Reviewer #1, comment 1:

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.

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.


Bathiany, S., Notz, D., Mauritsen, T., Brovkin, V., and Raedel, G.: On the potential for abrupt Arctic winter sea-ice loss, J. Clim., 2016.

Eisenman, I.: Arctic catastrophes in an idealized sea-ice model, in: 2006 Program of studies: Ice (geophysical fluid dynamics program), 133-161, Woods Hole Oceanographic Institution, Woods Hole, Mass., 2007.

Ferreira, D., Marshall, J., and Rose, B.: Climate determinism revisited: multiple equilibria in a complex climate model. J. Climate, 24, 992-1012, 2011.

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

Kemp, D. B., Eichenseer, K., and Kiessling, W.: Maximum rates of climate change are systematically underestimated in the geological record. Nat. Commun., 6, 8890, 2015.

Marotzke, J., and Botzet, M.: Present-day and ice-covered equilibrium states in a comprehensive climate model. Geophys. Res. Lett., 34, L16704, 2007.

National Research Council: Abrupt Climate Change: Inevitable Surprises, Natl. Acad. Press, Washington, DC, 2002.

Pierrehumbert, R. T., Abbot, D. S., Voigt, A., and Koll, D.: Climate of the Neoproterozoic, Annu. Rev. Earth Planet. Sci., 39, 417–460, 2011.

Rahmstorf, S.: Abrupt Climate Change. Encyclopedia of Ocean Sciences, eds Steele J, Thorpe S, Turekian K (Academic, London), pp 1–6, 2001.

Rose, B., Ferreira, D., and Marshall, J.: The role of oceans and sea ice in abrupt transitions between multiple climate states. J. Climate, 26, 2862-2879, 2013.

Thorndike, A. S., Rothrock, D. A., Maykut, G. A., and Colony, R.: The thickness distribution of sea ice. J. Geophys. Res., 80, 4501-4513, 1975.

Wagner, T. J. W., and Eisenman, I.: How climate model complexity influences sea ice stability. J. Climate, 28, 3998-4014, 2015a.

White, J. W. C., and Coathors: Past rates of climate change in the Arctic. Quat. Sci. Rev., 29, 1716-1727, 2010.

Reviewer #1, comment 2:

“In reply to point 1, Hezel et al. (2014) find that “in all but two models, however, sea ice volume demonstrates a continuing linear or slower rather than faster rate of decline through the disappearance of winter ice, and thus we conclude that apparent threashold behavior is not occurring in this set of models as the winter sea ice disappears”.
With the MPI-ESM-LR model being the model that shows the most notable non-linear decline in sea ice towards an ice-free state. Which is opposite to the claim of the authors that “MPI-ESM is no outlier in terms of the underlying mechanism, and we will clarify this point in the revised version”, and supports my concern that the MPI model is not the right model to use for this study, as it behances differently than other CMIP5 GCMs.

In regards to point 2, the authors description of the CMIP5 model simulations they used did not at all reflect that they used the extended concentration pathway simulations (but I can see now that the lines in Fig 8 extend past 2100). The use of the term RCP8.5 (which describes simulations from 2005-2100), and the reference that “reaching a radiative forcing of approximately 8.5 Wm2 in the year 2100” directly before the statement that these models all loose their winter sea ice in RCP8.5 is very misleading, and also shows a lack of familiarity with the CMIP5 models/scenarios (also shown in the absence of any references for these scenarios/simulations, which could have clarified the text for the informed reader). Hezel et al. (2014) used RCP8.5 to refer to the continous simulations (2005-2300), but clearly explained what they were doing and cited the relevant literature, which was both not done here and needs to be improved upon greatly if the editor decides to request a revised submission. The relevant papers for the extended concentration pathway experiments the authors used are Moss et al. (2010) and Meinshausen et al. (2011).”

We thank the referee again for these constructive comments.
Regarding the concern about the realism of MPI-ESM, see our reply above.

Concerning the simulations we analyse, we now explain them and the selection of these simulations more explicitly. In addition, we have obtained model output from one additional comprehensive model (bcc-csm1-1) that we now also analyse.

