Response to Reviewer 3's comments

This paper conducts a stability analysis to explicitly include sediment suspension in order to determine the role of suspension on the suppression of bed topography. Their analysis shows that the presence of suspended load is a controlling factor on upper plane bed stability with implications for the understanding of hydrodynamics of deposits such as those formed by turbidites and other high suspended sediment concentration flows. The model framework was well described and understandable. However I do agree with most comments posed by Reviewers 1 and 2, particularly the contextualization and success criterion questions brought up by reviewer 2 and the appropriateness of using the dominant wavenumber to define the formation of plane bed.

Thank you for the comment. As we replied to Reviewers 1 and 2, we will consider to describe the diagram using the growth rate and to show the error rate.

There are a few additional contextualization issues to clarify and put the work into the broader picture should those prior issues be adequately addressed. First, in the abstract, there are a number of sentences that seem repetitive, and the mechanistic component of the role that suspension plays is not described. Again in the introduction, the hypothesized role of suspension is not mechanistically discussed. In the discussion, there is brief allusion to the fact that this analysis demonstrates that turbulent suppression for example is not required, but I think the exact mechanism by which the presence of suspended load is not fully described in the written work.

Thank you for the comment. We will describe the mechanistic component in the abstract and the introduction. Also, we will not state that the turbulent suppression is not required, but it may contribute to the deformation of dunes, and thus the model can be improved by the inclusion of such effect in the future studies.

I should note that I don't necessarily agree with the dimensional arguments made by Reviewer 1 - while it is certainly common practice and more relevant in predictive modeling to non-dimensionalize, the framing of the arguments in this paper does not necessarily require it in my opinion.

Thank you for the comment. As you commented, it is common practice to use dimensionless parameters. Therefore, it will be changed to the D/H values in the revised manuscript.