Does "Zero-point vacuum energy" really exist for boson fields? And fermion fields?

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Abstract. Although this was written as a chapter for my book-in-progress Completing Quantum Electrodynamics and other Quantum Field Theories with "cloud" (and perhaps even creating "quantum gravity" & "theory of everything"), it can stand on its own as a helpful compilation of evidence that "zero-point vacuum energy" exists.

> It is probably simpler and more satisfying to assume that for electromagnetic fields this zero-point radiation does not exist at all.
> – Pascual Jordan & Wolfgang Pauli, 1928 paper.
>
> It is clear that this "zero-point energy" has no physical reality.
> – Wolfgang Pauli, 1945 Nobel lecture.
>
> According to quantum mechanics, a harmonic oscillator of frequency v has a lowest energy state the energy of which is hv/2. When the electromagnetic field is treated... as an assemblage of independent harmonic oscillators, one of which is associated with each of the normal modes of vibration of the ether, this leads to the result that there is present in all space an infinite positive energy density. It is infinite because there is supposed to be no upper limit to the frequencies of possible normal modes.
> – Edward U. Condon & Julian E. Mack, 1930 paper.

Various people besides Jordan and Pauli, for example much more recently Robert D. Klauber in his *Student friendly quantum field theory* books, have expressed skepticism or even outright denial that QED's claimed fermionic and/or bosonic vacuum "zero point energies" really exist. This chapter will explain good **reasons to believe both really exist**. Although this is a matter of fundamental importance, I had not previously seen any decent collection of evidence and reasons in one place.

Bosons

It already dawned on Nernst 1916, and also Planck 1911-1913 ("Planck's second and third quantum theories"), that the expected energy $[\exp(\hbar\omega/(k_B T))-1]^{-1}\omega\hbar$ above ground in each angular-frequency-ω (single polarization) mode of Planck's temperature T blackbody radiation, in the $T\to\infty$ limit does *not* approach Boltzmann's classical "**equipartition** law" value $k_B T$ arbitrarily closely. (The latter is the expected kinetic *plus* potential energy of a classical 1D harmonic oscillator at temperature T.) Presumably you want it to. If so, then you need to add $\omega\hbar/2$ energy to each such mode. I.e. the ground state energy of the mode must be $\omega\hbar/2$, *not* zero. This is because of the Laurent series $[\exp(w/T)-1]^{-1}w=T-w/2+w^2T^{-1}/12-w^4T^{-3}/720+\ldots$ in ascending powers of T^{-1}.

The facts that the natural ground state energy of any such mode indeed equals $\omega\hbar/2$, and the full set of energies is $\omega\hbar(n+1/2)$ for integers $n\geq 0$, are a consequence of the (later-developed) Schrödinger equation applied to the "simple harmonic oscillator." For **molecular-mechanical harmonic oscillators**, this zero-point energy was first **experimentally established** by Mulliken's studies (especially 1930) of the isotope effect on spectra of boron monoxide BO; and also via its direct observation via Xray diffraction in crystals, i.e. the fact that the "Debye-Waller factor" governing diminution of Xray diffraction peaks (also observable with neutron diffraction) does not vanish in the limit of zero temperature, e.g. see James, Waller, Hartree 1928 for NaCl. (See Sears & Shelley 1991 re Debye-Waller. More early zero-point evidence discussed by Mehra & Rittenberg 1999, especially their §2.)

It also is interesting that assigning an energy Kf to each vacuum mode of frequency f (here K is arbitrary constant, physically K=h/2) is the *only* energy-assignment permitted (considering Doppler and mode-density-changing effects) once we demand relativistic **Lorentz-invariance**. For example nonzero energy proportional to f^p would be forbidden for any fixed p≠1. The only freedom relativity grants you is to change the value of K (albeit if we were only considering the subgroup of *time-direction-preserving* Lorentz transformations, then we could permit *two* constants K_+ and K_- to be used depending on the sign of f, for mode-types for which "negative frequencies" is a sensible distinct concept). The temperature-T Planck blackbody spectrum (either with or without the extra $\hbar\omega/2$ "zero-point energy" term) is *not* Lorentz invariant, *except* when T=0 (Nernst 1916, Ford & O'Connell 2013).

> Spontaneous emission is thus a stimulated emission of one quantum of light caused by the zero-point fluctuations of vacuum.
> – Victor F. Weisskopf 1935.

The "Einstein A and B coefficients" (Einstein 1917) govern the decay, un-decay i.e. photo-excitation, and stimulated decay of excited atomic states to yield emitted photons. The A term is proportional to the energy density of the zero-point modes of the photon field. This allows interpreting the "**spontaneous decay**" of excited atoms as really "stimulated emission" which is stimulated by the zero-point field not actually being zero; and if so the A term can be absorbed into the B (stimulated) term. This "ZP stimulation" interpretation apparently first was pointed out by Weisskopf & Wigner 1930, then reviewed by Fermi 1932 and Weisskopf 1935. Welton 1948 also wrote that spontaneous emission "can be thought of as forced emission taking place under the action of the fluctuating [zero-point] field." If the zero-point field did not exist, i.e. really was zero, then isolated excited atoms would not decay. Einstein's model achieved great success, e.g. underlies the operation of (and successfully predicted the existence of) lasers. Einstein derived this whole model semi-empirically in 1916 well before the Schrödinger equation was officially invented around 1925. However, Einstein's model later was re-derived by others (especially Dirac 1927 and reviewed by Fermi 1932) based on the time-dependent Schrödinger equation. They then were able to predict exact numerical values for the A and B coefficients for, e.g, excited states of hydrogen. The mean **lifetimes** of those hydrogen states agree excellently with experiment. For example, the predicted mean lifetime for hydrogen's 2P→1S transition (Lyman-α 121.56nm line) is $(3/2)^8\alpha^{-5}\hbar m_e^{-1}c^{-2}\approx1.5953$ns using Schrödinger equation; Boudet 1993 relativistically corrected that prediction to **1.5960**ns; and Bickel & Goodman 1966 measured **1.600±0.004**ns. For the 3P→1S transition (Lyman-β 102.57nm line) the Schrödinger mean life prediction is $(3/2)^{11}\alpha^{-5}\hbar m_e^{-1}c^{-2}\approx$**5.3842**ns; and published measurements include 5.4, 5.41±0.18, 5.5±0.2, and 5.58±0.13 ns. For more data see Chupp, Dotchin, Pegg 1968; Hughes, Dawson, Doughty 1966/7; and Etherton et al 1970. The 2S→1S transition cannot happen via a single-photon emission (because photon spin=±1≠0) and therefore takes place via a 2-photon emission and has a much longer lifetime ("metastable"). Its theoretical mean lifetime (more difficult theory) is **0.122** sec while one measurement (fig.5 in Cesar et al 1996) found **0.110** sec. Also predicted: the line**shape** and line**widths**. Some of that is redone in books like Loudon 2000 and Bethe & Salpeter 1957.

The $|\Psi|^2$ of excited states of isolated atoms are exactly time-invariant for solutions Ψ of the Schrödinger (or Dirac) equation. For the hydrogen atom these Ψ can be written in closed form. Because of that time-invariance, those states would persist forever, never decaying, if the Schrödinger equation in zero background field were all that governed the situation. But in reality decays for most atomic excited states are rapid, with mean lifetimes of order 1 nanosec. This stark contrast constitutes **evidence** suggesting that the usual picture of zero-point vacuum modes for photons, involving time-*varying* electric fields, is valid.

Some later authors did not like all that and tried to invent their own versions of QED in which zero-point fields were abolished. I will focus on the **two most important attempts**:

1. The "neoclassical theory of radiation" (Crisp & Jaynes 1969, Stroud & Jaynes 1970, Jaynes 1973) by Edwin T. Jaynes (1922-1998). In this theory there is no zero-point electromagnetic field, and Jaynes attempted to quantize the classical notion of the "radiation reaction field of an oscillating dipole" to explain why atomic transitions spontaneously happen.
2. "Source theory" by Julian S. Schwinger (1918-1994). Source theory is summarized in the 3-volume Schwinger 1998; for a scientific biography of Schwinger see Mehra & Milton 2000.

Jaynes had numerous reasons to believe his "**neoclassical theory**" agreed far better with "common sense" than QED. Maybe so – but the trouble is: it's just **wrong**. Jaynes' theory and QED make quantitatively different predictions, enabling experimentally deciding between the two. Those experiments were done, and the results conclusively refuted Jaynes (and the wide class of "semiclassical theories of radiation" generally) but remained compatible with QED. See Kocher & Commins 1967, Clauser 1972, Mandel 1976, and Norden 2018. Jaynes became aware of some of these experiments when writing his 1973 "survey" hence included this rather sad final line: "[if Clauser is correct, and I cannot see an error, then] my own work will lie in ruins."

Incidentally, Clauser eventually won a share of the 2022 Nobel prize for the work he did in the 1970s on the foundations of quantum mechanics. This also included the experimental **overthrow of "local realism,"** which were a class of ideas dating to Einstein, Podolsky, Rosen 1935, that again seem a priori to agree far better than quantum mechanics with "common sense" – but are wrong.

Once you believe that Maxwell's "electromagnetic field" exists, then it is very hard to get rid of its "zero-point fluctuations" because their existence and size both seem logically forced by standard "**uncertainty principles**" in quantum mechanics. So probably the only way we can hope to abolish zero-point vacuum energy is to abolish fields. (And presto – that also would abolish the infinite classical electron "self energy.") That was the idea of **Schwinger source theory**, which I'll try to explain despite the severe handicap that I do not understand it. Schwinger, following up on ideas Feynman abandoned, starting about 1965, thought/hoped he could reformulate QED without any renormalization or "high-energy speculations" by *getting rid* of all "fields" by only discussing their "sources." E.g. the "source" for a photon is the 4-vectorial current distribution. (Note, incidentally, Schwinger's desire to get rid of, and renunciation of, renormalization *despite* being heavily honored for previously being one of the main inventors of renormalized QED.) E.g. with Source Theory there is no such thing as an "electric field" in the absence of matter. Schwinger was able to redo many calculations in this way, sometimes arguably more nicely than the old way – although to me the whole thing seems ugly. In particular Schwinger in his vol.3 rederived the electron magnetic moment up to and including terms of order α^2. And he re-derived the Casimir force (for parallel plates in 1975 and the hollow sphere in 1978) despite "regarding the vacuum as truly a state with all physical properties equal to zero." But I did not understand why, in Schwinger 1975, the current and charge on each plate could not just be taken as zero, causing zero Casimir force. Although Schwinger claimed Source Theory avoided infinities, as far as I know it still yields infinite Casimir forces in generic geometries (ala Deutsch & Candelas 1979), e.g. a conductive hollow sphere with *non*zero wall-thickness – an inconvenience Schwinger blithely ignored. But later, Schwinger published the idea such spherical Casimir infinities (or large finite energies, if he assumed plausible values of UV cutoffs) actually were *good* since they could explain the remarkable phenomenon of "**sonoluminescence**" in collapsing bubbles in liquids! That idea might have seemed brilliant to him at the time, but is utter bunk. I also do not know whether Schwinger ever was able to recapitulate Dirac radiation theory from source theory – but am I guessing "not" since I failed to find that in a brief search (at least, not explicitly?).

