Testing floc settling velocity models in rivers and freshwater wetlands, Author's Response, June 2, 2024

Our responses to referees are listed first. Then, we explain additional edits outside of those suggested by referees.

RC1

This is a very interesting and meticulously written paper that explores in detail the settling velocity of flocs. The knowledge gap is clearly identified, the methods are extensively explained, and the findings of this study are useful and clearly presented. I only have a few comments that may add some clarity or extend the discussion a little bit.

Reply: We thank the reviewer for their helpful feedback and positive reception of our manuscript. We have addressed their questions and comments below.

My most important question is how representative a depth-averaged Kolmogorov microscale value is to infer the effect of turbulence on flocculation and floc settling velocity. In lines 245-249, the authors show that they estimate profiles of Kolmogorov microscales across the depth (based on turbulence dissipation rate profiles), but later on, the influence of the Kolmogorov microscale is evaluated as a depth-averaged quantity (as mentioned in Table 2 and e.g. line 533). It is not clear to me if the authors coupled the vertical variation of the Kolmogorov microscale (to introduce an expression for turbulence intensity) with the vertical variation of the suspended sediment concentration or they used the depth-averaged value of the Kolmogorov microscale across the whole depth for the variations across the sediment concentration profile. I would appreciate it if you further elaborate on this.

Reply: We agree that our definition of Kolmogorov microscale was confusing. We used the depth-averaged Kolmogorov microscale throughout the manuscript except for the comparison with the semi-empirical model (Fig. 14-16 in the original submission), in which we used the near-bed Kolmogorov microscale because the model was calibrated using the near-bed value (Nghiem et al., 2022). We find depth-averaged values are about a factor of 1.5 larger than the near-bed values. For simplicity and clarity, we have replaced the depth-averaged values with the near-bed values throughout the manuscript (lines 257-261). Using the near-bed Kolmogorov microscale also presents a more reasonable comparison with the suspended floc size distribution because the maximum stresses occur near the bed and thus play a strong role in limiting the maximum floc size (Mehta and Partheniades, 1975).

A followup question is about the role of bed-generated turbulence on the sediment concentration in general. The authors mention that there is limited vertical variation in sediment diameter and concentration (e.g., lines 481, 484, 522), but the turbulence intensity varies a lot with depth, especially near the bed. How does this fact relate to the discussion in Section 6.3? Is this relevant, or do the authors prefer to focus on depth-averaged quantities?

Reply: Although the turbulence intensity does indeed depend on the height in the water column, we do not believe this fact is particularly relevant to the discussion in Section 6.4, *Environmental Controls on Flocculation*. The main message of Section 6.4 is simply that higher turbulence intensity generally promotes floc breakage at equilibrium, which is unrelated to the vertical variation in turbulence intensity on two fronts. First, we describe the effect of turbulence on sediment entrainment in terms of the boundary shear velocity, which by definition does not rely on the vertical variation of turbulence. Second, although the specific floc aggregation and breakage rates will depend on the vertical turbulence structure (i.e., through the Kolmogorov microscale), we elected to omit this detail because it does not impact the main message. As such, we have kept the text as is.

Finally, in lines 451-452, does the sediment stratification affect the velocity (and turbulence) profile, or is it not that strong?

Reply: The reviewer makes a good point about the possibility of sediment stratification effects on turbulence. We did not account for sediment stratification effects on the flow because the sediment concentration was too low to significantly affect the eddy viscosity. We calculated the flux Richardson number, using the appropriate settling velocity depending on the flocculation state of each sediment class, to verify this fact. The flux Richardson number had a median of 2.7×10^{-4} and maximum of 7.1×10^{-3} across all suspended sediment samples, indicating a negligible stratification effect (Wright and Parker, 2004). We have added this result to Section 4.5 (lines 357-360).

Another point for clarification comes from line 245 and the rest of the paragraph: How representative was the law of the wall for the measured velocity profiles? What were the coefficients of determination? In other words, did the measured velocity profiles exhibit a fully developed turbulent boundary layer to allow for the proper calculation of the shear velocity at the bed?

Reply: We appreciate the reviewer’s detailed questions about the shear velocity quantification from the ADCP velocity profiles. The law of the wall fits are reasonable because each velocity profiles display a clear linear trend when plotting flow velocity versus the logarithm of height. The coefficients of determination have a median of 0.90 and range from 0.17 to 0.99. Deviations from the linear trend near the bed (where data quality is poor) are responsible for the smaller coefficients of determination. However, we examined plots of the semi-log velocity profiles and confirmed that the fitting procedure indeed captures the linear portion of the velocity profile. We have added these details to shear velocity methods in Section 4.1 (lines 251-256).

