Interactive comment on "Inter-shelf nutrient transport from the East China Sea as a major nutrient source supporting winter primary production on the northeast South China Sea shelf" by A. Han et al.

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Response to Anonymous Referee #1

Comments:
This study provides estimate of the contribution of nutrient from the East China Sea to the northeast South China Sea in winter. The estimates are derived from combining in situ survey of hydrography, nutrients and chl a (with support from satellite remote sensing), knowledge of flow fields from observation and numerical modeling. The authors made a significant effort to derive the estimation through making best use of the available information from different sources.
The paper is well written. I only have a few minor points of comments. I recommend the manuscript to be accepted for publication after a minor revision.

[Response] We appreciate the very positive comments from the reviewer.

1. To further convince a reader about the robustness of the conclusion, one should exam the inter-annual variability, or the sensitivity of the nutrient flux to varying nutrient distribution and oceanic flow. The present work sets a step for the further study.

[Response] We fully agreed that the present work sets a step for the further study to examine the inter-annual variability. The present estimate was based on the best knowledge/data available but is subject to uncertainties, which are associated with the variability in both nutrient concentrations and the volume transport. We point out, as reasoned in our MS that there was no noticeable inter-winter variations in DIN concentrations in the CCC of the TWS segment based on the available observational data (although very limited), and the estimated transport velocity based on our numerical model we adopted is as a matter of fact able to reasonably well validated by the observations that are however limited and within a variation range. We are to further address this in responding to the general comments raised by Reviewer 2.

2. Detail description of the ROMS model is apparently provided in Gan et al (2013) which is not included in the list of references. The model is driven by climatological surface and lateral boundary forcing. I am particularly curious about the initial condition and the length of simulation. Such information should be included in 2.3.

[Response] Gan et al. (2013) (Gan, J., Liang, L., and Liu, Z.: Transport and exchange in China Seas: Modeling Study) is in preparation. However, we have added the information of the initial condition and the length of simulation in our revision. The initial conditions for temperature and salinity field were from the World Ocean Atlas (WOA) 2005 (http://www.nodc.noaa/gov/OC5/WOA05/). The length of simulation is twenty years during which the model reaches quasi-steady state. In this study, we adopted model derived winter data from the last three years to calculate the average winter transport.

3. Beginning of 3.1: you cannot compare salinity with the observed SST; it is not clear the meaning of temperature increase from 12.1 to 16.9 deg C – apparently this is spatial variation.

[Response] Revised. We deleted the salinity description, and made a comparison between observed SST and satellite SST.The reviewer is right that the range in temperature is actually a spatial variation. We have revised "observed an increase in temperature from ~12.1-16.9^{o}C" into "observed that the water temperature ranged from ~12.1 to 16.9^{o}C".

4. Beginning of 3.3: the rapid decrease of DIN seaward, is it because lacking of coastal water or the entrainment of Kuroshio water?

[Response] The influence of the oligotrophic Kuroshio water and/or the decrease in terrestrial nutrient input may both contribute to the seaward decreasing in DIN. We have revised it into "due to the influence of the oligotrophic Kuroshio water and/or due to the decrease in terrestrial nutrient input.".

5. Section 4.2: the DIN flux is obtained by simply multiplying the average DIN and Tt in the CCC – clearly several significant assumptions are made here but are not explicitly mentioned. Because this is the most critical result of this study, a bit more justification can be very helpful. The 260 error bar is obtained by simply assuming the estimate of Tt has a zero error bar, is this correct? This is an over simplification and I do not feel comfortable to see the 260 error bar even appears in the abstract!

[Response] The comment has been taken. We have made more justification and more discussion on the uncertainty of the DIN flux estimation in the revised MS. The uncertainty in the DIN flux estimation is associated with the concentration of DIN in the CCC and the volume transport T_T. The error bars for both the DIN concentration and for the T_T were considered.

The length of model simulation is twenty years during which the model reaches quasi-steady state. And we adopted model derived winter data from the last three years for the T_T estimation. The modeled T_T was averaged from December, January and February, being 0.13 Sv, and the standard deviation was 0.09 Sv. Thus, the modeled T_T was 0.13±0.09 Sv.

In addition, based on our field observation during two winters and the previous reports, we can assume that there was no noticeable inter-winter variation in the DIN concentration in the CCC of the TWS segment. Moreover, considering that there was no significant vertical gradient for stations with water depth <30 m and was considerably stratification for stations with water depth >30 m, it is reasonable to use the average depth-integrated concentration of ~ 11.0 ± 2.0 μmol L^{-1} to be the average DIN in the CCC across the TWS.

Thus, DIN flux was estimated to be 1430 ± 1024 mol s^{-1}, using the averaged DIN in the CCC of ~ 11.0 ± 2.0 μmol L^{-1} and the modeled T_T of ~ 0.13 ± 0.09 Sv.

6. Conclusion: should “might be” at least be “may be”?

[Response] Agreed.