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B. Liebchen[1,2,a] and H. Löwen[1,b]

AFFILIATIONS

[1] Institut für Theoretische Physik II: Weiche Materie, Heinrich-Heine-Universität Düsseldorf, 40225 Düsseldorf, Germany
[2] Institut für Festkörperphysik, Technische Universität Darmstadt, 64289 Darmstadt, Germany

[a] Electronic mail: liebchen@fkp.tu-darmstadt.de
[b] Electronic mail: hlowen@hhu.de

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Surprisingly, all points raised in the comment by Popescu *et al.*[1] seem to scientifically agree with the paper[2] it addresses. Let us try to clarify what appears as a virtual dissent in the following.

Reply to the key point of the comment: Both here and in Ref. 2, we fully agree that both phoretic and hydrodynamic interactions can in principle be important in active colloids. Which of them dominates depends on the specific setup under consideration as detailed in Secs. V B and V D of Ref. 2. We quote from the conclusions of Ref. 2: "*In a broad class of autophoretic Janus colloids (half-capped, uniform or moderately nonuniform surface mobility) and corresponding active-passive mixtures, they [phoretic interactions] even seem to dominate over the more commonly considered hydrodynamic interactions. Conversely, hydrodynamic interactions probably dominate over screened phoretic interactions at a very low density [. . .] and for cases of strong effective screening [. . .]. Finally, for Janus colloids with a strongly asymmetric coating geometry or a strongly nonuniform surface mobility [. . .] phoretic interactions and hydrodynamic interactions may be similarly strong. Note also that in certain swimmers,[23,62] phoretic interactions might be more complicated than described here.*"

Irrespective of this, the comment, Ref. 1, notes: "*Here we demonstrate that their claim, that in the far field the phoretic interactions generically ('often seem to') dominate the hydrodynamic ones, is flawed on account of the points detailed below.*" This misrepresents Ref. 2 where no such general claim occurs.

Before systematically replying to the more detailed points of the comment,[1] note that also the sentence "*contrary to the opinion of LL that such aspects have been neglected in the literature, the importance of the phoretic interactions is well known for more than a decade*"[13–15] lacks evidence[3] and also misrepresents Ref. 2 which mentions the following 18 references regarding phoretic interactions.[4–21]

Reply to (i): "*In contrast to the claim by LL, the model of self-phoresis considered by them typically involves hydrodynamic interactions with precisely the same far-field decay as the unscreened phoretic ones.*" As discussed quite explicitly in Sec. V B of Ref. 2, we fully agree that hydrodynamic and unscreened phoretic interactions show the same far-field scaling in most cases ($\mu_r \neq 0$). Thus, the statement "*In contrast to the claim by LL*" appears to be misleading. The rest of the discussion following point (i) in the comment agrees with Ref. 2.

Reply to (ii): "*Equation (9) in Ref. 12 is incompatible with the model [Eqs. (1), (3), and either (5) or (8) therein] if $\mu_r \neq 0$.*" We fully agree that $\mu_r \neq 0$ implies the occurrence of a hydrodynamic field decaying as $\propto r^{-2}$ (this is discussed in Sec. V B of Ref. 2) which is indeed incompatible with Eq. (9). For this reason, unlike suggested by the comment, we do not use Eq. (9) for the case $\mu_r \neq 0$. Instead, Eq. (9) is used only for the important special case $\mu_r = 0$ (Sec. V A of Ref. 2), which may serve as a useful approximation to half-coated colloids with a weakly nonuniform surface mobility at typical densities. So, there is no dissent also here, and what remains to be said is only that the statement "*The model, claimed by the authors to be 'generic' is actually internally inconsistent*" again misrepresents Ref. 2 which does not make such a claim (see also Sec. VI of Ref. 2 and footnote[22] regarding the justification of the model).

Reply to (iii): "*It is an ad-hoc procedure to discard, within a far-field theory and based on the analysis of their relative amplitudes (Sec. V in Ref. 12), one of the two contributions (phoretic*

or hydrodynamic), both of which exhibit the same far-field power law decay." We agree also to this point. However, from our viewpoint, such "*ad hoc*" simplifications of reality performed in the framework of models being as-simple-as-possible are often crucial to progress our understanding in physics. Therefore, when considering active colloids with phoretic and hydrodynamic interactions featuring the same far-field scaling ($\mu_r \neq 0$) but very different coefficients in some cases (see Sec. V B of Ref. 2), it seems to be sensible to formulate a simplified model involving only the stronger interaction. Following such an approach leads to a remarkably simple model naturally creating dynamic clustering, as in experiments.[5,6,23,24] This indicates that attractive phoretic interactions (if effectively screened) and self-propulsion may be sufficient to generate dynamic clustering, but of course, this implies in no way the absence of "decoupled" effects due to hydrodynamic interactions. (Note also that the statement that the model considered in Refs. 25–27 is "*basically the same [. . .] as considered by LL*" is not true.[28])

In conclusion, scientifically all points raised in the comment[1] seem to agree with Ref. 2. The dissent suggested by Ref. 1 therefore appears to be rather virtual and might root in a semantic debate. In particular, the usage of terms like "generically important" (deliberately not meaning "generically dominant") or "many active colloids" (not meaning "all active colloids") may deserve further quantification in future publications.

To close the discussion, let us emphasize once more that *one* key aim of Ref. 2 was to question the still common negligence of phoretic interactions in models of active colloids, rather than arguing that hydrodynamic interactions are generally irrelevant there. (The other key aims have been to derive a minimal description of phoretic interactions and to eliminate free parameters, the latter being crucial to understand the strength of phoretic far-field interactions.)

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[2] B. Liebchen and H. Löwen, "Which interactions dominate in active colloids?," J. Chem. Phys. **150**, 061102 (2019).

[3] The point "more than a decade ago" lacks evidence in the comment, as the only work from before 2015 to which it refers is Ref. 4 which simply mentions the existence of such interactions in a single sentence and does not comment on their relevance.

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[22] Notice in addition, that keeping terms in leading order in r only, Eqs. (5), (7), (8), and (11) in Ref. 2, can be viewed as the lowest order expansion in μ_r, to be performed separately for center of mass and orientational coordinates (see Sec. VI).

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[28] While Ref. 25 does not discuss chemotaxis/phoretic interactions and Ref. 26 discusses run-and-tumble bacteria adapting their tumbling rate in response to chemical gradients, the interesting work[27] explores, among others, a smooth turning particle model, which is conceptually closer to Ref. 2, but discusses results for slender rods (mainly in the absence of rotational diffusion) for which self-propulsion and chemical interactions are controlled by independent parameters and where the chemical field has not been integrated out. All of this contrasts Ref. 2, the aim of which was to derive a minimal model for active colloids with phoretic interactions.