In any case, it seems to me that **QCD** presents a major, likely insuperable, problem for anybody trying to sourcify the "Standard Model." Consider **gluons**. If we regard the gluon field as not really existing, only its sources (quarks) exist, then the trouble is that QCD's "nonexistent" gluons carry color, hence themselves can act as sources for more gluons, which is crucial to have any hope of explaining the short-ranged nature of the strong force via "**color confinement**." Oops. [There will be further killing if and when a consensus arises that experimentalists have clearly detected "glueballs." Brünner & Rebhan 2015 argued that the "f_0(1710)" particle probably is a glueball, but Janowski et al 2014 argued against them. Abazov et al 2021 and Csörgö & Szanyi 2021 claimed "odderon" glueballs were finally clearly detected in 2021. Ablikim et al 2024 is the BES III collaboration announcing that the exotic "X(2370)" particle, detected with huge statistical significance>11.7σ may be the lightest glueball predicted by the Standard Model. It has mass≈2395 MeV/c^2 with linewidth≈188, and spin-parity 0^{-+}.] As far as I know neither Schwinger, nor anybody else, was ever able to overcome this problem. Schwinger did think he was able to handle electromagnetism, even with the addition of hypothetical "magnetic monopoles" (and "dyons": hypothetical particles with both magnetic and electric charge), and perhaps even electroweak unification(?) – and even claimed Source Theory made important statements about gravitons (albeit gravitons, like gluons, can serve as sources for more gravitons, crucial for allowing "black holes"). But QCD defeated him. Apparently Schwinger's response to that was to oppose QCD and hope it somehow all was wrong. But the numerical successes of lattice QCD in predicting many experimentally measured quantities to around 1% accuracy eventually made that stance untenable, even if it perhaps still was tenable while Schwinger was alive. (It also did not help that during the later part of his life, Schwinger used his status as a Nobelist to publicly attack the scientific community for its rejection of the "cold fusion" fraud.) So the only way known to Schwinger to try to claim that electromagnetic zero-point energy does not exist, fails to work for gluons – which then in some unknown way would need to be handled differently – which would force unpleasant disunification in Source Theory, compared versus the standard model.

Today (year 2023) Schwinger's source theory is almost forgotten and ignored. Virtually nobody has read and comprehended his books (proven by their lack of Amazon reviews). It is difficult to read them because he redoes everything in his own notation that nobody understands (anyway, not me). Schwinger resented that – perhaps with good reason. I do not know which parts of QED can be re-established sourcically, and which (if any) cannot, i.e. I do not know whether source theory and QED (and/or the Standard Model) are compatible. If incompatible, then somebody should devise an experiment to distinguish between them! But Schwinger and his followers never did. **Conjecture:** they indeed are incompatible and inequivalent, and essentially everything the "strong force" does is a suitable experiment. On the other hand, if they ultimately are equivalent theories, then it is mysterious how the zero-point vacuum energies both do, and do not, exist, depending on your point of view – and what we should conclude from that.

The Standard Model's **Higgs field** is another kind of "zero-point energy" whose existence and properties are nowadays experimentally well-confirmed (and which I do not see how Schwingerian Source Theory could handle). Actually the Higgs field is a nonzero *constant* in the vacuum ground state (up to fluctuations) due to "symmetry breaking" and a self-interaction term in its Lagrangian, but anyhow the most important point for our present purposes is: it is *not* zero in the standard model vacuum.

> The best known example of [a] consequence of zero-point field energy is the Casimir [attractive] force between uncharged, perfectly conducting [parallel] plates.
> – Peter W. Milonni 2009. Casimir's 1948 discovery of the very real (and later well-measured) physical Casimir effect rather refuted Pauli's quote. (Incidentally, after I showed a draft of this chapter to PWM, he replied that he was working on his own invited manuscript, titled *Zero-Point Energy is Real*, then showed it to me, although the draft he showed me was only about 20% complete.)

The **Casimir effect** (Casimir 1948) is another "experimental proof" of the existence of zero-point energy of the electromagnetic vacuum. Two perfectly conducting (i.e. the tangent electric field is zero) parallel **plates** – say disks with area A each – are separated by distance S small compared to $\sqrt{A}$. Casimir predicted they *attract* with force $(\pi^2/240)\hbar cA/S^4$, i.e. energy $E=(-\pi^2/720)\hbar cA/S^3$. Lamoreaux 1997 introduced a slightly different scenario friendlier to experiment: a conducting plate and ball, with minimum separation S small compared to the radius R of the ball. ("Friendlier" because angular orientation now is irrelevant.) More generally we can consider two balls of radii R_1 and R_2 both large compared to S. These are predicted (EQ91 of Schoger, Spreng, et al 2022) to attract with energy $E=(-\pi^3/720)\hbar cR/S^2[1-15\pi^{-2}(S/R)\pm O(S/R)^{3/2}]$ where $R=R_1R_2/(R_1+R_2)$. The existence, magnitude, sign, and separation-dependence of these forces nowadays is experimentally well confirmed (e.g. Lamoreaux 1997, Krause et al 2007); and indeed they are important in micro-mechanism technology.

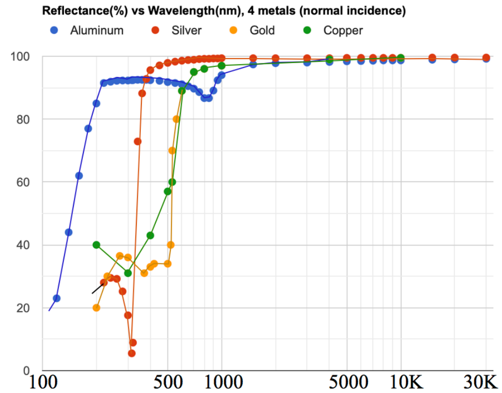
The Casimir energy of a conducting hollow **sphere** of radius=R and infinitesimal wall thickness is $E\approx0.04618\hbar c/R$, which note is positive, i.e. in this case the Casimir force acts *repulsively* to inflate the sphere (Balian & Duplantier 1978; and this repulsion increases when temperature is increased from 0). For a rectangular a×a×b box with infinitesimal wall thickness ther Casimir energy is repulsive-signed if $0.408<b/a<3.48$, otherwise attractive-signed. That has not been experimentally confirmed, but there are other geometric scenarios involving dielectrics in which Casimir repulsion was both predicted and experimentally confirmed. Some critical reviews of Casimir force experiments are Klimchitskaya, Mohideen, Mostepanenko 2009, Lambrecht & Reynaud 2012, and Dhital & Mohideen 2024.

Recreational aside about Casimir's crazy classical model of electrons: Casimir 1953 published the crazy idea that the electron was a charged hollow sphere, which due to Coulombic electrostatic repulsion would "try" to expand; but hoped that Casimir force would cause it to "try to contract," thus obtaining a classical stable model of the electron with finite and calculable self-energy. But that idea was destroyed when the Casimir force was discovered to have repulsive sign for the hollow-sphere geometry; this electron would energetically-prefer to expand to infinite radius. Furthermore, the fact that *both* the classical Coulomb electrostatic energy $Q^2/(8\pi\epsilon_0 R)$ of a sphere-surface charge distribution (total charge=Q), and the Casimir energy $0.04618\hbar c/R$, of such a sphere, have the *same* form const/R, means that even if they did have opposite signs, then the electron would prefer to

expand to $R\to\infty$ or shrink to $R\to 0+$; or the two energies would exactly cancel to 0 in which case there would *not be* any preferred energy-minimizing size R since all $R>0$ would yield the same energy. Which leads me to suggest...

New, less-crazy analogous classical electron model: The Kerr-Newman exact solution of the combined classical Einstein & Maxwell (gravity & electromagnetism) equations would for an electron (or muon, tauon, or any other known nonzero-spin fundamental particle with nonzero charge or mass) *not* be a "black hole" but rather a "naked singularity" due to its high spin and charge compared to its small mass. This singularity is not a point. It is a circular ring. This suggests that a better classical model of an electron than a hollow sphere would be a hollow **torus**. Straley & Kolomeisky 2014 computed the Casimir energy E_{Cas} of a torus with major radius R and minor radius r ($0<r<R$, surface area$=4\pi^2 rR$, infinitesimal wall thickness) for 7 values of R/r with $2\le R/r\le 10$; and I find that their numerical results agree to all decimal places S&K gave, with the formula $E_{Cas}=c\hbar(B r-AR)/r^2$, where $A\approx 0.056168$ and $B\approx 0.0049102$. I do not know whether any formula of this kind is exactly valid, or whether that excellent numerical agreement was merely a remarkable coincidence. Note that this E_{Cas} has the desired attractive sign for all $R\ge r$. Therefore the *toroidal*-Casimir idea is *not* ruled out by any simple sign consideration. The capacitance C of this torus (Snow 1954, Queiroz 2000-2018) is $C=16\epsilon_0(R^2-r^2)^{1/2}F(R/r)$ where $F(x)=\sum'_{n\ge 0} Q_{n-1/2}(x)/P_{n-1/2}(x)$ where P_v and Q_v denote (the real parts of) Legendre functions, and the prime on the summation means the summand with n=0 is halved. The total (Casimir+Coulombic) classical energy then equals $E_{tot}=E_{Cas}+E_{Coul}$ where $E_{Coul}=e^2/(8\pi\epsilon_0 C)$. I wrote a computer program to evaluate E_{Cas} and E_{Coul} as functions of R and r. The result (under the conjecture that the S&K Casimir energy formula holds) was that $|E_{Cas}|\ge 9|E_{Coul}|$ for all $0<r<R$. Because the Casimir contractive force is stronger than the Coulombic expansive force by a factor≥9, any such classical toroid electron would, to minimize its energy, contract to a point ($r,R\to 0+$).

