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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.

Anonymous Referee #2

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In their manuscript, the authors assess the net CO$_2$ sink sensitivity due to changes in temperature, salinity and sea ice duration arising from future climate scenarios in three Arctic seas (Greenland, Barents, Kara). Such a study is very timely in important, since the Arctic is undergoing rapid changes presently.

However, the Arctic seas are highly heterogeneous areas with respect to gas transfer. Coverage by sea ice significantly complicates matters. Several processes affect gases' dynamics within sea ice. Such processes can be temperature change, brine concentration/dilution, brine transport, primary production and respiration by sympa-

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gic microbial communities and others biogeochemical processes. This all sums up to the fact that sea ice cannot be seen as a simple open/closed pathway between ocean and the atmosphere. At the same time, polynyas and ice leads have significant effects on atmospheric and oceanic turbulence, strongly enhancing gas transfer due to strong convection. At the same time, clouds are frequent in these conditions. Most of the aforementioned processes are highly non-linear. Therefore, a simple linear interpolation and weighted averaging of gas transfer by sea ice coverage seems highly questionable. Also, the authors do not take into account any sea ice properties such as ice thickness, which are currently available also from satellite measurements.

In this study, there are several areas in which much too simple models are used for capturing highly complex and non-linear processes. For example, in areas in which data is lacking, the mean of the fluxes from surrounding areas is used (cf page 12388, lines 23-24). It could be argued that this introduces a strong bias, as for example data in areas of polynyas might show lack of EO data while an enhancement of fluxes is present. Also, the Takahasi climatology is simply linearly interpolated from the $4 \times 5°$ to the $1 \times 1°$ used in this study. The authors need to clearly address the justification of using linear models and the errors/bias they introduce. On the same note, for the results they present in Fig.7-Fig. 10, no error bars or variance is given. Particularly given the simplifications introduced from their linear models, error bars and biases are essential to putting these plots into perspective.

I find the presentation of Fig. 12 very misleading. Different parameterizations are identified through the usage of error bars or not showing error bars. The authors should present error bars for all of the data and use different symbols for identifying the different parameterizations.

Interactive comment on Biogeosciences Discuss., 9, 12377, 2012.

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