General Comments

The article by Toullec couples satellite measurements of PIC production, based on PIC concentration and coccolithophore physiological information, with deep-sea sediment trap PIC fluxes. The linkage, or lack of linkage in some regions, is then explored in terms of ecosystem structure, driving factors, and relationships between NPP and POC fluxes. Exploration of the deep PIC fluxes globally and regionally provide some interesting new insights into the fate of sinking PIC, however linking these fluxes with ocean color measurements should be treated with caution.

Fundamentally the article assumes that the PIC measured by satellites and that collected in the deep ocean are the same – largely ignoring that while satellite PIC may measure coccolithophore dynamics, it does not provide any information on the other known pelagic calcifiers (foraminifera, pteropods) who may contribute to export. What proportion of the deep fluxes originate from coccolithophores, foraminifera and pteropods? While coccolithophore dominance of PIC production may make sense in terms of their short generational times relative to the larger (>100 um) calcifiers, foraminifera and pteropods, these organisms are well known to be present in sediment traps and deep sea sediments. This is a major issue for the current study – this is an incomplete comparison of PIC production and PIC export, meaning that a comparison of export efficiency is not comparing like with like (but noting that transfer efficiency is a valid comparison as it only relies on the deep fluxes). While the author mentions the relative importance of other pelagic calcifiers in terms of PIC production, there is no discussion of their relative importance to export. As this is a fundamental assumption to the paper, it makes interpretation of the other relationships presented in the paper (e.g., to the modelling work of Nowicki et al., 2022) questionable.

Author's response: *I would like to thank Reviewer 1 for pointing out the lack of consideration of the other known pelagic calcifiers (foraminifera and pteropods) who indeed contribute to the PIC export in open ocean. I also strengthen the justification of the choice to consider the seasonal bias of PIC production (satellite based) and PIC export, to discuss the export efficiency.*

Supported by my justification and responses to comments (detailed below), please consider the changes made to the manuscript for publication in Biogeosciences.

I understand Reviewer 1's concerns about the implication of other pelagic calcifiers (pteropods and foraminifers). All Pelagic calcifiers contribution is crucial to explain processes of PIC flux in the water column (regarding the PIC bulk observed inside *sediment traps). To be more realist and complete regarding all pelagic calcifiers role the carbonate pump, I implemented more caution and pointed out the limitation of the assumption made that the PIC measured by satellites could be directly linked with PIC export and deep PIC flux made in this study.*

Lines 260-261: "This study highlights the importance of rapid calcification event observed by satellite (such as blooming coccolithophore episodes, on a monthly basis)", and less than 30 days integrated PIC flux from sediment traps (excluding longer deployment).

Lines 261-263: "Only including short term sediment traps (less than 30 days), give us a picture of relatively fast sedimentation event, this way, coupling with monthly satellite climatology provides greater meaning regarding potential process involved.

Lines 263-265: Despite pteropods and foraminifers contribute to PIC production and deep flux, their seasonality pattern and residence time were not coupled with PIC flux observation, considering short time deployed sediment traps (less than 30 days).

Lines 265-268: In other the hand, other pelagic contributors (Pteropods and foraminifers) are expected to be more peripheral in the export efficiency of the PIC in this study, regarding their respective residence time (more than 30 days for foraminifers and months to year for pteropods). This aspect is the main constraint in the present study.

To keep caution on the relative implication of each calcifying taxa, I added a figure showing the estimated contribution of coccolithophore to the standing PIC stock, which are dominant (Figure 3, foraminifers and pteropods annual standing stock and seasonal variation along the year). The supporting data can be found in Knech et al., 2023. A significant part of the data treatment and discussion in the submitted study are based on the spatial seasonality of PIC production (satellite based) and short-term PIC flux (<30 days deployed sediment traps data). The new Figure 3 presents the seasonality pattern of each pelagic calcifiers (within 200m layers). Coccolithophore standing stock are strongly associated with a strong spatial seasonal pattern, that is not the case for pteropods and foraminifers (Fig. 3).