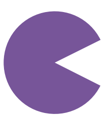
However: that calculation ignored the *magnetic* energy (if the toroid-electron is assumed to spin about its axis of symmetry, it would generate a magnetic dipole field and with the combined $\vec{E}$ and $\vec{B}$ fields possessing angular momentum) and also ignored Uehling's logarithmic correction to the Coulombic energy formula, which causes electrostatic energy to behave, not proportionally to R^{-1}, but rather to $R^{-1}|\log R|$, when $R\to 0+$. With Uehling, the contraction would stop and there would exist absurdly tiny – but positive! – values of r and R minimizing the energy $E_{Cas}+E_{Uehl}$. The best torus shape probably would arise from rotating a somewhat *non*circular 2D shape about a line. I do not know what this best toroid shape would be, nor whether one exists causing the total surface pressure to equal 0 everywhere, nor whether it would be "stable" against, e.g, flattening the torus.



To **derive** his force, Casimir considered a three-plate geometry, with the left two separated by S, and the right two by L-S. Casimir computed the summed energies of the EM-vacuum zero-point modes within the two interplate regions, then *subtracted* the same energies if the middle plate were not present, weighting modes of frequency F by exp(-kF) to make these sums converge. The result *converges* to an answer that is *finite* in the $L\to\infty$ and then $k\to 0+$ limits with S held fixed (perhaps most slickly computable with the aid of the Abel-Plana summation formula), which yields Casimir's energy- and force-predictions. The sphere-plate and hollow-sphere calculations are more difficult and were done by other authors well after Casimir. Many other regularizing functions besides exp(-kF), for example $(1+kF^2)^{-B}$ for any B>1/2, also work to yield the exact same parallel-plate attraction in the k→0+ limit. Indeed, Casimir 1948 essentially proved (after his mistakes are corrected...) using Euler-Maclaurin summation theory (theorem 4 of Apostol 1999) that *any* regularizing function G(kF) such that G(X) is

i. analytic as a function of X for all real X≥0;
ii. bounded between 0 and 1 for real X≥0;
iii. $G(0)=1$, $\lim_{X\to\infty} X^2G(X)=0$, $\lim_{X\to\infty} XG'(X)=0$, $\lim_{X\to\infty} X^2G''(X)=0$, $\lim_{X\to\infty} X^2G^{(3)}(X)=0$, $\lim_{X\to\infty} X^2G^{(4)}(X)=0$

will work (all yield identical limit "Casimir force" when k→0+). Call that **cutoff-insensitivity**. Using such a regularizer is physically justified because any real mirror presumably would lose its reflectivity for sufficiently-high frequency light. The plot displays the reflectivities of aluminum, silver, gold, and copper as a function of wavelength λ from 100 to 30000nm. All four metals have reflectivity≥94% when λ≥1000nm, ≥98% when λ≥4000nm, and ≥99% when λ≥20000nm. But they are much less reflective for short wavelengths; the changeover occurs roughly at that metal's "plasma frequency." E.g. all four of our plotted reflectivities drop below 31% for at least some λ with 100<λ<350nm. For 1nm Xrays, I am unaware of any material with reflectivity≥1%. Xray reflectivities empirically appear to fall proportionally to exp(-kF) where $k\geq 6\times 10^{-19}$ sec, in materials I am aware of when $F>10^{19}$ Hz.



Incidentally, the "parallel planes" and "zero-thickness hollow sphere" geometries are mathematically rather special in the sense that they yield **finite** electromagnetic Casimir forces and energies. Deutsch & Candelas 1979 showed that for generic shapes, the analogous calculation predicts *infinite* (and, in general, surface-position-dependent) forces! Lukosz 1973 found that parallelipipeds and other *polyhedra* have infinite Casimir forces and energies. The "infinite energy" claim for a hollow cube with perfectly conducting walls of infinitesimal thickness was numerically confirmed by Straley & Kolomeisky 2014 who computed the Casimir energies of surfaces $|x|^p+|y|^p+|z|^p=1$ (this is a regular octahedron if p=1, a sphere if p=2, and tends to a cube when p→∞) and found it to be positive (repulsive) for all p≥2 and when p→∞ found it went infinite apparently proportional to p^2. But according to Lukosz, and also Balian & Duplantier 1978, total Casimir energy is finite for *everywhere-smooth* mirror shapes with *bounded* surface |curvatures|. Further, Balian & Duplantier around their EQ 7.2 claim that the Casimir energy of perfectly-conductive infinitesimally-thin polyhedral shells is always *positive*-signed infinity, which means that polyhedron-edge dihedral angles should tend to get "rounded" (thus finitizing their curvatures) by the action of Casimir forces, – presumably making it in practice impossible to manufacture an atomically-sharp concave edge (as pictured above left) with metal. Theory and experiment always seem to agree at least roughly (so far, anyhow) in geometries where theory predicts finite Casimir forces and "cutoff insensitivity." Otherwise they disagree, which prior quantum field theorists and popularizers have usually handled by simply falsely declaring over and over that "no discrepancy between the standard model and experiment has ever been found." Since I want to be better than them, let me make a few remarks. A fundamental source of the generic infinities is V.Ivrii's improvement (to include boundary terms) of H.Weyl's asymptotic count (when ζ→∞) of the number of eigenvalues below ζ of the laplacian in a region Ω. Specifically in 3 space dimensions this count N is

$$N = (6\pi^2)^{-1}\mathrm{vol}(\Omega)\zeta^{3/2} \pm (8\pi)^{-1}\mathrm{SurfaceArea}(\Omega)\zeta \pm o(\zeta).$$

where the surface term has + sign for Neumann and - sign for Dirichlet boundary conditions. The second (surface) term causes the zero-point energy of Ω's vacuum modes below a given UV cutoff, to be *less* with Dirichlet (but more with Neumann) than the energy the same volume of vacuum would have had, in the absence of Ω's reflective walls; and in the limit $\zeta\to\infty$, less by an *unboundedly* large total amount of "Casimir" energy. However, in the case of *infinitesimal* wall thickness for the mirrored boundaries of Ω, the energy increments for the inner and outer regions (Ω and its complement set) are equal, causing the corresponding outward and inward Casimir *forces* on ∂Ω to cancel to zero. But for *non*zero wall thickness (assuming the wall is made of constant-density material) that force cancelation will not happen, whereupon infinite inflatory force would naively be predicted for any convex Ω. **But**, for *electromagnetic* modes in a simply-connected *vacuum* region inside a perfect-conductor container, the electric↔magnetic interchange symmetry of the (3+1)-dimensional Maxwell equations causes the corresponding Casimir *energy* term also to cancel out to **zero**! This cancellation arises because the Maxwell cavity modes come in pairs of "sisters," often called "TE-" and "TM-modes" (although §II of Balian & Bloch 1971 calls them "transverse" and "longitudinal"), one arising from Dirichlet and the other from Neumann boundary conditions. This cancellation is special to (3+1)-dimensional electromagnetism and in general will not happen for gravitons, scalar fields, etc.

At least for well-enough-behaved Ω (e.g. having smooth boundary with bounded maximum |curvature|) with smooth-enough regularization functions, we expect **further terms** beyond Ivrii. The next term should involve the integrated signed mean-curvature of ∂Ω, and should generically yield **unboundedly great** (position-dependent) Casimir surface pressure forces. However the flat planes special case escapes that fate since its mean curvature is everywhere zero. The infinitesimally-thin-walled hollow-sphere special case also avoids it due to its perfect symmetry eliminating any position-dependence, and the fact the *total* Casimir energy is finite, so the position-independent Casimir pressure must be finite. The *next* term ought to involve the integrated value of $(k_1-k_2)^2$ where k_1 and k_2 are the two principal curvatures of the boundary ∂Ω. That term is zero in the special cases of spheres and planes. Physically, what saves us from these generic infinities is the imperfect reflectivity of real mirrors at high frequencies. This effectively causes "UV cutoffs" (in the case of silver, there is a quite dramatic cutoff which quite literally *is* ultraviolet, at wavelength≈350nm) which will cause the Casimir forces and energies in any real experiment to be finite. However, whenever we naively predict infinity, those finite forces can be *large*, and usually will be *sensitive* to the details of the UV cutoff, i.e. to the particular mirror material. In contrast, the thin-walled hollow sphere and parallel planes special cases, enjoying cutoff insensitivity, do not care much which metal you use. (The plane+ball case approximates the parallel planes case.)

At **nonzero temperature T**, there will be forces on mirrors caused by the temperature-T blackbody radiation present on both sides of the mirror, and the Planck-Bose-Einstein distribution governing such radiation for the mode-spectrums available in both cavities can, in principle, be computed exactly – and is not exactly the same as Planck's distribution in unbounded 3-space. What we have been calling the "Casimir force" is the $T\to0+$ limit of that. [Mehra 1967 computes the parallel planes Casimir force at nonzero temperature T in his EQ 14, where T' is defined in EQ 15 and he gives low-T and high-T asymptotics in EQ 21 and 23. Fierz 1960 also computes this force, along with his own slick redo of Casimir 1948; and Brown & Maclay 1969 does it again by still another method. If we define T_c by $2k_BT_c=\hbar c/S$, then the Casimir force F(T) is $F(T)\approx F(0)+(T/T_c)^4F(0)/3$ when $0\le T\ll T_c$, but $F(T)\approx(4\pi)^{-1}\zeta(3)AS^{-3}k_BT$ when $T\gg T_c$. Suchkov et al 2011 experimentally confirmed this "thermal Casimir force."]

The fact that this force is nonzero even in the $T\to0+$ limit is **evidence** for photon-vacuum "zero-point energy."

> The reason the global Casimir energy of a (hyper)sphere is finite is that there is a perfect cancellation between the interior and exterior divergences. This perfect cancellation is spoiled if the spherical shell has nonzero thickness, or if the speed of light is different on the two sides of the boundary [e.g. an idealized dielectric ball]. Fluctuating fields of nonzero mass also yield unremovable divergences except for the case of plane boundaries.
> – Kimball A. Milton [Phys.Rev. D68 (2003) #065020].

