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Comment on egusphere-2022-199

Anonymous Referee #1

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, EGUsphere, https://doi.org/10.5194/egusphere-2022-199-RC1, 2022

The manuscript focuses on demonstrating the validation of a 3-D numerical hydrodynamical model (Adaptive Hydraulics) that uses an implicit time-stepping method to investigate the flushing characteristics and water age in a highly stratified system (i.e. Mobile Bay).

The manuscript it appears to be surprisingly similar to work that has already been done in this system. It appears to be a near complete re-hash of Du et al. 2018 with nearly the same methods (flushing time and water age, new ocean flux) and a number of figures that look extremely similar. I guess this speaks to the reproducibility of Du et al. (2018) with a different numerical model, which is good. But what is new and novel about this work?

The main findings are basically same as that of Du et al. (i.e. similar methods provided similar results and conclusions) so it feels a bit strange as why this work is so duplicative. It is clear that the authors are aware of Du et al. 2018 as they reference the manuscript several times and even note in the conclusions that "This behavior is consistent with that reported by Du et al. (2018)." There was even overlap in the simulation years. Du et al. simulated 2008.7 to 2010.6 which has a ½ year overlap.

I can understand taking the same methodology and applying it to a different shallow stratified system to see if the findings hold in a general way, but to do the same analysis in the system seems redundant.

The were some difference in the findings but these seemed like minor differences as well as some additional information on various aspects in the system (i.e. impact of Coriolis).

The authors need to clearly highlight that this work largely duplicates the Du et al. (2018) and why such duplication was pursued. I would better highlight how the prime 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.) particularly in the introduction, methods, and conclusions. To me, it seems like the authors need clearly credit the finding Du et al. (2018) as their work is duplicative.

I am not sure whether duplicative studies are okay with Ocean Science, if they are than I would recommend publication after major revisions, if not then the manuscript should be rejected. To me this is an editorial descision.

Minor comments

Section 3.1 – Maybe show a plot of the grid mesh? The shoal-channel region of the mesh is a particularly interesting region to get a sense of how the mesh resolved this area.

Section 3.3 Line 232-234 – I believe there may have been persistent salinity measurement during this time period that are publicly available. Looks like several station in the ARCOS network maybe have had continuous salinity data during this time period (https://arcos.disl.org/). Not saying they have to be used, but it is probably not correct to say there are not data.

Figure 13. Caption: Maybe use 'difference' rather than 'stratification'

Figure 15 – The impact of Coriolis looks second order to me. At first glance the water age maps look quite similar. The main differences are in the lower mid-bay region which seems like a pretty subtle change me.

Reference:

Du, J., Park, K., Shen, J., Dzwonkowski, B., Yu, X. and Yoon, B.I., 2018. Role of baroclinic processes on flushing characteristics in a highly stratified estuarine system, Mobile Bay, Alabama. Journal of Geophysical Research: Oceans, 123(7), pp.4518-4537.