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Interactive comment on “Seasonality of sea ice controls interannual variability of summertime Ω_A at the ice shelf in the Eastern Weddell Sea – an ocean acidification sensitivity study” by A. Weeber et al.

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Biogeosciences Discuss., 12, C1380–C1385, 2015 www.biogeosciences-discuss.net/12/C1380/2015/ © Author(s) 2015. This work is distributed under the Creative Commons Attribute 3.0 License. Open Access Biogeosciences Discussions Interactive comment on “Seasonality of sea ice controls interannual variability of summertime Ω_A at the ice shelf in the Eastern Weddell Sea – an ocean acidification sensitivity study” by A. Weeber et al.

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Response to referee: Anonymous Referee #4 Received and published: 17 April 2015

Thank you for the time and effort that you spent on our paper, as well as for the valuable comments that you have made. Please note that due to the recommendations of a referee, we have removed Figure 3, and thus Figure 4 becomes Figure 3, Figure 5 becomes Figure 4, Figure 6 becomes Figure 5, Figure 7 becomes Figure 6 and Figure 8 becomes Figure 7.

This study makes use of underway sampling data collected in the Eastern Weddell Gyre spanning four years and a conceptual seawater density model to investigate seasonal to interannual variability in the aragonite saturation state (Ω_{arag}) and determine the physical and biological drivers for Ω_{arag} variability. The authors find that summertime sea-ice thaw, driven by changes in physical properties affecting the buoyancy flux, affects the availability of light and nutrient supply to the surface ocean, so that increased biological productivity is favored and thus Ω_{arag} increases. From this, they conclude that the main drivers for Ω_{arag} variability are the timC1380 BGD 12, C1380–C1385, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper ing and rate of the sea ice thaw, which directly affect phytoplankton blooms. In summary, I think the topic of this manuscript is very interesting and relevant to the broader field of research on the Southern Ocean and believe the data to be of good quality and generally well presented in the figures. However, there are several points, referring mainly to the structure and presentation of the manuscript, that in my opinion need to be improved first before the manuscript can be submitted in its final form (see my general comments below). More specific, minor comments can be found after the general comments. General comments: - I found the title of the manuscript to be somewhat confusing. The title implies that it is the seasonality of sea ice that is the main driver for interannual variability in Ω_{arag} . However, in the introduction (P1656, L28/29), the authors state that they will investigate how interannual variability in the drivers of Ω_{arag} will influence the variability of the seasonal cycle of Ω_{arag} . Thus, a clear use of the terms

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“seasonal”, “interannual”, “intra-seasonal”, and “intra-annual” is essential for more clarity in the manuscript. Thank you for this comment, we agree that this is unclear. We have changed the line in the introduction to clarify this. “We investigate how interannual variability in these processes drives variability in the seasonal cycle.” - I would strongly urge the authors to separate the results and discussion of the results. I am aware that this is a personal preference, but I found the results section to be too long and very difficult to follow. The authors introduce results from many other studies in the results section, so that I found it very difficult to clearly distinguish which results were produced for this study. Although I believe the authors have interesting results which are worth presenting, this would need to be done in a much more condensed way. In my opinion, the manuscript’s structure and clarity would greatly benefit from presenting the results in a concise way (i.e., not mixing them with the discussion) and then, in a separate section, present a clear, to-the-point discussion of these results, where references are brought in to put the authors’ results into context. The authors don’t need to add any more information to the results and discussion – a simple re-structure of the results and discussion would be sufficient. Thank you for these comments. We have considered this but we have decided to keep the results and the discussion sections combined to maintain the flow of the manuscript. The reason is that our results need to have some interpretation as they are read. As our study relies on the continual interpreting and discussing of the results, we did not feel that the results made sense when they were separated from the discussion. We do however agree with section 3 being difficult to follow and we have worked on it so our results and discussions are clearer and more structured. - I enjoyed reading Section 4 “Ecosystem implications” and Section 5 “Conclusions”, which I thought were the most well written parts of the manuscript. If the authors could manage to re-write the sections their results and discussion in a similarly concise fashion, then I believe the manuscript would be greatly improved. Thank you. - The manuscript should go through another couple of internal reviewing rounds, to improve the spelling, grammar