In the introduction we now write:

*"We also analyse eight additional comprehensive models from the Coupled Model Intercomparison Project 5 (CMIP5), using simulations of the historical period, the RCP8.5 scenario and its extension until the year 2300. The models are all the available models that lose their Arctic winter sea ice in these simulations. The level of complexity in these models is comparable to MPI-ESM, but some of them explicitly resolve several ice-thickness classes on the subgrid scale. Although one of the models (CSIRO-Mk3-6-0) also produces an abrupt loss of winter sea-ice area, most models show a retreat of winter sea ice that is gradual (Hezel et al., 2014), though faster than the preceding summer sea-ice loss (Bathiany et al., 2016)."*

In addition, we elaborate on this in Sect. 3.3:

*"To test this prediction, we finally analyse CMIP5 simulations from MPI-ESM and eight other comprehensive climate models. For this analysis we combine the historical simulation, the RCP8.5 simulation, and the extended RCP8.5 simulation that ends in the year 2300. In this scenario, atmospheric CO2 shows an accelerated increase until the year 2100, when a radiative forcing of approx. 8.5 W/m2 is reached. Thereafter, the CO2 concentration stabilises at almost 2000 ppm (Meinshausen et al., 2011), yielding the largest warming of all CMIP5 simulations. The extended simulations until 2300 were performed with nine models (Hezel et al., 2014). Here we analyse all models where Arctic sea-ice area falls below one million square kilometres in the full RCP8.5 scenario, no matter when this event occurs. Two of the models analysed in Hezel et al. (2014) do not lose their winter sea ice by 2300, while two other models not analysed by Hezel et al. (2014) have lost their winter sea ice already by 2100 (the nine models we analyse are therefore not identical to the nine models in Hezel et al., 2014)."*

While this includes a reference to Meinshausen et al (2011) as suggested by the reviewer, we do not cite the paper by Moss et al. (2010) because it does not discuss the extended RCP8.5 simulation.


Bathiany, S., Notz, D., Mauritsen, T., Brovkin, V., and Raedel, G.: On the potential for abrupt Arctic winter sea-ice loss, J. Clim., 2016.

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

Meinshausen, M., and Coauthors: The RCP greenhouse gas concentrations and their extensions from 1765 to 2300. Climatic Change, 109, 213–241, 2011.

Moss, R. H., and Coauthors: The next generation of scenarios for climate change research and assessment. Nature, 463, 747-756, 2010.


Reviewer #2:

In general, I find the analysis presented convincing and technically sound, but I share the concerns that have also been expressed by the other reviewer. Specifically:

1) It is hard to judge the relevancy of this work for the actual world. Wagner and Eisenman

shows that if you include meridional heat transport (a mechanism not included in the box models considered in the current study) the non-linearity from albedo changes is effectively removed and no tipping point is found to occur. Hence, the box models considered here are likely too simple to be relevant to the real world.

2) It's also hard to judge the novelty of results presented in the current study. Which aspects of the results are novel, and which are simply confirmations of results already published in previous literature (such as the two Wagner and Eisenman papers)? On page 3, line 14 the authors state that 'it has not been investigate how these factors affect the prospects for early warning signals, especially in more complex, spatially explicit models...'. In the previous sentence, the authors state that Wagner and Eisenman have investigated this issue...

3) P. 6, line 29. Even though Wagner and Eisenman also find the lack of a bifurcation point in their model, this seems to be the case for a fundamentally different reason. In their case, they increased the complexity of their model (by including meridional heat transport), whereas here you decreased it.

4) The implications for other systems are unclear to me. The presented results seem to be very specific to sea ice area and the specific feedback processes relevant for sea ice.

5) P.9: For easier interpretation it would be helpful if you could quote the CO2 quadrupling time time in extended RCP 8.5 simulations.

We thank the referee for these constructive comments which helped us to improve the manuscript.

1. We see two major aspects in our study that are relevant for reality. First, the robust link between mean state and variability of sea ice is useful to know in order to infer the variability of sea ice in future and past climates. For example, our results would allow to formulate a simple stochastic parameterisation of sea-ice variability. Second, we assess the performance of statistical stability indicators that are sometimes applied to observations and reconstructions. It is often argued that the method could provide information on climate stability, independently of any complex model. However, the success of the theory is usually only demonstrated in very simple stochastic models. In more complex systems, there can be many counteracting effects, and it is not self-evident if a simple one-dimensional theory holds in a complex world. Therefore, it is necessary to investigate if the approach can yield meaningful results in the case of Arctic sea ice, and how the results depend on the model formulation and complexity.

