Response Reviewer 1

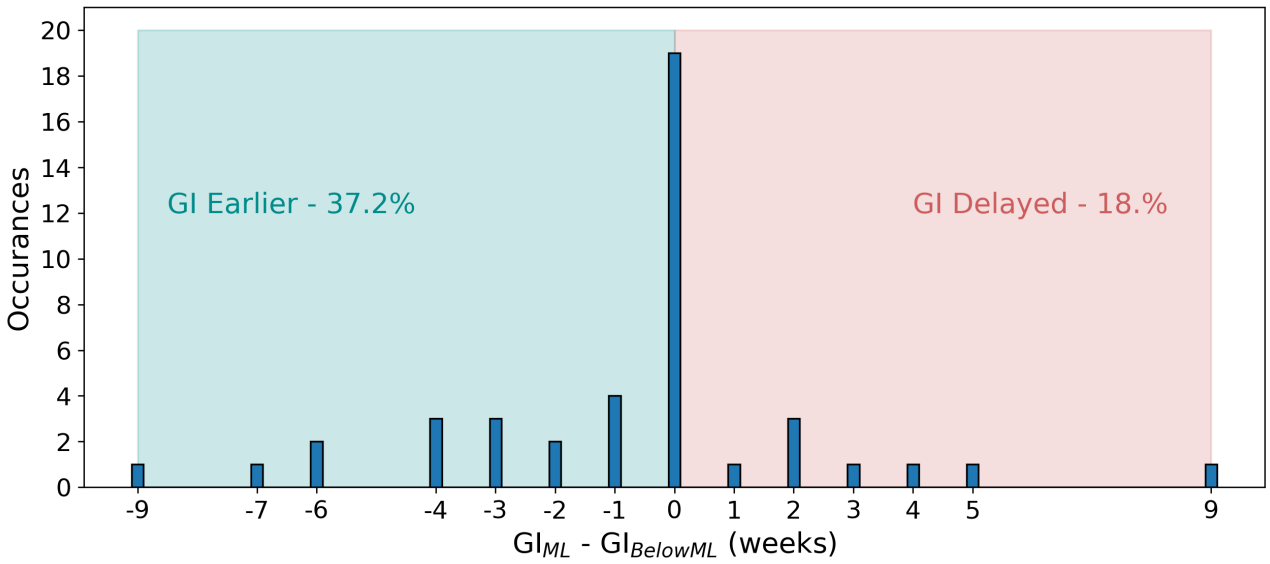
We would like to sincerely thank the reviewer for their time and valuable input. These comments and suggestions have surely enhanced the quality of the paper.

1. **Lines 147-149: Have you looked at whether examining Chl-a below the mixed layer changes the timing of GI? Does the timing of GI differ from the 'onset' proposed by Behrenfeld and Boss (2018)?**

Part 1 of the Question:

Below we plot the difference in GI when taking into account chl-a below the mixed layer (we average now to ~50 m below the ML by including an additional 7 depth levels below the ML). The left side of the distribution represents events where the timing of GI is moved earlier in the year, the right where GI is delayed. In general, around 45% of events have no change, while ~70% have a change of less than 2 weeks. It is also clear that the overall effect would be to shift the timing of GI earlier in the year, since twice as many events are shifted earlier than are delayed.

These changes appear to driven by the extent to which chl-a concentrations are diluted by including data from below the estimated mixed layer. In cases where GI is shifted earlier, concentrations are significantly reduced, especially in the summer, which then has the effect of shifting the location of the median growth rate earlier. That is, since growth rates are reduced more in late spring/summer (than in the early spring) the median rate now occurs earlier in the year. This is contrasted with cases where GI is delayed, which do not display significant dilution of the spring/summer chl-a concentration (evidently there is some chl-a below the estimated ML). Indeed, in these cases the spring growth rate is enhanced by the inclusion of chl-a below the ML, which shifts the location of the median growth rate later in the season. Another way to think of it is that the late winter growth rates are now smaller compared to those in the spring, so it takes longer for the criteria for GI to be met. We note that this only delays GI by more than 2 weeks in 4 cases, so the presence of significant chl-a below the ML appears to be fairly rare in this data set. We will add this point to the revised manuscript at line 216.

