The Cryosphere
Discussions

Interactive comment on "How does internal variability influence the ability of CMIP5 models to reproduce the recent trend in Southern Ocean sea ice extent?" *by* V. Zunz et al.

Anonymous Referee #2

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General Comments:

The authors do an excellent job examining the sea ice capabilities of 24 CMIP5 GCMs, mainly focusing on the ability of the models to simulate the observed Antarctic-wide summer and winter trends. While I like the paper I have two fundamental suggestions that may help the presentation:

1. I found this paper laborious to read. From my experience that usually suggests an organizational issue, though the paper does seem to be organized in a logical manner. I believe some of this difficulty arises from: (1) too many times I had to search back earlier in the paper to better understand what I was currently reading. For example,

when dealing with the historical and then hindcast simulations, it was clear I had already forgotten what the difference between these two were (though I obviously did not memorize it when explained earlier in the text). For that particular case, a brief reminder of the differences when introducing the results would have been useful (just saying "initialized" did not do it for me, a simple sentence or two, such as those appearing on pages 4 and 5 would have been ideal reminders). (2) The prose is sometimes laced with lengthy technical detail which I think would have fared better in tables. For example, take much of the discussion of atmospheric ozone from page 6 and move into Table 2 with other ozone information — as it is, it is a bit of distraction when additional details of ozone appear here and there throughout the text.

2. Regarding figures 2-4; I found that while I could differentiate the various colors of the models in the graphs, I could not successfully match those colors to the colors of the models they represent in the legend. This was because there were 7 subtly different shades of green and of blue and it was difficult to pick which was which in the very short line segments next to the acronyms (Figure 2) and the colored letters of the acronyms (Figures 3 and 4; though 4 was easier since there were not as many subtle shades given fewer models in the graphs). Perhaps the authors could use more dramatically different colors (or limit the shades to just a couple per color — e.g., light green and dark green), or adding different shaped symbols to the lines (cross, squares, etc.). Not sure best fix, but even blown up full screen it was difficult.

I think addressing the above would make reading and comprehending the paper much easier.

Specific comments:

P4L3-7: The Liu and Curry mechanism mentioned is relevant to perennial ice, which represents a trivial contribution to the sea ice extent or trends in the Antarctic, where seasonal ice is the issue. On seasonal ice, more snow leads to seawater flooding which leads to thicker ice, but that extra thickness is still so thin that its impact on the

length of survival during melt is in the noise level.

P4L7-9: Could you say what low frequency internal variability you are talking about (or give a reference)?

P5L17: Should the word "rapid" be "initial"?

P5L18-21: I understand the practical reason for looking at the whole Southern Ocean, but given the regional trends, it does make me worry that some models may be getting the right answer for the wrong reason (e.g., uniformly increasing tend everywhere). Did any models get increasing trend in the west, and decreasing in the east?

Figure 1 was very encouraging! Figures 2-4: results are quite interesting, but as mentioned above, I sure wish I could more clearly figure out which models were performing best with more unique color coding.

Section 3 and higher: The analyses, results and interpretations are excellent, I would like to be able to quickly access this information for future use, but for that, a table summarizing the performance results for each model would be very helpful.

P12L10-15: Regarding ozone, a simple hypothesis is that the lack of ozone keeps the Antarctic continent cold while the rest of the planet warms, thus intensifying the meridional temperature gradient, driving stronger westerlies, leading to the Antarctic Circumpolar Current migrating closer to the continent. Since the ACC limits the equatorward extent of polar waters capable of growing sea ice, wouldn't we expect to see a decreasing ice extent with better ozone simulation? Or is this one of those easy to recite, but overly naïve scenarios? In the text (P12L10-15) it is noted that models with interactive chemistry and those with higher vertical resolution underestimate ice extent, but we are not told if those models better simulate the ozone? Perhaps they would have overestimated the ice if not for good ozone.

Section 4.1, P13-15: There is often a desire to present all of the impressively detailed analyses that lead to a consistent interpretation. Seems that much of the discussion

here is leading to the rather reasonable solution that if a hindcast simulation is initialized to produce less sea ice than the model's climatological solution, the trend will naturally be positive, or vice-versa. I don't think we need the analysis details of various sets of models leading to this general result. More words, more laborious reading.

Technical comments:

There are a number of minor English mistakes that I leave to the journal editor.

P16: I am concerned about the validity of correlation significance here. How was the autocovariance included to determine the effective degrees of freedom taken into account in the t-test. And, with so few points in the test, even allowing for this with the small sample size t-test, 1 or 2 points passing a 5% significance test is a stretch. One can actually compute the significance to the significance, but I think that would be taking this too far. But, if the effective DOF was handled properly, I do think the results were presented responsibly.

Despite my numerous though minor criticisms, I do think this paper will make an excellent contribution to the literature. I do hope the authors try to address my 2 general comments.

Interactive comment on The Cryosphere Discuss., 6, 3539, 2012.

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Discussion Paper