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and structure of the text, before it can be submitted in its final form. In general, the text could be shortened and condensed to be more flowing and coherent, which would greatly facilitate the reader's comprehension of the text. There are numerous spelling mistakes, of which I will highlight just a few in the more specific comments below. I would urge the authors to do a more rigorous spell check during the next round of internal reviews. Thank you, we have taken note of this. We have corrected the spelling errors and have shortened and condensed sections where possible, thank you for this valuable comment. - The abstract should be revisited after the manuscript has been improved. Currently, it does not significantly reflect the content of the manuscript, and it does not make it clear that the results of this study are based on observational data as well as on a conceptual model. The abstract has been revisited as suggested and we hope that the improved abstract reflects the manuscript. "As anthropogenic CO₂ increases, surface water aragonite saturation state (Ω_A) decreases, negatively affecting calcifying Euthecosome pteropods and the wider Antarctic ecosystem. However, the seasonal and interannual variability of the physical (stratification and mixing) and biological (photosynthesis) processes in this vulnerable Antarctic ecosystem are poorly understood. We collected surface water Ω_A data over four consecutive summers from the Eastern Weddell Gyre (EWG) ice shelf region, and investigated the drivers of (Ω_A) variability and the role played by the seasonal cycle of physical and biological processes in the interannual variability of Ω_A . Interannual variability in the timing and the rate of the summer ice thaw were the primary factors explaining interannual variability in surface water Ω_A . During the summers of 2008/2009 and 2010/2011, sea ice thaw was initiated in late November/early December, and the summertime increase in Ω_A was 1.02, while in 2009/2010 and 2011/2012 when sea ice thaw was delayed until late December, the summer increase in Ω_A was 0.46 and 0.59 respectively. We propose that two critical climate (physical-biogeochemical) sensitivities for Ω_A are the timing and the rate of sea ice thaw, which play an important role in summertime surface water stratification due to the influx of fresh sea-ice melt water and hence in the resulting onset, magnitude and persistence of phytoplankton blooms. The strength

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of summertime carbonate saturation depends on seasonal characteristics of sea ice, stratification and primary production. The sensitivity of surface water biogeochemistry to interannual changes in mixed layer - sea ice processes in this region suggests that future trends in climate and the seasonal cycle of sea ice, combined with increasing anthropogenic CO₂ may negatively affect the Antarctic ice shelf ecosystem within the next few decades. Our results suggest that any future reductions in primary production due to changes in stratification dynamics, combined with increasing anthropogenic CO₂, may culminate in the emergence of EWG summertime surface water aragonite undersaturation by the middle of this century.”

- Throughout the manuscript, it wasn't clear to me what the authors meant by “seasonal phasing”. Do they refer to the timing of the maximum and minimum seasonal cycle? This should be made clearer to the reader. We agree that this is unclear in the manuscript. By seasonal phasing we mean the timing of the variability in the seasonal cycle. Throughout the manuscript we have corrected this where possible.

Specific comments: - P1654, L2: What is “its”? Thank you, we agree that this sentence did not make sense. “Its” is referring to (̂ĐęA) and we have changed the sentence to: “As anthropogenic CO₂ increases, surface water aragonite saturation state (̂ĐęA) decreases, negatively affecting calcifying Euthecosome pteropods and the wider Antarctic ecosystem.” - P1654, L3-4: The seasonal cycle and interannual variability of what variables within this ecosystem? Thank you, we agree that this is not clear. The seasonal cycle of the physical (sea ice thaw and wind-induced mixing) and biological (photosynthesis) processes. This sentence has been changed to: “However, the seasonal and interannual variability of the physical (stratification and mixing) and biological (photosynthesis) processes in this vulnerable Antarctic ecosystem are poorly understood.” - P1654, L7: Drivers of what? Thank you, we were referring to the drivers of ̂ĐęA variability. This sentence has been changed to read: “We collected surface water ̂ĐęA data over four consecutive summers from the Eastern Weddell Gyre (EWG) ice shelf region, and investigated the drivers of (̂ĐęA) variability and the role played by the sea-

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sonal cycle of physical and biological processes in the interannual variability of ΔpCO_2 .”

