Response to comments by reviewer #2

Manuscript entitled "A hydrodynamic model for Galveston Bay and the shelf in the northwestern Gulf of Mexico" by Jiabi Du et al. presents a 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 organized, with some additional information should be valid contribution and appropriate for the journal.

Thanks very much for your comments, which have helped clarify multiple potentially confusing statements in the manuscript.

General comments:

1) 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).

The numerical experiments have virtually the same setting as in the realistic 2007-2008 model run, including tide, heat exchange, and ocean boundary conditions. The only differences from the realistic run are: (1) an initial salinity condition of 36 psu over the entire domain; (2) using repeated monthly wind forcing of January or July 2008; (3) long-term mean constant river discharges from three estuarine systems (Mississippi, Atchafalaya, and Galveston Bay). The table below shows the differences in numerical experiments relative to the realistic run. With only three differences, we do not plan to add this table in the revised manuscript. Instead, we will add additional sentences describing the setting of the numerical experiments in the revised manuscript.

Table A: Settings for the numerical experiments.

	Jan-G	**Jan-GAM**	**Jul-GAM**
River discharge (m3/s)	Q(Trinity River)=190 Q(San Jacinto)=52 Q(Buffalo Bayou)=14 Q(Chocolate Bayou)=3	Q in Jan-G, plus Q(Atchafalaya River)=15525 Q (Mississippi River)=6664	
Wind	Repeated wind of January 2008		Repeated wind of July 2008
Initial condition for salinity	36 psu over the entire domain throughout the water column		
Initial condition for temperature	Same as in realistic model run, based on HYCOM output on 2007-01-01		

Air temperature, pressure, solar radiation	Same as in realistic model run, spatially and temporally varying from 2007-01-01
Ocean boundary condition	Same as in realistic model run, spatially and temporally varying from 2007-01-01

2) 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 parametrized 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.

The heat flux is included in numerical experiments. We will clarify this in the revised manuscript. The temperature profile at a station in the deep Gulf (Fig. A) indicates that in this baroclinic model run, the deep ocean is persistently stratified, with the surface temperature changing seasonally. The stratification at the bay mouth also persists (Fig. B).

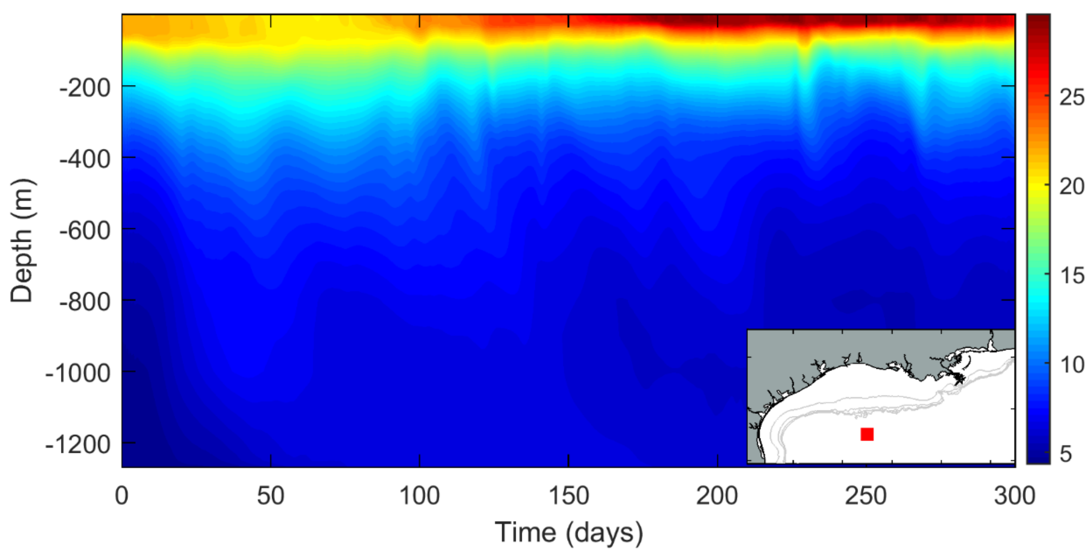

Figure A: The temperature profile (low-pass filtered with a cut-off period of 50 h) at a station in the deep Gulf (see the inset for the station location): the grey lines in the inset denoting the 50, 100, 150, and 200 m bathymetric contours. (Note: we do not plan to include this figure in the revised manuscript).

