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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, C1229–C1231, 2015 www.biogeosciences-discuss.net/12/C1229/2015/ © 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 âĎęA at the ice shelf in the Eastern Weddell Sea – an ocean acidification sensitivity study" by A. Weeber et al.

Response to referee: T. Johannessen (Referee) Truls.Johannessen@gfi.uib.no Received and published: 16 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.

(Green or the colour on the pdf-file text are just to remind myself of things and should not be regarded a part of the review, unless there is comments attached.) In general this paper is well written and their opinions well documented and their statements refer to the relevant international publications. Technically speaking this paper can without doubt be published. A read this paper with great pleasure, then being a scientist concern with Arctic reC1229 BGD 12, C1229–C1231, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper search and environment as well and then on the other hemisphere. I liked the reasoning and discussions putting the physical, chemical and biological impacts together. In general, Sverdrup in 1953 made a comprehensive model bout critical depth and seasonal change. The new aspects of this paper that are rare opportunity from the AWC is the documentation of different interannual state of operations of the physics and how this interact with both changes in the general biogeochemistry very well summarized in chapter 3. This a very complex story taking into account several physical properties and the critical ones seems to be the start of the melt season and wind field. The optimal start will be when the melt season, wind and light conditions are in symphony. In addition, that the density change about 0.4 kg/m3. Higher or lower values will both lead to less productivity. It is also well known the coupling between biological production and the variability in omega either it is for calcite or aragonite. Aragonite is of cause the most important to address because of its higher sensitivity to ocean acidification. I went through all calculation using the same program as the authors i.e. tha latest version based upon the heroic work done by Lewis and Wallace, 1998. The handling of the inorganic carbon chemistry calculation seems to me to be proper done. The precision on the analytical work At and Ct could have been better, but taking into account the harsh weather and that these measurements most likely is done at sea under stressful condition their results are acceptable and a better precision would not have changed the general story much. Then does this paper increase our knowledge about the impacts on different species in the high CO2 world? One of the critical points is: Is the statement that when the Omega get less then 1, that a calcifier becomes vulnerable? Some species protect themselves with protoplasm and generate their own microchemistry inside and can produce carbonate even in under-saturated conditions. My point in that there are not necessary a direct link between OMEGA depicting the chemical equilibrium state and the biological induced C1230 BGD 12, C1229–C1231, 2015 Interactive Comment Full Screen / Esc Printer-friendly Version Interactive Discussion Discussion Paper state where species might have strategies to protect themselves. This is of cause out of the scope for this paper to answer, but should be a general concern. And then there is the question of adaptation. As we all know in experimental mode we expose the biota with an unrealistic fast change that will not be the case under natural condition. These general comments will apply to the introduction part of the paper and almost all references therein. My point is that not all work on the ecosystems implies that catastrophe is about to happen, but at the same time it is important to strongly express our concern under the precaution act. There is a comprehensive discussion about these problems in discussion section 4 in the first paragraph. In general (My comment and suggestions of changes is written in red in the pdf file). 1. Introduction is clear and well written. 2. Methods need to be revisited by the autor(s). 3. In general well written, need some minor revision 4. Excellent and very clear. 5. Conclusions strait to the point and clear. To conclude: Some attention is needed on chapter 2. I expected a regression line in figure 3. In general revisit also the table and figure text. As earlier told all my detailed comments are directly written into the pdf file. With my suggested changes this paper is well worth to be published with minor to moderate revision. Please also note the supplement to this comment: http://www.biogeosciences-discuss.net/12/C1229/2015/bgd-12-C1229-2015- supplement.pdf

We have made the following corrections in the paper, as suggested in the supplement to this comment: http://www.biogeosciences-discuss.net/12/C1229/2015/bgd-12-C1229-2015- supplement.pdf

1.P1655, L18: (carbonate saturation state (âĎęA) > 1) Thank you we agree with this comment and this has been changed to: "Marine calcifying organisms such as coccolithophores, pteropods, molluscs and corals rely on carbonate supersaturated state > 1 waters to minimize energy costs for the formation of their shells and skeletons (Fabry et al., 2008)." 2. P1658, L5: Thank you, the spelling of 'biogeochemical' has been corrected. 3.L14-15: Is the precision of DIC and TA swapped around, as the precision of DIC usually varies from 1-210μmol.kg-1? Thank you, we have checked this and the precision of our data is in the correct order. We are aware that the precision of our DIC data is slightly high but we unfortunately cannot change this. 4. L16: "Was estimated to have been located" has been changed to: "is defined as" 5. L17: "As in" has been changed to: "following" 6. L18: "This WML water was" has been changed to: "and" 7. L20-24: Thank you, As suggested, "TA and DIC data was only available at specific bottle depths from a CTD section sampled in January 2011, and thus the closest bottle to the WML for each CTD cast was used to calculate a WW âĎęA $\sim$ 1.3" has been changes to: "Residual WW is defined by a potential temperature (θ) minimum in the winter mixed layer (WML), following Jones et al (2010) and Geibert et al (2010) and found between 55-110m depth (Fig 2a). WW âĎęA was derived from the mean TA and DIC concentrations in the WML using the CO2Sys code (Lewis and Wallace, 1998). Following the definition of the WML and calculating the WW based on the mean TA and DIC therein, gave a WW âĎęA of 1.3 (Fig. 2a). A contour of âĎęA from the ice shelf to 61.5°S was plotted using the bottle CTD data (Fig. 2b). The change in density (Δ) from WW (WW$\sim$ 1027.61 kg.m-3) to summer surface water was calculated."