Lines 242-252: "At the global scale, coccolithophore PIC standing stock dominates the total estimated PIC standing stock, except in few areas of Equatorial Atlantic (EA), Equatorial Pacific (EP), North Atlantic (NA) and North Pacific (NP), where the pteropods PIC standing stock reach almost 50% (Fig. 4). Foraminifera PIC standing stock represents less than 10% of the PIC standing stock, with higher value in the North Atlantic (NA). Regarding the seasonal variation index, coccolithophores are characterised by high seasonal variation in high latitudes (>30°N and <30°S), while pteropods' seasonal index is higher only >30° N, and in the Equatorial Pacific (EP). Foraminifera tend not to reveal any seasonal variation at a global scale, except in the southeastern North Atlantic (NA) (Fig. 4). Note that the depth of integration is different for *coccolithophore (100m) and zooplankton taxa (200m for Pteropods and Foraminifera, from Knecht et al., 2023). Although coccolithophore integration to 100m over 200m for pteropods and foraminifera leads to an underestimation of coccolithophore contribution to the global PIC standing stock in a 200m layer, the general statement that coccolithophore dominates the standing stock remains unchanged. In addition, the foraminifera maximum abundance peak between 0-100m depth (Chaabane et al., 2024)."*

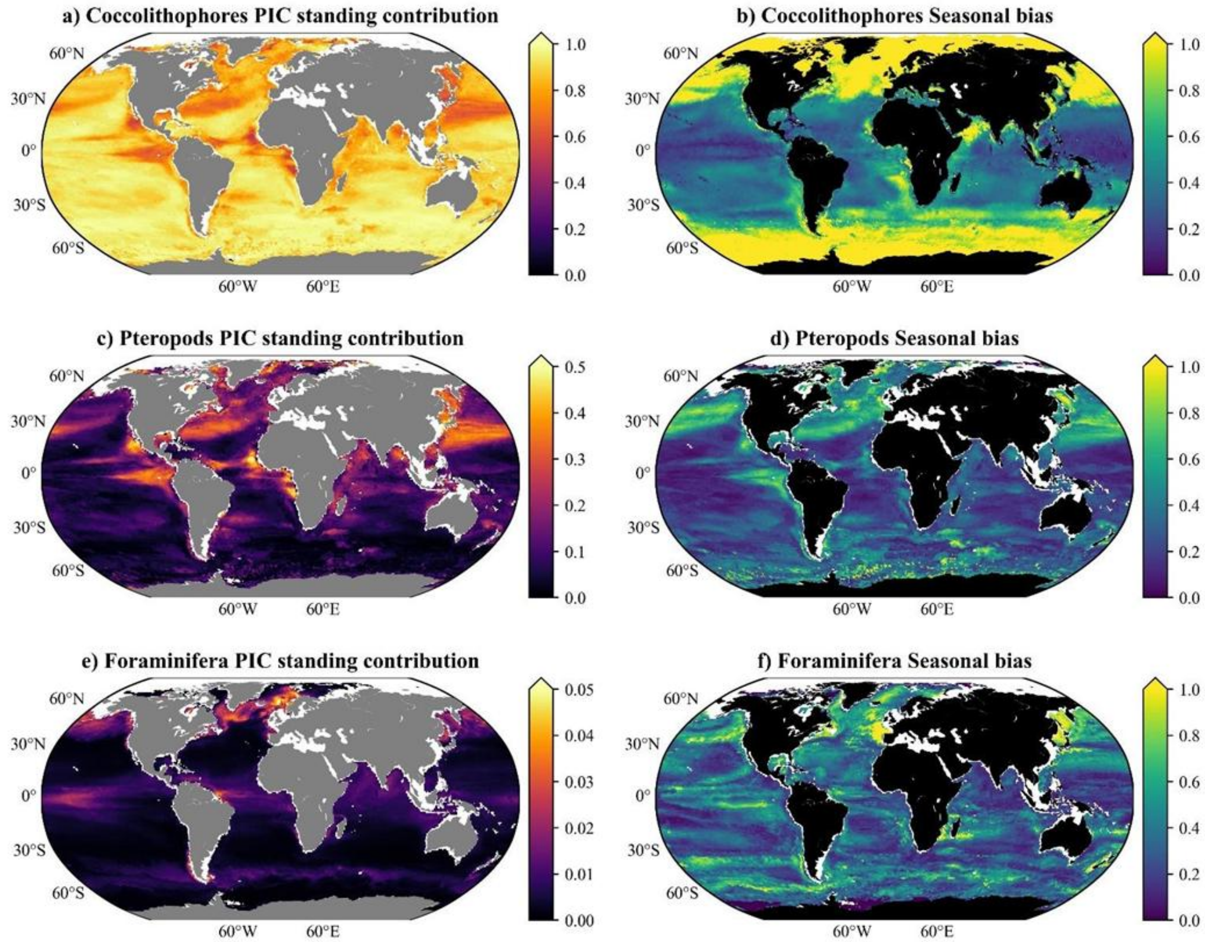

Figure 3: On the left, maps of taxa contribution to the total PIC standing stock regrouping the 3 calcifying taxa: a) Coccolithophore, c) Pteropods and e) Foraminifera. On the right, maps of temporal variability of PIC standing stock as measured by the seasonal bias expressed as coefficient of variation (σ/μ) of the 3 calcifying taxa: b) Coccolithophore, d) Pteropods and f) Foraminifera. Note that the depth of integration is different for coccolithophore (100m) and zooplankton taxa (200m for Pteropods and Foraminifera, from Knecht et al., 2023).

Considering that pteropods[1] and foraminifers[2] have longer residence time compared to coccolithophores[3], and less seasonal bias (Figure 3), the contribution in short term PIC flux is assumed to be peripheric in the context of this study. I assume that pteropods and foraminifers sinking flux is also less seasonal and occurs all along the year. That is why, *pteropods and foraminifers found in short terms sediment traps should more stochastic than periodic (monthly).*

