Sonin Replies: My paper [1] and the Comment by Gaifullin *et al.* [2] are in disagreement as to whether the magnetoabsorption resonances observed in Bi layered superconductors in strong magnetic fields were the Josephson-plasma resonance (JPR). Gaifullin *et al.* [2] insist that these were JPR because of a good agreement of the experiment with the JPR theory of Bulaevskii *et al.* [3,4] (I call it the strong-pinning theory, see below). But my analysis [1] (call it the revised JPR theory) did not confirm crucial predictions of the strong-pinning theory. If the revised theory is correct, the agreement is fictitious. I cannot discuss here alternative interpretations for resonances and *new* experiments in fields 0–15 mT presented by Gaifullin *et al.* [2] because my paper addressed only the JPR interpretation of *old* experiments in much higher magnetic fields 3–6 T. So the topic of discussion is as follows: *What does the JPR theory predict in strong magnetic fields?*

If the dc magnetic field H is normal or nearly normal to layers, the JPR frequency $\omega(H) = \omega(0)\sqrt{\langle\cos\varphi\rangle}$ depends on H because vortices produce space fluctuations of the phase difference φ between layers. In order to explain the experiment, $\langle\cos\varphi\rangle$ must be much smaller than unity (about $1/25$) and the strong-pinning theory explained it by large fluctuations of φ due to strong pinning of pancakes. But the revised theory estimated $1 - \langle\cos\varphi\rangle$ to be about 1%, i.e., 100 times less than in the strong-pinning theory.

Where does the disagreement start from? The revised theory [1] estimates the φ fluctuation using the expression $\langle\varphi^2\rangle \sim L_J^2/a^2 \ln(\lambda_J/L_J)$ which agrees with Eq. (9) of Bulaevskii *et al.* [3]. Here, L_J is the average length of a Josephson string connecting two pancakes in neighboring layers, λ_J is the Josephson penetration length, and $a = \sqrt{\Phi_0/H}$ is the intervortex distance. The factor $\ln(\lambda_J/L_J)$ originates from the φ variation far from the string. So the disagreement is not due to my assumption that "the phase is large only inside a Josephson string". In fact, implicitly Bulaevskii *et al.* also used it.

But the two theories disagree on the value of L_J. According to Ref. [1], $L_J \ll a$ and the phase fluctuation is small. But the strong-pinning theory assumes that $L_J \sim a$ due to strong pinning of pancakes. Using the formula $\langle\cos\varphi\rangle = \exp(-\langle\varphi^2\rangle)$ (see below) yields $\langle\cos\varphi\rangle \propto 1/B^\nu$ ($\nu \sim 1$), in agreement with the observed anticyclotronic dependence. Neither the previous works [3,4], nor Gaifullin *et al.* [2] present any serious argument to prove $L_J \sim a$ in the whole interval of fields excepting that "correlations in vortex positions along the c axis are very weak", i.e., compared with pinning. But I shall explain that strong pinning is not enough for the condition $L_J \sim a$ to be valid for the whole interval of fields.

Let us introduce the average distance L_d between pinning sites. Suppose that $L_d \ll a$. Then pancakes may choose among the closest pinning sites, i.e., $L_J \sim L_d \ll a$ and the fluctuation $\langle\varphi^2\rangle$ is small, in agreement with Ref. [1]. Now assume that $L_d \gg a$. In this case there are some long Josephson strings with length $\sim a$. But there are not enough pinning sites for all pancakes, and the number of long strings is by factor a^2/L_d^2 smaller than the number of vortices. This means that the average Josephson string length is about a^2/L_d. So the fluctuation is weak again, and $\omega(H) \propto \langle\cos\varphi\rangle \approx 1 - \alpha\Phi_0/HL_d^2$, where $\alpha \sim 1$. Thus the correct JPR theory disagrees with the experiment even if pinning is strong: It predicts a nonmonotonous dependence on H, and $L_J \sim a$ only for nearly equal densities of vortices and pinning sites.

Gaifullin *et al.* argued that one may use the formula $\langle\cos\varphi\rangle = \exp(-\langle\varphi^2\rangle)$ even for large $\langle\varphi^2\rangle$, stating that *sometimes* it is correct (for the Gaussian distribution, e.g.). They do not present any argument why it is correct for *the case under consideration*. However, the dispute over how to calculate $\langle\cos\varphi\rangle$ at large $\langle\varphi^2\rangle$ is important if $\langle\varphi^2\rangle$ is large but, as discussed above, $\langle\varphi^2\rangle \leq 1$.

For fields parallel to layers, I pointed out [1] that the collective-mode spectrum in Ref. [4] was incorrect since it had a gap between "optic" and acoustic branches at the Brillouin-zone boundary, in contradiction with symmetry of a one-site vortex-lattice cell. Gaifullin *et al.* deny the fact that this gap was present in the spectrum of Ref. [4]. But then the discussion is beyond physics: Any reader may check himself that, according to Eqs. (54) and (57) in Ref. [4], frequencies of optic and acoustic branches are not equal at the Brillouin-zone boundary ($q = \pm\pi$ in notations of Ref. [4]) and the gap between them is proportional to k^2. Here $\vec{k}(k_x, k_y, 0)$ is the wave vector in the layer plane. Gaifullin *et al.* stressed that spectra in my paper and in Ref. [4] coincide if $k = 0$. It is right, but the spectrum in Ref. [4] is incorrect for $k \neq 0$.

In summary, the Comment cannot challenge my conclusions on wrong predictions of the strong-pinning theory both in normal and parallel magnetic fields. Therefore the question in the title of my paper remains without an answer, at least for strong fields.

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