Close-orbiting black hole pairs are macroscopic quantum-gravitational systems. Or quantum mechanics is invalid.

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Abstract. Close-orbiting black hole pairs with near-equal masses $M \approx m$ are a new kind of macroscopic quantum object because they have inherent mass-uncertainty $\Delta M_{total} > (M+m)/380$. That makes them the largest and heaviest macroscopic quantum systems ever found, the first observable physical system plausibly requiring quantum gravity for an accurate description, and the first which plausibly will enable learning about quantum gravity via direct observation. To show that, we combine (1) rigorous forms of the energy-time uncertainty principle with (2) graviton-emission rate estimates – large rates force high mass-uncertainties ΔM; and more gravitons are emitted during a few minutes of super-super hole tight orbiting than the total count of non-graviton particles in the observable universe.

The final section, added for the third draft, argues that this all probably represents a *crisis* in physics in the sense it yields a *self-contradiction* derived from the axioms of quantum mechanics, which therefore are mathematically *inconsistent*. It then proposes, as the probable resolution of this crisis, that the $\Delta E \Delta t$ uncertainty relations lose their validity when ΔE exceeds order 1 in Planck units, and explains how that is predicted by the author's "Cloud QFT" theory.

Significance

Fundamental theoretical physics determines how the universe works. After quantum field theories jelled into the "standard model" in the 1970s, the top task became unifying it with Einstein's 1915 gravity theory, "general relativity." But that task has not been accomplished. A big reason: quantum gravity is almost inaccessible to experiment. Arguably, not one successful experiment has ever been done in that area.

We for the first time, present a physical system that's (a) quantum gravitational, (b) observable, (c) stunning, mind-boggling, (d) has a solidly-based undergrad-level derivation.

LIGO (Laser Interferometer Gravitational-Wave Observatory) and the "Event horizon telescope" got $>10^9$ \$US funding and a physics Nobel. There are many proposed big enhancements including "LIGO on the moon," LISA, and EHT movie-making. The present work, since suggesting new goals/directions for such projects and justifying and guiding them, has value of order 10^9 dollars.

Ingredient #1: Energy-time uncertainty principle

The vast majority of quantum mechanics textbooks say that $\Delta E \Delta t \geq \hbar/2$ where $\hbar \approx 1.055 \times 10^{-34}$ joule seconds, unfortunately *without* providing any precise meaning for ΔE or Δt and without telling the reader what, exactly, this supposed inequality even means. Fortunately, some precise statements are available. Bauer & Mello 1976 considered an unstable quantum system with **"survival probability" $Q(t)$** as a function of time $t \geq 0$, and **probability-density $\varrho(E)$** for its initial energy E. If the system were described by a wavefunction $\Psi(x,t)$ then $Q(t)$ with $0 \leq Q(t) \equiv |\int \Psi^*(x,0)\Psi(x,t)dx|^2 \leq 1$ (the integration is over the whole real line) is the probability the system remains in its initial state $\Psi(x,0)$ after time t.

Define the "**Bauer-Mello timespan**" $\tau_{BM} \equiv (½)\int_{t>0} Q(t)^{1/2}dt$. For any system obeying the classic "exponential decay law" $Q(t)=\exp(-t/L)$ this definition would exactly yield its mean lifetime $\tau_{BM}=L$. And any $Q(t)$ falling proportionally to $t^{-\gamma}$ (or faster) when $t \to \infty$, for any fixed exponent $\gamma > 2$, will yield a *finite* τ_{BM}.

A measure of the energy-width of the system is $W_E = 1/\max_E \varrho(E)$. The **Bauer-Mello theorem** then may be written $\tau_{BM} W_E \geq \pi\hbar/2 = h/4$ or equivalently (which I prefer)

$$\max_E \varrho(E) \leq 4\, \tau_{BM} / h \quad \text{where} \quad h = 2\pi\hbar \approx 6.626 \times 10^{-34} \text{ joule seconds is Planck's constant.}$$

Bauer & Mello's constant 4 is *best possible* in the sense that their inequality becomes an equality in the classic exponential decay case $Q(t)=\exp(-t/\tau)$ when the energy necessarily is described by the Cauchy density $\varrho(E)=2\pi^{-1}\Gamma/(4[E-E_0]^2+\Gamma^2)$ where Γ is the width of the energy-interval where $\varrho(E) \geq \max_E \varrho(E)/2 = \varrho(E_0)/2$ and Γ and τ obey the **linewidth-lifetime relation** $\Gamma\tau=\hbar$.

Experimental confirmations: For the 134.24 keV excited state of Re-187, Mössbauer & Wiedemann 1960 measured the linewidth $\Gamma=(4.4 \pm 0.5) \times 10^{-5}$eV using the Mössbauer effect (the line shape indeed is Cauchy to within measurement errors), deducing τ_{mean}=**15.2**±1.7 picoseconds. Blaugrund et al 1963 confirmed that prediction by measuring τ=**14.5**±2.0 ps using a microwave method. Steiner et al 1969 deduced τ_{mean}=**2.73**±0.02 nanoseconds from the Mössbauer linewidth of the 77.34keV level of Au-197 (superb fit to Cauchy lineshape in their fig.2), whereas delayed-coincidence timing found 2.65±0.029 (Gupta & Rao 1972) and 2.78±0.043 (Lynch 1973) which I combine to get **2.69**±0.05. This 1.5% agreement plausibly① is the best obtainable by Mössbauer methods. Steiner et al obtained their excited Au-197 by beta decay of Pt-197 (19.9-hour halflife, 719 keV) inside crystalline platinum. Their absorbers were gold foils of numerous precisely controllable thicknesses, allowing excellent extrapolation to zero thickness, all this at temperature 4.2°K. It helps that both Pt and Au have the maximally-symmetric FCC crystal structure (nearest neighbor distances 277 and 288pm) to help cancel out extra-nuclear fields; and that gold is fully soluble as a solid solution in platinum up to 100 atomic%. In hundreds of experiments, there has never been a case where any Mössbauer linewidth was *less* than $\hbar/\tau_{mean}$ by any significant number of experimental error bars. Thus *all* Mössbauer experiments support the validity of the Bauer-Mello *inequality*, while the above two support the precise optimality of their numerical constant. One to two orders of magnitude more precision came when Oates et al 1996 used trapped ultracold Na atoms to precisely measure the natural linewidth Γ=9.802±0.022 MHz= $(4.054 \pm 0.009) \times 10^{-8}$eV of the $3p^2P_{3/2}$ excited state, while Volz et al's adjacent paper measured its lifetime τ=**16.254**±0.022 nanosec using beam-gas-laser spectroscopy, agreeing within the experimental errors with the predicted $\hbar/\Gamma$=**16.237**±0.035 nanosec.

Other precise statements were obtained by Mandelstam & Tamm 1945, for example $Q(t)\ge\cos(t\Delta E/\hbar)^2$ when $0\le t\le(\pi/2)\Delta E/\hbar$ where $\Delta E\equiv[\int(E-\bar{E})^2\varrho(E)dE]^{1/2}$ and $\bar{E}\equiv\int E\varrho(E)dE$ and the integrations are over the full real line. In particular, if we define the "**half life**" $\tau_{1/2}=\min_{t>0}\{t \mid Q(t)\le 1/2\}$ then $\tau_{1/2}\Delta E\ge\pi\hbar/4=h/8$. This and the prior $\cos^2$ inequality also were obtained by Bhattacharyya 1983 in his EQs 10 & 14, who also gave (his EQ 16) $Q(t)\ge\exp(-2\Delta Et/\hbar)$ when $0\le t\le\tau_{1/2}$, which also prevents too-rapid decay. The original textbook claim can be given this precise meaning: $\tau_{RMS}\Delta E\ge\hbar/2$ where $\tau_{RMS}=[\int_{t>0}Q(\sqrt{t})dt]^{1/2}=[2\int_{t>0}tQ(t)dt]^{1/2}$ is the **root mean square lifetime**. And if we define the "**mean life**" $\tau_{mean}=\int_{t>0}Q(t)dt$ then **GiSaWo** – Gislason, Sabelli, Wood 1985 – showed

$$\tau_{mean}\,\Delta E \ge 5^{-3/2}3\pi\hbar = 5^{-3/2}3h/2.$$

GiSaWo's constant also is best possible, in the sense that their inequality is tight when $\varrho(E)=(3/4)(1-E^2)$ for $|E|\le 1$, else 0. With that $\varrho(E)$ the survival probability Q(t) has $Q(t)t^4$ bounded below a positive constant always, but bounded above a (different) positive constant on a positive-density subset of the halfline t>0.

The four timespans we have discussed always obey $0<\tau_{1/2}\le\min(2\tau_{mean}, 2^{1/2}\tau_{RMS}, 2^{3/2}\tau_{BH})$ and $\tau_{mean}\le 2\tau_{BH}$, $\tau_{mean}\le\tau_{RMS}$. [The first arises from Markov's inequality in probability theory; the second from Hölder's $(1,\infty)$ inequality, and the third from the concave-∪ nature of the squaring function.]

It now is natural to ask whether there is any uncertainty relation *combining* the virtues of *both* GiSaWo and Bauer-Mello, i.e. of the form $h\cdot\max_E\varrho(E)\le\varkappa\tau_{mean}$ (or $\le\varkappa\tau_{1/2}$) for some positive constant $\varkappa$. The answers both are **no**, because the probability density $\varrho(E)=\pi^{-1/2}\Gamma(v+3/2)\Gamma(v+1)^{-1}(1-E^2)^v$ for $|E|<1$, else 0 (where v>-1 is a constant) corresponds to a survival probability Q(t) with $Q(t)t^{2v+2}$ bounded below a positive constant always, and above another positive constant on a positive-density subset of the halfline t>0. That's due to, e.g, EQ 2-7-19 of Sneddon 1972 combined with Dodonov 2015's discussion around EQ 16-17. So if -1<v<0 then $\max_E\varrho(E)=\infty$, while if -1/2<v then both $\tau_{1/2}$ and τ_{mean} are finite and positive. So any v with -1/2<v<0 yields a counterexample. I do not know whether there is any uncertainty relation of form $\max_E\varrho(E)\le\varkappa\tau_{RMS}/h$.

