must consider whether properties of that collapsed state can be maintained long enough to measure them. In the context of a thought experiment the burden is on the experimenter to show that all parts of the experiment, including this auxiliary component, are possible in principle. Eppley and Hannah have not done so. Without a characterization of the experiment, and good reason to think it would have an outcome favorable to their claims, we have not seen that well-established physical principles show that their experiment functions as advertised.

There are thus reasons, even in advance of a consideration of their device, to think that their experiment is impossible in principle.

B. The device

I will level four objections against their device. One involves materials, one involves the temperature, one involves preparations, and the last involves gravity itself. The first three are quite serious and prevent their detector from functioning the way they claim it should. I doubt strongly that these objections could be overcome in any modification of their device. The fourth objection is damning, and I show that any modification of their device to mitigate the effect of this problem, itself reintroduces the problem. These objections show that their thought experiment cannot be considered persuasive evidence that gravity must be quantized.

One might argue that the first three objections could be met in another universe. That however would itself require significant argument. Are the initial conditions of the universe contingent or lawlike? I do not know, and nor does anyone else. Mach's observation that the universe is only given once may not underwrite his sweeping rejection of any counterfactual claims about its initial state, but his observation is germane here nonetheless. The issue is whether nonquantized gravity is possible in *this* universe, so any appeal to alleged facts about universes in general needs careful analysis that is beyond the scope of this note, and which is not undertaken in Eppley and Hannah's proposal. No reason has been given to suppose that the existence of universes (spacetime manifolds) where semiclassical gravity is impossible has any bearing on the issue of its possibility simpliciter. I am skeptical of such a conclusion especially since there are results that apparently establish the consistency of semiclassical gravity in 2D and 3D spacetime—the only cases that are mathematically tractable. [See for example Wald's (1994) discussion [7] and references therein.] The fourth objection cannot be met in any universe where c, G, and $\hbar$ play the same role governing the behavior of matter as they do in ours, whatever their particular values. And various "fine-tuning" arguments seem to suggest that values very close to those they do have are necessary for the existence of stable matter at all. And yet even showing that gravity is quantized in some universes still would not establish it for our universe.

Here is how the device is supposed to operate. Eppley and Hannah wish to localize sharply a mass that has a precisely defined momentum. They do this by scattering a classical gravitational wave of very low energy and very short wavelength from the mass. In their Appendix C, Eppley and Hannah attempt to show that the device can be built—at least in principle. As I mentioned above, this consists, for their purposes, of showing that the universe contains enough mass, is large enough, and will last long enough. Here is what is required for the experiment:

1. A (small) mass that will be localized using a gravitational wave.
2. A method for giving the mass a small and very well-defined initial momentum.
3. A gravitational wave generator to produce short wavelength, low energy radiation.
4. A detector array to measure the new trajectory of the gravitational wave to determine the location of the interaction region.

Requirement (1) is easily enough accommodated. From the earlier part of their article, one would assume an uncharged particle, a neutron for example, should suffice. Instead Eppley and Hannah use a 10 g mass. The reason for this is that, as they say, "if we wish to keep the detector mass M_{tot} within limits, we need to make the masses of the generator and the probed particle as large as possible." As I pointed out above, we have no hope of diffracting a beam of particles this large. From their equation (A2) (see also [8]), we find that the radius of the detector R is linear in the distance of closest approach and inversely as the mass of the object to be localized. If we use a molecule of mass $\approx 100 m_p$ where m_p is the proton mass, we find that, rather than needing a detector radius merely of approximately 10^{15} cm we need instead a radius of order 10^{37} cm. Since the mass of a spherical shell detector goes like the square of the radius, and their original detector was roughly 1000 galactic masses, we see that the new detector would mass at least 10^{47} galactic masses. There is not *this* much mass in the universe even for the best case estimate. I will leave this issue aside for now and return to the detector actually proposed by Eppley and Hannah.