We agree with recent studies that Arctic sea ice is probably not approaching a tipping point. However, given the model uncertainties such projections are never completely certain. Our study shows that if sea ice was approaching a tipping point, observations of sea-ice variability would not help to detect it. Hence, we indeed do have to trust the models, but we think that it is useful to know this.

We have revised the introduction and conclusions sections of our study to point out these aspects more clearly.

2. Our study is novel in mainly two aspects. First, it is more comprehensive than previous studies by analysing and interpreting variability between the states of perennial ice cover and an ice-free ocean. In contrast, Moon and Wettlaufer (2011, 2013) did not analyse variability at all, whereas Wagner and Eisenman (2015b) only focussed on the mixed-layer effect. We show that statistical stability indicators do not work either in other regimes.

Second, previous studies only used simple models, the most complex being the model by Wagner and Eisenman (2015a). This model is based on the single column model by Eisenman and Wettlaufer (2009) which only predicts one state variable (enthalpy). The additional complexity Wagner and Eisenman (2015b) included in the model was to couple many 'single columns' together with a simple heat diffusion term and in only one spatial dimension (latitude). Their model describes an aquaplanet without any continents, and does not resolve an open-water fraction at the subgrid scale, which can have consequences for the heat flux between ocean and atmosphere and thus the adjustment to perturbations. Their model is therefore still much simpler than the general-circulation models used in CMIP5.

In a nutshell, we go beyond previous studies by explicitly demonstrating how sea-ice variability can be explained in the complete range of climate regimes. And, for the first time, we also analyse statistical stability indicators of sea ice in comprehensive climate models. Again, we refer the referee to our revised introduction and conclusions where we point out these aspects more clearly.

3. The part of text the reviewer refers to explains why the relaxation time of sea ice increases while seasonal sea-ice is lost. Our paper in general, and the mentioned paragraph in particular, do not analyse under what conditions bifurcations occur or do not occur. What we show here is that the system approaches the mixed-layer ocean's time scale when $CO_2$ is increased. We do this in the mentioned paragraph by directly changing this time scale in the model. This has nothing to do with the existence of the bifurcation that occurs at the transition to an ice-free ocean, and changing the mixed-layer time scale does not affect this bifurcation. The result is also not in conflict with the model of Wagner and Eisenman (2015a,b), which is another version of the model we discuss in the text (only that it has a spatial dimension), and which shows the same phenomenon. It is a main point of our paper that all models agree on this phenomenon despite their disagreement on the abruptness of the last bit of sea ice.

4. Of course, the physical mechanisms we analyse in the paper are restricted to sea ice. However, the general form of the problem has analogies in other systems. The differential equations that describe these systems can be understood to describe a state variable with a certain inertia (imposing a certain relaxation time scale), and processes that can perturb the system away from equilibrium. The concept of stability and the question how slowly a system responds to perturbations can apply to any physical system that can be modelled as a stochastic dynamical system. To illustrate this, we explicitly mention two examples in Sect. 4, namely vegetation dynamics and sea-surface temperatures. Due to the specific focus of our paper on sea ice we refrained from explaining more details here but added references to other studies instead.

5. We now describe the extended RCP8.5 scenario in more detail (see last comment to reviewer 1).


Eisenman, I., and Wettlaufer, J. S.: Nonlinear threshold behavior during the loss of Arctic sea ice, Proc. Natl. Acad. Sci. U. S. A., 106, 28-32, 2009.

Moon, W., and Wettlaufer, J. S.: A low-order theory of Arctic sea ice stability, E. P. L., 96, 39001, 2011.

Moon, W., and Wettlaufer, J. S.: A stochastic perturbation theory for non-autonomous systems, J. Math. Phys., 54, 123303, 2013.

Wagner, T. J. W., and Eisenman, I.: How climate model complexity influences sea ice stability. J. Climate, 28, 3998-4014, 2015a.

Wagner, T. J. W., and Eisenman, I.: Early warning signals for abrupt change raise false alarms during sea ice loss, Geophys. Res. Lett., 10333-10341, 2015b.

