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

This paper studies an active particle (AOUP) with spatially-dependent activity in a confining potential and more especially in a harmonic well. The authors have a written english of good quality which makes the reading pleasant. In the first half of the paper, they tackle their model analytically by using two approaches: the UCNA approximation and an exact perturbative scheme. While I did not try to reproduce their computations in details, their study seems rigorous and well-grounded in the literature: the uncontrolled assumptions are stated and the terms neglected are mentioned. In the second half of the paper, the authors perform numerical work to support their study. They distinguish three regimes for the density depending on the parameter $v_0\tau/S$: monomodality, bimodality and multimodality. For each of these three regimes, they also supply the distribution of the velocity as well as its variance when the spatial position is varied. Despite all these results, I had troubles to understand the novelty or the take-home message of the paper. In particular, I have four major concerns which I develop below

- To which extent is taking a spatially-dependent self-propulsion $u(x,t)$ different than just having a single AOUP in a complicated potential? From the authors' results, it does not seem that there is unexpected physics due to the spatially-dependent $u(x,t)$. In particular, all the features highlighted by the authors for an AOUP with spatially-dependent self-propulsion (accumulation near walls for example) are already present for an AOUP with constant self-propulsion. I have remarked that, when $u(x)$ is a bijection, one can map the dynamics (3) of the authors to the dynamics of an AOUP with spatially-independent self-propulsion by making the change of variable $\tilde{u} = \ln(u(x))$. Indeed, dividing both sides of (3) by $u(x)$ leads to

$$\frac{\dot{x}}{u} = -\frac{\nabla U}{u} + \eta \qquad \Leftrightarrow \qquad \dot{\tilde{u}} = g(\tilde{u}) + \eta \tag{1}$$

  where $g(\tilde{u}) = \nabla U(u^{-1}(e^{\tilde{u}}))/e^{\tilde{u}}$ and η is an Orstein-Uhlenbeck process as described in (2b) of the paper. From (1), I intuitively do not expect much differences between an AOUP with a spatially-dependent self-propulsion and one with a constant self-propulsion in a more complicated potential. Have the authors studied the question?

- The authors' results can be obtained by applying two main results of active matter that are already well-established. The first one is that active particle with spatially-dependent self-propulsion $u(x)$ accumulate where they are slow as $\rho(x) \sim 1/u(x)$ (Refs [28] and [29] in the introduction). The second one is that confining an active particle with a potential U leads to an accumulation at the position where the force $\nabla U(x)$ is equal to the self-propulsion amplitude u (Ref [71] and [86] from the authors). This last result, which has been documented, could also be seen as a consequence of the former one: the confining force effectively reduces the self-propulsion of the active particle which in turn triggers an accumulation due to slowness.

Figure 1 and figure 2 are intuitively explained in light of these two established results. For example, in figure 1d, spikes of the density correspond to positions where $\nabla U(x) = u(x)$. In figure 2d, the two spikes close to the center correspond to local minima of $u(x)$ while the two highest peaks correspond to both local minima of $u(x)$ and positions where $\nabla U(x) = u(x)$. Thus, it is not surprising that these spikes are the highest ones: the effective self-propulsion at these positions is lowered by the potential compared to the self-propulsion at the center.

Finally, I believe that the bimodality of the velocity distribution displayed in figure 3h is not surprising; it is due to particles coming from the cluster on the right and going leftway as well as to particles coming from the cluster on the left and going rightway. Because the confining force increases away from the center, the position of the spike with negative velocity diminushes from yellow to brown while the trend is opposite for the spike with positive velocity.

- I do not see what is the new physics brought forward in this work which has not already been discussed in Ref [70] by the authors. For example, in the present paper, the authors affirm that "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". However, in their previous work [70], the authors have already demonstrated that the density of their model follows the law $\rho(x) \sim 1/u(x)$ in the absence of potential. Thus there is no need of a confining potential if one wants to sort and accumulate active particles in distinct regions: a spatially-dependent self-propulsion is enough. What is the motivation and the new physics when studying the interplay between a potential and a spatially-dependent self-propulsion?

- As the authors point out in their work [70], there is already a literature studying spatially-dependent self-propulsion amplitude and memory time for AOUP (ref [61] for example).

In [70], the authors detailed, from a mathematical point of view, why their model (2) is actually different from the model studied in [61]. However, from a physical point of view, the difference remains unclear: what are the physical features present in (2) that are not in [61]? In particular, when it comes to experiments [25-27], what is the correct model one should use? It would be interesting if the authors show that their model is indeed the one relevant for the experiments.

Given the four major points discussed above, I would not recommend this paper for publication in SciPost. I believe that the authors should more significantly ground the novelty as well as the physical motivation that drove the writing of this paper. Beside the important points raised above, I only have minor remarks which I listed below:

- I did not see an indication about the dimension considered in simulations: are there performed in $d = 1$ or $d = 2$?
- the change of variable presented right after (3) might benefit from a clearer presentation. I was first confused about whether v included the confining force.
- In the appendix eq (27), one side has $u(x)$ while the other has $u(x, t)$
- Right before the part on density distribution: "when $v_0\tau/S$ grows, the spatial period of u(x) increases" should be "when $v_0\tau/S$ grows, the spatial period of u(x) decreases" if I understood correctly the authors (S being the spatial period).
- Right in the beginning of the part Velocity distribution, "display an almost Gaussian shape in agreement with Eq (3)" should probably be "display an almost Gaussian shape in agreement with Eq (10)". The same reference lapsus is also found later in the same part "Gaussian distribution with space-dependent variance given by Eq (3)".
- On figure 3; is the x-axis rescaled? Is it x or $\tilde{x}$?
- In the end of the part called profile of the kinetic temperature, I could not understand well the phrase "the variance of the particle velocity becomes steeper and decreases to zero in the regions which are not explored by the particle. This is consistent with the scenario observed in Fig. 1 (c): the particles accumulate in the regions where they move slowly and the velocity variance is small". How can particles accumulate in regions that they do not explore?