Lines 268-272: "It is assumed that G. huxleyi blooms would be present in the water for an average of 30 days (Hopkins et al., 2015), hence, monthly PIC production climatology coverage map represents the mean monthly conditions. Pelagic calcifiers are characterized by different surface stock and seasonality (Fig. 3) and residence time: less than a month for coccolithophore (Hopkins et al., 2015), a month for foraminifers (Schiebel and A. Movellan., 2012) and months to years for pteropods (Lalli & Gilmer.,1989, Bednaršek et al., 2012)."

[1]Foraminifera residence time (month):

2 cycles per years: Jonkers, L., & Kučera, M. (2015). Global analysis of seasonality in the shell flux of extant planktonic Foraminifera. Biogeosciences, 12(7), 2207-2226.

Berger, W. H. (1971). Sedimentation of planktonic foraminifera. Marine Geology, 11(5), 325-358.

Chernihovsky N, Torfstein A and Almogi-Labin A (2023) Daily timescale dynamics of planktonic foraminifera shell-size distributions. Front. Mar. Sci. 10:1126398. doi: 10.3389/fmars.2023.1126398

[2]Pteropods residence time (months to years):

Lalli, C. M., & Gilmer, R. W. (1989). Pelagic snails: The biology of holoplanktonic gastropod mollusks. Stanford University Press.

Bednaršek, N., Mozina, J., Vogt, M., O'Brien, C., & Tarling, G. A. (2012). The global distribution of pteropods and their contribution to carbonate and carbon biomass in the modern ocean. Earth System Science Data, 4(1), 167–186. https://doi.org/10.5194/essd-4-167-2012

Van der Spoel, Dadon S. (1999). "Pteropoda," in South Atlantic zooplankton. Ed. Boltovskoy D. (Leiden: Backhuys Publishers, Leiden), 649–706.

[3]Coccolithophores residence time:

In the literature, a median bloom duration of 30 days (Berelson et al., 2007, Brown and Yoder, 1994b, Harlay et al., 2010, Hopkins et al., 2015, Schlüter et al. 2013).

Also, regarding the fact that coccolithophores contribute to 70-90% of PIC production in the North Pacific Ocean when the period is optimal (Ziveri et al., 2023), I hypothesize that less productive period would also lead to lower PIC and POC pelagic flux.

Unfortunately, (to my knowledge) no solid dataset exists presenting highly contribution of each pelagic calicifers recovered into sediment traps. No model nor machine learning based global PIC export flux pelagic calcifiers group contribution exists so far.

