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

By Warren D. Smith, `warren.wds@gmail.com`, August 2023. Version 3 (Sept-Oct.2023) after detected/corrected factor-2 error; reader comments/questions mostly by David J. Broadhurst & Veit Elser; then finally adding new section proposing possible "resolution of crisis." Version **4** (Dec.2024): new section added re "Copenhagen controversy." http://vixra.org/abs/2309.0044.

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Abstract. Close-orbiting black hole pairs with near-equal masses M≈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. Lengths and times measured in the vicinity also should have relative uncertainties≳1/380. 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 1 hour of super-super hole tight orbiting than the total count of non-graviton particles in the observable universe.

Perhaps you think it crazy anything that huge can have this large inherent mass, length, or time uncertainties. If so, then you interpret this as a **paradox** or **crisis** which needs resolution via adding some extra ingredient to today's laws of physics. The penultimate section, added for the third draft, proposes as that extra ingredient: the ΔEΔt 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. The final section, added for the fourth draft, covers the "Copenhagen controversy" and related data analysis, which I *wish* the LIGO team would publicly perform every time instead of ignoring.

Significance

> The central problem of fundamental theoretical physics today is reconciling general relativity with quantum theory.
> – Stephen A. Fulling [p.217 of *Aspects of quantum field theory in curved space-time*, London Math'l Soc. (Student texts #17), Cambridge Univ. Press 1989, reprinted 1996].

> Even a few years ago it was a generic consensus that, most likely, it's not even conceivably possible to measure quantization of the gravitational field in any way.
> – Igor Pikovski ("theoretical physicist at Harvard University"), quoted by Natalie Wolchover in *The Atlantic*, March 2018, although disputed by Smith 2021.

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" (EHT) got $>10^9$ \$US funding and a physics Nobel. There are many proposed big enhancements including "LIGO on the moon," LISA, pulsar timing arrays, 10×-bigger LIGO, 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 $\hbar\approx1.055\times10^{-34}$ 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≥0, and **probability-density ϱ(E)** for its initial energy E. If the system were described by a wavefunction Ψ(x,t) then Q(t) with $0\leq Q(t)\equiv|\int\Psi^*(x,0)\Psi(x,t)dx|^2\leq1$ (the integration is over the whole real line) is the probability the system remains in its initial state Ψ(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 τ_{BM}=L. And any Q(t) falling proportionally to $t^{-\gamma}$ (or faster) when t→∞, for any fixed exponent γ>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)

$$\max_E \varrho(E) \leq 4\, \tau_{BM} / h \quad \text{where} \quad h=2\pi\hbar\approx6.626\times10^{-34} \text{ joule seconds is Planck's constant.}$$

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/τ) 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 $\Gamma=(4.4\pm0.5)\times10^{-5}$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 plausibly[①] 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 extranuclear 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±0.009)×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 "**halflife**" $\tau_{1/2}=\min_{t>0}\{t \mid Q(t)\le 1/2\}$ 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,∞) 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) $\tau_{mean}\Delta_1 E>0.2889\hbar$ and more generally for any fixed k>0 that $\tau_{mean}\Delta_k E>c_k\hbar$ where $\Delta_k E\equiv[\int|E-\bar{E}|^k\varrho(E)dE]^{1/k}$ and the c_k are appropriate positive constants. I can also prove: If the narrowest energy interval $\mathcal{I}$ containing at least 31% probability $[\int_{\mathcal{I}}\varrho(E)dE\ge 0.31]$ 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 $\Delta_k E$ 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 $\Delta_k E=\infty$ for each real 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 340 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, i.e. *twice* orbital frequency) 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. 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.

But really Pb-206 is not the end of the story. Gamow's 1928 model of α-decay indicates that Pb-206 must be unstable too, just with unobservably-long halflife $10^{54.8\pm2}$ years. Further, Pb-206 → Hg-202 → Pt-198 → Os-194 ⇒ Ir-194 ⇒ Pt-194 → Os-190 → W-186 → Hf-182 ⇒ Ta-182 ⇒ W-182 → Hf-178 → Yb-174 → Er-170 → Dy-166 ⇒ Er-166 → Dy-162 → Gd-158

where all the → are α-decays with halflives too long to have ever been observed (my predictions range from $10^{26\pm2}$ years for W-182 up to $10^{372\pm2}$ years for Er-170), while all the ⇒ are β^- decays with halflives ranging from 19 hours to 9 Myr.

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 much shorter than an orbital period. So whether graviton emissions are "distinct" versus "overlapping" does not have a plain yes/no answer.

What is a "decay"? 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 mω=ℏ, and the H_K denote Hermite polynomials] 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 typical amount of emitted-particle overlap appears *irrelevant*. Hence 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. In other words: this quantum-state-orthogonality argument indicates that even if many or all the emitted gravitons were identical (analogous to photons from a laser or radio-transmitter's antenna) that would not hurt my upcoming uncertainty lower-bounding argument.

(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 $\Delta M_{binary}\ge5^{-5/2}432\pi c^{-7}G^{7/2}M^2m^2(m+M)^{1/2}L^{-7/2}$.