- P1654, L10: What does “optimal” mean here? C1382 BGD 12, C1380–C1385, 2015
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Discussion Paper Thank you, as we only explain this later on in the paper, we agree that it does not make sense here in the Abstract. In this study we have defined “optimal” summers as the summers when the timing of sea ice thaw was in phase with the initiation of summertime phytoplankton blooms. We have changed the sentence to: “During the summers of 2008/2009 and 2010/2011, sea ice thaw was initiated in late November/early December, and the summertime increase in ΔpCO_2 was 1.02, while in 2009/2010 and 2011/2012 when sea ice thaw was delayed until late December, the summer increase in ΔpCO_2 was 0.46 and 0.59 respectively.” - In general, I am missing a clear explanation in the introduction as to why this work is relevant and why it is important to investigate the connection between interannual and seasonal variability of Omega_arag. This is currently not clear to me from the introduction. Thank you for this valuable comment. We feel that there was one sentence at the end of the introduction that started to explain this, but we agree that a clear explanation was missing from the introduction. We have added a section at the end of the introduction that will hopefully provide a sufficient answer to this comment. “An understanding of the links between the drivers of mixed layer physics such as sea ice thaw, buoyancy and wind-induced mixing are of key importance to understanding the climate sensitivity of surface water ΔpCO_2 and the ecosystem in the 21st century. Interannual variability in the magnitude of the seasonal cycle of ΔpCO_2 also highlights the importance of regional studies, particularly at the ice shelf ocean domain around Antarctica, which has one of the highest sensitivities in the global ocean for ocean acidification. The intraseasonal and seasonal scales of the analysis also help provide an understanding of how the progression of OA is modulated by surface layer physics which itself is linked to climate: it makes the carbon – climate links explicit.”

- P1655, L12: Where is this definition of the Revelle Factor from? Typically, the Revelle Factor is defined as $d(pCO_2)/d(DIC) \cdot DIC/pCO_2$ (see Gruber and Sarmiento, 2006).

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Please provide a reference if this definition is indeed commonly used. Thank you, we agree with this comment and, as suggested, the Revelle Factor definition has been changed to $(\partial \ln[\text{CO}_2]/\partial \ln \text{DIC})$ but the definition used in the text is derived from a simplification of the terms of this differential expression (Williams and Follows, 2011 Ocean Dynamics and the Carbon Cycle : “The high Revelle Factors $(\partial \text{pCO}_2/\partial \text{DIC}) \cdot \text{DIC}/\text{pCO}_2$, where DIC is Dissolved Inorganic Carbon) and cold water temperatures in Polar Regions, make aragonite saturation state (Δp^*) in these areas more sensitive to increases in CO_2 (Sabine et al., 2004; Egleston et al., 2010).”

- P1655, L23: reductions in growth rates (be consistent with L22) Thank you, this has been corrected. “Under natural and experimental conditions, elevated seawater pCO_2 has a variety of detrimental effects on marine calcifiers, including decreases in respiration rates (Hennige et al., 2014), shell corrosion and reductions in calcification rates (Crook et al., 2013) and reductions in growth rates and development (Kroeker et al., 2013).” - P1656, L8: constraints, not constrains Thank you, this has been corrected.

- P1656, L11/12: I don't understand what the authors mean by “Taking seasonality into account. . .”. Maybe they could explain in the introduction how seasonality affects the timing of future aragonite undersaturation events. This is also important in the light of their own study and should be properly introduced. Thank you, we agree that this should have been added to the introduction. Many papers predicting future aragonite undersaturation produce data for the mean annual levels of undersaturation, and do not look at the seasonal variability of these annual means. McNeil and Matear (2008) “take seasonality into account” meaning that in their predictions that are looking at the interannual variability when predicting undersaturation and we have added the following sentence to the introduction to explain this. “The extreme seasonality of sea ice processes and light availability, and hence primary production in the SO, further complicate carbonate processes, as summer primary production is a key driver of the summer increase in surface water Δp^* (Arrigo et al., 2008).” - P1656, L18: Define PAR. Thank you, PAR has been changed to “light availability” as requested by another referee. - P1656, L17-22: These two sentences are somewhat repetitive. Thank you,

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the second sentence has been taken out of the paper. - P1657: The authors mention towards the end of the introduction what they will be looking at in this study but don't provide any of their own results. I would like to see one to two sentences at the end of the introduction on their major results and findings, so as to guide the reader into the following text. Thank you, we have added these two sentences to end off our introduction: "Our findings suggest that in years when summer ice thaw was late (late December), primary production was restricted and the seasonal increase in $\Delta\sigma_t$ was limited to between 0.46 and 0.59, approximately half that of the summer increase in years when sea ice thaw was initiated in early December. Our simplified model proposes winter aragonite undersaturation at the EWG ice shelf by the middle of this century."