Were there wind waves at the shallower sites affecting the velocity profiles (and sediment concentrations)? All sites except one have flow depths less than 4 m (Table 2), thus the 20% of the depth that was used for the law of the wall fit, was less than 0.8 m.

Reply: We did not observe any features in the velocity profiles indicative of wind effects. As noted above, we attributed deviations from the law of the wall to poor data quality. Since the submission of the manuscript, we have also expanded the fitting procedure to include the bottom 50% of the flow depth to incorporate more data but still exclude data near the water surface where tide and wake effects might be significant (Soulsby and Dyer, 1981; Nezu and Nakagawa, 1993). We have added this change to the shear velocity methods in Section 4.1 (lines 251-256).

How many measurement cells did you use to fit the law of the wall to measurement data from such shallow waters? How was the ADCP deployed (looking downwards or upwards), and what was the sampling resolution (cell size, sampling frequency, averaging duration)? How close to the bed did you measure? This is particularly relevant for the sites M1 Spring and M2 Spring which have depths of less than one meter.

Reply: The number of bins depended on the flow depth. For the deeper flows (>10 m) in Wax Lake Outlet and the delta apex, the velocity profiles contain about 50 bins. The shallower channels (~3-4 m depth) have about 10-30 bins. The islands, with depths 1 m or less, have about 5 bins. The vertical bin size is about 10-20 cm for the deeper flows and about 5-10 cm for the shallower flows. Although the shallow island velocity profiles have fewer points in the vertical, we are still confident in our law of the wall fits because those profiles still show the expected linear trend in semi-log space.

We deployed the ADCP at the water surface, looking downward. With this field configuration, we were able to measure to within 5-15 cm of the bed. The precise sampling frequency varied, but was generally higher than 1 Hz. The number of velocity profiles averaged together in time varied because we only included measurements located within 1.5 times the flow depth of the sampling location. Typically, we averaged 100-1000 velocity profiles in the island and about 50 in the channels. We have added these ADCP measurement details in the shear velocity methods in Section 4.1 (lines 241-250).

Finally, were there any vegetation effects in sites M1 and M2 Springs?

Reply: We did not observe any "kinks" in the velocity profiles that might be indicative of the presence of submerged vegetation (Baptist et al., 2007). We have added this detail to the shear velocity methods in Section 4.1 (lines 249-250).

Some other minor comments in order of appearance:
Lines 9-12: When I first read this sentence in the abstract, I was expecting the development of a new model but the analysis relies a lot on models from the literature (Lines 48 and 57). Can this be clarified in the abstract?

Reply: We have removed this line from the abstract to avoid confusion.

Equation (1): Please explain each variable when it is firstly mentioned (Df is explained in line 94 instead of here).

Reply: Thanks for catching this error. We have moved the first definition of D_f to immediately follow Equation (1).

Line 81: What does b express? Is the value of 20 a result of calibration or observation or something else from the cited study? Since you explain in detail everything else, maybe it's a good idea to also explain this in one line.

Reply: b_1 quantifies particle shape effects. The value of 20 was calibrated from settling experiments in Ferguson and Church (2004). Although we already discuss the impact of the choice of b_1 on our results in Section 6.3 (lines 796-803), we have added a few words about the meaning of b_1 where it is first introduced in Section 2.1 (line 79).

Line 121: How does Eq. (2) support that "Flocs tend to be less dense at their edges"?

Reply: Equation (2) demonstrates this point because it shows that solid fraction decreases as flocs grow, indicating that floc density at the edges must be smaller than in the center. We have clarified this statement in the sentence in question and where we first introduce Equation (2) (lines 85-89).

Lines 285-286: Can you please elaborate a bit on the "empirically-determined gradient cutoff"?

Reply: We determined the gradient cutoff by trial-and-error to optimize the tradeoff between the number of detected particles and the gradient of those particles (i.e., whether they are in focus). We have added this explanation to the floc cam methods in Section 4.4 (lines 327-329).

Line 300: Correct to "in a given time-scale"

Reply: We have reworded the sentence to make it more readable (line 340).

Figure 15a: Equation 2a is mentioned but there is no such equation. Maybe Equation 6a is meant?