However, Table 1, Figure 3 and figure 4a presented in the literature review by Neukermans et al. (2023) provides a first response concerning the contribution of each pelagic calcifiers to mesopelagic and bathypelagic flux. The literature review reveals that most of the PIC mass flux remains unspecified (Figure 3 in Neukermans et al. 2023), but *in north Indian ocean, PIC flux could be constituted half constituted of coccolithophore and half foraminifers (note that is annual PIC flux, without any seasonal consideration and potential dissolution event inside the trap collector). Indeed, Table 1 in Neukermans et al (2023), presented long term sediment traps data (more than a month of deployment, sometimes more than a year), whereas in the present submitted version, only short-term sediment traps data (less than 30 days of deployment) were used and compared to monthly satellite data.*

Concerning the production of both foraminifers and pteropods compared to coccolithophores. Very large estimates are discussed in section ***4.3. Taxa contribution to global PIC stock and production****.*

There are a high number of editorial mistakes in the article, where spelling or syntax errors cause lines to be unclear and confusing, and several parts of the paper need better explanation. Further, there are several sections of the discussion (4.2, 4.3, 4.4.1, 4.4.2) that contain lots of information and interesting literature, but lack any direct link to the results presented. Other sections have only limited links to the results presented leaving the reader well informed of the subject but with little clear insights into how this relates to that which is presented.

Author's response: *I would like to thank Reviewer 1 for demonstrating the lack of direct link to the results presented (also suggested by Reviewer 2). Significant changes and improvement have been made to the manuscript to remedy these issues. Indeed, some subsections need more relevant explanation regarding the data presented (especially the mentioned subsection), I am conscious that these subsections are clearly hypothetic and not directly supported by the data presented. However, I'm convinced that these aspects and potential processes (e.g. regarding the fecal pellet contribution to particle export and/or microzooplankton implication on a global scale) are very important and constitute the missing key of our understanding. Details and used of Nowicki et al. 2022 model output were inappropriately reduced in the submitted version. My apologies for that.*

Supported by my justification and responses to comments (detailed below), please consider the changes made to the manuscript.

Specific Comments

Ln 19, what is meant by "plankton network community"?

Author's response: *It means trophic interaction in the planktonic community or foodweb network; the sentence has been modified.*

Line 19 "...modification of planktonic community"

Ln 26, how would atmospheric CO2 concentration be twice as important? Does the author mean twice as high?

Author's response: *Yes indeed, thank you for noticing it, the sentence has been modified.*

Line 26: "Without the BCP, atmospheric CO2 concentration would be twice as high"

Ln 27, Photosynthesis does not 'uptake CO_2 from the atmosphere' – the source of C is dissolved in seawater.

Author's response: *Indeed, this is more accurate, atmosphere has been removed.*

Line 27: "Phytoplankton, due to photosynthesis, uptake CO2 and produce particulate organic carbon (POC)."

Ln 33, As phrased here, this sentence makes no sense – the C in CaCO3 (inorganic C) is not equivalent to POC (organic C).

Author's response: Indeed, I meant that calcifiers are not only made of inorganic *carbon but also organic carbon. The sentence has been modified.*

Lines 33-34: "all planktonic calcified organisms (such as coccolithophores, foraminifera and pteropods) transport also POC to deep waters through gravitational settling"

Ln 35, 'are' rather than 'were'.

Author's response: *The sentence has been modified.*

Line 35 "Thorium-234 activity (^{234}Th activity) are the most widespread techniques"

Ln 37, What does the author mean by 'period is optimal'?

Author's response: *I meant optimal in term of stratification, light and nutrient availability. The sentence has been modified.*

Line 38: "...when nutrient and light are available (Ziveri et al., 2023)"

Lns 47-19, Definitions of export efficiency and transfer efficiency are identical here but are used throughout to mean different things (and are incorrect relative to the literature, e.g., Henson et al., 2012). This makes the article very confusing.

Author's response: *The definition of export efficiency and transfer efficiency has been checked the meaning homogenized as well.*

Lines 49-50: "particle export efficiency (PE_{eff}, corresponding to the POC sinking flux in the euphotic layer/ POC production)"

Lines 50-51: "The transfer efficiency (T_{eff}, corresponding to the proportion of exported organic matter that reaches the deep ocean)".

Also, does the author mean PEeff only for POC (POC flux/NPP) or for both POC and PIC (PIC flux/PIC production)? The meaning of the terms change in the paper, causing further confusion (see next comment).