C. Materials

The first problem the idealized experimenter faces is one of materials. Because the energy of the gravitational wave used to localize the particle is so low, the wave itself is very difficult to detect. Eppley and Hannah require extremely sensitive detectors. They use very loosely bound mechanical oscillators—i.e. masses joined with very weak springs. How weak? They never say explicitly. We are given that the ground state frequency of the oscillators is of order $\omega_0 \sim 10^{-5}\ \mathrm{sec}^{-1}$. This gives a ratio of $\sqrt{k/m} \sim 10^{-5}\ \mathrm{sec}^{-1}$, where k is the spring constant of the oscillator. For their estimates of the total mass of the detector array, we can conclude that the mass at each end of the spring is approximately 1 g. But it may be possible to vary this by an order of magnitude in either direction. What does this give for a spring constant? For $m = 1$ g, $k = 10^{-10}\ \mathrm{g/sec}^2$. This truly is a loosely bound oscillator. Typical spring constants of the type described by Eppley and Hannah have $10 \leq k \leq 10^{66}\ \mathrm{g/sec}^2$. The smallest spring constant I know of is for certain extended polymers where $k = 2 \times 10^{-3}\ \mathrm{g/sec}^2$ [9]. Considering that c and $\hbar$ presumably limit the possible values of spring constants, it is incumbent upon Eppley and Hannah at least to suggest that their value is possible. Everything we know about springs suggests not only that their value is not feasible, but that it is not possible *even in principle* [10].

D. Temperature

The springs pose another problem for Eppley and Hannah. Simply put, the springs are *too* loosely bound. Or rather, given how hot it is in the universe, the springs will be too excited to allow the idealized experimenter to determine if and when the scattered gravitational wave has interacted with one of the springs.

The detector array envisioned by Eppley and Hannah is a closely packed spherical shell of the harmonic oscillators described above. For the gravitational wave to be detected, it must interact with one of these oscillators and excite it in such a manner that its excited state can be distinguished from its normal operating state. (The details of the following discussion come from [4].) We require that the transition time be known with precision of order $\lambda/c \sim 10\ \mathrm{cm}/10^{10}\ \mathrm{cm/sec} = 10^{-9}$ sec in order that the particle's position be measured to accuracy $\Delta x \sim \lambda$. To measure the final state of the detector we would need a time of order $1/\omega_0 \sim 10^5$ sec, much too great for our purposes. But if the oscillators begin in the ground state we can simply determine whether the state of the detector is above the ground sate, and that allows a much shorter observation time. Therefore, Eppley and Hannah do not attempt to measure the energy of the oscillators to great precision. Rather they measure a broad band of energies. The lower edge of this band is above the ground state energy, but the width of the band is much greater than ω_0. Thus the time required for the measurement can be made as short as necessary. The problem with their argument is in the assumption that they only need to detect oscillator states above the ground state. In fact, virtually all of the oscillators making up their detector will be in highly excited states.

They calculate the necessary physical characteristics (and the requisite number) of the oscillators that will allow a probability of order 1 that the gravitational wave excites one of the oscillators. They find that their detectors should have linear dimension of order 1 cm with a period of order 10^5 sec. This gives an angular frequency of $\omega_0 \sim 10^{-5}/\mathrm{sec}$. Standard quantum mechanical calculations give the ground state energy of such oscillators as $E_0 = \hbar\omega_0 \sim 10^{-20}$ eV. For a quantum oscillator in a heat bath of temperature T, the expectation value of the energy is given by

$$\langle E\rangle = \frac{\hbar\omega_0}{2} + \frac{\hbar\omega_0}{\exp(\beta\hbar\omega_0) - 1}.$$

Here $\beta = \frac{1}{kT}$, where k is Boltzmann's constant. Taking $T = 2.7$ K, the cosmic background temperature, we find

$$\langle E\rangle \sim 10^{-20}\ \mathrm{eV} + \frac{10^{-20}\ \mathrm{eV}}{\exp(10^{-20}\ \mathrm{eV}/10^{-5}\ \mathrm{eV\,K^{-1}}2.7\,\mathrm{K}) - 1} \sim 10^{-20}\ \mathrm{eV} + \frac{10^{-20}\ \mathrm{eV}}{\exp(10^{-15}) - 1} \sim 10^{-20}\ \mathrm{eV} + \frac{10^{-20}\ \mathrm{eV}}{1 + 10^{-15} - 1} \sim 10^{-5}\ \mathrm{eV}.$$

Eppley and Hannah give the expectation value of the energy absorbed by an oscillator from the gravitational wave as

$$\langle E_{\mathrm{absorbed}}\rangle = 10^{-1} m_{\mathrm{osc}} L_{\mathrm{osc}}^2 \hbar G / R_{\mathrm{detector}}^2 \lambda^2 c.$$

Their estimate of the density of oscillators at the detector shell is 10 g/cm^3, so we can take $m_{\mathrm{osc}} \approx 10$ g. The detector radius, $R_{\mathrm{detector}} \sim 10^{15}$ cm, L_{osc} was given above as $\sim$1 cm, and $\lambda \sim 1$ cm. Thus

$$\langle E_{\mathrm{absorbed}}\rangle \sim \frac{10^{-1} 10\ \mathrm{g}\ 1\ \mathrm{cm}^2 10^{-16}\ \mathrm{eV\ sec}\ 10^{-7}\ \mathrm{cm}^3/\mathrm{gm}\cdot\mathrm{sec}^2}{10^{30}\ \mathrm{cm}^2\ 1\ \mathrm{cm}^2 10^{10}\ \mathrm{cm/sec}} \sim 10^{-63}\ \mathrm{eV}.$$