Reply: Thanks for catching this error. Figure 15a is now Fig. 13a in the revised manuscript. We have corrected the reference to the equation in Fig. 13a.

References

Baptist, M. J., Babovic, V., Rodríguez Uthurburu, J., Keijzer, M., Uittenbogaard, R. E., Mynett, A., and Verwey, A.: On inducing equations for vegetation resistance, Journal of Hydraulic Research, 45, 435–450, https://doi.org/10.1080/00221686.2007.9521778, 2007.

Ferguson, R. I. and Church, M.: A Simple Universal Equation for Grain Settling Velocity, Journal of Sedimentary Research, 74, 933–937, https://doi.org/10.1306/051204740933, 2004.

Mehta, A. J. and Partheniades, E.: An investigation of the depositional properties of flocculated fine sediments, Journal of Hydraulic Research, 13, 361–381, https://doi.org/10.1080/00221687509499694, 1975.

Nezu, I. and Nakagawa, H.: Turbulence in open-channel flows, AA Balkema, Rotterdam, 1–281, 1993.

Nghiem, J. A., Fischer, W. W., Li, G. K., and Lamb, M. P.: A Mechanistic Model for Mud Flocculation in Freshwater Rivers, Journal of Geophysical Research: Earth Surface, e2021JF006392, https://doi.org/10.1029/2021JF006392, 2022.

Soulsby, R. L. and Dyer, K. R.: The form of the near-bed velocity profile in a tidally accelerating flow, Journal of Geophysical Research: Oceans, 86, 8067–8074, https://doi.org/10.1029/JC086iC09p08067, 1981.

Wright, S. and Parker, G.: Density stratification effects in sand-bed rivers, Journal of Hydraulic Engineering, 130, 783–795, https://doi.org/10.1061/(ASCE)0733-9429(2004)130:8(783), 2004.

RC2

This is a very well planned, well thought-out and very well-written paper. In particular the finding that the effective primary particle diameter is not related to a particular length scale was revealing to me. I only have a couple of very basic comments:

Reply: We thank the reviewer for their positive feedback and comments on the manuscript. We have addressed their questions below.

1) not being familiar with the area at all, is the Wax Lake Delta truly a freshwater delta? It seems odd to me that Mike Island, only 6-8 km from the Gulf of Mexico would be an all-freshwater environment? Do the authors have some CTD measurements or similar they could add to the results to show this?

Reply: The reviewer raises a good point because high salinity from seawater, if present, would affect flocculation processes in Wax Lake Delta. However, our water chemistry measurements, when converted into salinity, show that the salinity had a median of 0.25 ppt and a maximum of 0.29 ppt. These values indicate the water was fresh (< 0.5 ppt) and well below the average salinity of seawater (~35 ppt). This is consistent with the results of Holm and Sasser (2001), who did not observe any saltwater incursion into Wax Lake Delta using an in situ CTD sensor. We have added these notes about freshwater to Sections 3 (lines 175-176) and 5.1 (lines 510-511).

2) I am personally not a big fan of satellite images as the ones in 1A and 1C - a proper map that shows the system is less cluttered as far as I'm concerned.

Reply: The reviewer's point is well-taken. However, we have retained the satellite image in Fig. 1 because we think it is helpful to establish the general area to readers.

I would also be curious to know how the shorelines of Mike Island changes from spring to summer during the high and low discharges. This would provide a better system understanding by showing how much of it is submerged at various flood stages.

Reply: Geleynse et al. (2015) conducted a detailed study of the shoreline of Wax Lake Delta and showed that it varies widely depending on the interaction of river discharge, tides, wind, and vegetation. We have added a short note about the shoreline to the study site introduction in Section 3 (lines 173-174). We have also noted in the caption that the satellite image in Fig. 1 was taken at relatively low discharge and tide and used to show the full extent of islands (lines 181-182).

References

Geleynse, N., Hiatt, M., Sangireddy, H., and Passalacqua, P.: Identifying environmental controls on the shoreline of a natural river delta, Journal of Geophysical Research: Earth Surface, 120, 877–893, https://doi.org/10.1002/2014JF003408, 2015.

Holm, G. O. and Sasser, C. E.: Differential salinity response between two Mississippi River subdeltas: implications for changes in plant composition, Estuaries, 24, 78–89, https://doi.org/10.2307/1352815, 2001.

