Comments on "Linear stability analysis of plane beds under flows with suspended load" by Koji Ohata et al.

1 General comments

In their manuscript entitled "Linear stability analysis of plane beds under flows with suspended load", Koji Ohata and collaborators show how dune development in underwater environments can be suppressed by adding suspended load to the transported sediment. For this purpose, they use a stability analysis of a plane sediment bed at the linear order, coupling the theoretical framework of hydrodynamics and sediment transport, and compare its results to experimental/natural data.

The issue tackled here is important for bedforms emergence, in natural environments but also in the lab, where the study of some types of currents may require to avoid bedforms formation. I can therefore imagine the utility of having a parameter diagram showing where bedforms arise, and where they do not. The linear stability approach may therefore be well suited for the establishment of such diagrams. It is especially interesting considering the study of Naqshband et al. (2016), where the authors highlights a transition from dune to plane bed at longer time scales, clearly involving non-linear processes.

Overall, I therefore think that these results deserve publication, especially considering the convincing comparison with experimental/natural data, the but not in the present form. I indeed have some concerns/remarks about the chosen sediment transport model, as well as technical considerations. Note that my comments are suggestions only, and I leave to the authors and the associate editor the final choices concerning modifications/acceptance of this manuscript.

2 Specific comments

2.1 Sediment transport model

My main concern about the sediment transport model relate to the absence of a spatial lag, needed for the sediment flux to adjust to a change of flow condition (i.e basal shear stress). This relaxation length, also called the saturation length, has been shown to be the relevant length scale for small-scale bedforms emergence underwater, and should be, in my opinion included in a stability analysis which aims to look at bedforms emergence suppression.

Furthermore, while this length is small (mm to cm) for bedload transport, and could, under some assumptions that should be clearly stated, neglected, it largely increases in the suspended load dominant transport regime (several meters).

I therefore urges the author to consider including this relaxation length in their sediment transport model, or strongly comment and discuss their choice of leaving it aside. A lot of useful references and material can be found in Naqshband et al. (2016) under the section *Sediment transport module*, as well as in the supplementary material of Vinent et al. (2019).

This is particularly important as this spatial lag is discussed in section 4.1 as the mechanism inducing the transition from dunes to plane bed. This suggests that this is also the reason of the increase of the stable region of the stability diagram in your study while, to me, **this effect is not present in your model**.

2.2 Choice and definition of relevant parameters/quantities

I find the definitions and choices of the parameters/quantities used for the theoretical derivation and the final diagrams unclear, for several reasons:

- This is discussed at the beginning of the Methods section, as well as in in section 2.3. It results in things said twice. For example, why the wavenumber can not be used in any axis of the diagrams is explained in lines 298-302 and in lines 53-55. I feel like these two sentences should be grouped, and more generally that the discussion of section 2.3 should come before the derivation of the equations (or at least part of it).
- While I understand why the "classical" space Froude – non-dimensional wavenumber can not be used here, I do not think that using a dimensional quantity for a regime diagram is satisfying. From what I have understood of the technical derivation of the equations, it feels like the remaining non-dimensional parameter of the problem could be $\tilde{D}/\tilde{h_0}$. However, lines 285 highlights that the growth rate is a also a function of the Particle Reynolds number, as well as the friction coefficient. These two parameters are not discussed later, while in the literature, diagrams for dunes/ripples/bedforms emergence have been used in the $(Fr,\, R_p)$-space (see (Vinent et al., 2019) for example). Why don't you use this parameter space instead ? (i.e, it is not trivial for me to got from eq. 101 to eq. 104).
- The authors should clarify the use of non-dimensional and dimensional quantities. This definition arrives only in line 85, while a lot of quantities with and without $\sim$ are used before. It may be useful to clarify at the very beginning that $\tilde{X}$ are dimensional quantities and X without $\sim$ are not.

2.3 Technical derivation

- My main comment concerning the first part, describing the theoretical framework, adresses the use of numbers within literal equations (eq. 41, eq. 55, eq. 60 and others) which are coefficients assumed as constants and calibrated in other studies. For sake of generality, I feel like they should be replaced with symbols, and then given a number when citing the study that calibrate these constants.
- The use of subsections in the formulation of the problem could greatly increase the readability of this section, separating the hydrodynamics, the sediment transport model, the base state etc ..
- I think that part of the technical resolution of the linear stability analysis 2.1.2 might fit better in an appendix, rather in the text itself (especially from line 240 to 283), but it is a personal opinion.

  However, as the authors assume that the hydrodynamics adjust instantaneously to the bed evolution, and do no explicit any feedback of the sediment transport on the hydrodynamics, I suggest to solve to hydrodynamics separately from the sediment transport model, as presented in (Fourriere et al., 2010). This comment is more a suggestion for later studies.

3 Other comments/technical corrections

- line 66-67: Fourriere et al. (2010) have tested (present in the supplementary material) the effect of a moving (growing/propagating) bottom on the resolution of a turbulent flow on a sinusoidal bottom. They should be cited here.
- line 90: D is a non dimensional parameter. While this is explained by eq. 10, it is misleading as the formulation is the same for the neighboring dimensional quantities (water density, etc ..). See my general comment above.
- line 104: Why can you assume this ? The characteristic times scales should be discussed somewhere.
- line 109-110: Is it always true that the diffusion coefficient of suspended sediments is equal to the turbulent viscosity ? It would be nice to discuss when this assumption is correct or not, and add some references.
- line 116: Why don't you take the coefficients for natural grains instead of smooth spheres ?
- line 171: I think this equation could be simplified by the sole use of a hydrodynamic roughness, later set to a fraction of the grain size. This would remove the use of unused quantities like m or the 8.5 number inside the equation:

$$\frac{u}{u_*} = \frac{1}{\kappa} \ln(z/z_0), \tag{1}$$

  with $z_0 \simeq d/12$. Note that there should be references to argue on the choice of the hydrodynamic roughness, which has been shown to be modified by the presence of sediment transport.
- lines 303-304: this requires some comments. It does not look trivial to me.
- lines 314: By doing this, you change the particle Reynolds number of an order of magnitude, and thus maybe the state of the sublayer close to the sediment bed (viscous or fully turbulent, i.e smooth vs rough turbulent flow regimes). It has been shown that this is an important number for bedforms emergence (Vinent et al., 2019), and I think you should comment on that.
- lines 316: There is many of evidences in the literature of underwater bedforms with wavelengths smaller to much smaller than the flow depth. We may need a comment on why you do not expect them from your model, thus choosing this range for the wavelengths/wavenumbers.
- lines 334: it would be nice te recall the reader what are the physical reasons inducing the stable region, even is the case of bedload transport only. Note that this could also be done when presenting figure 1.

References

Fourriere, A., Claudin, P., and Andreotti, B. (2010). Bedforms in a turbulent stream: formation of ripples by primary linear instability and of dunes by nonlinear pattern coarsening. *Journal of Fluid Mechanics*, 649:287–328. 2, 3

Naqshband, S., van Duin, O., Ribberink, J., and Hulscher, S. (2016). Modeling river dune development and dune transition to upper stage plane bed. *Earth surface processes and landforms*, 41(3):323–335. 1

Vinent, O. D., Andreotti, B., Claudin, P., and Winter, C. (2019). A unified model of ripples and dunes in water and planetary environments. *Nature Geoscience*, 12(5):345–350. 1, 2, 3