**Review report of EGUSphere-2023-211 ESD**

In this paper the influence of lee-slope angle and shape on flow properties is investigated through numerical modelling simulations. Results are presented in outstanding figures, with findings that valuably contribute to knowledge of the interaction between bedforms and hydrodynamics. However, the classification of dunes (i.e. the bounding lee-slope angles defining the classes) does not follow the results in this paper. Please see suggestions for an alternative line of thought and Figure 9.

**Reviewer’s comments:**

Please see \*211\_Rev.pdf, for comments with the text (balloons) and text corrections (Text insertions and deletions).

* Good abstract, describing the relevance and results
* Excellent introduction, with strong background information; rationale and hypothesis.
  + In the first 100 lines of the paper, incl Table 1, I can’t seem to find whether simulations are for unidirectional or both uni-dir. and oscillating flows. The only clue is ‘rivers’ (=uni-dir). Please add to the aim (lines 59-60): “for unidirectional flow”.
  + Some suggestions for the use of existing literature.
* **Main point #1:** The paper needs a more consistent use of symbols, and equations vs. explanation in words. This balance in the text is quite unlogic in this matter, e.g. lack of explanation in words for k-ε in the model description (line 95), and on the other hand, it contains words in equations where you’d prefer symbols/an equation (line 160-161: e.g. *Lsr* = 0.17 *αmean* – 0.67, TKEmean = 0.00004 *αmean* – 0.0009 and appendix (line 442). Furthermore, the paper would benefit from adding units, see comment with lines 92-96). Suggestions of how to do this:
  + Add a symbol after the description in words, then use the symbol in an equation. E.g., “The mean turbulent kinetic energy, *TKEmean*, is computed as …”; and then in the results: “ TKEmean = 0.00004 *αmean* – 0.0009”.
  + NB. These symbols could then be used on axes of result figures as well (since the text explains how these are calculated, see section 2.3 and caption Fig 3)
* Excellent reasoning for initial choices of simulation runs (section 2.2) and flow parameter analyses (section 2.3). However,
  + The runs are all for unidirectional flow. That is okay, but the introduction suggested that crest sharpness in tidal flow conditions was also relevant to investigate. See comment above, to add to aim. P.S. Having read the paper: sharp and rounded crests are created by the position of ‘the steep portion’ (max lee slope segment); this became more clear when reading the results and discussion, but not so much in section 2.
  + Line 123: no intermittend flow separation can be investigated. See Main point #2.
  + The 5-30 degrees slopes with increments of 5 degrees will appear to be insufficient to find the angles bounding the dune classes in the discussion. See Main point #2.
* Results
  + Outstanding figures in the results sections and well described in the results texts (sections 3.2, 3.3 and 3.4; which are also well structured).
* Discussion: relevant topics in headings, however:
  + **Main point #2:** The bounding lee-slope angles for the classes of dunes here proposed (lines 274-276), do not follow from the results presented in this paper. Please consider the following line of thought:
    - Firstly, the results show for 5 and 10°-angle dunes (Figs 4 and 5): no (nearly no) flow separation, relatively long shear layer, TKE and length wake dependent on max slope position (lee shape). The 15° dunes (Fig 6): most discriminating observation is that flow separation, shear layer, TKE and wake length are not (hardly) correlated to lee shape (max slope position, the configurations). The 20° dunes (Fig 7) show clear effects of high-angle dunes. Thus, based on these distinctions (results): the 10° dunes would (still) fall in the class low-angle dunes (≤10°), and the 20° dunes would (already) fall in the high-angle dunes (≥20°). The intermediate dunes would fall in the approximate range >10° to <20°. This would better follow the results in this paper. The distinction into the three classes thus seems based more on previous findings in the literature than on results presented in this paper. Moreover, the classification was already proposed (i.e. defined) early in the paper. And does not differ from an existing classification in the - not so useful - explicit/implicit reasoning.
    - However, strictly, this paper presents av. slope angles “between 5 and 30°, in increments of 5°” (5, 10, 15° with max angles of 20° and av angle of 20° and max of 30°), but what the bounding av. lee-slope angles are, cannot be concluded from the presented results (Figures 4 – 7). The classification proposed in this paper thus needs to search for the angles that bound the 3 classes (11 – 15° and 15-19°), like how in mapping one would have to search for the boundaries. Even though the discussion states: this would not be an exact angle (lines 307-308), the proposal of this classification is a main point of the paper, and cannot be done without searching for the bounding angles.
    - Leaving the 17° angle (early in the paper) and replacing by 20°: does that mean that if we go back to Fig. 3 that the red circles of ~17° would then be green crosses? Why would this be a more logical distinction?
    - Line 216: difficult to find distinctive trends for intermediate-angle dunes. Is this a consequence of not being able to model intermittent flow separation? (lines 122-123) I.e. in the literature the specific flow for intermediate-angle dunes. Please discuss in the discussion what the exact (or expected) consequences are for not being able to model intermittent flow separation. For example, would the independency of max lee slope position for most flow parameters for the 15° dunes be a direct consequence, and if so why? Lines 306-307 say: needs more research, but that is not sufficient here, for proposing a classification.
  + A question that arose from Figure 6 for the 15° dune results: flow separation is absent in Config1 (whereas it is present in all other configs): why?
  + **Figure 9** will benefit in conceptual power with a more schematic visualisation (not mimicking results figures 4 – 7), preferably without needing the text to explain.
    - Do you really need the 2 configurations? Firstly, differences are not very clear from the figure, and secondly, this could be differently visualised, for example by indicating the max slope location of flow parameters (e.g. red dot near crest or trough for shape-dependent parameter; absence of dot means no dependence). Or something alike.
  + The paragraph on continental shelves (lines 327-337) is pure speculation (more or less a repetition of the introduction). This could bworked out adequately, but requires a fuller study of the literature. Here, therefore, I would suggest to not discuss this in the discussion, but formulate the application to continental shelves as wider implication of this work. That would turn this paragraph (weakness of the paper) into a strong point.
  + With section 4.3: bed roughness: in this paper, relative lengths of wakes were used. To what extent the wakes extend into the flow (water column; exceeding dune height) is not mentioned. Would this not add to the discussion on bedform roughness?
  + Section 4.4: Lines 378-384 is a repetition of the results. Why not relate to the critical shear stress for incipient motion, as indicated in Fig. 8 (see earlier comment in .pdf that slope effect should be taken into account for critical shear stress?). Also, interesting in discussing the consequences for sediment transport in this section (4.4) would be:
    - does the detachment of the shear layer in higher angle cases lead to more suspended sediment (locally) i.s.o. bedload?
    - the line of critical shear stress (Fig 8) implies that sediment will only be mobilised at the crests of dunes. However, empirical studies show that large parts of stoss sides are being eroded. Is this a consequence of the choice of initial dune shape in the simulation runs?
* Conclusions fit the current line of thought in the current text. With adjusting the proposed classification, the conclusions would have to be adjusted as well.
* Ref list:
  + See additional literature suggested in the comments in \*\_Rev.pdf
  + To the Kostaschuk & Vendetti 2019 paper, a comment was written (2020, Cisneros is one of the co-authors). Would it be wise to also use the comment?

**Reviewer’s recommendation to Editor**

I recommend publishing in ESD with revisions, taking into account the comments and textual suggestions/corrections. Main point #2 requires extra runs (in search of class-bounding angles) and is expected to lead to modification of the proposed classification. (This would make it major revisions.) Despite the comments on the discussion section, the paper is a valuable addition to knowledge of the influence on flow by bedforms.