Interactive comment on “Investigating future changes in the volume budget of the Arctic sea ice in a coupled climate model” by Ann Keen and Ed Blockley

Anonymous Referee #1

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We thank the reviewer for reading our manuscript, and for his/her comments. Our responses are included below in blue text.

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.

We intend the scope of the manuscript to be both the introduction of the method, illustrated by the application to the HadGEM2-ES model, and the investigation of the impact of the forcing scenario on the budget changes. This will be stated more explicitly in the revised manuscript and the statement on page 5 will be included in the introduction as suggested.

#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?

We have also considered changes w.r.t the 30 year period 1960-89, and the results and conclusions are almost identical. In the revised manuscript we will use this 30 year period for reference to avoid any concern about the period being too short.

#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.

Apologies, this is incorrect and this paragraph will read as follows in the revised manuscript:
We focus on changes in the sea ice over the domain shown in Fig. 2, covering the Arctic basin, and the Barents Sea. Figure 3 shows how the ice area and mean effective ice thickness within this domain declines for each of the model integrations during the period 1960 to 2090. The effective ice thickness includes the impact of any overlying snow by converting the snow to an equivalent thickness of ice using Eq. (1). Hereafter, whenever ice thickness or volume is mentioned it refers to an *effective* value, which includes the overlying snow as well.

#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.

Yes, it is most likely that the steeper decline in winter ice cover towards the end of the 21st century in RCP8.5 is associated with the warming ocean surface: certainly the DJF ocean top level temperature increases more rapidly towards the end of the integration. The link between the declining basal ice growth and the declining ice area is shown in Figures 9 and 10, but we agree that the faster decline towards the end of the 21st century could be discussed more explicitly, and linked with the ocean changes, and this will be done in the revised manuscript.

#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.

In a 'present day' (year 2000) long equilibrium run of our latest (CMIP6) model HadGEM3 GC3.1, the lateral melting term is important in the mean budget of the Arctic sea ice during JJA, when it is at most about 14% of the ocean to ice heat flux. It may become more important in a warming climate, and while this is outside the scope of this manuscript it will be possible to investigate this using data from CMIP6 scenario runs once they are available.
HadGEM2-ES does include an adjustment [*] to the ocean to ice heat flux when the ice concentration drops below 0.05, to provide a crude representation of increased lateral melting of small ice floes in the marginal ice zone.
[*] The heat flux is scaled by 0.05/ice_area so that the grid box integral of the flux becomes independent of the ice area .

In the revised manuscript we will include more about the lateral melting in the discussion.

#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.

The point we intended to make here was that given Holland et al found that CMIP3 models did not even agree on the relative role of melt and growth in the ice decline, we might also expect considerable inter-model differences when we can break these terms down further for the CMIP6 models. We agree that it would be better to move this to the discussion, and will clarify our text.

#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.

Yes we can see reductions in the surface albedo directly in the model. We now plan to look at the surface fluxes in more detail and provide a more comprehensive discussion of the factors contributing to changes in the surface melting.

#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.

Yes, the advection of oceanic heat from lower latitudes does also play a role, and as shown in Burgard and Notz it is the main driver for the long term warming of the Arctic Ocean. However a seasonal analysis of the Arctic Ocean budget for HadGEM2-ES shows that during the spring (MAM) and summer (JJA), when large increases in the basal melting are seen, atmospheric surface fluxes are the major driver of warming, especially for the upper ocean. We will expand the discussion here to more fully describe the causes of ocean warming.

#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 CO_2 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.

Yes, there is a linear relationship between anomalies in the ice area and the near-surface air temperature in HadGEM2-ES, which holds for all the forcing scenarios considered here. We did consider plotting Fig 9 and Fig 10 using temperature instead of ice area. The main reason that we decided not to was because we felt that the changes in the budget terms and the changes in ice area were more directly and closely linked (eg smaller basal growth term in the ice budget because the growth is occurring over a smaller area). We agree that it would be advantageous to widen the discussion to include these relationships as well and we will do this, and mention that the relationships shown in Fig. 9 and Fig. 10 are similar for the near-surface temperature.

#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.

We will expand the summary and discussion section to clarify what we feel are the key novel aspects of this study, which are:

- Our formulation of the volume budget includes individual components of the melt/freeze terms, so we know whether changes are attributable to atmospheric or oceanic processes.
- We consider the seasonal cycle of these changes, to understand the (sometimes opposing) changes at different times of year.
- We consider different ways of looking at changes in the budget components:
    - Ensuring the (declining) ice area is taken into account in order to construct a budget that balances the changing ice volume, and
    - Considering 'local' changes over the ice itself, which are more easily related to physical processes (eg more surface melting in a warmer climate)

  The combination of these approaches helps to understand the (important) impact of the declining ice cover on the budget.

In terms of future research, this approach can be applied to the CMIP6 models to gain a greater understanding of the process changes behind the modelled decline in ice area and volume, and we intend to do this once the CMIP6 data becomes available.

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.

The above suggestions will be incorporated into the revised manuscript

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

Yes, agreed, some linking text will be added to improve the flow here.

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

Thank you – yes this will be checked and corrected.

#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.

This section will be redrafted.

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

Apologies, this was not intended to refer to processes acting at the top surface of the ice, and should read: For this model, the processes that change *most per unit area of the ice* as the climate warms are......

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".
C5
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.

The changes suggested above will be incorporated into the revised manuscript.

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

The revised manuscript will contain a reformulated version, for example:
So during July and August, the amount of top melting per unit area of the ice is about the same during 2010-19 and the reference period (Fig. 7). Since the ice cover is lower in 2010-19, this results in a smaller volume of ice melt, expressed in Figure 6 as a net ice gain w.r.t the reference period. This same effect is also seen during later decades.

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”.

The changes suggested above will be incorporated into the revised manuscript.

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

This whole paragraph will be rewritten to address comment #10 above.

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

The suggested changes to Figures 1,7 and 9 will be made.

Regarding Figure 6, yes we looked at these changes for every decade to the end of the 21st century, and for each of the forcing scenarios. We did consider including a third decade, later in the scenario, in figure 6 (we had included 2070-79 in this figure in earlier drafts of the manuscript). However we decided not to include it because the general signals of change seen in these seasonal cycles plots were similar and did not seem to add to the discussion, and we felt that the impact of the Arctic becoming seasonally ice free was better illustrated in figures 9 and 10. In the revised manuscript we will mention that the changes in the later decades are similar.

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

The references will be checked and corrected where necessary.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-216, 2017.