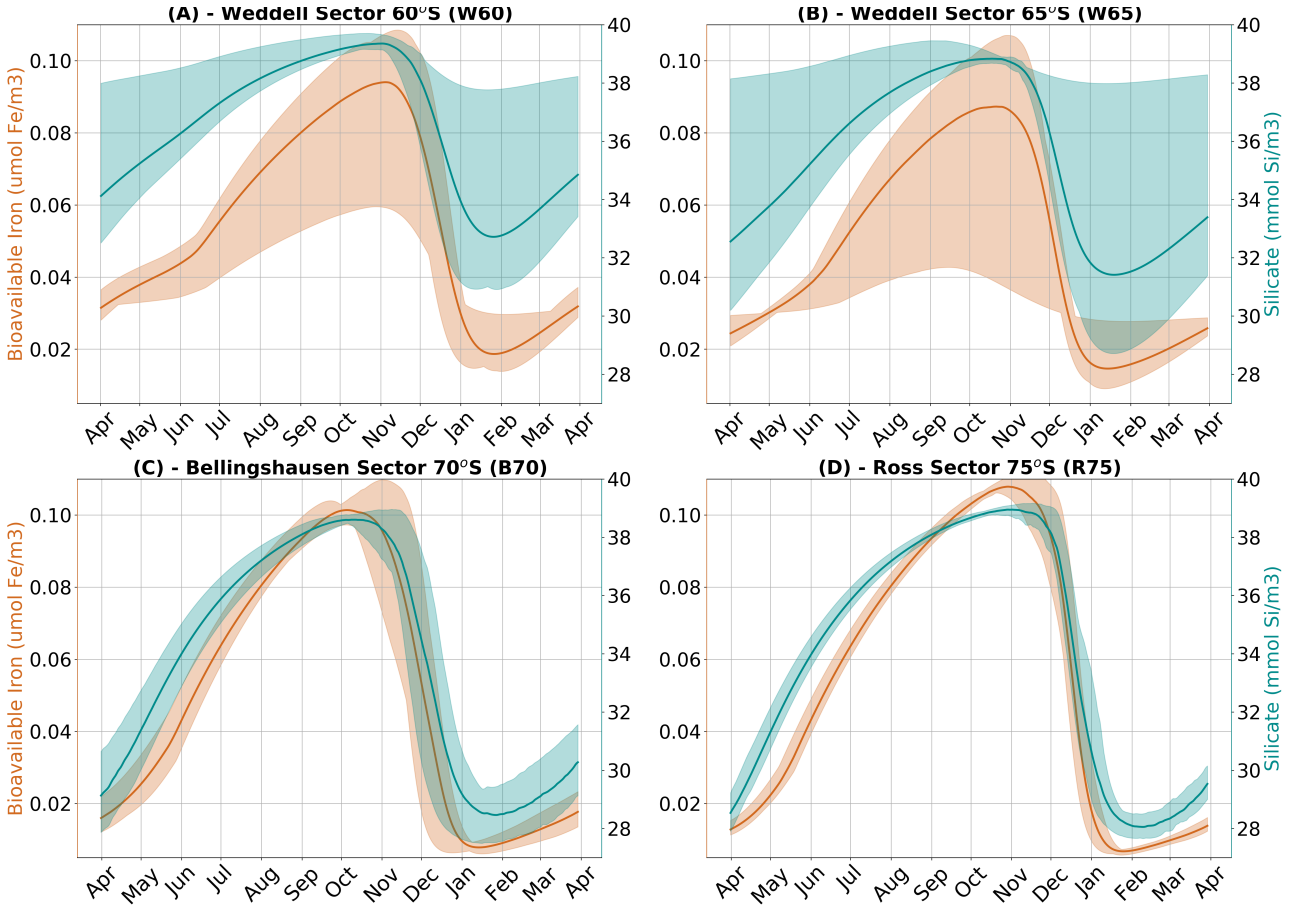


Part 2

Our definition of GI is distinct from that proposed by Behrenfeld and Boss (2018). Firstly, in their paper they refer to “bloom initiation” (their table 1), where as our definition of “growth initiation” is intentionally indifferent to whether or not a “bloom” occurs after GI, for reasons that are discussed in lines 156 - 162. Second, the Behrenfeld and Boss (2018) definition requires that the loss term be estimated, where as we define GI purely based on the time derivative of mixed layer chl-a.