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 $L_{merge}\equiv2([m/M]^{1/2}+1)(m+M)Gc^{-2}$. 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 point 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 $P_{pl}\equiv c^5/G\approx3.6283\times10^{52}$ 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 M/m 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 M/m≥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; and also agree with the idea by Palmese, Bom, Mucesh, Hartley 2021 (which they trace to Schutz 1986) that black hole mergers are "standard sirens" which (like the older idea of "standard candles," but better) usefully enable deduction of the "Hubble constant." 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. H&L's 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≈2ln(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 $\Delta M_{binary} \ge 5^{-5/2}2^{1/2}3\pi\, M^2m^2(m+M)^{-3}\, ([m/M]^{1/2}+1)^{-7/2}$ which when m=M becomes

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

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 halflife inequality $\tau_{1/2}\Delta E\ge\pi\hbar/4$ 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 $<10^{91}$ 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 10^{-3}-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 $\lim_{r\to0+}L_n(r)=0$ 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 $\lim_{r\to0+}L_n(r)=0$ 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 $WYmass(B)\le WYmass(A)$. 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 $A\cap B=\varnothing$. "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 $T_{abcd}E^aF^bG^cH^d\ge0$, 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 trying to understand how amazing this all is and what it might mean

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

The rod-shaped interstellar **asteroid** "Oumuamua" has $L\approx400$ meters, rotation period≈8 hours so $\Omega\approx2\times10^{-4}$/second, and if made of iron has mass $M\approx4\times10^{9}$ kg. I compute $\Delta M\approx8\times10^{-61}$ 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 $\Delta M\approx9\times10^{-41}$ kg, which still is 10 orders of magnitude smaller than the mass of a single electron.

Our **sun** has luminosity≈3.83×10^{26} watts, with surface temperature $T\approx5772$°K and hence mean energy for emitted photons $2.7k_BT$. Hence it emits about 1.8×10^{45} photons/second. If we regard the whole sun as a quantum system with mean decay time 5.6×10^{-46} second, then its inherent mass-uncertainty is $\Delta M\approx c^{-2}\hbar 1.8\times10^{45}$/second≈1.8 milligrams. The **earth** receives 1.7×10^{17} watts of sunlight, and re-radiates it mostly as 287°K blackbody radiation, i.e. emitting 1.6×10^{37} photons/second. The latter causes mass-uncertainty $\Delta M\approx1.9\times10^{-14}$kg.

Those were two instances of the formula $\Delta M=0.37021P/(k_BT)$ for the mass-uncertainty of any hot (temperature=T) object caused by it radiating power=P worth of blackbody radiation. The precise formula for 0.37021 is $30\zeta(3)\pi^{-4}$. We now apply that formula to other objects. BAT99-98 in the Large Magellanic Cloud arguably is the **most luminous known star**. It is believed to have mass 226 times our sun, luminosity≈1.9×10^{33} watts equivalent to 5×10^{6} suns, surface temperature≈45000°K, and radiates 1.1×10^{51} photons/second. If we regard this entire star as a quantum system with decay time 0.9×10^{-51} second, then its inherent mass-uncertainty is $\Delta M\approx c^{-2}\hbar 1.1\times10^{51}$/second≈1.1 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 $\Delta M\le10^{-28}M$ 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.

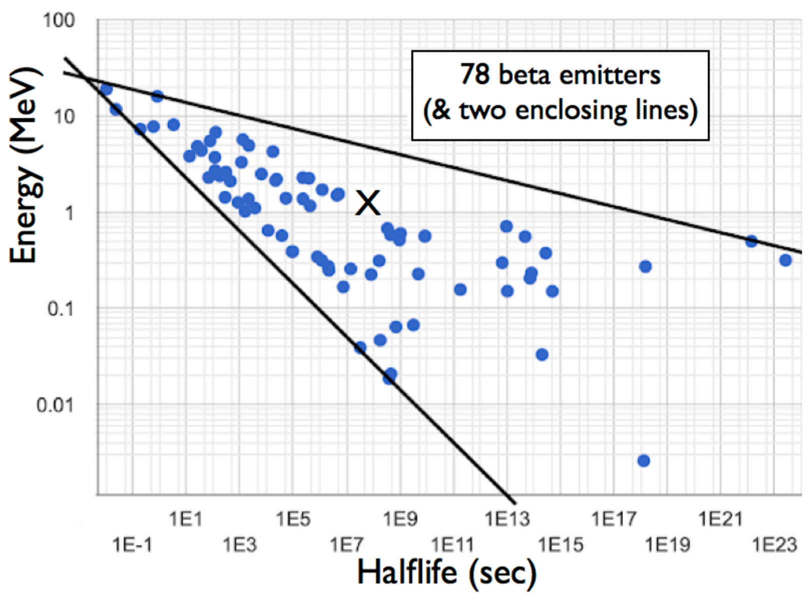
2. The power P classically radiated by a **rotating electric dipole** (moment=D, angular velocity=ω) is $P=mu_0D^2\omega^4/(6\pi c)$. If you wanted to radiate huge numbers of photons, you would face two problems: (a) QED prevents sustaining electric fields greater than the "Schwinger critical field" $E_{crit}=m_e^2c^3/(e\hbar)\approx1.32\times10^{18}$ volts/meter because they would break down the QED vacuum by rapidly generating e^-e^+ pairs. (b) Rotation speed is bounded by c. These force $P\lessapprox P_{QED}=(E_{crit})^2\varepsilon_0c\approx4.63\times10^{33}$ Watts, which is numerically much smaller than the Planck power unit $P_{pl}\approx3.63\times10^{52}$ Watts. At F rotations/second the emission rate $P_{QED}/(hF)$ of photons/second therefore would be upper bounded by something 19 orders of magnitude smaller than the graviton emission rate, of order $P_{pl}/(2hF)$, from a near-merger black hole binary with the same rotation rate.