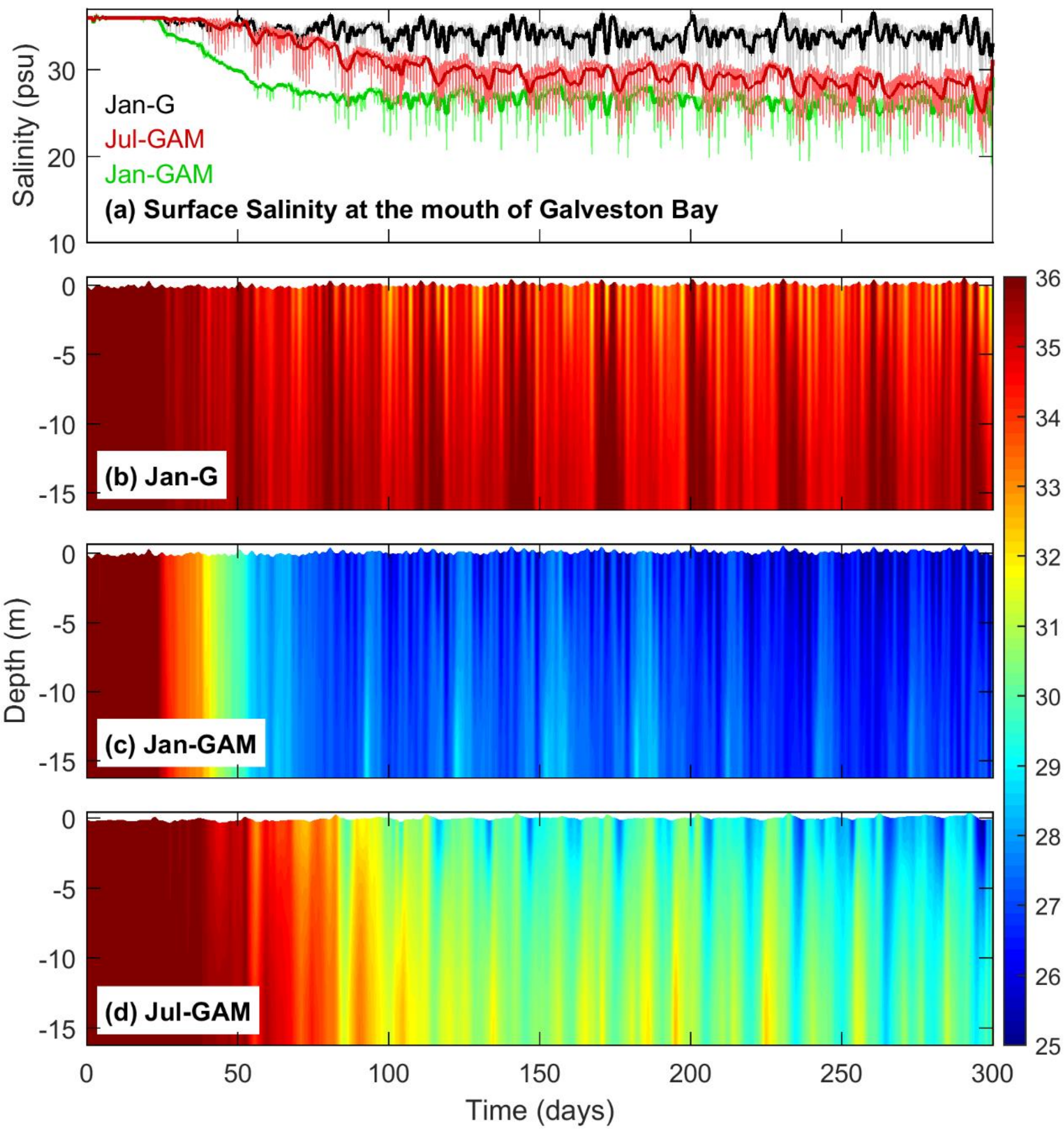

Figure B: (a) Surface salinity at the mouth of Galveston Bay (the bold lines indicating the sub-tidal salinity) and (b-d) the vertical salinity profiles at the mouth (low-passed filtered with a cut-off period of 50 h). The periodicity in (b-d) is the tropic-equatorial cycle. Note: we do not plan to include this figure (showing the water column is not well mixed in this a full 3D baroclinic mode run) in the revised manuscript.

