

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

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(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 re-

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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 about 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/m<sup>3</sup>. 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. the 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 CO<sub>2</sub> world?

One of the critical points is: Is the statement that when the Omega get less than 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

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state where species might have strategies to protect themselves. This is of course 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 author(s). 3. In general well written, need some minor revision 4. Excellent and very clear. 5. Conclusions straight 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>

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