Active particles driven by competing spatially dependent self-propulsion and external force

I thanks the authors for their answers to my concerns and for the changes implemented. I further apologize for the change of variable that I used to support my point about the similarity between spatially-dependent self-propulsion and driving forces: it contains indeed an error as the authors correctly noticed. I used it as a fast example to make my claim more precise but I should have detailed it more to exemplify what I meant. Let me correct it below by detailing how this change of variable should be carefully processed in order to map a dynamics with position-dependent self-propulsion speed onto one with a constant self-propulsion speed. I also take the opportunity to comment on the replies sent by the authors. All the references below correspond to the new version of the manuscript.

- The starting point is the evolution equation considered by the authors in one spatial dimension

  $$\dot{x} = -\partial_x U + u(x)\eta \tag{1}$$

  where η is an OU noise. Assuming that the spatially-dependent activity $u(x)$ is a positive, bijective function then we can divide both sides by $u(x)$ to obtain

  $$\frac{\dot{x}}{u(x)} = -\frac{\partial_x U}{u(x)} + \eta \; . \tag{2}$$

  Then we define the primitive of u^{-1} as $G(x) = \int_1^x 1/u(s)ds$. Because u is a positive function, G is also a bijection. Furthermore, we have that

  $$\frac{dG(x)}{dt} = \frac{\dot{x}}{u(x)} \tag{3}$$

  So, if we define the change of variable $\tilde{x} = G(x)$ we then have

  $$\dot{\tilde{x}} = -\frac{\partial_x U(G^{-1}(\tilde{x}))}{u(G^{-1}(\tilde{x}))} + \eta \; , \tag{4}$$

  which I think achieves to show that, in this case, one can map the model of the authors to the dynamics of an AOUP with a constant self-propulsion speed. This is only a rapid reasoning which might not be exempt of misstakes: I hope the authors will point out these caveats if they find them. However, if (4) is indeed confirmed, it really maps the model of the authors to the dynamics of an AOUP with constant self-propulsion speed evolving in a complex (possibly non potential) landscape. And this is why I am not convinced by the authors' arguments supporting the motivation of their model. It seems to me that the effect of the interplay between spatially-dependent self-propulsion speed and potential forces will be equivalent, in a lot of cases and at least in one dimension (as studied in this paper), to the effect of a more complicated landscape in a normal setting with constant self-propulsion speed.

In addition, I don't understand the authors' description "non-Gaussianity induced by the interplay of confinement and spatially modulating swim velocity". As it is already known that an active particle with potential forces and constant self-propulsion speed has a non-Gaussian distribution both in x and v, then there is no need to have an interplay with a spatially-dependent self-propulsion to get a non-Gaussian distribution.

- Concerning my remarks explaining how the findings of the authors on figure 1 and 2 could be retrieved by applying general and common principles of active matter, I understand the stance of the authors with respect to the accessibility of their manuscript to less experienced readers. However, I think that the Ref[74] (written by the authors), already provides an accessible manuscript with detailed numerics and analytics about a very similar problem. Furthermore, there are other references already tackling the problem of position-dependent self-propulsion in AOUPs such as Ref[63]. It is not clear to me what are the authors' additional contribution with respect to these previous works.

- In their replies, the authors pointed out a main difference between the model in Ref[63] and the model described in their manuscript, which, according to them, makes their proposed model more relevant for experiments. Indeed, they assert that the model in Ref[63] has a distribution of the form $\rho(x) = 1/u(x)$ (with $u(x)$ being once again the spatially dependent self-propulsion) only in a limited regime depending on the persistence length. However, I believe that this statement is wrong because a careful reading of Ref[63] shows that the distribution $\rho(\mathbf{r}) = 1/u(\mathbf{r})$ is recovered in a limited regime only when the self-propulsion speed $u(\mathbf{r})$ depends on the *set of position* $\{\mathbf{r}\}$ of the other active particles. When the self-propulsion speed $u(\mathbf{x})$ depends on the absolute position of only the particle under consideration, then the result $\rho(\mathbf{x}) = 1/u(\mathbf{x})$ of Ref[63] holds without any limitations. Thus, the authors have not convinced me about the relevance of their models with respect to the other ones in the literature. Why should it be relevant and more adequate for describing experiments?

- Finally, I would like to come back to the concern I expressed with respect to the authors' statement "the interplay between the external force and the modulation of the swim velocity can be used to manipulate the behavior of a confined active particle, for instance by locally increasing the kinetic temperature or by forcing the particles to accumulate in distinct spatial regions with different probability". My concern was that in their previous work [74] the authors have already shown that an active particle with spatially-dependent self-propulsion $u(x)$ has a distribution $\rho(x) \propto 1/u(x)$. Thus, my point was that the potential was not needed if one wanted to manipulate and sort active particles: a spatially-dependent $u(x)$ is enough.

  The authors reply that in a typical experimental setting the active particles are confined and that thus there is a need to study the interplay between confinement and self-propulsion speed.

  I am not convinced by this reply because if the particles are confined then one can just manipulate the confining potential in order to force the particles to accumulate where one wants. In my opinion, the authors did not clearly explain what are the physical features exhibited by their model which are not already present in an AOUP evolving in a complex potential with fixed self-propulsion speed (see the first point above with the change of variable). That is why I am not convinced by several claims of the authors about the emerging complex behaviour from the interplay between spatially-dependent self-propulsion and potential forces. For example, "the deviations of the velocity distributions from a Gaussian shape exclusively arise from the interplay of these two fields". For a single AOUP with fixed self-propulsion evolving in a (not harmonic) potential, the velocity distribution is already non gaussian without needing to invoke a complex interplay. Another example is the following statement "We demonstrated that by combining these two physically distinct effects, it is possible to generate complex density patterns through two relatively simple fields, which is surely easier to realize in practice than generating a single external field with a complex shape.". First, it is not clear to me why generating two external fields is easier than one, especially when this last one is a confining potential. I don't believe that it is hard to manufacture a sheet of plastic with some meanders and up-and-downs where you can put your active particles on. Second, it is not clear to me what is the aforementioned "complex density pattern" because, as I explained in my first review, one could have deduced it by applying two fundamental principles of active matter.

  Nonetheless, I agree with the authors that there are physical differences between an AOUP with spatially-dependent self-propulsion speed and an AOUP in a confining potential which are clarified in the new footnote [76]. My concern is that I do not see the new complex behaviour or the new features due to the interplay between the two ingredients (potential forces and spatially-dependent self-propulsion) which are not already present whenever only one of the two ingredients is present.

My opinion is that the authors have not answered to my main concerns, and especially that the novelty and importance of their work with respect to the current literature (Ref[74] and Ref[63]) is not clear to me. Because of all the points discussed above, it seems to me that the main message of "interplay between spatially-dependent self-propulsion and potential forces yields complex physics" is not really grounded. Therefore, I would not recommend this manuscript for publication in scipost.