Similar reasoning but involving Schwinger's critical *magnetic* field $B_{crit}=m_e^2c^2/(e\hbar)\approx4.41\times10^{9}$ Tesla, indicates the same P_{QED} also upper bounds the electromagnetic power radiated by a rotating magnetic dipole.

3. And 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 $T\approx1/\lambda$). 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: $T\ge T_{pl}\approx1.417\times10^{32}$°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: $R>L_{pl}\approx1.616\times10^{-35}$ 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	$(1\text{-}5)\times10^{-22}$	0.00004435	
Beryllium-8	7456.895	$(118\pm5.3)\times10^{-18}$	6.3×10^{-10}	0.5
Tauon	1776.86±0.12	$(2.903\pm0.005)\times10^{-13}$	10^{-12}	
short kaon K^0	497.611±0.013	$(8.954\pm0.004)\times10^{-11}$	10^{-14}	0.7
kaon $K^\pm$	493.677±0.016	$(1.238\pm0.002)\times10^{-8}$	9×10^{-17}	
long kaon K^0	497.611±0.013	$(5.116\pm0.021)\times10^{-8}$	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}



4. 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. We also could regard a bare neutron as β-decaying "atomic element" with atomic number 0, mass 1, and halflife≈611 seconds (1.29 MeV released). If that were added to the plot it would be the "X" shown. The slopes of those two lines indicate that **energy^γhalflife=constant** for β-emitters, for appropriate exponents γ with **3.65<γ<14.72**. Fermi's model predicts **γ=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 **γ=3**. So to summarize, Fermi's model predicts **3<γ<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.)

5. 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.

6. Obviously, every **normally-encountered macroscopic object** has undetectably small inherent mass-uncertainty, $|\Delta M|\ll10^{-20}M$.

In view of 1-6 above, I propose the **maximum relative-mass-uncertainty 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* microscopic 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 treatment[4].

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 $\Delta M/M$'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.

Question: Suppose, by magic, that the interior of the Earth (say, everything ≥10 km below ground) were placed in an uncertain-mass state – a **quantum superposition** of states with mass $0.5M_{Earth}$ to $1.5M_{Earth}$ (and perhaps with the spatial *distribution* of that mass also being uncertain) – persisting for one month. *What would you experience?* Because nobody knows the correct theory of quantum gravity, it is very hard to say. But if this experiment happened, and you survived it, you would know much more about quantum gravity than anyone knows now. The point of the present paper is: experiments of this ilk *are* happening out there, and we can observe them.

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 (Schwarzschild 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. But for some ideas suggesting some parallels nevertheless may exist, see Hawking 1976.

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. For example, suppose our merging black hole binary lensed light from some light source behind it. One conceivable way our system's mass-uncertainty might manifest, is that this lensing would have high quality (focus well) both long before and long after the merger; but during it, that lensing would become "blurry" and/or fluctuating with time. Another example: the LIGO team apparently fits each merger-signal they observe, to supercomputer-generated predictions from numerical GR. Because their fits seem good, they conclude that they have not seen any clear indication of anything wrong with GR. However, their fits are not perfect, a fact the LIGO team (2019-2021) attributed to "measurement noise." Question: does that "noise" actually have the right amplitude, and pass statistical "tests of randomness" from the correct noise-distribution, to be entirely attributed to their measurement devices? If not, e.g. there is "extra noise" (which really is genuine, i.e. it's a random part of the "signal"), it then could be attributed to quantum uncertainty in the gravity-wave-source, thereby proving for the first time that there *is* something wrong with plain GR, while also finding the first ever experimental clue about the nature of quantum gravity.

Although the LIGO team's "tests of GR" papers did perform many tests of GR (never finding anything wrong), they never tried *this* kind of test, nor considered any alternative-to-GR of our ilk. Up to Oct.2024, all my attempts to notify the LIGO team of this have been 100% ignored. Let me re-iterate: A simple KEY QUESTION they could feasibly address is: **Does LIGO find more noise, or does the character of that noise change, when observing an event, versus when not?** See the final section for more discussion of that.