3) Part with residence time (336-351) seems as added to the manuscript without needed explanation of method MacCready (2011). Authors could at least give basic equations for completeness of the study and to show how they computed values.

The equations for the salt flux including the salt flux decomposition and the TEF will be added. It will read:

"We also estimated total exchange flow (TEF) to quantify the overall change in estuarine water renewing efficiency using the isohaline framework method proposed by MacCready (2011), which was found to be a precise way to calculate the landward transport (Chen et al., 2012). In this method, the tidally averaged volume flux of water with salinity greater than *s* is defined as:

$$Q(s) = \left\langle \int_{A_s} u dA \right\rangle \quad (4)$$

where A_s is the tidally varying portion of the cross-section with salinity larger than *s*. In this study, we calculated $Q(s)$ for the limited salinity bins from 0 to 35 psu with an interval of 0.5 psu. The volume flux in a specific salinity class is defined as:

$$-\frac{\partial Q}{\partial s} = -\lim_{\delta s \to 0} \frac{Q(s + \delta s / 2) - Q(s - \delta s / 2)}{\delta s} \quad (5)$$

where the minus sign indicates that a positive value of $-\partial Q/\partial s$ corresponds to inflow for a given salinity class. The total exchange flow (Q_{in}) indicating the flux of water into the estuary due to all tidal and subtidal processes, is then calculated as:

$$Q_{in} \equiv \int \frac{-\partial Q}{\partial s}\Big|_{in} ds \quad (6)$$

The resulting salt flux into the estuary (F_{in}) is given by:

$$F_{in} = \int s(-\frac{\partial Q}{\partial s})\Big|_{in} ds \quad (7)$$

and the ratio of salt mass inside the estuary to the salt influx gives the mean residence time (T_{res}):

$$T_{res} = \frac{\int s dV}{F_{in}} \quad (8)$$

where *V* is the estuarine volume."

Specific comments:

In Abstract:

1) I think the main message is to present RESULTS of the study using 3D SCHISM and not to present model (first sentence)?

We agree. The first sentence in the Abstract will be changed to "A 3D unstructured-grid hydrodynamic model was developed for the northwestern Gulf of Mexico including main estuarine systems along the Texas-Louisiana coast, with a high-resolution horizontal grid and a hybrid vertical grid."

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.

We will revise it as suggested. It will read "HYCOM global model"

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?

We agree that it is not relevant to mention the Galerkin method here. We will revise the sentence as “It uses highly efficient semi-implicit finite-element/finite-volume methods with a Eulerian-Lagrangian algorithm to solve the turbulence-averaged Navier-Stokes equations, including continuity, momentum, salt-balance, and heat-balance equations, under the hydrostatic approximation.”

In Forcing conditions (2.3):
1) line 124: model or models?
We will revise “from the global models” to “from HYCOM global model”.

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.
The model uses the bulk aerodynamic module of Zeng et al. (1998). The reference will be added. A sentence reading “The bulk aerodynamic module of Zeng et al. (1998) is used to compute the air-sea heat exchange” will be added in the model introduction (Section 2.1).

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?
The global HYCOM doesn’t provide hourly output but one instantaneous output per day. Therefore, we used a longer cut-off period to obtain sub-tidal components. The eddy condition at the open boundary was not smoothed out by this filtering process, as meso-scale eddies (e.g., loop current eddies) move slowly in the Gulf.

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?
Yes. We used long-term mean constant discharges into Galveston Bay for all three numerical experiments and long-term mean constant discharges from the Mississippi and Atchafalaya rivers in Jan-GAM and Jul-GAM. We will add the values in the main text of the revised manuscript (see the values in Table A of this document).

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 fields. 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).
Variability in the wind is important in determining the fate of the Mississippi-Atchafalaya plume. Multi-year “mean” wind may lead to unrealistically strong stratification and weak wind mixing, as the averaging will smooth out the peak winds. The 2008 Jan and July winds are used as the wind-induced shelf currents in January and July 2008 represent typical seasonal variations in the northern Gulf. Wind roses for both months show the

dominant winds blowing from distinctively different directions, with the January wind mainly blowing from NE, E, and SE, while the July wind blowing mainly from S. Such distinctively different wind patterns cause great differences in shelf transport (Fig. D) and thus the distribution of low-salinity water from the Mississippi-Atchafalaya rivers.

