

Different viewpoints of the Casimir effect

We greatly enjoyed reading Steve Lamoreaux's fascinating account of the history of Casimir forces (PHYSICS TODAY, February 2007, page 40). This is a phenomenon that still amazes, is a fundamental aspect of quantum mechanics, and may well account for most of the energy in the universe, as well as explain the remarkable abilities of geckos.

However, we would like to offer a few points on which we differ from Lamoreaux.

First, we think that it is not self-consistent to regard the absence of large effects of quantum fluctuations through the cosmological constant as somehow evidence that the quantum vacuum does not exist. Certainly, one can derive the Casimir effect between different bodies in many different ways, including through the local effects of fluctuations in the quantum electric and magnetic fields and through the action-at-a-distance effects of interactions between sources (dipole moments). Mathematically, the two viewpoints, action-at-a-distance and local action, are equivalent, and physically both must exist.

In March 1972 one of us (Brevik)—then a postdoc at the Institute of Theoretical Physics at the Norwegian Institute of Technology in Trondheim—attended a lecture by Hendrik Casimir on the occasion of the 60th birthday of Harald Wergeland, the head of the institute. Casimir and Wergeland were good friends, and Casimir used to visit the institute occasionally. The Casimir effect was not well known then, but Brevik had come to know about it probably via Wergeland. In the discussion session after Casimir's lecture (which was about quite a different topic), he

asked, "Is the Casimir effect due to the vacuum fluctuations of the electromagnetic field, or is it due to the van der Waals forces between the molecules in the two media?" Casimir's answer began, "I have not made up my mind."

Eleven years later, in the summer of 1983 at the national Norwegian Physical Society meeting in Oslo, Brevik met Casimir again, and in a personal conversation asked him exactly the same question. Although Brevik cannot recall now exactly how he replied, it was clear that his opinion about this matter was the same as it had been earlier. Casimir's apparent vagueness was simply a precise observation of a central issue: A dichotomy is present in the Casimir effect. One can argue either way, at least in the electromagnetic case: Ascribe the effect either to a quantum mechanical zero-point effect or to molecular interactions. The molecular picture may be another matter in more extreme contexts, such as in hadron bag physics or in cosmology.

The other of us (Milton), who started working on the Casimir effect in 1976 as a postdoc with Julian Schwinger, had demonstrated to Schwinger's satisfaction that Casimir forces did not require the concept of zero-point energy. However, since Schwinger's approach is based on Green's functions, which can be thought of as vacuum expectation values of products of quantum fields, it now seems apparent that this approach builds in it the concept of field fluctuations.

Second, we are particularly attuned to the issue of temperature corrections to the Casimir effect. Milton, in fact, has been on both sides of the issue. In contrast to Lamoreaux's remarks, we think Lamoreaux's 1997 experiment is not sufficiently accurate to provide a definitive answer to the question of whether the transverse electric zero mode contributes to the Casimir force. There seem now to be overwhelming theoretical reasons for supporting the Drude-model prediction that it does not, and that therefore relatively large thermal corrections should be observable.¹ As Lamoreaux notes, it is likely that precision experiments at shorter distances

are not sufficiently accurate to shed light on the issue. In our opinion, the claimed accuracies on the 1% level in some recent experiments are most likely due to curve fitting, which is thus something different from what we usually mean when describing a specific level of agreement between theory and experiment.

Finally, Milton was very taken by the paragraph referring to wetting. His first introduction to research, as a high-school student in an NSF program at Whitworth College in 1961, involved the experimental determination of the rather large voltages produced during the freezing of water, which was thought to be related to lightning. Apparently, at age 16, he was already on the road to studying Casimir energies.

Reference:

1. I. Brevik, J. S. A. Ellingsen, K. A. Milton, *New J. Phys.* **8**, 236 (2006).

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Lamoreaux replies: I will briefly address some of the points raised in the entertaining letter by Kim Milton and Iver Brevik. Unfortunately, there appears to be some misunderstanding of my intent: I am not proposing the nonexistence of photons or zero-point energy of the electromagnetic field; I am proposing that their introduction presents new problems that must be addressed completely and consistently. Their contribution to the cosmological constant remains a possibility.

