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Title: “Disentangling spring-neap SPM dynamics in estuaries”

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**Preliminary remark.** I collaborate closely with two authors (YMD and HMS), though not on this particular topic. Nonetheless my comments would make this probably clear. Hence I have unveiled my identity to the authors. Any discussion on this paper, however, has been and will be in writing only or will at least be made reproducible in some way.

**General comments.**

The authors extend an existing numerical tool (Brouwer et al. 2018) that describes long-term subtidal variations by discharge to include spring-neap behavior as well. This model is applied to two cases. First the authors consider an idealized estuary that is somewhat inspired on the Ems. In this case the backreaction of sediment on water motion ( “turbulent damping”) and non-linear effects like hindered settling are absent. The second case specifically describes the hyperturbid Loire estuary and does include turbulent damping and hindered settling.

Three (although maybe four) analysis tools are defined and adopted to enhance interpretation of the model results, concentrating on horizontal and vertical sediment dynamics as well identification of dominant processes that govern the horizontal sediment balance.

The model setup and solution method (Sect. 2.1-2.3) are well written. I also applaud the approach to identify the role of individual processes, as this is an inherent advantage of the adopted model approach.

However, I do think that a number of things need to be clarified. Among these are the nature of the quasi-stationary runs, in general the analysis methods as outlines in Sect. 2.4 and the way they are adopted when interpreting the model runs. Below, I will elaborate on these points.

I find this paper acceptable with minor revisions provided that these concerns are addressed.

**Nature of the quasi-stationary runs.**

The authors state (lines 190-191) that the quasi-static runs are done by setting the time derivative of stock S to zero in Eqs. (11)-(12). I was very confused by this as this precludes the possibility of mudpools being formed which do occur in the quasi-stationary state. Moreover from the initial condition S=0 (Sect. 2.3) no sediment stock is expected to form at all. This cannot be correct.

I have given it some thought and it seems to me that the authors actually use the instantaneous D2 and D4 forcing at each time step to compute the corresponding equilibrium sediment distribution (which indeed may contain erosion limited regions, hence mud pools).

To me, further explanation of what the quasi-stationary runs are and how they can lead to mud pool formation is crucial to understand the authors’ findings. As it is presented now, all discussion of 'mud pools', 'erosion limitation' and 'f=1' with reference to the quasi-stationary runs seem inconsistent to me.

**Analysis methods (Sect. 2.4) and their applications (Sects. 3.2 and 3.5)**

Here I lost count of the actual number of tools the authors use: they claim three (first on line 185, second on line 210 and third on line 220). However, the D2-tide averaged results (which give the horizontal transport capacity and individual physical mechanisms) on lines 192-209 seem to be a fourth one, which is now confusingly described under 'Next, .. '.

I would propose that the authors to make clear whether there are 3 of 4 analysis tools and (in the former case) motivate why lines 185-209 are one tool. Use additional section numbering (2.4.1 to 2.4.3 / 2.4.4) to introduce each analysis tool in a slightly more structural way. I would also recommend to have their analysis sections (3.2 and 3.5) discussed in a similar way (i.e. as subsubsections). Sections 3.2 and 3.5 really contain a lot of information, linking the analysis explicitly to the tools (subsections) in Sect. 2.4 would be most helpful.

I think the authors may have to slightly rewrite the abstract as well to keep consistent with Sect. 2.4. For instance, sediment capacity is mention as an analysis tool while this is not mentioned as such in Sect. 2.4. Indeed, in Sect. 2.4 a comparison between quasi-stationary and dynamic runs is presented as an analysis tool (but not in the abstract).

I think this will provide a more convenient guide for the reader who is often not familiar with the authors' approach and has to absorb quite an amount of information along the way.

**Further remarks**

1. Line 20: "This leads" → "This *may* lead". To me trapping, as characterized by e.g. ETMs, does not necessarily imply erosion limited conditions.
2. Line 77: "in over" → "over" (typo)
3. Line 195-196: "were added on the bed", perhaps extend to "were added on the bed, i.e. global erosion limited conditions (f=1)." To me, this would clarify the definition od sediment transport capacity as being the maximum sediment transport that is possible under local hydrodynamic conditions.
4. Line 262-263: "For alle model settings ... refer to DgD24". I would give the authors the consideration to include the settings in an Appendix. After all, DGD24 has not yet been accepted yet...
5. Line 400-402: The downward zero-crossing at 8 km is not well visible during neap tide.
6. Line 402-403: "This helps to explain the quite minor differences between spring and neap ... noted earlier.". I think this should be minor *qualitative* differences as the magnitude of the concentration varies by a factor two.
7. Line 403-405: To what extend can hindered settling also contribute to sediments being kept higher in the water column and thus contribute to the increased sediment capacity?
8. Line 418-419: "Erodibility is nevertheless ... and strong stratification." To me, this may be seen as mimicking a situation of low erosion parameter M which indeed corresponds to a greater likelihood of erosion limitation (see Fig. 5).
9. Line 420-421: "..., where the quasi-stationary case shows flushing of the bottom pool. In the dynamic case, the bottom pool remains present". I agree that the bottom pool is

flushed in the quasi-stationary case. However, as it it written here I interpret the remark about the dynamic case as that the bottom pool is *always* present which is not correct. Indeed, f=1 occurs only after neap at the downstream located ETM. Please explain that there is a permanent mud pool or restate this remark.

10. Line 420-423: I agree that sediment is transported to the bottom pool at the entrance at neap. I also agree that at spring there is transport towards the ETMs at 40 km, but I don't think there is a bottom pool there (f<1 at spring, see previous remark). I think that 'trapping areas' is more appropriate here. Besides, it also seems that there is accumulation of sediment at the 40 km ETM after neap.
11. Line 423: "..., sediment is transported between the two bottom pools between spring and neap". First, I only think there is one bottom pool (see previous remarks above). Second, I found this not so clear from what the authors wrote. I think that sediment is being transported from the downstream to the upstream ETM at 0.5<t/T<neap (blue region) and visa versa for 0<t/T<0.35 (red region). Is this what the authors want to convey? I would think that this back and forth transport is necessarily a recirculation (both net transports being equal) since the authors consider a situation that is equilibrium on the neap-spring timescale. Could the authors comment on this?