Manuscript ref: egusphere-2025-5706

Title: Vertical and temporal mud dynamics during spring-neap tidal cycles

Summary and recommendation:

The manuscript reports on in-situ data of fluid mud dynamics in the Ems Estuary on two different time scales: semi-diurnal tidal and fortnightly cycle. Two different data sets were analyzed: one of a 6 week-mooring period collected in 2019, and another one set of four times height hours profiling period collected in 2023. The datasets include measurements of velocity, turbidity, salinity and dissolved oxygen. Based on their observations, authors concluded that spring tides favor persistent and stable fluid mud layers, while neap tides enhance turbulence reducing the stability of the fluid mud layers due to a smaller hydrodynamic cross-section.

I am usually really interested by novel measurements of fluid mud dynamics in hyper-turbid estuaries and I am pleased to see that the authors have attempted to do so. Such studies could be of a great interest for the scientific community. However, I have major reservations about the data post-treatment and quality check, the interpretation of the results and generalization of the findings. I would not recommend the present manuscript for publication at this stage. A new submission should be encouraged after major revisions.

Major comments:

1) Scientific significance:

The scientific gap that justifies the present study is not clearly identified in the introduction. What is the main research question? What is the difference with previous studies focusing on fluid mud dynamics? The novelty of this study is not clear.

Moreover, the findings are presented without broaden significance. The contribution of the present study to global knowledge on fluid mud dynamics is missing.

Thank you for this comment and raising these questions. Our research is driven to increase temporal and vertical resolution of measurements to improve our understanding of fluid mud dynamics. We do this utilizing in situ density measurements and long-term mooring data with higher vertical resolution than existing monitoring stations. We rewrote the introduction to state novelty and research aim more clearly

2) Scientific quality:

The calibration of the instruments especially the turbidity sensors are not presented not even mentioned. No water sample from the field site is mentioned. Therefore, the question arises on what was the calibration procedure to convert turbidity values to SSC. Such conversion is highly dependent on the suspended sediment nature and should be done with field site water samples. Moreover, most of the turbidity sensors are not able to measure properly under concentration of several tens of grams per liter with natural sediments. The authors indicates that TSS probe has measured concentration up to 500g/L. This needs to be clarified.

Thank you for pointing this out. We now add a correlation of density measurements and SSC data from direct fluid mud samples. The TSS probe used in this study is similar to the monitoring stations used by the NLWKWN and reliably measure TSS in hyper-turbid estuaries, as they align well with density measurements during our campaigns.

For the velocity data, we have no information on the measurements (continuous sampling or burst, acoustic frequency, 2 or 3 velocity components...) and the post-treatment procedure (especially the quality control and filtering processes). Regarding the quality control, I am very surprised that authors were able to measure flow velocity under such high turbidity conditions (hundreds of grams per liter). Acoustic Doppler current meter are usually not able to make reliable measurements in more than a few tens of grams per liter. That's why we are usually using electromagnetic technology to complement the measurements close to the bed (e.g. Becker et al, 2018). In addition, the current meters were placed on profiling/mooring lines and were sampling at 20Hz. Movements of profiling/mooring lines and vibrations from the motor of the ship should have been discarded from the data set with filtering processes. The full post-treatment procedure should be given.

Thank you for this comment, we added direction as this is recorded by the current meter and give streamwise velocity accordingly. We added a paragraph regarding data acquisition with the current meter used in this study.

Finally, I'm not sure to understand what "velocity" means: is it the magnitude of the velocity? If yes, could you provide the direction. Or is it the along channel velocity component? If yes, could you provide the across channel velocity component? Usually the velocity is projected on a local coordinate referential (with along channel velocity and lateral velocity component).

See comment above, we use direction to calculate streamwise velocity, thus along channel velocity is given in the revised version for the 2019 and 2023 data set.

For the multi parameter probe, the acquisition frequency is very low and the profiling speed very high, it results in a very poor vertical resolution (10-20 points per profile). We usually used multi-parameter probes at 2-4Hz for vertical profiles or we strongly reduce the profiling speed. This should be clarified.