Comment by Till Wagner

In agreement with, and in addition to, the insightful and constructive comments of the reviewers, I would like to provide the following feedback to this interesting study, which I think will make a valuable contribution to the literature:

1) Overall, this paper provides interesting and novel insight into the statistical differences between summer and winter sea ice loss, as well as the evolution of sea ice thickness and volume. It therefore goes beyond previous work, including our paper (Wagner & Eisenman, 2015), where we focused on sea ice area during summer. It further fills in important gaps regarding the effects of different types of stochastic forcing.

2) Title and introduction: Regarding the title, I agree broadly with Reviewer 2 that it may be better if the title referred specifically to the statistical indicators, since they are at the core of this study. Regarding the introduction, I would proffer that the focus could be shifted somewhat toward the evolution of variance and autocorrelation under sea ice loss in general, rather than focusing on their (lack of) utility as early warning signals for critical transitions. Introducing the concept of using variance and autocorrelation to help estimate the future mean state and variability of the sea ice cover is an excellent contribution of this study that could be given more weight here in my opinion.

3) Relatedly, a slightly clearer presentation of what has been published on this topic and what is novel, in line with comment 2 by Reviewer 2, may improve the exposition of the paper.

4) As a side note, I want to point out that the spatially explicit model results from Wagner & Eisenman (2015) show an increase in autocorrelation before the loss of the summer sea ice, something that single-column models like E07 may not always pick up on. We suggest that the increase in autocorrelation is due to the growth of the (long-memory) open-water region as the ice retreats, in agreement with the conclusions drawn here.

5) The analysis and discussion of GCM results appears to me (not a GCM expert) very valuable. It highlights a number of important operational limitations in applying statistical indicators as early warning signals, and it provides the first steps toward the use of statistical indicators in GCMs to predict changes in the sea ice cover. I would hope this motivates fruitful further research in the community.

Reference: T.J.W. Wagner and I. Eisenman (2015) "False alarms: How early warning signals falsely predict abrupt sea ice loss", GRL (23) 42, DOI: 10.1002/2015GL066297

We are grateful to Till Wagner for these constructive comments which help to clarify several points in the discussion and will help to improve the manuscript.

1. and 5. We fully agree on these comments concerning what is novel in our study. We emphasise these points in our revised manuscript.

2. We have chosen this general title because our study has relevance beyond the phenomenon of slowing down and early warning signals. What we analyse is the relation between the mean state of Arctic sea ice (or its annual cycle in equilibrium with a certain forcing) and the fast variability around this state. Our main result is that we find a relation between these properties that is fundamental (arising from physical processes) and robust (independent of the model and the description of its variability). Regarding the idea of early warning signals, this is a negative result. Regarding the prospects for stochastic climate models or the inference of past and future climate variability, it is a positive result. Hence, we like to reflect the genericity of our result in the title. We think that this argumentation is in perfect agreement with the rest of the comment, suggesting to focus more on what can be inferred from observations instead of focussing too much on false alarms.

3. We fully agree that we should inform the reader more clearly about the novelty of our manuscript, something we have considered in the revised version.

4. We agree that the inertia of the open ocean causes the increase in autocorrelation in both models. As stated in Wagner and Eisenman (2015b), the autocorrelation of sea-ice volume decreases before Arctic summer sea-ice loss in their model, in agreement with our findings. We note that this happens in all models, also including MPI-ESM which is spatially explicit. As shown in Wagner and Eisenman (2015), there seems to be a somewhat different timing in the onset of slowing down in other variables, like polar temperature and total hemispheric sea-ice area, which tend to increase already before Arctic summer ice is lost. This can occur due to the spatial coupling of grid cells via the atmosphere: As more and more grid cells become ice free with increasing long-wave forcing, the variability of the whole coupled system slows down, which can also affect latitudes where sea-ice is still present, and which can cause a slowing down of the fluctuations of the sea-ice edge's position. For a strict model comparison regarding this issue of the timing, more analysis would be required. We leave this to future studies because it does not affect our results.

Wagner, T. J. W., and Eisenman, I.: Early warning signals for abrupt change raise false alarms during sea ice loss, Geophys. Res. Lett., 10333-10341, 2015b.