However, I can prove (but② will not here) $\tau_{mean}\Delta_1E>0.2889\hbar$ and more generally for any fixed k>0 that $\tau_{mean}\Delta_kE>c_k\hbar$ where $\Delta_kE\equiv[\int|E-\bar{E}|^k\varrho(E)dE]^{1/k}$ and the c_k are appropriate positive constants. I can also prove: If the narrowest energy interval $\mathbb{I}$ containing at least 31% probability $[\int_{\mathbb{I}}\varrho(E)dE\ge 0.31]$ has width $W_{31\%}$, then $\tau_{1/2}W_{31\%}>0.005969\hbar$.

For **exact-exponential** decay $\tau_{1/2}/\ln 2=\tau_{mean}=\tau_{BH}=2^{-1/2}\tau_{RMS}$; and Δ_kE is finite for each k with 0<k<1, for example $\tau_{mean}\Delta_{1/2}E=\hbar$ and $\tau_{mean}\Delta_{2/3}E=2^{1/2}\hbar$; and $W_{31\%}\approx 0.5295\hbar/\tau_{mean}$; but $\Delta_kE=\infty$ for each k≥1. That infinity is one reason that exact exponential decay is, under traditional quantum mechanics, considered impossible (Fonda et al 1978); but if, say, radium decays exponentially for 300 halflives then switches to $t^{-\gamma}$ style decay for some exponent γ with $2<\gamma<5$ (which is roughly what most analysts contend), then (a) those infinities would not arise, and (b) detecting this departure from exponentiality would be infeasible.

Ingredient #2: Gravitational radiation from rotating quadrupoles

Two rotating systems are

a. Uniformly-dense rigid thin rod of length=L and mass=M rotating about an axis perpendicular to the rod through its midpoint.
b. Two point masses m and M, separated by distance L, both circularly orbiting their center of mass (either because joined by a massless length-L rod, or because of their mutual gravitational attraction according to Newton's laws).

Let the angular velocity be Ω, so the period is $2\pi/\Omega$. Either way, we have a "rotating quadrupole" which therefore **emits gravitational-wave radiation**.

(a) Eddington 1922/1923 (where we've also used the formula $I=ML^2/12$ for the moment of inertia I of the rod) calculated the emitted power

$$P_{rod} = 32GI^2\Omega^6c^{-5}/5 = 2GM^2L^4\Omega^6c^{-5}/45.$$

(The reason Eddington published this twice, using two different methods, was to become confident that Einstein previously had been a factor of 2 too small.) This corresponds to a rate of emission of gravitons (each graviton having angular frequency 2Ω) with mean time τ between graviton emissions equal to $\tau=2\hbar\Omega/P=45\hbar G^{-1}M^{-2}L^{-4}\Omega^{-5}c^5$. See Smith 2021 for analysis of the claim "gravitons exist" and with energy E=hf for a frequency=f graviton. (And if 2Ω were only an *upper bound* on the mean graviton frequency, then our formula would only upper-bound τ, which would be adequate for the purposes of this paper.) If we now regard the rotating rod as a *quantum* system with mean decay time τ, we see from the GiSaWo bound and $E=mc^2$ that the mass of the rod necessarily is *uncertain*, with $\Delta M\ge 5^{-3/2}3\pi\hbar c^{-2}/\tau$, that is, $\Delta M_{rod}\ge(5^{-5/2}\pi/3)GM^2L^4\Omega^5c^{-7}=5^{-5/2}48\pi GI^2\Omega^5c^{-7}$.

Decay chains. We have regarded the rotating rod as a quantum system which "decays" by repeatedly emitting gravitons until reaching its ground state with angular momentum 0. A well known historical example of a decay chain begins with the transuranic isotope Lawrencium-258. After a combination of 13 α, one β^+, and 6 β^-, decays, and I presume a goodly number of γ's too (although I do not know how many) it finally reaches the apparently-stable Pb-206 atom. (In fact, Gamow's 1928 model of α-decay indicates that Pb-206 must be unstable too, just with unobservably-long halflife. Also, this is not really a "chain" but rather a "directed acyclic graph," in the sense that multiple decay-options happen for some intermediate isotopes; but all high-probability pathways begin with Lr-258 and end with Pb-206.) At each step in this process we have a new atom, in a new nuclear state, with its own individual mean lifetime τ and hence its own individual mass-uncertainty $\Delta M\ge 5^{-3/2}3hc^{-2}\tau^{-1}/2$. The longest halflife in this chain is 4.5 Gyr for U-238, which is about 2×10^{21} times the shortest halflife 64µsec for Po-214. A different well-studied decay chain is

Co-60m ⇝ Co-60 → Ni-60b ⇝ Ni-60a ⇝ Ni-60

where ⇝ denote energy-drops via gamma-emission while → denotes beta-decay. These decays are, in chronological order: (i) 58.59 keV gamma, halflife=10.47 minutes, (ii) 312 keV beta decay, halflife=5.271 years, (iii) 1173.24 keV gamma, halflife=3.3picosec, (iv) 1332.54 keV gamma, halflife=0.73picosec.

A **critic** wanted to fight the present paper by claiming those nuclear decay chains are *not* analogous to Eddington's spinning rod, because somehow large numbers (10^{30}?) of graviton emissions should not be treated individually, but rather as just *one* (collective) "decay." Mandelstam & Tamm's and Bhattacharyya's inequalities *prevent* too-closely-spaced (i.e. "collective") decays, but my critic was unaware of that. And in that critic's defense, there is this qualitative difference: the gravitons emitted by binary near-equal nearby black holes have long wavelengths – so long that each emitted graviton usually *overlaps* in time with many others. In contrast, those nuclear decays involve particles with short wavelengths, so short that each emitted particle usually *negligibly* overlaps any other of its same type; so it is reasonable to regard all those nuclear emissions as *distinct*.

However, that depends on the numbers. For a swung baseball bat (L=74cm, M=567 grams, Ω=100 radian/sec) as our "spinning rod," τ=5660 years. So *that* rod's graviton emissions are very much "non-overlapping distinct events"! For inspiraling near-equal binary black holes, we'll see that when the holes are far apart, the time between graviton emissions becomes arbitrarily longer than an orbital period; but later, when they approach merger, it becomes short. Whether graviton emissions are "distinct" versus "overlapping" does not have a yes/no answer.

Mathematically, Mandelstam & Tamm's uncertainty relations need only this: whenever a wavefunction evolves to a state orthogonal – or at least separated by at least some given positive angle in Hilbert space – from the prior one, that is a new "decay." E.g. in the case of nuclear α-decay, once the probability the α-particle still is inside the nucleus drops to (say) 1/2, that's a legitimate "decay lifespan." If N successive such decays are highly "distinct," it certainly is legitimate to use those lifespans to compute (lower bounds on) the ΔM's within each interdecay time-interval. But suppose the extreme-opposite behavior: some large number K of gravitons all are emitted to co-occupy the exact *same* mode. Then I *still* claim it is legitimate to compute ΔM lower bounds from the inter-emission time-intervals! That is because the wavefunction $\Psi_K(x)=2^{-K/2}K!^{-1/2}\pi^{-1/4}\exp(-x^2/2)H_K(x)$ of the Kth energy level of a 1D simple harmonic oscillator [here with mass m and angular frequency=ω obeying $m\omega=\hbar$, and the H_K denote Hermite polymnomials] is *orthogonal* to $\Psi_J(x)$ for all J≠K. Therefore the state with K gravitons co-occupying a mode, is orthogonal to all the prior states (with J gravitons, 0≤J<K). So the degree of typical overlap appears *irrelevant* and I cannot accept the critic's contention that it is illegitimate to compute ΔM's for graviton-emitting rotating systems in the same manner as nuclear decay chains.

(b) can be treated using the more general analysis in §10.5 of the book by Weinberg 1972, but Eddington's I-based formula also works given that our two masses M and m have respective distances R and r to their center of mass, whereupon solving MR=mr and R+r=L for r=LM/(m+M) and R=Lm/(m+M) determines the moment of inertia $I=mr^2+MR^2=L^2mM/(m+M)$. The radiated power is $P_{binary}=(32/5)Gm^2M^2(m+M)^{-2}L^4\Omega^6c^{-5}$. If the masses obey the **Kepler-Newton** law $(m+M)G=\Omega^2L^3$ then the radiated power can be rewritten as $P_{binary}=(32/5)m^2M^2(m+M)L^{-5}G^4c^{-5}$. Then as before we find $\tau_{binary}=2\hbar\Omega/P_{binary}=(5/16)\hbar G^{-7/2}M^{-2}m^{-2}(m+M)^{-1/2}L^{7/2}c^5$ and $\Delta M_{binary}\ge 5^{-5/2}432\pi c^{-7}G^{7/2}M^2m^2(m+M)^{1/2}L^{-7/2}$.

Without loss of generality 0<m≤M. Now suppose that the center-separation L happens to be near minimum possible. The Schwarzschild radii of the two masses in isolation would be $r=2mGc^{-2}$ and $R=2MGc^{-2}$. So clearly if $L\le r+R=2(m+M)Gc^{-2}$ then our "two" black holes would actually be one merged entity. The Newtonian equipotential surface at the same potential as a single isolated hole's horizon (corresponding to escape velocity=c for an infinitesimal test mass) becomes topologically *two* spherical surfaces exactly when L satisfies L>x+X with $M/X+m/x=c^2/(2G)$ and $MX^{-2}=mx^{-2}$. It is simplest to solve these equations when m=M (hence r=R), the answer then being $L>L_{merge}$ where $L_{merge}=2x=2X=4R=8MGc^{-2}$. The fully-general answer is $L_{merge}\equiv 2([m/M]^{1/2}+1)(m+M)Gc^{-2}$. Of course, our uses of the "Newtonian potential" and the "Kepler-Newton law" both are only approximately valid since we have ignored general relativistic time dilation, space distortion, and dynamics. So the reader should keep in mind that all our formulas about black holes at near-minimal separation are only **approximate**, i.e. are the leading order terms in the "post-Newtonian" sequence of approximations. This still must yield a *lower bound* on radiated power, valid to within a dimensionless constant factor. (For more accuracy one could use Will & Wiseman 1996's "second post-Newtonian order" calculation; and to get the constant presumably arbitrarily near exact, one could do computer simulations ala Healy & Lousto 2017.)

If our two masses indeed are black holes separated by that minimum possible distance L_{merge}, then the radiated power becomes

$$P_{minsep.binary} = m^2\, M^2\, (m+M)^{-4}\, ([m/M]^{1/2}+1)^{-5}\, P_{Pl} / 5$$

where $P_{Pl}\equiv c^5/G\approx 3.6283\times10^{52}$ watts is the **Planck power unit**. Therefore if m≈M then P_{minsep} is about $2^{-9}5^{-1}$=1/2560 Planck power units, i.e. about 1.4173×10^{49} watts, *regardless* of m+M.

