Review of the manuscript ***"Budget of the total nitrogen in the Yucatan Shelf: driving mechanisms through a physical-biogeochemical coupled model"*** *by Sheila N. Estrada-Allis et al.*

General comments

This work presents an estimation of the Total Nitrogen (TN) budget in the Yucatan Shelf (YS). The estimate is obtained using a coupled physical-biochemical model (ROMS), validated by some in-situ and satellite observations. The model solution is available for 9 year (2002-2010) while the in-situ observations used to validate the solution within the YS are available for Nov 2015. Physical processes that are relevant in explaining the estimated TN budget are identified and described. The main input of N is at the eastern boundary through the interaction of the western boundary current with the shelfbreak, presumably mainly due to Ekman transport at the bottom boundary layer. The imported N is then advected westward by the wind driven-circulation along the shelf. Most the N that enters the inner shelf (depths shallower than 50 m) is consumed by phytoplankton, and part of the N that enters the outer shelf (depths 50-250 m) is exported to the deep ocean in the west and northwest parts of the YS. This export of N is modulated by Coastally Trapped Waves with a typical period of ~10 days.

I think this manuscript addresses a relevant scientific question within the scope of BG, and the modeling results suggest a very interesting case for the relevance of likely physical processes controlling or modulating the import and export of N in and out of the YS. However, I think some revisions are needed in terms of validating the model, justifying the model physics or at least acknowledging the limitations and implications for the estimated budget, and the analysis and overall presentation of the work done. Below I outline some suggested revisions that might guide the authors when improving the manuscript.

Specific Comments

<u>Model validation:</u>

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 and 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.

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 lon-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?.

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.

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*frictional_velocity/Corilis_paramater, 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., tha main sources and sinks and the TN pathways), but this limitation needs to be acknowledged.

Technical Corrections

I noticed that the quality of the manuscript in terms of typos, clarity of the statements, grammar, etc. degrades towards the end. Please revise it carefully. Below is a list of some of them.

(L = Line, P = Page)
Abstract:
L4: I think it should be “Coastal-Trapped Waves” or “Coastally Trapped Waves”
L8: Define DIN or spell it out.
Introduction:
L6: Processes
Section 2.1:
L23: run
L25: In what sense it is “consistent with the observational data”?
L27: Is the boundary condition daily means? Monthly? What is done with the tides?
L30: Mention that the surface fluxes are computed using the bulk formulae for the marine boundary layer and provide a reference.
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??
L19: How the unknown groundwater sources were ”inversely estimated”? How many? Where? What are their fluxes?
Section 3.1 is good enough but more work is needed in section 3.2 (regional validation) as suggested above.
P7,L3: “Below 55 m the modeled and …”
P7,L4: “since there is no data assimilation”. This sentence doesn’t makes much sense since you are not comparing contemporaneous values.
P7,L4: “and ARE in”
P7,L5: “To the best of our knowledge this is the first modeling study focusing on the nitrate budget of the YS.”
P7,L16: Usually the word “trend” is used for time, so I’ll suggest erasing “along time”.
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.
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.
P7,L31: “inner shelf”
P8,L2-4: Is this the situation all year round? Is it well mixed all year?
P8,L11: Please indicate all the geographical locations mentioned in the text in Fig 1a (Campeche Basin, YS/Campeche Bank, Campeche shelf, etc.).

P9,L6-7: Please rephrase.
P9,L16 onwards. Maybe include a coherence plot of crosh-shelf velocity vs SLA and wind stress?
P10,L1: EASTERN!
P10,L7: “Another”. Revise the whole sentence.
P10,L30: “due to”
P10,L31:shows or showed?
P11,L1: erase “produce bottom”
P11,L3:”A similar mechanism has been found for the southern …(Shaeffer et al., 2014)”
P11,L13: revise the value of \rho_0
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?
P11,L17: How Fig 14c shows that "the Ekman transport is responsible for 65% of the TN that is entering the shelf."?
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.”?
P11,L28: change “but is not the unique” by “but is not the only process at work.”
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.
Figure 8: Is it possible to combine (a)+(b) and (c)+(d)? And maybe include the model equivalent of (e)?
Figure 10: Plot the sections using actual depths instead of sigma levels.
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.