# Response to reviewer 2, of "Budget of the total nitrogen in the Yucatan Shelf: driving mechanisms through a physical-biogeochemical coupled model"

S. Estrada-Allis on behalf of all co-authors.

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Dear Reviewer, thank you so much for your time reading this paper and providing your valuable suggestions to improve it. Following are responses (in blue) to your detailed comments.

## 1 General Comments

In agreement with the reviewer general comment we have extended the model validation, especially for the Yucatan shelf (YS). More specifically, we compare the transport in Sv and current velocity with three sections of moorings located at the Yucatan Channel, and on the northern side of the shelf (Sheinbaum et al., 2002; Athié et al., 2015; Sheinbaum et al., 2016). The model transport, velocity and its standard deviation are in good agreement with the observed mean values reported in the literature. We added a salinity qualitative comparison against the observations reported in Enriquez et al. (2013). We also included a point-by-point comparison with salinity, temperature and chlorophyll profiles from 3 GOMEX cruises covering different periods. These are all the existing *in situ* observations available for the YS to the best of our knowledge. In order to show that the model reproduce the seasonality of the upwelling, the bottom shelf temperature is also shown. Moreover, the model sea level anomaly over the YS is compared with the merged satellite altimetry product from AVISO. We hope that all these new comparisons and model evaluations improve the robustness and reliability on the background model physics and dynamics. Additionally, a new subsection is added in order to acknowledging the impact of the model uncertainties in the quantification of the total nitrogen budget for the YS.

# 2 Specific Comments

## 2.1 Model Validation

While this study focuses on the YS and its vicinity, most of the convincing validation presented is for the whole Gulf of Mexico (GoM). There is a need to validate and characterize the background state in the YS in order to add credibility to the results. While I understand there is a scarcity of *in situ* observations in the YS, some vertical sections of temperature and salinity have been reported previously (e. g., Enriquez et al., 2013). There are also lots of *in situ* observations in the western boundary current (e.g., Sheinbaum et al., 2002, 2016) that could be used to convince the reader that the model physics is reliable. Specially those closer to the 250 m isobath if available. There is no need to do an exhaustive analysis of such observational datasets, but to show a congruence between model and observed mean background state.

In agreement with the reviewer, there are not many *in situ* observations available for the YS. To expand the model results validation in the YS, a comparison to T-S profiles for August 2016 and July 2018 are included. These are all the existing profiles for the YS to the best of our knowledge. A cross-section of the Yucatan Current is extracted from the model and compared to **?**.

The *in situ* biochemical observations were taken in Nov 2015, while the model solution is available until 2012. Ideally one would have a solution contemporaneous with the observations but if that is not possible then the approach presented is the best next choice: compare the mean and standard deviation for Nov with the observations. But then one wonders is the agreement with observations will hold for other months given that the authors present long-term means for the budgets. The authors seem to suggest that seasonality is weak, but I think that needs to be shown. For instance, does the inner shelf remains well mixed throughout the year as it seems to be the case in Nov?, or does some stratification develops during summer and if so, how that affects the budget?.

As in the comment above for the T-S profiles, we have added a comparison between chlorophyll profiles from different years (August 2016 and July 2018) temporal and spatially averaged for the whole YS. The mean and standard deviation profile of chlorophyll and salinity are presented to show the degree of variability within the shelf. Unfortunately, nutrient and particulate organic nitrogen observations are scarce.

A temporal series of the bottom shelf temperature is also added to the new manuscript to show the model is capable of representing the seasonal variability. More details about the stratification conditions of the shelf are also included. This is a relevant question in the sense that a more stratified system will prevent the uplift of nutrient-rich deep waters that enters into the shelf.

Similarly, more analysis can be done using satellite products to validate the

circulation in the YS. This is specially important to compensate for the scarcity of *in situ* observations. How does the observed Sea Level Anomaly correlates with that of the model in the YS? The authors could for instance compare the annual cycles of the aviso SLA and the model (without Mean Dynamic Topography) to convince the readers that the background model physics on the shelf is reliable. Is the pressure gradient across the shelf break well resolved by the model? A similar comparison could be done with scatterometer winds. The use of OSCAR currents, while very low resolution, might be possible given the wide YS. SST is not a robust validation set since the authors are using bulk fluxes at the surface and the CFSR model (I assume) is forced by satellite SST. Therefore the model is implicitly nudged to observed SST via the 2 m air temperature.

Following the reviewer suggestion, we improved the model validation by comparing Sea Level Anomaly from AVISO and the model. However, one should be careful when using the AVISO product in shallow areas and close to the shore. In addition, we use OSCAR (Ocean Surface Current Analysis Real-time) currents to evaluate the shelf model velocity. Thank you for your comment regarding the SST, this is taken into consideration for the basin-scale model validation.

