

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

Anonymous Referee #2

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This paper investigates temporal characteristics of Arctic sea ice area and volume under increased radiative forcing, in two box models and a comprehensive coupled atmosphere ocean sea ice model. Previous literature has investigated the possibility that, due to the positive ice-albedo feedback, sea ice could reach a tipping (or bifurcation point), and whether or not this would be preceded by increased auto-correlation and variance as predicted by theoretical models. Prior to summer sea ice loss, the response time found to decrease, as thinner ice can adjust more quickly to perturbations. Between summer and annual sea ice loss the response time is found to increase, as the system is more constrained by the large heat capacity of the ocean. Switching off individual mechanisms leads in some cases to a loss of a bifurcation point, but otherwise similar characteristics in response time.

In general, I find the analysis presented convincing and technically sound, but I share

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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.

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