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The Cryosphere Discussions

Interactive comment on "Influence of Sea Ice Anomalies on Antarctic Precipitation Using Source Attribution" *by* Hailong Wang et al.

Harald Sodemann (Referee)

harald.sodemann@uib.no

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Review of "Influence of Sea Ice Anomalies on Antarctic Precipitation Using Source Attribution" by Wang et al., submitted to The Cryosphere.

The authors present a sensitivity study of the global water cycle, testing the response of precipitation origin over Antarctica to combined sea ice cover and sea surface temperature changes, using a climate model capable of water vapour tagging. The results are interesting and novel, and fit well into the scope of TC. However, some aspects need further clarification, concerning the more general implications of the findings and the relation to previous results. In addition, the presentation quality of some figures can be improved, as detailed below.

Major comments

1. The findings should be placed more clearly into the wider scientific context to make their significance more obvious. This concerns the abstract, introduction and conclusions.
2. The relation to previous studies should be more clearly specified. The present study is based on a long control run, whereas previous studies mostly used reanalysis data. What are the differences? How valid is the control run for the present-day simulation? And, more specifically, how do the uppermost and lowermost percentiles used here compare to observed natural variability?
3. The tagging setup should to be modified, or justified more clearly, and be documented more comprehensively. The Antarctic land mass is currently part of a general land tracer in the tagging setup. It would be interesting to specify an Antarctic-land only tracer, to allow for distinguishing local from remote contributions. In addition, the Southern Ocean tag appears specified in a confusing way, with a narrow stripe going around the globe, and some boxes for the Weddell Sea and others placed inside. It should be stated more clearly why this specific setup has been chosen. Furthermore, a table or other form of description of the exact box coordinates are needed in order to compare and reproduce results.
4. The findings in some figures should be condensed further to facilitate grasping the main findings, as detailed in the specific comments

Specific comments

Abstract

Pg. 2, L. 1: highlight the relevance to other research, now it is just described as a sensitivity study in the first sentence.

Pg. 2, L. 6: "but": consider splitting into two sentences

Pg. 2, L. 11: A percent number could be more informative here

Introduction

Pg. 3, L. 1: "SMB ... plays a role... by supplying" does not make sense

Pg. 3, L. 10-13: Not sure the exact mechanism is assessed quantitatively in this study. May need rephrasing.

Pg. 3, L. 17: "Because of..." ultimate meaning/logic of this sentence not clear. "Relies" for what purpose? Maybe it helps to rephrase in terms of *mean* temperature?

Pg. 3, L. 20: The relevance of knowing the moisture source could be highlighted here.

Pg. 3, L. 24 onward: Some link to the state of the Antarctic hydrologic system from observation or reanalysis data would be useful here (e.g., Papritz et al., 2014).

Pg. 4, L. 1: Clarify the distinction between internal and natural climate variability

Pg. 4, L. 2: "such responses": unclear what exactly is referred to

Pg. 4, L. 10: "and/or" - rephrase

Pg. 4, L. 17: "unacceptably" - I think this depends on the approach and purpose. One frequently used countermeasure is to consider the problem stochastically, by calculating many trajectories, as is done in Sodemann and Stohl (2009). Interpreting single trajectories beyond 10 days in contrast may be meaningless. On the other hand, tracer studies suffer from numerical diffusion and the uncertainty of parameterisation processes, and do not provide the spatial detail of source contributions available from backward trajectories. A more balanced discussion would be justified here.

Pg. 4, L. 20 onward: The relevance for ice core studies could be included as an additional motivation, see for example Winkler et al., 2012, Wang et al., 2013, Masson-Delmotte et al., 2011, Buizert et al., 2018 and references therein.

Methods

Pg. 4, L. 22: "such studies" - which ones specifically?

Pg. 5, L. 3: Are all 5 references needed as reference to the method, or rather example applications? Please clarify.

Pg. 6, L. 3: "assuming" - has it been checked that the simulation stabilises after 1 year?

Pg. 6, L. 13: It would be helpful to more convincingly illustrate and quantify this aspect.