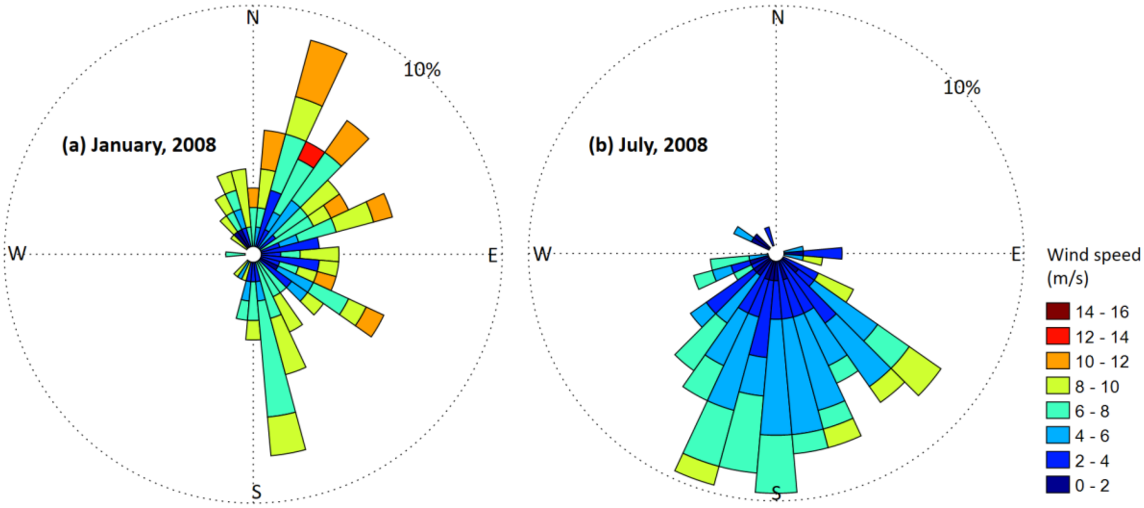

Figure C: Wind roses for January and July of 2008 at the Galveston Bay mouth. (Note: this figure will be put in supplemental material in the revised manuscript).

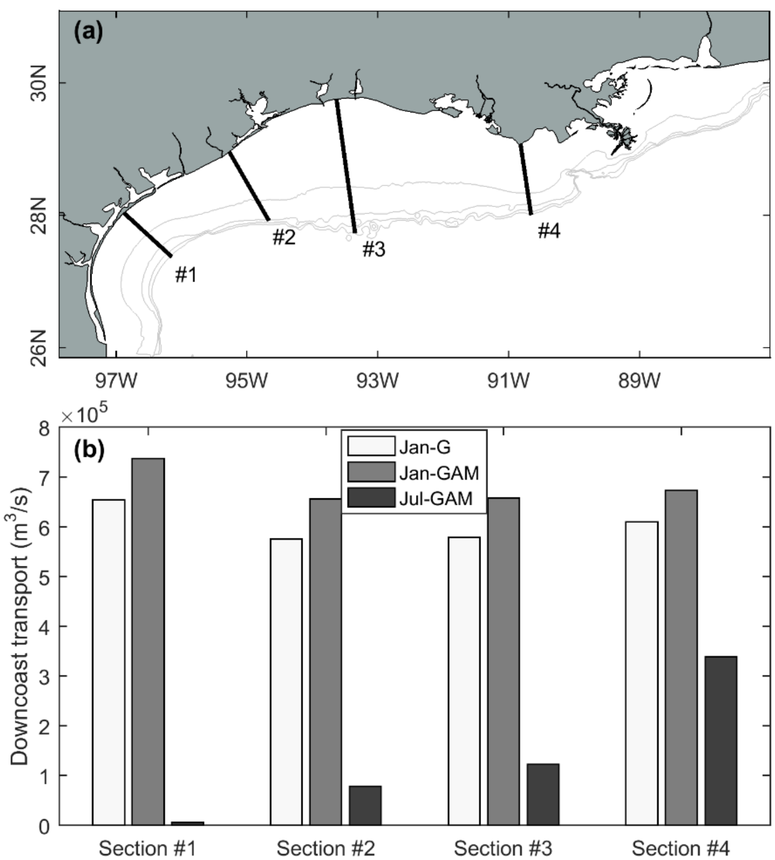

Figure D: (a) The location of four selected cross-shelf sections on the Texas-Louisiana shelf and (b) the downcoast shelf transport for three numerical experiments. The grey lines in (a) indicate the 50, 100, 150, and 200 m bathymetric contours. (Note: this figure will be added in the revised manuscript).