Comparison vs. Experiment: The table lists seven LIGO-detected black hole mergers enjoying comparatively high-quality data and analyses, and with primary/secondary mass ratios all fairly near 1.

Event name	Mass=M+m (suns)	Lost mass	Lost/Total	M/m	Peak power (10^{48}W)	Comments
GW190521	150=85+66	7.6	5.1%	1.29	37±8	60Hz for 100msec (4 cycles)
GW170814	56=32+24	2.7±0.35	4.8%	1.33	37±5	
GW200202	17.6=10.1+7.5	0.82	4.7%	1.35	?	
GW150914	68=38.7+32.5	3.1±0.4	4.6%	1.19	35±5	1st detected; 50M CPU hours for simulations
GW170608	19=12+7	0.85±0.12	4.5%	1.71	34±11	
GW151226	21.8=14.2+7.5	1.0±0.15	4.6%	1.89	33±12	
GW170104	48.7=31.2+19.4	2.0±0.65	4.1%	1.61	31±10	

The peak power was always between 27 and 40 in units of 10^{48} watts, and showing as expected a noisy *decreasing* trend with mass ratio≥1. The lost/total mass ratios were always between 3% and 6%. The apparent *constancies* of peak power and lost/total for fixed primary/secondary mass ratio, despite total mass varying by an order of magnitude, agree with what our model predicts; and also agree with our model's prediction⑤ that LostMass>(M+m)/32. But the observed numerical *values* of the peak power are a factor somewhere between 2 and e times our model's prediction. We have several valid excuses for that:

1. As we'd said, our model makes Newtonian *approximations*, which become poor as we approach (and ridiculously poor after) horizon merger.
2. Our model ignored the fact that actual black holes have different *spins*, simplistically regarding all black holes of a given mass as identical.
3. *After* topological merger occurs, gravitational waves will *still* radiate while the event horizon changes from a dumbbell shape into its ultimate nice round Kerr

shape. That relaxation might well involve greater energy-loss and/or more power than the pre-merger stage, but my model is *only* applicable pre-merger.

And indeed, **computer simulations** by Healy & Lousto 2017 predict that the maximum possible peak power (which happens near the time of horizon-merger; they did not say whether before or after) occurs for equal-mass black holes, each with maximum spin aligned with orbital angular momentum, and equals 7.1368×10^{49} watts, i.e. 5.0355 times our prediction, a ratio suspiciously near both 5 and $(5\pi/7)^2\approx5.035512$. Even greater power might be possible if the two holes were oppositely electrically *charged*, since then photons also would be radiated. But astrophysical holes presumably are near-neutral, a hypothesis supported by the non-observation of giant EM-radiation pulses from hole mergers. Their same peak-power-maximizing scenario also maximizes the fraction of total initial mass ultimately radiated, i.e. lost: 11.3%. The *minimum* loss fraction (≈3%) in the equal-mass case occurs for maximum hole spins *anti*-aligned with orbital angular momentum. In the *spinless* equal mass case their peak power is 3.7226×10^{49} watts, i.e. $2.6265\approx2\ln(1+e)$ times our model's prediction, with 4.857% of the initial mass allegedly radiated. This all makes it clear our Newtonian model **underestimates** peak power.

We now use our peak-power formula to deduce the mean time τ between graviton emissions $\tau_{binary}= 2^{-1/2}5\hbar c^{-2} M^{-2}m^{-2}(m+M)^3 ([m/M]^{1/2}+1)^{7/2}$ which when m=M is $\tau_{binary}=320\hbar c^{-2}m^{-1}$. Then via the energy-time uncertainty principle (GiSaWo bound) and $E=mc^2$, the uncertainty ΔM in total mass is lower bounded by $\Delta M_{binary} \geq 5^{-5/2}2^{1/2}3\pi M^2m^2(m+M)^{-3} ([m/M]^{1/2}+1)^{-7/2}$ which when m=M becomes

$$\Delta M_{binary} \geq (5^{-5/2}2^{-6}3\pi)\ (M+m) \approx 0.002634\ (M+m) > (M+m)\ /\ 380$$

regardless of c, G, and $\hbar$.

If, further, we assumed the inter-graviton time delays were approximately *exponentially* distributed, then we could increase the constant 0.002634 in the lower bound. E.g. using the half-life inequality $\tau_{1/2}\Delta E\geq\pi\hbar/4$ instead of GiSaWo would increase it to 0.00354. Indeed, it would increase to arbitrarily large values under the (false) assumption of arbitrarily precise exponentiality. I suspect that exponentiality should be quite precise because gravitons are being emitted at the huge rate $\tau^{-1}\approx10^{88}$ per second for $M+m=10^9$ solar masses – by far the greatest particle-emission rate I ever saw for anything (there are only $\approx3\times10^{80}$ quarks and electrons, and $<10^{90}$ neutrinos and photons, in the observable universe!) – with chronologically-adjacent graviton-emissions from source locations $\gtrsim c\tau\approx10^{-79}$ meters apart presumably *independent*. Hence I expect probability correlations of order 10^{-180}. Therefore the graviton emission times presumably well-approximate a Poisson process. Poisson process gap lengths are exactly exponentially distributed. Therefore I expect our lower bounding constant 0.002634 is extremely conservative (and of course would be multiplied by 5.0355 if we used Healy & Lousto's peak power formula instead of our model's), with the truth probably somewhere between **0.04 and 0.6**.

Remarkable Conclusion

Two closely orbiting comparable-mass black holes always form a *quantum* system, whose inherent mass *uncertainty* necessarily exceeds 1/380 of its total mass and probably a lot more. This is by far the largest intrinsic mass-uncertainty I ever heard of for anything macroscopic. This can be (which presumably has happened many times) a "macroscopic quantum phenomenon" weighing 10^9 solar masses, with diameter comparable to the solar system, with mass-uncertainty exceeding millions of solar masses – all again by far the largest I ever heard of – lasting for months③. As far as I know no prior author has ever pointed out that black holes, despite their giantness, can be *quantum* in nature and **require**④ quantum gravity for accurate description. In fact, **this is the first physical system anybody ever thought of, in which quantum gravity plays such a large role that it should be feasible to "observe" it in action**. And given the recently developed capability of the "event horizon telescope" to "see" certain black holes with high resolution, and LIGO's ability to "hear" black hole mergers in real time, this for the first time opens up serious hope that it might be possible to learn about quantum gravity *by direct observation*.

And note: we could apply our arguments to just the "north half" and "south half" of the binary system, finding those half-masses also must be highly uncertain, presumably (considering speed-of-light causality constraints) with a large amount of independence. Consequently it isn't just the *total amount* of mass in the system that is uncertain; its *distribution* also is.

What is "mass"?

A critic objected: "In general relativity (**GR**), the 'mass' of a source (star, planet, black hole, etc.) can only be defined via its effects far away (e.g. the period of an orbit around the source). That makes me think that the individual masses of two close-orbiting black holes *already* are poorly defined, so there's a fundamental 'uncertainty' that has nothing to do with quantum mechanics."

To respond: The critic presumably had in mind Emmy Noether's 1918 proof that "There is no covariant total energy-momentum density tensor for gravitating systems." An intuitive reason (Weinberg 1972 p.68) is Einstein's "equivalence principle" asserting the equivalence of gravitational and inertial mass: "At every space-time point in an arbitrary gravitational field it is possible to choose a 'locally inertial (aka freely falling) coordinate system' such that [locally] the laws of nature take the same form as in unaccelerated Cartesian coordinate systems in the absence of gravitation."

But there *is* a mathematically precise definition of "**quasilocal mass**" in GR by Wang & Yau 2009, which (if their claims are valid) completely resolves the critic's worry. (Although of course I do not know what "mass" is in whatever future theory of quantum gravity ultimately will supplant GR.) Readers who do not want to know more may now skip to the next section. For those who do want more mass, you will need familiarity with GR to comprehend the rest of this section.

"Quasilocal mass" measures the mass-energy of a system contained within a closed spacelike 2-dimensional *surface* with S^2 topology and everywhere-inward-spacelike mean curvature vector, by associating to each such surface an energy-momentum 4-vector. This 4-vector depends only on the induced surface metric and the mean-curvature vector field on the surface embedded in spacetime. To define their mass formula, Wang & Yau used an isometric embedding of the S^2 surface directly into flat Minkowski (3+1)-spacetime. They proved such an embedding *exists* for a large class of surfaces (it is unclear how large, but includes every surface with everywhere-positive Gaussian curvature, and every surface "close enough" to having that status) – although it might not be *unique* – a wonderful theorem unavailable to Einstein & Noether. The WY mass definition then involves a *minimization* of a quasilocal energy (infimum: Theorem 3 & EQ 5 in Wang-Yau PRL 2009) among all admissible isometric embeddings into Minkowski (3+1)-spacetime and all observers in it. [Wang & Yau provided absolutely no clue about how *computationally* difficult it is to perform this minimization numerically.]