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, or perhaps a better word is "waiting," 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 $\hbar \to 0$. 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 ΔM/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 ΔM/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 ΔM/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 ΔM/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 ΔE/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 ΔM/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 ΔM/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 had 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 suspect 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 QFT" 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 our argument 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 *un*certain. 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 – hence their total energy has not yet been "measured," – 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 suggest⑦ that all $\Delta E \Delta t$ lower bounds **lose justification** whenever ΔE exceeds ≈1 Planck mass unit ($M_{pl} \approx 21$ micrograms) times c^2, or equivalently whenever Δt is shorter than ≈1 Planck time unit ($t_{pl} \approx 5 \times 10^{-44}$ 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). Different Lorentz observers have different notions of what various frequencies are, and different reinterpretations, but all can regard there as being a UV cutoff and all with the numerically same frequency for that cutoff. Fourier analyses that employ unboundedly high superPlanck frequencies therefore are physically invalid in the view of every observer; the only valid Fourier analyses concern "band limited" spectra. 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* inherent 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 invariant way – despite that naively seeming impossible since two different observers regard a given length (or energy, or frequency) as different.

But, no explicit nontrivial 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**. Note: if there were no UV cutoff and Fourier analysis were valid for all frequencies below, say, $(10/h)\Delta E$, then the usual $\Delta E \Delta t$ uncertainty inequalities, justified by their usual derivations, still should hold (perhaps with slight alterations weakening the constants). Therefore the *only* way they do *not* still hold is if a UV cutoff exists below $(10/h)\Delta E$.

I call this the **boring resolution** of the paradox because cloud QFT predicts that all observations of black hole mergers will behave the way ordinary non-quantum general relativity says they should. That's because quantum gravity's effect in this instance is merely to "switch off" the effects quantum mechanics predicts!

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$\approx 3^N 250$amu – 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, and in exactly the way he worried about. (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 ^{250}Cm→Ceres *argument* for its quantumness is invalidated.) The reason for this impossibility is that if such a superposition could exist, then we would have energy uncertainty of order ≳1 Planck mass within some experimentally accessible region⑧, which under cloud-QFT is not possible.

The reader may not have heard of my "cloud QFT" theory before and might be more interested in other quantum-gravity theory-attempts. So...

What about other "quantum gravity" theory-attempts, e.g. superstrings and LQG?

> Black holes should be subject to the same rules of quantum mechanics as ordinary elementary particles or composite systems.
> – Gerardus 't Hooft [*On the Quantum Structure of a Black Hole*, Nucl.Phys.B 256 (1985) 727-745, first sentence].

So presumably 't Hooft's favored approach to developing "quantum gravity" would predict the same large mass-uncertainties we do here. But the two most prominent (or hyped), approaches to quantum gravity are **"superstring theory"** and **"loop quantum gravity" (LQG)**. What are their predictions for our 2-hole scenario? And what do they say about the energy-time uncertainty principle? Wikipedia provides a helpful article "generalized uncertainty principle (GUP)" which claims that "advanced theories of quantum gravity, including string theory and loop quantum gravity [predict that there is a] minimal measurable length... and that $\Delta p \Delta x \geq \hbar/2 + (\Delta p)^2 \beta$ where Δx and Δp denote the uncertainties in position and momentum, respectively... while β is a [positive real] parameter that embodies the minimal length scale." Wikipedia then cites 20 specific papers to back that up. Two specific suggested values for β in Planck units have been $8\pi^2/2 \approx 8.773$ and $82\pi/5 \approx 51.52$.

First of all: I looked at all 20 of those papers, plus about 80 more found with arXiv search tools, and *none* of them derived any such thing from LQG. I also tried searching works of (one of the top LQG authors) Carlo Rovelli for "uncertainty principle" and failed to find it. Therefore wikipedia's claim that the GUP follows from LQG looks unjustified.

Second, Amelino-Camelia & Doplicher 2004 contend (further contradicting wikipedia's claim) that string theory does *not* prevent measuring (their word was "probing") lengths x or times t arbitrarily shorter than 1 Planck unit using "D-particles" (D0 branes) – but you cannot do both together, in the sense that they conjecture a "space time uncertainty principle" $\Delta t \Delta x \gtrapprox 1$. Essentially the same conjecture was made earlier for different reasons (which AC&D disparage) by Yoneya 2000.

Third, many of the papers deriving GUPs of this ilk employ a "gravitational self-interaction" argument, which, they hope, ought to be valid for a wide class of (i.e. every reasonable?) quantum gravity theory. E.g: masses with too-small positional uncertainty, would under classical GR be black holes. They then argue that this prevents Δp from exceeding order 1 in Planck units, and also prevents Δx from being smaller than order 1 in Planck units. However, that kind of argument *cannot* produce any analogous generalized *energy-time* uncertainty principle that prevents ΔE from exceeding order 1, or which prevents Δt from being smaller than order 1, in Planck units. The reason is that the gravitational self-interaction energy is *arbitrarily small* for a spatially "very spread out" mass-uncertainty ΔM, and hence will be negligible even if, say, ΔM were 10000 Planck masses. (On the other hand, if we restrict attention to a spatial region Ω of with surface area A Planck lengths^2, then ΔM for a system *contained within* Ω could be argued to be at most of order $\sqrt{A}$ Planck masses.)

And indeed, as far as I could tell, none of the 100 papers I examined derive any generalized *energy-time* uncertainty principle. The energy-time principles *differ* from all the other quantum uncertainty principles because time is not a quantum observable (unlike momentum p and position x, which both are). This difference kills most or all of those papers' arguments.

