We thank reviewers for their comments on this manuscript. We have tried to address all comments, and please find our responses to comments below.

Anonymous Referee #3 comments:
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The manuscript applied a mass balance model based on N to two regions (GOM and CSK) with different N input source to estimate potential primary production (PPP) rate. Although similar box model has been used in many other studies, I still have some concerns about the model in this manuscript:

1. During the peak season of nutrient loading (May to July) of Mississippi-Atchafalaya River, P-limited primary production has been observed in the river plume. In the manuscript, one assumption for the box model is that "DIN is fully utilized by phytoplankton growth". Is this assumption appropriate for the brown zone in GOM? Will the model overestimate biomass in the brown zone in GOM?

   Response: Yes, it probably will. Rowe and Chapman (2002) pointed out that in the brown zone, high river discharge occurs together with high sediment loading which is likely to reduce productivity because of shading. However, as our model is estimating potential production rather than actual production in the brown zone this may not matter. This explanation is described in the main text (line 72-75) as follows:

   "They named these the brown, green, and blue zones (Figure 2). Nearest the river mouths is a 'brown' zone, where the nutrient concentrations are high, but the discharge of sediment from the river reduces light penetration and limits primary productivity within the plume."

2. Besides the nutrient and light, the temperature is another limitation for phytoplankton growth. In February, climatology SST in the CSK region is around 5 degrees celsius. As shown in Fig 7a, the brown zone has the highest PPP rate in February. I doubt very much if the model suitable for CSK region.

   Response: According to the ocean color remote sensing image near the CSK river mouth from Son et al., (2005), the results suggest that primary production in the CSK would not be limited by temperature. The PPP results of our model agreed with their ocean color remote sensing results in this region. Also, during all seasons, the Keum river consistently supplies high amounts of DIN (Lim et al., 2008) into the coastal water (especially in front of the Keum river mouth cell), and our model is of PPP based on DIN. Thus, in the Keum river mouth (brown zone in figure 7a), the higher value of PPP in the winter season is reasonable. This explanation is described in the main text (lines 405-414) as follows:

   "One question that has not been investigated is the temperature dependence of primary productivity in the two areas. While the GOM is temperate throughout the year, winter temperatures in the CSK fall to ~5°C. However, according to the ocean color remote sensing images from near the CSK river mouth reported by Son et al., (2005), primary production in the CSK does not appear to be strongly affected by temperature. The PPP results of our model

(0.2 to 2.2 gC m^{-2} day^{-1}) agreed with their ocean color remote sensing results (0.4 to 1.6 gC m^{-2} day^{-1}) in the CSK. Also, during all seasons, the Keum River consistently supplies high amounts of DIN (average: < 60 μM) (Lim et al., 2008) to the coastal zone (especially close to the Keum mouth). We believe, therefore, that the higher value of PPP in winter near the Keum mouth (brown zone in figure 7a), is reasonable."

3. The manuscript declared the current velocity data for the advective flow factor calculation in GOM but didn't for CSK.

   Response: We have addressed this point in another response (reviewer 1, #5 of general comments). CSK velocity was from Lim et al., 2008, and the range of current velocity in the CSK was similar with the range which we used in GOM. We explained more details of advection term in the main text (lines 166-170, lines 261-266) and the caption of Fig. 3 (lines 954-960) as follows:

   "As an output term, $\boldsymbol{F_{Export}^{DIN}}$ as an advection term was calculated from the current velocity in each region from observations (Nowlin et al., 1998a, b) and from literature data (Jacob et al., 2000; Lim et al., 2008) and the exchange between boxes from the residence time in each box. Note that water and nutrient exchange can take place through all four sides of each box, so the array is two-dimensional."

   "The annual current velocities in the CSK are more affected by tidal exchange and the presence of the Yellow Sea Current, but velocities are similar to those in the GOM (Jacob et al., 2000; Lim et al., 2008). The annual range of the currents is around 0 to 28 cm s^{-1} and 0 to 7 cm s^{-1} for the cross-shelf component. Thus, we used the mean value of the current velocity for the time of year during each cruise in both the GOM and the CSK for calculating the advective flow in both alongshore and onshore/offshore directions."

   Added to caption of Fig. 3: "Export N (Mixing) represents the advective transport term. The processes of biogeochemical and transport processes of both regions are the same and each in/out put factor is the same in the GOM and CSK. Note that transfer between boxes occurs in both directions alongshore and onshore/offshore and is not a one-dimensional process as suggested in the diagram."

4. The manuscript says that AD-N added N to the surface and enlarged green zone in the CSK all season. The AD-N "mainly came from" the China side. As we knew, this region is under the control of the East-Asian monsoon. If the AD-N primarily came from the China side, it should have a significant seasonal variation because of wind direction changes. Dose the AD-N input in CSK time vary? In Table 2, the authors list AN-D values from references but didn't contain AD-N they used in the model.

   Response: Not in our model. We fully agreed that AN-D in the CSK depends on wind direction, but we used the same AN-D concentration in each season because there is not enough observational data from both sides (Korea and China). Thus, at this point, all we want to say is that AN-D contributes considerably in the CSK region and this may need to be considered in future work. Based on this, we used the mean values of Asian data in table 2, which is initially 5 times higher than that of GOM. As we addressed this point in another response (reviewer 1, #4 of specific comments), we used different value of F_{Atmo}^{DIN} for the GOM and CSK and we added this information in the main text (lines 187-194) and the caption of Table 3 (lines 1003-1004) as follows:

"The mean value of Asian data, as shown in Table 2 (Kim (JY) et al., 2010; Luo et al., 2014; Shou et al., 2018; Zhao et al., 2015), is used for $\boldsymbol{F_{Atmo}^{DIN}}$ of the CSK region, which is initially five times higher than that of the GOM (1.4 X 10^5 mol day^{-1}; Wade and Sweet, 2008). We also considered vertical sinking as an input for the sub-pycnocline layer box and as an output from the upper layer. Other possible input factors might be upwelling/downwelling processes; however, these factors are neglected in the model because both regions are shallow and close inshore (Feng et al., 2014; Lim et al., 2008) and we have no observational data on upwelling/downwelling rates."

Added to caption of Table. 3: "*$\boldsymbol{F_{Atmo}^{DIN}}$ of CSK region is used as mean values of Asia data in Table 2, which is initially 5 times higher than that of GOM (1.4 X 10^5 mol day^{-1})."

5. In conclusion, "Our results agree well : : : and ocean color remote sensing in the MCK (Son et al., 2005)." Can authors add some details about the comparison in the "results" part?

   Response: This comparison is based on color remote sensing imagery from Son's paper and our predicted model results (son et al., 2010). As we mentioned above response (#2), the PPP results of our model agreed with their ocean color remote sensing results in this region (Son et al., 2010). This is described in the discussion part of revision to explain why our model is suitable in the CSK (line 405-414). Please see above response (#2).

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