Response to reviewers

We thank the reviewer for their 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 of the reviewer's comments in turn. Reviewer's comments are in Italic font, with our response both indented and in Roman font.

The manuscript presents a numerical study of dune-dune collisions in two-dimensions. For that, the authors carried out cellular automaton simulations and compared the results with quasi-2D experiments. The subject is interesting, and the manuscript is well written and should be considered for publication. However, I have some concerns that I list below.

General comments

- You affirm that the collisional processes are not deterministic. In my opinion, you must better justify this affirmation, or reformulate some of your sentences. For me, the physics here is deterministic, since the motion of each sand grain is deterministic. Of course, one can analyze or model the problem as probabilistic, but, in principle, it is (I believe) deterministic.

> We have rephrased some sentences to make clear that we are modelling the process as probabilistics (lines 66-67, 193-194, 263-264)

- 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 affect significantly 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.

> We agree with the reviewer that, even in 2D systems, the recirculation region downstream of a dune's crest can affect dune-dune collisions. Critically however, in 3D systems, this wake will decay into a 3D turbulent field. This is the case in the experiments of Bacik et al. (2020) which, although they take place in a narrow flume, are now pure 2D. In our pure 2D simulations, however, the wake behind a dune cannot produce turbulent fluctuations. We have now tried to explain this better in the revised manuscript. For line numbers of specific changes, please see the response to specific comments below.

- 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?

> The simulations cannot reproduce the dune-dune repulsion observed by Bacik et al. (2020). This is because the repulsion phenomena occurs through the action of turbulent wakes downstream of dune crests. Since we are in a pure 2D system, unlike the quasi-2D system of Bacik et al. (2020), the simulations do not produce turbulent wakes and thus dune-dune repulsion does not occur. We have now edited the introduction of the manuscript to try and explain better why we do not observe dune-dune repulsion in these simulations (lines XX).

- In my opinion, in their current form the comparisons with experiments are most qualitative. In order to be more quantitative, you should present the dune profiles (perhaps superposed), celerity of crests, values of mass exchanges, etc. This would strengthen your conclusions.

The focus of this manuscript is to specifically consider the transition between coalescence and ejection, rather than the general properties of collisions. Therefore, we feel that Fig. 4C presents a quantitative comparison between simulations and experiments for this purpose. Whilst comparing other properties may be interesting, e.g., migration speed, mass exchange values, these wouldn't help to constrain the transition and, thus, we don't consider them in this study.

Specific comments

- The manuscript is well written and agreeable to read, congratulations for that.

- Line 24, "Such collisions have been frequently observed in subaqueous experiments and numerical simulations…". They have also been observed, although with incomplete time series, for aeolian dunes.

We have now edited lines 25-26 to include this.

- Lines 27-32: It should be stated clearly here that these collisional processes are specific for 2D dunes. For 3D dunes, processes are more complex, with the existence of splitting mechanisms and other types of ejection (of new dunes), as you explain briefly in the next paragraph. In addition, it is not only the dune-dune collision that redistributes sand, but also dune-dune interactions through disturbances of the fluid flow (mainly the wake of the upstream dune), which can cause, for instance, surface waves (on the downstream dune) and, consequently, calving.

We now clearly state that coalescence and ejection are specific to 2D dunes (line 39) and now also refer to wake interactions causing calving (lines 42-44)

- Lines 41-42: the presence of a recirculation bubble in the wake of the upstream dune (independent of turbulence, since it can be simply a recirculation region) can affect significantly 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 sentence.

We agree with the reviewer that recirculation, even in the absence of turbulence, can affect the collision. However, for dunes sufficiently large to be self-similar, the recirculation zone length scales with the dune size. Thus, in pure 2D, the collision outcome still only depends on the size ratio between the dunes. We have reworded this section to make this clear, as well as emphasise that 3D turbulence will be present even in quasi-2D laboratory experiments (lines 45 – 47).

- Line 51: Please consider stressing here that those results are, in principle, valid only for subaqueous barchans, and have not been tested against aeolian barchans.

We have now stated that the results in Assis & Franklin (2020) are for subaqueous barchans (lines 55-57)

- Line 58: Ok. But please note that 2D dunes in laboratories have finite thicknesses and are not strictly 2D.

As stated in the response to the above comment, we now emphasise that quasi-2D dunes in experiments are, in reality, 3D (lines 45-46). However, we wish to remphasise that the numerical simulations are pure 2D.

- *Line 64, about comparison with experiments: Please consider comparing your results also with the experiments of Bacik et al., PRL, 2020.*

As stated in response to an above comment, our numerical results are not directly comparable to the results of Bacik et al. (2020). The quasi-2D nature of the experiments in Bacik et al. (2020) means that turbulent wakes occur, and these cause dune-dune repulsion phenomena. In our pure 2D cellular automaton, these turbulent wakes are not reproduced (although recirculation zones are) and, therefore, we cannot obtain dune repulsion. We have reworded the introduction to try and make clear that effects caused by 3D turbulence, including dune repulsion, cannot be reproduced with this model.

