Interactive comment on "Partitioning of carbon export in the upper water column of the oligotrophic South China Sea" by Yifan Ma et al.

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Anonymous Referee #2

The article submitted by Ma et al investigates carbon export from the euphotic layer of the South China Sea, considering two layers (nutricline and euphotic layers). Carbon exports were calculated based on ^{234}Th particulate fluxes and POC:^{234}Th ratio. The authors made a complicated discussion on the potential origin of nitrogen sources based on ^{15}N-isotopic budget. The article is rather difficult to follow as the description of the dataset is not clear, and the sections are not always in the appropriate order. For example, it is really strange to discuss the impacts on physical transport on ^{234}Th fluxes, whereas there were several pages where the ^{234}Th fluxes and derived product were extensively discussed.

[Response]: We appreciate the constructive comments from the reviewer. Our manuscript is being thoroughly revised according to the reviewer's comments in order to optimize the discussion and the logic flow, and to enhance the readability. To do so, we have made a new table (Table R1) for better describing the dataset being used as suggested by the reviewer. In addition, the discussion of the physical transport on ^{234}Th flux (section 4.1) will be moved to the methods part, before the ^{234}Th flux and derived fluxes are discussed.

Table R1: Sampling logs and site information along with the accessed parameters and their utilizations.

Station	Arriving time	Latitude [ºN]	Longitude [ºE]	Bottom depth [m]	Parameters		Data utilizations	
					Total ^{234}Th	Trap	Partitioning POC flux estimate	Nutrient source diagnosis
SEATS	2017-06-07 00:06	18	116	3907	√	√	√	√
A1*	2017-06-11 23:55	16	116	4205	√		√	
SS1	2017-06-12 20:08	14	116	4107	√		√	
H06	2017-06-20 02:28	14.1	116	4289	√		√	
H08	2017-06-20 07:51	13.9	116	4063	√		√	
H01	2017-06-20 23:41	14	116.1	4139	√		√	
H11	2017-06-21 05:18	14	115.9	4297	√		√	
B1	2017-06-22 11:43	14	113	2537	√		√	
C1	2017-06-23 04:40	12	113	4313	√		√	
A2	2017-06-24 03:05	12	116	4079	√		√	
B2	2017-06-24 21:42	14	117	3947	√		√	

* Sampling station might be influenced by the typhoon event passing through the South China Sea. Station A1 was visited after typhoon Merbok, which was generated on June 9, 2017 at 13.1ºN, 119.8ºE in the southern China Sea. Merbok landed on June 12 at 27.5ºN, 117.3ºE.

The dataset: there is a need of a table that presents clearly sampling, which station / when / what was measured (water column, trap). It is indicated that the cruise took place from June, 5 to 27, 2017. But typhoon Merbok occurred the 10th. "before our field campaign". This needs to be clarified. In case of a typhoon had occurred during sampling, one could expect it had impacted the water column and chemical budget. In addition, how could it be possible to use the described ^{234}Th model which is a steady-state model

[Response]: Thanks for the advices from the reviewer, and a new table of the sampling information will be included in the MS and is shown above.

Note that we did not conduct samplings before or during the typhoon, thus it is impossible for us to build up a non-steady state (NSS) model for ^{234}Th flux estimation. However, we reasoned that a SS model is in order in the condition under study as the Chl *a* concentration was not significantly enhanced under the impact of typhoon as shown by the remote sensing derived 8-day averaged surface Chl *a* (Figure R1). Indeed, the SS ^{234}Th fluxes remained pretty low, mostly <800 dpm m^{-2} d^{-1} during our study, suggesting that again, export was not much beyond steady state as shown in many prior studies (e.g., Resplandy et al., 2012; Savoye et al., 2006). Additional justification of the SS model will be added during the revision.