Pg. 6, L. 23: tags -> tag

Pg. 6, L. 25: remove "well"

Pg. 6, L. 26: "differencing": rephrase, e.g. distinguishing?

Pg. 6, L. 15 onward: SST mean and anomalies should be shown/discussed and placed in relation to observations/reanalysis.

Pg. 6, L. 15: see major comment 3.

Results

Pg. 7, L. 11: "echoing the finding" - what aspect specifically?

Pg. 7, L. 20: This number could be useful to include in the Abstract

Pg. 7, L. 22 onward: would be helpful to put these numbers into the context of observation/reanalysis data

Pg. 7, L. 27: what is meant by "more significant"?

Pg. 8, L. 1: It would be interesting to know what land region contributes, in particular anything from Antarctica.

Pg. 8, L, 2 onward: it would be helpful to collect these results in a table.

Pg. 8, L. 8: The discussion could be more comprehensive. My take is that the overall results are quite similar, and specific numbers depend on how the ocean sectors have been defined here and in Sodemann and Stohl (2009). That itself is a useful finding that should be stated clearly. Part of the differences may then be due to the fact that Sodemann and Stohl (2009) based their results on ECMWF analyses for a specific period, while you consider a control run of a climate model. Comparison to reanalysis data may therefore be helpful to better explain the differences found here. Furthermore, a table or other form of description of the exact box coordinates are needed in order to compare (and reproduce) results.

Pg. 8, L. 30: Fig S4 seems to contain useful results but the information needs to be condensed more (see below).

Pg. 9, L. 9: See, for example, Stohl and Sodemann (2010), which also clearly illustrates the thermodynamic transport barrier to low-level airmasses from the Southern Ocean (their Fig. 3).

Pg. 9, L. 9: "at the source" - or underways!

Pg. 9, L. 17: The presentation of the results can be improved, see comments on figures below.

Pg. 9, L. 25 onward: The discussion here is quite vague, and could be made more specific

Pg. 10, L. 4 onward: What is the region for the apparently substantial changes in the NH? Maybe better to only show the SH.

Pg. 10, L. 30: It appears you imply a direct and indirect impact from the SIC and SST anomalies - if this is a main result, this should be introduced more prominently already in the Introduction. Is it really possible to separate both aspects, as dynamic changes can also affect evaporation of the source regions?

Pg. 11, L. 15: The takeaway from this paragraph is not fully clear.

Summary

Pg. 12, L. 15: It could be helpful to state whether or not this confirms earlier findings (to my understanding it does)

Pg. 12, L. 34: "intuitively" - may not be applicable to all readers

Pg. 13, L. 1-5: Would be useful to highlight the wider implications of this study in the end.

Figures

Fig. 1: In Antarctica, seasons are more commonly defined as JFM and ASO, in line with the sea ice seasonaliry. Maybe it would probably be sufficient to show these seasons only along with the annual mean.

Fig. 2: The E panel does not appear to be relevant here, but could be part of a "validation" figure where you compare SH plots of annual mean P and E from the model simulation with reanalysis/observation data.

Fig. 4: This figure could be made more easily readable by either showing bars for 3-month periods, or by removing the white space in between the individual monthly bars. Similarly, Fig. S4 could be made into a much simpler figure that only compares the annual mean or summer/winter differences for the 3 regions.

Fig. 5: Panel a is method validation and could be removed in this context, the big red spot draws a lot of attention, and prevents a more useful color scale to be used. For the purpose of the paper, it seems it would be more useful to highlight the contributions to precipitation in Antarctica only, by masking the tracer contribution over the Oceanic regions, and zooming in on Antarctica.

Fig. 6: Instead of a zonal mean covering all latitudes, it would be more useful to highlight the Southern Hemisphere only. In fact, Fig. 6 may be dropped altogether, and be replaced by Fig. S5.

Fig. 7: Similarly, the figure would be improved by showing the SH part only.

Fig. 8: May be dropped in favour of Fig. 9

Fig. 9: Show SH section only (or discuss the NH changes which are in general larger or as large as the NH changes)

Fig. 10: Busy figure with 15 panels and the SLP difference contours. Consider removing panel c and d, and e, or keeping e and removing the contours from the other panels.

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