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

Anonymous Referee #1

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The paper shows a lack of familiarly with sea ice and the relevant literature. While a contribution from outside the field of sea ice modeling is generally very welcome and can lead to insights previously missed due to the application of different methods, this paper unfortunately does not provide any such insights. It stays limited to the application of an interesting method (statistical stability indicators theory), but fails to draw any useful conclusions from the analysis. The fact that a recent paper of Wegner and Eisenman (2015) explains why simple models such as EBMs and SCMs (as used in this study) tend to show instabilities and tipping points in sea ice, but complex earth system models generally do not, makes this analysis even less useful for understanding Arctic sea ice evolution in the real world (or in climate models). The paper is also build on the wrong claim that several CMIP5 models loose winter sea ice by the end of the 21st century, which is not correct. I very much regret to recommend a rejection of the

paper, as the authors clearly invested a lot of work into this contribution and wrote a well structured paper. But the factual errors, lack of awareness of the relevant literature, and the lack of any relevant conclusions does not allow me to recommend publication of this paper in The Cryosphere.

Specific comments:

1. The study by Wagner and Eisenman (2015) showed that "It is found that the stability of the ice cover vastly increases with the inclusion of spatial communication via meridional heat transport or a seasonal cycle in solar forcing, being most stable when both are included." And "the present model simulates sea ice loss that is not only reversible but also has a strikingly linear relationship with the climate forcing as well as with the global-mean temperature. This is in contrast with SCMs and EBMs, and it is consistent with GCMs. The results presented here indicate that the nonlinearities in the model are essentially smoothed out when latitudinal and seasonal variations are included. " This important study was not cited, despite the fact that it was published over a year ago (in Feb 2015). As the authors are using SCMs and EBMs to study instabilities in the sea ice system, and Wagner and Eisenman showed that these models overestimate instabilities due to their lack of a spatial dimension, this paper removes the basis of the work presented here.

2. Even without this recent paper, the title is misleading, as no physical insights into "Trends in sea-ice variability on the way to an ice-free Arctic" are shown. If anything, the title should be "Relaxation time and autocorrelation in the Arctic sea ice cover on the way to an ice-free Arctic" or "Statistical stability indicators theory applied to Arctic sea ice", as the study focused only on the application of the method, without providing physical insights into the actual system of sea ice decline.

3. Page 4, line 18-22: "The models are all the available models that lose their Arctic winter sea ice in RCP8.5". This is wrong. I don't know of any CMIP5 models that lose their Arctic winter sea ice in RCP8.5 by 2100. Some of them do by the end of the 24th

century in the extended concentration pathway scenarios (see Hezel et al. 2014), but not by the end of the 21st century. So if the authors wanted to study the winter sea ice going away in GCM simulations, the extended concentration pathway simulations (shown in Hezel et al. 2014) would need to be used. Also, why is the sea ice volume time series not shown in Figure 8? It would show that these models do not lose winter sea ice under RCP8.5 by the end of the 21st century simulations, so the authors should have plotted it to avoid this mistake.

4. Page 9, line 35: “the inclusion of spatial differences and processes like advection and mechanical redistribution of sea ice apparently has not changed the behavior of sea ice variability. We therefore argue that E07 is an appropriate model to explain the behavior in MPI-ESM and it is probable that the same processes are behind the evolution of the statistics.”

5. This statement is in direct conflict with Wagner and Eisenman (2015), and therefore needs further investigation. Maybe the MPI model is an outlier in the CGM’s that participated in the CMIP5, due to its very simple sea ice model (compared to the other GCMs in CMIP5)? The authors would need to present results from more than one CGM in order to be able to make the results robust. There are many other models that have run 4xCO2 experiments for CMIP5; the authors would need to analyze these to show that the MPI model is not an outlier in that it shows a abrupt transition in winter sea ice, as Henzel et al 2014 does not show any CMIP5 models showing abrupt transitions to winter ice free in the extended RCP8.5 simulations. Furthermore, “abrupt” is not defined anywhere in the paper. The “rapid” ice loss shown in the MPI model occurs at a relatively small Arctic ice volume, so it does not constitute a large change or big transition in any case.

References: Till J. W. Wagner and Ian Eisenman, 2015: How Climate Model Complexity Influences Sea Ice Stability. J. Climate, 28, 3998–4014. doi: http://dx.doi.org/10.1175/JCLI-D-14-00654.1

Hezel, P. J., Fichefet, T., and Massonnet, F.: Modeled Arctic sea ice evolution through 2300 in CMIP5 extended RCPs, The Cryosphere, 8, 1195-1204, doi:10.5194/tc-8-1195-2014, 2014.

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