Dear Dr. Zhang and co-authors,

We have now received two reviews for your revised manuscript. Referee 3 is satisfied but has some final comments. Referee 2 states that the manuscript is closer to publication but thinks it still needs some work. The referee has listed very helpful comments which can further improve the manuscript.

I agree with the referees that publication has come closer. However, the manuscript is not there yet. Please consider the comments by the two referees and submit a revised manuscript accounting for these comment and suggestions.

I will make my decision after this round of reviews and revisions.

With best wishes

Mario Hoppema

- editor

Dear Dr. Hoppema,

Thank you for your continued support and the opportunity for us to revise and resubmit our manuscript.

We have carefully addressed all comments from both reviewers, corrected previous issues, and made substantial improvements. A detailed point-by-point response is provided in the accompanying letter.

We appreciate your time and consideration, and look forward to your further evaluation.

With best regards,

Wenping Gong

Response to Referee 2 (Report #2)

General comments and summary:

I have read the responses to reviews of the resubmission, and the revision. The authors have done a conscientious job of revising the manuscript to address concerns and the paper is improved. This submission seems closer to the standard expected for your journal, but in my opinion is not there. While the paper is improved, it suffers from being almost entirely focused on the numerical model with less attention paid to the process-based implications of the results. The organization of the paper should be improved to increase clarity and reduce redundancy.

Response:

Thank you for your continued review and for acknowledging the improvements in our revised manuscript. We appreciate your comment that the paper is now closer to the journal's standard.

In this revision, we have further improved the clarity and structure of the manuscript by reducing redundancy and enhancing the organization. While our study is based on a numerical model, we agree that emphasizing the process-based implications is important. We have adjusted the text accordingly to better reflect what the results reveal about sediment transport processes on the northern South China Sea shelf.

We hope these revisions address your concerns and improve the overall impact of the paper.

General Comments:

1. The paper would be more impactful if it emphasized more the lessons learned about

dispersal processes, rather than emphasizing the work of modeling dispersal.

Response:

Thank you for the insightful suggestion. While previous studies on the long-term distribution of Pearl River-derived sediments have primarily relied on seismic profiles, sediment cores, and radiometric dating methods, numerical investigations of their transport processes remain limited, particularly in terms of capturing the seasonal variability and spatial connectivity of cross-shelf and along-shelf sediment dispersal, despite the widespread use of numerical modeling in sediment dynamics research.

Our study aims to address this gap by employing a well-calibrated and validated numerical model, supported by observational data, to investigate sediment dispersal processes. We agree that geological methods provide valuable insights into long-term sediment deposition patterns, but we also believe that numerical modeling serves as a powerful and complementary tool for examining the dynamic mechanisms that govern sediment transport across a range of spatial and temporal scales.

1a. This is reflected in the current title of the paper which focuses on the act of implementing a model; not the process-based results derived from the model, or the timescales covered. Is that appropriate for this journal?

Response:

Thank you for the valuable comment. We have revised the manuscript title to better reflect the focus on process-based results and timescales. The new title is: “Physical Drivers and Parameter Sensitivities of Pearl River-derived Sediment Dispersal on the

Northern South China Sea Shelf: A modeling study”.

1b. In comparing the cases studies to the control run: the paper presents these in terms of the act of modeling dispersal instead of what is learned about processes via the comparison. For example, in the abstract (lines 27 – 29) “the absence of tidal forcing reduces bottom shear stress…”; (lines 32 - 33) when “ambient currents” are “omitted". These describe what happens in the model when forcings are removed from the model. The paper would have more impact if it emphasized what these results show about the importance of tides and large-scale currents in dispersing sediment. These are two examples from the abstract, but the paper takes this approach for all the comparisons of case studies to the control run.

Response:

Thank you for the constructive feedback. We have revised the abstract and relevant sections throughout the manuscript to place greater emphasis on the physical insights gained from the case comparisons, highlighting the roles of tides, waves, and large-scale circulation in sediment dispersal, rather than focusing on model setup or scenario differences.

1c. For the “cycle experiment”, the results show that riverine sediment continues to be dispersed at timescales that exceed 1 year. This is not surprising for such a large-domain model. The analysis does not provide insight into the actual residence times expected.

