

Earth Surf. Dynam. Discuss., referee comment RC1
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Comment on esurf-2022-55

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Referee comment on "The probabilistic nature of dune collisions in 2D" by Paul A. Jarvis et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2022-55-RC1>, 2022

General comments:

Overall, this work represents an interesting development in the field of Earth Surface dynamics and, in particular, the study of bedform interactions. The finding that dune interactions are probabilistic represents a novel result and may come as something of a surprise to many researchers in the community. I welcome this finding and believe that this work represents a sufficient advancement to warrant publication. The authors should also be commended for the style of the report which is generally well written, organised, and presented.

I do, however, have some general problems with the study. These issues centre primarily around the limitations of 2-dimensional regimes and the question of how well these results scale to realistic 3-dimensional systems. More specifically, I find the attention paid to exactly determining the form of the observed stochasticity rather irrelevant since any real-world system will behave differently because of the increased dimensionality. Additionally, the comparison with the experiments presented in Jarvis et al. (2022) is perhaps an odd choice since, as the authors point out, those experiments involved a "train" of interacting dunes rather than the binary system considered in the simulations presented in this work. Although the authors mitigate some of the issues this might present by considering only interactions where there was no physical overlap with the additional bedforms, there may be wake effects such as those identified in Bacik et al. (2020) which are not accounted for in this work. I believe that the discussion of the limitations of this work should be made more detailed as, in my opinion, these problems are more substantial than they are made to seem in the current manuscript. Nevertheless, the essential finding that collisions are stochastic rather than deterministic is an important one and the authors should be congratulated for their work.

I will now provide some more specific comments.

Specific Comments:

Line 25 - *"pattern coarsening, whereby a larger number of smaller dunes transition to become a smaller number of larger dunes..."*

Coarsening has been observed in many experimental and numerical studies of dune dynamics. However, many natural dune systems have been shown to be homogeneous rather than coarsening. This point should be explicitly made here.

Line 41-42 - *"Turbulence, however, is an inherently 3D phenomenon..."*

Interesting claim given that the water tank experiments of Bacik et al. (2020) reported that induced turbulence led to repulsion even in their quasi-2D setup. Furthermore, the claim that only the size ratio controls the collision outcome is likely only true for sufficiently large bedforms where the assumption of scale invariance applies.

Line 59 - *"However this means we can exhaustively..."*

The merit of such an exhaustive study is severely limited however by the fact that 3D and 2D systems are inherently different. Although you may be able to fully understand the problem in 2D, one must recognise that this is still a toy model and that a study in 3D is going to have more real-world impact even if it cannot be quite as exhaustive.

Line 60 - *"By performing large numbers..."*

This makes it sound as though thousands of simulations have been performed but we later find out that it was only ~ 50 and that, in fact, the number of simulations that could be performed was a limiting factor on the uncertainty of the findings.

Line 126 - *"Two distinct types of coalescence..."*

This phrasing makes it sound like they are very different processes, however the authors themselves describe that the intermediate stages are only "slightly different" and later (line 151) state explicitly that "...close to the transition it is very difficult to distinguish the two types of behaviour". This suggests that the types of coalescence are not really "distinct" as claimed here but two regimes of a single coalescence process between which

there exists a continuous transition.

Line 149 - *"...so many simulations would be required to gain meaningful outcomes."*

But the authors claimed in the introduction that they had performed "large numbers" of simulations and were able to "exhaustively study" the phenomena. This is a direct contradiction and makes it seem as though the claims made in the introduction were unwarranted.

Line 160 - *"...creating multiple small bedforms"*

Would it not make sense to define the cases where different numbers of bedforms were generated as different types of collisions, particularly as in 3D it may be possible for these new bedforms to escape from between the dunes? This would also be more consistent with the distinction between the types of coalescence identified by the authors for which the intermediary stages were key.

Line 188 - *"Performing further simulations..."*

Again, this ought to be mentioned earlier as the introduction makes it seem that the study was not restricted in this manner.

Line 194 - *"finding $a = 14 \pm 2$ and $b = 0.509 \pm 0.005$."*

Given that the results of 2D experiments are not likely to be fully scalable to 3D I do not believe that exact determinations of these constants are particularly relevant to real-world systems. As such, I think these values could easily be removed to an appendix.

Figure 3 caption - *"...is due to only 50 simulations..."*

Same point made previously, this is a major shift in tone from the introduction.

Line 204 - *"...only simulated interactions between two discrete dunes... train of interacting dunes"*

I think this is a more important caveat than the authors make it seem. Other similar studies (e.g. Bacik et al. (2020) have found that in these systems wake induced turbulence plays a critical role. The turbulence generated by multiple interacting bedforms in these experiments is likely to be greatly affecting the outcomes. Whereas, this is not the case in the simulations where only two dunes were present.

Line 205 - "...the number of simulations... is relatively small"

Same point made previously!

Line 209 - "However, additional simulations..."

If these simulations have been performed already and the authors wish to compare with the experimental results of Jarvis et al. (2022) then why not simply present the results from the experiments where $\theta = 18^\circ$ rather than those where $\theta = 35^\circ$?

Line 225 - "There is reasonable agreement..."

I would like to see a table containing these data and ideally some statistical tests about whether the experimental observations are statistically similar to the stochastic rules defined in this work.

Line 253 - "...agree well with numerical determined..."

Again I would like to see these data.