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.

We thank the reviewer for pointing out the significant aspect. Indeed, our model considers the spatial lag in suspended load transport. The model in this model describes the temporal development by fluxes of entrainment and settling of sediment.
The transport rate of the suspended sediment requires more time and distance to respond to the changing flow conditions. It has been suggested that the delayed response of sediment transport rate plays a significant role in determining the stability conditions of bedforms. This effect is implicitly considered in the sediment transport model employed in this study. Let $\tilde{U}$ and $\bar{C}$ denote the depth-averaged flow velocity and suspended sediment concentration. The sediment transport model in our study can be expressed as:

$$\frac{\partial \tilde{U}\bar{C}\tilde{h}}{\partial \tilde{x}} = \tilde{w}_s \left(e_s - r_0 \bar{C} \right) \tag{1}$$

where r_0 is a ratio of the near-bed sediment concentration c_b to the depth-averaged concentration C. The right-hand side of the equation 1 describes fluxes of entrainment and settling of sediment. Here we define the rate of suspended load $\tilde{q_s}$ and its equilibrium rate $\tilde{q_e}$, which take the forms:

$$\tilde{q_s} = \tilde{U}\bar{C}\tilde{h} \tag{2}$$

$$\tilde{q_e} = \frac{\tilde{U}\tilde{h}e_s}{r_0} \tag{3}$$

Using the equations 2 and 3, the equation 1 can be recast as:

$$\frac{\partial \tilde{q_s}}{\partial \tilde{x}} = \frac{\tilde{q_e} - \tilde{q_s}}{\tilde{L}_{sat}} \tag{4}$$

where $\tilde{L_{sat}}$ is defined as:

$$\tilde{L_{sat}} = \frac{\tilde{U}\tilde{h}}{\tilde{w_s} r_0} \tag{5}$$

This parameter $\tilde{L_{sat}}$ describes the saturation length to reach the equilibrium state of the suspended load in response to the flow condition. Regarding the saturation length of the suspension, Claudin et al. (2011) proposed the following equation:

$$L_{sat} = \frac{u_*}{w_s} \exp\left(-\alpha \frac{w_s}{u_*} \right) \frac{C}{\sqrt{g}} h = \frac{Uh}{w_s r_e} \tag{6}$$

where $r_e = \exp(-\alpha w_s/u_*)$ denotes a ratio of the near-bed sediment concentration to the depth-averaged concentration in the equilibrium state (α = 2.0 in their study). Thus, our formulation of the suspended sediment transport is nearly identical to that of the previous study, while the near-bed sediment concentration is dynamically computed in this study.

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).

Thank you for the comment. We moved the section 2.2 before the section 2.1 in the previous version, and merged the explanation about the parametric space.

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 $\tilde{D}/h_0$. 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, Rp)-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).

We modified the regime diagram to employ the dimensionless parameter $\tilde{D}/h_0$. In addition, we add the diagram for the (Fr, Rp) space.

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 tilde are used before. It may be useful to clarify at the very beginning that $\tilde{X}$ are dimensional quantities and X without ˜ are not.

Thank you for the comment. We moved the description about the use of the symbol ˜ for distinguishing non-dimensional and dimensional quantities to the beginning of the section 2.1.

2.3 Technical derivation

My main comment concerning the first part, describing the theoretical framework, addresses 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.

Thank you for the comment. We replaced the numbers in eq. 41, 55, and 61 with symbols.
Also, we correct eqs. 19 and 20 as follows:

$$F_x = uc - \nu_T \frac{\partial c}{\partial x} \tag{7}$$

$$F_z = (w - w_\mathrm{s})c - \nu_T \frac{\partial c}{\partial z} \tag{8}$$

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 ..

Thank you for the comment. We use the subsections in the formulation of the problem in the Method section.

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.

Thank you for the comment. We moved the part of the technical resolution of the linear stability analysis to the appendix. Although the approach used in Fourriere et al. (2010) effectively described the formation of ripples using the linear stability analysis, we could not employ their approach because the depositional/erosional rates in our model are coupled with the vertical concentration profile of suspended load.

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.

Thank you for the suggestion. We cited their work 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.

Thank you for the comment. We revised Lines 88–89 to explicitly explain that D is an non-dimensional parameter as follows : where D is the non-dimensional diameter of a bed particle, $\tilde{u}_{\mathrm{f0}}$ denotes the shear velocity in the basic flat-bed state, and $\tilde{\rho}$ is the water density ($= 1000\ \mathrm{kg/m^3}$).

line 104: Why can you assume this ? The characteristic times scales should be discussed somewhere.

The migration celerity of bed waves is sufficiently small compared to the flow velocity. Therefore, it is possible to assume that time variation of the suspended sediment concentration can be neglected in the dispersion/diffusion equation of suspended sediment as discussed in previous studies (Fredsøe, 1981; Sun and Parker, 2005). This approximation can be justified in our paper as well.

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.

The assumption on the diffusion coefficient of suspended sediment is widely used in many studies. The classic study of Rouse (1939) employed this assumption to model the equilibrium profile of suspension, and his model has been proved to fit the natural and experimental observations (i.e. van Rijn, 1984). We cite their studies here.

line 116: Why don't you take the coefficients for natural grains instead of smooth spheres ?

Thank you for the comment. We use the coefficients for natural grains in the new calculation.

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:

$$\frac{u}{u_*} = \frac{1}{\kappa} \ln\left(\frac{z}{z_0}\right) \tag{9}$$

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.

We incorporate your suggestion, and added the references for the choice of the roughness coefficient (Colombini, 2004) as well as the mention about the modification of the roughness by the sediment transport (Dietrich and Whiting, 1990).

lines 303-304: this requires some comments. It does not look trivial to me.

Thank you for the comment. We added a description about the equations to clarify that $\omega = \omega(k, \mathrm{Fr}, C_\mathrm{z}, \mathrm{Re_p})$ can be rewritten as $\omega = \omega\left(k, \mathrm{Fr}, \tilde{D}, \tilde{h}_0\right)$ as follows:

Here, using $\mathrm{Re_p} = \mathrm{Re_p}(D) = \mathrm{Re_p}(\tilde{D}, \tilde{h}_0)$ (Eq. (24)) and $C_\mathrm{z} = C_\mathrm{z}(R_0) = C_\mathrm{z}(\tilde{D}, \tilde{h}_0)$ (Eq. (49)), we can rewrite Eq. (A30) as:

$$\omega = \omega\left(k, \mathrm{Fr}, \tilde{D}, \tilde{h}_0\right) \tag{10}$$

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.

It is true that the transition from the hydraulically smooth to rough boundaries affects the formation of ripples. However, our study focuses on the influence of the suspended load to the formation of dunes and plane beds. Therefore, we only explored the conditions of the rough boundary. The suspended load with the smooth boundary occurs only when the sediment particles are extremely fine grained (less than silt size), so that we did not include in the experimental conditions. We add this explanation in the text.

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.

As described above, we focus on the influence of suspended load to the transition from dunes to plane beds. Bedforms with wavelengths smaller to much smaller than the flow depth (i.e. ripples) are unlikely to be formed with active suspension, so we exclude it from our research subject.

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.

Thank your for the comment. We added a description as follows:
The contour maps of $\tilde{h}_0$ versus Fr show that the stable region, which denotes that hydraulic conditions where the plane beds appear, for fine sediments is larger in the diagram with suspension than in that without suspension (Fig. 2).