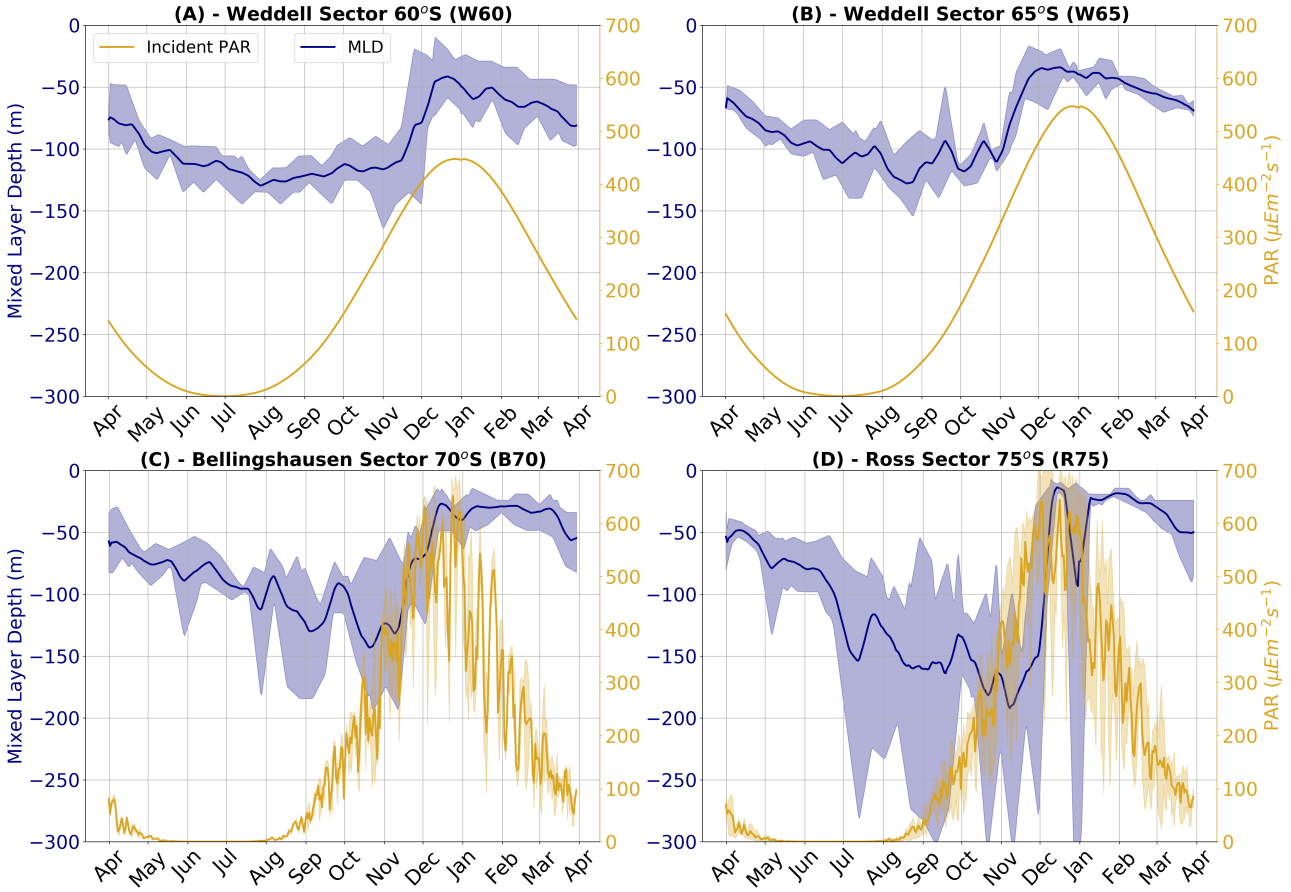
2. Lines 175-177: I would like to see how iron and silicate (presumably being the limiting nutrients for diatom) vary over the course of your simulation. Is the ecosystem always light depleted or do nutrients also become a limiting factor for growth in your simulation?

Below we plot time series of iron (brown) and silicate (green) in each of the 4 study regions (only for the “LLA” experiment). The shaded regions correspond to the variability present in each study region (both spatial and temporal) as is discussed in the paper. Neither nutrients are limiting for the late winter/early spring period under consideration. For completeness we will add this figure to the supplementary material and add this point to the revised manuscript.



3. Lines 178-180: Could you add a figure/time series of the input variables (solar radiation etc.) to the model?

We thank the reviewer for bringing this up and will add the figure below to the supplementary material. Note that the sea ice concentration forcing is already shown in the Supplementary figure S5 for the Weddell and Bellingshausen Sea regions, and for the Ross Sea it is shown in the main paper in Figure 6. One detail which was left out of the paper is that the more northerly experiments in the Weddell Sea (W60 and W65) used an analytical light forcing. This will now be added to section 2 of the paper.