Author's response: *In the introduction I considered PE_{eff} for POC (as mentioned in the literature). In the presented results, I presented PE_{eff} as PIC export $_{eff}$ (considering PIC and not POC in the calculation = PIC export flux/PIC production). I modified the notation throughout the manuscript (PIC E_{eff} & PIC T_{eff}) to avoid confusion.*

Ln 53, Please rewrite and/or explain better 'CaCO3 incorporation into aggregates and fecal pellets support the idea that high PEeff could be coupled with high CaCO3 flux'. It is not clear what the point that the author is making is here. Does PEeff refer only to POC in this instance or to PIC?

Author's response: *I referenced the ballast effect in this sentence. PE_{eff} referred to POC export. I deleted part of this sentence to avoid any confusion, moreover this sentence was redundant with the following statement.*

*Lines 56-58: ("...the incorporation of biominerals (such as CaCO3 and biogenic silica) induces a ballast effect (excess of density) on marine snow sinking velocity (Iversen and Ploug, 2010; Laurenceau-Cornec et al., 2020) and hence **is** expected to boost the BCP.")*

Ln 55, 'is' is missing from 'and hence expected'.

Author's response: *I corrected the sentence.*

Ln 62, Is 'the packaging factor' theory the correct terminology to be used?

Author's response: *The packaging factor' theory has been introduced by Francois et al. (2002): "Particles sinking from carbonate-dominated ecosystems with complex food webs would be more tightly packaged in fecal pellets, while particles sinking from seasonal, opal-dominated systems would be looser, less hydrodynamic aggregates, produced in large parts by aggregation of senescent diatoms." (François et al., 2002). This terminology is in harmony with the ideas developed in the paper.*

Francois, R., Honjo, S., Krishfield, R., & Manganini, S. (2002). Factors controlling the flux of organic carbon to the bathypelagic zone of the ocean. Global Biogeochemical Cycles, 16(4), 34-1.

Figure 1 – This is not quantitative in any way and does not fully make any sense? The arrows on the far right imply latitudinal variability in Teff and PEeff and sinking POC lability. Also, where is lability of sinking POC discussed in the introduction?

Author's response: *I removed this figure, which was a big source of confusion and to a lesser extent suggested contradictory statements (also noticed by Reviewer 2).*

Ln 75, Please change 'Emiliania huxleyi' to 'Gephyrocapsa huxleyi' after Bendif et al. (2023).

Author's response*: I changed 'Emiliania huxleyi' to Gephyrocapsa (Emiliania) huxleyi. I kept (Emiliana) between parenthesis, so as not to lose readers who are unaware of this change in genus name.*

Ln 78, Please explain and cite relevant literature for 'There remains a gap between the amount of photosynthetically produced organic carbon, and it transferred fraction to the deep.'

Author's response: *I modified the sentence and cited appropriate literature for this sentence.*

Lines 81-82: "There remains a gap in our comprehension of processes controlling the transfer of photosynthetically produced organic carbon to the deep (Buesseler et al., 2007; Henson et al., 2012)."

Ln 79, Please explain and cite relevant literature to support 'Nowadays, heterotrophic respiration in sinking aggregates is considered to dissolve CaCO3 particles in the upper ocean'.

Author's response: *In Suplis et al. (2021), biotic factors such as heterotrophic respiration is considered to dissolve $CaCO_3$ "metabolic CO_2 production creating acidic* *microenvironments or dissolution of more-soluble $CaCO_3$ phases such as aragonite or Mg calcites (Buitenhuis et al., 2019; Morse et al., 2006). However, the relative importance of biologically mediated $CaCO_3$ dissolution is still controversial (Friis et al., 2006), and many basic questions, such as which $CaCO_3$ polymorphs are dissolving and what the controlling factors are, remain unanswered."*

I modified the sentence and referenced the papers cited in Suplis et al., 2021.

Lines 83-85: "Nowadays, heterotrophic respiration in sinking aggregates is considered creating a microenvironment supporting dissolution of CaCO3 in the upper ocean (Morse et al., 2006; Friis et al., 2006; Buitenhuis et al., 2019; Sulpis et al., 2021)."

Ln 81-82, Please rewrite and explain better 'Without upper ocean CaCO3 dissolution, the ocean output 20% more CO2 to the atmosphere through low-latitude upwelling regions'.