Arguably an exception is Salecker & Wigner 1958. They consider measuring a time-interval T to accuracy ΔT using a "quantum clock" of mass M, and find $M > \hbar c^{-2} T^{1/2} (\Delta T)^{-3/2}$, and under stronger assumptions $M > \hbar c^{-2} T^1 (\Delta T)^{-2}$. However their arguments never employ gravity or Newton's gravitational constant G, hence have nothing to do with "quantum gravity."

If one somehow could derive $\Delta E \Delta t \geq \hbar/2 + (\Delta E)^2 \beta$ from LQG or string theory, *then* that would prevent ΔE from exceeding order 1, and prevent Δt from being smaller than order 1, in Planck units, and thus also would predict the "boring" resolution. However, I have not seen any such uncertainty principle derived from either LQG or string theory in my literature-search attempts so far. If the purveyors of those theories really never managed to do that in 50 years of trying, then maybe that is because they are incompetent, or maybe it is because it cannot be derived. If the latter, then LQG and/or string theory might predict "**non-boring**" merging-black-hole quantum behavior.

At least some flavors of LQG predict (specifically, at least two LQG papers by Lee Smolin predicted) that the speed of light varies as a function of photon-frequency at high frequencies. That prediction contradicts all experimental evidence so far. In contrast, in both cloud QFT and string theory, the speed of light is invariant. Also, string theory inherently rests upon "supergravity" which in turn rests upon "supersymmetry" (SUSY), which contradicts all experimental evidence so far. In contrast, cloud QFT appears to be *logically incompatible* with SUSY (i.e. they cannot both be true), while LQG apparently doesn't care. Therefore, all three are **inequivalent**. Therefore, they might make different predictions about the present paper's black-hole-merger scenario. If they do, then we could have a way to, via observation, jettison at least two of these three approaches.

In cloud QFT, it is effectively impossible to accelerate anything (e.g. an electron) to energies and momenta much exceeding 1 Planck unit. Why can't you just build a very long linac to do that? (If similar to the famous SLAC linac, its length would need to be comparable to the size of a small galaxy; but that seems merely an engineering cost issue, as opposed to being in principle prevented by the laws of physics.) Cloud QFT's answer is that the forward-pushing electric fields, would, once the electron gets moving fast enough, stop being perceived by an observer sitting on the electron, as "forward-pushing electric fields." They instead would be perceived as something like "random noise." I can devise ridiculous schemes in cloud QFT to accelerate an electron to K times Planck energies for arbitrarily large K, but the ridiculousness of these schemes grows with K to the point where achieving, say, K=30 would seem to be beyond even a god who could commandeer the entire mass-energy of the observable universe. This gives another view of the claim that with cloud QFT there effectively is a "UV cutoff" preventing ultrahigh particle energies, which in turn invalidates the traditional "energy-time uncertainty principle" if applied to ΔE exceeding order 1 in Planck units.

According to David Tong's Lectures on String Theory (chapter 6 "scattering amplitude," section "high energy scattering," EQ 6.14) string interaction amplitudes are exponentially suppressed for trans-Planckian center-of-mass-frame momenta. Consequently a different flavor of my same kind of argument against super-Planckian linacs also can be made in string theory. This suggests (but with less confidence) that string theory also might invalidate the traditional energy-time uncertainty principle, similarly to the way cloud QFT does it. If so, it too will yield the "boring prediction." However, that presumably would be disputed by Amelino-Camelia & Doplicher.

I would like to see answers from the string theorists and LQGers (I am neither) about these questions.

Strong candidate for Ignobel prize: Key data analysis question largely ignored by LIGO team – and "Copenhagen controversy" cautionary tale about faked data

> Oh what a tangled web we weave / When first we practice to deceive – Walter Scott: Marmion [epic poem].
> Something is rotten in the state of Denmark. – William Shakespeare, Hamlet, Act I, scene iv.

Although the LIGO team's "tests of GR" papers performed many tests of GR (never finding anything wrong), they never tried *this* kind of test, nor considered any alternative-to-GR of our ilk. Up to Dec.2024, all my attempts to notify the LIGO team of this have been 100% ignored.

Let me re-iterate: A simple **KEY QUESTION** they could feasibly address is: **Does LIGO find more noise, or does the character of that noise change, when observing a merger, versus when not?** [And of course, "noise," here meaning the difference between the best-fit numerical-GR prediction minus LIGO observation, if it occurs *correlated* in multiple gravitational-wave detectors, then isn't really "noise"; it is additional "signal," telling us something important.]

If the answer to that question were "no," then that perhaps might be considered as adequate proof of, or perhaps merely evidence consistent with, my proposed "boring resolution." If (a) that "no" (optimistically) *is* considered "adequate proof" – or (b) if the answer were "yes" – then that would be the first quantum gravity experiment ever successfully conducted, and with the results being: (a) new physical law, contradicting a well-accepted theorem of quantum mechanics, or (b) first discovery of amazing quantum gravity effect, shedding even more, and the first, light on the nature of quantum gravity, and indeed refuting Einstein GR.