Properties of WY quasilocal mass:

1. *Assuming* any "matter" (described by Einstein's "stress-energy tensor" T_{ab}; this is taken to include the contributions of all fields, such as a magnetic field, and the Einstein cosmical constant Λ if nonzero) obeys the "dominant energy condition" (**DEC**) forbidding mass traveling faster than c and stating that all observers agree local mass density is nonnegative, they prove (Wang & Yau PRL 2009 theorems 2 & 3): WY quasilocal masses are always **nonnegative**, i.e. more precisely their 4-vector is always future-timelike (or possibly future-null for certain "pure radiation spacetimes," although they never produced any example of that).
2. Furthermore, WY mass is **zero** (under the DEC) if and only if the surface may be regarded as bounding a spacelike 3-surface in a *flat* Minkowski (3+1)-spacetime; otherwise positive.
3. **Large-sphere** limits in asymptotically-flat spacetimes: WY mass yields the ADM (Arnowitt-Deser-Misner) mass at spatial infinity (Wang & Yau 2010), and the Bondi mass at infinite distance along a null cone (Chen et al 2011), both these "masses" really being 4-vectors as usual.
4. WY mass is **invariant under Lorentz transformations** of the Minkowski spacetime.
5. WY mass is **conserved** (but observer-dependent, which is why it is a 4-vector) under GR. A sense in which this is true is explained by Chen,Wang,Wang,Yau 2022 "at null infinity"; another arises from the fact Wang & Yau (preceded by Brown & York) based everything on the "Hamiltonian formulation of GR" by Arnowitt, Deser, Misner 1962.
6. WY mass is independent of the choice of "**gauge**" in GR.
7. **Small-sphere** limits (Brown, Lau, York 1999): Suppose the surface is a small sphere (radius=r) surrounding an observer, and let t^a be that observer's "unit time" tangent 4-vector. Let $L_n(r)=r^{-n}W_a$ where W_a is the energy-momentum 4-vector Wang & Yau associate with that surface. Then $\lim_{r\to 0+}L_n(r)=0$ if $0<n<3$. If T_{ab} denotes Einstein's "**stress-energy tensor**" for the matter, and we employ Einstein summation convention and geometrized units, then $L_3(r)=(4\pi/3)T_{ab}t^b\pm O(r)$. If the spheres contain $\Lambda=0$ field-free vacuum, then $\lim_{r\to 0+}L_n(r)=0$ if $0<n<5$, and if T_{abcd} denotes the Bel-Robinson tensor, then $L_5(r)=T_{abcd}t^bt^ct^d/90\pm O(r)$.
8. Wang & Yau conjecture the WY mass "satisfies all the requirements necessary for a valid definition of quasilocal mass, and it is likely to be the **unique** definition that satisfies all the desired properties." More precisely, Yau conjectured uniqueness given properties 1,2,3,4,7. However, as of year 2023 such uniqueness results remain unproven.
9. Whenever the spacetime possesses a timelike Killing field, WY mass reduces to "Komar mass."
10. If the surface encloses an **event-horizon**, then the WY mass never is less than a (universal) positive-constant times the "irreducible mass" of the hole, which for a Schwarzschild hole is $M_{irred}=(16\pi)^{-1/2}c^4G^{-2}A^{1/2}$ where A is the horizon's surface area (theorems 2.12 & 2.13 of Alaee-Khuri-Yau 2020; Mondal & Yau 2022 claim a valid constant is 1 for Kerr-Newman hole metrics).
11. WY mass is a **decreasing set function** outside a source, i.e. if $A\subset B$ and B-A is source-free ($T_{cd}=0$), then WYmass(B)≤WYmass(A). For the usual Schwarzschild metric (Schwarzschild's coordinates involving "circumferential" radial coordinate r), the WY mass of the sphere r=R, t=0 in geometrized units is $[1-(1-2m/R)^{1/2}]R$ which equals 2m for the horizon R=2m and monotonically decreases toward m as $R\to\infty$. So evidently WY mass is **non-additive** i.e. the WY mass of C in general is less than (or anyhow does not equal) the summed WY masses for A and B if $C=A\cup B$ with $A\cap B=\emptyset$. "Less" is compatible with the intuition that "gravitational binding energy is negative."
12. Wang & Yau in 2015 and 2021 (with Po-Ning Chen and/or Ye-Kai Wang) later gave compatible quasi-local definitions for "angular momentum" and "center of mass," which they proved invariant under "super-translations." But I will not discuss them, and those extensions might not be unique.

Remarks about those two tensors. Einstein's stress energy tensor T_{ab} is symmetric ($T_{ab}=T_{ba}$) and under GR is divergence-free, i.e. conserved ($T_{ab}{}^{;b}=0$, where semicolon denotes convariant derivative). The **Bel-Robinson** tensor is defined (in two equivalent ways, albeit the second only is defined in 4 dimensions because it employs tensor "duals") by

$$T_{abcd} = W_{aecf}W_b{}^e{}_d{}^f - (3/2)\ g_{a[b}W_{jk]cf}W^{jk}{}_d{}^f = W_{eabf}W^e{}_{cd}{}^f + {}^*W_{eabf}{}^*W^e{}_{cd}{}^f.$$

in terms of the traceless Weyl curvature tensor W_{abcd}, which in turn is defined in terms of the Riemann curvature tensor R_{abcd} and its contractions $R_{bc}=R^a{}_{bac}$ and $R=R^a{}_{ba}{}^b$ in (n+1)-dimensional spacetime by

$$W^{ab}{}_{cd} = R^{ab}{}_{cd} - 2(n-1)^{-1}\ \delta^{[a}{}_{[c}R^{b]}{}_{d]} + 2n^{-1}(n-1)^{-1}\ \delta^{[a}{}_{[c}\delta^{b]}{}_{d]}R$$

(cf. Misner Thorne Wheeler EQ 13.50 p.325). In the other direction, the Weyl tensor can be reconstructed from the Bel-Robinson tensor *only* (i.e. without needing any derivatives), although several cases are needed depending on the "algebraic type" and the Weyl tensor is only determined up to an arbitary-θ "duality rotation" $(\cos\theta)W_{abcd}+(\sin\theta)^*W_{abcd}$, see Ferrando & Saez 2010. The Bel-Robinson tensor is zero at a point if and only if the Weyl tensor also is zero there. It is traceless ($T^a{}_{abc}=0$) and completely symmetric (any permutation of the 4 indices of T_{abcd} has no effect). From EQs 8-10 of Garecki 1999 we deduce that for any value of the Einstein cosmical constant Λ in a GR(Λ)-vacuum T_{abcd} is divergence-free, i.e. "conserved": $T_{abcd}{}^{;a}=0$. (Garecki only noted this in the special case $\Lambda=0$, but his derivation shows it holds for any constant value of Λ. Indeed, more generally, this holds in any spacetime metric *conformally related* to a Λ-vacuum.) Collinson 1962 proved the Bel-Robinson tensor is the *only* 4-indexed tensor either quadratic in the Riemann curvature tensor, or linear in its second derivatives, with coefficients constructed from products of metric tensors, which is divergence-free in vacuum. If E^a, F^b, G^c, and H^d are any four future-timelike (or future-null) vectors, in signature –+++, then automatically $T_{abcd}E^aF^bG^cH^d\geq 0$, a remarkable *nonnegativity* property. Also automatically nonnegative is $T_{abcd}T^{abcd}/64 = (W_{abcd}W^{abcd})^2+({}^*W_{abcd}W^{abcd})^2$. Incidentally $T_{abcx}T^{abcy}=\delta^y{}_xT_{abcd}T^{abcd}/4$. In weak-field Einstein GR, the Bel-Robinson tensor is related to the partial derivatives of the Landau-Lifshitz (LL) and Einstein (Ei) "energy density" pseudotensors by $T_{abcd}=\partial_c\partial_d\ (LL_{ab}+Ei_{ab}/2)$. Many of our claims about Bel-Robinson tensor work *only* in 4 spacetime dimensions. J.M.M.Senovilla produced a great generalization of the Bel-Robinson tensor which he called the "super-energy tensor" associated with any given tensor. (His construction, if applied to the Weyl tensor in 4 dimensions, yields Bel-Robinson.) But we shall not discuss super-energies.

Senovilla argued that the Bel-Robinson tensor's positivity property can be regarded as analogous to the DEC for the stress-energy tensor. Also the fact that the Bel-Robinson tensor is divergence-free in 4D spacetime Λ-vacuums is analagous to the conservation of classical mass-energy. Such analogies had long mysteriously suggested that *something* associated with T_{abcd} is nonnegative, cannot move faster than light, and so somehow T_{abcd} resembles a "gravitational-energy density" for the vacuum. The WY mass resolves that mystery.

Experimental measurement of mass. Wang & Yau (mathematicians) gave no inkling of what their quasilocal mass meant *physically*, e.g. how to measure it. (I also find their insistence on using comparatively poorly defined ad hoc notation, rather than standard physicist tensor notation, extremely annoying; which probably has a great deal to do with why nobody else ever explored/used their ideas in published research even 15 years later.) So I'll address that now. Suppose the S^2 surface is triangulated, i.e. approximated by a *polyhedron* with a large number of vertices and all faces triangular and roughly equilateral. Place little robot spaceprobes at the polyhedron's *vertices*. (These probes have masses negligible by comparison with the black holes, or whatever other big mass is enclosed by, our surface.) Each probe is equipped with lasers, telescopes, light sensors, protractors, and clocks. They shine laser beams along the polyhedron edges to nearby spaceprobes, thus communicating with them. Each probe can deduce its *distance* to other probes by 2-way laser transit-time measurements. It also can measure the *angles* between laser beams. Thus the entire geometry of the polyhedron can be measured. In particular we can measure the *Gaussian curvature* of the surface (deduced from the "angle defect" for each face, i.e. the discrepancy between its angle-sum and 180°) as a function of position and time. Each probe also can deduce the local *mean curvature vector* by fitting the surface to a quadratic in 3-space. If all probes transmit this data to a faraway scientist, he can use it to compute the enclosed mass as a function of time. In the simplest case where the surface is a sphere of circumference $2\pi R$ surrounding a Schwarzschild black hole, both Gaussian and mean curvatures are constant everywhere on the surface. The enclosed mass arises from the *discrepancy* between them – there would have been no discrepancy, hence zero mass, if the underlying spacetime had been flat.

Discussion

To see just how "remarkable" that conclusion was, let us compare it versus various other systems.

The rod-shaped interstellar asteroid "Oumuamua" has $L\approx 400$ meters, rotation period≈8 hours so $\Omega\approx 2\times 10^{-4}$/second, and if made of iron has mass $M\approx 4\times 10^{9}$ kg. I compute $\Delta M\approx 8\times 10^{-61}$ kg. If we replaced the iron by high strength steel and sped up the rotation period to 3 seconds (any faster and steels would not be strong enough) then $\tau=10$ picoseconds and $\Delta M\approx 9\times 10^{-41}$ kg, which still is 10 orders of magnitude smaller than the mass of a single electron.