Response:

Thank you for the comment. While it is indeed expected that sediment dispersal can extend beyond one year in a large-domain model, we believe it is still necessary to explicitly simulate and analyze this process. Our “cycle experiment” reveals not only that Pearl River-derived sediment continues to disperse after one year, but also that the transport direction remains predominantly southwestward. In particular, sediment deposited in the Beibu Gulf is shown to continue moving southward, which offers additional understanding of the longer-term redistribution of riverine material.

We agree that quantifying sediment residence times is a distinct and important topic. However, it is beyond the scope of the present study, which focuses on identifying the physical drivers, sediment properties, and sediment conditions that influence the along- and cross-shelf transport of Pearl River-derived sediments. We consider residence time analysis a promising direction for future work and potentially a separate publication.

1d. Some model experiments focus on forcing (tides, waves, large-scale currents), and others on sediment properties (critical shear stress, settling velocity, seabed grain sizes). The analysis is presented in terms of model inputs (if tides are omitted, then sediment is retained in PRE, etc.); rather than what can be learned from the experiments. The paper would be more impactful if it phrased results in terms of the relative impacts of different forcings (for example: which has more effect, the tides or the waves?); and relative impacts of uncertainty in sediment properties.

Response:

Thank you for the constructive suggestion. We fully agree that emphasizing the relative impacts of different physical drivers and sediment property uncertainties strengthens the interpretation of the results. In fact, our analysis already highlights these aspects.

We have shown that tidal forcing is the dominant driver of sediment dynamics within the estuary, while wave forcing plays a more significant role at the river mouth and along adjacent coastlines. In contrast, large-scale background circulation primarily influences the eastward dispersal of sediments offshore during the summer. These region-specific impacts of physical drivers are explicitly discussed in the revised manuscript to further clarify their relative importance.

Regarding sediment property uncertainties, we have already addressed this point in Section 4.1. Our results indicate that variations in settling velocity have a stronger influence on sediment distribution than the seasonal variation in critical shear stress for erosion. Additionally, changes in seabed sediment grain size distributions have a relatively minor effect on the overall dispersal pattern of Pearl River-derived sediments. We also find that the timescale of analysis, whether seasonal or extending beyond one year, has a more substantial impact on the shelf-wide distribution of sediment than the uncertainty in seabed properties. These conclusions have been further emphasized in the revised discussion.

1e. Finally, the conclusions (Section 5) are very focused on modeling rather than lessons learned regarding the system as a whole. This makes the paper less impactful than it could be.

Response:

Thank you for the helpful suggestion. We have revised the Conclusions (Section 5) to place greater emphasis on the key physical insights gained from the study, rather than focusing solely on the modeling approach.

2. Comparison of 1-year model to Holocene records (Pages 51, etc). It is interesting to compare patterns calculated by the model for 1-year to the longer-term geologic record; but the paper would be more impactful if the differences in the timescales were acknowledged. Also, the paper needs to be clear about whether (or how) the geologic data were used to calibrate sensitive model parameters (settling velocity).

Response:

Based on our calculated Pearl River-derived sediment thickness, and considering that the Holocene sediment load is 2.61 times that of the modeled year, we estimated the long-term accumulation by multiplying the number of Holocene years by 2.61 and the modeled annual sediment thickness. This approach is straightforward. We also mention that the calibration of SSC, including associated sediment properties such as settling velocity, was conducted without any reference to geological data.

2a. Text starting line 929 should begin a new paragraph. This compares the 1-year model results to Holocene sediment thicknesses (6,500 years). Were the sediment settling velocities “calibrated” to match this result? If so this should not be cited as confirmation that the partitioning is valid.

Response:

Thank you for the comment. We would like to clarify that the sediment settling velocities, along with other sediment characteristics associated with SSC, were calibrated independently of any geological data. At no stage did we adjust or tune the model parameters to match the Holocene sediment thicknesses. Therefore, this comparison was not used as a basis for model calibration, and we believe it provides an independent line of support for the modeled sediment partitioning.

2b. It is not clear how the values in lines 947 – 950 were scaled to bridge the large differences in timescales.

Response:

Thank you for the comment. As noted, the Holocene sediment load is estimated to be 2.61 times that of the modeled year. To account for the timescale difference, we multiplied the modeled annual sediment thickness by both the number of Holocene years and the 2.61 scaling factor. This provided an estimate of the long-term accumulation, allowing for a direct comparison of spatial patterns.

