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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.

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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 CO2 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).

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