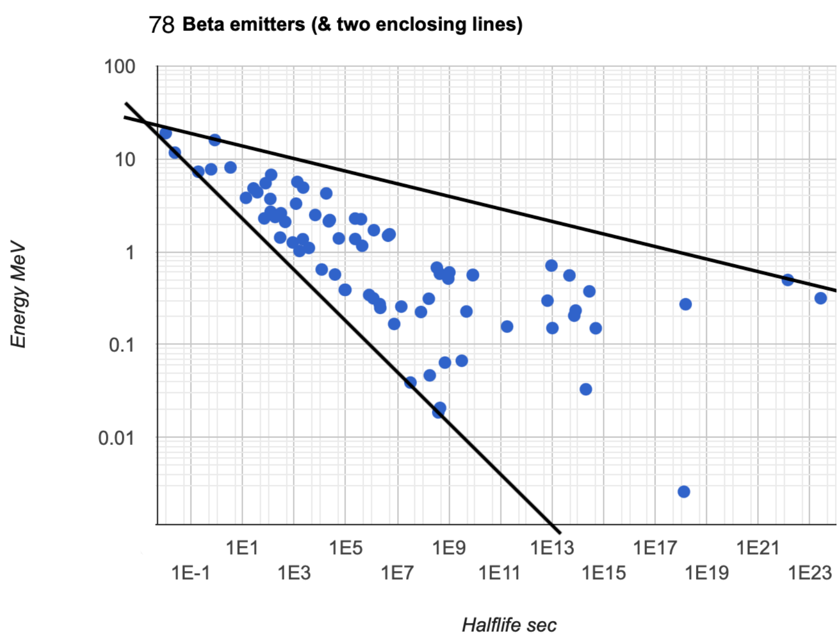
BAT99-98 in the Large Magellanic Cloud arguably is the most luminous star currently known. It is believed to have mass 226 times our sun, luminosity$\approx 1.9\times 10^{33}$ watts equivalent to 5×10^{6} suns, and surface temperature 45000°K. I deduce that it emits about 10^{51} photons/second. If we regard this entire star as a quantum system with decay time 10^{-51} second, then its inherent mass-uncertainty is $\Delta M\approx c^{-2}\hbar 10^{51}$/second$\approx 1$ kg. Peak supernova luminosities can reach 5×10^{9} suns (1.9×10^{36} watts), suggesting by the same calculation ΔM of order ≤ 1000 kg. That still is peanuts in the sense that $\Delta M\le 10^{-28}M$ is far too small to detect.

The tininess of those ΔM's was not merely due to luck.

1. We can readily argue that the graviton-emission-caused ΔM of *every* rotating gravitationally-bound system is (as a fraction of its total mass M) *maximized* when it is a black-hole close binary system – and if it does not involve at least 2 black holes, is always much smaller.

2. For simplicity in the following argument let me work in Planck units ($\hbar=c=G=k_B=1$) and ignore constant factors of order 1. Consider a Euclidean ball of radius=R with at least the outer layer (layer thickness λ) of this ball consisting of hot material (temperature $T\approx 1/\lambda$). Regard this as a quantum system which "decays" by emitting photons, e.g. of blackbody radiation at temperature≈T and wavelength≈λ into the region outside the ball. The "decay time" (i.e. mean time between such photon emissions) will then be of order $R^{-2}T^{-3}\lambda$. This decay time will cause our system to have ΔM of order R^2T^3/λ. Meanwhile the mass M of that outer layer is at least of order $R^2T^4\lambda$. Hence $\Delta M/M\approx T^{-1}\lambda^{-2}\approx T$. We conclude that ΔM/M has order≥1 *only* when the temperature T is at least of order 1 Planck temperature unit: $T\ge T_{Pl}\approx 1.417\times 10^{32}$°K. However, that assumed Euclidean geometry. In fact, $T<R^{-1/3}$ is necessary otherwise our ball will be so heavy it is a black hole (and therefore not emit radiation at all). Therefore, ΔM/M of order ≧1 is *impossible* for any system of our "**hot ball**" type whose radius R exceeds order 1 Planck length units: $R>L_{Pl}\approx 1.616\times 10^{-35}$ meters.

Particle	Mass (MeV/c^2)	Est.Mean Lifetime (sec)	ΔM/M	(lost mass)/M
Roper resonance	1370	3.7×10^{-24}	0.11	0.315
$W^{\pm}$ boson	80377±12	3×10^{-25}	0.02301	
Z^0 boson	91187.6±2.1	3×10^{-25}	0.02028	
Top quark	172760±300	5×10^{-25}	0.00642	0.535
Lithium-4	3751.304±0.002	1.31×10^{-21}	0.000113	0.251
Higgs boson	125110±110	$(1\text{-}5)\times 10^{-22}$	0.00004435	
Tauon	1776.86±0.12	$(2.903\pm 0.005)\times 10^{-13}$	10^{-12}	
short kaon K^0	497.611±0.013	$(8.954\pm 0.004)\times 10^{-11}$	10^{-14}	0.7
kaon $K^{\pm}$	493.677±0.016	$(1.238\pm 0.002)\times 10^{-8}$	9×10^{-17}	
long kaon K^0	497.611±0.013	$(5.116\pm 0.021)\times 10^{-8}$	2×10^{-17}	
Polonium-212	197466.38	4.31×10^{-7}	7×10^{-21}	0.0189
Cobalt-60	55828.0019	2.4×10^{8}	4×10^{-35}	5.06×10^{-5}
Curium-250	232947	3.8×10^{11}	6×10^{-39}	0.5
Bismuth-209	194664	9×10^{26}	3×10^{-54}	0.0192
Tellurium-128	119142.2	10^{32}	3×10^{-59}	7.285×10^{-6}



3. The table shows some of the fastest-decaying unstable particles known, and computes their ΔM/M from their mass M and estimated mean lifetime τ via the GiSaWo bound. The "Roper resonance" (Burkert & Roberts 2019) is the first excited state of the proton. The isotopes from cobalt-60 onward (not ultrashort-lived) are included merely for comparison purposes. We also tabulate "lost mass," the mass difference between the heaviest decay product and the initial mass (in cases with sufficiently-unique decay reaction) merely to demonstrate its non-relationship to the mass *uncertainty* ΔM.

Beta decay: a look at mass-uncertainty causing important observed physics. The W^- boson's high mass-uncertainty has the very important practical consequence of allowing "β^- decay" of many radioisotopes, e.g. Cobalt-60. In this kind of decay, a neutron converts into a proton by emitting an electron and antineutrino. A more detailed picture: A down-quark inside that neutron converts into an up-quark, emitting a W^- boson; then that decays into an electron and an antineutrino which fly away. Now we must ask: how can a neutron (mass≈1 GeV) possibly convert into a W^- boson with mass over 85 times heavier? (Indeed, the W-boson outweighs the entire Cobalt-60 atom.) In non-quantum physics that would be impossible. One way to explain this is: due to an extraordinarily large (and hence *rare*) mass-*fluctuation*, this W^- boson happens to have mass on the order of 10^{-4} times its usual mass! This rarity causes β-decay to be *slow*: As far as I know, the fastest β-decay halflife is 10.5 millisec for B-15, which is 122000 times longer than the fastest α-decay halflife 86 nanosec for Fr-215. It also causes β-emitted neutrinos to interact very weakly, e.g. measurements show they could pass through lightyears of matter without participating in any inverse β-decay, even for an atom-type energetically favoring that. However, no such obstacle exists for *high*-energy neutrinos, i.e. with energy comparable to or exceeding the W-boson mass 80.4GeV, so they interact quite well with matter. For those, the "weak" force is comparable to electromagnetism, not weak at all. There is a nice way to **assess** *how much* the W's mass-uncertainty, due to its short but positive lifetime, alters beta-decay reality: contrast the old "Fermi model" of beta decay versus reality (which presumably agrees with the later "standard model"). Fermi died in 1954 hence never conceived of W-bosons, which were proposed in the late 1960s and discovered in 1983. Therefore Fermi's 1934 model of beta-decay is essentially the same as the "standard model" picture *except* in the limit of infinitesimally short W-lifetime. The *discrepancy* between Fermi's and the standard model's predictions, therefore, is entirely due to the W-boson's finite positive lifetime and mass. Let us now examine the data. The reason B-15 has such a short halflife is the extraordinarily high energy (19 MeV) released in its β-decay. The longest-lived beta-decaying isotopes are unknown because it becomes difficult to observe decays with lifetimes>10^{24} seconds, but undoubtably they live far longer than that. I plotted the halflives and decay-energies of the 78 beta-emitters B-15, Be-12, Ne-26, Ne-25, Li-8, O-21, O-20, O-19, Ne-23, Pa-234m, Tl-210, Ac-232, Ac-230, Bi-211, Tl-208, Ne-24, Tl-207, Cu-66, Ac-231, n-1, Bi-214, K-44, Pb-214, Pb-211, Cl-38, Tc-94, Ac-229, Ar-41, Pb-209, Ac-228, Pa-234, Pb-212, Na-24, Th-231, Sn-121, Au-198, Y-90, Ca-47, Bi-210, Er-169, Pr-143, P-32, Th-234, P-33, Sr-89, Y-91, S-35, Ca-45, Ru-106, Pm-147, Co-60, Ra-228, Kr-85, H-3, Cd-113m, Pu-241, Pb-210, Sr-90, Cs-137, Ar-42, Ni-63, Si-32, Ar-39, C-14, Nb-94, Tc-99, Cl-36, Se-79, Be-10, Cs-135, Fe-60, Pd-107, Hf-182, I-129, Re-187, Rb-87, In-115, Cd-113 (listed sorted by halflife) on loglog paper, finding that all these datapoints lie between two lines. The slopes of those two lines indicate that **$\text{energy}^\gamma\text{halflife}=\text{constant}$** for β-emitters, for appropriate exponents γ with **$3.65<\gamma<14.72$**. Fermi's model predicts **$\gamma=5$** at decay-energies substantially greater than the electron rest-energy 511 keV, i.e. (almost) equivalently, halflives≲20 minutes; while for energy-releases small in comparison to 511 keV (long lifetimes), it predicts **$\gamma=3$**. So to summarize, Fermi's model predicts **$3<\gamma<5$**. If we **improve Fermi** by postulating reasonable guesses for the (presumably increasing from 0) behavior of the W-mass probability density near zero mass, that would **increase** Fermi's γ's thus making his predicted γ-interval (3,5) agree better with the experimental (3.65,14.72). So, at least naively, my plot seems to demonstrate the W's mass-uncertainty in action causing an important effect. (Less naively, we could consider the "selection rule" status for each datapoint. But since it appears that would only strengthen our conclusions, I will not delve into that.)

4. The lifetime of a composite of N identical subsystems should be of order 1/N times the subsystem lifetime, my point being that ΔM/M is *unaffected* by N-fold cloning. If the subsystems are *independent* one could perhaps argue the net ΔM should be smaller than the sum of the N component ΔM's (e.g. only about $N^{1/2}\Delta M$) due to partial cancellations. Either way, any system made of the tabulated (or any other known) particles should have ΔM/M≪1.