8. P1659, L7: Fig 3 should show the regression line. Thank you for this valuable suggestion but due to the recommendations of another reviewer's comment we have decided that Fig 3 does not contribute to the manuscript and has thus been removed. 9. L12: Was empirical âĎęA calculated to fill gaps for missing data? We used fCO2, TA, SST and SSS to calculate an empirical surface water âĎęA dataset, as we did not have any data for surface water âĎęA. 10. L15: "increased to" has been changed to: "as" 10. L15-17: As suggested a sentence has been added to discuss how realistic these shortcuts are. In a high CO2 world, lower pH will lead to more CaCO3 dissolution and higher alkalinity. "This estimation does not take into account that with increased CO2, pH will decrease and thus there will likely be more CaCO3 dissolution resulting in higher TA, but the complexities of this dynamic system are not well understood."

11. P1661, L2-3: The reviewer wrote:"most plausible also the vulnerability of". We are not sure where in the text this is referring to but it does not seem in agreement with the paragraph in which it was written. We apologise for thus not addressing this comment. "Interannual variability in the magnitude of the seasonal cycle of âĎęA highlights the importance of regional studies at the ice shelf ocean domain around Antarctica, in order for us to begin to understand the vulnerability of the carbon system and ecosystems in this region to century scale increases in anthropogenic atmospheric CO2." 12. P1661, L12 and 13: "to" has been removed but due to the recommendations of other reviewers this section has been rewritten. We hope that this addresses your comment. "Our data showed coherence in the response of âĎęA (mean summer increase in âĎęA $\sim$ 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

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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 pCO2 resulting in an increase in surface water pH and âĎęA. The direct impact on the biology is driven by the magnitude of omega 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."

13. P1661, L24: Have you produced a forcing curve based upon the windstress and melting and then correlated this with the annaul and interannual changes in omega in a statistical form? Thank you for this comment. We feel that this would be a very interesting addition but is beyond the scope of the current study. We will follow up on this in our next phase of work on this region. 14. P1662, L18: Thank you, as suggested the brackets have been removed and "indicating" has been added so that the sentence now reads: "Phytoplankton production and âĎęA (âĎęA> 1.7) peak when Δ is approximately 0.4 (i.e. moderately stratified). When $\Delta < 0.4$, indicating weak buoyancy forcing from delayed or slow sea-ice thaw and/or strong wind stress, or $\Delta > 0.4$ indicating strong buoyancy forcing from sea-ice thaw and/or weak wind stress, phytoplankton production and summer âĎęA (âĎęA < 1.6) are at varying minimum magnitudes (1.3 – 1.7), (Fig. 3 and 5)."

15. P1681, Fig 2: Your figure text is a bit unclear to me. I assume for a) you mean the lowest measured potential temperature and this correspond to an WW Omega of 1.3 and is situated between 50 and 100 meters?

Thank you, we agree that this Figure text is unclear. The figure caption has been changed to:

"Figure 2. (a) Potential temperature profiles with aragonite saturation state (âĎęA) overlaid at the depths where potential temperature is at a minimum for each profile, between South Georgia and the Antarctic ice shelf. CTD stations are indicted with grey lines. (b) Aragonite saturation state (âĎęA) from bottle data, CTD stations where TA and DIC were sampled are indicted with grey lines."

16. P1687, Fig 8: Figure 8 is now Figure 7 as we have removed Figure 3. As suggested, "in due" has been changed to "is" and "to" has been changed to "caused by" so that this caption now reads:

"Figure 7. Vector plots (DIC-TA) showing the seasonal evolution of mean December, January and February total alkalinity (TA) and dissolved inorganic carbon (DIC) at the ice shelf. Winter Water (WW) conditions calculated from CTDs (ΩA WW $\sim$ 1.3). Vectors depict the rate of dilution and photosynthesis that altered TA and DIC, contours show aragonite saturation state (ΩA) with temperature = -1°C and salinity = 33.3. The two seasonal extremes are depicted: (a) a high ΩA summer during December 2010, January and February 2011, (b) a low ΩA summer during January and February 2012. Total seasonal dilution is comparable so the difference in ΩA is primarily caused by photosynthesis."

Interactive comment on Biogeosciences Discuss., 12, 1653, 2015.