3. The parametrization of the sediment model remains confusing, even after revision. Part of the confusion comes from the fact that the paper is not well organized and not concise. Topics are repeated in different parts of the paper and sometimes seem to

contradict each other.

Response:

Thank you for your valuable feedback. We acknowledge that the organization and clarity of the sediment model parametrization could be improved. In response, we have carefully revised the manuscript to enhance its structure, reduce redundancy, and ensure consistency throughout. We have also clarified key points to avoid any contradictions. We hope these changes have improved the readability and understanding of the sediment model parameters.

3a. For example: the setting of riverine sediment properties includes the choices of 60%/40% split and settling velocities (0.005 and 0.6 mm/s). These are said to follow Bever and MacWilliams (line 350); Zhang et al 2019 (line 359, 388). Then, line 366 lists 6 source papers plus "model calibration" but does not refer to the Supplement. Line 897 attributes the choice of settling velocities to Xia et al. and calibrations and cites the Supplement. The Supplement shows modeled and observed SSC but does not mention calibration of ws.

Looking through the source papers, the 60/40 split is from Zhang et al. 2019. The choice of settling velocities seem to be calibrated but is not clearly described. The authors need to be concise and clear about selection of these important values.

Response:

Thank you for the detailed comment. Xia et al. (2004) conducted in situ observations of settling velocities for suspended sediments in the Pearl River Estuary and reported a range of values for different grain sizes. These ranges were used to guide extensive sensitivity experiments and model calibration. Through this process, we selected representative fixed settling velocities that allowed the model to reproduce observed SSC values reasonably well. This calibration approach is a standard and widely accepted practice in sediment transport modeling.

As we have acknowledged in Section 4.1, the uncertainty in settling velocities, especially for slow-settling fine particles, can lead to variability in the model results. Nevertheless, we have made great effort to constrain the values within reasonable bounds based on both observations and model performance.

The 60%/40% class split is necessary, as the model requires an assumed distribution. This ratio was adopted from Zhang et al. (2019), where the median grain size of suspended sediments in the estuary was around 8 μm, making the 60/40 partition a reasonable approximation. As noted in Section 4.1, we have also discussed the implications of this choice and confirmed that it is within a plausible and representative range for the system.

3b. Another example is treatment of riverine sediment for the spin-up and cycling cases. The authors revised this but not achieved clarity and conciseness. At Line 351, the paper states that after the spin-up class 4 and 5 sediment were “added as class 1 and 2” (do the authors mean “added to class 1 and 2”?). Then in line 464 the “Cycle experiment” is described in a confusing way. After several readings: I think that the difference between the Control Run and the Cycle experiment is whether the Class

4&5 sediment from the “spin-up” period was added to Class 1&2 in the initial bed before the model run.

Response:

Thank you for pointing out the typo. Our original intention was indeed “added to class 1 and 2,” and we have corrected this in the manuscript.

Regarding the description of the Cycle experiment, as stated in the revised manuscript:

“Finally, to assess the model's sensitivity to the spin-up duration of Pearl River-derived sediment, particularly regarding the retention of riverine sediments in both the water column and the seabed, we adopted the sediment distributions (Classes 1 to 5) from the final state of the Control run on March 31, 2018, as the alternative initial conditions for the Cycle experiment (designated as Exp 7, Cycle hereafter). This setup carries over the full year's evolution of riverine sediment transport and deposition from the Control run (Exp 1), including changes in all sediment classes, into the start of Exp 7. As a result, Exp 7 mainly evaluates how the presence of previously deposited riverine sediments influences subsequent sediment transport estimates.”

Thus, the primary difference between the Control Run and the Cycle experiment is that the Cycle experiment includes an additional year of accumulated Pearl River sediment.

4. Section 4.2 is improved but the writing is still a bit rough. The paper does not

provide the context of Walsh and Nittrouer's classification scheme. Lines 976 and 986 seem contradictory (line 976 says your results are "unlike" Walsh & Nittrouer's framework, whereas Line 9787 says your results are "consistent").

Response:

Thank you for the insightful comment. To clarify, our findings demonstrate that most results are consistent with the hierarchical decision tree proposed by Walsh and Nittrouer (2009), except for the application of the 2 megatons per year threshold for riverine sediment discharge. While the Pearl River's sediment discharge exceeds this threshold, most sediment remains deposited near the estuary, indicating an estuarine accumulation-dominated (EAD) system. This behavior differs from the predictions of Walsh and Nittrouer's hierarchical decision tree.

