Norrköping, 2018-11-30

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.