The acquisition frequency is given by the multi-parameter probe, which has a minimum interval of 0.5Hz. 10-20 points relate to vertical spacing of 20-30cm for most vertical profiles, thus velocity profiles are sufficiently resolved in the water layer. Measurements in the fluid mud layer are treated with caution. Thus, we added a new paragraph regarding the limitations of our measurements.
The profiling speed can only be adjusted during times with low current velocity, otherwise we observe extreme drifts in the profiling line and strong scatter in the density measurements. Thus, we acquire high quality density measurements and velocity profiles with 20-30cm spacing in a single profiling line.

3) Interpretation of the results:

One of the major flaw in the interpretation of the results is that the mud dynamics is only considered along the vertical. However, fluid mud patches in estuaries are advected by

the tidal current and the river flow, they moves back and forth along the estuarine channel. Along channel mechanisms such as advection should be considered and discussed when interpreting the results.

We agree with this comment and revised accordingly, as we focused to close on vertical dynamics alone. Thus we added a new figure regarding along channel dynamics.
However, along channel dynamics, as well as back and forth moving mud patches can not be resolved by simple point measurements as are available for the majority of the Ems estuary and would require an entirely different monitoring approach. We are not aware of a dataset that exists in sufficient vertical, lateral, and temporal resolution, thus along channel dynamics underlie strong assumptions and high uncertainties as well.

In addition, strong assumptions have been made that does not match the observations. In particular, the assumption that considers that the fluid mud does not contribute to the flow, while Fig.4 demonstrates the opposite. However, this could be linked to the fact that the velocity measured in the fluid mud layer is not reliable (see previous comment).

We agree and added a paragraph regarding limitations of our measurements

In addition, the luthocline is usually defined as the location where SSC gradient is maximum and not 10g/L.

We agree that the lutocline is defined where the SSC gradient is at a maximum, our measurements rely on in situ density measurements, thus technically we define a pycnocline, however the term lutocline is used commonly in literature and Table 1 shows that the lutocline is very often defined at 10g/L.

Finally, the salinity and dissolved oxygen data set remains underutilized.

We rewrote large parts of section 4.1 and 4.2 and put more emphasis on dissolved oxygen data.

4) Presentation quality:

In the section “Results and discussion”, please introduce each figure before discussing the results. In addition, numerous citations are not appropriate in text. Please check your references.

In the interpretation of the results section, the reasoning is sometimes very difficult to follow and the information is often imprecise.

Thank you for pointing this out, we rewrote large parts of the results/discussion section with focus on clarity.

5) Discussion:

In order to broaden the significance of the results, comparison with other study sites should be given. Is the Ems Estuary an exception or similar results have been observed elsewhere? If so, why?

We agree and rewrote large parts of the discussion and conclusion to address this.

In addition, the limitations of the study should be acknowledged and discussed. For example, the fact that profile measurement campaigns do not cover the entire tidal cycle but only 8h, should be discussed and considered when interpreting the results.

We agree and added a new paragraph regarding the limitations of our measurements more thoroughly. We also mentioned why measuring >8h is challenging due to ship availability.

Other comments:

L.31. References are normally ordered alphabetically or by date of publication. This comment applies to the whole manuscript.

Wang et al. (2020) is not an appropriated reference here.

References to other study sites than the Ems Estuary would underline the fact that hyper-turbidity occurs in estuaries worldwide.

"we added appropriate references accordingly and removed Wang et al. (2020)"

L.32. High turbidity and Estuarine Turbidity Maxima are not caused by storms events or construction. Please clarify.

"thank you for this comment. However, we partially disagree – construction works such as fairway deepening, leading to tidal asymmetry and tidal pumping are a well-known reason as driver for hyper turbidity in the Ems estuary. We rephrased for clarity"

L.33. Reference is not appropriate.

