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Interactive comment on "Implications of different nitrogen input sources for potential production and carbon flux estimates in the coastal Gulf of Mexico (GOM) and Korean coastal waters" *by* Jongsun Kim et al.

Anonymous Referee #3

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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: 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? 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.

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

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.

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?

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