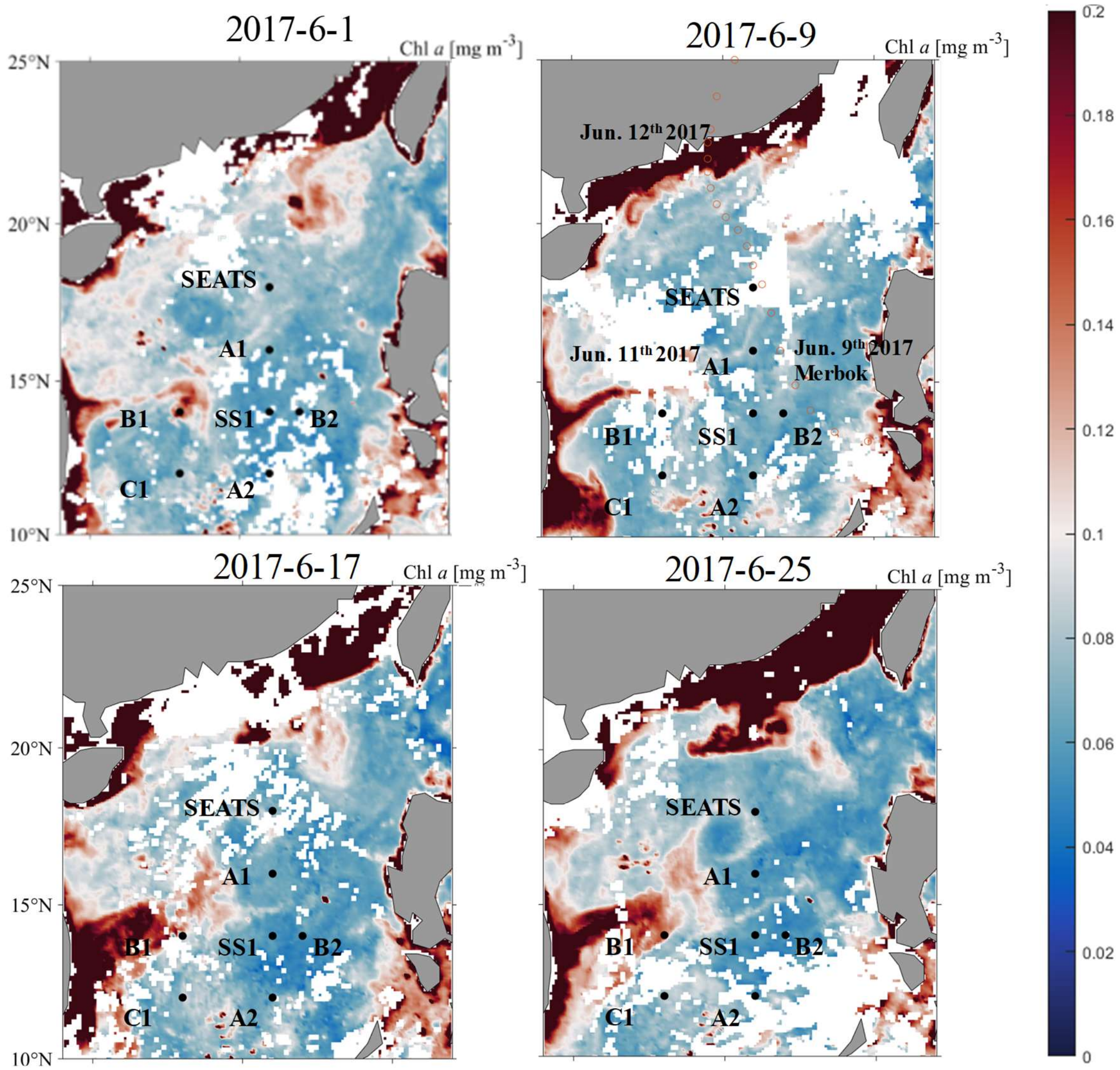

Figure R1: Satellite-derived the 8-day averaged surface Chl *a* in the SCS basin during June 2017, showing that sea surface Chl *a* concentration was little enhanced during our ship-based sampling period. Note that Station A1 was visited after typhoon Merbok, which was generated on June 9, 2017 at 13.1^{o}N, 119.8^{o}E in the southern China Sea. Merbok landed on June 12 at 27.5^{o}N, 117.3^{o}E.

Four stations (H01, H06, H08, H11) were sampled around SS1 to check the spatial variability of ^{234}Th. But it is indicated later that the mega station SS1 was revisited during August 2019 (after a second typhoon Mun, July, 1th) with trap deployment. A clarification must then to be made on what was sampled / when / where.

[Response]: We apologize that we were not clear enough in describing the multiple events happened prior to and post our sampling campaign. We have now included such information in the Table R1 with clarifications throughout the revised MS. Note that our ship-based sampling occurred from June 5^{th} to June 27^{th}, 2017 with samplings at station SS1 and its surrounding stations (H01, H06, H08 and H11) on June 12^{th}, 2017 We did deploy sediment traps at Station SS1 but unfortunately, the traps were not retrieved. We thus used a trap results deployed two years later on July 13^{th}, 2019 From Station SS1. It must be pointed out that the data accessed from sediment traps deployed at Station SS1 in 2019 was only utilized to evaluate the contribution of subsurface nutrient by $\delta^{15}N_{PN}$.

