Feigel Replies: The authors of the preceding Comment [1] declare that they have serious doubts about the predictions of my Letter [2]. A few points, potentially interesting for discussion, are mentioned. However, I found most of the arguments not directly relevant to the specific physical situation considered in the Letter.

The authors of the Comment are right that the L_{fluid}, together with density ρ as additional degree of freedom, would have to be taken into account to describe general compressible fluid-electromagnetic (EM) field interaction problem. However, it was not the main target of the Letter; therefore, the matter part in Lagrangian (9) was chosen to be $\rho v^2/2$. It is correct for the case where spatial derivatives of v can be omitted. Most of the cases considered in the Letter (ideal incompressible liquid interacting with plane EM wave) fit this condition, and lead to homogeneous, directed liquid flow. The accurate microscopic approach [12] leads almost to the same Lagrangian (9). However, the macroscopic approach based on general physical laws (relativistic invariance) seems to be simpler. In addition, the macroscopic relativistic approach allows one to eliminate the numerous difficulties of the microscopic approach. These difficulties are summarized in the Comment: nonrelativistic limit, proper summation over all degrees of freedom, and derivation of consistent macroscopic properties (e.g., refractive index) from microscopic ones.

The $\varepsilon(\rho)$ dependence (outlined by the authors) for the case of ordinary (almost incompressible) liquids is generally small. As far as I know, there is no experimental evidence that this specific effect is important in the experiments considered in the Letter. The bending of an interface due to radiation forces can not be explained using momentum conservation arguments, but rather exact ponderomotive forces (forces which are indeed associated with the gradients of optical constants or with the gradients of the fields) have to be taken into account. It is clearly stated in the Letter and relevant references are provided. In all other experimental situations considered in the Letter integration over time of equation of motion (13) leads to Eq. (14). Equation (14) is similar to the pseudomomentum conservation law and can be understood as the integral of the Lorentz force acting on the medium. Integration constant of Eq. (14) is 0 in all considered experiments in the Letter (the matter initially is at rest).

Derivation of Eq. (10) is based on translational invariance of space. The translational invariance of space is not affected by possible $\varepsilon(\rho)$ dependency. The authors state, incorrectly in my opinion, that vacuum fluctuations cannot contribute to the motion of the matter if ($\chi_{xy} \neq 0$, $\chi_{yx} = 0$). They derive this result from Lagrangian L_{eff}. However, L_{eff} does not correspond to the Lagrangian (19) of the Letter. It can be easily checked by simple substitution of $E = -\frac{1}{c}\frac{\partial A}{\partial t}$ and $B = \nabla \times A$ into (19). The field-matter interaction terms of (19) $\frac{1}{\mu c} B \hat{\chi}^T (v \times B) + \frac{1}{\mu c} \times (E \times v) \hat{\chi}^T E$ are omitted in L_{eff}, although they do not vanish when $\chi_{xy} \neq 0$, $\chi_{yx} = 0$. The absence of the interaction part in L_{eff} leads to complete decoupling between EM field and motion of the matter. This decoupling is described in the Comment. As far as I can guess, the authors of the Comment neglect interaction part by taking into account only one specific (''relevant'') polarization of EM field. However, all possible polarizations (17, 18) have to be taken into account. The choice of only one polarization assumes some polarizers distributed in the matter and is not consistent with the dispersion relations (15).

Both energy and momentum of EM field depend on the optical properties of matter. The change of its optical properties (e.g., χ) can modify the EM modes and the corresponding properties of EM field (including its energy and momentum). The conservation laws require the corresponding changes in momentum or energy of matter. The possibility of energy redistribution between quantum vacuum and matter in Casimir effect is widely accepted. There are no evident physical reasons that can prevent momentum redistribution in similar ways. There are two ways to treat divergence of high frequency modes contribution: renormalization approach and a physically reasonable choice of cutoff frequency ω_{cut}. In some systems there exists a threshold ω_{cut}, such that higher frequencies physically cannot contribute to the considered phenomenon. In the Letter $\omega > \omega_{\text{cut}}$ cannot contribute to polarization. Renormalization approach is definitely more rigorous, and it is interesting and important to know its prediction about the value of the phenomenon described in the Letter. However the Comment provides no estimation of possible corrections.

In conclusion, I would disagree with authors' doubt about the existence of the proposed phenomenon. It is possible to understand their concern about the predicted value, however, the authors of the Comment do not provide any alternative result. It is difficult to estimate the possible error limits of the prediction of the Letter (50 nm/sec), however, this value justifies experimental verification of the proposed effect.

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