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Interactive comment on "Ocean-atmosphere exchange of organic carbon and CO_2 in the Antarctic Peninsula – physical and biological controls" *by* S. Ruiz-Halpern et al.

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Review: Ocean-atmosphere exchange of organic carbon and CO2 in the Antarctic Peninsula – physical and biological controls

Authors: Sergio Ruiz-Halpern et al

Reviewer: Wiley Evans

Ruiz-Halpern et al present a unique and novel data set of exchangeable dissolved organic carbon measurements made in seawater (EDOC) and in the lower atmosphere (GOC) adjacent to the Antarctic Peninsula. They compute sea-air organic carbon fluxes

from these data and compare these fluxes to estimates of sea-air CO2 exchange. There are very few measurements of sea-air OC flux available globally, and for that reason I would recommend this paper for publication following moderate revision. I have described below points I believe will aid the authors in a successful publication. Best regards.

General Comments:

Given the dynamic nature of the region you have sampled, do you believe the order days of data you have collected well represent the austral summer season as a whole? Did you observe any inter-annual variability? It is hard to follow your discussion of the data because it seems like you are talking about the 3 years collectively. If that is the case, you might consider changing the focus from characterization of the season to specifically to the source/sink nature of the EDOC data. That seems to be the punch line of the paper given these results are very different (i.e. some source regions) from measurements you have made in other regions.

The title of this paper suggests there will be a detailed description of the physical controls on OC and CO2 fluxes. Other than brief discussion of the winds and its influence on gas exchange, the paper is lacking in the analysis of physical controls and instead focuses on biological controls. Consider revising the title or expanding your discussion of physical controls. Specifically, in that regard, how does sea ice melt (and/or glacial melt from the peninsula) impact surface fCO2 and EDOC concentrations? I understand there might not be data in this set of measurements to resolve sea ice melt contributions, but some discussion should be included. How do fCO2 and EDOC compare in T/S space? Is there any evidence of upwelling as a driver of fCO2 and EDOC variability? I found the diel variability very interesting. You should consider comparing fCO2 diel variability to that of EDOC. How are they related, if at all, and what does this imply of the data collected from broad ship surveys in this region?

Specific Comments:

“air-sea”, “air-water”, air-seawater”, and “air-sea water” are all used a number of times in this manuscript. Pick one and stick to it throughout.

“Indeed” is used a large number of times in the manuscript. Consider trimming the use of this down.

Introduction:

Page 16176, Line 26: remove second “neither”

Page 16177, Line 11: replace “to” with “with”.

Page 16177, Line 15: Consider citing newest IPCC report.

Methods:

Section2.3: This section needs more clarification. Atmospheric measurements were made every minute? Or do you mean equilibrated air? What was the sample frequency of the seawater data? Fugacity was calculated from xCO2 (I’m assuming, maybe add an equation?), so why is partial pressure mentioned? How frequently was a calibration sequence run? State that your zero concentration is N2 and your span gas is 541 ppm.

Section 2.5: Did you calibration your echosounder data using net samples? Are the chlorophyll and Krill data presented in this manuscript depth integrated (i.e. what are the data shown in Figure 4?)

Section2.7: k600 needs to be adjusted to in situ SST and salinity (or at least SST since the salinity correction is usually « 4%). It isn’t clear how you calculate k0. Consider adding an equation here. Am I correct in thinking you averaged 1-min FCO2 data for +/- 30 minutes about the EDOC and GOC measurements to do your comparison? How you center your hourly averages might affect your comparison.

Results section:

In each section are you discussing your results from each cruise collectively? There

was no inter-annual variability apparent between 2005 and 2009?

Section 3.1: Include some description of the variation in surface ocean solubility with the discussion of temperature and salinity. Move mention of fluorescence and chlorophyll to Section 3.2.

Section 3.2: Line 25: insert “times” following 300-fold. Line 28: delete “Surprisingly”.

Section 3.3: Line 8: fCO2 is not partial pressure. Line 13: replace “than” with “to”. Solubility had no influence on the variation of FCO2? Line 17: replace “seem to act” with “acted”. Page 16186, Line 2: I recommend holding to the convention of uptake being negative fluxes. Lines 5-8: consider rewriting this sentence using past tense and replacing the word “prevailed”. Lines 9-12: were chlorophyll and krill concentrations depth integrated? If not, perhaps doing this integration would improve the fits.

Section 3.4: How was the surface micro-layer sampled? This is not in the methods.

Section 3.5: The diel EDOC data is fascinating. Was a particular water mass tracked during the collection of these samples? If so, how did you ensure you weren’t crossing between water masses during the diel sampling?

Discussion:

Line 5: delete “an”.

Line 18: replace “form” with “from”.

Section 4.2: Sea ice melt plays a large role in shaping surface ocean pCO2 distributions in this region. Based on Figure 4, it looks like this and other physical-chemical alterations to the carbon system are driving variability well beyond the biological forcings as indicated by chlorophyll and krill concentrations. Please comment on this.

Page 16190, Line 1-2: You could test this by comparing your 1-min (?) data to the hourly average data. You should do this to rule out any possibility that the hourly averages are not capturing short-term large CO2 fluxes. As it stands the comparison

of OC flux data to CO2 fluxes is a bit apples-to-oranges given that the methods were not clearly described (how is k0 calculated?), and have issues (k600), as well as the possibility of temporal mismatch (is your hourly average centered about the EDOC measurements?).

Section 4.4: How does the diel variability of EDOC compare to that of fCO2? Please comment on how this variability could corrupt your interpretation of the source/sink nature of a region depending on when sampling was conducted.

Conclusions:

Lines 15-16: Delete “as 57% of the stations indicated an OC flux from the atmosphere toward the ocean” as this is redundant.

Line 22: insert slash between “source” and “sink”.

Line 23: replace “other” with “others”.

Page 16193, Line 17: delete “to”.

Page 16194, Line 11: replace “on” with “about”.

Figures and Tables:

Table 1: Are these hourly average data? So the ranges are hourly averages for each cruise and area?

Figure 1: Consider color-coding station locations. Even with different symbols, overlapping stations are hard to discern.

Figure 2: Which cruise are these data from? All? Consider changing projection and reducing white area.

Figure 3: Which cruise are these data from? All? Consider changing projection and reducing white area.

Figure 4: Are chlorophyll and krill concentrations depth integrated?

Figure 6: Flux distributions peak near zero, so maybe it would be good to show refine the binning to show this better? You could stack the plots vertically, increase the binning and change the aspect ratio to highlight the large positive and negative Faw data.

Figure 9: Why is there no solar radiation data for 3-5 February in top panel?

Interactive comment on Biogeosciences Discuss., 10, 16173, 2013.