- Titles of Sections 2.2 and 2.3 are misspelled. Thank you, these have been corrected.
- P1657, L13: GPS device Thank you, this has been added. - P1657, L15: Replace "Depth" with "depth" Thank you, this has been corrected. - P1657, L119: Where is this reference density of 1027 kg m⁻³ from? How did the authors choose this value? C1383 BGD 12, C1380–C1385, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper Thank you for pointing this out. This reference density was put here by mistake as we did not use a reference density to calculate rho, we used SSS and SST. We have removed this from the methods, thank you. - P1657, L20/21: I don't understand this sentence. What does the density (should be "rho" not "p") in parentheses refer to? Thank you for pointing this out. As explained above, this was a mistake and we have thus taken it out of the methods. - P1658, L2: At what depths were the samples collected (vertical resolution)? The depths of the bottle samples between CTD casts were not always consistent, as we tried to close at least three bottles in the upper 10m and two bottles around the depth of the thermocline during each CTD cast. We agree that this was not clearly explained in our results and we have therefore explained it more accurately. "Niskin bottle water samples (24 per CTD cast) were collected from relevant depths to ensure that specific features were captured: surface, thermocline, chlorophyll a (chl-a) maximum, bottom of chl-a maximum and export depth. Thus, each CTD cast sampled water at different depths from

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the surface to 300m in order to measure chl-a , DIC, Total Alkalinity (TA) and nutrients (nitrate, silicate and phosphate). In addition, 48 CTD profiles were conducted at two locations at the Antarctic ice shelf edge (cyan circles in Fig. 1) over the austral summers of 2009, 2010, 2011 and 2012 to assess physical and biogeochemical processes and related interannual variability (Fig. 4).”

- P1658, L3-5: It isn't clear here where the additional 48 CTD profiles were measured. Are these the black dots in Fig. 1? Please make this clearer in the text and the caption of Fig. 1. Thank you, we agree that this is not clear. The additional 48 CTD's were measured at the ice shelf and are represented in Fig 1 by the two small groups of cyan circles. We have tried to make this clearer in the text and in the caption: “In addition, 48 CTD profiles were conducted at two locations at the Antarctic ice shelf edge (cyan circles in Fig. 1) over the austral summers of 2009, 2010, 2011 and 2012 to assess physical and biogeochemical processes and related interannual variability (Fig. 4).”

“Figure 1. Map showing stations for all years: underway sampling region (black dots), 48 repeat ice shelf CTD stations (two small groups of cyan circles) and the January 2011 CTD (red dots) UCTD (green triangles) stations. The mean locations of the southern ACC front (SACCF) and the southern boundary of the ACC (SBdy), as determined from satellite altimetry (Swart et al., 2010), are depicted with magenta lines. The regional bathymetry (ETOPO2) is overlaid (m below sea level).” - P1658, L13: “by van Hoven”. Please provide the correct citation. Thank you, “van Heuven” has been corrected. - P1658, L13/14: How was the uncertainty in DIC and Alk computed? The uncertainty was computed using the Dickson and Goyet (1994) guidelines by analysis of certified reference materials. The following sentence has been added to the text: “The precision of the DIC and TA data was determined by analysis of CRM's (Dickson and Goyet, 1994) and was found to be $3.10\mu\text{mol.kg}^{-1}$ and $2.60\mu\text{mol.kg}^{-1}$, respectively.

- P1658, L18: How was Ω_{arag} calculated from DIC and Alk, using which formulation? Thank you, this was not clear in the text. Ω_{arag} was calculated from DIC