Given the length constraints on a review article, it was not possible to touch on every aspect and application of the Casimir and related effects. My goal was to give my unique historical overview and then describe effects and applications most easily accessible to a wide audience. Several books in addition to Peter Milonni's, which I referenced,

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describe the wide applications of Casimir or van der Waals forces and their generalizations—for example, books by Milton,¹ by V. Adrian Parsegian,² and by Vladimir Mostepanenko and N. N. Trunov.³

As to the reported accuracy of various experiments, I prefer to not second-guess those authors, including myself. I have no illusions about the perfection of my own work, but I was careful and found the data internally consistent at the level of error I reported. I did find one calibration error and published an erratum. Given the attention I have paid to other corrections, the recent Drude-model finite-conductivity thermal correction appears incompatible with my experimental result.

A different analysis, done independently by me, Mostepanenko, Giuseppe Bimonte, and others, in which the metal plates are treated as a conducting waveguide, shows a relatively small correction and good agreement with my experimental result. Until the differences between the theoretical approaches are resolved, rejecting experimental results is premature.

As to the experiments that have yielded a 1% level of agreement with theory, the authors of those papers ap-

pear insistent that they have used no adjustable parameters. That work has gone largely unchallenged because the level of accuracy, at submicron plate separations, has not produced significant theoretical controversy; for example, the Drude-model thermal correction becomes very small for plate separations that are significantly below one micrometer.

References

1. K. Milton, *The Casimir Effect: Physical Manifestations of Zero-Point Energy*, World Scientific, River Edge, NJ (2001).
2. V. A. Parsegian, *Van der Waals Forces: A Handbook for Biologists, Chemists, Engineers, and Physicists*, Cambridge U. Press, New York (2006).
3. V. Mostepanenko, N. N. Trunov, *The Casimir Effect and Its Applications*, Oxford U. Press, New York (1997).

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SSC price tag rose from \$3 billion in 1986 to to \$5.9 billion in 1989 to more than \$10 billion in 1993, it steadily lost political support—which was quite strong initially—and was finally terminated by Congress.

With such a track record lurking in its recent past, the US particle-physics community can ill afford to start down such a tortuous (and torturous) road again. We need to play the costing game straight this time and be honest about what the ILC will really cost, or it won't have a chance of getting off the blocks—at least not in this country.

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Randomness and strong disorder

The elegant experiments on photonic localization in randomized photonic crystals (PHYSICS TODAY, May 2007, page 22) raise an interesting question: What happens when an electronic system is strongly disordered but not random? Traditionally, “random” has been thought of as a convenient mathematical tool that gives a good approximation to any strongly disordered system, but a series of experimental studies, beginning in the 1980s with the metal-insulator transition in semiconductor impurity bands, have shown many properties that are inconsistent with random models. Thus the photonic experiments have confirmed idealized models of strong disorder and have simultaneously emphasized just how rare such idealized disorder really is.

Why shouldn't random models work just as well for electronic as for photonic systems? Electronic interactions are normally much stronger than photonic ones, but they often are also randomized by strong disorder. That is especially true in the presence of several kinds of incoherent disorder—for instance, mixed impurities, or impurity clustering, in the case of semiconductor impurity bands. For a long time it was thought that strong disorder is always incoherent, but modern materials science has become so sophisticated that it can produce not only rare examples of ideally randomized materials but also, and much more often, the opposite extreme: self-organized networks embedded in, and actually made possible by, strong disorder.

The classic examples of strongly disordered materials are found in network glasses, which William H. Zachariasen

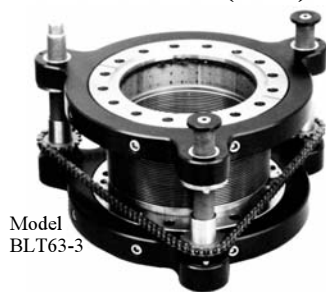
The true cost of the ILC

In his story on the proposed International Linear Collider (ILC), Bert Schwarzschild does his usual meticulous job of reporting the news on particle physics and cosmology (PHYSICS TODAY, April 2007, page 26). But behind the cost figures presented, there's a deeper story that he did not discuss.

The \$7.5 billion total estimate cited is what such a collider might cost according to European accounting practices, assuming it were located at an existing laboratory, like CERN, that could absorb much of the construction management, R&D, and other costs into its normal operating budget. Nor does it include the costs of experimental detectors, contingency, or inflation. Adding those costs would push the total well north of \$10 billion, by my calculations. If, as many of us hope, the ILC were to be built in the US, the Department of Energy would insist that all of the other costs be included, making it—as correctly reported in *Science*—a \$10 billion to \$15 billion project.

Advocates of the ILC are taking a risky path that resembles all too closely the one followed by promoters of the Superconducting Super Collider in its formative years. In that case the additional costs were ignored, and only that of the collider itself was given during the early going. Thus the project initially seemed much less expensive than it eventually turned out to be. But as the

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