Eppley and Hannah's detector must make the probability of exciting *one* of the detectors by *one* increment of $\hbar\omega_0$ of order unity. But with the detectors, on average, in an energy state 10^{15} times the ground state, we cannot use their expedient of looking at a wide energy interval the lower edge of which is higher than the lowest unexcited state. For here we expect *lots* of the oscillators to be in states higher than $\langle E\rangle$ (around $N/2$ of them, for N total oscillators). No longer can finding an oscillator above its ground state establish that it has registered the gravitational wave. Instead, we must observe the phase characteristics of the oscillators' wave functions. Now we will not be able to detect an interaction on anything like the short time scale required for accurate determination of the interaction time, a crucial component of the experiment. The experiment fails.

Thus at the current temperature of the universe, there is no way to make the experiment work. One might suppose that we can simply refrigerate the (10^6 cubic astronomical unit) region of the experiment. This might work. To make the experiment successful, one would need to cool the region sufficiently that temperature effects are unimportant. The reference Eppley and Hannah use [8] suggests that we would need to make $\hbar\omega_0 \gg kT$ so that $[\hbar\omega_0/\exp(\beta\hbar\omega_0 - 1)] \ll 1$, i.e., $T \ll 10^{-16}$ K. Even within the confines of a thought experiment, such a low temperature over such a large region seems out of bounds. Moreover, this refrigerator would have to be fantastically large itself. Could such a device be built in principle? Is there time enough and mass enough to build the refrigerator, and allow it to work? It is doubtful. Naturally one could wait until the universe itself cools to a temperature of this order. There are three problems with this suggestion however. First, if the universe is closed—and thus will "bounce back" at some finite time—the universe should never become this cool. Second, if the universe ever were at a temperature of order 10^{-16} K, there is no indication that enough free energy would be available to construct Eppley and Hannah's device. In any case, we need a concrete argument that shows the possibility of cooling the experimental region before the experiment can be considered performable in principle.

There is, moreover, a more conjectural reason to think that temperature will be necessarily too high: The mass of the device itself may make the springs too hot. The Unruh effect implies that an observer in a gravitational field will experience a thermal bath of temperature $kT = \hbar a/2\pi c$. A mass constrained to the surface of a sphere experiences an acceleration given by the surface gravity, since in general relativity the mass is constrained to *deviate* from its proper geodesic. A naive application of the Unruh effect therefore implies that each of the detector oscillators experiences a heat bath due to the mass of the detector array. For Eppley and Hannah's device, the surface acceleration is $a \sim G \times 1000 m_{\mathrm{galaxy}}/10^{15}\ \mathrm{cm}^2$ so at the surface $kT \sim 10^{-14}$ eV. Thus $T \sim 10^{-10}$ K, making the device itself much too hot for us to perform the experiment, even in principle.

E. Preparation

To localize the 10 gm test particle in momentum space, with its position uncertainty greater than its linear extent, requires an auxiliary experiment. For this Eppley and Hannah propose to measure very precisely the momentum of a proton, scatter it off the particle, and then measure its momentum again. Since the test mass is effectively infinite compared to that of the proton, we can take the x component of its velocity to be $v_{fx_p} + v_{ix_p} = 2v_{x_{tm}}$ where v_{fx_p} is the final x velocity of the proton v_{ix_p} its initial x velocity, and $v_{x_{tm}}$ is the x velocity of the test mass. To determine these quantities within the lifetime of the universe, and within its observable radius (as Eppley and Hannah demand for their experiment) requires a diffraction grating of linear extent 10^{26} cm and a measurement time of 10^{17} sec. For a larger mass, the uncertainty required in the proton's velocity decreases proportionally to the increase in mass. The detector extent $W \gtrsim \hbar/(m\Delta v)$ is invariant under changes in test particle mass. The question of whether there is enough mass in the universe to manufacture such an array, and what effect its mass would have on the remainder of the experimental apparatus is not a negligible issue. But I will not pursue that question here. Instead let us focus on their diffraction grating. The required spacing of the scattering centers is $d \sim 10^{-13}$ cm, and this is tightly constrained by cosmological considerations. But this is impossible for ordinary matter. Crystal spacing is of order atomic radius. For hydrogen $R \approx 0.37\ \text{Å} = 0.37 \times 10^{-8}$ cm or approximately 10^5 times too great. Again it seems incumbent on Eppley and Hannah to show that such a preparation is possible given the materials allowed by the laws of physics.