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and Alk using the CO₂Sys code (Lewis and Wallace, 1998). We have added this to the methods: “WW Δ was derived from the mean TA and DIC concentrations in the WML using the CO₂Sys code (Lewis and Wallace, 1998).” - P1659, L4: replace “were” with “where”. Thank you, this has been corrected. - P1659, L10: The uncertainties in DIC and Alk were mentioned previously, no need to mention them twice. Thank you, this has been removed. - P1659, L16: I assume the authors are referring to the RCP8.5 scenario, when they write “IPCC business-as-usual”. Please specify. Thank you, we are referring to the RCP8.5 scenario and this has been corrected in the manuscript. “Atmospheric CO₂ levels are predicted to reach approximately 550 μ atm by the middle of the 21st century and double the current value by the end of it (IPCC 2013, RPC 8.5).” - P1659, L24/25: I don’t understand what is meant by “. . .using midnight as the daily time step”. Please clarify. – Thank you, this was a mistake as we feel that it is irrelevant to state this if we looking at daily averages. We thought it would make it clearer to the reader to mention that our days started and ended at midnight, but we have taken it out of the methods. “The 6 hourly estimates were averaged to daily wind fields.” In general, the results section should be written in the present tense. Thank you, this has been corrected. - P1660, L3-5: I don’t understand what is meant by “. . .an equally strong interannual variability”. Please provide numbers in the text for this interannual variability of the phasing and magnitude of Omega_{arag}. We agree that this sentence was a bit confusing. It has been changed to: “The four-year data set obtained from the ice shelf in the EWG shows a strong seasonal mode of aragonite carbonate saturation (Δ), (Fig. 3a-d). There is also strong interannual variability in the summertime maximum Δ , with a summer Δ maximum of 2.32 in 2009 and in 2010/2011 and 1.76 and 1.89 in 2010 and 2012 respectively (Fig. 3a-d, Table 1).” - P1660, L5-10: This sentence should be part of the discussion, not merged with the results, as this is not part of the analysis done in this manuscript (see my general comment above). C1384 BGD 12, C1380–C1385, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper We do agree that this sentence is part of the discussion but as we have decided to keep the results and discussion sections

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together this is not possible. We do agree that it is misplaced and we have addressed this by: Moving this sentence to the introduction, as we felt that it would be of value to define the contributing factors to the omega minimum in WW in the introduction. “The winter sub-surface minimum $\Delta\delta_{\text{A}}$ is due to a combination of convective mixing, the entrainment of CO₂-rich Weddell Sea Deep Water (WSDW), brine rejection associated with the formation of WW (Mosby, 1934; Carmack and Foster, 1975; Carmack and Foster 1977) and winter light limitation of ocean primary productivity (Arrigo et al., 2008; McNeil and Matear, 2008; Thomalla et al., 2011), with the entrainment of WW being the dominant contributor to the winter minimum in $\Delta\delta_{\text{A}}$.” By changing the first paragraph of the discussion to: “3.1 The characteristics of $\Delta\delta_{\text{A}}$ variability The four-year data set obtained from the ice shelf in the EWG shows a strong seasonal mode of aragonite carbonate saturation ($\Delta\delta_{\text{A}}$), (Fig. 3a-d). There is also strong interannual variability in the summertime maximum $\Delta\delta_{\text{A}}$, with a summer $\Delta\delta_{\text{A}}$ maximum of 2.32 in 2009 and in 2010/2011 and 1.76 and 1.89 in 2010 and 2012 respectively (Fig. 3a-d, Table 1). Within the upper 200m, $\Delta\delta_{\text{A}}$ reaches a sub-surface minimum (~ 1.3), (Fig. 2b) in winter, primarily due to the entrainment of WW.”

- P1661: The first paragraph should go into the introduction, in my opinion. This is what I was lacking from the introduction: a clear explanation as to why it is important to investigate interannual variability in Omega_arag. Thank you, we agree with this and have moved this section to the introduction. - P1661, L20: What do the authors mean by intraseasonal? It seems that this is sometimes used instead of interannual. Please be consistent. Thank you for this valuable comment. By intraseasonal we mean within a season not necessarily between years, as is meant by interannual. We do tend to confuse the two and we have corrected this throughout the manuscript. In this case, L20 we feel that intraseasonal is the correct word to use as we are referring to the variability within a season. We have corrected this throughout the manuscript. – P1661, L8-23: This whole paragraph mainly states results found in other studies (e.g., correlation between primary productivity and the magnitude and phasing of the summer increase in Omega_arag). However, the authors don't provide any quantification of