Nikolic 2016 & 2017 objected to all that. He claimed to "present a simple general proof that Casimir force cannot originate from the vacuum energy of electromagnetic (EM) field. The full QED Hamiltonian consists of 3 terms: the pure electromagnetic term H_{em}, the pure matter term H_{matt}, and the interaction term H_{int}. The H_{em}-term commutes with all matter fields because it does not have any explicit dependence on matter fields. As a consequence, H_{em} cannot generate any forces on matter. Since it is precisely this term that generates the vacuum energy of EM field, it follows that the vacuum energy does not generate the forces."

All that by Nikolic is garbage. First of all, Casimir forces as calculated by Casimir (and as defined by Milonni in his quote) are "between uncharged, perfectly conducting plates." Note, such plates therefore are *not* made of "matter" at all (since nobody ever knew how to make perfect conductors or perfect reflectors from matter) but rather are treated as *Dirichlet boundary conditions* for the electromagnetic field. Therefore there is no H_{matt} and no H_{int}. You might object that *experimental* plates *are* made of matter, e.g. silver atoms in mirrors. The approximation of mirrors as Dirichlet boundary conditions has a long history of successful use in electromagnetic calculations. Better approximations (which indeed yield better Casimir theory-experiment agreement) involve, e.g. "skin depth" and frequency-dependent and complex dielectric constants (both semi-empirical), which note, still model the mirrors as continua, *not* atom-by-atom, which would be extremely difficult. If Nikolic wants to ignore all that, then I am not going to join his team.

Also, note that silver atoms have a nonzero diameter$\approx 3\times 10^{-10}$ meter, which prevents shaping mirrors arbitrarily precisely, and that – as well as the finite mass of those atoms combined with the uncertainty principle – prevents precisely localizing such surfaces. However, we could in principle replace all the atoms' electrons with *muons* (207 times heavier), shrinking all atoms by a factor≈207. This also would increase the maximum energy of the photons the mirror reflects (about 6eV in the case of aluminum) and decrease their wavelengths, both by factors≈207. Admittedly there would be the slight problem that muons are unstable with mean lifetime≈2.2μsec, but my point is that in principle mirrors could be made more precise and with higher UV cutoffs by increasing the masses of their component particles, and QED *by itself* in principle permits taking that arbitrarily far; and QED happily permits calculations in the presence of magical perfect-mirror boundaries, not caring whether or not such objects actually physically exist. This suggests that zero-point vacuum energies are *logically* forced in QED.

But I agree with Nikolic (also pointed out by Milonni, and Casimir himself) that "Casimir force" and "**Van der Waals attraction**" are largely the same phenomenon. It ought to be possible, in principle, to compute the ground state energy of *two* hydrogen atoms with fixed proton positions, as a function of the proton-separation, e.g. by solving the 2-electron Schrödinger equation, and in this way determine the Van der Waals attraction between the two atoms, which then would arise without need of any zero-point photon vacuum. This would happen due to *correlations* between the two electrons, e.g. whenever the left H-atom's electron had an unusually leftward location, the right H-atom's electron would prefer also to have an unusually leftward location. If the two H-atoms were replaced by perfectly conducting balls, their Van der Waals attraction again could be explained by correlations developing between their surface charge-density functions (these being functions of both surface-location and time). However, if these atoms or balls were *far separated* then making that work would require those correlations to be appropriately *retarded* (given the finiteness of the speed of light c) and the forces to be transmitted via electromagnetic radiation. However, since no energy can be transmitted when the balls are held in fixed locations, there can be no actual radiation; it all must be entirely "virtual." It then is very natural to regard this "radiation" as the zero-point vacuum modes; and then it naturally, ala Casimir, causes the attraction and whatever correlations are necessary to make it happen.

But if you object to zero-point energy of the photon field in vacuum, then presumably you would try to insist on some sort of 2-charge-correlated-wavefunction explanation, no matter how difficult retardation made that for you. But a crushing difficulty facing any such objector is the so-called ***dynamical* Casimir effect**. That is: suppose Casimir's two parallel plane mirrors are *not* stationary with fixed separation S, but rather S *oscillates*. In that case, QED predicts that the moving mirror will *convert* zero-point vacuum photons into real ones, which could then be detected. (This is sometimes called "Moore's effect" after Gerald T. Moore in 1970.) Two papers on this are Sassaroli, Srivastava, Widom 1994 and Dodonov 1995; the latter predicted "The possibility of creating from a vacuum up to 10^4 photons in a cavity with a Q-factor of about 3×10^{10}." Any experimental proof of that would be very hard for a denier of zero-point photons to live with.

Up to year 2008 there had not yet been any experiment confirming or denying the dynamical Casimir effect, although it seemed one might be (barely) feasible. But then a **breakthough** occurred. To get the largest effects you want the wall to oscillate at relativistic speeds – infeasible. However, Dodonov & Dodonov 2022 and Johansson, Johansson, Wilson, Nori 2009 pointed out that we can *effectively* accomplish that with either an electrically-modulated "Kerr effect," or a magnetically-modulated SQUID, at one end of a waveguide. Wilson et al 2011 then implemented the latter idea, successfully conjuring broadband microwave noise (with the predicted spectrum) out of the vacuum! This appears to be the first successful experimental demonstration of the dynamical Casimir effect. But it would be better if there were a second, more variations, etc, e.g. see the suggestions by Dodonov & Dodonov or by Rego et al 2014. UPDATE: This desired confirmation perhaps was provided by Vezzoli et al 2019, or perhaps that was not good enough.

Summary so far. It is theoretically very difficult to deny the existence of bosonic (e.g. electromagnetic) vacuum zero-point energy. As of year 2023 nobody has found any decent-looking way to do it, and the most pre-eminent attempts all failed. There are many quantitative experimental verifications of vacuum zero-point electromagnetic energy. In short, I am 99.9% convinced electromagnetic zero-point vacuum energy exists. Then the desire for theoretical parsimony makes it also seem likely for bosons other than photons, and for fermions; but for them the situation *experimentally* speaking, is much less convincing.

Fermions

QED claims the vacuum is filled not merely with photon zero-point modes, but also with modes of the *electron-positron* field, albeit since vacuum fermion modes are "unoccupied" they now have *negative*-signed zero-point energies $-\hbar\omega/2$.

Really?

There should be a fermionic analogue of Casimir forces acting in vacuum on surfaces impenetrable to electrons and positrons (or neutrinos). While that is a perfectly fine *theoretical* assertion, it **experimentally seems useless** since there are no real surfaces that reflect neutrinos; and the impermeability for electrons needs to happen for *relativistic* electrons, i.e. with energies$\gtrapprox$511keV, which again no available material can do, and this effect only should become large with separations S between the Casimir plates S$\lessapprox$1 electron Compton wavelength=2.4×10^{-12} meter, i.e. 100× smaller than atoms.

Scenario	Accel (meter/sec^2)	T_{Unruh}(°K)
Planck acceleration unit $c^{7/2}G^{-1/2}\hbar^{-1/2}$	5.561×10^{51}	2.26×10^{31}
Centripetal acceleration of outer part of spinning proton≈$m_pc^3/\hbar$	4.274×10^{32}	1.73×10^{12}
Natural QED acceleration unit $m_ec^3/\hbar$	2.327×10^{29}	9.44×10^8
Mean acceleration of 104GeV electron in LEP ring formerly at CERN (electron frame)	8.7×10^{25}	353000
Mean acceleration of 4GeV electron in SPEAR storage ring at SLAC (electron frame)	1.4×10^{23}	570
Orbital acceleration of electron in Bohr-model hydrogenic atom (immovable nucleus with charge=Ze) ground state: $A=Z^3(\alpha c)^2/a_0$	$9.044\times10^{22}Z^3$	367 Z^3
Wakefield plasma accelerator?	10^{22}	40
Mean acceleration of 6.5TeV proton in LHC at CERN (proton frame)	1.0×10^{21}	4.1
Acceleration corresponding to $\mathbf{T_{Unruh}=1°K}$	2.466×10^{20}	1
Acceleration of proton in Bohr-model hydrogen ground state	4.93×10^{19}	0.20
Electron in 10 MV/meter electric field (≈SLAC linac)	1.759×10^{18}	0.0071
Mean acceleration of electron in SPEAR storage ring (diam=80m) at SLAC (human frame)	2.2×10^{15}	8.9×10^{-6}
Mean acceleration of proton in LHC (Large Hadron Collider, circumf=26659m) or electron at LEP at CERN (human frame)	2.1×10^{13}	8.5×10^{-8}
Surface gravity of middling neutron star	7×10^{12}	2.8×10^{-8}
Rim of 35cm diam carbon-fiber flywheel, 56000 rpm	6.0×10^6	2.4×10^{-14}
SS190 Al-core pistol bullet when in 122.5mm-long barrel (muzzle veloc=650 meter/sec)	1.7×10^6	6.9×10^{-15}
10m long railgun with 3km/sec muzzle velocity	4.5×10^5	1.8×10^{-15}

The theoretically-predicted **Unruh effect**, if it could be experimentally tested, would make it completely clear whether these zero-point vacuum fields really exist: Any accelerated detector (acceleration A) in vacuum at absolute 0 temperature will perceive itself as being in a vacuum at the **Unruh temperature** $T_{Unruh}=\hbar A/(2\pi ck_B)$. E.g. it will detect photons with Planck spectrum, and also (if T is hot enough) thermal neutrinos, electrons, positrons, etc; and will, after long enough acceleration, itself reach temperature T_{Unruh}. (The acceleration magically converts zero-point field modes into "real particles.") The Unruh effect makes it clear that in QED, which particles are "real" and which "virtual" is *observer-dependent*. Detecting this effect is difficult because of the enormous accelerations required: T_{Unruh}=1°K corresponds to acceleration A=2.466×10^{20} meters/second^2. That acceleration is enough to substantially distort any atom. However, Bell & Leinaas 1983 pointed out that the ultra-relativistic electrons in the magnetic fields B>1 Tesla in high-end accelerator storage rings both (1) experience high accelerations, and (2) act as "thermometers" in the sense that electrons at absolute zero temperature would become 100% spin-polarized in a fixed magnetic field – but at positive temperatures T (after enough time has passed) will be expected to be $\exp(-2\mu_e B/(k_B T))$:1 wrong:right-way polarized. The energy-splitting $\Delta E=2\mu_e B\approx e\hbar B/m_e$ between spins 1/2 and -1/2 is 1.16×10^{-4} eV at B=1 Tesla, corresponding to temperature scale $\Delta E/k_B\approx$1.34°K. Bell & Leinaas claim the timescale for decay of the upper spin state (in the electron's frame) should be (3/4) $(\Delta E)^{-3}\,\alpha^{-1} m_e^2 c^4 \hbar$. And in fact, the builders of electron accelerators tried to produce spin-polarized electron beams, but were unable to obtain 100% polarization in storage rings.

