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Interactive comment on "Wind-driven interannual variability of sea ice algal production over the western Arctic Chukchi Borderland" *by* E. Watanabe et al.

Anonymous Referee #2

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The study by Watanabe et al. focuses on the output of a lower trophic ecosystem model for the Arctic Ocean, including sea ice algal components. It demonstrates the relevance of wind, and the resulting ocean physical and biological responses including the vertical export of biological material from the euphotic zone with a focus on the relevance of the sea ice component. This is an interesting research idea and question, and the model output demonstrates interannual variability in the biological response due to the physical environment. As such the research questions and many parts of manuscript deserve to be published. However, it requires substantial revision to clarify its contents and make it most useful for the reader.

First I would like to state, that the entire manuscript requires substantial language edit-

ing by a native speaker, as frequent language issues are distracting from the contents. This is going to be a substantial task.

Secondly I encourage the authors to make a stronger link between the model output and the observations from the field. This has not been strongly developed so far and is missing in many sections

I recommend to restructure the manuscript according to a more conventional style so that the authors truly provide a thorough discussion of the various aspects of the model output. this is by far the weakest part of the manuscript, which reads in large parts more like a report than a peer reviewed publication. The authors should also be asked to reflect the current state of the literature.

It appears to be very important to more clearly explain the building blocks of the model and justify the chosen variables and parameters, followed by a sensitivity analysis. All model output should be compared to field observations.

Specific comments:

Abstract The abstracts provide a general overview about the study purpose and the outcome. It will be much clearer after a language editing. I suggest to add the depth of the sediment traps (line 6). I have a major issue with the use of sediment trap to understand sea ice algal primary productivity. Statements like in line 13 are wrong, as traps only capture export production and not true in situ primary production. The abstract highlights the differences in the model output and field observations and the interannual differences.

Introduction:

The introduction provides a short overview about the research question of vertical flux measurements, sea ice algal modeling and fate of sea ice algae after ice melt. It appeared to me rather unstructured, and the readers were left alone to link the various sections. The references appear outdated in many parts, and some of the information is misleading or wrong. Several key papers about primary production, vertical flux and shelf basin exchange for the Chukchi/Beaufort Seas are not used to make the case. For example: newer models dealing with ice algal activity include Duarte et al. 2015 (J. Mar. Systems), work by Tedesco, or Moreau et al. 2015 (J. Geophys Res.). For Chukchi Sea: Moran et al. 2005 - flux (Deep Sea Res.), Gradinger 2009 – sea ice algae (Deep Sea Res.)

Again thorough language editing is needed to clarify the scientific message.

Specific suggestions: Pg 7742, line 4: how can relative abundances suggested nutrient conditions, not clear from the text.

Pg. 7743, line 5: not clear to me: during freezing brine convection is a major supplier of nutrients into the ice, as well as boundary layer processes. Same page line 11: the statement that melt causes sea ice algae to be detritus is wrong – by definition, they are then phytoplankton. Detritus is defined as dead organic material.

Material and methods

Section 2 explains the model components. It uses an established model for ocean properties. The sea ice component is explained in general, however it lacks detail to fully understand the applied approach. It is hard to assess the value of the output if there is great uncertainty in the validity of the input.

For sea ice, they suggest a maximum growth rate of 0.8 per day for sea ice algae. This appears to be high compared to the maximum rate suggested by Eppley (1972) of 0.85 – how were temperature effects compensated?

The nutrient exchange calculation is difficult to follow. The sea ice algae are exposed to the brine nutrient concentrations and not bulk concentrations of melted ice– did they include brine pumping during freezing? How were conditions in the brine calculated? Are any of the suggested variables and parameters for determining the algal growth response related to any published measurements or are they just guesses – this needs

to be much better explained for all algal growth variables. I suggest to include a table similar to Diane Lavoie (2005) modeling paper table 1, including references for the used variables and parameters chosen.

As a side note – there is no zooplankton in sea ice – check the definition of plankton. You should use the term sea ice fauna or sympagic fauna for those animals living inside the ice. However there is true feeding of zooplankton on sea ice (e.g. Durbin 2013 paper from Bering Sea).

Regarding the export – it is not clear from the paper, whether any part of the released sea ice algae are consumed in the surface waters by zooplankton or stay there as part of the phytoplankton community to start the pelagic spring bloom – how are these two processes included? Also dissolution of diatom frustules can be substantial – accounted in any way?

In section 2.3 I would like to know whether the pacific inflow matches the observations from e.g. Woodgate and others.

I do not understand pg 7749, line 5: does “dissolution” refer to bacterial reminerailzation including e.g. annamox processes or what is meant by this? Same page, line 8: what is the lower limit of ice algal concentration?

Section 2.4 The traps were deployed in two very different depths in the two years – any impacts on the results? Also why were PON filters acidified? Did you remove swimmers prior to analyses from the filters?

Section 3 The biological respresentation of the model output is rather limit. N to Chl ratios can vary widely, and comparisons are made to outdated review papers (e.g. Cota et al. 1991) – here a thorough discussion of model output in comparison with real data is needed. E.g read studies by Sang Lee. Also I would encourage the authors to conduct a detailed sensitivity analysis – see e.g. studies by Jin.

The section of imported nutrients does not offer anything new – this has been published several times how important the pelagic nutrient pool is for sea ice algae – again, here it needs a more in-depth discussion and comparison to observational data.

Section 4.1 Data on ice velocity seems reasonable, this section is lacking any comparison to observational data.

Section 4.2 Again more or less a description of the model output and no discussion of the output. The flow regime in the area is complicated as outlined in many peer reviewed papers, that could form the basis for a discussion. The 2014 book chapter by Kinney et al. provides a useful analysis for the Bering sea region. Weingartner et al. 2013 for Chukchi Sea etc.

Section 4.3 The analysis of the impact of various environmental factors on ice algal productivity would benefit from a sensitivity analysis (see above).

Section 4.4 Good that the authors conduct a comparison of field observations and model output in this section. I recommend to explore further the role of advective processes leading to sedimentation event (for 2011 May peak).

I did not find a clear explanation what happens to the ice algae during periods of strong ice melt (e.g. loss of 4 cm d-1) – are all the algae lost into the water – and how is growth reseeded in the new bottom layer?

Interactive comment on Biogeosciences Discuss., 12, 7739, 2015.