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Interactive comment on "A hydrodynamic model for Galveston Bay and the shelf in the northwestern Gulf of Mexico" *by* Jiabi Du et al.

Anonymous Referee #2

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Manuscript entitled "A hydrodynamic model for Galveston Bay and the shelf in the northwestern Gulf of Mexico" by Jiabi Du et al. presents study of the influences and effects that seasonal wind forcing has on the salinity distribution along the Louisiana shelf with focus on Galveston Bay. As this problem is having multi-scale dependency i.e. shelf dynamics has important influence on the coastal dynamics and vice versa, unstructured modelling approach seems to give reasonable answers and is appropriate. Manuscript is well organised, with some additional information should be valid contribution and appropriate for the journal.

General comments: Study is covering big portion of work done, however I think readers would benefit from clear and possibly additional explanation of baseline method used in the study. In other words, it is not clear to me if authors assumed and explored

exclusively wind (January vs. July) effects in the 3 numerical simulations (Jan-G, Jan-GAM, July-GAM) using the same wind field (Jan in first 2 or July in 3rd) replicated in time during the whole year without using any heat flux (or other atmospheric model forcing) or tidal forcing. One table listing used/or not used assumptions would help (i.e. no heat flux, no boundary conditions from Hycom, no tides, initial field, winds from Jan or July replicated during the whole simulation). If this is the case (when there is no heat flux, but initial stratification), then the simulation represent only partly barotropic approach which is valid in shallow part of the domain and during the winter only - as there is no vertical heat flux supporting vertical stratification in balance with vertical mixing parametrised with turbulence. I would be surprised that model didn't vertically mixed the whole vertical column as 1 year of simulation is quite enough time. In the case they used vertically uniform density field for start then I have doubts it does represent valid approximation of GOM in July. Possibly, then more correct title of the experiments would be to call (and explain) those sensitivity experiments as sensitivity of the salinity field to the wind field effects (and not mix that with July/January as seasons) of the simple barotropic system interacting with coastline and bathymetry. In that case dynamics will be only due to buoyancy effects of the rivers via salinity and some wind mixing/transport without any temperature variations. Validation period using full forcing is then confirmation of model setup and tunning. Part with residence time (336- 351) seems as added to the manuscript without needed explanation of method MacCready (2011). Authors could at lest give basic equations for completeness of the study and to show how they computed values.

Specific comments: In Abstract; 1) I think the main mesage is to present RESULTS of the study using 3D SCHISM and not to present model (first sentence)? 2) If they used only Hycom boundary conditions then it is global model and not models (line 16), or if they used added tidal elevations then should state that precisely.

in Methodology (2.1): 1) line 90: Does Schism use simple 1 order Galerkin method for momentum of higher order (as it does for tracers)? If not does the authors think this is

not relevant for the study where wind dynamics and momentum plays important role?

in Forcing conditions (2.3): 1) line 124: model or models? 2) line 142: what was used to compute heat/momentum/fresh water processes between ocean and atmosphere? If this is bulk flux then they should reference. 3) line 146: definition of sub-tidal period for boundary condition filtering was set to 15 days and later in the text they use 2 days? Is there particular reason why they chose 15 days and not less (i.e. 2 days) which would allow for inclusion of eddy dynamics embedded in Hycom model?

in Numerical experiments (2.4): 1) line 150-152: Authors used constant and the same river flux in Galveston Bay during the whole year in all 3 experiments? Did they used the same and constant fluxes for GAM in experiments Jan-GAM and July-GAM? What were the values? 2) line 156: methodology of replicating January wind during the whole year is a bit strange; as it captures some variability within month that is replicated 12 times. What would make more sense is to use "typical winter / summer" case where they could compute multi-year mean wind field from ECMWF fileds. Otherwise January/July as generic names have different meaning (authors used specific 2008 winds so they are not really generic i.e. seasonal in strict definition). 3) line 160: I am not sure what authors mean with computing boundary conditions form 2 years temporally constant?

in Water level (3.1): 1) in line 180: Why not to state what is the Cd equivalent to Manning coef as authors used quadratic bottom friction, instead of reporting Manning's coef? What was the method and how they tuned Manning coef is not really clear. 1) line 185: They speculate that low skill at Pilot Station is due to proximity of boundary conditions, which seems not plausibly as boundary conditions are fare away. Another point is that low-frequency MAE (i.e. boundary effect contribution) is much better than total which implies that other dynamics is important contribution to the MAE (Table 1)?

in Salinity (3.2): line 204: How authors explain lower MAE for global than low-passed filtered case in BOLI station? This seems hard to believe in mathematical sense, possibly some error.

in 4. (Remote influence): line 263-264: Sentence is not clear and make no sense: "Horizontally, their distribution influences but is also regulated by the shelf dynamics, and exhibits significant seasonal variation." lines 336-351 should include equation how they computed residence time.

in 5. (Summary): I think that authors should emphasise main results from their study and answers they provided on questions posed in the last paragraph of introduction (i.e. time needed for information originating at Mississippi-Atch rivers to arrive to GB? About extent and portion of seasonal influence of winds to the horizontal distribution of salinity etc.). This way written summary seems too short and doesn't summarise the study.

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