I searched for papers with keywords like "LIGO," "gravitational wave," "unexplained correlation" and the like, and... bingo. Tremendous excitement when I hit the innocently-titled paper (Creswell et al, Aug.2017) "On the time lags of the LIGO signals" by a team led by Andrew D. Jackson, emeritus at the Niels Bohr Institute in Copenhagen and something of a time-signal-analysis statistics expert. Note: the 5 Copenhagen authors and the ≈1000 LIGO-team authors, were *disjoint* sets. Their

> **Abstract [my italicization]:** To date, the LIGO collaboration has detected three gravitational wave (GW) events appearing in both its Hanford and Livingston detectors. In this article we reexamine the LIGO data with regard to correlations between the two detectors. With special focus on GW150914, *we report correlations in the detector noise which, at the time of the event, happen to be maximized for the same time lag as that found for the event itself*. Specifically, we analyze correlations in the calibration lines in the vicinity of 35 Hz as well as the residual noise in the data after subtraction of the best-fit theoretical templates. The residual noise for the other two events, GW151226 and GW170104, exhibits similar behavior. A clear distinction between signal and noise therefore remains to be established in order to determine the contribution of gravitational waves to the detected signals.
> [Note: if there are 2000 possible ways to time-align the two time series, and the *one* corresponding to the alleged grav-wave arrival-time difference is exactly the one maximizing inter-detector noise-correlation, then 99.95% confidence something is fishy.]

WOW! This was exactly the sort of proof (b) that I wanted. Instant Nobel prize. It was astonishing that for over a year, LIGO had ignored me entirely, while meanwhile these little-known non-LIGO authors from Copenhagen had already done the test, and observed my effect, years before I invented it!

But the Copenhagen team did not regard their observation as a great discovery of new physics. Rather, they regarded it as strong evidence that *something was severely wrong with the LIGO team's instrumentation and/or procedures* that might nullify their 14 Sep. 2015 (published 11 Feb. 2016) "discovery" of gravitational waves. E.g, the gravitational wave "signals" might not have been true signals at all, but merely louder noise. (This Creswell et al paper provided an excellent reason *not* to award the 2017 Nobel prize to LIGO, but the Royal Swedish Academy simply *ignored* it, announcing the prize to LIGO luminaries Barry Barish, Kip Thorne, and Rainer Weiss in a 3 Oct.2017 press release.)

Five days after Creswell came out, LIGO team member Ian Harry posted on Sean Carroll's internet blog a "refutation" of Creswell et al: he'd redone their analysis by writing his own computer program, fed the LIGO data into it, and presto: the "correlations" detected by Creswell et al were *gone*! But then Creswell et al examined Harry's computer program and found "several errors in his code." Upon correcting those errors, Creswell et al found that Harry's "script reproduces our results." That independent confirmation showed that this all was not merely the result of some mis-computation by Creswell et al. It was genuine.

The mysterious case of the missing(?) correlations. Then things got really weird. On 30 June 2017, an article came out by Mark Kim-Mulgrew in Quanta magazine about this controversy. It quoted Jackson as saying the implications of the Copenhagen correlation "could range from a minor modification of the extracted waveform to a total rejection of LIGO's claimed [gravitational wave] discovery." Meanwhile "LIGO representatives" desperately told *Quanta* that "there may well be some unexplained correlations, but they should not affect the team's conclusions." The latter was hogwash because it was a rock bottom design principle of LIGO, underlying everything they did, that noise at Hanford and Livingston (3000 km apart) could not have any correlations whatever. (An ultrashort-duration earthquake exactly equidistant – to within a few meters – to the two detectors, occurring at exactly the same time as the cosmic wave? No way.)

> **I proclaim:** any discovery of any statistically significant such correlations – no matter how small – automatically has *very* major implications, in particular refuting either Einstein GR or the entire LIGO experiment.
> Re "refuting Einstein GR": keep in mind that unlike, say, modeling the *Earth* as a sphere (an imprecise approximation; actually the Earth is complicated and messy), *black holes* are the most perfect macroscopic objects in the universe. Given enough compute-time (and LIGO devoted millions of dollars to such computations), the waveform output by two Einstein-GR black holes that best-fits data can be computed *extremely* precisely. Any deviation from that prediction – I do not care how small it is, provided it is statistically-significantly above the measurement-noise – constitutes a disproof of Einstein GR. Period, end of story, immediate Nobel prize.

But then Harry told *Quanta* that his code-bugs did not matter, because [he now contended] his corrected program still *refutes* Creswell et al! It then appeared that the only possible explanation was one of these two was lying. Fortunately, after 1-3 years three papers then came out attempting to resolve the controversy:

1. Green & Moffat 2018 (unaffiliated with the LIGO team) said "Statistical analysis of residual noise, after filtering out spectral peaks (and considering finite bandwidth), shows no evidence of non-Gaussian behaviour. There is also no evidence of anomalous causal correlation between noise signals at the Hanford and Livingston sites." Unfortunately the LIGO team, e.g. Abbott et al 2020, has long said their noise has a "rich structure," and is *not* Gaussian (although "away from transient disturbances the LIGO-Virgo data can be approximated as stationary and Gaussian") – facts Green & Moffat blithely ignored.
2. Nielsen, Nitz, Capano, Brown 2019 (all 4 of whom were LIGO team members and coauthors of the 2016 original discovery paper, despite at least two false reports by the press that they were "independent") said "We find that after subtracting the maximum likelihood waveform there are no statistically significant correlations between the residuals of the two detectors at the time of GW150914." And Marcoccia, Fredriksson, Nielsen, Nardini 2020 found:

