Response to "Comment on 'Dispersion relation for the dust ionization and dust acoustic waves in the gas discharge complex plasma'" [Phys. Plasmas 29, 073701 (2022)]

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In the article,[1] a theoretical approach was proposed for the dust ionization (DIW) and dust acoustic waves (DAW) propagating in the cloud of microparticles in the low-pressure gas discharge under microgravity conditions. I welcome the Comment by Pustylnik[2] regarding this work because it contributes to clarification of the importance of a new phenomenon reported in Ref. 3 and its understanding, which was in fact the main task of the article.[1]

Since many more of the issues addressed in the Comment[2] have already been noted and discussed in Ref. 1 as shortcomings of the theory, I believe that in this Response, I have to discuss them in more detail. The notation and numeration of formulas and figures is the same as in Ref. 1.

The first point of the Comment is that the shapes of the theoretical and experimental dispersion relation curves for DIW are qualitatively different. Still, the main result of the article[1] is the DIW dispersion relation (32), where the wave number is almost independent of the frequency (above the cutoff frequency $\omega'_{\rm min}$), i.e., in the (ω, k) plane, the dispersion relation curve is almost a straight line parallel to abscissa. Such behavior is qualitatively the same as follows from the experimental data analyses (taking into account a significant data dispersion), so the shapes of the calculated and experimental curves are principally similar. As for the details of the shapes concerning concavity, convexity, and the presence of a maximum, they cannot be compared directly based both on the existing experimental data and on the theoretical assumptions. Indeed, further analysis of the experimental data[3] has revealed that the behavior of the experimental dispersion relation curve in the vicinity of the cutoff frequency depends on the choice of the interval for the coordinate x selected for recovery of the dispersion relation. This is clearly indicated in the first paragraph on p. 7 of Ref. 1. Note that a specific region from 4 to 15 mm was selected because the microparticle number density n_{d0} was assumed to be determined with the highest accuracy. By variation of the interval start and end points between 1 and 21 mm, one can obtain either slightly convex or slightly concave curve shape with or without a maximum. In addition, the shape proved to be dependent on the excitation amplitude. At the same time, k dropped sharply at low frequencies and assumed approximately the same value at high frequencies demonstrating a weak dependence on the frequency. In the absence of a theory that includes the sloshing oscillations, it is an open issue whether the theoretical dispersion relation curve for a finite-size cloud of microparticles is concave or convex. The concave shape obtained for an infinite cloud could be simply smeared out by the effect of a finite size, and, in my opinion, this is currently a sufficient physical argumentation. The better argumentation can solely be drawn from a forthcoming theory. Note that this point was also discussed in Ref. 1 (pp. 6 and 7). Hence, with the uncertainties discussed above, one can state a correspondence between the experimental and theoretical dispersion relations.

The next point of the Comment concerns the difference between the estimates of n_{d0} adopted in Refs. 1 and 3. This point really needs detailed clarification. A natural shortcoming of the experiments with extended microparticle clouds formed on the Plasmakristall-4 (PK-4) setup is the problem with the width of illuminating laser beam (laser sheet). For an effective 3D depth scan, it should be on the same order of magnitude as the interparticle distance. However, if the camera field of view is far apart from the laser sheet bottleneck then its effective width does not meet such condition. Further analysis of the experimental data[3] concerning determination of n_{d0} showed that it is the case. This means that n_{d0} cannot be determined more accurately than up to an order of magnitude. In view both of this fact and of a considerable variation of n_{d0} along the coordinate x, the variation of n_{d0} by less than two times (down to the number density typical for the dust clouds formed on the PK-3 Plus setup) cannot be regarded as a critical one. Under such conditions of uncertainty, the selected set of complex plasma parameters can be considered as an "inverse estimate" that fits the experimental wave number.

I strongly disagree with the statement of the Comment that according to Fig. 5 of Ref. 3 (cited in this Response as Ref. 1) k_d will drop to significantly lower value when changing n_{d0} from 7×10^4 to 1.34×10^5 cm^{-3}. Instead, Fig. 5 illustrates *a relatively weak dependence* of k_d on the complex plasma parameters. It is this fact that leads to the conclusion of a reasonable correspondence between the theory[1] and experiment.[3] This has already been noted (see the penultimate paragraph of Sec. V on p. 7 of Ref. 1).