> Could the 8% residual depolarization be regarded as a thermal effect associated with the centripetal acceleration? We consider this question of the second part of the paper (section 5) and answer with a qualified affirmative.
> – J.S.Bell & J.M.Leinaas: *Electrons as accelerated thermometers*, Nuclear Physics B 212,1 (1983) 131-150.
> But due to the complications discussed above this measurement cannot be considered as a direct demonstration of the (circular) Unruh effect. Therefore measurement of the vertical fluctuations would be of interest as a more direct experimental demonstration of this effect. However, these fluctuations are small and it is not clear whether it be possible to separate this effect from the other perturbations in the orbit.
> – J.S.Bell & J.M.Leinaas: *The Unruh effect and quantum fluctuations of electrons in storage rings*, Nuclear Physics B 284 (1987) 488-508.
> The Unruh effect does not really require any more experimental confirmation than free quantum field theory as a whole does. [It] is necessary [for] consistency between inertial and Rindler frame calculations of physical observables. An analogy is the appearance of inertial (centrifugal, Coriolis, etc.) forces in noninertial frames. They do not require any more confirmation than classical mechanics does... The Unruh effect is not really a new phenomenon... a variety of lines of argument lead to the same conclusion... Nevertheless, a more direct demonstration of the effect would be highly satisfying...
> – Stephen A. Fulling & George E.A. Matsas: Scholarpedia Unruh effect (2014). F&M also mention that no theorist has disputed the claim that accelerated objects in cold vacuum will thermalize at the Unruh temperature, *but* O'Connell 2020 argued (ridiculously) that the resulting hot object will have a very special magic kind of internal heat which magically *will not radiate* – unlike every other temperature-T body anybody ever heard of, which of course will. Let me just say that O'Connell's "demonstration" of this assertion was based on the "quantum Langevin equation," which (unlike Unruh temperature) is *not* a part of accepted fundamental physics at all, but rather (at best) an approximate model. I therefore regard O'Connell *not* as having refuted "Unruh radiation" but rather the "quantum Langevin equation."

As you can see from their above quotes, Bell & Leinaas at first hoped the experimental finding of 92% electron right-way polarization at SPEAR (agreeing with theory) represented an experimental verification of the Unruh effect, but after deeper analysis in 1987 retracted that claim. Akhmedov & Singleton 2007 reanalysed all this showing the equivalence of the "circular Unruh" and prior "Sokolov-Ternov" effects, *but* the successful experimental verifications of the latter do *not* prove the former because the Unruh effect is numerically "hidden" inside Sokolov-Ternov due to the electron g-factor being (unfortunately for this purpose) numerically near 2. There have been other authors claiming to have experimentally verified Unruh, but I've examined their papers and consider them garbage. So up to year 2024, I claim that no experimenter has ever been able to verify or refute Unruh. The whole Bell-Leinaas story suggests to me that it *is* within the power of the human race to verify/refute the Unruh effect, e.g. by building a purpose-redesigned enhanced version of the SPEAR or LEP machine, but the expense might be tremendous.

Related (and also unconfirmed experimentally) is **Hawking radiation** from black holes.

Detecting the **Sauter-Schwinger effect** would be almost as convincing, and is a lot closer to experimental feasibility. It predicts that in an electric field E over any length L such that $EL>2m_ec^2/e\approx1022$ kilovolts, electron-positron pairs will appear out of the vacuum. These pairs already were there as "virtual" i.e. zero-point vacuum modes, but the electron by getting pulled to one end of the electric field, while its positron mate gets pushed to the other end, thus get converted into "real" particles. There has been some speculation/hype that it might barely be feasible to build an enormous laser like the European "Extreme Light Infrastructure" project, focus its flashes into a tiny spacetime region thus creating an enormous E-field in vacuum, and thus create electrons and positrons from nothing – which then could easily be detected. I suspect their lasers are 10-10000 times too small, but for the purposes of the present discussion let us grant those hypesters the benefit of the doubt. (E.g. see Hur, Ersfeld, Lee et al 2023, Dunne 2009, and Dunne-Gies-Schützhold 2008/9 for recent hype of this ilk.) If you want, you can interpret this as "pair creation" caused by photons representing the E-field. Sauter 1931 and Schwinger 1951 both calculated the pair-creation rate as a function of E and L. The rate only becomes large when |E| nears or exceeds the "**Schwinger critical field**" $m_e^2c^3/(e\hbar)\approx1.32\times10^{18}$ volts/meter. (The critical *length* scale is roughly the electron Compton wavelength 2426 fm.)

However, Sauter-Schwinger is *not* the usual sort of pair creation caused by, e.g. two gamma rays with huge energies (e.g. two 511keV rays) and thus describable by a Feynman diagram with two vertices. Sauter-Schwinger is normally regarded as a "nonperturbative" effect, never calculated via Feynman diagrams. If, however, it *were* to be described by a Feynman diagram, then in that laser scenario, assuming laser wavelength $\lambda\approx1\mu m$ and hence photon energy $hc/\lambda\approx1.24eV$, the smallest Feynman diagram would need to involve at least 824200 input photons and hence at least 824200 vertices!

Unfortunately, it probably will be extremely expensive, perhaps infeasibly so, to demonstrate Sauter-Schwinger in a lab. But I now want to point out that a "poor man's version" of the Sauter-Schwinger effect is entirely experimentally feasible; the experiment has been done many times; and it confirms the existence of electron-positron zero-point vacuum modes! And that is: the "**Uehling potential**."

To set the stage, consider the classical Coulomb field E of a point-charge Q located at the origin (or rotationally-symmetric ball of charge centered at the origin, outside the ball), namely $|E|=(4\pi\epsilon_0)^{-1}Q/|\vec{x}|^2$. The greatest such fields arise when $|\vec{x}|$ is small and Q large. The table shows some isotopes with halflives>12 hours. I gullibly extracted their "nuclear charge radii" from a table by the International Atomic Energy Agency. Apparently those radii were intended to be *RMS* charge radii, although the IAEA did not say so! (The RMS charge radius of the proton, i.e. hydrogen-1, is 0.8414±0.0019 fm according to CODATA 2018, and of an alpha-particle, He-4, is 1.67824±0.00083 fm according to Krauth et al 2021.) Then if the nuclear charge distribution were *uniform* within a ball, the *ball* radius would equal $(5/3)^{1/2}\approx1.291$ times the *RMS* radius. Our table's "surface E-field" and "surface potential" (intended to mean the potential energy experienced by a hypothetical +1 test charge) are computed assuming that uniform-in-ball model.

Isotope	Halflife	Nuclear radius (fm)	Charge (e)	Surface E-field (volt/meter)	Surface potential (MeV)
dubnium-268	16 hours	5.9?	105	2.6×10^{21}?	19.9?
mendelevium-258	51.5 days	5.9?	101	2.5×10^{21}?	19.1?
californium-251	898 years	5.9?	98	2.4×10^{21}?	18.5?
curium-247	15.6 Myr	5.86	96	2.4×10^{21}	18.3
uranium-238	4.5 Gyr	5.86	92	2.3×10^{21}	17.5
lead-208	apparently stable	5.50	82	2.3×10^{21}	16.6
niobium-93	stable	4.32	41	1.9×10^{21}	10.6
sulfur-32	stable	3.26	16	1.3×10^{21}	5.5
helium-4	stable	1.68	2	0.61×10^{21}	1.3
hydrogen-1	stable	0.878	1	1.12×10^{21}	1.3

For us the important thing is that these ball-surface fields exceed the Schwinger critical field by factors 460 to 2000 while the surface potentials exceed pair-production threshold 1.022 MeV by factors 1.24 to 19.5. Hence one would naively expect Sauter-Schwinger pair production to occur all the time in the vicinity of atomic nuclei! If that happened, then the nucleus would absorb an electron (presumably converting one of its protons to a neutron), while a positron would be emitted, i.e. effectively a "β^+ decay" or perhaps "electron capture." And Db-268 and Md-258 indeed do, in part, decay via electron capture, although much slower than the naive Sauter-Schwinger rate prediction. But all the other isotopes tabulated are either stable or decay only by some other mechanism (mainly alpha). The reason for the non-observation of rapid β^+ decay presumably is that the required conversion of a proton to a neutron would consume too much energy (i.e. when *all* energies, not just electrostatic, are taken into account, this decay is disfavored) – and/or due to this E-field not being *uniform* (as in Sauter & Schwinger's calculations) but rather radial – and/or takes a lot of time even in the energetically favored cases because this conversion can only happen via an intermediate W-boson.

