The Cryosphere Discuss.,
doi:10.5194/tc-2015-209-AC2, 2016

The Cryosphere
Discussions

Interactive comment on "Trends in sea-ice variability on the way to an ice-free Arctic" *by* S. Bathiany et al.

S. Bathiany et al.

sebastian.bathiany@wur.nl

Received and published: 4 April 2016

We thank the referee for the constructive comments. The first comment raises the concern that the study by Wagner and Eisenman (2015a) could affect the relevance of our study. Wagner and Eisenman show that tipping points can occur as a model artefact in simple models (EBMs and SCMs) because the seasonal cycle and spatial differences are not resolved properly. We now cite this important paper in the introduction of our revised article. However, our article does not make any assumptions on the existence of a tipping point, but focuses on the relation between the mean state and the variability of sea ice, before sea ice is lost completely. By doing so, we assess if statistical stability indicators can predict a potential tipping point. Such an analysis is useful because observations might then provide an additional source of information about sea ice stability, besides the predictions of climate models that are always uncertain so some

extent. Moreover, multiple steady states have also been found in complex models. The latter results are not directly relevant for the loss of sea ice in the coming centuries, but they are potentially important to understand past climate change. We understand that we should have made these arguments more specific and have revised the manuscript accordingly. In particular, we point out in the introduction:

"Wagner and Eisenman (2015a) recently showed in detail how resolving the seasonal cycle and latitudinal differences can eliminate bifurcations in sea-ice models. Nonetheless, bifurcations also occur in comprehensive climate models: In a complex general-circulation model with current continental distribution and solar insolation, Marotzke and Botzet (2007) identified a globally ice-covered stable state analogous of the 'Snowball Earth' conditions in the Neoproterozoic (Pierrehumbert et al., 2011). Ferreira et al. (2011) and Rose et al. (2013) even found three stable states in a complex model with idealised ocean geometry. Such alternative stable states imply the possibility of large-scale abrupt climate changes when external conditions are varied. Moreover, Ferreira et al. (2011) and Rose et al. (2013) show that the existence of multiple stable sea-ice states depends on the structure of the ocean circulation, a nonlinear system that can even show tipping point behaviour on its own. Such nonlinear interactions are not captured by the model of Wagner and Eisenman (2015a) because heat transport is formulated as a simple diffusion term in their model which has only one spatial dimension. Given these model uncertainties, it is worthwhile to investigate the changes in variability that are associated with sea-ice loss, mainly for two practical reasons. First, if these changes depend on the abruptness of future sea-ice loss, observations might provide an alternative source of information and indicate which model is most reliable in its prediction. Second, one might draw conclusions about the climate variability and the rates of change in the Earth's deep past, something that is difficult to reconstruct directly (White et al., 2010; Kemp et al., 2015), and that can help to build simple stochastic climate models.."

As we already pointed out in our previous reply, we do not make any (false) claim about when Arctic winter sea ice would be lost in the models.

Specific comments:

1. As noted above, we now cite the paper by Wagner and Eisenman (2015a), and we explain why our study is not in conflict with their results.

2. We do provide physical insights, in particular in Sect. 3.1 where we demonstrate the physical reason for the decrease in time scale during summer ice melt (growth-thickness feedback), and the increase in time scale during winter ice melt (mixed-layer effect). Although the existence of these effects is already known, it has not been tested before if they would also dominate sea-ice variability in comprehensive models. Our study investigates this question for the first time. As the link between mean state and variability proves robust in the models, we think that the title is not misleading. It is true that we focus on ice volume in the paper because several papers have been published about the variance of ice area, and because the autocorrelation of ice area shows no clear trends (as we mention in the paper). We have decided to not make the title too technical and mention these details in the abstract and the rest of the paper.

3. We now show the time series of sea-ice volume in Fig. 8. These figures and a revised methods section make clearer that we do indeed also analyse the extended RCP8.5 scenario. We now also refer to Hezel et al. (2014) in this section.

4./5. Our revised manuscript points out more clearly that we do analyse several Earth system models, though MPI-ESM is indeed analysed in most detail. The fact that the model by Eisenman (2007) can explain the behaviour of MPI-ESM is confirmed by a previous study (Bathiany et al., 2016) which we now cite. The reviewer has also raised concerns about the realism of our results given the abrupt ice loss in MPI-ESM compared to other CMIP5 models. In our revised manuscript we explain more clearly that our analysis concerns the changes in sea-ice variability that occur before the final loss of winter sea ice, and that these changes do not depend on how abrupt this final ice loss is. In particular, we have added a paragraph in Sect. 4 (Conclusions) to explain

why the MPI-ESM is not an outlier in terms of its representation of sea-ice variability:

"The comprehensive model we analysed in most detail, MPI-ESM, likely exaggerates how rapidly the final bit of winter sea-ice volume disappears (e.g. as seen in the top right panel of Fig. 8). This abrupt volume loss is probably related to the ice-growth parameterisation, which attributes a single thickness to all newly formed ice in a grid cell (Bathiany et al., 2016). Although the abrupt event itself is not part of our time series analysis above, it points to potential limitations of the applied model and one may ask how models with several ice-thickness classes would behave. It is reassuring in this regard that eight other models agree with MPI-ESM in their decrease of the sea-ice volume's variance, although time series were too short to show clear trends in autocorrelation. Moreover, the mechanistic insight obtained with the simpler models suggests that these model agreements are no coincidence because they can be explained from fundamental physical processes. Both the fast adjustment of thin ice and the slow response of the mixed-layer ocean are represented in all the models and would also not change in even more complex models. For example, in models with many ice-thickness classes, the variability of the total ice volume in a grid cell is the result of the variability of all thickness classes. The trends in variance and autocorrelation would have the same sign for each thickness class because the thickness-growth relationship is monotonous (Thorndike et al., 1975). Even the precise realisation of the weather-induced variability would be identical because all thickness classes within a grid cell are coupled to the same ocean and atmosphere grid cell. Hence, the level of sophistication in the representation of the subgrid-scale ice-thickness distribution is not relevant for our results. Furthermore, it has been shown in Bathiany et al. (2016) that radiative feedbacks and mechanical redistribution mechanisms are unimportant for the abruptness of sea-ice loss in MPI-ESM, which is instead determined by thermodynamic processes. It is therefore plausible that the same processes also determine the variability of sea ice before the final ice loss occurs."

Following the reviewer's suggestion, we now also define abrupt change in the introduction: “Such a change is loosely referred to as ‘abrupt’ if the acceleration is due to mechanisms internal to the climate system (such as the positive ice-albedo feedback) whereas the forcing changes linearly over time (Rahmstorf, 2001; National Research Council, 2002).” We do not use the word rapid anymore in this context.

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