Review for Keen and Blockley
Investigating future changes in the volume budget of the Arctic sea ice in a coupled climate model
Submitted to *The Cryosphere*

Summary

The authors investigate changes in the Arctic sea-ice volume during the 21st century. To do so, they use the Earth System Model HadGEM2-ES and output variables that describe the different components of the ice volume budget, i.e. basal melting and growth, top melting, snowfall, frazil ice formation, and ice advection. The effects of these processes on the ice volume can directly be quantified as they can be transformed into meters of ice thickness. Therefore the ice volume budget can be closed.
The method enables a thorough analysis of the evolution of the sea-ice volume budget during the 21st century. The authors find that the sea-ice loss is mainly driven by a decrease in basal growth over the 21st century in the decadal mean. However, by investigating the seasonal cycle, they show that different processes are at work depending on the time of the year. Finally, another important result of the study is that the changes in the processes do not depend on the forcing of the scenario but rather on the sea-ice area that is still present.

As there is still a high spread in climate model projections, this topic is interesting and could bring more insight into differences in ice volume budget evolution in different models. This method could be used for comparison between CMIP6 models, if these provide the needed variables, as suggested by the SIMIP protocol. Therefore, the topic is of relevance in the current context of sea-ice and climate research. The manuscript is well written but I would appreciate if the authors would clarify some points. I also have some additional suggestions.

Thematic comments

#1 In the end of the introduction, it is not clear to the reader what precisely is the scope of the study and what is new about it. It is clear that the authors will describe the evolution of the sea-ice volume budget, in a similar way to Holland et al. (2010), with the method of Keen et al. (2013). However, it is not clear if the scope of the manuscript is to introduce the method for further application (as is suggested in the conclusion) or to draw conclusions from the ice volume budget evolution to improve the understanding of changes in the Arctic climate system as a whole. I would appreciate if the authors make this point very clear in the beginning. It is difficult to follow the story of the manuscript otherwise.
The authors write on page 5: “although here we are also able to include individual components […] volume budget”. I suggest including this information in the introduction as it is a strong statement about what makes this study special.

#2 The reference period is chosen as the years 1960-79. I would like the authors to comment why they chose this period and not the period 1960-1989 (as usually used in studies for IPCC assessment reports) or the period 1950-79 (to have at least 30 years). I find this period rather short to be a reference period. I wonder if they have tried other reference periods? And if yes, do they yield different results?

#3 P4 L22-24: I do not understand why Eq. 1 should result in an ice volume that has to be converted back to effective ice thickness. As far as I understood, Eq. 1 gives thicknesses directly. I would appreciate if the authors could clarify this.

#4 P4 L30: Can the authors explain the especially steep decline of the winter sea-ice cover from 2080 onwards in the RCP8.5 scenario with their results? I would guess it has to do with the increase in water temperature inhibiting the formation of a winter sea-ice cover (see Bathiany et al. 2016). I would find interesting to hear if the authors have another explanation. It would be worth mentioning in the manuscript as well.
On the same note, I would suggest that the greater decline in basal ice growth (P9 L26) is linked to the greater decline in ice area in RCP8.5 stated earlier in the manuscript. Maybe these two could be linked to make a statement about the processes at work here.

#5 P7 L4: The lateral melting is not explicitly modeled. Do the authors have an idea of how important this term is? I could imagine that it is an important term in summer, as a component of the sea-ice albedo feedback.

#6 P7 L19-22: Holland found large differences between CMIP3 models. I would like the authors to comment on the implications of their findings for these differences or differences between CMIP5 models. In any case, I suggest moving this paragraph to the discussion in the end of the manuscript.

#7 P8 L29: The authors write that the extra top melting is enhanced by reductions in the surface albedo. Do they infer this directly from the model simulation? I wonder if maybe longwave radiation also has an influence on surface melting (see e.g. Notz and Stroeve, 2016), for example through clouds and water vapor? I would like the authors to comment on that.

#8 P9 L2: I am not convinced that the in-situ warming of the ocean is only a consequence of the ice cover retreat in your model. Could it not also partly be due to a higher advection of oceanic heat from lower latitudes, as stated for example in Burgard and Notz, 2017? I would like the authors to comment on that.