We acknowledge that the original wording was somewhat rough and potentially confusing. We have revised this section to better contextualize Walsh and Nittrouer's classification scheme and clearly explain the apparent inconsistency.

5. Page 58 and 59 use a lot of text to argue for more sensitivity tests to better constrain sediment dispersal. This can probably be shortened. But more importantly, the argument should be that more DATA is needed. Without additional data streams, there seems little argument for doing more model runs. The paper might be more impactful if the authors provided insight into what type of data would be most useful.

Response:

Thank you for the valuable comment. We agree that additional observational data are essential for improving model constraints and advancing understanding of sediment dispersal. In response, we have revised the relevant section (Pages 58 and 59) to shorten the discussion on sensitivity tests and to emphasize the need for more data. We now highlight the importance of direct measurements, such as settling velocities and erosion thresholds under varying hydrodynamic conditions, as particularly useful for better parameter calibration and model validation.

Specific Comments (by line number in the preprint)

6. Line 32, Lines 452, 453, 471: use of the term "Ambient circulation" is not clear. You need to define what you mean by this (large, non-local scale forcing?).

Response:

Thank you for pointing this out. We have revised the text to clarify the meaning of "ambient circulation." It now refers specifically to the remotely forced ambient shelf current and residual (non-tidal) water levels, which are driven by large-scale, non-local forcing.

7. Line 44 (abstract): use of the word "dispersal mechanisms" seems a mis-statement. "Dispersal mechanisms" are things like bedload, suspended load, gravity flows. This paper looks at the sensitivity of suspended transport and dispersal to forcings and parameters.

Response:

Thank you for the clarification. We agree with your observation and have revised the text accordingly. The term “dispersal mechanisms” has been replaced to better reflect our focus on the sensitivity of suspended sediment transport and dispersal to physical forcings and sediment-related parameters.

The updated sentence now reads: “*Overall, this study reveals the transport pathway and fate of the Pearl River-derived sediment and provides a model-based assessment of its seasonal behavior and the sensitivity of suspended sediment dispersal to physical drivers and sediment parameters or conditions on the northern SCS shelf.*”

8. Introduction seems a bit long and could be shortened. Some of the text seems too specific and is redundant with later methods (for example lines 159 – 163).

Response:

Thank you for your valuable suggestion. We have shortened and streamlined the Introduction by removing overly specific details and reducing redundancy with later sections, enhancing clarity and focus.

9. Paragraph starting at line 328: Revise for clarity. Suggest (line 333) “initially estimated sediment distribution to evolve…”; (line 335) “introduced by Kriging, sparse or problematic data”; (line 338) “after the spin-up period is thought to exhibit spatial patterns that are better aligned…”.

Response:

Thank you for the helpful suggestions. We have revised the paragraph accordingly to improve clarity, incorporating the recommended wording where appropriate.

10. Lines 406 – 419: this paragraph provides information about the climatology of summer conditions. It is unclear what these values represent. Are these values climatological averages over several years? (if so, data sources should be cited and the time-period noted). Or are they the values from the 1-year period modeled?

Response:

Thank you for the comment. The values presented are not climatological averages. As noted in the first paragraph of Section 2.5, all values correspond to the period from April 1, 2017, to March 31, 2018, which matches the 1-year duration of the model simulation. The data sources used for these values are also clearly stated in the manuscript.

11. Section 3.2, Page 33: I found this description confusing.

Response:

Thank you for the helpful comment. We agree that the original description could have been clearer and have made appropriate revisions to improve readability and clarity. The revised text is as follows:

"*We present the sediment fluxes and retention amounts in different regions. Figure 7a-c illustrates the proportion of riverine sediment retention budget within each region, expressed as a percentage of the total annual river sediment load input (Figures 3a), for the wet summer season (Figure 7a), the dry winter season (Figure 7b), and the entire year (Figure 7c), based on the Control run, respectively.*

Meanwhile, Figure 7d illustrates the annual deposition over the shelf.