***Author's response**: In Kwon et al. (2024), the author* explored how the *biological export production could control upper ocean calcium carbonate dissolution through the CO_2 buffer capacity "Upper Ocean dissolution, shown to be sensitive to ocean export production, can increase the neutralizing capacity for respired CO_2 by up to 6% in low-latitude thermocline waters. Without upper ocean dissolution, the ocean might lose 20% more CO_2 to the atmosphere through the low latitude upwelling regions".*

I modified the sentence to avoid any misunderstanding.

Lines 87-88: "It is estimated that $CaCO_3$ dissolution in the upper ocean contribute to uptake 20% of atmospheric CO_2 through the low latitude upwelling regions (kwon et al., 2024)."

Ln 85, Please rewrite and explain better 'which holds the uptake of atmospheric carbon and acidification in surface waters.'

***Author's response:** Ocean alkalinity of surface ocean drives marine uptake of atmospheric CO_2 (Millero et al., 1998). Here $CaCO_3$ dissolution diminished carbonate pump on global ocean carbon uptake and surface ocean acidification, it can have a large impact on regional air–sea carbon fluxes, particularly in the Southern Ocean (Planchat et al., 2023, 2024). I meant that $CaCO_3$ dissolution, conduct to convert CO_2 to alkalinity. Alkalinity balance at the surface ocean is a key component of carbon sequestration (Renforth & Henderson 2017).*

I modified the sentence to avoid any misunderstanding, and removed the mention of acidification, which is out of context here.

Lines 90-92: "In addition, processes of PIC production, sinking flux and dissolution are crucial to understand ocean alkalinity balance, which control atmospheric carbon uptake in surface waters (Millero et al., 1998; Renforth & Henderson 2017; Planchat et al., 2023)."

Ln 94, More details are needed on the 'physiological constant associated with' (Emiliania) Gephyrocapsa huxleyi.

Author's response: *I modified the sentence to be more detailed (more details are available in Hopkins and Balch, 2018 and reference in here).*

Lines 100-101: "...coupled with physiological constant (growth rate under variable light intensity and temperature) associated with Gephyrocapsa (Emiliania) huxleyi"

Lines 108-109: "...In this model, general assumptions are made, such as the PIC production is proportional to the coccolithophore growth rate. The coccolithophore growth rate is a function of temperature and irradiance (parameters established on G. huxleyi culture)."

Ln 122, It would be helpful to avoid confusion and explain how PIC concentrations and PIC production were put into the same units.

Author's response: *The monthly surface PIC concentration is expressed in* ***mol C m^{-3}****, then converted into 100m integrated stock (eq 4: $PIC_{100m} = 40.555 \times PIC_{surface}^{0.560}$) and expressed in mol C m^{-2}. Then the mol is converted into g (multiplied by 12), hence we obtain PIC stock concentration (**mg C m^{-2}**). PIC production (model output) is expressed in **mg C m^{-2} d^{-1}**. PIC flux is expressed also in **mg C m^{-2} d^{-1},** considering the deployment duration (in day), the sediment trap collecting surface area (m^{2}) and the quantity of collected matter (mol of g).*

I added the information to be more detailed.

Lines 152-154: "The monthly surface PIC concentration (mol C m^{-3}) was converted into 100m integrated stock (using Eq. 4) and expressed in g C m^{-2}. PIC productions are expressed in mg C m^{-2} d^{-1}. PIC fluxes are expressed in mg C m^{-2} d^{-1}, considering the deployment duration (in day), the sediment trap collecting surface area (m^{2}) and the quantity of collected matter (g)."

Ln 179, Rough sentence which seems to be missing context '54 tations were out of the RECCAP2 mask and then have been removed from the 6057 PIC flux observations subset.' Where does this belong? Text above or Figure legend.

Author's response: *My apologies for this mistake. Indeed, this sentence belongs to Fig. 1's legend.*

Ln 181, 'ocean colour' observations rather than 'ocean colours'.

Author's response: *The typo has been corrected*

Ln 243, This is a (very) late point to introduce a new dataset, which then becomes important to the rest of the paper (e.g., Figure 6b) – it's also introduced in very little detail. To make this part of the article, much more information needs to be introduced throughout.