"we disagree, the authors clearly state the impact of engineering measures leading to accumulation of fluid mud in the Lower Ems"

L.36. Reference Zhou et al. (2019) is not given in the reference list.

"thank you for pointing this out, we added Zhou et al. (2020) in the reference list"

Table.1. The table might be moved to supplementary material.

"thank you for this suggestion, in accordance with Reviewer 1 we removed the Table and put more emphasis on our own definition of fluid mud"

L.98. What is the novelty of your study? What is the research question here?

"Thank you for raising this question, we rephrased for clarity"

L.100. Are you investigating the effects on fluid mud on navigation and ecological functioning in this study?

"Thank you for pointing this out, we rephrased for clarity and removed the focus on ecological functioning accordingly"

L.101-102. The importance of permanent monitoring system in hyper-turbid estuaries is already acknowledged for decades and several European estuaries are already equipped with permanent monitoring systems for years.

"We are aware of permanent monitoring systems in the Ems estuary, however, existing monitoring systems mainly consist of 1-2 sensors. No monitoring system that we know of is equipped with a vertical resolution of 3 sensors measuring continuously with high temporal resolution"

L.121. Please introduce acronym when used for the first time.

"acronyms LWS and HWS where introduced in line 72 and 75, respectively"

L.124. "dry periods along with low upstream river discharge". Strange wording: dry periods=low river discharge.

"thank you for pointing this out. We rephrased for clarity."

L.130. The ETM is where the turbidity is maximum, not where the strongest salinity gradient is reported. Please clarify.

"We agree with this statement, however, ETM and strongest salinity gradient often coincide. We rephrased for clarity"

L.134-138. This should be moved to Section3.

"We moved the part accordingly"

L.150. Exact dates of measurements should be given, as well as the forcing conditions (tidal range, river discharge...)

"We agree and added forcing conditions for each measurement campaign in new Tables 1 and 2"

L.151. 8h/day only covers 2/3 of the tidal cycle. This could be justified and you should discuss the influence of missing 1/3 of the tidal cycle on your interpretation of the results. How did you choose the 8hrs to be sampled? Why around the LWS? And not the HWS?

"Thank you for pointing this out, we added justification – 8 h measuring time leads to an overall workload of > 10 h per day, thus we prioritize LWS as we focus on the entrainment and formation of the fluid mud layer, thereby, it is crucial to measure around LWS and capture the early flood, as it yields the highest current velocities"

L.154. Why a single point current meter? Why not a velocity profiler? What is the acoustic frequency of the instrument? Did you do continuous sampling? How did you filter out the vibration of the profiling line, the ship motion and motor vibration?

"Thank you for raising these questions. We use a single point current meter, as it is part of a multi-parameter probe – this is the same multi-parameter probe that was used for the 2019 measurements, thus results should be comparable. A velocity profiler, would lack dissolved oxygen, salinity, and electrical conductivity sensors.
The acoustic frequency is 1.9 to 2.0 MHz. We added specifications to the text. We did not filter out vibration of the profiling line, the ship motor is turned off during measurements, as the ship is anchored between 2 dolphins. Furthermore, the Aanderaa instrument's inherent compass is tilt-compensating and ZPulse multi-ping averaging leads to low-variance data"

L.159. 10-20 points per vertical profile is insufficient. Water depth at the sampling site should be given.

"We agree with the reviewer that vertical resolution can be improved, however using a multi-parameter probe, higher resolution is simply not possible, as 0.5Hz is the maximum recording setting. Water levels, are added in the section regarding forcing"

L.162. "Vertical profiles were analyzed based on in situ measurements" Please clarify.

"Thank you for this question, the sentence reads 'in situ density measurements', we use a tuning fork density probe to measure density along with the multi-parameter probe"

L.164. Low water slack is supposed to be time of minimum velocity, not maximum velocity.

"We agree with this comment, the text does not state that low water slack coincides with maximum velocity"

L.168. Why do you consider the luthocline being where SSC>10g/L? Why not looking for the maximum of SSC gradient?