## 2.2 Model Physics

One the main findings reported in the manuscript is that the input of N in the southeast part of the shelf comes from Ekman transport at the bottom boundary layer. The vertical stretching used was developed to study the surface vorticity balance and while this might be a wise choice for the whole GoM, it is not the best for the YS NT budget given the relevance of the bottom boundary layer. This limitation needs to be further analyzed and acknowledged in the manuscript. The authors could for instance estimate the bottom boundary layer thickness (a common estimate is thickness 0.4*frictionalvelocity/Corilisparamater, where frictional velocity is estimated from the bottom stress). How this thickness compares to the thickness the the first model sigma layer for the 250 m isobath?. That is, how well is the bottom boundary layer resolved?. If it is not very well resolved I still think the bulk characteristics of the budget will hold (i.e., the main sources and sinks and the TN pathways), but this limitation needs to be acknowledged.

We agree with the reviewer in the sense that the chosen vertical stretching (Azevedo Correia de Souza et al., 2015) was developed to provide higher resolution near the surface. In fact, in most areas of the study region important fluxes are concentrated near the surface (see for eg. manuscript figure 10). Therefore, the chosen vertical scheme is justified. That said, we agree with the reviewer that the bottom Ekman layer needs to be well presented for us to be able to draw conclusions on the balance and the physical processes modulating it. To verify the model is able to represent the this layer, its thickness was calculated

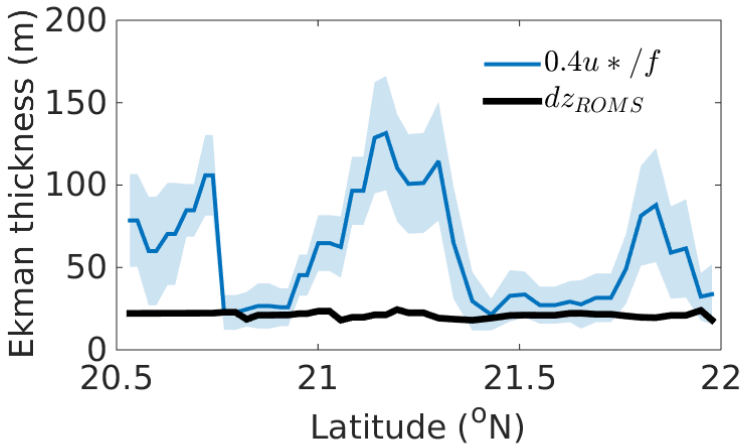


Figure 1: Bottom Ekman thickness, temporally averaged for the Yucatan Shelf. In blue is the Ekman thickness computed theoretically as shown in the legend. The friction velocity ($u^*$) is obtained from $\sqrt{\tau_{by}/\rho_o}$, where $\tau_{by}$ is the along-shelf bottom-stress, $\rho_o$ is the reference density of 1025 km m$^{-3}$, and $f$ is the Coriolis parameter. In black, is the bottom-most layer ($dz$) resolved by the model. Shadow areas denote the temporal standard deviation for one simulated year.

and compared to the model vertical grid (see Figure 1 below). It is shown that, despite the stretching used the model can properly reproduce both the surface and bottom Ekman layers. Even in periods when the velocity is very low, the bottom Ekamn layer is still larger than the model last layer. In these particular cases, the Ekman transport will not be important. Following the reviewer suggestion, comments are added to the text to make clear the limitations in the vertical resolution and the possible consequences.

## 2.3 Technical Corrections

Abstract:
L4: I think it should be "Coastal-Trapped Waves" or "Coastally Trapped Waves"

Thank you, this has been corrected as "Coastal-Trapped Waves" in the revised version.

L8: Define DIN or spell it out.

The DIN term is spell it out in the Abstract in the revised version.

Introduction:
L6: Processes

Thank you, this has been corrected in the revised version.

Section 2.1: L23: run

Thank you, this has been corrected in the revised version.

L25: In what sense it is "consistent with the observational data"?

To be time-consistent, i.e., in the sense that the model and observations match in the same time range, as far as possible.

L27: Is the boundary condition daily means? Monthly? What is done with the tides?

The boundary conditions are daily averaged. The tides are hourly and added as a separate spectral forcing at the boundaries. This is clarified in the revised version.

L30: Mention that the surface fluxes are computed using the bulk formulae for the marine boundary layer and provide a reference.

We mentioned this in the revised version, thanks.

