

Interactive comment on “Winter to summer evolution of $p\text{CO}_2$ 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 #3

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General comment I have a mixed feeling about the work of Nomura et al. On the one hand, the data-set presented here is not very common. I've been surprised by the winter under saturation in $p\text{CO}_2$, that is not a very common feature in the Southern Ocean ice covered areas. There is also an attempt to present also air-ice/sea CO_2 fluxes that is pretty original. But for me the reconstruction of the assessment of the contribution of the different factors is not fully convincing and very exciting. There are some assumptions that are not 100% robust, especially in a sea-ice covered zone. I'm confident that the authors, who are well versed in sea ice studies, know how the ice can affect the underlying water. From a physical point of view sea ice affects turbulence and then piston velocity. Sea ice hosts also peculiar biogeochemical processes (DIC changes,

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precipitation of CaCO_3), and these processes are ignored in the computation of eq (1) and following equations. I know that it could be difficult to take sea ice processes into account. But at least the authors could discuss the residual. Such discussion might be the most interesting part of the manuscript, since residual corresponds in some extent to the contribution of sea ice peculiar processes to surface $p\text{CO}_2$ changes. It would have been something new, compared to the computations presented in the present form that have been done several times in the past.

Detailed comments: P664, line 6. I do not see so much demonstration here. The chl a concentrations are not so large compared to what can be encountered in marginal ice zones. Furthermore, in the manuscript, $p\text{CO}_2$ appears to be mainly affected by temperature, rather than primary production. In some other sea ice marginal zones, huge effects of primary production of primary production on $p\text{CO}_2$ are observed, but, in my opinion, it is not the case here.

P665, line 6. Why do you present so many digits, while obviously the last one is not significant?

P665, line 23. I would have replaced “would be supported by these results” by “are supporting these observations.”

P665, line 5. same comment that for P664, line 6.

P666, line 14. I would have written “that sea ice thickness was around 1 m thick” instead of “a sea ice thickness of 1.0 m here in winter”

P666, line 15. I do not see the purpose of this paragraph. I would have understood that you compare previous estimate from the literature, if you would have proposed your own assessment of the impact of CaCO_3 sea ice precipitation. But this is not the case, since these assessments are based on the results of (Dieckmann et al 2008) and you do not provide an assessment of how much CaCO_3 could have been dissolved in the surface layer either. So for me, this part is useless for the understanding of the

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manuscript. P666, line 24. Finally, what is your overall feeling? Is CaCO₃ precipitation a significant process, or not? While reading your manuscript, I have just the feeling that this process can occur locally (only one station) and is not significant. You could address the significance of this process on pCO₂ more clearly.

P667, line 4. Why don't you try to assess the influence of sea ice melting? If you know DIC and TA of the ice, you could derive the effect of sea ice melting using the shift of salinity as a tracer of sea ice melting.

P668, line 5. Why did you use parameterization from Wanninkhof [1992], while other gas transfer velocity coefficients were developed more specifically for the Southern Ocean (see [Edson et al., 2011; Ho et al., 2006] for example) and could be of interest for this study? You have made the assessment of a linear relationship between sea ice concentration and the overall fluxes. So you suppose that when the ice is present there are no fluxes, and when sea ice is not present, parameterisation developed for open water can be used readily. But parameterization of gas transfer velocity depends on turbulence that it is not the same in open ocean, and sea ice covered areas. Indeed fetch and turbulence that are also affected by sea ice. Thus in my opinion transfer velocity coefficients for open ocean cannot be used readily water for sea ice covered area by just "switching off" the fluxes in the presence of ice. In addition, work of Loose et al has shown that they are specific turbulence generated in sea ice covered areas that you do not take into account.

P668, Line 7. You should precise in the text that wind speed is derived from NCEP re-analysis.

P668, eq 10. So far, I understand that the primary production contribution is derived from the change in DIC, but how do you take into account the contribution of air-sea exchanges to changes in DIC in the mixed layer? It is not clear to me. Furthermore, by using nDIC to correct DIC for salinity change, you assume that any meltwater does not contain DIC, while it is not the case. Such formulation is convenient to compensate

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any input of rain/snow of low DIC content. How is it valid for sea ice?

P682, Fig. 2. What the grey parts corresponds to? I guess that it should help to improve the readability of the figures, but in my opinion, a figure should be as simple as possible with no "extra elements".

P689, Fig. 9. I would replace air-ice-sea by air-ice/sea to underline that it is correspond to bot-air-ice and air-sea exchange. air-ice-sea is a bit confusing, because it might corresponds also to ice-sea fluxes, or to air-sea exchange through the ice.

References

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