The retention of Pearl River sediment on the continental shelf exhibits significant seasonal variations (Figures 7a-c). During the wet summer (characterized by high discharge and relatively calm wind/waves), the PRE and continental shelf receive 95.17% of the annual sediment input (Figures 3a and 7a). Of this, about two-thirds is retained in the "Proximal" region (Figure 7a). Influenced by the prevailing southerly winds and northeastward shelf currents, 13.01% of the annual sediment load is retained in the "Eastern" and "Southeastern" regions (Figure 7a). Meanwhile, the shelf west of the PRE (⑤-⑧ regions) retains 15.87% of the annual load, with the "Western" region alone accounting for 8.48% (Figure 7a). In contrast, only 0.92% and 2.3% enter the more remote "Gulf" and "Distal" regions, respectively (Figure 7a). The "Southern" region retains a mere 1.22% of the sediment (Figure 7a).

In the dry winter (characterized by low discharge and energetic winds/waves), the PRE and the continental shelf receive only 4.83% of the annual sediment load (Figures 3a and 7b). The sediment distribution during this season primarily reflects reworking of previously retained sediments from summer (Figure 7b). Retention in the "Proximal" region increases slightly (+1.38%)in retention, while retention decreases in the ②-⑥ regions. Much of this remobilized sediment is transported farther offshore and retained in the "Gulf" and "Distal" regions (Figure 7b).

The annual sediment budget reveals that 66.45% of the Pearl River sediment is retained in the "Proximal" region (Figure 7c). Additionally, 9.2% is retained in the "Eastern" and "Southeastern" regions (Figure 7c), primarily during summer (Figures

7a vs. 7c), while 24.12% is retained on the shelf west of the PRE (⑤-⑧ regions), with most of that occurring in the "Gulf" and "Distal" regions during winter (Figures 7b vs. 7c).

The annual deposition thickness of the Pearl River-derived sediments (Figure 7d) reveals significant deposition within the "Proximal" region, with many areas exceeding 10 mm despite wintertime resuspension and redistribution. In the "Eastern" region, deposition reaches a magnitude of 0.1 mm, while the inner shelf west of the PRE ("Western" and "Gulf" regions) exhibits significantly greater accumulation. For instance, the deposition west of the Chuanshan Islands reached a magnitude of 0.5 mm. In the "Gulf" region, deposition is primarily concentrated in the northeastern part, extending southwestward along the 30-60 m isobaths. Sediments transported southwestward along the east coast of Hainan Island and into the "Distal" regions remain largely suspended in the water column due to the greater water depth, with limited deposition on the seabed.