3) line 160: I am not sure what authors mean with computing boundary conditions form 2 years temporally constant?
To make the wind field the only controlling factor in the numerical experiments, we used the realistic boundary conditions in the 2007-2008 model run for all other variables. The sentence in the text is mistaken. It will read in the revised manuscript as "Except for the wind forcing, river input, and initial salinity condition, the numerical experiments use the same model configuration as in the realistic 2007-2008 model run."

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.
The drag coefficient, calculated in the model as a function of the Manning coefficient and total water depth, varies spatially and temporally. It is therefore not feasible to provide an equivalent value of the drag coefficient for the given Manning coefficient.

For the model calibration, we carried out multiple model runs with different Manning coefficient ranging from 0.015 to 0.025 and chose 0.016 as it gives the best reproduction of the tidal amplitude and phase. Figure E shows how the harmonic water level is reproduced by the model with the Manning coefficient of 0.016.

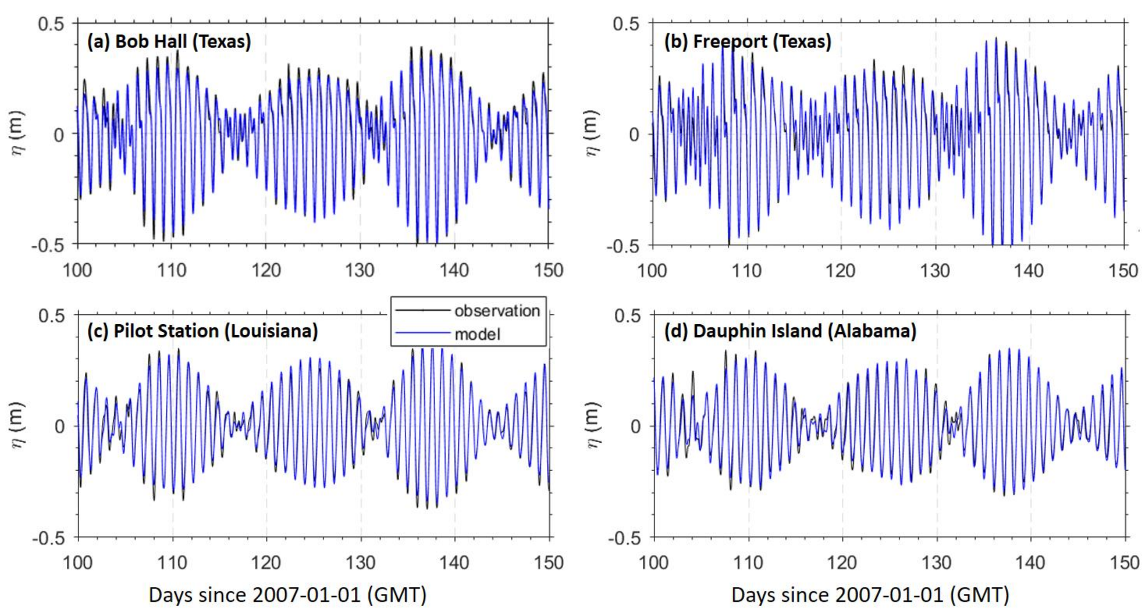

Figure E: Harmonic water level comparison between model and observation at four selected stations: Bob Hall (Texas), Freeport (Texas), Pilot Station (Louisiana), and Dauphin Island (Alabama). (Note this figure will be added in the supplemental material).

2) 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)?
Thanks for your careful reading. We double checked the model-observation statistics and found an error for the Pilot Station. For the total water level, the *MAE* and skill are 7 cm and 0.93; we will update the table. Pilot Station and Dauphin Island show the poorest skills for the subtidal water level. The revised manuscript will have "The *MAE* is in the range of 7-9 cm and 5-7 cm for the total and subtidal components, respectively. The model skill varies spatially, with relatively low skills for the subtidal components at Pilot Station and Dauphin Island."

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.
We double checked the model-observation statistics and found an error for the *MAE* at BOLI. The *MAE* is 4 psu for total salinity and 4 psu for subtidal; we will update the table. We also checked all other statistics in the table (thanks for your careful reading).