4. Line 220: Thank you for also examining particulate organic matter (POC). Could you add in text how you calculated POC from the BGC-Argo floats? Also, considering the capacity for photoquenching/photoacclimation, do you think POC is a more robust variable in quantifying the temporal variability in phytoplankton/biomass? The weaker dependence on different rates of cooling, as you note, seems to indicate so.

POC comes as a variable in the SOCCOM float files. More information regarding its calculation can be found in Boss & Haëntjens (2016), although this equation is given:
POC = 3.23 × 104 × bbp(700) + 2.76 [mg m−3], where bbp(700) refers to particle backscattering at 700nm measured by the float.

In terms of which quantity is a more robust estimate, we would argue that both are associated with uncertainties of a similar magnitude (as is discussed in Boss & Haëntjens, 2016). We agree that POC appears to be a more stable estimate of the amount of scattering particles under sea ice. However, the relationship between backscattering and phytoplankton carbon is still quite uncertain in the Southern Ocean, as has been analysed by Thomalla et al. (2017). Once strengthened with additional studies, the relationship between POC and chlorophyll would certainly help to better understand the under ice acclimation. With regard to quenching, we believe that it is unlikely to play a role under sea ice, given the low light levels.

5. **Figure 7: Could you comment on the systematic offset between the float data and the ICE/LLA simulations you see in panels C and D?**

We assume the reviewer refers to the offset in the winter time chl-a concentrations?
The model was not specifically tuned to simulate the transitional periods between autumn winter and spring-summer. As explained in the text, the model was meant to explore the mechanisms during the melting phase, and therefore the focus was on the relative growth rates and the phenology. The discrepancy is likely due to the need to adjust the metabolic loss terms for phytoplankton in full darkness, a point which will be added to the discussion in section 4.

6. **Lines 294-296, 345: The PAR condition seems to be a key factor in interpreting the float and model results. Is it possible to estimate the PAR under sea ice either using satellite data (Morel et al., 2017) or Argo floats? (I am not sure whether the floats you have examined have the instrumental capacity but some BGC-Argo floats measure also PAR.)**

The float data we have used unfortunately do not include any PAR information. Looking at the global BGC-Argo array and filtering for irradiance data reveals that under ice data are exceedingly rare in the Southern Ocean (2, possibly 3 floats may have some profiles under ice). Rather, under ice floats with that capability have exclusively been deployed in the North Atlantic. This point will be added to section 2 at line 66. It may be possible to infer the under ice light environment in the Southern Ocean based on data in the North Atlantic (if one could find profiles which sampled under ice conditions similar to that found in the Southern Ocean - i.e. fairly thin and unconsolidated ice).

