Reply to Referee 1

We thank Referee 1 for useful suggestions and comments. These comments and suggestions have helped us improve the manuscript and its readability substantially. In the revised manuscript, we have clarified all the comments and questions raised by Referee 1.

1. **Q:** In the introduction, the authors write "Theoretical investigations of this phenomenon have thus far concentrated on continuum models [8-11]". I think that Ref 8 deals with lattice models and would be better placed below with ref 14. The reference [Whitelam, Klymko, Mandal, "Phase separation and large deviations of lattice active matter.", The Journal of chemical physics 148.15 (2018): 154902] should also probably be cited.
   Ans: Both references are cited.

2. **Q:** The second paragraph of the second column of the introduction should be fixed to reflect the content of the cited articles.
   Ans: Corrected.

3. **Q:** Speaking about grand canonical ensemble in the introduction is surprising at first since ensemble equivalence for active matter has not been demonstrated. I think this is coming too early and that it would be much clearer later on, once the factorized steady-state has been put forward.
   Ans: The focus of the article is to show that MIPS phase transition is not possible in 1D. We thought it is important to mention 'how we are going to prove this point' in the beginning. Since both high and low densities co-exist in MIPS phase, this phase can not be characterized in the grand canonical ensemble with a unique macroscopic homogeneous density. Thus beyond the transition point (if any), the density-fugacity relation breaks down. In other words, a stable MIPS phase exists when the maximum achievable density in GCE is less than unity (fully occupied lattice).

   We think these discussions will provide the reader an idea about the flow and the target of the article.

4. **Q:** "Nonexistence of MIPS transition in restricted tumbling model would imply that the same can not occur in any other RTP model in 1D". "The same" refers here to "Nonexistence of MIPS". I think the authors wanted to state the opposite: "Nonexistence of MIPS transition in restricted tumbling model would imply that MIPS cannot occur in any other RTP model in 1D".
   Ans: Thank you for mentioning- it is now corrected.

5. **Q:** The "restricted" tumbling hypothesis is a crucial difference between this model and other models previously studied. Its definition and its discussion should come much earlier in the article. The role of the asymmetry (right neighbor vs any neighbor) should also be discussed.
   Ans: Corrected. We also use the word "restricted" tumbling in the abstract.

6. **Q:** When the authors discuss the mapping to the beads-in-urn model, they should comment on the relationship with the mapping from TASEP to ZRP.
   Ans: Yes, we commented and added a reference [23].

7. **Q:** The beginning of the second column of page 2, when the current $J(m)$ is discussed, is very hard to understand. This part should be clarified and detailed.
   Ans: Now it is mentioned clearly in Eq. (6).

8. **Q:** The two-urn section is not very clear: it implies $N = 2$ but then an arbitrary N comes back in Eq (2) and the $N = 2$ case is only introduced later on in page 3. This should be completely rewritten: if the authors first want to do the general case, they should do so and only specify later on to the $N = 2$ case. If they want to consider $N = 2$ all along, they should do so. Furthermore, page 3 is amazingly dense and hard to read. I would strongly suggest detailing this part so that this can be understood by active-matter readers who are not familiar with the matrix-product Ansatz.
   Ans: Without changing the flow of the article we pushed large part of the discussions on Matrix Product Ansatz to a separate section, "APPENDIX". More detailed calculations are given there for better readability.

9. **Q:** The authors should discuss the implication of the large- M limit, on page 3. This seems to imply a dilute limit which is not where MIPS is expected. Later on, they show the exponential distributions to survive at larger densities and I suggest that the authors clearly explain if this is a justification for using their results beyond the low-density limit.
   Ans: Large M limit is not required while setting the two-urn current as the effective hop-rate of the coarse-grained urn model. We consider large M limit to get an asymptotic functional form that matches well with the two-urn current obtained from Monte Carlo simulations. We clarified this point in the article.

10. **Q:** Figure 2: The authors should explain how the color code is related to the values of ω.
    Ans: This is now mentioned in the caption of Fig 2.

Other general comments:

1. **Q:** I.e., there is no phase separation in 1d. I thus think that, contrary to the authors' claim, phase separation in 1d has never been considered possible. Furthermore, the authors consider a model in which at most one particle per site is allowed. It has been shown by Soto and Golestanian that this only leads to finite-size clusters (in 1d and 2d; See ref 14 and [Sepúlveda Soto Phys. Rev. E 94, 022603 2016]). I thus think that the results presented by the authors are rather in line with the existing literature, contrary to what the article suggests.
   Ans: Although indication about non-existence of MIPS phase in 1D is mentioned, directly or indirectly, in some articles based on the results from Monte Carlo simulations or mean-field theory, a clear statement or a proof has never been obtained. Our article rather attempts to prove this point for hardcore RTPs in 1D.

2. **Q:** That said, the mapping to the urn model is interesting, and reminiscent of the mapping between the TASEP and the ZRP (See Section 2 of [MR Evans and T Hanney 2005 J. Phys. A: Math. Gen. 38 R195). Unfortunately, its analysis is very hard to follow and the algebra presented on page 3 will be impossible to understand for readers who have not worked on matrix ansatz and related models.
   Ans: Large part of the discussions on Matrix Product Ansatz is now pushed to a separate section, "APPENDIX". We hope, it will help the reader to arrive at the results and conclusions of the article without bothering much about the detailed mathematical steps of matrix product ansatz.

Summary of changes

The article has gone through a **major revision** following the valuable comments of the referees. It is difficult to provide details as in the revised manuscript, the whole structure and almost all the paragraphs are modified. In the following we list some important changes.

1. Restricted tumbling dynamics is now given separately as Eq. (2) followed by a longer discussion.

2. Fig. 1 is modified - exact and coarse-grained urn models are now described more clearly in Fig1(b).

3. Discussions on Matrix Product Ansatz (MPA) is described in the APPENDIX. We hope it helps the readers to arrive at the results and conclusions of the article without bothering much about the detailed mathematical steps of MPA.

4. New references [15], [16] and [23] are added.