This manuscript provides a review on the recent progress on how BGC-Argo can be used to advance our understanding of marine productivity, with a special focus on the two key fluxes: gross primary production and net community production. The manuscript started with an explicit introduction about the fundamental concept of these two parameters, followed by a detailed recap on the main approaches used to constrain these two fluxes by using the high-frequency BGC-float observations. Subsequently, an in-depth analysis is performed using a compilation of datasets from prior studies to assess the natural variability of marine productivity, as well as the main uncertainties and challenges that persist in the float-based methodology. Lastly, the authors present a new estimate of the global meridional pattern of carbon export ratios by combining the float estimated GPP and NCP, demonstrating an encouraging agreement when compared with traditional estimates.

Overall, the manuscript is well-written and logically organized, and the figures and tables effectively support the conclusions. I believe this work is of great value and will be of interest to the broad community in the field of marine biogeochemistry, autonomous platforms, sensor technology, and climate. Additionally, it contributes to the ongoing global BGC-Argo project (GO-BGC) and provides valuable guidance for future float deployments.

During the revision, I suggest the authors consider the following comments to improve the clarity of the manuscript:

Figure 1: The PP profile displays the subsurface maximum, in contrast with the light attenuation as described by the authors. I believe this reflects the trade-off between light and nutrient availability, and it would be helpful to provide an explanation in the caption. Furthermore, consider adding an elementary equation describing the organic carbon production somewhere in the figure, as this can provide necessary context regarding why different tracers (O2, NO3, and DIC) are used to track productivity.

Table 1: Ensure that the literature citation format within the table is consistent.

Line 80: Provide information on the global range of GPP and NCP estimates, highlighting the large uncertainties in current estimates, which may exceed the magnitude of air-sea CO2 flux.

Figure 2d: Clarify how the number of BGC-floats was determined. Do you include all floats equipped with at least one chemical or bio-optical sensor? Please clarify this point.

Line 315: Note that DIC, TA, and POC are secondary-derived variables and not directly observed from floats. Describe how these variables are obtained from BGC-float observations to make the paper self-explanatory.

Line 315: Mention that salinity normalization is another commonly used approach to account for the EP term.

Figure 4: Spell out all abbreviations shown in the figures in the caption to enhance readability. This issue should be addressed for all figures throughout the manuscript.

Line 410: Consider adding a short sentence to introduce the background of OSP.

Figure 5: Add a legend to panel a to make the figure easy to interpret. Denote the geographic location of OSP in the caption. Also, consider using the carbon unit in all figures.

Line 585: Subscript "2" alongside the O2.

Figure 7: There appears to be no clear response of the GPP_O2:GPP_bbp ratio to depth. To explore potential geographic patterns. Based on the current knowledge, fractional contribution of DOC to the total carbon production is highly correlated with the NO3 concentration. I would suggest replacing the dot color with the latitude band or background NO3 concentration (i.e., derived from WOA2018) to see if we can derive some geographic pattern.

Line 720: Point out that the tracer budget approach typically assumes the float follows the same water mass, which is not always the case in reality.

Line 735: Change to something like "reflect the fraction of suspended particle organic carbon."

Line 735: I don't quite understand why the relative importance of new production (based on NO3-) versus recycled production can affect the coupling between O2 and NO3-based NCP estimates. The biological term solved from the NO3 budget reflects net production fueled by NO3, aligning with the original definition of NCP.

In contrast, the GPP_O2:GPP_DIC ratio (GPP_O2:GPP_N ratio) may be impacted by the relative importance of new production versus recycled production, as GPP is supported by the bulk inorganic nitrogen (=NO3+NH4), and the C:O ratio (or O:N) differs depending on the nitrogen sources (i.e., C:O=1.1 when the substrate is NH4 and C:O=1.4 when the substrate is NO3; for more details, see Laws et al., 1991, and Huang et al., 2021, GBC).

The extent of denitrification and N2 fixation indeed affects the consistency between O2 and NO3-based NCP estimates. On one hand, denitrification can lead to some degree of decoupling between O2 and NO3-based NCP because it generates NO3 without consuming O2. Regarding the influence of N2 fixation, it depends on whether we account for the external NO3 source inherited from N2 fixation in the NO3 tracer budget. If not, the NO3-based biological term solved from the tracer budget will be biased toward low values. This bias is particularly pronounced in the oligotrophic ocean (see Huang et al., 2023, PNAS). Additionally, it is worth pointing out that budgeting nitrate may be subject to considerable uncertainty in the oligotrophic ocean, as the magnitude of surface NO3 and associated seasonal evolution in this area is typically close to the instrument-to-noise ratio.

It would be also helpful to mention that the reliance on empirical estimate in TA will introduce error in TA-and DIC NCP.

Line 750: I noticed that many prior studies don't account for the effect of in situ sea-level pressure on oxygen solubility, leading to biases, particularly in high-latitude regions where the sea-level pressure is lower than the standard pressure. Therefore, it is crucial to emphasize this point in the manuscript and call for attention to it in future work.

Line 825: NCP results from Johnson et al., (2017) represent the seasonal maximum of NO3 drawdown during the austral productive period, rather than true ANCP. I wonder if the authors applied any corrections to convert it to aNCP.

Line 835: "We present NCP and ANCP values integrated to the annual maximum mixed layer depth (MLD), scaling values from Huang et al. assuming constant NCP between 56 m and the maximum MLD." and "Accordingly, we scaled all independent NCP estimates to the annual average maximum MLD at OSP". Based on this statement, I still have difficulty understanding how you performed the depth conversion throughout the manuscript.