"Thank you for pointing this out, we rephrased our definition of fluid mud for more clarity. We use 10g/L as lower threshold, as this is the most common threshold used in existing literature and relate it to our direct density measurements based on our density-SSC correlation"

L.170. This is a strong assumption which seems not in line with your own observations. For example, in Fig.4b current velocity in the fluid mud layer seems to be up to 1m/s. Therefore, based on your data set this assumption does not stand. Please justify.

"Thank you for this comment, we agree that our measurements show high values in Fig 4, thus we calculated streamwise velocity and adjusted the plot."

L.174. Again this is a strong assumption and should be justified. How is your cross section? Rectangular? Or with shallower banks?

"Thank you for pointing this out, we agree that the 1D approach is an oversimplification and removed it as suggested by Reviewer 1"

Eq.1. The Richardson Number is generally expressed as $Ri = -(g/\rho_0).(\partial\rho/\partial z)/(\partial u/\partial z)^2$

Your way to express Ri seems to indicate that you have interpolated ρ and u on the same depth profile. If so, this should be mentioned.

"Equations are adjusted accordingly to comments made by Reviewer 1"

L.184. "time-averaged velocities" Tidally averaged? Phase averaged? Please clarify over which period of time the averaged is made.

"***We clarified the sentence"***

L.187. The Richardson Number is usually expressed as the squared ratio of the buoyancy frequency and the velocity shear: $Ri = N^2/S^2$, with $S=\partial u/\partial z$.

What is the point of estimating Ri and N? The idea with Ri is to compare the stability of the water columns due to stratification with the instability produces by mixing.

"We agree with this comment. We adjusted the equations of RiG and N2 to determine stability of water column and mud layer"

L.193. In the introduction, the fluid mud is said non-Newtonian with thixotropic properties, and rightly so. Therefore, this assumption should be justified.

"Thank you for this comment, we calculate entrainment rate now based on Winterwerp et al. 2017 and Kirby 2010"

L.195. Define first u* and Rib and then give their expression.

"Accordingly to comments by Reviewer 1, we adjusted the Equations"

L.198. Give references to justify this expression of the velocity shear.

"Accordingly to comments by Reviewer 1, we adjusted the Equations"

L.202. Δb is known as the reduced gravity and it is generally named g'.

"Accordingly to comments by Reviewer 1, we adjusted the Equations"

L.212. "Approximately 1,95 m above the bed". Instruments were not equipped with pressure sensor? Please provide total water depth at the mooring site. Does the river flow change during the 6 week mooring period?

"Thank you for this comment, the instruments are equipped with pressure sensors, we added the river flow and other data regarding forcing for each measurement campaign and the mooring in Tables 1 and 2"

L.217. Please provide calibration curves between 0,1-500 g/L with in-situ sediment coming from your field campaigns.

"We add correlation of density measurements and SSC from direct mud samples used from the measurement campaign (Fig. 2)"

L.222. "vertical measuring transect". Do you mean vertical profile? Because transect are measurements made along a cross-section of an estuary with a boat equipped with a winch. Here, you are talking about a mooring station, right?

"We revised the wording"

L.237. Please introduce first the figure and the data presented before making conclusions.

"We revised the paragraph accordingly"

Fig. 2. Please clarify "mean flow velocity". Do you mean depth-averaged velocity?

Is it the velocity magnitude? Or the along channel component of the velocity? Why does the velocity is positive during all tidal cycle? Is it the absolute value?

Could you please express the velocity in m/s?

Time to LWS 0h should correspond to a zero velocity current (current reversal). Why does the velocity remains constant around 1m/s?

"Thank you for pointing this out. Velocity is averaged for the water column above the lutocline, we added this in the caption and in the main text. The velocity is given as magnitude, thus we added direction and changed the unit to m/s.

Time to LWS 0h does not necessarily correspond with zero velocity (see Becker et al. 2018), thus our results are in agreement with previous findings at the Ems estuary. A time of 0 velocity is given through the addition of direction."