Author's response: *Reviewer 1 is right, details and used of Nowicki et al. 2022 model output were inappropriately reduced in the submitted version. My apologies for that. I added a mention of this dataset in the methods part (section 2.2.5. Supporting dataset). I also added more detail of the model output detail and use in the present manuscript:*

Lines 214-216: (section 2.2.5. Supporting dataset): "1° by 1° grid map of fecal pellet and aggregates contribution to the total particles export were obtained from model ensemble output from Nowicki et al., 2022 (FigShare database: https://doi.org/10.6084/m9.figshare.19074521)."

Lines 304-309: "The model output of Nowicki et al. (2022), estimated that the contribution to the gravitational carbon pump of zooplankton fecal pellets and sinking phytoplankton aggregates were respectively of 85% and 15%, (Nowicki et al., 2022). Once mapped of 1° by 1° grid map, zooplankton fecal pellets and sinking phytoplankton aggregates contribution are characterized by a significant latitudinal pattern (Fig. 7 in Nowicki et al., 2022). The seasonality of NPP and PIC production can be overlapped with aggregates's contribution to the export (estimated by Nowicki et al., 2022), as shown in Fig. 6b"

Ln 255-257, This line (about diatoms and coccolithophores dominating productive areas) appears to contradict with much of the paper and the themes explored. This paradox needs some explanation.

Author's response: *Fig. 1 (opal vs $CaCO_3$ productive regions) has been removed, the figure was a big source of confusion and to a lesser extent contradictory statements suggested in the discussion (also noticed by Reviewer 2).*

My point here was to compare ecosystem dominated by blooming species (temperate regions with high seasonal amplitude) and annually productive region (subtropics with low seasonable amplitude)

I modified the sentence to be clearer on my statement:

Lines 321-324: "Bloom of diatoms and coccolithophores (e.g. Gephyrocapsa (Emiliania) huxleyi), which are expected to cause intense particle sedimentation, take place mostly in areas associated with high annual mean and amplitude of NPP, while nanoplankton/picoplankton global production are dominant in oligotrophic areas associated with low annual amplitude of NPP (Lima et al., 2014)."

Ln 260, Different (and correct) definition of transfer efficiency to that given earlier in the article.

Author's response: *Apologies again for the incorrect definition given in the introduction. Both definitions have been homogenized in the new manuscript version:*

Lines 49-50: "particle export efficiency (PE_{eff}, corresponding to the POC sinking flux in the euphotic layer/ POC production)"

Lines 50-51: "The transfer efficiency (T_{eff}, corresponding to the proportion of exported organic matter that reaches the deep ocean)".

Line 325-329: "...particle export efficiency (PEeff), which corresponds to the proportion of primary production that is exported from the surface ocean. In the present study, the PIC Eeff (which corresponds to the proportion of PIC production that is exported from the surface ocean) is commonly higher above 40°N and below 40°S (temperate and subpolar oceanic regions), while the PIC Teff (which corresponding to the proportion of exported PIC that reaches the deep ocean), is higher between 40°N and 40°S (subtropics)...."

Ln 271, 'sinking fecal pellets' rather than 'singing'.

Author's response: *The typo has been corrected*

Sections 4.2 and 4.3 – how does this section relate to the results presented? No link is made to the article. Also, sections 4.4.1 and 4.4.2 include no links to the present study.

Author's response: *I completely understand Reviewer 1's points (also raised by Reviewer 2).*

Section 4.2: *This section seems to be important to consider carbonate chemistry and dissolution pattern in the water column. I added a supplementary figure showing the saturation depth of calcite and aragonite (according to Feely et al., 2002), together with some explanation regarding the dissolution pattern:*

Lines 365-371: "In most of bioregions, saturation horizon for calcite and aragonite reach respectively 1000 and 800m depth (Fig. S1). North Subtropics Pacific (NSTP) is characterized by a shallow saturation horizon for both calcite and aragonite (respectively 500m and 400m, Fig. S1), despite the positive correlation between PIC production and flux is observed between 1000 and 2000m (Fig. 5). This observation means two hypotheses: In one hand the CaCO3 is well protected from dissolution inside packaged vehicles such as low porous aggregates and fecal pellet (protected by the peritrophic membrane). On the other hand, because packaged CaCO3 sink fast (≈ 100m d-1), the dissolution rate of CaCO3 can be too slow to significantly decrease deep PIC flux."