RC3

MY REVIEW COMMENTS:
I see these finding to be quite interesting. Improving our understanding of the flocculation within coastal and estuarine regions is one of current challenges. It is of great importance to understand how sediment transport for any sediment type that is even partly cohesive, including mud:sand/silt mixtures, can influence the depositional characteristics of microplastics. This will also mean that the flocculation is a vital component of the sedimentary settling process. The manuscript is generally well structured, some relevant illustrations, and a reasonable range of relevant literature cited and referenced. The manuscript is of an appropriate length for egusphere.

Before publication can be considered, I would like to see the following comments and modifications accounted for within the manuscript.

Reply: We thank the reviewer for their constructive feedback and detailed recommendations of relevant papers. We have addressed their comments and questions below.

Good to include a few key quantitative findings within the Abstract.

Reply: We agree with the reviewer that the key quantitative findings should be reported in the abstract. They are included throughout the abstract (lines 12-14; line 16; line 18). We have also reported these findings in the conclusion as a summary (lines 854-855; lines 861-862; lines 867-868).

Although there are numerous references cited, I would suggest that a number of references reporting key advances in flocculation and near-bed processes are included in this manuscript, being cited initially in the Introduction (1) and also in relevant parts of the manuscript such as Results and Discussion sections.
* Soulsby, R.L., Manning, A.J., Spearman, J. and Whitehouse, R.J.S. (2013). Settling velocity and mass settling flux of flocculated estuarine sediments. Marine Geology, doi.org/10.1016/j.margeo.2013.04.006.
* Wolanski, E. and Elliott, M. (2015). Estuarine Ecohydrology. An Introduction. Elsevier, Amsterdam. 322p.
* Craig, M.J., Baas, J.H., Amos, K.J., Strachan, L.J., Manning, A.J., Paterson, D.M., Hope, J.A., Nodder, S.D., and Baker, M.L. (2019). Biomediation of submarine sediment gravity flow dynamics. Geology, vol. 48, no. 1, pp. 72-76. https://doi.org/10.1130/G46837.1
* Lawrence, T.J., Carr, S.J., Wheatland, J.A.T., Manning, A.J., and Spencer, K.L. (2022). Quantifying the 3D structure and function of porosity and pore space in natural sediment flocs. Journal of Sediments and Soils 22, 3176-3188, https://doi.org/10.1007/s11368-022-03304-x.

* Winterwerp, J. C., van Kesteren, W. G. M. (2004). Introduction to the physics of cohesive sediment in the marine environment. In: van Loon, T. (ed.), Developments in Sedimentology, 56. Amsterdam: Elsevier.
* Whitehouse, R. J. S., Soulsby, R.L., Robert, W. and Mitchener, H.J. (2000). Dynamics of Estuarine Muds: A manual for practical applications. Thomas Telford, London, ISBN 0-7277-2864-4.
* Mehta, A.J. (2022). An Introduction to Hydraulics of Fine Sediment Transport. 2^{nd} Edition. Advanced Series on Ocean Engineering, Vol. 38. Hackensack, NJ: World Scientific Publishing Co.
* Mietta, F., Chassagne, C., Manning, A.J. and Winterwerp, J.C. (2009). Influence of shear rate, organic matter content, pH and salinity on mud flocculation. Ocean Dynamics, 59, 751-763, doi: 10.1007/s10236-009-0231-4.

Reply: We appreciate the thorough suggestions on important papers to cite. For this comment and following comments with suggested references, we have selected several of the most important recommended papers to cite, but did not add all of them in the interest of maintaining a reasonable number of references.

In the Methodology sections I would like to see a slightly clearer scientific statement indicating the rationale for the experimental set-up and protocols. This will assist future scientist researching within this particular Sedimentary-MP field.

Reply: We have invested much effort into overhauling the Methods section (Section 4) to better communicate the rationale and strategy behind our methods. First, we have reframed the summary at the beginning of Methods to indicate that our methods are designed to elucidate the knowledge gaps in the semi-empirical and explicit models (lines 204-219).

As follows, we have also emphasized the novel parts of the data analysis to highlight the advances made in our study in Section 4.6. We have added Fig. 4 to illustrate how we distinguished between flocs and unflocculated grains using the in situ laser diffraction (LISST) and suspended sediment data (Section 4.6.1). We rewrote Section 4.6.2 to more clearly motivate the equations used to calculate fractal dimension and effective primary particle diameter. We dedicated Section 4.6.3 solely to drag ratio and added Eq. (12) as background to show the comparison between data and theory for drag ratio in Fig. 9a. Finally, we used Section 4.6.4 to describe calculation of the floc settling velocity distribution.

