Response to reviewers

We thank both reviewers for their further constructive criticisms and suggestions. We have taken these on board to improve the manuscript. We hope the paper is now acceptable to both reviewers.

In the following, we respond to each reviewer's comments in turn. Reviewer's comments are in Italic font with our response both indented and in Roman font.

Along with these responses, we have uploaded two copies of the revised manuscript, a marked-up version showing changes from the previous submission as well as a "clean" copy. Where these responses include line numbers, these refer to the marked-up version.

Reviewer 1

General comments:
The authors have taken on board many of the suggestions from myself and the other reviewer and the changes they have made to address our comments have improved the manuscript. In particular, I felt that the limitations of the study were not made clear enough in the original manuscript whereas, in the revised manuscript, the authors have made an effort to highlight these limitations to a greater extent. I am still of the opinion that precise quantitative analysis of two-dimensional simulations (and experiments) has very little relevance to real-world dunes as acknowledged by the authors, which limits the impact of studies such as this. However, there has been a precedent in recent years for the publication of similar studies and so this work does merit publication and I would like to again commend the authors for their work.

> We thank the reviewer for their positive comments and their honest appraisal of this work, during both rounds of the review process so far. We acknowledge that the extent to which 2D experiments and simulations are relevant to dunes in the natural world remains uncertain, and that there is clearly a gap between the complexity of the processes we consider here and what occurs in nature. Hopefully, this gap can be addressed as new knowledge is gained, both through studies which consider detailed specific subsets of processes as well as those which consider a more comprehensive viewpoint.

Specific comments:
Line 70 - "Although our study is strictly only valid…"

- *New sentences like this that make the limitations more explicit have greatly improved the manuscript.*
- *It is also important to note though, that the differences between 2D and 3D systems are not solely confined to properties of the flow but also to the interactions (e.g. avalanching etc.) between longitudinal cross-sections in a dune.*

> > We have now added an extra clause to this sentence to make clear that lateral sediment transport during collisions is also an important difference between 2D and 3D systems (lines 73-74).

Line 210 - "It is important to note…"

- *It's good that you have made this more explicit.*

Line 212 - "We compare…"

- *Perhaps the authors could explain here why the repulsion observed in Bacik et al. (2020) was not observed in Jarvis et al. (2022). The authors have sufficiently explained why their model cannot reproduce the wake-induced repulsion but have not explained why that effect was not observed in their chosen quasi-2D experiments.*

There are multiple possible reasons why repulsion was not observed in Jarvis et al. (2022). In particular, it is important to note that, in the experiments of Bacik et al. (2020), which considered the interaction between a pair of dunes in a periodic domain, dune repulsion acted to push the system to a state where the dunes would find equilibrium in antipodal positions. In the experiments of Jarvis et al. (2022), the dunes were allowed to form spontaneously from a flat bed. Consequently, at the time of interactions between discrete dunes, there were approximately 10 dunes, almost evenly spaced around the flume circumference. Since these dunes were already almost evenly spaced, if dune repulsion did occur, the effects would have been very small and difficult to observe. We have now added text to the manuscript to explain this (lines 236-241).

Figure 4 and lines 251-261

- *I am not sure if the separation plot is the easiest figure to interpret although it was helped by the paragraph the authors included. I did not feel that this added to the work.*

We have moved the separation plot and the associated text to a new Appendix C (lines 320-331 and Fig. C1).

<u>Reviewer 2</u>

Dear authors,

You revised the manuscript and answered to my comments properly. Although in many parts I still disagree with you (you will see from my comments below), I recognize that this is a well conducted work that represents a significant contribution to the field. Therefore, my advice is the manuscript acceptance.

Comments

- You could have sent a marked-up version. This would facilitate the task of reviewers. Please consider doing that next time.

We did upload a marked-up version and apologise to the reviewer if this was not clear. We have ensured to do this again.

- I am satisfied with the modified sentences on the probabilistic nature of the method (opposed to the deterministic characteristics of the problem itself).

> We are pleased that we were able to address this to your satisfaction and feel that the manuscript is better for this.

- I reproduce here one of my comments in the previous review: "After briefly discussing the turbulent wake shed by the upstream dune (line 39), you state that details of turbulence are negligible in the 2D simulations because turbulence is inherently 3D. However, the presence of a recirculation bubble in the wake of the upstream dune (independent of turbulence, since it can simply be a recirculation region) can significantly affect the dune-dune collision (even avoiding it, as shown in the experiments of Bacik et al., PRL, 2020). In addition, 2D dunes in nature (or in labs) have a finite thickness, and, therefore, the flow can be turbulent. Please consider reformulating your sentences".