But anyhow, the important lesson to draw from this for our present purposes is that near atomic nuclei, the QED vacuum electron-positron field clearly must be severely *distorted*. Vacuum zero-point electrons will move toward the nucleus, while vacuum zero-point positrons will move away from it, causing a net negative-charge density to appear in the vacuum near the nucleus, with the compensating positive-charge excess located further away from it – "vacuum polarization." (I say this is the "poor man's" Sauter-Schwinger both since it costs much less money, and also since Uehling does *not* pull the zero-point e^+e^- pairs completely apart, but rather only partially apart – like stretching, but not tearing, rubber.) That in turn will distort the Coulomb-law electric potential near nuclei (or near point charges generally). The mathematical form of the resulting altered potential was first calculated by Edwin A. Uehling in 1935 as an integral. Frolov & Wardlaw 2012 pointed out that Uehling's integral can actually be expressed in closed form, with the aid of the modified Bessel function $K_0(z)=\int_{0<t<\infty}\exp(-z\cdot\cosh(t))dt$ and the related special functions $Ki_1(x)$ and $Ki_2(x)$ where $Ki_n(z)=\int_{z<u<\infty}Ki_{n-1}(u)du$ with $Ki_0(x)=K_0(x)$. Let $R=rm_ec/\hbar$, i.e. R equals r measured in units of $\hbar/(m_ec)\approx3.86\times10^{-13}$ meter, Then here is the Uehling-improved Coulomb potential $\Phi(r)$ for the interaction of charges Ze and e with center-separation r:

$$\Phi(r) = (4\pi\epsilon_0)^{-1} Ze^2 \, [1+U(R)] \,/\, r$$

where

$$U(R) = (3\pi)^{-1} 2\alpha \int_{1<u<\infty} \exp(-2Ru/\alpha)\,(1+u^{-2}/2)\,u^{-2}\,(u^2-1)^{1/2}\,du$$
$$= (3\pi)^{-1} 2\alpha \left\{ [1+(R/\alpha)^2/3]\, K_0(2R/\alpha) - [R/(6\alpha)]\, Ki_1(2R/\alpha) - [5/6+(R/\alpha)^2/3]\, Ki_2(2R/\alpha) \right\}$$

Table assumes α=1/137.0359991

	R	U(R)		R	U(R)
	2	10^{-245}		10^{-9}	0.02229
	1	5.938×10^{-126}		10^{-10}	0.02585
	10^{-1}	2.251×10^{-17}		10^{-20}	0.06151
	10^{-2}	1.9515×10^{-5}		10^{-50}	0.1685
	10^{-3}	1.3535×10^{-3}		10^{-100}	0.3468
	10^{-4}	4.5087×10^{-3}		10^{-200}	0.7033
	10^{-5}	8.023×10^{-3}		10^{-500}	1.773
	10^{-6}	1.159×10^{-2}		10^{-1000}	3.556
	10^{-7}	1.516×10^{-2}		10^{-2000}	7.122
	10^{-8}	1.872×10^{-2}		10^{-5000}	17.82

Uehling's correction function U(R) differs substantially from 0 only for distances r below 1 electron Compton wavelength. The Frolov-Wardlaw expression then can be used to determine the asymptotic forms of the Uehling potential both very near and very far from the nucleus (despite some pre-2012 textbook authors publishing wrong answers):

$$U(R) = (4\sqrt{\pi})^{-1}(\alpha/R)^{5/2} \exp(-2R/\alpha) \quad \text{when } R\to\infty,$$
$$U(R) = (3\pi)^{-1}\alpha\,[2\ln(\alpha/R) - 5/3 - 2\gamma] \quad \text{when } R\to 0+ \quad (\text{here } \gamma\approx 0.5772156649).$$

albeit Frolov & Wardlaw contend that Uehling's U(R) is physically wrong when r→∞ because other corrections exceed it, hence they suggest subtracting $(225\pi)^{-1}2Z\alpha^7(R+\alpha)^{-5}R$ from it ("Wichmann-Kroll correction"). Note that the Uehling-corrected potential actually is logarithmically *unboundedly stronger* than Coulomb for tiny R. This also has been called the "running of the coupling constant" since it also could be interpreted as α effectively increasing at small distances r.

Although the Uehling correction is numerically small, the "running of the coupling constant" is quantitatively well confirmed in numerous accelerator experiments, e.g. is crucial to allow the angle- and energy-dependent e^+e^- scattering cross section (Bhabha scattering) to be computed today to about 0.003 maximum relative error between QED theory and experiment.

Fortunately there is a brilliant experimental trick that is highly sensitive directly to the Uehling *correction* U(R) and *not* to 1+U(R): the "**Lamb shift**." That is: the known exact solution of the Dirac equation for hydrogenic (i.e. 1-electron) atoms with an assumed exactly-Coulomb potential, claims that the energies of the 2S and 2P states are *exactly equal*. But in reality, they are not exactly equal, and the energy-difference between them can be measured highly precisely as a frequency by microwave absorption techniques. For plain hydrogen-1, this frequency experimentally is **1057847±9** kHz (Lundeen & Pipkin 1986), while QED theory predicts **1057834.12±0.27** (Yerokhin, Pachucki, Patkos 2019). Note that the Lamb shift is only 4.3×10^{-7} reckoned as a fraction of the 2P→1S decay energy.

Any effect QED knows about, but the plain exact Dirac equation 2S and 2P solutions in a Coulomb potential do not know about, contributes to the Lamb shift. The most important are:

1. electron self-energy / self-force, mainly interactions of electron with photon zero-point vacuum;
2. vacuum polarization, mainly Uehling correction (and to a much tinier extent, Wichmann-Kroll);

3. deviations from Coulomb law whenever the electron lies inside the nucleus (i.e. finite nuclear size effects).

If we construct our "hydrogenic atom" not using an electron, but rather a **muon**, then the atom shrinks about 207 times smaller (since muons are 207 times heavier than electrons) causing the Uehling correction *near* the nucleus to be a much more important Lamb-contributor. Also, instead of a *proton* as the atomic nucleus, we could use, say a U-238 nucleus (i.e. 91-times-ionized uranium as a "hydrogenic" atom with Z=92), for similar shrinkage effects. We also could employ μ^+e^- "atoms," which have the advantage that both components are *point* particles, eliminating "finite nuclear size" effects. So the combination of the {electron,muon} and Z-choices give experimenters a wide palette of Lamb shifts to choose from to allow increasing and decreasing sensitivity to various effects.

For **plain hydrogen-1** (proton & electron) QED theory claims the main Lamb contributor is (1), contributing about 1086 MHz, with Uehling contributing -27 MHz (note Uehling has the "wrong" sign), and everything else combined below 2 MHz. In short, this Lamb shift is **97% explained by the zero-point photon vacuum**, whose existence is thereby nicely experimentally confirmed yet again.

For **proton & muon "hydrogen,"** QED theory predicts the Lamb shift (about 50000 GHz) to within about 1.5 parts in 1000 error versus experiment. (Karshenboim, Korzinin, Shelyuto, Ivanov 2015). QED theory claims the main Lamb contributor is (2), i.e. Uehling, contributing about 49600 GHz, with everything else combined below 1000 GHz. So *this* Lamb shift is **98% explained by the zero-point *electron-positron* vacuum**, whose existence is thereby experimentally confirmed.

I'll now explain how the plain-H Lamb shift arises from "interactions of the electron with the photon zero-point vacuum" in a intuitively understandable (albeit not as precise as full QED) way originally dreamed up by **Welton 1948**. (Said interactions also could equivalently be regarded as the electron emitting then readsorbing a photon.)

Welton's story (and some abbreviated calculations) about Lamb shift: The zero-point photon field causes the potential acting on hydrogen's electron *not* to be either the Coulomb or Uehling-corrected functions (both of which are time-invariant), but rather to also include a small *randomly-varying* component. These cause the electron to oscillate to-and-fro randomly, causing the electron to behave more like a somewhat-blurred charge distribution rather than a point. Hence the Coulomb-Uehling potential $V(\vec{x})$ acting on an electron located at $\vec{x}$ really effectively gets blurred over a region centered at $\vec{x}$ with RMS distance-to-center δ, thus altering its functional form by adding $(\delta^2/6)\nabla^2 V(\vec{x})$. Perturbations of $V(\vec{x})$ like this and Uehling's cause energy-level alterations $\Delta E=\iiint \Delta V|\Psi|^2 dxdydz$ which may be computed using the known exact expressions for the pre-perturbation $\Psi(x,y,z)$. To be concrete, a photon mode with electric field $E\sin(\omega t)$ would (under Newton's laws) classically move an electron to-and-fro with RMS amplitude $\delta=2^{-1/2}(e/m_e)\omega^{-2}E$. Because all the photon modes presumably perturb the electron's position "independently randomly" with different oscillation directions and/or frequencies, their effects on the electron's positional perturbation should "sum in quadrature." Using the known expression $8\pi c^{-3}\nu^2 d\nu$ for mode-density in frequency (ν) space, Planck's mode-energy formula $E=\hbar\omega$, the known expression $(\epsilon_0 \vec{E}^2+\vec{B}^2/\mu_0)/2$ for electromagnetic energy-density in terms of the electric and magnetic field strengths $\vec{E}$ and $\vec{B}$, and the known formula $\alpha=e^2/(4\pi\epsilon_0\hbar c)$ defining the fine-structure constant α, and replacing the mode-sum by an integral (valid in limit of large box-size), we find that $\delta^2=(2\alpha/\pi)(m_e c/\hbar)^{-2}\int d\omega/\omega$.

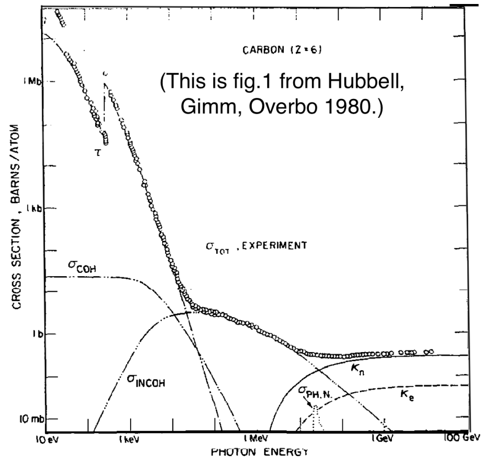


FIGURE 1. Contributions of (a) atomic photoeffect, τ, (b) coherent scattering, σ_{COH}, (c) incoherent (Compton) scattering, σ_{INCOH}, (d) nuclear-field pair production, κ_n, (e) electron-field pair production, κ_e, and (f) nuclear photoabsorption, $\sigma_{PH.N.}$, to the total measured cross section, σ_{TOT} (circles) in carbon over the photon energy range 10 eV to 100 GeV. The measured σ_{TOT} points, taken from 90 independent literature references, are not all shown in regions of high measurement density.

That integral naively should extend from $\omega=0$ to $\omega=\infty$. However, we may argue that only the zero-point modes with frequencies<$\mathbf{F_{hi}}=2m_e c^2/\hbar\approx1.55\times10^{21}$ Hz, i.e. energies<$4\pi m_e c^2\approx6.42$ MeV and wavelengths>$\hbar/(2m_e c)\approx1.93\times10^{-13}$ meters (which is half the "reduced Compton wavelength" of the electron), should especially matter for the purpose of determining the size of the blurring-region, since higher frequencies ought to yield unboundedly smaller positional amplitudes for the electron than Newton's laws would predict, due to quantum and relativistic effects then becoming important enough to invalidate Newton-law treatment.

