



EGUsphere, referee comment RC4  
<https://doi.org/10.5194/egusphere-2022-199-RC4>, 2022  
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## Reply on AC1

Anonymous Referee #1

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Referee comment on "Numerical Modeling Investigation of Flushing Characteristics and Water Age in a Highly Stratified estuary: Mobile Bay, Alabama, U.S.A." by Gaurav Savant and Tate O. McAlpin, EGU sphere, <https://doi.org/10.5194/egusphere-2022-199-RC4>, 2022

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I appreciate the additional information and discussion that the author's response has provided. However, it was surprising to me that almost no changes in the manuscript were made (as far as I can tell from the response). I am a bit clumsy with the online interactive review system may I missed something.

The author's do make a case that this work is an incremental contribution and better highlight what is new relative to the work of Du et al. 2018. Yet, they have not incorporated this into the manuscript. As is, the manuscript does not give a sense of the similarity and differences between these works. Again, I feel the authors need to better highlight how the primary findings are consistent with Du et al. (2018) in a number of places (e.g., importance of baroclinic in flushing time, the relationship be river discharge and flushing time, the circulation maps, etc.) as well as some of the differences, particularly in the introduction, methods, discussion and conclusions.

I will say that the differences are very Mobile Bay centric without much generalization. Are there not broader things we can learn from this work? Ideally, the authors could add a paragraph or section about what new general understanding has been found?

I can live with the work staying locally focused, but the author should at least better frame the work in the context with previous work. The original manuscript does reference Du et al. 2018, but they do it way the does not clearly highlight the similarities and differences in the work. Why not include all the points made in the response to reviewer comments with the manuscript? Why not clearly state "provide ample evidence that the science of flushing and other mixing processes in Mobile Bay is unsettled" in the manuscript. Importantly, what might provide clarity? Is vertical resolution the main key? I realize you state this is out of the scope of the work, but to me that is what might make this work interesting to a broader audience.

Authors response on the importance of Coriolis was not particularly effective. But revisiting that section of the manuscript, I think a more compelling argument would be

that “The 2.5 day increase in flushing time represents a ~17% increase in flushing time relative to the case without Coriolis.” Again, Figure 15 still does not strike me as significantly different. Only one region of the upper portion of the lower bay is notably different. To me this is a second order effect. I am not saying that it is not worth noting, but in my opinion it seems to be second order and should be framed that way.

Note the one minor change to the manuscript is very limited. There is a time series from Middle Bay Light station directly adjacent to the ship channel ([https://arcos.disl.org/stations/disl\\_stations?stationnew=188](https://arcos.disl.org/stations/disl_stations?stationnew=188))

with profiling hydrographic data for years including the study period. I am not saying this data needs to be used and technically the authors' revised statement is correct as the station is not directly in the ship channel, but it makes it seem like there is no data time series data near the ship channel. I guess it is fine since it is technically not incorrect. But the state makes it seem like there is no data available, rather than the authors either being unaware of the data or choosing not to use it. Maybe it is better to just remove the statement or at least remove the salinity part.

Since almost no changes to the manuscript have been made, I am generally left with the similar comments that I provided in the first reviewer comments. Overall, if Ocean Science is okay with local focused studies, then I would recommend publication after revisions (to me having the context of what Du et al. 2018 did is important to this work given the similarity in methodology and results), if not then the manuscript should be rejected (or further change regarding broader findings is needed). To me this is an editorial decision.