Section 2.2 Formula (2) is confusing. While the model is in sigma levels the vertical integration is in depth (I hope so!) where the "dz" is the corresponding sigma layer thickness. The formula also uses summation indexes (x1:xn an y1:yn) as limits for integrals, which is very confusing. One option could be just to do a single integral over area elements dA. Also this expression is equated with the same abbreviation used for expression (1). Maybe use an overbar??

Effectively, the model is in sigma levels and the vertical integration is in the corresponding depth for each sigma level. We agree that formula (2) is confusing. In order to make the text more readable we have avoided this equation, which can be easily explained in the text of the revised manuscript as follows "Accordingly, the total budget is obtained as the integral over the area of the shelf and over the depth of the water column for the inner and the outer shelf".

L19: How the unknown groundwater sources were "inversely estimated"? How many? Where? What are their fluxes?

With "inversely" we mean that the groundwater sources are estimated by fitting the freshwater model sources to the scarce transport, salinity, temperature and nutrient data cited in the literature. We have avoided the term "inversely" to clarify this sentence. Moreover, we will add an extra appendix B to present examples of the monthly climatology of Mexican rivers and freshwater sources.

Section 3.1 is good enough but more work is needed in section 3.2 (regional validation) as suggested above.

Thank you for this suggestion. More validation is presented in the new version of the manuscript.

P7,L3: "Below 55 m the modeled and . . . "

Thank you, this has been corrected in the revised version.

P7,L4: "since there is no data assimilation". This sentence doesn't makes much sense since you are not comparing contemporaneous values.

Thank you, this sentence has been erased in the revised version.

P7,L4: "and ARE in"

Thank you, this has been corrected.

P7,L5: "To the best of our knowledge this is the first modeling study focusing on the nitrate budget of the YS."

Thank you, this has been corrected.

P7,L16: Usually the word "trend" is used for time, so I'll suggest erasing "along time".

Thank you, this has been corrected.

P7,L17: How Fig 8e compares to the model equivalent Chl? Does that also show the trend? Given the very large std in the Chl I can't help wonder how big are the error bars for the trend.

The model Chl compares well with the satellite Chl as shown in Figure 2. The model also exhibit the positive trend. The large std for the Chl is related with the strong variability given by upwelling episodes in the shelf. To better show the Chl trend one option is to avoid the std of the time series and show the values in the text as the temporal mean and its std, i.e., The value for the satellite Chl averaged over the shelf area and over the period of time between 2002-2010 is 0.38 $\pm$0.09 mgChl m$^{-3}$, similarly, the model shows a spatially-temporal mean of 0.36 $\pm$0.13 mgChl m$^{-3}$ for the same area and time period.

P7,L21: "associated with the seasonal cycle AND INTERANNUAL variability". The trend is presumably related to the interannual part. It could be good to compare that with the physics of the model such as the western boundary current strength, across-shelf pressure gradient, etc.

We agree with the reviewer, and further details on the causes of this trend will be investigated in the revised version of the manuscript, thank you.

P7,L31: "inner shelf"

Thank you, this has been corrected.

P8,L2-4: Is this the situation all year round? Is it well mixed all year?

The mixing state of the shelf will be analyzed in more detail in the revised manuscript.

P8,L11: Please indicate all the geographical locations mentioned in the text in Fig 1a (Campeche Basin, YS/Campeche Bank, Campeche shelf, etc.).

The geographical locations will be indicated in Fig. 1a.

P9,L6-7: Please rephrase.

The sentence has been rephrase as: "The CTWs are remotely forced by alongshore winds, propagating from the western GoM (Dubranna et al., 2011;

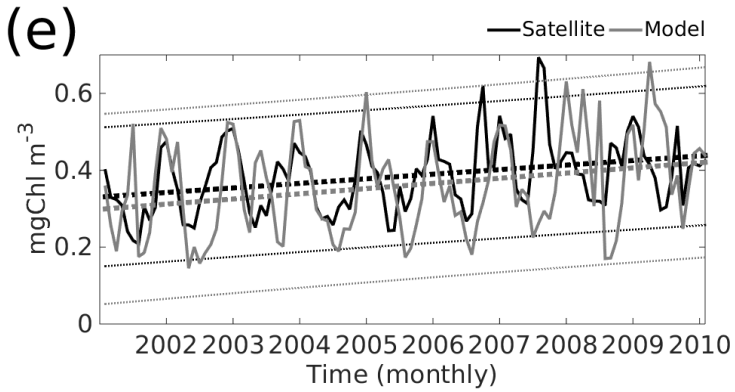


Figure 2: Temporal series of surface chlorophyll (mgChl $m^{-3}$) given by the satellite (thick gray line) and by the model (thick black line), averaged for the period of years between 2002-2010 and for the YS area. Thick dashed lines are the fitted trends for each temporal series, and thin dashed lines are the corresponding 95% confidence interval. Equations for the linear fits are $Chl_{trend}$ = 0.0010 month + 0.28 for satellite, and $Chl_{trend}$ = 0.0011 month + 0.30 for the model chlorophyll trend.