- *Lines 75-76: 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?*

For a single simulation within the cellular automaton model, it is necessary to set the angle of repose, and therefore the maximum slope angle, as a constant input to the model. While it would be possible to run lots of simulations with different angles of repose drawn from a probability distribution (effectively treating it as a stochastic variable) this would present both practical and scientific difficulties. Practially, this would require substantially increasing the number of simulations beyond the 1600 we have already performed. Scientifically, randomly selection values of the angle of repose from a probability distribution prevents any accountability of the physical controls on this parameter.

We therefore chose to not explicitly consider the effect of angle of repose on the collision dynamics in this study.

- *Lines 76-77: I find this sentence strange: for me, the physics here is deterministic. One can analyze or model the problem as probabilistic, but, in principle, the motion of each sand grain is deterministic.*

The cellular automaton model is probabilistic, not deterministic. Importantly, the model does not solve for the motion of individual sand grains. Instead, as described in lines 77 – 85, sediment transport is modelled using transitions of pairs of nearest-neighbour cells corresponding to physical processes (erosion, deposition, transport). The rates for these transitions are set as input parameters and determine the probability for a particular transition to occur. Precise details on the model can be found in Narteau et al. (2009) and Rozier and Narteau (2013).

- *Lines 95-96: I do not totally agree. I would expect variations in quasi-2D experiments (or simulations), since the flow disturbances (wake) generated by the upstream dune vary with the flow strength.*

In 3D (or quasi-2D) simulations, we agree that flow strength would impact the wake downstream of dunes. In our pure 2D simulations, where the flow regime is also far from the sediment transport thresold, the length of the recirculation zone is determined purely by the size of the dune. We have now tried to better explain this in the manuscript.

- Line 100, about comparison with experiments: Again, please consider comparing your results also with the experiments of Bacik et al., PRL, 2020.

Please see our comment above where we have addressed this.

- Lines 135-136: Please note that you compare your data with dunes that are not strictly 2D (then, turbulence may be important)…

We now explicitly stated throughout the manuscript, most notably in lines 221 – 224.

- Lines 163-179. This part deserves a deeper discussion: you could present some statistics of mass exchange in order to strengthen your points. Another thing is that you should better justify your analysis, since your simulations do not compute the trajectory of each grain based on Newton's second law: as a reader I would expect a discussion on how accurate the simulations are and if they are physically consistent.

The paragraph the reviewer refers to qualitatively describes the morphological structures that are created during the dune collision, as depicted in Figure 1. This figure clearly shows that the morphology of each type of interaction is clearly distinct, even without quantification. We agree with the reviewer that aspects of this could be quantified, e.g., the proportion of mass which is exchanged during the interaction. However, the aim of this paper, is to describe and quantify the transition between coalescence and ejection. We are not aiming to go into quantitatively describing the different types of transition themselves.

On the 2nd point, the reviewer is correct that the simulations do not explicitly compute sand grain trajectories. However, the model has previously been shown to reproduce dune dynamics successfully. The model produces dunes from unidirectional flow over a flat bed (Narteau et al., 2009) and this emergent behaviour can be compared with natural systems to set the length and time scales of the model (Narteau at al., 2009; Zhang et al., 2010; 2014). Beyond this, the model has also been shown to predict pattern coarsening (Gao et al., 2015).

- Lines 182-183: This affirmation is rather strong. In my opinion, the processes are deterministic (since each grain follows Newton's second law). What happens is that one can analyze the problem (from experiments, for example) from a probabilistic point of view, or use a probabilistic model to compute/simulate the processes. The fact that we can use a probabilistic model that works does not mean that the problem is not deterministic in essence.

We have now reworded this sentence to clarify this (lines 194-195).

- Line 199: I do not agree. In my opinion, the comparisons are most qualitative. In order to be more quantitative, you should present the dune profiles (perhaps superposed), celerity of crests, values of mass exchanges, etc.

We have now reworded the sentence to be clear that we are comparing the morphology of the interacting beforms qualitatively and the regime transition quantitatively (lines 212-213). Whilst all of the other suggestions are possible, as noted above, the primary aim of this paper is to quantify the regime transition. The additional proposed analysis would not contribite towards this.

- Lines 210-211: You should better justify this assertion.

The text in the previous version of the manuscript was erroneous and misleading. Although we have performed some simulations with a leeside slope angle of $\theta = 18$ º, they are only indicative, showing that ejection and coalescence occur as observed in the simulations where $\theta = 35$ º. We have now clarified in the manuscript this caveat (lines 225-227).

- *Lines 214-215: So, you could not compare the results for this case. Can you find published data of experiments for this case?*

Experimental observations of downstream-dominant coalescence have been reported by Coleman & Melville (1994). However, these were between continuous, transverse bedforms. To the authors knowledge, there are no experimental data concerning downstream-dominant coalescence between discrete dunes.

- *Lines 236-237. Again: you need to better justify this affirmation. In my opinion, the process is not probabilistic in essence (it is deterministic), but can, perhaps, be analyzed (or computed) in a probabilistic way.*

We have reworded this sentence to make this clearer (lines 264-265).