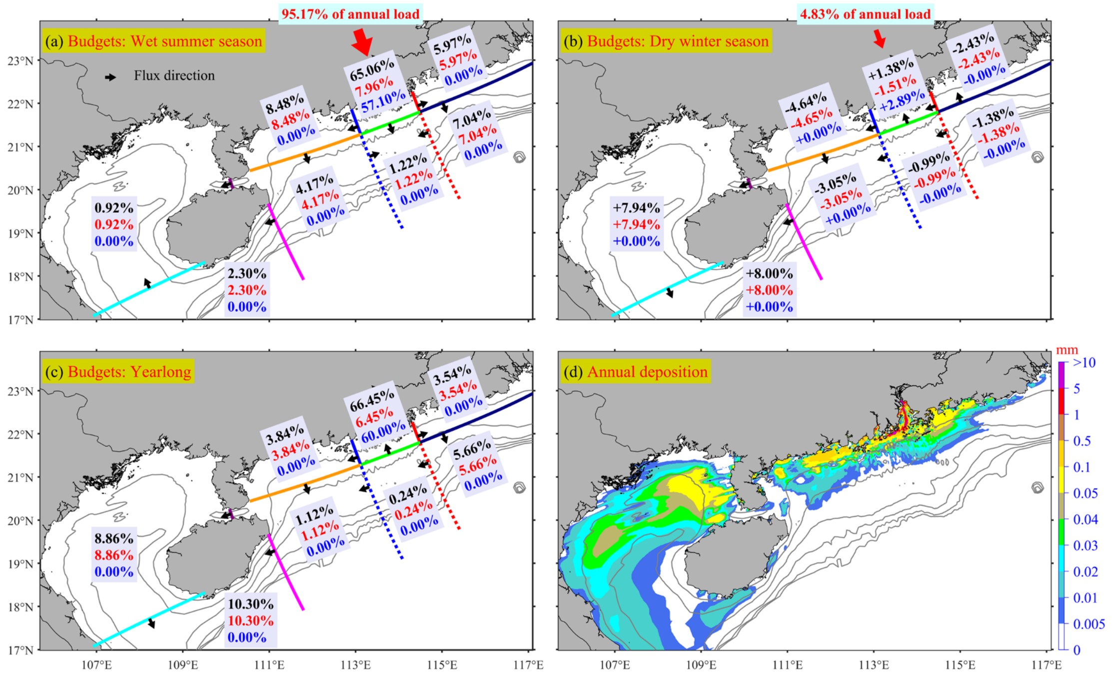

Figure 7. *Riverine sediment (classes 4 and 5 in Table 1) retention budget percentages at eight regions (see Figure 1) during (a) the wet summer season, (b) the dry winter season, and (c) the entire year in the Control run case. (d) the annual deposition patterns spanning from April 1st, 2017, to March 31st, 2018 in the Control Run. All percentages displayed in the figure are relative to the annual riverine sediment load (see Figure 3a). The black percentage values represent the combined total of riverine sediment Class 4 and Class 5, while the red and blue values denote sediment Class 4 and Class 5, respectively. Arrows indicate the direction of net riverine sediment flux at each transect during the specified period.*"

12. Figures 7 and 12: These figures are still confusing and there is probably a more effective way to present this data. The numbers are hard to see and hard to compare. The percentages are relative to the "total riverine load" which was 40% for class 4 and 60% for class 5. The blue numbers therefore show that *all* of class 5 is retained

in the PRE; except Figure 12(f) where 59.99% remains.

Response:

Thank you for your comments. The results presented in Figures 7 and 12 reflect our calculations accurately, showing that nearly all of Class 5 sediment is retained in the PRE, except for Figure 12(f) where about 59.99% remains. Regarding the clarity of the figures and numbers, we believe that the PDF version we uploaded is sufficiently clear for interpretation. However, we would appreciate any specific suggestions or examples from you on how we might improve the presentation for better readability and comparison.

13. Table 3: this is interesting but seems tangential to the paper and adds ½ a page to an already long paper.

Response: Thank you for the suggestion. We have removed Table 3 as recommended.

Text suggestions:

14. The writing could be improved. I will not try to provide an exhaustive list, but here are some specific examples.

Response:

Thank you for the valuable feedback. We have carefully revised the manuscript to improve the writing throughout.

15. Line 104: “SCS shaped by the East Asian Monsoon”. That is confusing, the sea is

not “shaped” by the monsoon.

Response:

Thank you for the comment. We have revised the sentence to read “*The northern SCS, under the influence of the East Asian Monsoon,*” to avoid the confusion caused by “shaped by the East Asian Monsoon.”

16. Line 143: “Holocene sedimentary processes”: do you mean “Holocene sedimentary deposits”?

Response:

Thank you for the comment. We have revised “Holocene sedimentary processes” to “Holocene sedimentary deposits” as suggested.

17. Line 156: suggest the word “factors” or “drivers” instead of “processes”.

Response:

Thank you for the helpful suggestion. We have replaced the word “processes” with “drivers” in the manuscript as recommended.

18. Line 241: suggest “Water level and current velocity”.

Response:

Thank you for the suggestion. We have revised the phrase to “Water level and current velocity” as recommended.

19. Line 296, 300 and elsewhere: suggest “1,981” instead of “1981”.

Response:

Thank you for the suggestion. We have revised the number format to “1,981” instead of “1981” as recommended.

20. Line 743: “Vectors show the seasonal mean surface current fields in each experiment”. This is unclear because the figure panels are difference plots; not plots for a single experiment.

Response:

Thank you for your comment. We confirm that the figures are indeed plotted this way, following the approach used in previous studies. Specifically, Figures 9 and 10 present the seasonal surface currents and the differences in suspended sediment concentration (SSC) between the Control and sensitivity runs. The colors indicate riverine SSC differences between model runs, while the arrows represent surface currents from the non-Control runs.

This presentation allows us to show both the circulation patterns in each experiment and the differences in riverine SSC relative to the Control run. Because the experiments in Figure 10 share the same hydrodynamics as the Control run, comparing Figures 9 and 10 reveals the circulation differences between NTS, NWS, NAS, and the Control case.