Jouanno et al., 2016)."

P9,L16 onwards. Maybe include a coherence plot of crosh-shelf velocity vs SLA and wind stress?

We will consider to include a coherence plot between the three variables.

P10,L1: EASTERN!

This has been corrected.

P10,L7: "Another". Revise the whole sentence.

The whole sentence is revised.

P10,L30: "due to"

This has been corrected.

P10,L31:shows or showed?

shows.

P11,L1: erase "produce bottom"

This has been erased, thank you.

P11,L3:"A similar mechanism has been found for the southern ... (Shaeffer et al., 2014)"

This has been modified, thank you.

P11,L13: revise the value of rho 0

This has been revised.

P11,L14: "We found that" ... The formulation used for the bottom Ekman

transport implies westward flow for a northward flowing western boundary current, so was that really a finding?

This has been revised.

P11,L17: How Fig 14c shows that "the Ekman transport is responsible for 65 % of the TN that is entering the shelf."?

The sentence is rephrase since the Figure 14 c does not show the percentage.

P11,L24: Rephrase. Maybe "The high-resolution modeling work of Jouanno et al. (2018) suggest that the bathymetric notch could be responsible for as a much as 50 % of the upwelling."?

The sentence has been rephrased, thank you.

P11,L28: change "but is not the unique" by "but is not the only process at work."

The sentence has been rephrased, thank you.

P12,L17: This is not shown at all. How do we know it is the case? All figures and figure captions need to be carefully revised. The sentence has been erased. All the figures and captions will be revised in the revised version of the manuscript.

Figure 8: Is it possible to combine (a)+(b) and (c)+(d)? And maybe include the model equivalent of (e)?

We will combine panels (a+b) and (c+d). The model equivalent is now included in the revised version (see previous Figure 2).

Figure 10: Plot the sections using actual depths instead of sigma levels.

Actual depths are shown in each sigma level. We will plot the sections using actual depths however.

Figure 11: Is this signal visible in the aviso analysis? The signal is strong enough but the temporal variability might be "too fast" for the altimeter constellation to catch it.

The signal from AVISO altimetry will be compared.

# References


Athié, G., J. Sheinbaum, R. Leben, J. Ochoa, M. R. Shannon, and J. Candela, 2015: Interannual variability in the yucatan channel flow. *Geophys. Res. Lett.*, **42**, 1496–1503, doi:10.1002/2014GL062674.

Azevedo Correia de Souza, J., B. Powell, A. C. Castillo-Trujillo, and P. Flament, 2015: The vorticity balance of the ocean surface in hawaii from a regional reanalysis. *Journal of Physical Oceanography*, **45 (2)**, 424–440, doi:10.1175/JPO-D-14-0074.1, URL https://doi.org/10.1175/JPO-D-14-0074.1, https://doi.org/10.1175/JPO-D-14-0074.1.

Dubranna, J., P. Pérez-Brunius, M. López, and J. Candela, 2011: Circulation over the continental shelf of the western and southwestern gulf of mexico. *Journal of Geophysical Research*, **116 (C08009)**, doi:10.1029/2011JC007007.

Enriquez, C., I. Mariño Tapia, G. Jeronimo, and L. Capurro-Filograsso, 2013: Thermohaline processes in a tropical coastal zone. *Continental Shelf Research*, **69**, 101–109, doi:10.1016/j.csr.2013.08.018.

Jouanno, J., K. Ochoa, E. Pallàs-Sanz, J. Sheinbaum, F. Andrade, J. Candela, and J. Molines, 2016: Loop current frontal eddies: Formation along the campeche bank and impact of coastally trapped waves. *Journal of Physical Oceanography*, **46 (11)**, 3339–3363, doi:10.1175/JPO-D-16-0052.1.

Sheinbaum, J., G. Athié, J. Candela, J. Ochoa, and R.-A. A., 2016: Structure and variability of the yucatan and loop currentsalong the slope and shelf break of the yucatan channel andcampeche bank. *Dynamics of Atmospheres and Oceans*, **76**, 217–239, doi:10.1016/j.dynatmoce.2016.08.001.

Sheinbaum, J., J. Candela, A. Badan, and J. Ochoa, 2002: Flow structure and transport in the yucatan channel. *Geophys. Res. Lett.*, **29 (1–6)**.