Outside of Methods, we have added Section 6.1, *Leveraging Multiple Floc Data Sources*, to once again emphasize the new floc constraints made possible by combining sediment concentration-depth profiles, in situ laser diffraction, and camera.

I would like to see just a few more comments on the effects of sedimentary organic cohesion levels in the Discussion and earlier in the Introduction. I see some of the reference listed in the manuscript mention organic material effects. Some other more recent research on the role of organic cohesion relating to purely cohesive and naturally mixed sediment bedform are listed below. I would suggest that key points could be included from some of these publications (listed below) in both the literature review and also within the discussion and interpretation:

* Eisma, D., (1986). Flocculation and de-flocculation of suspended matter in estuaries. Neth. Journal of Sea Res., 20 (2/3): 183-199.
* Deng, Z., He, Q., Manning, A.J. and Chassagne, C. (2023). A laboratory study on the behavior of estuarine sediment flocculation as function of salinity, EPS and living algae. Marine Geology 459:107029-107029, doi.org/10.1016/j.margeo.2023.107029
* Malarkey, J., Baas, J.H., Hope, J.A., Aspden, R.J., Parsons, D.R., Peakall, J., Paterson, D.M., Schindler, R.J., Ye, L., Lichtman, I.D., Bass, S.J., Davies, A.G., Manning, A.J., Thorne, P.D. (2015). The pervasive role of biological cohesion in bedform development. Nature Communications, DOI: 10.1038/ncomms7257.
* Gregory, J. and Barany, S. (2011). Adsorption and flocculation by polymers and polymer mixtures. Advances in Colloid and Interface Science, 169(1), 1–12.
* Parsons, D.R., Schindler, R.J., Hope, J.A., Malarkey, J., Baas, J.H., Peakall, J., Manning, A.J., Ye, L., Simmons, S., Paterson, D.M., Aspden, R.J., Bass, S.J., Davies, A.G., Lichtman, I.D. and Thorne, P.D. (2016). The role of biophysical cohesion on subaqueous bed form size. Geophysical Research Letters, 43, doi:10.1002/2016GL067667.
* Paterson, D.M., Crawford, R.M. and Little, C. (1990). Subaerial exposure and changes in the stability of intertidal estuarine sediments. Estuarine Coastal and Shelf Science, 30, 541-556.
* Paterson, D.M. and Hagerthey, S.E. (2001). Microphytobenthos in contrasting coastal ecosystems: Biology and dynamics. In: Ecological comparisons of sedimentary shores (K. Reise, Ed.), Ecological studies, pp. 105-125.
* Gregory, J. (2005). Particles in Water: Properties and Processes. CRC Press, zeroth edition.
* Schindler, R.J., Parsons, D.R., Ye, L., Hope, J.A., Baas, J.H., Peakall, J., Manning, A.J., Aspden, R.J., Malarkey, J., Simmons, S., Paterson, D.M., Lichtman, I.D., Davies, A.G., Thorne, P.D. and Bass, S.J. (2015). Sticky stuff: Redefining bedform prediction in modern and ancient environments. Geology, doi: 10.1130/G36262.1.
* Wolanski, E. and Elliott, M. (2015). Estuarine Ecohydrology. An Introduction. Elsevier, Amsterdam. 322p.
* Tolhurst, T.J., Gust. G. and Paterson, D.M. (2002). The influence on an extra-cellular polymeric substance (EPS) on cohesive sediment stability. In: J.C. Winterwerp and C. Kranenburg (Eds), Fine Sediment Dynamics in the Marine Environment - Proc. In Marine Science 5, Amsterdam: Elsevier, pp. 409-425, ISBN: 0-444-51136-9.

Reply: As the reviewer points out, the role of organic matter in cohering sediment grains is a key mechanism for constructing flocs. There is an interesting connection between organic matter and turbulence because biological cohesion might link turbulence and the fraction of organic-rich flocs in the bed through the bedform roughness. We have added a few more comments on the effects of organic matter on freshwater floc dynamics in the Introduction (Section 1; lines 40-42) and Discussion (Section 6.4; lines 841-843).