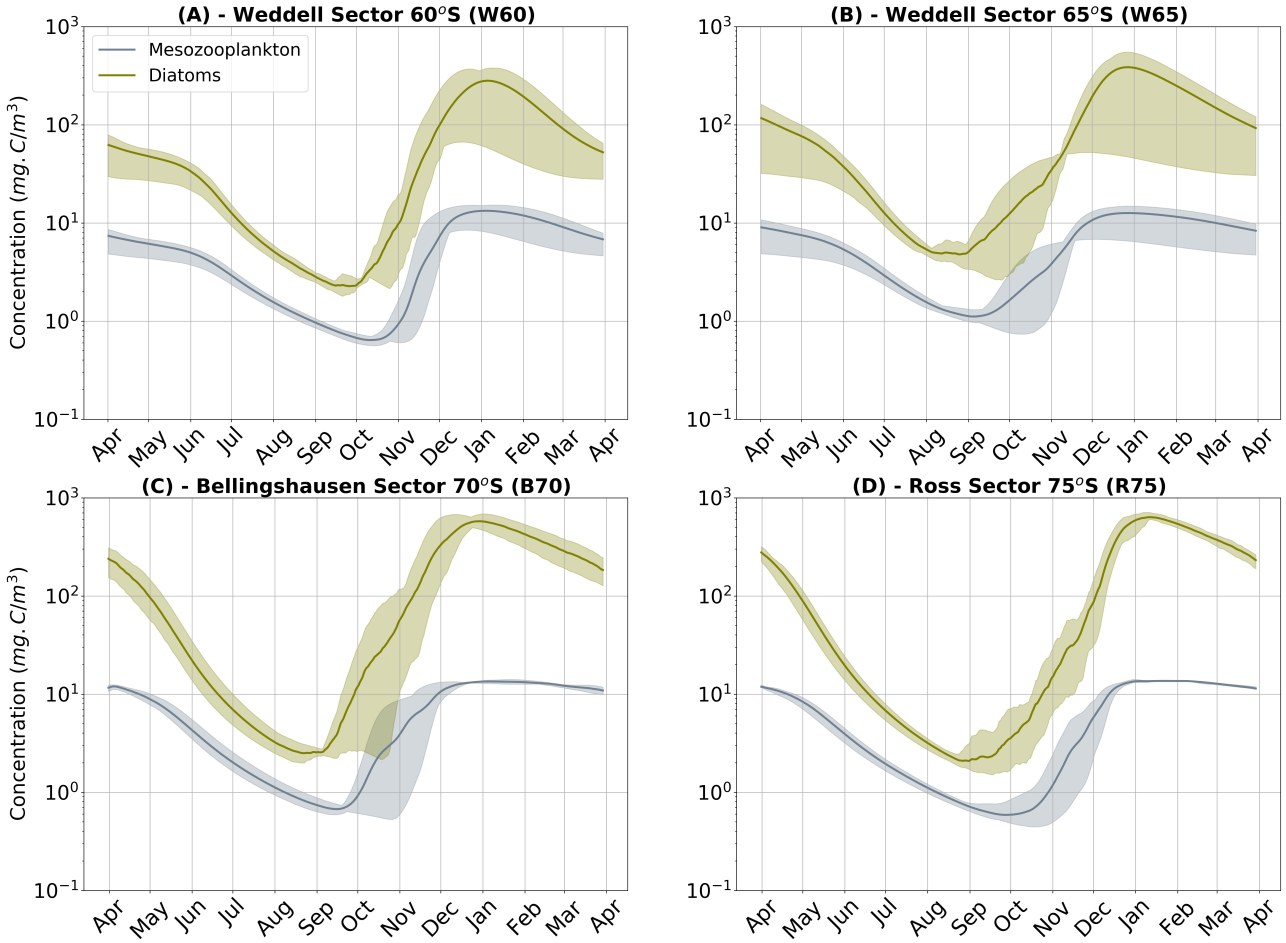
In terms of satellite data, as far as we know there is no way to estimate irradiance under sea ice from space. In open ocean regions one may estimate the diffuse attenuation coefficient and thereby infer the depth of the euphotic zone (as is done in Morel et al., 2017). While it may be possible to estimate this quantity in rare cases where an observation is retrieved from an open ocean region within the SSIZ (e.g. leads), this would not address the question of how light is

transmitted through thin and frazil (ice/water mixture) ice. We say that such observations would be rare because ocean colour data are generally missing in the winter/early spring months south of 60S under consideration here.

7. Lines 324-326: Can you comment on the role/impact of zooplankton in your box model experiments? Does grazing by zooplankton affect the GI timing?

This is an important point and we thank the reviewer for highlighting it. In the model, zooplankton does not seem to affect the phenology of the diatoms in any of the three core experiments (OPEN, ICE or LLA). Rather, zooplankton have a lagged response to the diatom growth in early spring, suggesting that other factors are responsible for the timing of initial growth. This relationship is clearly shown in the figures below, which plot time series of diatom and mesozooplankton concentrations for each study region for the "LLA" experiment. The other 2 core experiments show the same relationship.

However, we are not making the argument that zooplankton play no role in phenology in the real Southern Ocean, just that the under ice growth identified in the float data can be accounted for without assuming a strong role for zooplankton. Furthermore, altering the zooplankton model parameters (such as lowering the diatom availability) did not lead to a phenology resembling the float data. Again, we will add this figure to the supplementary material and add some discussion of these points at lines 324 - 326.



References:

Boss, E.B. and N. Haëntjens, 2016. Primer regarding measurements of chlorophyll fluorescence and the backscattering coefficient with WETLabs FLBB on profiling floats. SOCCOM Tech. Rep. 2016-1. http://soccom.princeton.edu/sites/default/files/files/SOCCOM_2016-1_Bio-opticsprimer.pdf.

Thomalla, S. J., Ogunkoya, A. G., Vichi, M. and Swart, S. 2017. Using Optical Sensors on Gliders to Estimate Phytoplankton Carbon Concentrations and Chlorophyll-to-Carbon Ratios in the Southern Ocean. *Frontiers in Marine Science.* 4, 34.