L.238. Around LWS+2h.

"near-continuous presence of fluid mud […] regardless of tidal conditions" This is not true. In Fig2, during spring tide, fluid mud is absent during hours, from HWS-1h to (we don't really know because of missing data but we could expect) LWS-1h.

"We added LWS+2h,

the near-continuous presence is reflected based on a very short time of absence (~30min – 1h) over the 8h period that was measured. We changed the wording to account for the missing data"

L.240. Only **0**%-40%, there is period with no fluid mud at all.

"The sentence starts with: The maximum vertical extent – thus 30-40% are related to the maximum vertical extent."

L.241. The five stages defined here, do they correspond to those defined by Becker et al. (2018). If so, it should be mentioned. If not, it should be discussed.

"According to comments by Reviewer 1 we adapted the Stages to Becker et al. (2018) to make it more comparable"

L.241. Stage I seems not appropriate for spring tide where fluid mud is not persistent.

"see comment above"

L.244. Stage V (same comments) fluid mud is not persistent during spring tide.

"see comment above"

L.247. Reference to Wu et al. (2022) to clarify.

Are you presented your own data? or are you talking about Wu et al. (2022) observations? If so, clarify under which conditions (type of estuary, forcing conditions etc ….) other studies have shown the same behavior.

"We expand the relation to findings by Wu et al. (2022)"

L.254-255. Maximum current velocity (and velocities in general) are very similar between spring and neap tides: around 1.40 m/s.

"Thank you for pointing this out, it is related to mean velocity – we changed the wording. However, we do not agree that velocities are very similar when we have a discrepancy of > 20% during flood around 3 to 4h after LWS"

L.256. Neap tides rising currents are only slightly stronger than spring tide rising currents. Please provide numbers.

"Numbers are adjusted as we use streamwise velocity now which makes the differences more clear"

L.257. Where can we see this information?

"Thank you for pointing this out. We add plots of density measurements to the supplement and give reference to this here."

L.258-259. This is during ebb phase (Stage I) not flood phase (Stage II, III, IV, V).

"Thank you, we added ebb phase"

L.260. As per Fig4, Current velocities are really similar during Stage I, II and III, and spring tide velocities seem more important during Stage IV and V. Therefore, I don't understand how you can conclude that the fluid mud modulates turbulence and current velocity from Fig2.

"As we would expect current velocities to be higher during spring tide, but measure higher velocities during neap tide, we conclude that the relative amount of fluid mud modulates current velocity."

As previously mentioned, if you are presenting conclusion from your data set, do not add reference to another study at the end of the sentence. If you want to compare your results to another study findings, you have to clearly state it.

"We added a new sentence to relate our findings to existing literature"

L.263. Is one data point sufficient to support a 1DV model?

This 1DV model could be applicable if the fluid mud was stagnant or moving very slowly compared to the upper layer, as it is shown in Becker et al. (2018) with one order of magnitude difference between the upper and the lower layer. In the present study, the lower layer flows at very high velocities (similar to the upper layer). Please justify.

"Given the concerns raised by Reviewer 1 as well, we removed the 1DV model and put more emphasis on differences based on streamwise velocity and the impact of upstream discharge"

L.270. Please introduce the figure first.

"average entrainment rate" Please clarify: is it tidally averaged, phase averaged, depth averaged?

"We added depth-averaged and reference to Table 2"

L.273. During ebb-neap tide (Stage I) the buoyancy does not remain constant. Please clarify.

"Thank you for pointing this out, we rephrased this"

Tab. 2. Is this table necessary?

Entrainment rate, buoyancy frequency and hnorm have been average over the stage and depth, right? If so it should be clearly stated in the caption and in the text.

"Hnorm is the normalized height" of what? The luthocline height?

Please introduce the table in the text.

"The table is removed in the revised version"

L.275. Again if you are presenting your data please do not refer to other studies. If you want to make comparison, do it in another sentence.