> We adopt the Pearson cross-correlation measure to analyze the LIGO Hanford and LIGO Livingston detector data streams around the events GW150914, GW151012, GW151226 and GW170104. We find that the Pearson cross-correlation method is sensitive to these signals, with correlations peaking when the black hole binaries reconstructed by the LIGO Scientific and Virgo Collaborations, are merging. We compare the obtained cross-correlations with the statistical correlation fluctuations arising in simulated Gaussian noise data and in LIGO data at times when no event is claimed. Our results for the significance of the observed cross-correlations are broadly consistent with those announced by the LIGO Scientific and Virgo Collaborations based on matched-filter analysis. *In the same data, if we subtract the maximum likelihood waveforms [from numerical relativity data fits] corresponding to the announced signals, no residual cross-correlations persists at a statistically significant level.*

What do I think about this? First of all, I agree that LIGO genuinely detected genuine gravitational wave signals because later, at least 60 mergers got detected by *three* interferometric detectors simultaneously (i.e. the less-sensitive Italian "Virgo" detector near Pisa too). On the surface, the most impressive was a neutron star collision that also got detected <2 seconds later as a gamma ray burst by the Fermi satellite in the same sky-location, i.e. was seen by 4 detectors simultaneously, and then its aftermath was observed by numerous other optical/radio means. Therefore, the 2017 Nobel does not need to be revoked. Further, let me express my admiration of LIGO as a great technological accomplishment. [But unfortunately, delving deeper, this particular multi-messenger detection had low a priori confidence and happened simultaneously with what the LIGO team calls a "noise glitch," hurting its credibility. During 2019 LIGO suffered 10-100 such glitches daily. And the vast majority of neutron star collisions LIGO allegedly detected and sent email alerts asking the rest of the world to confirm optically or by radio or gamma or X-rays, never got any such confirmation. And as of year 2024, Japan's KAGRA detector is not yet good enough to have detected anything, while India's third LIGO detector is not yet operational.]

Second, Green & Moffat's key words "considering finite bandwidth" seem nullified because that been taken into consideration by Creswell et al by the use of "pre-whitening," and Liu, Creswell et al 2018 disputed Green & Moffat. The real crux problem seems to be that **the original Abbott et al. 2016 grav-wave discovery announcement paper "improved," aka "faked," their data!** Specifically, an un-named LIGO-team graphic artist "lined up by eye" various plots of their data in order to make Abbott et al 2016's figure 1 "look good." I.e. that figure was *not* the actual LIGO data, but rather altered fake data. That alteration might have seemed harmless at the time, but introduced statistically-significant artifacts which were duly spotted by Creswell et al's computation (and *were* genuine, despite Green & Moffat). Note, *neither* the original 2016 grav-wave discovery paper, nor the 2019 and 2020 defenses of that paper by LIGO team-member Nielsen (& his collaborators), nor a 1-paragraph response to Creswell et al LIGO posted on (but later removed from) their web site (quoted in full by Hossenfelder 2019) , even *mentioned* this graphic artist or admitted that data-alteration at all. The key sentence of that 1 paragraph read "The features presented in Creswell et al. arose from misunderstandings of public data products and the ways that the LIGO data need to be treated." What utter gall: Creswell "misunderstood" that the data they relied upon was *faked*!? And don't tell us that LIGO public data has been "mistreated" while giving no clue what "treatments" are allowed versus forbidden (apparently, "treatments" resulting in criticisms of the LIGO project are "mistreatments"). Here's an idea: provide genuine data, then everything anybody anywhere does with that data, will yield truth. There will be no "mistreatments." In short: LIGO covered it up, outrageously pretended Creswell et al were lying and/or incompetent, and their so-called "response" was unacceptable. And unfortunately for their reputations, investigative reporting uncovered this:

> *New Scientist* has learned that the collaboration decided to publish data plots that were not derived from actual analysis. The paper on the first detection in Physical Review Letters used a data plot that was more "illustrative" than precise, says [LIGO team member Neil] Cornish. Some of the results presented in that paper were not found using analysis algorithms, but were done "by eye." [Duncan A.] Brown, part of the LIGO collaboration at the time, explains this as an attempt to provide a visual aid. "It was hand-tuned for pedagogical purposes." He says he regrets that the figure wasn't labelled to point this out.
> This presentation of "hand-tuned" data in a peer-reviewed scientific report like this is certainly unusual. New Scientist asked the editor who handled the paper, Robert Garisto, whether he was aware that the published data plots weren't derived directly from LIGO's data, but were "pedagogical" and done "by eye," and whether the journal generally accepts illustrative figures. Garisto declined to comment.
> There were also questionable shortcuts in the data LIGO released for public use. The collaboration *approximated* the subtraction of the Livingston signal from the Hanford one, leaving correlations in the data – the very correlations Jackson noticed. There is now a note [added 18 July 2017 to] the GW150914 data release web page stating* that the publicly available waveform "was not precisely tuned."
> – Michael Brooks: Grave doubts over LIGO's discovery of gravitational waves, New Scientist, 31 Oct. 2018. Also, quoted in that article: Andrew D. Jackson: "We believe that LIGO has failed to make a convincing case for the detection of any gravitational wave event... We came to a conclusion that was very disturbing: They didn't separate signal from noise."
> (*) **Erasure from history:** Their words "not precisely tuned" would more accurately read "intentionally altered." When I re-examined that page in Dec.2024, over 9 years after the GW150914 event, it appeared LIGO *never* explicitly made genuine "precisely tuned" data available; only the faked "imprecise" data was ever given to the outside world; and certainly no explicit response to, nor even acknowledgment of the existence of, Creswell et al is anywhere on that page. The page contains no "best fit Einstein numerical relativity" waveform, at least none labeled with any of those five key words (or "residuals" or "differences"). Furthermore, I attended a public 90-minute lecture "LIGO results" by Barry Barish on 2 Dec.2024 at Stony Brook University in which he displayed exactly the figure 1 from the discovery paper, and remarked that this figure finally "convinced" him of the validity of their detection – thereby mightily impressing the ≈50 audience members. Barish's lecture never mentioned that that figure's data was "hand-tuned," i.e. altered, not genuine; nor that any controversy had ever been raised about it.