Upshot

I have offered good grounds to believe that Eppley and Hannah's experiment cannot work. To convince us that the experiment could be performed *in principle*, they would need to provide arguments that show (1) c, G and $\hbar$ together allow materials with the requisite spring constants; (2) the entire experiment can be refrigerated sufficiently to allow reliable detection of the gravitational radiation used in the experiment; (3) appropriate auxiliary measurement protocols could be devised, and the devices to carry them out could be constructed. Instead of these arguments, a *new* device could be "constructed," that is, a *new* experiment could be performed—one that uses materials that are possible in principle and takes into account the temperature of the region of spacetime occupied by the device.

I cannot comment on the possibility of the latter option—a new experiment—but I *can* say that the arguments suggested in the former option—establishing the possibility of the old device—is itself hopeless. For, even granting, as we should not, the in principle existence of their ultra-high-tech materials and super-refrigerators, the experiment cannot (meaningfully) be conducted. The entire device, it turns out, sits inside its very own black hole.

F. Gravity itself

1. Their detector is in a black hole

The mass of the detector array is sharply constrained by the demands of the experiment: R must be large enough that the angular resolution set by the detector elements and the radius together is fine enough; the density of the detector elements must be such that there is an appreciable probability of a detection event. Eppley and Hannah's detector masses approximately $1000M_{\text{galaxy}}$, given by $M_{\text{tot}} \sim R^3_{\text{detector}} \times \rho_{\text{oscillators}} \sim 10^{15}\ \text{cm} \times 10\ \text{gm/cm}^3$. Thus its Schwarzschild radius is of order 10^{19} cm. Since their estimate of the detector's mean radius is only 10^{15} cm, the Schwarzschild radius is 10 000 times that of the detector. But if a mass is contained within its Schwarzschild radius, it is inside a black hole. Whatever else we can say about this experiment, it should be obvious that this is a serious problem. How, for example, does one communicate the results with the outside? Our experimenters, whatever they observe, are completely cut off from the rest of the universe. Moreover, the gravitational stress energy is enormous. The linear approximations Eppley and Hannah use to derive the sensitivity of their oscillators cannot hold good in the interior of a black hole. Further, since null rays (e.g. gravitational waves) are trapped inside the black hole, the experimenter would have a real problem establishing the time of interaction. Why? If we suppose that the gravitational wave propagates out as far as the detector array, we know that it should return to the array rather than escaping to the exterior of the black hole. The question then becomes, "did it interact with the detector on the way out or on the way back in?" Naturally this problem presupposes that there is still a detector to consider. Matter that forms a black hole very rapidly collapses to a central region of extremely high density. It is then impossible that the detector, as such, would survive its own construction.

2. Possible modifications

(1) Could we not simply enlarge the detector and avoid the whole problem of gravitational collapse? No. The mass of the detector as a whole has to increase at least as fast as the square of the radius. Simply put, as the detector radius increases, the surface area increases and we need more detectors to ensure a reasonable probability of detection. But the Schwarzschild radius increases linearly with the mass, that is, with the square of the detector radius. So increasing the radius gets us into worse shape—

the detector is further and further inside its own Schwarzschild radius.

(2) What about Eppley and Hannah's suggestion that we can focus the gravitational radiation? If we do that, then the total mass is an upper limit, and need not reflect the ''real'' quantity of mass we need to use. The first problem with this suggestion is that to do so requires even more mass. To focus gravitational radiation, there is no substitute for mass. Putting this issue aside for the moment, we can ask how much mass we expect to save through focusing. Notice that using only a small portion of the detector does not help by itself. For example, if we focused the radiation into 1% of the detector, then we would need only 1% of the mass. But the effective radius of the new detector (the radius within which all the mass would lie) is also only reduced to 1% of its earlier value. But we *would* pick up a decrease in the required mass density if we could focus all of the radiation into the reduced detector. For we would need only 100th as many detectors, the density of radiation being increased by a factor of 100. This is really the lower limit on how much of the radiation we need to focus—a factor of 100 reduction in the mass density would bring us just outside the Schwarzschild radius.

