**Climate
of the Past**

Discussions

EGU

Interactive comment on "Evaluating seasonal sea-ice cover over the Southern Ocean from the Last Glacial Maximum" *by* Ryan A. Green et al.

Anonymous Referee #1

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Green et al presents a new valuable compilation of summer and winter sea ice and SST data for the Southern ocean during the LGM. The paper is, for the most part, quite well written and presented. I very much like that both sea ice and SST data are compiled and then used to evaluate model simulations. The manuscript is however, in its current form, sometimes rather weak on explaining its aims. I do nevertheless find it is suitable in principle for Climate of the Past, after some revision.

I have three main comments on this paper.

(1) There is no suitably clear explanation of what is new, or what is being evaluated, or indeed why, in the introduction or abstract. Clearer sign-posting about the paper containing a new compilation of sea ice and SST data for the LGM would also be helpful in the abstract. I found it a difficult to tell whether the new marine data were

being evaluated for internal consistency. Or whether CMIP5-PMIP3models were being evaluated for their ability to simulate Summer and Winter across the Southern Ocean sea ice in the LGM. Or perhaps whether the ability of models to simulate a large (or small) seasonal change in the area or extent of sea ice in the Southern Ocean was being tested. Its also unclear what the motivation is for evaluating models.

(2) Closely linked to (1), the choice of models being evaluated is difficult to defend. Evaluating CMIP5-PMIP3 versus CMIP6-PMIP4 – and whether models have improved in their ability to simulation SO sea ice would seem valuable and more easy to understand as motivation. However the paper in its current form does not do this. Instead, it shows results from the older PMIP3 simulations, alongside some more recent LoveClim simulations. At least one of the LoveClim simulations also has a very strange seasonal cycle of sea ice. The motivation for doing this approach, as opposed to PMIP3 versus PMIP4 is never explained. I would encourage the authors to consider focussing on PMIP4 versus PMIP3/2. More than 10 PMIP4 LGM simulations are available to the authors.

(3) The section on wind stress results is not particularly helpful. It needs more on how glacial-interglacial wind changes depend on model biases, and the sea ice itself. It can help to start with considered wind velocities above the surface, before then considering wind stress/curl changes. Otherwise results can tend to confuse.

Minor comments

L90-~95. Be specific about what 'common' (e.g. 1 in 2 years?) and 'episodic' (e.g. 1 in 10 years, 1 in 50 years?) mean.

Table 2 – It is most strange that LOVECLIM1 has a SIE minimum in Jan-Feb. Suggest excluding this model from the analysis. Hard to see how the results can be meaningful.

L110 "The proxy"

Figure 1 – add latitude grid lines.

L160 onward. Given the freezing point of seawater is ~-1.8C, I find the lack of <0C datapoints a little strange. Would be useful to have some discussion of the relationship between the freezing point (~-1.8C) and the model and obs datapoints here.

Figure 2. Consider adding a -1.8C or -2C line on panel (c).

Figure 2. Obs data is too faint on panel (c). Make this more visible please.

Table 3 – order of columns seems strange. Maybe should be Winter: SI edge, SIE, SIA, Obs agreement, SST, SST agreement; then Summer: SI edge, SIE, SIA, Obs agreement, SST, SST agreement.

Figure 3 – Cannot see Southern Ocean temperature variation with current colorbar. Change scale to -2 to +8? Figure not useful with this colorscale.

Line 229-253 There are three main problems with this section. Firstly it currently lacks adequate discussion of the fact that SHWs are too poorly simulated by most PMIP3 models to be able to straightforwardly interpret wind results. Secondly, the discussion needs to take account of the control that sea ice exerts on SH winds. Thirdly, by focussing on stress and curl, rather than simpler above surface speed/velocity measures (which are less affected by surface roughness) it leads to a discussion that probably confuses the reader more than clarifying anything.

Suggest re-reading relevant SO/sea ice wind papers by Bracegirdle et al (2013, 2018 and others), Sime et al (2013, 2016), and Kidston et al (2011, The influence of Southern Hemisphere sea-ice extent on the latitude of the mid-latitude jet stream, Geophys. Res. Lett., 38, L15804, doi:10.1029/2011GL048056).

Rewrite or remove this section?