

Review of Li et al:

I will keep it short and to the point. Li et al bring in an important aspect into discussion here, i.e. the radiative effects (particularly longwave warming) of falling snow. From the process point of view, I do appreciate that the authors highlight its potential importance and encourage modelling community to take this process into account. The manuscript is written and presented nicely. The analysis is robust and the arguments are justified well based on the results presented here. I do however have few major comments.

- 1) The overwhelming focus on the radiative effects, by neglecting the dynamical and surface aspects, concerns me. I understand that the authors neglect them for the sake of simplicity, but they are actually important here. For example, between the two sets of CMIP5 models, SoN and NoS, the former shows more realistic trends in sea-ice extent. Could it be a coincidence? How much of it is really down to including FIRE and/or down to having different dynamical responses and surface descriptions in these sets of models? Please note that CMIP5 models vary widely in their description of sea-ice (e.g. Koenigk et al., 2014). Could the authors please check how the SoN and NoS models differ in these aspects?
- 2) FIRE would depend not only on how much it precipitates, but also on the frequency of falling snow. But there seems to be hardly any discussion about this (and how it varies across NoS and SoN). Or am I missing something here?

I hope the authors comment on these issues.

References

Koenigk, T., Devasthale, A., and Karlsson, K.-G.: Summer Arctic sea ice albedo in CMIP5 models, Atmos. Chem. Phys., 14, 1987-1998, <https://doi.org/10.5194/acp-14-1987-2014>, 2014

Thanks for taking the time to read & review our paper. You have highlighted ways in which the original submissions was unclear so we have substantially re-written the paper to address your concerns and those of reviewer 1.

Basically, we think you're right: the CMIP5-SoN September retreat looking "better" relative to observations is largely due to chance, so FIRE alone are not a big enough factor to overcome all other inter-model differences. Nevertheless, we are convinced that if the magnitude of FIRE as calculated by CESM1-CAM5 are realistic, then FIRE are important to improve simulated Arctic sea ice.

Regarding comment 1 we added text to Section 1 detailing some ways in which CMIP5 sea ice simulations can be affected. We refer to Koenigk et al. (2014) as well as Karlsson & Svensson (2013), and Massonnet et al. (2018) while discussing the variation in sea ice schemes. The bit about mixed phase clouds as an example of how atmospheric components can matter (with the Cesana & Tan papers) has been moved here from the discussion, and a comment on how the representation of ocean eddies with a citation to Horvat & Tziperman (2018) has also been added.

We don't see the benefit of a detailed analysis of model schemes, beyond how our discussion & conclusions comments that the two CMIP5-SoN models which reach ice free states the earliest are the GISS models and that is likely due to other parts of their cloud schemes resulting in underestimated IWP and way too much summer SW_↓.

To link from CMIP5 to our analysis, Section 5 paragraph 2 now specifically states that “the faster September retreat of CMIP5-SoN in Error! Reference source not found. is likely due to the full combination of properties in these models and not directly due to FIRE. Nevertheless, the controlled CESM1-CAM5 simulations demonstrate that the inclusion of FIRE...”.

Regarding comment 2 we do not have the properly calculated atmospheric snowfall frequency from our outputs. Our introduction now specifies that our two main aims are to determine whether FIRE in simulations leads to important differences in simulated sea ice, and whether our hypothesis of a thinner and more vulnerable pack is supported.

Section 2.1 now includes the following: “The strength of FIRE and the simulated response of other properties to FIRE depend on the frequency as well as the intensity of snowfall. This is accounted for in the model as radiative transfer is calculated at each model time step even though outputs are only provided monthly.” Further text explains that by looking at the differences in radiation terms directly we are actually comparing the fully coupled response due to FIRE, and cites Chen et al. as an example of how FIRE can cause such coupled responses. This means we fully capture the factors that are physically relevant to the sea ice retreat, but we cannot disentangle how much of the “real” cause is direct FIRE versus changes induced in circulation. We think that our re-written paper is sufficiently careful in its phrasing to emphasise this point and limit our conclusions to those for which we have sufficient supporting evidence.

We made many changes in response to reviewer 1, including extended statistical testing and analysing initial sea ice thickness in CESM1 SoN and NoS, finding that it supports our conclusion. We hope that our changes have clarified our approach and that you agree our main conclusions are suitably supported with the caveats and uncertainties adequately explained.

Finally, we made some unrequested changes: 1) fixing a bug in the CMIP5 data when we joined historical-RCP8.5. This mainly affects January 2006, so you can see the removal of a bug in the CMIP5-NoS spread in January for the new Supplementary Figure 3, and the removal of some positive trends in January for Figure 7(b). 2) adding reference to Jahn et al.’s CESM1 large ensemble analysis to support that our results show a FIRE-driven change that is larger than internal variability.