E.g, examining the figure for photon-carbon cross-section (and ignoring nucleus-involving effects for present purposes) we see that for photon energies below about 10 keV "Thomson scattering" (with electron motion described classically by Newton's law) is the most important photon-electron interaction mechanism; but from 10 keV to about 150 MeV it's "incoherent Compton scattering," and above 150 MeV "pair production" (both quantum-relativistic); with the net effect being greatly reduced cross-section versus Thomson's classical energy-independent formula $(8\pi/3)(r_e)^2\approx66.5\ \text{fm}^2$. Results differ somewhat for elements other than carbon, e.g. for copper Compton begins to dominate at 200 keV not 10 keV. The cross-section falls at greater photon energies, by a factor$\approx10^5$ as we go from 1 keV up to 100 MeV in copper. Welton of course had this UV-cutoff idea himself, although year-1948 knowledge was unable to justify it as well as I just did.

Welton also argued that wavelengths above $2\pi/\alpha$ times the Bohr radius ($a_0\approx52.9$ picometer) of our hydrogenic atom, i.e. frequencies below the electron's naive orbital frequency $F_{lo}=c/(2\pi a_0)\approx9\times10^{17}$ Hz (equivalently energy<3.7 keV) also are irrelevant, because they ought to move the entire hydrogen atom bodily rather than producing relative movement of the electron versus nucleus on time scales faster than an orbit. (In the old Bohr model of hydrogen atom, the electron travels in a circular orbit with Bohr's radius and speed=αc; for nuclear charge Ze multiply the speed, and divide the radius, each by Z.) Then note $F_{hi}/F_{lo}=4\pi/\alpha\approx1722.045$. The Lamb energy shift then is $\Delta E=(1/3)(\alpha/\pi)(m_e c/\hbar)^{-2}\ln(F_{hi}/F_{lo})\int\nabla^2V(\vec{x})|\psi|^2d^3\vec{x}$ using the exactly-known $\Psi(\vec{x})$ for the 2S state (the corresponding 2P integral turns out to equal 0). After doing the integral, the Lamb frequency shift due to this effect in Welton's model turns out to be $\alpha^5(6\pi h)^{-1}m_e c^2\ln(F_{hi}/F_{lo})\approx1011$ MHz, tolerably near the observed value 1058 MHz. Of course, the truncation of the integral to the frequency-interval (F_{lo}, F_{hi}) is only approximately valid and the precise values of F_{lo} and F_{hi} fairly arbitrary. If we instead had chosen $E_{hi}=m_e c^2\approx511$ keV and E_{lo}=1 Rydberg≈13.6eV, then we would have found 1429 MHz. But fortunately the ln(q) function is quite insensitive to q>1000, hence all reasonable-sounding choices of F_{lo} and F_{hi} end up predicting a Lamb frequency within a factor 2 of the experimental value.

Objection: that wasn't really "vacuum"!?! The most die-hard objectors to zero-point energy will now claim (*technically* correctly) that, e.g, the Lamb shift was not really a *vacuum* effect. That is: In QED, "vacuum diagrams" by definition are those with *zero* input and output lines. But all Feynman diagrams used to calculate phenomena like the Lamb shift (or, for that matter, any other experiment), of course do have input and output lines. Therefore the QED vacuum does not "really" affect the Lamb shift or any other experiment describable via the Standard Model.

Response. Well, first of all, the Casimir effect (and its "dynamical" version, if agreed that is experimentally confirmed) goes outside the "Standard Model" by introducing "magic mirrors" (boundary conditions) not made of standard matter. But the die-hard objectors would just fall back on insisting real experimentor's mirrors are made of matter, regardless of how idiotic pretending that is key makes them look. Second, the "vacuum" could arguably be detectable gravitationally (if we regard gravitons as not part of the standard model and hence allowed to interact with vacuum diagrams), and via the Unruh and Hawking effects. And indeed, the **Einstein cosmical constant Λ** is nonzero according to the astronomers.

Third... well look. If some diehard skeptic takes the attitude that the "vacuum" by definition is undetectable by experiment, then nobody will ever detect it experimentally – since if they did, the skeptic would just declare it "wasn't the vacuum" because that vacuum's purity got "polluted" by interacting with an experiment! – in which case this whole argument is unresolvable. I simply do not believe that a teeny tiny, arbitrarily small, arbitrarily far-removed amount of such "pollution" always suddenly completely changes everything. The experimental fact is: the experiments we can think of that come the closest to "trying to detect vacuum zero-point modes," *do* detect them, and keep quantitatively agreeing with predictions to within experimental error bounds. For me, that means the vacuum zero-point modes should be regarded as "existing."

That is not as convincing as detecting the Sauter-Schwinger, Unruh, and Hawking effects would be. Those, especially the latter two, would seem tremendously crushing. But I still consider it pretty good. Game over.

Addendum (Dec.2024): what happens when we examine anti-zero-point-energy papers?

The preceding parts of this paper were written in ignorance of – but I recently became aware of – the papers by Jaffe 2005 and Gründler 2013 & 2017, as part of an apparent physicist subculture disputing the existence of zero-point energy. I will now demolish them. (I emailed this section to both Jaffe and Gründler to offer them a 1-week-long chance to respond or object. Neither replied.) The fact that these (pre-eminent?) two anti-zero-point-energy papers both happened to be so hugely flawed, is not helpful for zero-point objectors.

> Also, before commencing that demolition, let me point out that there is a gluon QCD version of QED's Uehling vacuum polarization effect, causing "running of the strong-force coupling constant," "color confinement," and "asymptotic freedom," all of which have been experimentally confirmed.

Jaffe (a longtime physics professor at MIT who authored over 200 publications, including about a dozen containing "Casimir" in their titles) attacked the Casimir effect as non-evidence for zero-point energy, grandly proclaiming at the end of his paper "No known phenomenon, including the Casimir effect, demonstrates that zero point energies are 'real'."

Jaffe offered no evidence whatsoever for that grand final quote *except* for his attack on the Casimir effect. His paper's abstract contains

> Casimir effects can be formulated and Casimir forces can be computed without reference to zero point energies... The **Casimir force** (per unit area) between parallel plates $\mathbf{F = \hbar c \pi^2/(240 d^4)}$ at separation d vanishes as the fine structure constant α goes to zero, and that standard result, which *appears* to be independent of α [actually] corresponds to the $\alpha\to\infty$ limit. [Later on, Jaffe continues:] My paper shows that the Casimir effect gives no more (or less) support for the reality of vacuum energy fluctuating quantum fields than any other one-loop effect in quantum electrodynamics, like the vacuum polarization contribution to the Lamb shift for example. The Casimir force can be calculated without reference to vacuum fluctuations, and like all other effects in QED, vanishes when $\alpha\to 0+$.

The reason, Jaffe contends, that really $F\to 0$ when $\alpha\to 0+$, is that the plates are made of metal atoms, and photons in the vacuum-gap bounce off electron clouds in that metal, and all such photon-electron scattering vanishes when $\alpha\to 0+$. Therefore the Casimir effect is not a "vacuum" phenomenon at all, it depends upon the properties of *matter*. (That really is the same as the argument by Nikolic 2016/2017, which I already addressed.) Jaffe's EQ 4 states known Drude model approximate formulas for the "plasma frequency" ω_{plasma} and "skin depth" δ_{skin} of a metal. [He cites the textbooks by Ashcroft & Mermin 1976 and Jackson 1998 as sources for those formulas. Unfortunately Jaffe employs non-SI units; SI versions of his formulae are $\omega_{plasma}=e^2n/(\varepsilon_0 m)$ and $(\delta_{skin})^{-2}\varepsilon_0 = 2\pi\omega n e^2 c^{-2} m^{-1} / |\gamma - i\omega|$.] Jaffe notes that the right hand sides of both those formulas depend on the electron charge e and go to 0+ when $|e|\to 0$, i.e. when $\alpha\to 0+$.

Now let me refute Jaffe. For concreteness let us consider two parallel 1-cm-thick metal plates separated by 1 micron vacuum gap.

First of all, if you tried to have metal plates made of electrons and atomic nuclei, and then magically reduced $\alpha\to 0+$ or equivalently reduced the |charge| on each electron and each nucleus (keeping net neutrality), then you wouldn't *have* metal plates. For example, each metal atom would become huge, far huger than 1 cm or especially than 1 micron (e.g. since the Bohr radius formula is proportional to α^{-1}). Therefore, all Jaffe's arguments about the "$\alpha\to 0+$ limit" are utter bunk, since he simply never was allowed to take that limit in this physical problem.

Also, it was ludicrous for Jaffe to try to mischaracterize the Casimir effect as merely another "one-loop effect in quantum electrodynamics." The reflection of a photon off a metal mirror involves the coordinated action of $\approx 10^{24}$ electrons, i.e. any foolish attempt to describe it with a Feynman diagram would need 10^{24} vertices.

Second, Jaffe also speaks of the opposite $\alpha\to\infty$ limit, claiming the usual Casimir-force formula really is only valid in that limit. However, quantum electrodynamics *self destructs* for all sufficiently large α, because, e.g. the hydrogenic atom (exact solution of Dirac equation with stationary point "proton") features "fall in" with infinite energy release (no ground state), if $\alpha>3^{1/2}/2\approx 0.866$. That infinite energy release occurs with positive probability for, e.g. a "hydrogenic atom" made of an electron and antimuon, and will destroy the entire universe. Therefore, neither Jaffe, nor anybody, is ever allowed to employ that opposite limit either, either for considering this Casimir effect experiment, or any other QED problem, nor even allowed to use any $\alpha>0.875$ as the foundation of any such argument. QED simply is not allowed to have too-large α.

Third, an actually-legitimate limit to take (in pure QED, and with gravity "switched off": $G_{Newton}=0$) would have been to make the electron and nuclear *masses* go to $+\infty$ (while holding the elementary charge and α both fixed) causing all atoms to shrink to arbitrarily tiny sizes [e.g. due to the proportionality of the Bohr radius to $(m_e)^{-1}$]; and hence causing the metal in the plates to acquire arbitrarily huge mass-density and atomic number-density. (It also would acquire arbitrarily huge compressive and tensile strength, stiffness, etc.) In this limit, $\omega_{plasma}\to+\infty$ and $\delta_{skin}\to 0+$ according to Jaffe's EQ 4, i.e. the metal plates become perfect mirrors. Although large α are forbidden in QED, pure QED has no objection to heavier electrons. Indeed some exist, called "muons" and "tauons." It is entirely possible (for all we presently know) that some yet-undiscovered fourth vastly heavier lepton exists, even one that is stable. Whther or not such particles exist, *the logical structure of QED is consistent with that assumption*. These particles could be used to build metals far denser and mirrors far superior to any we have today. In this limit, we get perfect mirrors, and we get the usual formula for Casimir force F, which does *not* depend upon α. At all.

