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Interactive comment on "Climate change impacts on sea-air fluxes of CO_2 in three Arctic seas: a sensitivity study using earth observation" *by* P. E. Land et al.

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We thank the reviewer for his/her comments.

The paper is focused on net fluxes across each of the three seas and their sensitivity to changes in environmental conditions, whereas the reviewer's comments mainly focus on the gas transfer velocity in the sea-ice margins and areas of ice. We acknowledge that our method simplifies or neglects many processes within sea ice. However, we also note that the data on these processes in the published literature is far too scant to produce parameterizations applicable to the whole region, and these issues are currently the focus of considerable international effort. The main evidence seems to be

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that gas transfer velocities are greatly enhanced locally in regions of broken sea ice, i. e. marginal ice zones (e. g. Else et al 2011). It may also be the case that transfer velocities are reduced in the open water adjoining the marginal ice zone, where the fetch is low (Woolf, 2005). It is unclear how most of the effects mentioned by the reviewer could be parameterized in terms of variables available from satellite remote sensing. Information on this will be added into the discussion.

In addition, the main thrust of our approach was to use remote sensing to estimate the sensitivity of open ocean CO2 flux to long-term temperature and salinity changes, rather than to produce an authoritative estimate of the air-sea flux itself. In all but the Kara Sea, ice covered regions are smaller than open ocean areas for most of the year (annual mean ice cover 27%, 16% and 57% for the Greenland, Barents and Kara Seas), and the Kara Sea has by far the smallest magnitude of CO2 flux of the three seas due to its low concentration difference, so contributes very little to the regional flux. Much of this will be fast ice, further reducing the effect. Information on this will be added into the discussion.

Our assumption thus far has been that total flux is linearly proportional to open water fraction down to 10%, after which it remains at 10% to account for leads, polynyas etc. This is the assumption made by Takahashi et al 2009, which also supplies the CO2 climatology on which our study is based. To address the reviewer's concerns about non-linearity, we will replace this with the parameterization of sea ice effect on gas transfer suggested by Loose et al 2009, which is that flux is proportional to (open water fraction)ˆ0.4. This is then combined with the Takahashi et al 2009 limit that open water fraction can be no less than 10% to account for leads, polynyas etc. The methods and results will be updated accordingly.

The reviewer's concerns about linear interpolation of the Takahashi et al 2009 dataset are unfounded. We are not trying to infer higher resolution changes than are present in Takahashi et al 2009, but simply representing the given data in a way that doesn't introduce artificial discontinuities. For example, consider two Takahashi cells with delta_pCO2 values of 0 and 30 uatm. The best estimate of the delta_pCO2 variation between the two is not values of 0 up to the midpoint followed by a discontinuous jump to 30 uatm (as the reviewer implies by their comment), but rather a smooth variation between 0 and 30 uatm. The shape of the variation can be argued over, but not its smoothness. Interpolation is absolutely standard practice when converting data to a higher resolution, eg in map projection. For example, a 2009 paper in EGU Atmosheric Chemistry and Physics interpolates the Takahashi dataset to higher spatial resolution than we do, and also extrapolates it into waters with no Takahashi data (Kettle et al., 2009). The important issue is to highlight this assumption within the paper. We will do this and we thank the reviewer for highlighting this.

If we reduced our other datasets to 4x5 degree resolution as suggested by the reviewer, we would lose much of the finer detail visible in the Earth observation data (which are driving the flux estimates), particularly in the sea ice extent. If we used the uninterpolated 4x5 degree Takahashi data with the higher resolution EO data, we would introduce erroneous features (Takahashi cell boundaries) into the flux maps which would make real features harder to discern. Information on this will be added into the discussion.

We have looked at the suggested SOCAT dataset as we are using these data in another piece of work. However, the coverage in the SOCAT dataset is spatially sparser in these Arctic regions than the data that we are using. For example, there are no data in the Kara Sea, while only about 50% of the Barents Sea is covered. Therefore, the updated paper will continue to use the Takahashi dataset as it contains data for all three seas. Information on this will be added into the discussion.

We intend to implement the change in ice effect and reprocess all of our data and update the results accordingly. We will correct any identified typographic errors. We will also expand the discussion to include comments about all of the sea ice issues that the reviewer has mentioned. We will include information in the discussion about the interpolation of the Takahashi data and its advantages over the SOCAT data.

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