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."
To make the sentence clearer, we will revise it as "Horizontally, their distribution is regulated greatly by the shelf dynamics and thus usually exhibits significant seasonal variation due to seasonal variation of wind and shelf circulation."

lines 336-351 should include equation how they computed residence time.
Equations for the salt flux decomposition, the TEF, and the residence time will be added in the revised manuscript.

In 5. (Summary): I think that authors should emphasize 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 summaries the study.
It is a good suggestion to summarize what we found from the numerical experiments. We also plan to add additional analysis for the mixing and shelf transport under the influence of Mississippi-Atchafalaya discharge and different wind conditions (see Fig. F).

We will add the following paragraph (as the 2nd paragraph) to Summary:
"Three numerical experiments were carried out to examine the extent to which the neighboring major rivers influence a local coastal system. The Mississippi-Atchafalaya discharge has great influence on the salinity regime along the Texas coast and its influence depends on the wind forcing and the resulting shelf circulation. Winter wind causes a stronger downcoast shelf transport, an order of magnitude larger than that during

summer (Fig. D), transporting the Mississippi-Atchafalaya plume to the Texas coast. The mean discharge from the Mississippi-Atchafalaya rivers can lower the salinity by up to 10 psu at the mouth of Galveston Bay under winter wind. Lower salinity condition on the Texas shelf decreases the longitudinal salinity gradient inside the estuary, leading to a weakened estuarine circulation and weaker salt exchange. The vertical mixing is also sensitive to the wind forcing. The low-salinity water expands further offshore with summer wind, while it is constrained as a narrow ban against the coastline with winter wind. As a result, stratification is stronger over the shelf, inhibiting the vertical mixing on the shelf, during summer."

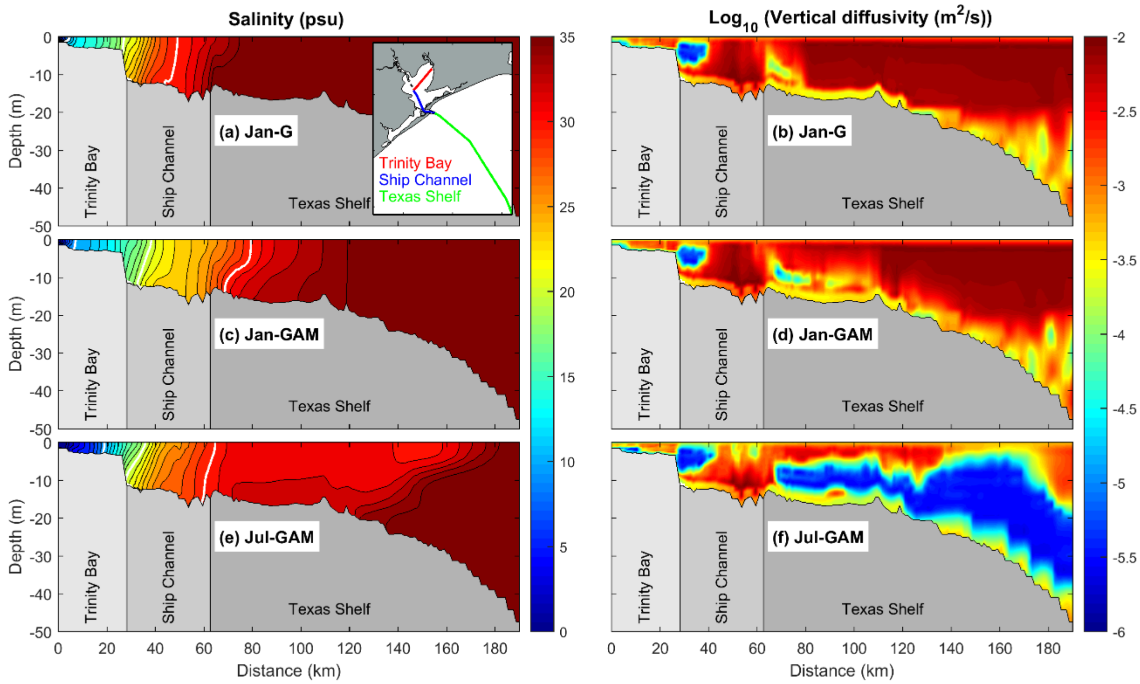

Figure F: Salinity (left panels) and vertical diffusivity (right panels) averaged over days 250-300 for the section through Trinity Bay, ship channel, and Texas shelf: see the inset in (a) for the section location. (Note: this figure will be added in the revised manuscript).