5. Obviously, every normally-encountered macroscopic object has undetectably small inherent mass-uncertainty, $|\Delta M|\ll 10^{-20}M$.

In view of 1-5 above, it seems reasonable to **conjecture** that

A. No macroscopic physical system can ever have greater ΔM/M than two close-orbiting near-equal black holes.
B. And the only physical systems whose ΔM/M values can compete are some of the most-unstable subatomic particles (which, of course, are inherently *quantum* objects), the best one I know being the (currently poorly understood) "Roper resonance."
C. Two close-orbiting near-equal black holes are an inherently *quantum* macroscopic system, innately requiring *quantum gravity* for precise treatment④.

Now we must ask – to use a technical term – what the hell?!?!

"Macroscopic quantum phenomena" (**MQP**) can be weird and mysterious. The two most familiar are *superconductivity* and *superfluid* liquid helium. I suspect that if these two phenomena had not been discovered by accident, then no theorist would have been smart enough to predict them. As it was, H.K.Onnes discovered in 1911 that mercury superconducts below about 4°K. It took until about 1961 (50 years later) before Bardeen, Cooper, Schrieffer, and Eliashberg (BCSE) gained (what some contend to be) theoretical "understanding" of that – although those 4 people remained not smart enough to predict the Josephson effect. This understanding, however, even as of year 2023 remains rather pathetic. If we had real understanding, then a supercomputer could mentally search all possible ≤6-atom chemical compounds and tell us the predicted best (e.g. highest T_c) superconductor. But that never happened; essentially all decent superconductors have been found by experimenters operating on hunches or by random trials, with near-zero quantitative theoretical help. And even BCSE far exceeds present understanding of the high-T_c cuprates. Incidentally, superconductor T_c's are not related to $\hbar$ in any simple way, unlike (say) their Fermi temperature – much like our black hole ΔM/M's independence from $\hbar$.

Superfluidity in liquid helium below 2.17°K was first discovered in 1937 and more-or-less explained within 30 years, but some questions remained disputed/unclear even as of ≈2018, such as the existence of "supersolids."

My point with this historical retrospective is that despite 50-100 years of intense theoretical *and* experimental examination, neither superfluidity nor superconductivity are understood nearly as well as we would like. Given this historical proof of human incompetence about MQP – and close black hole binaries are a completely new kind – plus our clear present incompetence about quantum gravity, I am unwilling to assert that I know what is going on.

As far as I am aware, the LIGO team until now has believed (LIGO GR-test papers 2019-2022) that all their data has been 100% compatible with non-quantum general relativity. I now advise them to gather that data more accurately and analyze it more!

How can we interpret this in human terms? I can only say a little. Although you might survive repeated shootings if given enough time to heal between shots, you would disintegrate if machine-gunned by 9000 bullets. What is happening here is the destruction of the deterministic nature of spacetime geometry, under a too-rapid outflux of a too-tremendous number of gravitons.

What is something that huge with that much mass quantum-uncertainty like? The GW150914 merger involved the inspiral of two holes of masses 39 and 32 suns (radii 115 and 95km), about 1.3×10^9 lightyears away from here, reaching maximum speed ≈0.6c, and radiating high power for about 0.2 seconds. The gravity waves from this event distorted lengths on Earth by factors $\pm10^{-21}$, but 1000km from the merger those distortions were ±1%. If a human went anywhere near black holes like those, i.e. with anywhere near stellar mass, then the tides would "spaghettify" and kill him. However, tides near *supermassive* holes are survivably small even at the event horizon. A human subjected to oscillating ±1% length distortions in an intense gravity wave would suffer shattered bones if the frequency of the wave were too high versus the characteristic acoustic frequencies of that bone, perhaps 3700 Hz for an adult femur, so that they could not readjust in time. But wave frequencies from super-super mergers would be very low, giving you plenty of time to readjust, suggesting this would pose no problem. So as far as *non*quantum effects are concerned, you should be able to survive being right near a super-super merger, the most energetic event in the universe! However, lifeforms and engineered mechanisms have always been able to take for granted the deterministic nature of lengths and times to extremely high accuracy. Near a binary black hole with relative mass uncertainty $\Delta M/M \in (1/380, 0.6)$, lengths and times also should exhibit the same-order inherent relative uncertainties. I do not know how lifeforms would react to or perceive an environment like that.

Contrast vs. Hawking: Hawking radiation is something that gets "more quantum" (e.g. hotter, more power) the *tinier* the black hole. But my black hole pair mass-uncertainty gets "more quantum" (e.g. bigger, lasts longer, shorter intergraviton timespans during each of which total energy is highly uncertain) the *larger* the holes.

What happens inside the hole? Time outside a Schwarzschild-metric's event-horizon proceeds forward, while time inside it points radially inward (which is why anybody incapable of backward time-travel cannot escape from the hole). The horizon itself is a "null surface" which "does not experience time." Long (as regarded by faraway observers) waves become infinitesimally short on Schwarzschild horizons due to infinite blueshift factors. So: can quantum length-uncertainties which occur merely *temporarily* around merging from the standpoint of an external observer, get blueshifted to microscopic size then "frozen" onto the horizon, thereby causing the spacetime metric inside the final hole to be quantum-weird *permanently*?

Is this related to "chaos"? Computer GR studies by Zelenka et al 2020 indicate that black hole binaries with one tiny-mass *spinning* hole orbiting a large-mass Schwarzschild (non-spinning) hole can exhibit "chaos" (permanent exponential amplification of infinitesimal perturbations in initial conditions) at astrophysically achievable parameter values. However there is no chaos (Wu & Huang 2015) if the two holes have comparable masses with only one spinning. Black hole binaries do not exhibit chaos if the spins are too small and/or if they are nearly aligned with the orbital angular momentum (Levin 2003 & 2006); although chaos is quite common in the relativistic regime (Hartl 2003) if both holes spin, and becomes more common with more misalignment and larger spins. Lyapunov exponents can be as large as one e-factor growth of infinitesimal discrepancies per 5 orbits.

Such chaotic nonquantum dynamics, when present, could provide an additional mechanism for generating quantum uncertainty which could synergize with our mechanism.

What experimental results does all this predict? My ignorance of the correct theory of quantum gravity makes it difficult for me to say. I would like to see others try to predict this based on different possible postulations about the nature of quantum gravity. However, let me discuss one possible experiment. This experiment is absurdly infeasible with present day human technology, but might be possible for some hypothetical super-advanced civilization who can travel near the merging black holes and set up measuring instruments ahead of time – and who could redo this multiple times for multiple examples of merging black holes with identical initial conditions each time. Suppose they set up a network of laser beams through the binary-black-hole system, plus numerous optical sensors. The laser beams are gravitationally deflected, hitting this or that sensor. **Question:** will the laser beams get deflected in the exact same way at exact same times upon redoing the whole experiment with the same initial conditions? If the answer is "no," then we've just observed an effect of quantum gravity contrary to anything predicted by classical gravity. If the answer is "yes," then we've proven that mass actually is *not* uncertain – contradicting the well-established energy-time quantum uncertainty principle – whereupon everyone would want to know *why*.

(More feasible versions of that experiment might involve mass measurements via "gravitational lensing"; or quantifying the "noise" that prevents complete reproducibility of LIGO signals.)

Despite the large mass uncertainty ΔM for close near-equal black hole binaries and evidence mass-uncertainty can cause important observable physical effects, it remains possible that our new MQP **might "not matter."** Why?

(a) Regard the black hole binary as an unstable quantum system, *but* each time it radiates another graviton, we get a *new* such system. If all these systems were "independent" in some suitable sense then their mass uncertainties might, if observed "blurred" over long time spans (say 10^{20} graviton-emissions) largely **cancel**, e.g. effectively reducing ΔM by a factor 10^{10}.

> And indeed, notice that the claimed experimental error bars on some of our tabulated particle masses are considerably smaller than their inherent uncertainties, due to averaging over many observed particles. Also note, the mass of the black hole pair is highly certain both long *before* and long *after* merger – and hence the total radiated wave energy also ultimately becomes highly certain – high uncertainty only occurs *during* the high power stage of merger. (Similarly, in most atomic β-decays, the mass is highly certain both long before and long after, even though the W^--boson must have high mass-uncertainty – at least comparable to ±2 proton masses – during.) That already is one sense in which "averaging" over time indeed gets rid of uncertainty.

But, at least naively, it seems as ridiculous to argue for such "independence" as it would be to argue that the Earth, after emitting one photon, reaches a state "independent" of its prior state – indeed, considerably *more* ridiculous if we want the *mass* to become a random deviate "independently" re-sampled from a distribution with standard deviation 10^6 solar masses, after each single graviton emission. I currently have almost no clue to what extent the "independence/blur/cancel" hypothesis is valid or useful; and to what extent, and how, the largeness of ΔM/M "really matters." This might relate to the so-called consistent histories interpretation of quantum mechanics, which ought to somehow constrain possible observation-results.

(b) We noted the unusual peculiarity that the peak "quantum uncertainty" ΔM in mass, does **not depend on $\hbar$**. Usually, "quantum effects" go to zero when $\hbar\to0$. Does this mean this quantum uncertainty is "really not quantum" somehow? And is it related to the "lost mass" (which also does not depend on $\hbar$, and has the same order of magnitude as ΔM)?

My answers: First, that was far too facile: our ΔM/M formula also is independent of c and G, but it would be absurd to contend it is "not really relativistic or gravitational." Second, at least for general decaying systems, lost-mass and mass-uncertainty clearly are very unrelated, and the derivation shows my peak ΔM really is quantum in origin despite $\Delta M/M$'s lack of dependence on $\hbar$. (Actually it *does* involve $\hbar$, but in two ways, which cancel.) Third, the ΔM and lost-mass concepts differ greatly in their time-behavior. For $M \approx 10^9$ suns, it takes months to lose that mass; but the mass-uncertainties ΔM arise on time scales 10^{-88} seconds.