This style of presenting both velocity vectors and SSC differences by comparing experimental cases with a Control run has been commonly used. For example, Xue et al. (2012) illustrated current velocity and SSC differences in their Figures 5 and 6 by comparing experimental cases with the Control run on the Mekong River Shelf (Continental Shelf Research, 37, 66–78).

References

Zuo Xue, Ruoying He, J. Paul Liu, and John C. Warner (2012). Modeling transport and deposition of the Mekong River sediment. Continental Shelf Research, 37, 66–78. https://doi.org/10.1016/j.csr.2012.02.010.

21. Line 901: suggest "all flocs in the model are retained…". Can the authors use this model result to conclude something about the processes that are important?

Response:

Thank you for your valuable suggestion. We have revised the text accordingly. Our model results indicate that high-settling flocs are largely retained within the "Proximal" region, suggesting that these sediment particles have limited transport beyond this area. This highlights the importance of settling velocity in controlling sediment retention and dispersal processes in the region.

22. Avoid terms in the conclusion that are very specific to this paper; e.g. "regions 3-4" (Line 1150).

Response:

Thank you for your valuable suggestion. We have revised the manuscript to ensure that specific region names such as "regions 3-4" are no longer used in the conclusion and abstract sections.

23. Data availability: The authors' data products should be listed at the beginning instead of at the end of this section. Lines 1193 – 1194 provide the link to the authors' own data products. Lines 1181 – 1192 provide links to data that were not generated by the authors.

Response:

Thank you for your valuable suggestion. We have rearranged the Data Availability section accordingly, placing the links to our own data products at the beginning, followed by links to data not generated by us.

Response to Referee 3 (Report #1)

I have read the revision and the responses to both reviews of the second submission. I think the authors have done a satisfying job in responding to all comments and have provided a new manuscript that presents its results much more successfully. The submission communicates the dominant processes that lead to the spreading of river-derived sediment over the continental shelf well.

I advise accepting this paper for publication, after some technical adjustments:

Response:

Thank you very much for your positive evaluation and constructive suggestions. We appreciate your time and effort in reviewing our manuscript and will carefully address the technical adjustments as advised.

1. Line 99: Is it possible to make the URL a reference?

Response:

Thank you for the suggestion. We have revised the sentence to cite the source as a reference, as shown below:

"*The present average annual (2001-2022) freshwater and riverine sediment loads are* 2.74×10^{11} m^3 *and* 2.84×10^7 *tons, respectively (Ministry of Water Resources of the PRC, 2022).*

References

Ministry of Water Resources of the PRC. 2022. Bulletin of River Sediment in China. http://www.mwr.gov.cn/sj/#tjgb."

2. Lines 160-163 and Line 171. In my opinion, manuscript-specific jargon should not appear in the introduction. This especially refers to the references to sediment classes and Table 1 in lines 156 – 175. Please consider rephrasing to retain generality in the introduction and specify further details in the methodology section 2.

Response:

Thank you for the insightful comment. We agree that manuscript-specific jargon, such as sediment classes and references to Table 1, is better suited for the methodology section. Accordingly, we have revised the introduction to maintain generality and moved the detailed descriptions of sediment classification and related references to Section 2.

3. Line 278, please consider more commonly used variables such as c or C_s for concentration and Q for discharge, similar to Zhang et al., (2012).

Response:

Thank you for the helpful suggestion. We have revised the notation to adopt more commonly used variables, specifically C_s for concentration and Q for discharge, in line with the conventions used by Zhang et al. (2012).

4. Line 299, Figure 2: Please rephrase "After realistic reworking" to "After spin-up"

Response:

Thank you for the suggestion. We have revised the label in Figure 2 from "After realistic reworking" to "After spin-up" as recommended. The updated Figure 2 and its caption have been modified accordingly.