"We added another sentence to refer to other studies"

L.280. "quasi-stationary" Do your observations match this statement?

"They do not, as we observe absence of the entire fluid mud layer, we added a sentence to emphasize this"

L.283. Ri values below and above the 50g/L horizontal line seems similar to me at Stage I. At Stage II, there is no data below 50g/L. And Ri remains below 0,25 (Fig. 4a, b),

therefore your lower “fluid mud” layer seems unstable all along the tidal cycle. We could have expected quite the opposite. This should be mentioned and discussed.

“The absence of values below 50g/L is likely due to the data availability which limits the calculation of Rig. We added discussion on limitations”

Fig. 4. Please introduce Fig. 4 in the text.

Why is there no Richardson number calculated above 10g/L? if you want to compare the stability of the upper layer compared to the lower layer, you need to calculate it over the whole water column.

Shaded data should be mentioned in the caption.

Be consistent between Fig. 2 and Fig. 4 regarding the water depth or the height above the bed.

The unit of the buoyancy frequency seems erroneous. A line indicating N=0,4 s-1 could be added.

The density seems to be correlated to SSC only, i.e. the salinity seems very low. This could be mentioned in the text.

“Calculations of Richardson number is based on the density measured by the RheoTune probe, thus the absence of a density gradient yields no Richardson number.

That density is correlated to SSC is right as SSC is estimated based on density values – not the other way around.

We adjusted the water depth/height above bed annotations.

We further adjusted the equations in accordance with Reviewer 1.”

L.310-313. Could it be linked to the fact that the measurement site is located at the end of the mud patch? Therefore, the mud patch is advected upward with the strong flood currents.

In Fig. 4, we can see that at Stage II the lower layer is moving at a velocity of 0,8 m/s or more.

“We took a closer look into the position of the mud patch, investigating the TSS values from the stations upstream and downstream. However, they are limited to 2m above bed as well, thus no reliable estimate can be given.”

L.340. Please introduce Fig. 5 and 6. What does mean ebb tides for you? Only stage I? And flood tides means Stage II to V? Or did you average between HWS and LWS?

Why do you analyze ebb and flood separately? I don’t see the point here. From my point of view, Fig. 5 and Fig. 6 should be merged, and fluid mud occurrence should be analyzed along whole tidal cycles.

“Thank you for this suggestion, we merge Figures 5 and 6.”

L.341. How could you state “persistent state” when the fluid mud disappears during flood?

“We changed the name of the state and refer to residence time of fluid mud.”

L.342. Fig.6 presents flood tides data. It should not be mentioned here as the sentence begins with “Based on ebb tides”.

“We removed the reference to Fig. 6”

L.343. “50% of the tidal duration” Is it true? Or do you mean 50% of the tidal phase duration (ebb/flood)?

“We changed duration to phase”

L.345. “throughout most of the tidal cycle” what does it means? Tidal cycle or tidal phase (ebb/flood)? And “most” means more than 50% of the time? Please clarify.

“Clarification is given in section 4.2 as persistent state is defined as > 50% of ebb phase. However, we changed the name of the state to prolonged”

L.345. “intermediate to neap tides” Please give clear value of tidal range.

“Thank you for this comment, we added values for tidal amplitude/range”

Fig. 5. and 6. The lower subplot should represent occurrence along the y axis, time along the x axis and with one colored line for upper probe and another colored line for lower probe.

As blue and orange were used to discriminate neap from spring tide in previous figures, it could be envisaged to use others colors to indicate the different probes.

“With merging Fig. 5 and 6 and changing it to colored line, the lower subplot becomes redundant. Thank you for this suggestion.”

L.380. Do not refer to other studies when presenting your own results.

“We added another sentence to relate to references”

L.382. “At mid TSS probe” At 2m above the bed.

“Changed the phrase”

L.384. How much tidal amplitude differences?

“Added tidal forcing for both mooring data and campaigns in Tables 1 and 2”

Fig. 7 please indicate the water level in meter.