The Copenhagen team was quite angry when the investigative reporting revealed this to them over a year later. Jackson remarked "This LIGO episode continues to be the most shocking professional experience of my 55 years as a physicist." Its effect, indisputably, was literally to waste over a man-year of researcher time, which is one reason why the Copenhagen team was entirely right to denounce LIGO's behavior as "absolutely unacceptable" methodology for such a huge discovery. I'm afraid I must agree with their complaint, although I'm glad this history happened the way it did, since otherwise it might have been *my* years wasted and *me* publishing a "Nobel-prize-worthy followup discovery" of "new physics" in the LIGO signal, which then the LIGO team (I presume/hope) ultimately would have disputed, although perhaps in reality they'd first be trying to grab the credit, etc. (As it was, I only wasted 2 days of my life uncovering this story.) If that had happened, I would have been absolutely raging in fury at those shitheads.

Lemonade from lemons? The bright side of this embarrassing fiasco: it caused at least some LIGO team members to actually examine the issues I raised in my paper, despite their continuing campaign of utterly ignoring me, which by the way is a carbon copy of their behavior of utterly ignoring the original enquiries from the Copenhagen team. I wish that the LIGO team had automatically incorporated the Copenhagen kind of statistical test into *every* observation they ever made – preferably *cumulatively* – not just the particular few questioned by Creswell et al. But as far as I can tell, they unfortunately never did (perhaps because they feared undermining themselves?). They did, however, at least learn their lesson to this extent: they started posting at least some of their (hopefully unaltered this time?) data on the GWOSC.org web site (grav.wave open science center, created 30 sept.2021), while Abbott et al 2020 wrote *A guide to LIGO-Virgo detector noise*, hopefully enabling third parties to try to examine it. But I cannot do that, because I *also* would need the best-fit numerical-GR waveforms as well as the raw data, or would need the software and computational resources to create them myself – which the LIGO collaboration possesses, but 99.9% of scientists (including me) don't. As Jackson told Brooks 2018: "It's problematic: there's not enough data to do the analysis independently. It looks like they're being open, without being open at all." Brooks continued: "[LIGO team member Duncan A.] Brown agrees there is a problem: 'LIGO has taken great strides... towards open data and reproducible science. But I don't think they're quite there yet'."

Meanwhile the Copenhagen team (Jackson, Liu, Naselsky 2019) re-examined and criticized Nielsen et al 2019 and re-did their old computations better using better input data, and thus seem to have successfully muddied the waters.

The net effect of this brouhaha, for what it is worth, if we believe Nielsen, Nitz et al, and Marcoccia et al, is evidence consistent with my guessed "boring resolution"; whereas if we believe in the water-muddying by Jackson, Liu, Naselsky 2019, then there might be something non-boring there. It would be best to apply this kind of re-examination to *all* the LIGO data for *all* merger-events (about 100 up to the end of 2024, and some of the later ones tend to have the best signal-to-noise ratios) but as far as I can tell, nobody tried. And the reason they did not try was that they were neither seeking new physics, nor trying to address my quantum issues. [I believe similar statistical examination of 10000 wave-detection events, if and when done, would cast considerable light.] Instead, all they were trying to do (far less ambitiously) was to make the Copenhagen controversy – and the consequent horrifying possibility that everything LIGO ever did, was garbage – go away. Once it became clear that LIGO's grav-wave detections were genuine, those motivations ended.

I emailed everybody involved asking if they had any objections to, and/or other comments about, my recounting of this story. After 1 week: none came.

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 a-f 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.

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

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

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**"

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

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

(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 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.

⑦ But energy-time uncertainty principles should still be valid when $\Delta E \lessapprox 0.1M_{pl}c^2$. Thus, my suggested resolution violates ordinary quantum mechanics, and also violates ordinary general relativity, and hence would require genuinely-new "quantum gravity."

⑧ Throughout this argument I have implicitly used $E=mc^2=hf$ to interconvert between energies E, masses m, and frequencies f.

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