Fourth, we can *combine* that actually-legitimate $m_e\to\infty$ limit with a simultaneous decrease (if sufficiently slow) of $\alpha\to 0+$. Here $\alpha=(1/205)\ln(1+m_e/m_0)^{-1.1}$ and $\alpha=(1/137)(m_e/m_0)^{-1/9}$ both ought to be "sufficiently slow." (These limits also would have the advantage of getting rid of QED's "Landau pole," giving it some hope to be a self-consistent physical theory for a refreshing change.) The point of this is that the known "running" of α in QED at high energy scales (or equivalently short length scales, caused by the tininess of atoms in our $m_e\to\infty$ limit) – causing α to effectively become arbitrarily greater than its low energy value $\approx 1/137.036$ – means all the metal atoms *still* will become arbitrarily tiny in this limit *despite* $\alpha\to 0+$, and the proliferation of those tiny atoms will be rapid enough to vastly outfight the slow decrease in α, causing the Casimir effect to still work with its usual F-formula, which is entirely *independent of α*. So I've jiu-jitsu'd Jaffe's argument: now using it against him! According to Jaffe's very own chosen and stated criterion, his own argument now "proves" that Casimir *is* a vacuum-energy effect. Because according to Jaffe's own quote, any electromagnetic energy that does not go to zero when $\alpha\to 0+$, is *not* a QED matter-effect, and therefore must be a vacuum effect.

Fifth, Jaffe's 1-sentence support for his claim that the Casimir effect can be computed "without reference to zero point energies, or even to the vacuum" consisted solely of citing Schwinger's "source theory" – with no hint provided that perhaps that was inequivalent to ordinary QED!

Jaffe also mutters something about Casimir-Polder / Van der Waals forces between polarizable spheres. But: Consider the standard-QED prediction that a rectangular box with wall-thickness comparable to the size of the box will have Casimir energy that is either-sign-infinite (for a perfect-conductor box), or using a metal box made of the gravityless high-lepton-mass matter we just discussed, the Casimir |energy| is finite but rises *quartically* with m_e as the latter is made large. Note this quartic rise is the *same* (to within constant factors) as the mass of the metal box itself. Now lighten the box by making it have a fractal structure: deep within the interior of the metal, use lighter kinds of leptons, using heavier kinds near the surface of the metal. And/or make the metal be a "foam" whose cavities are shaped to cause positive internal Casimir pressures thus helping prevent the box from collapsing by causing tensile stress in metal counteracting the compressive stresses from the box-interior's negative Casimir pressure. In these ways it seems plausible we might be able to decrease the growth of the mass of the metal box to somewhat below quartic, such as power 3.99. If so, we then would be able, in QED supplemented with an arbitrarily-wide palette of leptons, to generate metal boxes with, in net, *negative mass*. This would cause that QED's vacuum to be *unstable* since it would be energetically favored for it to "decay" into collections of such boxes (and antimatter-boxes). Is that possibility still within the scope of anything you can model with notions of polarizable matter without any vacuum energy? I doubt that, because of the proofs of "stability of matter" by Lieb et al, which suggest that QED *cannot* be emulated by any more-conventional-QM model of the sort used in Lieb et al's proofs.

> Speaking of **negative-mass constructs**: It has been pointed out (e.g. Costa & Matsas 2022, although I and others were aware of this 20 years before their paper) that with the usual Casimir two-parallel-metal plates model, if the metal plates were held apart by any kind of "struts" made of any kind of matter obeying the "dominant energy condition" then the positive-masses of those struts necessarily would exceed the negative mass of the Casimir vacuum gap, so that in net the mass of any such object necessarily would be positive.
> But what Costa & Matsas did *not* say (so I will) is: If instead of "struts" we use *light pressure* to hold the perfect-mirror plates apart, then we *can* obtain negative net mass for the vacuum-gap plus intentionally-introduced light (but ignoring the mass of the two plates themselves). Say the metal plates are parallel to YZ planes, Hold the plates apart using light pressure, from intentionally-introduced photon modes bouncing back & forth in the X direction within the vacuum gap. Note that the "missing" Casimir modes responsible for the "negative mass," go in *all* directions with nonzero X component, not just the X direction. They therefore contain greater energy/momentum$_X$ ratio than my intentional modes. Therefore, the plates are held apart in an (admittedly probably unstable) equilibrium, and our vacuum gap contains, in net, negative mass-energy. This equilibrium and negative mass in principle both could persist forever with some tiny amount of active-control assistance to overcome the instability of this mechanical equilibrium – or even wholy passively by adding springs to make the equilibrium become stable.

Sixth, both Jaffe and Nikolic's arguments were illogical. They began with Casimir's problem about perfect mirrors, i.e. boundary conditions on the EM field magically imposed on two parallel planes in vacuum, with no matter present anywhere. Then they insisted on introducing matter since such perfect mirror boundary conditions are not physically attainable, at least as far as they knew. Then, they declared themselves shocked, shocked to find that the physical problem they then had, involved matter – i.e. was *not* about the "vacuum" at all, therefore they declared the Casimir effect tells us nothing about the nature of the vacuum!

Sorry Jaffe & Nikolic – that was not Casimir's fault, it was your fault.

That all was absurd. Probably every physicist during the last 70 years has modeled mirrors, waveguide boundaries, etc, as boundary conditions. That is well known both in theory, and with huge experimental verification, to be a good approximation which would become arbitrarily good in the heavy-lepton limit I discussed, and whose limitations are known. Experiments with real mirrors (provided those "limitations" are obeyed by the experimental design – which, for Casimir experiments, they were) then genuinely do cast light on Casimir's perfect mirror idealization of the problem, which really is about the QED vacuum.

Jaffe perhaps is correct that it might be possible to try to model the Casimir effect without discussing vacuum zero-point energy, e.g. by modeling the 10^{24} particles inside the metal plates, as well as photon modes within the gap, all of them interacting. [Except actually the "photon modes" part of said model would, at least with usual quantum theory, involve zero-point energies,... oops. Jaffe could not do that model without inventing a replacement for quantum field and Dirac radiation theory, which he never did.] Similarly, whenever an electrical microwave engineer modeled a metal waveguide, he could abandon the usual approach and instead try to model every single atom in that waveguide one by one. Those would be insanely bad ways to try to proceed.

Nobody is stupid enough to work that way. And the nonrigorous foundations of QED, likely constitute a major obstruction to any attempt to do that for anybody crazy enough to try. But in principle it perhaps could be done if and when anybody succeeds in converting QED into a rigorous topic, with solid foundations, in which the key series converge instead of diverge. *And* in a version without zero-point energies. There should not have been any pretense by Jaffe that any such thing was already available.

Now let me move on to **Gründler**, who appears to be the sole member of the self-created "Astrophysical Institute Neunhof," apparently located in his domicile. Gründler begins:

> Since 1925, exactly four arguments have been forwarded for the assumption of a diverging [or after regularization very huge] zero-point energy of elementary quantum fields. And exactly three arguments have been forwarded against this assumption. In this article, all seven arguments are reviewed and assessed. It turns out that the three CONTRA arguments against the assumption of that zero-point energy are overwhelmingly stronger than the four PRO arguments.

First of all, of course the "exactly four" was utter bunk (counterexamples having already been stated earlier in this paper) and some of his 4 straw men are rather ridiculous, e.g. based on assuming "cosmic inflation" happened, which is hardly the sort of thing most people ever wanted to (or tried to) rest on as "evidence underlying the foundations of quantum mechanics." Looking into the details of Gründler's 7 arguments, Gründler agrees that the theoretically-predicted zero-point energy of *phonons*, e.g. the vibrational modes of solids, is genuine and experimentally proven, giving citations to back that up. So why are vibrational modes of quantum fields in vacuum (in Gründler's view) different? Well,

1. Gründler proclaims "no aether exists" – the vacuum is not a material substance and therefore is different. (Gründler blissfully ignores the fact that the harmonic oscillator and "raising and lowering operator" *mathematics* are essentially identical.)
2. Gründler proclaims that "interdeterminacy relations," by which he means uncertainty principles such as the Heisenberg-Kennard inequality $\Delta x \Delta p \geq \hbar/2$, are the underlying cause of zero-point energy of acoustic modes. [A view I consider entirely acceptable.] But (Gründler continues) no such principles hold for, e.g, electric and magnetic fields in the vacuum.

Gründler's (2) is simply false: uncertainty principles hold for many quantum mechanical observables, certainly including electric and magnetic fields, and they do indeed force zero-point energies to exist for those fields. This already was thought about by some of the earliest investigators of quantum field theory in works Gründler blissfully ignores, such as Bohr & Rosenfeld 1933. I suggest Gründler review such textbooks as Gottfried & Yan 2003 (see §10.2), Sakurai 1977 (§2-4), and Weinberg's *Quantum theory of fields* (Vol.I, Chapter 8, sections 1-3).

Gründler also discusses the experimental non-existence of huge gravitation from chunks of vacuum, simply ignoring the well known fact that fermion modes have *negative* theoretical zero-point energies hence could hope to cancel the positive energies from boson modes yielding net zero gravity. I'm not saying that magically instantly solves the cosmical constant problem, but it's step one, and Gründler never reached step one.

Gründler plaintively asks why couldn't vacuum quantum fields obey different laws than everything else in quantum mechanics? Maybe they could, but neither Gründler nor anybody else has put forward an acceptable alternative theory, and I've discussed the most prominent failed attempts, which Gründler did not even mention (again: him not reaching step one).

Finally, Gründler in his "appendix A" discusses the vacuum energy's huge quartic power-law infinity, claiming it really only is a logarithmic (much less severe) infinity. He "derives" that by following wrong papers by Evgeny Kh. Akhmedov 2002 and/or Jermoe Martin 2012 (I've refuted Martin elsewhere in my upcoming book so won't go into that here) and using either "dimensional regularization" or "Pauli-Villars regularization." In reality: you simply are not allowed to use 't Hooft / Veltman dimensional regularization on this integral. And the so-called "Pauli-Villars regularized" integral Gründler writes down, simply is not. It's just the wrong integral.

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