Are those non-dependences on $\hbar$ merely artifacts of my model being the leading order post-Newtonian approximation, so that $\hbar$'s would appear in higher-order correction terms? No: I claim peak $\Delta M/M$ is independent of $\hbar$ at *all* orders in the postNewton expansion. Why? General relativity (GR) obeys the *scale invariance* property that scaling up all masses, lengths, and times by an arbitrary factor s, leaves GR unaltered. Hence the power P radiated is s-invariant, while the frequency F scales like s^{-1}, hence the mean timegap $\tau = hF/P$ between graviton emissions will scale like h/s, hence peak ΔM will scale like hc^{-2}/τ which (like M) scales like s – and note the h has canceled out – so peak $\Delta M/M$ is both $\hbar$ and s-invariant. Q.E.D. Since the "lost mass" also is s- and $\hbar$-invariant for any standardized hole-inspiral scenario, we see⑤ that peak $\Delta M/M$ and ultimate (lost mass)/M indeed will be the same for our problem, up to a factor that is an evidently-nonconstant function of initial-spin data.

Perhaps **scale invariance is a *hallmark* of macroscopic quantum phenomena**, indeed exactly what permits them to reach macroscopic size. You might object that the entire description of superfluid liquid helium certainly is not scale-invariant. Yes, *but* the key parameter, the number density of helium atoms times the cubed thermal wavelength of one such atom, *is* unchanged (at any fixed temperature) by getting a bigger bottle of liquid helium. Another example is a single photon: its $\Delta E/E$ is invariant on scaling to long wavelengths, and does not depend on $\hbar$.

To conclude: that $\hbar$-independence indeed looked suspicious a priori, but seems less so a posteriori. Anybody still wanting to decry our $\Delta M/M$ as "not really quantum" probably would need to make major advances in "interpretation of quantum mechanics" before hoping to convince anybody.

(c) Suppose the mass of the black hole binary somehow keeps getting "**measured**" extremely frequently, thereby preventing it from being very uncertain. The concept of quantum measurement has always been mysterious... and it is hard to imagine how measurements could happen at any enormous-enough rate... but anyhow this is another conceivable way our effect might "not matter." On the other hand that very measurement *itself* might be a quantum-gravitational effect, in which case quantum gravity *would* "matter."

There simply is zero prior experience with any macroscopic system featuring nonnegligible $\Delta M/M$. All I can say for now is: this certainly seems worthy of investigation.

Interpretation as a "crisis" in physics, and suggested resolution

Everything up to here was present in the second draft of this paper. I circulated it to numerous physicists, especially ones involved in black hole observations and quantum gravity thinking. The vast majority of them (as far as I could tell) ignored it. However, a few (not the ones I would have expected!) got interested, leading to a correspondence involving numerous comments and questions. I got the feeling that they all wanted this to go away and felt that something must be wrong with it. I could sympathize with that feeling. However, neither they nor I were able, despite numerous attempts, to find anything wrong with my arguments. And all the deeper investigations I conducted, stimulated by those comments and questions, seemed only to reinforce my original arguments.

However, with the third draft, I now am adding this (new) section, which changes that situation. I will now present arguments suggesting that all this leads to a *contradiction* arising from two well known Theorems of Quantum Mechanics. (The prior drafts has simply taken quantum mechanics for granted, making no attempt to question it.) If so, at least one of those two theorems, and hence necessarily one or more axioms, of Quantum Mechanics must be *wrong*. And I believe I now know what that wrongness is, and will demonstrate how to repair it. Indeed, I've long had a book-in-progress about my attempt to rescue quantum field theory (see Smith 2023) and the new "cloud" ideas in that book already suffice (as far as I can tell) to repair this problem. If so, the present paper, and all the "tests of GR" conducted by the LIGO team until now, can be regarded as evidence supporting my book.

The **two theorems** are: (1) the energy-time uncertainty inequalities we already discussed, (2) the theorem that the entire energy probability distribution, for any state governed by Schrödinger equation (assuming the Hamiltonian does not depend explicitly on time), is time-invariant.

I'll now explain the **contradiction** in the context of a supermassive binary black hole inspiral with $M_{tot} \approx 10^9 M_{sun}$ occuring inside a galaxy-core and hence with stars nearby⑥, say 3 light-days distant. As we've said③, the high-power stage of this inspiral would last for ≈9 weeks. Under Schrödinger equation QM (if time-invariant Hamiltonian), the entire energy probability distribution is time-invariant. (With general relativity there is no absolute "time" anymore, confusing this issue. But for present purposes let us ignore that to avoid that confusion. If the Wang-Yau surface is large enough that the time dilation factor there stays bounded by, say, 2, presumably this cannot hurt us much.)

We begin the inspiral process with high-certainty mass. Once we reach the high radiation-power stage, the mass of the binary system (inside a Wang-Yau surface) then is highly uncertain, with standard deviation of same order as total mass. But the total mass (both inside *and* outside, that surface, i.e. system plus previously-radiated gravitons) still is highly certain, by theorem 2. Therefore the summed energy of those radiated gravitons also must be highly uncertain. However, once those gravitons disperse far enough to reach the rest of the universe, such as LIGO, or the nearest stars, assume their total energy gets *measured* and therefore becomes highly certain.

Therefore, the highly-uncertain mass inside the Wang-Yau surface *must* be accompanied by the same high-uncertainty for the total mass of the (comparatively few) radiated gravitons which have not yet reached nearby stars, i.e. <3 days worth of radiation. (And if the merging black hole binary happens to be a *quasar*, i.e. there also is an accretion disk made of hot inflowing gas, then such measurement ought to occur substantially sooner than 3 days.) But there simply is not enough total energy radiated over a 3 day period to contain enough uncertainty for that! **Logical contradiction.**

We conclude that either (1) energy-time uncertainty inequality theorems or (2) the time-invariance of energy-probability-distribution theorem (or both) are wrong when applied in our context.

Suggested resolution. I suggest that all (1) $\Delta E \Delta t$ lower bounds **lose justification** whenever ΔE exceeds ≈1 Planck mass unit (21 micrograms) or equivalently whenever Δt is shorter than ≈1 Planck time unit (5×10^{-44} sec). The reason for this failure is that all $\Delta E \Delta t$ lower bound theorems had been based on time-domain *Fourier analysis*. A known consequence of Smith 2023's "**cloud QFT**" theory is an *ultraviolet cutoff* preventing high frequencies (higher than ≈1 Planck frequency units) from existing – one reason being (what is called there) the "Debye-Nyquist argument." More precisely: any wavefunction frequency components above the UV cutoff automatically get *reinterpreted* in cloud-QFT physics as a linear combination of lower frequencies (below cutoff). Fourier analyses that employ superPlanck frequencies therefore are physically invalid. Cloud-QFT physics also does *not* conserve energy, nor is it wholy governed by any partial differential equation (like Schrödinger), at sufficiently microscopic length and time scales.

Further, this UV cutoff makes it impossible for *any* energy-uncertainty to exceed ≈±1 Planck mass, and indeed impossible for any object with mass M≳1 Planck mass even to *have* a coherent wave function with frequency f obeying $Mc^2=hf$; and the Schrödinger equation loses physical validity for such wavefunctions.

It had long been suspected that some sort of UV cutoff, most probably at near-Planck-scale energy, might exist – that idea did not originate with "cloud QFT." However, cloud QFT provided an explicit mechanism to accomplish that, and in a Lorentz inavariant way – despite that naively seeming impossible since two different observers regard a given length (or energy) as different.

Also, no explicit experimental evidence had ever before been found for the existence of (with quantitative bound produced that was not contradicted by other experimental evidence) such a UV cutoff. If astronomers now tell us they *have* evidence for the nonexistence of high-mass-uncertainty in black hole binaries, then that will be the first experimental evidence for a UV cutoff.

In light of this, let us now reconsider an argument made by R.P.Feynman at the 1957 Chapel Hill gravity conference. Feynman was trying to argue for the existence of gravitons and the quantum nature of gravity. To do so, he imagined the following kind of experiment. A Cm-250 atom (halflife≈8300yr) spontaneously fissions, or remains intact. If the latter, it gravitationally-deflects a 3× heavier mass. That mass in turn (if undeflected) deflects a 3× heavier-still mass. And so on. After N stages we get a quantum superposition of a deflected (and not) heavy object – with mass≈3^N250amu – in two far-separated locations. If N=95 the final object could be the asteroid Ceres. This is so heavy that its gravity would readily be perceived. Feynman contended this argument "proved" that gravity had to be quantum in nature, *except* that (he also remarked)

> ...I would like to suggest that it is possible that quantum mechanics fails at large distances and for large objects. Now, mind you, I do not say that I think that quantum mechanics does fail at large distances, I only say that it is not inconsistent with what we know. If this failure of quantum mechanics is connected with gravity, we might speculatively expect this to happen for masses such that $GM^2\approx\hbar c$, hence M near 10^{-5} grams, which corresponds to some 10^{18} particles... This would be a new [irreversibility] principle for masses>10^{-5} gram or whatever.

Well, in fact, with cloud-QFT, it is impossible to have a heavy (≳1 Planck mass) object in a superposition of two far apart locations! In other words, Feynman's escape clause *happens*, and his argument for the quantum nature of gravity indeed is invalidated in exactly the way he worried it might be. (As far as I know, this is the first time, during the 66 years since Feynman said that, that this has been pointed out; and no previous work on energy-time uncertainty relations has ever pointed out that they should not be valid for too-large ΔE. Note that gravity could still be, and I presume is, quantum; just Feynman's *argument* for its quantumness is invalidated.) The reason for this impossibility is that if such a superposition could exist, then we would have mass-energy uncertainty of order ≳1 Planck mass within some experimentally accessible region.

Endnotes

①: Mössbauer lines with lifetimes>100ps and hence linewidths<6.6×10^{-6}eV usually get substantially broadened by extranuclear fields hence rarely can match Re-187's accuracy. A countermeasure is to embed your isotope inside a suitably *symmetric* nonmagnetic crystal (e.g. Fe-57 inside ferrocyanides). That can work well for absorbers, but for emitters you also need the (chemically different!) parent isotope to be embedded in the same way, and for all that not to be wrecked when the parent disintegrates – difficult. Decker & Lortz [J.Appl.Phys. 42,2 (1971) 830-833] used both the ferrocyanide, plus the "multiple absorber thicknesses" trick enabling *separate* determination of the absorber and emitter linewidths (not merely their sum), to find in their Table III only 30% greater Mössbauer linewidth than deduced from Kistner & Sunyar's [Phys. Rev. B 139,2 (1965) 295] directly-measured mean life 140ns for the 14.41keV excitation of Fe-57. Better – 18% and 16% – agreements: τ_{Mossb}=131±4ps versus τ_{direct}=154±33ps for the 129.4keV Ir-191 line, and τ_{Mossb}=267±6ps versus $\tau_{Coul.excit.}$=308±16ps for the 46.48keV W-183 line. [Data sources: Bullard et al: Phys.Rev.B 43,10 (1991) 7405, Baglin: Nucl.Data Sheets 134,4 (2016) 149, Mössbauer: Z.Naturforschung A 14 (1959) 211, Steiner et al 1969, Wagner et al: Phys.Rev.Lett. 28,9 (1972) 530, Owens et al: Phys.Rev. 185,4 (1969) 1555, Lindskog et al: Zeitschr.f.Phys. 170,3 (1972) 347, Malmskog & Bäcklin: Arkiv Fysik 39 (1969) 411, Narismha Rao & Jnanananda: Proc.Phys.Soc. 87,2 (1966) 455, Berlovich et al: Sov.Phys.JETP 16,5 (1963) 1144.]