High resolutions profiles: the authors made a great announcement about high resolution profiles. In fact, there are only two, t detailed profiles: SEAT and SS1. The other profiles have a less resolution, and, except for the lower total ^{234}Th values at about 25 meters at station A1, the profiles of total ^{234}Th are not so different. It would be interesting that the authors reduce the depth resolution of the SEATS and SS1 profiles to compare the estimated ^{234}Th.

[Response]: The reviewer is right that the 10-m vertical interval samplings were only conducted at stations SEATS and SS1. We will clarify this in our revision. Following suggestions, we estimated ^{234}Th fluxes at the Ez base by reducing the vertical resolution to a 25-m interval, being 490±60 and 655±71 dpm m^{-2} d^{-1} respectively for station SEATS and SS1 compared to 522±43 and 631±48 dpm m^{-2} d^{-1} under the high-resolution sampling mode. The low-resolution sampling thus might induce a less than 6% of uncertainty for the ^{234}Th flux. However, the high-resolution sampling is essential in order to examine the partitioning of carbon export in the upper water column, especially for the oligotrophic ocean characteristic of the low export flux. Based on high-resolution total ^{234}Th pattern at stations SEATS and SS1, we first determined ^{234}Th deficit in the NDL, showing the substantial particle scavenging and POC export at the NDL base at both stations, and we subsequently found similar pattern at the rest of stations and estimated the partitioning in POC export flux between two layers. The reduced sampling resolution might introduce some additional uncertainty to estimates of ^{234}Th flux and ^{234}Th-derived POC export flux, but would not change our main conclusion that the base of NDL is the hotspot for particle scavenging and POC export. We will include the above clarification and reasoning in our revision.

Export model: from equation (2), the authors need to produce the two equations relative to the export estimate for the NDL- and NRL-layers, respectively. Use directly the symbol for Fndl and Fnrl. There is no need to use layer i /i-1, that only complicate the model presentation. Also from Fig 2, it seems that calculations are done for each box, but from the text it is less clear that the fluxes from NRL-layer is calculated considering only the lower box or the whole water column above the euphotic layer limit. Figure 2 needs also to be improved: if total ^{234}Th activities are related to U activities, what means 'absorb particles, total TH already includes particulate phase. The figure needs to be corrected.

[Response]: We agree with the reviewer's comments for the Figure 2. We actually calculated ^{234}Th flux at the export horizons of NDL base and the euphotic zone (Ez) bottom, with the integration carried out between 0-NDL base and 0-Ez bottom (not the lower box as mentioned by the reviewer). Here we use symbol F_{NDL} and F_{Ez} as suggested by the reviewer. In order to make the statement clearer, we will revise the main text to emphasize that the flux at the Ez bottom is integrated from the whole box from surface to the Ez bottom.

The reviewer is also right that we only measured total ^{234}Th activities during the cruises, and we will delete the "particles" in the figure and change "$A^{dissolved}$" into "A^{total}" as suggested by the reviewer (see the details in Figure R2).

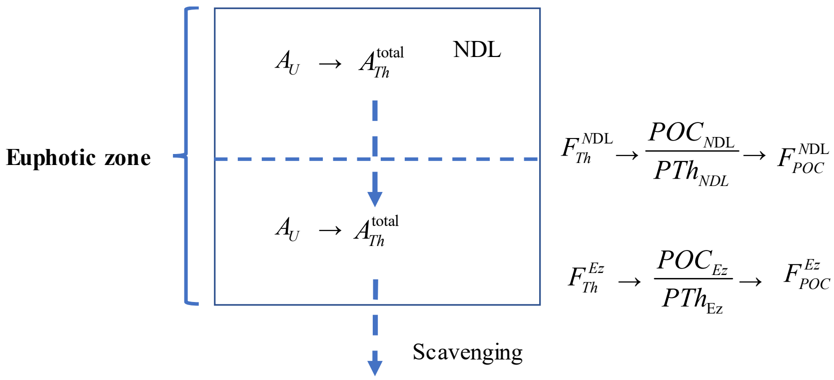

Figure R2: Schematic of the ^{234}Th model under the two-layer nutrient structure. All terms are defined in Equations (2)-(6).