For completeness, I think it is worth mentioning Derjaguin-Landau-Verwey-Overbeek (DLVO) theory, as this is often used to quantify particle-to-particle interactions. This requires relevant referencing, including:
* Chassagne, C. (2019). Introduction to Colloid Science. Delft Academic Press.
* Kruyt, H. R. (1949). Colloid Science. Technical report, Elsevier Pub. Co.
* van Leussen, W., 1994. Estuarine macroflocs and their role in fine-grained sediment transport. Ph.D. Thesis, University of Utrecht, The Netherlands, 488pp.

Reply: We appreciate the recommendation to discuss DLVO theory because it is an important framework for understanding charge interactions and flocculation potential between suspended particles. We have added a note about this mechanism of salt-driven flocculation in the Introduction (Section 1) where we review the known factors that affect flocculation (lines 36-37).

Furthermore, in estuarine areas, the suspended sediment particle will, according to DLVO theory, be destabilized and flocculate because of the increase in salinity between the river fresh water and the sea. However, in the presence of organic matter however, DLVO theory cannot be applied, as the flocculation mechanisms will be driven by the presence of polyelectrolytes and microorganisms which are not accounted for in the DLVO theory (see Deng et al., 2023). I would like to see a comment on this point within the Discussion and implications for cohesive sedimentary settling.
* Deng, Z., He, Q., Manning, A.J. and Chassagne, C. (2023). A laboratory study on the behavior of estuarine sediment flocculation as function of salinity, EPS and living algae. Marine Geology 459:107029-107029, https://doi.org/10.1016/j.margeo .2023.107029

Reply: We absolutely agree that the interactions of salinity and organic matter result in a much more complicated flocculation than can be captured by standard DLVO theory. We have added a note in the Introduction (Section 1) about the limitation of DLVO theory when organic matter drives flocculation (line 41). We have also added a note in the Discussion (Section 6.4) about the potential role of organic matter in setting floc settling velocity (lines 841-843).

I would not agree with the statement on line 153 that sand grains are not incorporated within mud flocs. As much of the study focuses on the roles of both cohesive and non-cohesive sedimentary dynamics - sandy / silts / clays, recent key publications on the flocculation processes of cohesive and mixed fine-grained sediment suspension (experimental results, applied modeling; floc properties; floc structure settling and deposition) need to be considered (and cited) within both the Introduction and Discussion / Interpretation, as these outline key processes relating to these suspended sediment types - including:
* van Ledden, M. (2002). A process-based sand-mud model. In: J.C. Winterwerp and C. Kranenburg (Eds.), Fine Sediment Dynamics in the Marine Environment - Proc. in Mar. Science 5, Amsterdam: Elsevier, pp.577-594, ISBN: 0-444-51136-9.

* Manning, A.J., Baugh, J.V., Spearman, J.R., Pidduck, E.L. and Whitehouse, R.J.S. (2011). The settling dynamics of flocculating mud:sand mixtures: Part 1 – Empirical algorithm development. Ocean Dynamics, INTERCOH 2009 special issue, doi: 10.1007/s10236-011-0394-7.
* van Ledden, M., 2003. Sand-mud segregation in estuaries and tidal basins. Ph.D. Thesis, Delft University of Technology, The Netherlands, Report No. 03–2, ISSN 0169-6548, 217pp.
* Manning, A.J., Baugh, J.V., Spearman, J. and Whitehouse, R.J.S. (2010). Flocculation Settling Characteristics of Mud:Sand Mixtures. Ocean Dynamics, doi: 10.1007/s10236-009-0251-0.
* Dankers, P.J.T., Sills, G.C. and Winterwerp, J.C. (2007). On the hindered settling of highly concentrated mud-sand mixtures. In: T. Kudusa, H. Yamanishi, J. Spearman and J.Z. Gailani (Eds), Sediment and Ecohydraulics - Proc. in Marine Science, INTERCOH 2005, Amsterdam: Elsevier, pp. 255-274.
* Waeles, B., Le Hir, P. and Lesueur, P. (2008). A 3D morphodynamic process-based modelling of a mixed sand/mud coastal environment : the Seine Estuary, France. In: T. Kudusa, H. Yamanishi, J. Spearman and J.Z. Galiani, (eds.), Sediment and Ecohydraulics - Proc. in Marine Science 9, Amsterdam: Elsevier, pp. 477-498, ISBN: 978-0-444-53184-1.
* Spearman, J.R., Manning, A.J. and Whitehouse, R.J.S. (2011). The settling dynamics of flocculating mud:sand mixtures: Part 2 – Numerical modelling. Ocean Dynamics, doi: 10.1007/s10236-011-0385-8.
* van Wijngaarden, M., Venema, L.B., De Meijer, R.J., Zwolsman, J.J.G., Van Os, B. and Gieske, J.M.J. (2002a). Radiometric sand-mud characterisation in the Rhine-Meuse estuary, Part A: Fingerprinting. Geomorphology, 43, 87-101.
* Spencer, K.L., Manning, A.J., Droppo, I.G., Leppard, G.G. and Benson, T. (2010). Dynamic interactions between cohesive sediment tracers and natural mud. Journal of Soils and Sediments, Volume 10 (7), doi:10.1007/s11368-010-0291-6

