Dalvit and Lamoreaux Reply: The central point of the Comment [1] is that supposedly our theory [2] leads to thermodynamic and experimental inconsistencies. It is argued that the alleged failure of our theory is due to "the inclusion of irreversible processes into the Lifshitz formula" (which indeed is derived under the conditions of thermal equilibrium), and that "drift and diffusion currents are initiated by only external electric fields." These statements are obviously wrong and show a clear misunderstanding of Lifshitz theory and quantum statistical physics. (a) Our approach does not involve any irreversible process. The authors have confused irreversibility with detailed balance: the fluctuating fields are dynamic by definition, and lead to (fluctuating) energy exchange between the material bodies and the electromagnetic field. The current driven by the fluctuating electric field is counterbalanced by diffusion. This compensation results in Einstein's relation between diffusion and mobility, and represents the dynamic equilibrium between a time-varying field and the charge distribution in the material. The original Lifshitz formulation itself [3] involves a dissipative component of the dielectric permittivity, and the fluctuations that lead to the Casimir force follow directly from the quantum fluctuation-dissipation theorem. (b) Not only external fields but also fluctuating vacuum fields can induce drift and diffusion processes.

The Comment [1] also discussed the extension of our technique to situations where the electrical conductivity of a material approaches zero at low temperatures, but the charge density in the material does not go to zero. It is assumed [4] that these charges remain "free," and the decrease in conductivity is due to a decrease in mobility of these free charges. We do not agree with the statements in [1,4] regarding this point. In particular, measurements in a restricted high-temperature interval [5] have shown that for dielectrics with ionic conductivity both the conductivity and the charge carrier concentration decrease as temperature is lowered, the mobility remains nearly independent of temperature, and the conductivity activation energy is primarily the energy needed to dissociate the ions. It is not clear to us how the authors of the Comment extrapolate these experimental observations down to $T = 0$ and infer just the opposite behavior.

Our approach, using the classical Boltzmann equation, is limited to those situations where the electron gas is nondegenerate, the case of intrinsic semiconductors being a particular example. We do not include, however, surface state effects which can lead to additional screening of low frequency electric fields. In [1] it is argued that our treatment can be extended to degenerate systems by simply replacing the Debye-Hückel screening length by the Thomas-Fermi screening length, and claimed (without supporting arguments) that such an extension of our theory is incompatible with Nernst theorem. Assuming that the reasoning follows [6], we think that it is wrong since [6] incorrectly describes the $T \to 0$ behavior of a perfect crystal lattice by the normal skin theory for metals, instead of the appropriate anomalous skin theory.

In our opinion important systematic effects have not been properly taken care of in the electrostatic calibrations and Casimir force residuals in the experiments mentioned in [1] at the level of the claimed precision. The experiments reported in [7] adequately addressed systematic effects and achieved an accuracy, in relation to the theory-experiment comparison, of about 10%. This is sufficient to verify the general validity of our theoretical approach; related approaches have recently been published [8,9]. Although these experiments were a measurement of the Casimir-Polder force, the calculational techniques are similar to those of the Casimir force.

Diego A. R. Dalvit[1] and Steve K. Lamoreaux[2]
[1]Theoretical Division
Los Alamos National Laboratory
Los Alamos, New Mexico 87545, USA
[2]Department of Physics
Yale University
Post Office Box 208120
New Haven, Connecticut 06520-8129, USA

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