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their own results and just refer the reader to the figures. Furthermore, in Line21, how did the authors determine the two main drivers? What is their proposal based on? I find it very difficult to follow the authors' argumentation here. Thank you, we agree that this section was not well written. We have addresses this by re-writing the section to correct this. As it is there as an introductory paragraph to the discussions, we feel that it should provide some previous research and a quick overview of the trends that we found in our data before we discuss the results in more detail and we provide quantification later in the manuscript. We hope that this answers your question. We have copied the corrected section below. Line 21, our proposal is based on our results and our conceptual model. The conceptual model is a representation of the possible three states of the summer physical and biogeochemical surface water conditions. We have addressed this by referring to the conceptual model in this section. "Our data showed coherence in the response of $\Delta\omega$ (mean summer increase in $\Delta\omega \sim 0.77$) to variability in buoyancy (temperature and salinity) and wind stress forcing (Fig. 3, 5, 6). Temperature (Fig. 3i-l) and salinity (Fig. 3e-h) reflect an expected seasonal cycle of decreasing salinity, with sea-ice thaw forming a shallow mixed layer, which enhances the associated warming rates and further strengthens stratification (see conceptual model, Fig. 6). It is well known that the summer increase in carbonate at the ice shelf ocean domain around Antarctica is highly correlated to the response of primary production to summer surface boundary layer dynamics (Roden et al., 2013; Shadwick et al., 2013, Taylor et al., 2013; Mattsdotter Björk et al., 2014). Our results are consistent with these studies and highlight the importance of summer primary production (Fig. 4) in the EWG as a key element to creating a more suitable habitat for calcifiers by reducing surface water $p\text{CO}_2$ resulting in an increase in surface water pH and $\Delta\omega$. The direct impact on the biology is driven by the magnitude of ω but the seasonal magnitude of the $\Delta\omega$ is influenced by the phasing of the sea ice thaw and its impact on the spring-summer phytoplankton blooms. What is remarkable in our data set are the contrasting magnitudes of both seasonal and intraseasonal variability in surface water temperature, salinity and $\Delta\omega$ observed during the four year period. Arrigo

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et al (2008) show how seasonal modulation of stratification and the mixed layer depth through the entrainment of denser WW reflects variability in the relative magnitudes of buoyancy and mixing. We propose through our conceptual model (Fig. 6) that the two key drivers of this variability in seasonal cycles are: the rate of sea ice thaw, which is the primary driver of surface water density (buoyancy forcing) and stratification through its impact on salinity (Fig. 3e-h, 6a-d), and wind stress (Fig. 5e-h), which regulates the mixing fluxes. ”

- P1662, L27 Thank you, we assume you are referring to the use of intraseasonal and interannual? We are aware that we have interchanged these words and we have addressed this by changing intraseasonal to interannual. “The seasonal and interannual variability of ΔA is dependent on changes in TA and DIC ($[\text{CO}_3^{2-}] \approx \text{TA} - \text{DIC}$), which are both predominantly modulated by sea ice thaw, dilution, mixing, and primary production (Sarmiento and Gruber, 2006).” - P1663, L9: This paragraph doesn’t belong here and should probably go into the introduction or into the methods. Thank you, this sentence has been moved to the introduction. - P1662: It was not clear to me when and where this model was used and which subsequent results were obtained by applying this model. This conceptual model has been used to create a visual representation which explain the different summer conditions that we found from our 4-year dataset. - Fig. 1: Caption: The regional bathymetry is not overlaid, but rather “underlaid”. “and” missing: “CTD (red dots) and UCTD (green triangles)”. In general, the phrasing of the caption is somewhat confusing. Please improve. Thank you, we agree that the Fig 1 caption is unclear and we have changed it to: “Figure 1. Map showing stations for all years: underway sampling region (black dots), 48 repeat ice shelf CTD stations (two small groups of cyan circles) and the January 2011 CTD (red dots) UCTD (green triangles) stations. The mean locations of the southern ACC front (SACCF) and the southern boundary of the ACC (SBdy), as determined from satellite altimetry (Swart et al., 2010), are depicted with magenta lines. The regional bathymetry (ETOPO2) is overlaid (m below sea level).” Fig. 4 (now Fig. 3 as we removed Fig. 3): They grey lines showing the +/- 1 std are barely visible. A shading in a different color around the

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black line might be easier to see. Thank you, we have made the shading of the grey lines darker as we felt that if we changed the colour of the standard deviation lines they would distract from the actual data. We hope that this looks better.

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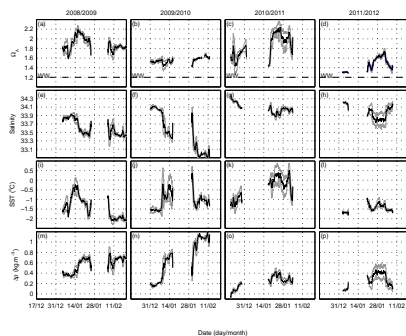


Fig. 1.

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