It is a tricky problem in general relativity to determine if suitable gravitational lenses could be developed for this application. The problem is that, at least for a spherical focusing mass, the angular deflection of a given ray goes roughly like M, the mass of the body, while the capture cross section for all rays (i.e. the likelihood of being absorbed by the lensing matter rather than propagated) goes like M^2 [11]. Thus we reduce the luminosity of the radiation by absorption faster than we increase its energy density by focusing. Without a concrete proposal for how to focus the gravity waves without damping out the signal, it is not possible to say that Eppley and Hannah have shown in principle how to keep the mass of the device within physically possible bounds. Simply claiming that one could focus the radiation does not show that one could really do so. Even coordinating so massive a lens with the rest of the detector—setting it far enough away not to distort the detector, but close enough to produce measurements on usable time scales—is not clearly possible in principle.

Moreover the 99% reduction in mass density given above is not really enough. As pointed out before, to measure a violation of the uncertainty principle requires that we observe a beam of particles that does not diffract, even though the individual members of the beam should—and Eppley and Hannah implicitly assent to this. Thus we need to reduce the mass density even further. Earlier I suggested a total mass of about 10 000 neutrons as the upper limit for a diffraction experiment. We will see that even 10^6 would not be large enough. Suppose we reduce the mass of the measured object to 10^6 times the mass of the neutron—yielding a mass of about 10^{-18} gm. Then (see Eppley and Hannah's equation C8), the total mass of the detector (in the absence of focusing) becomes 10^{39} galactic masses. Ignoring the question of whether there is 10^{39} galactic masses worth of matter in the universe (there is not), we need now to focus not merely 99% of the gravitational radiation into 1% of the detector, but $(10^{38} - 1)/10^{38}$ (or essentially 100%) of it. That is, if we use the same size sector of the detector, and if we wish to avoid gravitational collapse, we must not introduce more than 10 galactic masses into that sector. A massive enough lens to focus all the radiation into such a small sector is certainly out of the question; a lens that does that without absorbing an appreciable fraction of the beam energy is, apparently, impossible even in principle.

G. Outlook

Eppley and Hannah claim that their thought experiment would demonstrate the inadequacy of any semiclassical theory of gravity. As has been shown elsewhere [1–3] this claim is itself suspect. I have shown here that, regardless of how we interpret the results of the experiment, the experiment itself cannot be performed—even in principle. Their ''experiment'' thus provides no evidence that the gravitational field must be quantized. And thus, one of the most influential attempts to show that gravity is quantized fails.

I have not claimed that no experiment like the one envisioned by Eppley and Hannah could be made to work. However I do not see any way to modify this experiment in order to obtain their results. And *this* experiment, at least, does not work and cannot be performed—even in principle. Notice that the problems with the experiment are not particularly subtle; their analysis requires only basic results in quantum mechanics, statistical mechanics, and general relativity.

Perhaps the above discussion will after all prompt the development of a thought experiment that really would show the impossibility of nonquantized gravity. Such an experiment is likely to suggest directions for constructing a quantum theory of gravity. Investigating other possible thought experiments that rule out nonquantized gravity models may also lead to experiments that really *can* be performed at some later date.

[1] *Physics Meets Philosophy at the Planck Scale*, edited by Craig Callender and Nick Huggett (Cambridge University Press, Cambridge, 2001).

[2] James Mattingly, ''Lecture at the Fifth International Conference on the History and Foundations of General Relativity,'' 1999 (unpublished).

[3] James Mattingly, in *The Universe of General Relativity: Einstein Studies*, edited by Jean Eisenstaedt and A. J. Kox (Birkhäuser, Boston, 2005), pp. 325–337.

[4] K. Eppley and E. Hannah, Found. Phys. **7**, 51 (1977).

[5] D. N. Page and C. D. Geilker, Phys. Rev. Lett. **47**, 979 (1981).

[6] T. W. B. Kibble, in *Quantum Gravity 2, A Second Oxford Symposium*, edited by C. J. Isham, R. Penrose, and D. W. Sciama (Clarendon, Oxford, 1981), pp. 63–80.

[7] Robert Wald, *Quantum Field Theory in Curved Spacetime and Black Hole Thermodynamics* (University of Chicago Press, Chicago, 1994).

[8] Charles Misner, Kip Thorne, and John Wheeler, *Gravitation* (W. H. Freeman and Company, New York, 1973).

[9] Jens-Christian Meiners and Stephen R. Quake, Phys. Rev. Lett. **84**, 5014 (2000).

[10] One might suggest that a free particle is itself a simple harmonic oscillator bound with a spring of $k = 0$. But we need also that the average extent of the spring is of order 1 cm. However a free particle does not have a well-defined equilibrium position (i.e., a position to which it periodically returns). For their argument to be convincing, Eppley and Hannah must show that producing such springs is possible in principle.

[11] Robert Wald, *General Relativity* (University of Chicago Press, Chicago, 1984).