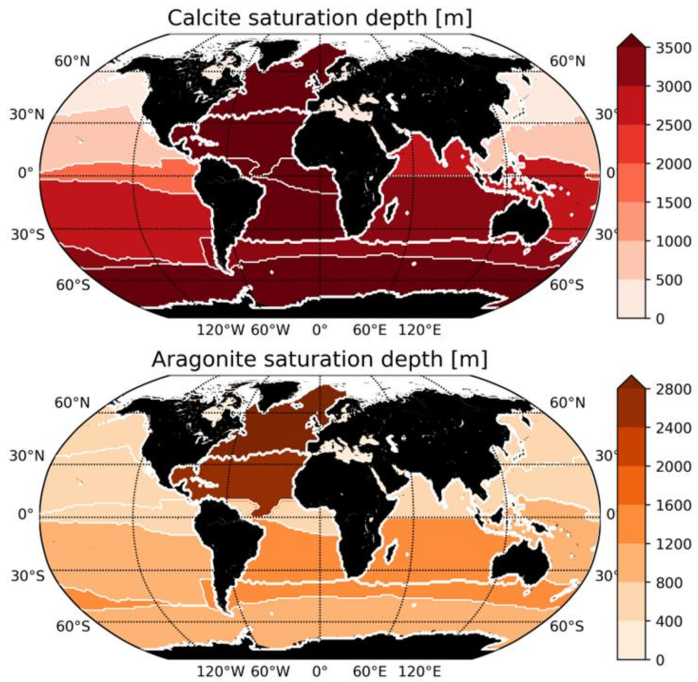

Figure S1: a) Map Calcite saturation depth (m) according to the RECCAP2 regions. b) Map of aragonite saturation depth (m) according to the RECCAP2 regions. Data are extracted from Feely et al., 2002

If Reviewer 1 is not at all convinced by keeping this section in the current manuscript, it can be permanently removed from the manuscript as requested.

Section 4.3: *Considering Reviewer 1 concerns about the implication of the other pelagic calcifiers (Pteropods and foraminifers). I included section 3.2. Taxa contribution to global PIC stock in the results (together with Fig. 3, foraminifers and pteropods annual standing stock and seasonal variation along the year) in the new version (see previous author's response). Section 4.3 in the discussion is now more relevant to being there*

***Section 4.4.1** & **Section 4.4.2:** These sections are indeed speculative and do not come directly from my results. However, the references listed support the hypotheses raised in the manuscript concerning the potential PIC dissolution inside aggregates and PIC "dissolution escaping" inside the fecal pellets. Most of study about carbonate pumps assume that PIC dissolution is associated with biological and ecological mechanisms, without explaining hypothetic mechanism:*

Lines 43-47: ". A large proportion of these CaCO3 produced in the euphotic zone is dissolved within the first 300m of the ocean (Sulpis et al., 2021, Feely et al., 2002; Milliman et al., 1999), … This shallow dissolution is not yet clearly explained but considered to be associated to biological and ecological mechanisms (zooplankton and procaryotes mediated dissolution)."

In these 2 sub-sections, I propose potential processes, supported by references.

I modulated the title ("Hypothetic processes…").

I also implemented some links to the results presented in the present study:

Lines 420-425: "However, PIC Eeff is higher at high latitude where fecal pellet contribution to gravitational pump is lower (Fig. 6b). This result suggests more complex mechanism and implication of plankton community phenology into the PIC Eeff control (see sections 4.4.3. and Fig. 7). In addition, PIC Eeff is higher at high latitude where phytoplankton aggregates contribution to gravitational pump is also higher (Fig. 6b), which suggest implication seasonal phytoplankton phenology."

Lines 436-439: "This latitudinal variation of microzooplankton grazing pressure could partially explain the PIC E_{eff} and PIC T_{eff} observed (Fig. 6). Annual higher grazing rates by microzooplankton are also expected in subtropical regions due to a low seasonal bias (Fig. 6a), leading to continuous grazing pressure from microzooplankton. This idea is also supported by the contribution of fecal pellet to the gravitational particles flux (Fig. 6b)."

Also, if Reviewer 1 is not at all convinced by keeping this section in the current manuscript, it can be permanently removed from the manuscript as requested.