

General comments:

Qin et al have thoughtfully revisited their manuscript and made a number of revisions that have strengthened the paper overall. In particular, their analysis of the timing of shelf water intrusion into the SCS versus the residence time of their O₂/Ar measurements, supported with satellite chlorophyll data, provides convincing and valuable support of their conclusion regarding the contribution of shelf water intrusion to NCP.

The authors' more detailed analysis of average mixed-layer PAR and their revised conclusion that light does not limit mixed-layer NCP in the study region is also an important improvement. Generally, the revisions have demonstrated care and critical thought in reevaluating the interpretation of this study's findings. The changes made have satisfied this reviewer's original criticisms of the manuscript.

The new assessment of the impact of the shelf water intrusion upon observed NCP rates is also quite clever and an interesting scientific contribution.

Specific comments:

Figure S3 is quite nice and I'm very tempted to recommend that this be included as a main figure. I certainly think it adds more value to the main article than Figure 9, for instance.

Line 15: The statement that NCP is a proxy of carbon export is slightly too strong, as NCP is more accurately a metric of export potential (excess organic matter production available for export to depth).

Section 2.3: How many replicates for nutrient analysis were collected at each CTD station?

Line 376-378: Upon further reflection, this statement reads as attributing somewhat too strong of a causal relationship. I find the NH₄ measurements, sparse though they are, to be useful evidence of ammonium contributing to the peak in NCP on this transect, and the residence time at these stations was quite short which further supports this, but at the end of the day these are just two stations. This also further emphasizes the importance of replication of nutrient sampling as noted above. If these are only single measurements, then only a very weak statement can be made here, and the associated discussion should be reconsidered more thoroughly.

Figure 5c and Figure 6c: As noted above, if the NH₄ nutrient sampling includes multiple measurements, the individual replicates in addition to the mean might be shown.

Lines 385-387: I would cite Figure S3 here, as this clearly shows the influence of shelf water.

Line 421 (and line 27 in abstract as well as line 559 in the conclusion): The figure of 376% is a little overly precise, especially given the variance in the NCP of the background and intrusion-influenced water masses. I would replace with a more general statement like "by

potentially more than threefold” or similar, following the convention the authors have adopted in lines 492-495.

Lines 510-529: this new passage and the associated new data figure and table are very strong additions. Again would make the case for Figure S3 to become a main figure given its importance to the manuscript’s conclusions regarding the July cruise.

Throughout: I would recommend that the authors double-check the manuscript text for minor grammatical errors, particularly in the newly-added text.

For instance, in “Dissolved oxygen-to-argon ratios (O_2/Ar) in the oceanic mixed layer has been widely used” (Line 13), “has” should be replaced with “have”.

Similarly, “Despite the coastal waters such as shelves and estuaries only account for 7 % of the global ocean surface area” (line 52) should be revised to something like “Despite coastal waters such as shelves and estuaries only accounting for 7 % of the global ocean surface area”, etc.