The conversion of ^{234}Th particulate fluxes in POC fluxes: the conversion is done using the POC/^{234}Th. The recommendation is to use the large particle ratio. In this work, the authors use the ratio obtained from bottle waters, that correspond to fine particles. The authors need to better argument the choice. The comparison with the trap ratio seems to be biased as trap was done in summer, no during the same sampling cruise. The authors need to be clearer on this aspect. If confirmed, it means that some paragraphs are not justified.

[Response]: Bottle filtration and trap deployment for POC/^{234}Th were done at Station SEATS during the same cruise (See Table R1). Bottle-derived POC/^{234}Th ratios at the depth of 50 m and 100 m were respectively 4.4±0.6 and 3.8±0.3 μmol C dpm^{-1} compared to 4.4±0.6 and 3.2±0.4 μmol C dpm^{-1} from trap samples. We thus confirmed that bottle-derived POC/^{234}Th was comparable with those derived from trap samples during this cruise. This is consistent with what Zhou et al. (2020) found showing that POC export fluxes based on bottle POC/^{234}Th was comparable with trap POC fluxes measured before. More importantly, it was impossible to deploy sediment traps at all stations due to practical reasons. For consistency with prior studies in the region (e.g., Cai et al., 2008; Zhou et al., 2013; Cai et al., 2015; Zhou et al., 2020), we primarily used bottle derived POC/^{234}Th in estimating POC export fluxes as we routinely did in our prior work.

Th/POC flux estimates: most of the article is based on fluxes, but the authors treated data as it was rather instantaneous fluxes, which is clearly not the case. Considering the half-life of ^{234}Th, a deficit of ^{234}Th in the water column represent a flux story of several weeks. The only way to have more "instantaneous" fluxes is to repeat profiles at the same station which was not done here. Therefore, it is the main problem with the article. The authors discussed a lot fluxes and potential nutrient sources, but the errors on the fluxes estimate do not support the discussion. There is an over-interpretation of the dataset and the derived fluxes to support the hypothesis of the authors.

[Response]: We completely agree with the reviewer that ^{234}Th-derived POC export flux is not instantaneous but with a timescale of weeks to months, and understand the reviewer's concern on the potential issues associated with the correlation between "instantaneous" nutrient and time integrated POC flux. In order to match the time scale between nutrient and POC fluxes, we also correlated ^{234}Th-derived POC flux with the model-derived monthly average of nutrients during summer (Figure R3, Du et al., 2021). The correlation in between is indeed statistically significant ($P<0.05$). This suggests that under the oligotrophic condition of the present study, the euphotic layer characterized by low biological productivity, and the system under study is pretty much under steady state. The overall low ^{234}Th flux as we explained in our above responses to the reviewer, also supports this notion. In addition, we examined the $\delta^{15}N_{PN}$ value measured in several previous studies in the region (e.g., Kao et al., 2012; Yang et al., 2017; Yang et al., 2022). Taken together, we contend that the conclusion of the subsurface nutrient supported largely is a well plausible interpretation of the dataset. Having said, we will fully consider the comments from the reviewer and revise our MS accordingly.

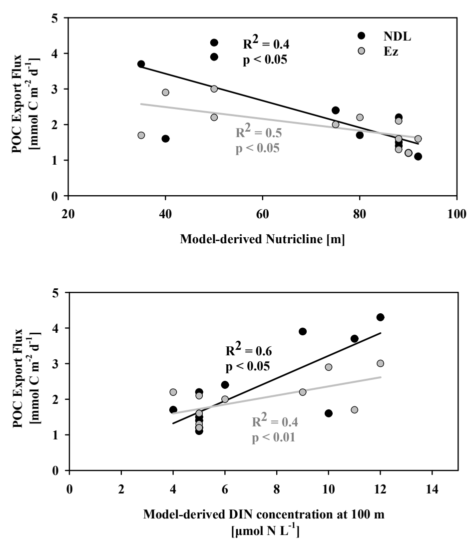

Figure R3: Relationship between POC export fluxes at the NDL base (black dots) and Ez base (grey dots) vs. the model-derived depth of the top of the nutricline (top) and DIN concentration in the subsurface water at 100 m (bottom).

Others comments: most figures need to be improved and some data combined differently. What is the interest of figure 3?

[Response]: Thanks for the comments, we have revised the figures based on the suggestion above from the reviewer. As our figure 4 has shown the vertical profiles of T and S, here we deleted the figure 3 to simplify the discussion.

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