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. I summarize my major comments below:

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.

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?

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?
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.

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.