Reply: We concur with the reviewer that sand can be incorporated within flocs. Our choice of words was confusing. We did not intend to imply that sand cannot be flocculated. We have rephrased this line to clarify that although flocs are typically composed of mud grains, sand can also be flocculated (lines 155-156).

Much of the modelling is based on flocs being represented by a fractal structure. Although this can be ustilised, I would like to see the authors including comments on the non-ftractal structure of natural flocs. See:
* Spencer, K.L., Wheatland, J.A.T., Bushby, A.J., Carr, S.J., Droppo, I.G. and Manning, A.J. (2021). A structure–function based approach to floc hierarchy and evidence for the non-fractal nature of natural sediment flocs. Nature - Scientific Reports, 11:14012, doi.org/10.1038/s41598-021-93302-9.

Reply: We recognize that the fractal model of flocs is only an approximation, as stated by the reviewer. We have added a brief comment about this fact when first introducing fractal theory (Section 2.1; lines 82-84).

In the latter part of the Introduction (page 6), I would like to see a slightly clear list of key Research Questions and the ones that are being addressed in this manuscript in terms of clear aims and objectives. This would greatly assist future researcher that follow this work.

Reply: We agree that a summary of the key objectives concluding the introduction and background is important for readers to follow the paper. We have added such a summary to the end of Section 2 (lines 164-167).

I see that a LISST-200X was used to measure floc population distributions. I would like the authors to provide some comments on the limitations of this instrumentation (e.g. SSC low turbidity range, Mie Theory application, sphere particle shape assumption. This is important to consider, as the LISST is the primary instrument being used to measure the floc/particle size distributions. References on floc measurements relating to the LISST that I suggest need to be mentioned in both the literature review and included in the discussion are:

* Gratiot, N. and Manning, A.J. (2004). An experimental investigation of floc characteristics in a diffusive turbulent flow. Journal of Coastal Research, SI 41, 105-113.
* Agrawal, Y. C., Whitmire, A., Mikkelsen, O. A., & Pottsmith, H. C. (2008). Light scattering by random shaped particles and consequences on measuring suspended sediments by laser diffraction. Journal of Geophysical Research, 113 (C4), C04023. doi: 10.1029/2007JC004403
* Manning, A.J. and Dyer, K.R. (2002). The use of optics for the in-situ determination of flocculated mud characteristics. J. Optics A: Pure and Applied Optics, Institute of Physics Publishing, 4, S71-S81.
* Fall, K. A., Friedrichs, C. T., Massey, G. M., Bowers, D. G., & Smith, S. J. (2021). The Importance of Organic Content to Fractal Floc Properties in Estuarine Surface Waters: Insights From Video, LISST, and Pump Sampling. Journal of Geophysical Research: Oceans, 126 (1). doi:672 10.1029/2020JC016787
* Manning, A.J., Friend, P.L., Prowse, N. and Amos, C.L. (2007). Preliminary Findings from a Study of Medway Estuary (UK) Natural Mud Floc Properties Using a Laboratory Mini-flume and the LabSFLOC system. Continental Shelf Research, doi:10.1016/j.csr.2006.04.011.
* Fugate, D. C., & Friedrichs, C. T. (2002). Determining concentration and fall velocity of estuarine particle populations using ADV, OBS and LISST. Continental Shelf Research, 22 (11), 1867–1886.
* Manning, A.J. and Schoellhamer, D.H. (2013). Factors controlling floc settling velocity along a longitudinal estuarine transect. Marine Geology, San Francisco Bay special issue, doi.org/10.1016/j.margeo.2013.04.006.
* Livsey, D. N., Downing-Kunz, M. A., Schoellhamer, D. H., & Manning, A. (2020). Suspended Sediment Flux in the San Francisco Estuary: Part I—Changes in the Vertical Distribution of Suspended Sediment and Bias in Estuarine Sediment Flux Measurements. Estuaries and Coasts. doi: 10.1007/s12237-020-00734-z

* Mietta, F., Chassagne, C., Manning, A.J. and Winterwerp, J.C. (2009). Influence of shear rate, organic matter content, pH and salinity on mud flocculation. Ocean Dynamics, 59, 751-763, doi: 10.1007/s10236-009-0231-4.