#9 The conclusion from Fig. 9 and Fig. 10 is that the changes in components of the ice volume budget are independent of the forcing and dependent on the remaining sea-ice area. I agree that this relationship is very clear. However, can the authors be sure that it is not rather dependent on the temperature? Several studies showed that the sea-ice area depends linearly on the air temperature (e.g. Winton, 2011; Mahlstein and Knutti, 2012) and cumulated CO2 emissions (Notz and Stroeve, 2016). It might be worth having a look at these relationships as well to get a larger picture and maybe a stronger conclusion.

#10 The last paragraph of the conclusion is somewhat unclear and is not very strong. This is not an advantage for the manuscript. I would suggest discussing a little more what makes this study special and what are its implications for future research. It is still not clear enough for me.

Writing comments

#11 The Section 2.2 about model integration is interesting but I think there are too many details. The effect of the different CO2 pathways on the temperature is what is important for the study. This effect can be seen well in Fig. 1. I therefore suggest that the authors leave in the reference to Moss et al. (2010) but that they leave out the bullet point list and the sentence “Fig. 1A of Caesar et al. […] scenarios.”

#12 I suggest writing down the exact limits for the study area in an appendix/supporting information. This might be useful for the comparison with future studies.

#13 Section 3.1, P5-6: The bullet point list makes the text well readable. To keep consistent, maybe the authors could add some numbers to the three first points. There, the results are described qualitatively in contrast to the three last points, where they are described quantitatively.

#14 The transition between Section 3 and Section 4 is quite abrupt. I would suggest working on a more logical transition.

#15 In section 4.1., Fig. 5B is cited instead of Fig. 5A and vice versa. I suggest reading through this section carefully again.

#16 In section 4.2., the reader is pointed to several different figures while the rest of the manuscript is very structured (one paragraph = one figure description). In this case, it is helpful for the message to look at the different figures. However, I find difficult to follow the story from P9 L1 to P9 L22. I suggest to try reformulating the message in a clearer way.

#17 P10 L26-27: The processes changing *at the ice surface* are listed and then "basal melting" is mentioned. Why?

Technical comments

P1 L9-13: These two sentences are long and contain too much information. Reformulating might clarify the message.

P3 L3: I suggest removing "for use in IPCC AR5". I think readers know the aim of CMIP5.

P3 L9: West et al., 2017 is cited. In the references, it is marked as "in prep.". I think they can therefore not be cited it in this context then.

P3 L12: Replace "as that used" by "as the one used"

P4 L6: Remove comma after the Moss et al., 2010 reference

P4 L15-16: The sentence is long. I suggest cutting after "scenario)" and starting the next sentence with "Fig.1".

P6 L25: The sentence is too long. I suggest stopping after "loss" and starting the next sentence with "The ice decline arises"

P6 L27: Add "seen in Fig. 3b" after "thickness".

P6 L29: The sentence is long. I suggest stopping after "line." and starting next sentence with "During"

P7 L8: Replace "and also how the seasonal cycle changes" by "and the changes in seasonal cycle".

P7 L27: "s" missing after 2040

P8 L 24-26: This sentence is too long. I suggest reformulating it to clarify the message.

P8 L31-32: Can the authors reformulate this sentence? I do not understand it.

P9 L9: add "process" between "this" and "that"

P11 L2: I suggest changing “and reduced basal growth during autumn/early winter” to “and in autumn/early winter due to reduced basal growth”.

P11 L15-18: This sentence is too long and unclear. I suggest reformulating it to clarify the message.

Figures

Fig. 1:
I suggest marking or shading the reference period
In the caption, replace “HadGEM2ES” by “HadGEM2-ES”

Fig. 6:
Have the authors looked into the period 2080-2099? Are the changes still similar? If not, would they bring additional information for the study?

Fig. 7:
Add in the caption for (b) that this is 2010-2019.

Fig. 9:
1960-79 instead of 1960-9

References

Arctic is written “arctic” in most of your references. I suggest reading through them carefully again.