Next, criticism is extended to a great change in n_{e0} in Refs. 1 and 3. From the quasineutrality condition $n_{e0} = n_{i0} - Zn_{d0}$, it follows that $n_{e0} = n_{i0}(1+H)^{-1}$. In the advanced version of the theory[1], the assumption $H \ll 1$[3], which is scarcely compatible with the notion of a predominate recombination on the surface of microparticles, is abandoned. Then it is reasonable to select $H > 1$. This decreases n_{e0}[1] by about three times as compared to the previous paper.[3] Accordingly, the Havnes parameter H is increased with the de-

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crease in n_{i0}, which was selected about seven times less than in Ref. 3. These changes together result in a noticeable difference in n_{e0} about 20 times.

The value of n_{i0} is clamed in the Comment to be too small, and higher experimental values[4] are cited. However, such reference is misleading. Indeed, (a) n_{e0} rather than n_{i0} is shown in Fig. 4 of Ref. 4; (b) the lowest pressure in this work is 20 Pa, while in Refs. 1 and 3, the pressure is 11.5 Pa; (c) Fig. 4 of Ref. 4 corresponds to the discharge current 1 mA while in the experiment,[3] it is 0.5 mA; (d) the measurements[4] were performed for the discharge in pure argon, therefore, the results cannot be applied to the complex discharge plasma. Presence of the microparticles would positively reduce n_{i0} provided that most of the recombination events occur on the microparticle surface. Note that this effect must be most pronounced in the dust striation treated in the works.[1,3] In addition, different estimates for k_d in Refs. 1 and 3 cited in Table I of the Comment are the consequence of a noticeable difference in the expressions for this quantity [cf. Eq. (21) in Refs. 1 and 3].

To the best of my knowledge, neither a direct measurement of the ion and electron number density nor a theoretical estimation was reported for a system compatible with that treated in Ref. 3. Generally, a great diversity of data for other systems can be found in the literature. Strictly speaking, no fully reliable parameter can be found for the conditions of experiment.[3] In such situation, a correspondence with the experiment can be claimed if such key quantity as k_d depends weakly on the plasma parameters, which is the case for the proposed theory.[1]

The next point addressed in the Comment concerns a parameter defining the wave damping. First, the Comment incorrectly cites the result of Ref. 1, according to which the DIW mode has an order of magnitude *larger* damping length as compared to the DAW mode but not vice versa as in the Comment. Then, it is speculated that a relevant parameter is $k_d\delta l/2\pi$, and that it is on the same order of magnitude for DIW and DAW modes. Instead, the damping length is by definition the length over which the wave propagates without a noticeable change in its amplitude, and an important parameter defining the experimental visibility of the wave is the ratio of the damping length to a real width of the camera field of view $\delta l/\Delta$, where $\Delta \sim 1$ cm. According to the estimates,[1] this parameter must differ by an order of magnitude for DIW and DAW. This accounts for the fact that it is the DIW mode, for which $\delta l/\Delta \sim 1$, rather than the DAW mode that is observed experimentally.

The last issue addressed in the Comment concerns validity of Eq. (2) of Ref. 1. Presence of the ambipolar-like time-averaged electric field, whose drag compensates the ion drag force, cannot contradict the idea of local ionization balance, which is essential for treatment of the DIW mode, already because solely the second and the third term on the right-hand side of Eq. (5)[1] are retained in the DIW mode, and the last term including the ambipolar field as well as the first term can be neglected. This is consistent with the conclusion that self-excitation of DIW is impossible (see the notes in Sec. II of Ref. 1). It is also noteworthy that, in contrast to the results of the article cited in the Comment as Ref. 5, the electrostatic and ion drag forces can be balanced at $n_{i0} < 10^9$ cm^{-3} if one takes into account the explicit dependence of the ion–microparticle collision cross section on n_{d0} (see article[1] and references therein). At the same time, I agree that numerical simulation (maybe not the first-principles one) of a dust cloud under the conditions of Ref. 3 is highly desirable as a potential validation of the approach developed in Ref. 1.

In conclusion, I would welcome any alternative theory of the abnormally fast long-wavelength longitudinal dust wave mode (even if it did not explore the effect of the increase in recombination rate due to the microparticle surface) if it was more consistent than the theory proposed in Ref. 1. Of course, further experimental and theoretical efforts are required in the investigation of the discussed phenomenon. However, taking into consideration the discussion above and the lack of an alternative, I believe that the experimental data still can vindicate a defining role of the ionization effects in the DIW propagation.

AUTHOR DECLARATIONS

Conflict of Interest

The author has no conflicts to disclose.

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