I do not totally agree with your answer. Turbulence can be inherently 3D, but not the recirculation region, which can exist in 2D (or quasi-2D) flows. Under high confinement, one should expect strong effects of this recirculation region on dune-dune collisions, as shown by Bacik et al., PRL, 2020. This is strongly related with another of my comments in the previous review: the simulations should reproduce the results of Bacik et al., PRL, 2020. Otherwise, it seems that there is something to be fixed.

> We fully agree that the recirculation zone can exist in 2D flows. Indeed, it is present in our simulations which are performed in a pure 2D domain, with flow separation at the dune crest and reattachment at some point downstream. However, the presence of a recirculation zone alone does not lead to the collision-suppression and dune-repulsion phenomena reported by Bacik et al. (2020). In fact, Bacik et al. (2020) show that, in their experiments, the migration velocity of the downstream dune is strongly influenced by fluctuations in sediment transport caused by the turbulent wake shed by the upstream dune. This turbulent wake is generated by decay of vortical structures into a 3D turbulence field. Thus, in a 2D domain, this decay does not occur, and the small-wavelength fluctuations do not form. Consequently, collision-suppression is an inherently 3D phenomenon and cannot be reproduced in our simulations.
>
> We have added a clause to try and better explain this in the manuscript (lines 46-47 and lines 291-297).

- I reproduce here other of my comments in the previous review: "You compare your numerical results against those of Jarvis et al. J. Geophys. Res: ES, 2022, in which a train of dunes was present. Please consider comparing your results also with the experiments of Bacik et al., PRL, 2020. For example: can your simulations reproduce the dune-dune repulsion observed by Bacik et al.? If not, why?"

Here again, I do not totally agree with your response: Since the experiments of Bacik et al., PRL, 2020 were conducted in a Hele-Shaw circular flume, then their results should tend to a 2D *problem. For instance, their results for 2D dunes are different from the barchan-barchan case, and the differences are assumed to be due to confinement. Perhaps the cause for the simulations not reproducing the results of Bacik et al., PRL, 2020 is some other limitation of the numerical method. I suggest that you consider that in future works.*

> The flume used in the experiments of Bacik et al. (2020) has a width *W* of 9 cm, whilst the total flow depth *D* is 40 cm. This is an aspect ratio of *W*/*D* = 0.225. Additionally, Figure 3d of Bacik et al. (2020) shows a dune height of about 8 cm. Therefore, the width of the dunes in these experiments is comparable to their height. Consequently, although the channel is sufficiently thin to reduce (albeit not remove) lateral variation in dune morphology, the spatial scale of turbulent fluctuations in the fluid flow is significantly smaller than the channel width. Therefore, whilst the mean fluid flow will tend to a 2D flow field, the turbulent fluctuations will still be 3D. We think this is a strong argument for why the 2D simulations fail to reproduce the simulations of Bacik et al. (2020).
>
> Nevertheless, we concur that there may be other limitations of the numerical model and that fully 3D simulations are necessary to verify this. We now acknowledge this in the manuscript (lines 291-297).

- *I still consider that some more quantitative comparisons should be incorporated.*

> Although we are unable to include further quantitative comparisons in this manuscript, we agree that such work would be highly valuable going forward. Indeed, this is something we would like to consider for further work. We have added some text to the manuscript to expand on this (lines 298-299).

- *I reproduce here other of my comments in the previous review: "On the one hand, experiments and DNS show that the slope angle of the leeside is important, and, on the other hand, your results are based on a probabilistic approach/analysis: should not the model consider the slope angle as a (stochastic) variable?"*

I understand the additional extra work that this would engender, but I believe that the slope of the leeside is crucial for what happens to the downstream dune. Could this be one of the causes for not reproducing the results of Bacik et al., PRL, 2020?

> We acknowledge that the leeside slope angle may very well play some role in the behaviour of the downstream dune. However, we feel that the 2D-3D difference is the more significant factor in the inability of the simulations to reproduce the dune repulsion of Bacik et al. (2020). Nonetheless, we have now added some text to the manuscript to emphasise that allowing for a variable lee slope angle may be necessary to fully capture the complexity of dune-dune interactions (lines 294-297).

- *You started your answer to one of my comments with "The cellular automaton model is probabilistic, not deterministic". I know that, and never stated the contrary. The problem with* *the previous version of the manuscript is that the sentences were misleading (you affirmed in that version that the collisional process was not deterministic…).*

> We never intended to suggest that you were unaware of how cellular automaton models work. Instead, we wanted to state everything in the clearest possible terms to try and be clear and minimise any confusion. Most importantly, we wanted to avoid assuming any knowledge. We are happy to hear that the reviewer is satisified with our sentences on the probabilistic nature of the method.