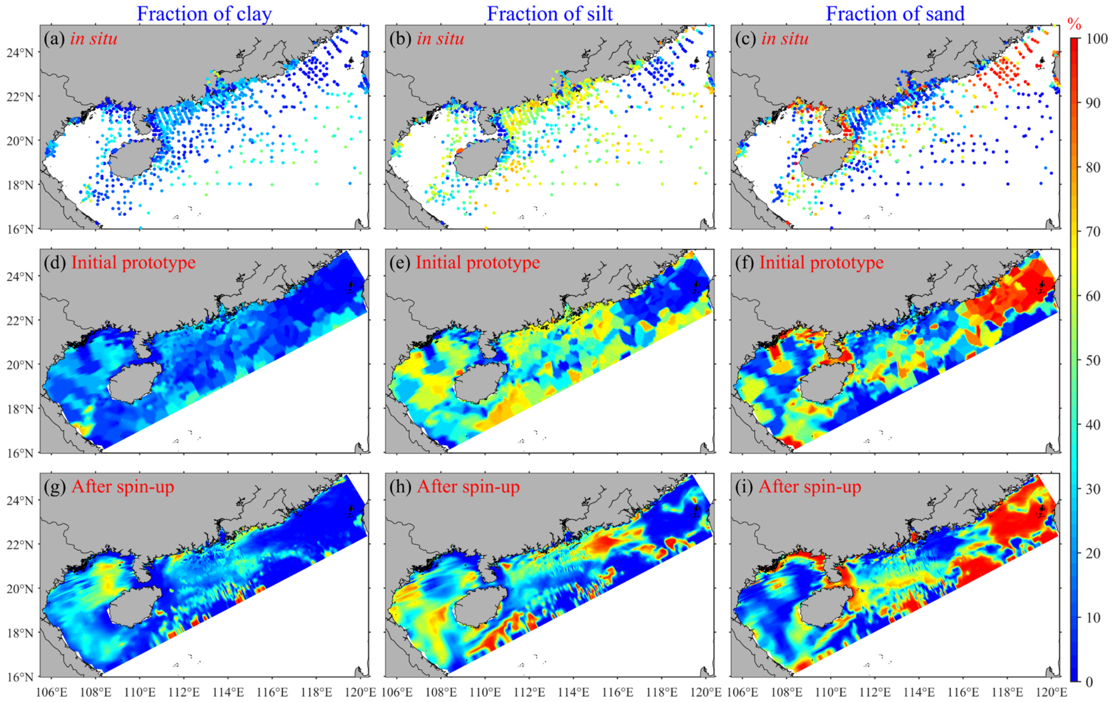

Figure 2. Row 1 presents the spatial distribution patterns of seabed sediment fractions derived from 1,981 sampling sites, while Row 2 demonstrates the initial spatial distribution prototype of seabed sediment fractions developed based on the observational data presented in Row 1. Row 3 shows the spatial distribution patterns of seabed sediment fractions following the completion of spin-up phase in the Control run case on April 1st, 2017, with Columns 1, 2, and 3 representing the fractions of clay, silt, and sand, respectively.

5. Line 651, Figure 7: Suggestion, is it worth it to add red and blue arrows as well to indicate the flux direction of each sediment class?

Response:

Thank you for the suggestion. We appreciate the idea of adding arrows to indicate flux directions for each sediment class. However, we believe this may not be necessary, as Class 5 sediments are largely trapped within the Pearl River Estuary (PRE), and the sediments transported over the shelf primarily belong to Class 4. Therefore, adding additional arrows for other classes may introduce visual redundancy without significantly enhancing interpretability.

6. Line 703-706. Please rephrase this sentence. The words "while" and "but" together seem to be used incorrectly?

Response:

Thank you for your comment. We have revised the sentence accordingly. The updated sentence now reads: "*the NTS case demonstrates that tides significantly enhance bottom stress (Figures 8a–b), while having minimal impact on the mean circulation.*"

7. Line 876: Please consider removing the term "realistic reworking".

Response:

Thank you for the suggestion. We have removed the term "realistic reworking" as recommended.

8. Line 1067 and Line 1072, Table 3: While I appreciate the efforts of adding this information, I do not think it is a suitable addition to this manuscript. Highlighting

that the PRE is dominant, with 83% of the sediment load, is sufficient to attain the goal of the paper. Advise is to remove Table 3.

Response:

Thank you for the suggestion. We have removed Table 3 as recommended.

9. Lines 1167-170. To me, this was a confusing addition for me at the end of the paper. Please avoid introducing a new term and explaining its meaning (Cycle2) in the last sentence of the manuscript.

Response:

Thank you for the comment. We agree that introducing a new term at the end could be confusing, and we have removed the related content accordingly.