Response to Reviewer 2's comments

In this manuscript, the authors use linear stability analysis to show that suspended sediment load could promote the stability of plane beds for open-channel flows with fine bed-material sediment. They propose that this mechanism could explain the observations of parallel laminations in turbidites, which typically lack dune-scale cross stratification. The authors also use observational data to test their hypothesis. Overall, the manuscript is reasonably well-written; however, the writing and presentation still needs a lot of work to clarify the results and avoid repetition. Importantly, I found that this manuscript needs significant amount of work to clarify several aspects of the analysis before being ready for publication.

1. The definition of a plane bed in terms of dominant wave number seems rather confusing to me. By definition, a plane bed is not a bed form that has a large wavelength. So, defining the plane bed this way and then using linear stability analysis to find parameter space that correspond to a small dominant wave number seems odd to me. At least, there is no justification given for why this should correspond to a strict definition of a plane bed. This is a major point as this assumption is the foundation for the entire manuscript.

Thank you for the comment. In this paper, the plane bed is defined as the bed state where the growth rate of the bed perturbation is negative for all wave numbers. Thus, the plane bed is supposed to be a completely flat condition in theoretical analysis. We will revise the manuscript to clarify this.

2. The limits on the parameter space explored here needs justification. For example, in lines 73-77, the authors describe the range of particle sizes and flow depths explored but also state that they set the grain size to 3 values and flow depths to 3 values. How is it that the data could not be recast into only dimensionless terms without the need for using a mix of dimensional and dimensionless variables?

Thank you for the comment. We already employed the particle Reynolds number in Figures 2 and 3, whereas the figure legend remained to indicate the dimensional value. We initially considered that the dimensional values of the flow depth could be helpful to understand the result intuitively in Figures 4 and 5, but it will be changed to the D/H values in the revised manuscript.

3. The authors need to give more detail about the observational data that is used to support their hypothesis. How are data from a range of grain sizes and

flow depths collated to plot on stability diagrams with a single value of grain size, for example? What is the sensitivity of these stability diagrams to the parameters?

Thank you for the comment. We show the range of the particle diameter of the observed data plotted in the diagram in Lines 253–254. Also, we will check the sensitivity of the diagram when we change the range of the Re_p.

4. What is the criterion for the success of the model? It appears from the results that a majority of the observations plotting in the stable region of the contour maps is enough to state that the model works. There is no discussion of how many points do not plot in the stable region and what it means for the model veracity. I think the authors need to lay out the metrics they will use to test the success of the model and then discuss how the field and flume data compare with this test. Right now, the entire model testing part of the manuscript is weak and arbitrary.

Thank you for the comment. We will show the error rate which denotes the ratio of the number of plane bed data plotted on the unstable region to the whole number of plane bed data.

5. The figures need some more explanation. It is not clear to the reader where each of these data points should lie in terms of model expectations? For example, I would expect that if larger fraction of actual plane bed data lining up with the stable region in the contour plots would be a model success but I don't see a lot of observational data matching up with stable regions on the contour plots. If I am mistaken about my interpretation here, then the authors need to do a better job of explaining the metrics for success of their model.

Thank you for the comments. As you stated, we interpreted that the model with suspension works because a majority of the plane bed data plotted in the stable region of the contour maps. We will show the error rate to state that more data plotted in the stable region for the case with suspension compared to the case for the model without suspension.