②: Proofs available on request.

③: About 50 black holes have been found in our galaxy, plus about 150 supermassives with confirmed mass measurements in other galaxies, plus over a million less-studied quasars. There are 3.5×10^{11} galaxies in the observable universe based on correcting galaxy counts from the "Hubble extreme deep field" image with dark-sky optical background measures by Lauer et al 2021 from the New Horizons spaceprobe. It's estimated there are over 10^8 holes in our galaxy alone and 4×10^{19} in the observable universe (Sicilia, Lapi et al 2022); and that average galaxies contain ≈1 supermassive hole, suggesting there are 3.5×10^{11} supermassives in the observable universe. Ours, "Sagittarius A*," has mass≈4.2×10^6 suns, while perhaps the heaviest presently known is TON 618 at 6.6×10^{10} solar masses. The Schwarzschild radius 19.7 AU of a 10^9-sun black hole is comparable to the Sun-Neptune distance 28.9 AU. Two big holes, currently ≈1600 light-years apart in the galaxy NGC 7727 and massing 6×10^6 and 154×10^6 suns, are forecast to merge within the next 250 Myr. The blazar OJ 287 is believed based on optical periodicity observed over 100 years, and other observations including very long baseline interferometries dating back to 1995, to be a binary supermassive with redshifted orbital period≈12 years, with component masses (depending on the modeler) each somewhere between 10^8 and 2×10^{10} solar; one merger forecast was only 10^4 years from now. Other super-super binary candidates include the quasar QSO B1312+7837 [period 6.4 years, est.masses $(1\text{-}3)\times10^8$ solar], SDSSJ1430+2303 whose period in optical and X-rays decreased from about a year to a month in 3 years, suggesting merger before year 2026 [estimated masses $(40\text{-}800)\times10^6$ solar], the blazar PKS 0346-27 [100-day period, mass of primary $(9\text{-}60)\times10^9$ solar], SDSS J025214.67-002813.7 [4.6 cycles detected over 20 years of observation, summed mass $10^{8.4\pm0.1}$ solar with mass ratio 10], and PKS 1302-102 [period 1884±88 days, separation 0.1 parsec, masses $10^{8.3\text{-}9.4}$ solar]. This all suggests that (very roughly) 1 super-super merger occurs per month in the observable universe. LIGO detected ≈170 mergers of black holes during 2016-2023 whose individual masses ranged from 5 to 90 suns. The lifetime of the high power output stage of such mergers is linearly proportional to their mass, and was 0.1 second for the "GW170608" merger on 16 Nov. 2017 of 10.9+7.6 M_{sun} holes yielding 17.8 M_{sun} result. Hence the merger of two equal holes yielding a 10^9 M_{sun} result should output high power for 9 weeks.

④: *Non*quantum gravity (Newton or Einstein) is generated by *deterministic* masses, meaning ΔM≈0M. Therefore the gravity generated by any system with large *inherent* mass uncertainty (meaning ΔM of order M/1000 or more) can only be described by *quantum* gravity.

⑤: **More consequences of Newtonian inspiral model: (a)** A similar scaling argument shows the duration of high power emission (meaning power exceeding any fixed positive constant times the peak power or Planck power) corresponds to a *constant* number of orbits, independent of m+M.

(b) Behavior of the power curve like $(t_{sing}-t)^{-5/4}$ at least for times t sufficiently before t_{merge}, where t_{merge} is the time of topological horizon-merger, and t_{sing} the later time at which the two point masses would in our model merge into one point; to get this result we need to assume/pretend the spiral always is very near-circular.

(c) Automatic circularization of elliptical orbits – which partly justifies (b).

Jupiter

Period(days)	#moons	Eccentricities
0.29-100	8	0-0.02
100-450	11	0.11-0.24
450-727	38*	0.14-0.30
727-800	36	0.25-0.44

We first explain the **circularization**. If you were circularly orbiting the sun and wanted to convert to an elliptical orbit with the same energy, then (i) fire your rocket to "slow down", causing you to drop toward the sun on an elliptical orbit with perihelion located where you'd fired the rocket. Then (ii) when you reach aphelion, re-fire the rocket to "speed up" to regain your lost energy. Conversely, if in an elliptical orbit you want to circularize, then contrive to lose energy at aphelion and regain it at perihelion. This is exactly what our radiated-power formulas predict happen – maximum radiative losses occur at aphelion and minimum at perihelion – therefore orbits should automagically circularize as black holes inspiral. The same idea also should work for Jupiter's moons, except their energy losses are caused by tides (my point being that tides are strongest at aphelion) rather than gravitational wave emission. **Experimental confirmation:** the table of Jupiter's 95 moons known by March 2023 shows the innermost have the least eccentricities, as expected since they experience the biggest tides. This table assumes Themisto's mean eccentricity≈0.25 [M.Brozovic & R.A.Jacobson: Astronomical J. 153,4 (2017) 147] and its third (starred*) row omits the two "outlier" moons Carpo (period=456, eccent=0.416) and S/2003 J 18 (period=598, eccent=0.09) to make the trend clearer. The other table shows Saturn's 146 moons; its last row would have eccentricities 0.087-0.551 if its two least and two greatest eccentricities were omitted.

Now we explain the $(t_{sing}-t)^{-5/4}$ behavior. The Newtonian potential energy of two point masses M and m separated by distance L is E_{pot}=-GmM/L. The Newtonian kinetic energy $\Omega^2 I/2$ if they orbit their center of mass according to Kepler-Newton laws equals E_{kin}=(½)GmM/L. Therefore the total energy is $E=E_{kin}+E_{pot}$=(-½)GmM/L. Incidentally, **(d)** an immediate consequence of this E-formula and our prior L_{merge} formula is that at the moment of topological merger, $|E|=mMc^2(m+M)^{-1}([m/M]^{1/2}+1)^{-1}/4$. This implies that in the m=M case, the radiated **"lost" mass** is predicted to equal **(m+M)/32** at the moment of merger. By conservation of energy and Eddington's radiation-power formula $dE/dt=(-32/5)m^2M^2(m+M)L^{-5}G^4c^{-5}$ as we inspiral (approximating the spiral as a succession of infinitesimally radially-spaced concentric circles). Hence $dE/dt=JE^5$ where for conciseness I have written $J=(1024/5)m^{-3}M^{-3}(m+M)G^{-1}c^{-5}$. The solution of this differential equation is $E=-2^{-1/2}J^{-1/4}(t_{sing}-t)^{-1/4}$, yielding the "**power curve**"

Saturn

Period(days)	#moons	Eccentricities
0.47-20	23	0.000-0.029
20-754.3	14	0.028-0.384
754.4-926	23	0.084-0.500
926-1640	86	0.060-0.625

$$P_{binary}(t) = -dE/dt = 2^{-5}J^{-1/4}(t_{sing}-t)^{-5/4} = (5^{1/4}/32)\, m^{3/4}M^{3/4}(m+M)^{-1/4}\, G^{1/4}c^{5/4}\, (t_{sing}-t)^{-5/4}.$$

(e) The Kepler-Newton proportionality of orbital period to $|E|^{-3/2}$ combined with our proportionality $|E|\propto(t_{sing}-t)^{-1/4}$, so that period$\propto(t_{sing}-t)^{3/8}$, usefully allows prediction of the future time t_{sing}: Suppose astronomers observe two successive orbital periods $P_1>P_2$, with t=0 at the start of period 2 (and end of period 1). We solve $Z\equiv(P_1/P_2)^{8/3}= (t_{sing}+P_1/2)/(t_{sing}-P_2/2)$ to find $t_{sing}\approx(P_1+P_2Z)/(2Z-2)$.

(f) Timescales. By combining the Kepler-Newton law $(m+M)G=\Omega^2L^3$ with our formula $L_{merge}\equiv2([m/M]^{1/2}+1)(m+M)Gc^{-2}$ we deduce the orbital-period $T_{merge}=2\pi/\Omega$ at the time of topological merger: $T_{merge}=2^{5/2}\pi Gc^{-3}(m+M)([m/M]^{1/2}+1)^{3/2}$.

By setting $P_{binary}(t)=P_{minsep.binary}$ we can solve for t and hence deduce the timescale $t_{sing}-t_{merge}=(5/16)(m+M)^3m^{-1}M^{-1}Gc^{-3}([m/M]^{1/2}+1)^4$.

⑥ **High star densities in galaxy-cores:** Proxima Centauri is believed to be the closest star to our sun at distance 4.2465 light years. The average stellar density near here is one star every 10 cubic parsecs (347 cubic light years), which if stars were 3D-Poisson distributed would imply about 4 light-years mean separation between a star and its nearest neighbor. But galactic *core* regions and the cores of globular clusters can exhibit much greater stellar densities. M15 is a globular cluster suspected to contain a $1700M_{sun}$ black hole. A Hubble Space Telescope image found over 150000 stars per cubic lightyear near its core (Baumgardt, Hut et al 2003; see esp. fig.2 at radius=0.01pc) i.e. >50 million times denser than our region of our galaxy. This corresponds under the same 3D-Poisson assumption to mean typical distance-to-nearest-neighbor 0.0104 light-year, or 659 AU, between stars. Tod Lauer (National Optical Astronomy Observatory) calculated that observers at the center of the compact elliptical galaxy M32 (satellite of Andromeda) would see a "night sky" as bright as twilight on Earth. But even at these high star densities, collisions are rare. Globular clusters have stars in their centers called "blue stragglers," which astronomers think are new stars formed by the collision of two old lower-mass stars. Fewer than one in 10000 globular-cluster stars are blue stragglers, indicating the rarity of stellar collisions even in these extreme environments.

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