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Interactive comment on "Winter to summer evolution of pCO_2 in surface water and air–sea CO_2 flux in the seasonal ice zone of the Southern Ocean" *by* D. Nomura et al.

Anonymous Referee #1

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The paper presents and discusses measurements of CO2 and accompanying variables (T, S, Chl) for one week in January 2006 in the southern ocean seasonal ice zone. It estimates the winter to summer evolution via estimations of the various contributing components and the assumption that conditions in the temperature minimum layer (TML) represent conditions of the previous winter. While the paper does not provide any particularly exciting and groundbreaking new science aspects it adds to the sparsely available pCO2 data in ice covered waters. The analysis is for the most part straight forward and clear although some assumptions could be evaluated a bit further and the addition of uncertainty ranges would be valuable. The paper is also lacking a conclusion section, which I however think the authors can easily provide. Generally, I

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think, the authors made the most out of the limited data they had available. I hence recommend publication after some minor/major changes/additions. (Please note that I clicked the button for accepted subject to minor revisions, but it should be somewhere between minor and major)

Specific Comments:

Abstract p658 line 11-14 rm “with” behind flux and rephrase sentence for clarity line 15 add “temporary” before CO2 source line 18 add is a CO2 sink => is again a CO2 sink

text p 658 line 20 the site => an area ...CO2 fluxes

p 661 Are there error margins available for the pCO2 measurements?

P 662 line 24 Maybe reformulate : sea ice was at it’s minimum during our observation period => sea ice just reached it’s minimum before disappearing in the summer (otherwise it sounds like the sea ice min is in January).

P 662 line 25 Sea ice concentration data . . ..this trend=> This part of the sentence can probably be deleted and Fig 4 just be referenced at the end of the previous sentence. P663 Line 4 occurred later here => give time

P663 is uncertainty measure for Seawifs Chl available?

P664 line 17-20 repetition, just refer to intro

line 24 ice covered water gave way => retreating sea ice gave way (The water is probably the same :-))

p665 line14-18 again repetition, refer to intro

p666 line 16 the authors provide a maximum ikaite concentration which they use for their calculations, shouldn’t rather a mean concentration be used? Or at least an error margin provided?

All equations are a bit convoluted due to the lengthy subscripts, I think they could sbe

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significantly simplified. Some suggestions are: PCO2 air => PCO2a PCO2water => PCO2w pCO2 water(winter to summer) =>PCO2w_w2s or win2sum also remove all periods when using abbreviations in variables: pCO2cal. => pCO2cal

equation 7 and 11contain S used for sea ice concentration. S is frequently used for Salinity, which is in fact done in this same paper in equation 10. I suggest picking a different variable. E.g. A is frequently used for sea ice concentration/ice area coverage.

P669 line6-8 could an actual value be calculated based on the earlier estimate from delta DIC (p666 line16-20).

Line 24/25 this statement is a bit unclear. Although I understand the reasoning, the fact that pCO2 obs changes and the difference reflects this biological effect, it seems odd to state that the difference only represents the thermodynamic effect. => reformulate Maybe on that occasion also point out the similarity between Fig 10 and Fig 2b.

Equation 11 is a bit confusing due and again the use of "Air-ice-sea FCO2" as a variable name adds to the confusion. I would suggest to just mention that a term FCO2i is added to equation 7 and limit eq 11 to FCO2i=Fice S/100 (note the minus sign is also confusing make sure sign convention is consistent , generally I would expect fluxes to be additive. If they then turn out to be negative due to a specific process, it maybe so, but the equation should state the addition.

If I understand correctly the "flooding" case assumes ALL ice is IMMEDIATELY flooded. This strikes me as unrealistic and needs further explanation. I can see the flooding case as an extreme state, with true solutions between the flooding case and no-exchange-if-ice case, however it would need to be discussed as such.

Conclusion section is missing and in consequence it is not really clear what are the main results and what is the take away message, please add.

Fig3: I don't think all the satellite pictures provided are needed, at least a and b can be removed, since they do not add value. The authors could rather consider adding a

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figure representing the annual minimum ice cover

Interactive comment on Biogeosciences Discuss., 11, 657, 2014.