Reply: We agree that it is important to present the conditions under which the LISST data are valid because they form a major part of our data. We did not experience any field conditions that invalidated the LISST data because we used the irregular particle shape model and had good values of optical transmission. We have added some comments regarding this in the LISST methods (Section 4.3; lines 297-299). However, we have refrained from elaborating more on the technical details of the LISST in the Discussion because, as already stated in the manuscript and shown here by the references featuring LISST, the LISST has been well-tested for measuring flocs in the field.

Also, as only particle / floc sizes are estimated by the LISST-200X Type C, I would like to see some comments on how wide ranges in floc density within populations are accounted for in the interpretations, or what limits this may place on the data interpretation and subsequent application. This also includes any floc settling and depositional rate behaviour interpretation.

Reply: The solid fraction is simply calculated using fractal theory. Using typical values for effective primary particle diameter, fractal dimension, and floc diameter, fractal theory yields a typical range of about 0.02 to 0.2 for the solid fraction distribution. There are little additional limitations that this adds to the data interpretation because the solid fraction is calculated following fractal theory. However, the floc size distribution does indicate an interesting floc settling behavior because there is a small, but non-trivial fraction of flocs that are larger than the Kolmogorov microscale. If these large flocs are the ones that are able to resist the high near-bed shear, then they might settle and build deposits at the bed. We have added a comment on this the floc size distribution results in Section 5.5 (lines 580-584).

I would like to see a little more key quantitative results placed in and commented on in the Discussion.

Reply: We agree that key quantitative results should be reported and discussed in the Discussion where relevant. However, we did not include more quantitative results in the Discussion because they would not be relevant to the discussion points. Beyond Results, the key quantitative findings are already summarized in the abstract and Conclusion.

I hope these comments are of help and look forward to reading the final draft.
RECOMMENDATION: Minor Revisions

Reply: We thank the reviewer again for the constructive comments, which have greatly helped us to improve the manuscript.

Additional Edits

On top of edits made in response to referee comments, we edited the manuscript with the goal to increase readability. As such, we had a detailed eye to eliminate redundancy, reorganize sections, simplify known concepts, and expand on our new ideas/methods. We also eliminated less important figures (Fig. 11, 12, A1, A2 in the original submission) to focus reader attention on the most important figures. This editing process will be clear when viewing the marked-up manuscript.

Despite these edits, the core science remained the same except for our floc permeability analysis as seen throughout Sect. 2.1 and Sect. 5.7. In the initial submission, we compared our drag ratio data to the Brinkman model. In our revised submission, we mainly compared our data to the Davies model instead because Kim and Stolzenbach (2002) found it to be better suited for fractal aggregates like flocs.

In both cases, the model compares poorly against our measured drag ratios, leading us to propose a new model to explain the physical reason for the poor correlation. In the initial submission, we proposed a new permeability model by modifying the Brinkman model to keep the same solid fraction but use a larger cluster diameter instead of effective primary particle diameter. In the revised submission, we proposed a new permeability model by modifying the Davies model to keep the same effective primary particle diameter but use a different solid fraction. Although both of these approaches are empirical in the end, we changed our proposed model because, as stated in the manuscript (lines 639-641), the new approach is supported by the fractal dimensions of the modified solid fractions falling within a physical meaningful range.

The new approach changes our interpretation of the rationale for the poor comparison between permeability theory and data. In the initial submission, our interpretation was that clusters within flocs were permeable. In the revised submission, our interpretation was that the primary particle size distribution, not accounted for by Kim and Stolzenbach (2002), caused preferential flow paths structured by a subset of primary particles.

References

Kim, A. S. and Stolzenbach, K. D.: The permeability of synthetic fractal aggregates with realistic three-dimensional structure, Journal of colloid and interface science, 253, 315–328, https://doi.org/10.1006/jcis.2002.8525, 2002.