Please expand y axis for water level to clearly display the tidal asymmetry.

Please use validated data set for water level (Fig. 7b and c) to avoid vertical lines.

Indicate date in addition of time, or give the date in the caption.

"We adjusted Fig. 7 accordingly, however, we are not quite sure what validated data means here and what it means to avoid vertical lines, as the only vertical lines in the plot indicate low and high water. We added the beginning date of the measurement"

L.388. Give numbers.

"We added length of tidal duration based on HWS and LWS difference and added Table 1 and 2 regarding forcing conditons"

L.392. Are we discussing the "absence state" (Fig. 7a) only? If so, clearly indicate it.

"Thank you for pointing this out, we added Fig 7a"

L.393-394. There is a clear vertical gradient in dissolved oxygen levels generated during ebb tide, with lower values close to the bed.

"Thank you, we added this to the text"

L.409. "continuous" the fluid mud occurrence is not continuous during the "build up phase".

"As suggested earlier, we adjust wording of continuous and persistent"

L.410. Replace "recorded by the mid TSS probe" by at 2 m above the bed.

"Replaced"

L.414-415. Fluid mud formation can be produced in totally fresh water condition (in most estuaries). There is no necessity for salinity stratification. Why are you specifying that?

"We agree with this comment and our text does not state otherwise, we state that stratification is/can either be related to salinity stratification or sediment induced. We rephrased for clarity"

L.416. The fluid mud is absent during part of the "persistent state" (during hours around HWS). Please consider changing the name of this "state".

"Done"

L.419. Is "dissolution" appropriate wording?

"We changed dissolution to dispersion"

L.421. What do you mean by "fluid mud layers modulates salinity stratification"?

L.422. "as dissipation is dampened". Please clarify.

"The fluid mud layer drives the vertical salinity gradient, as mixing between water column and fluid mud layer is dampened/reduced, thus salinity rich water can be trapped in the mud layer. We expand this sentence for clarification"

L.427. Uniform instead of consistent.

"Changed"

L.434-437. Have you compared river flow before and during both experiments (mooring vs profiles)? The river flow largely impacts the position of the fluid mud patches. That could probably explain the discrepancies between data set.

"We added data of forcing such as discharge from headwater"

L.438-440. This is probably due to advection instead of resuspension.

Thank you, we added this line of argumentation. However, as this data is not available for the Ems estuary this is highly hypothetical as well."

L.443. Defontaine et al (2019) is not an appropriate reference regarding organic matter, microbial activity or nutrient cycling.

"We removed this reference here"

L.441-446. This is highly hypothetical.

"The impact of organic matter on the stability of fluid mud has been reported in several studies before. So is the increase in oxygen during spring tides. We agree that this is hypothetical and removed it from this manuscript, as it likely needs more time to investigate this matter."

L.450-454. In your data, the fluid mud layer disappears at every beginning of flood phase impeding "consolidation" and solid structure to develop. Therefore, I don't see you point here. Please clarify.

"As stated in the paragraph, we observe higher density during neap tides.

L.456-457. As per Fig 4., velocity is similar or higher during spring than neap tides (not the opposite), and the fluid mud layer (>10g/L) is far from immobile (velocity up to 1 m/s). This reasoning seems quite inappropriate.

"The current velocity given in Fig. 4 is an stage-averaged velocity with low vertical resolution in the mud layer. The response time of the sensor is likely too slow to capture the current velocity reliably inside the mud layer. We add this limitation of the sensor to the text. Thus we do not expect the mud layer to move with 1m/s."

L.457-460. Does your current meter provides the lateral (cross sectional) velocity component? Could you have a look to it to investigate the potential lateral spreading of mud?

“The RCM captures velocity magnitude with direction. We add direction to the plot as we calculate streamwise velocity throughout the manuscript now. Thank you for this suggestion”

L.462. SOM content is not part of your data set.

“We removed the part regarding SOM content as the impact of SOM content is too hypothetical and we do not present own data in this manuscript”