Author Response to Referee's Comments

July 7, 2022

Thank you for the review of the manuscript. We are very grateful for your careful and insightful comments, which have contributed to the improvement of the original manuscript. We have worked hard to incorporate the feedback into the revised manuscript and have detailed here our thoughts and any changes made for each comment individually below. We hope you find the response and changes satisfactory.

Main Comment

Comment: "From your plots, it looks to me as though model differences are generally greater over steep, mountainous terrain (e.g. along the coastline and over the Transantarctic mountains). This highlights differences in the representation of orography and meteorological conditions over orography. Previous studies have demonstrated that a major source of such differences is model resolution, e.g. there are significant differences in the reproduction of influential mountains winds between simulations with grid spacings of 12-km and km-scale (e.g. Orr et al., 2014, 10.1002/qj.2296 for katabatic winds in E Antarctica; Heinemann et al., 2021, 10.3390/atmos12121635 for foehn winds over the Antarctic Peninsula). These differences are particularly pertinent for surface mass balance over ice shelves, since the Antarctic coastline is generally found at the foot of the steep slopes of the Antarctic plateau, and/or is in the vicinity of mountainous terrain. So I'm intrigued as to whether in your results you can see larger systematic differences in the vicinity of steep terrain that can be attributed to resolution (using the two MetUM models), and whether these differences are of sufficient magnitude and spatial scale to be pertinent for ice shelf mass balance. Also, as alluded to above, it could be that even the 12 km MetUM simulations are insufficient to reproduce climatically important influences of terrain-induced airflows. You do mention katabatic winds as a potential source of model differences, specifically on the Amery IS, but I wonder if it might be worth commenting further on the influence of orography on model differences and the implications of this."

Response: It is agreed that from Fig.4,5 model differences in snowfall and near-surface air temperature appear generally greater over steep, mountainous terrain and that this is an important feature worth highlighting. In the results we have therefore added:

Regarding snowfall: "The magnitude of the differences shown over the ice sheet appear greater over sharply varying topography, such as the Transantarctic mountain range and the steep coastal slopes of the ice sheet. An exception to this being high magnitude differences also shown in the mean component over the comparatively flat region of the interior of East Antarctica for the MetUM(011) and MAR(ERA5) (Fig.4d,g)."

Regarding near-surface air-temperature: "The magnitude of differences shown over the ice sheet again appear greater over regions of steep topography, particularly for the MetUM(011) and MAR(ERA5) outputs (Fig.5d,g)."

It is also agreed that a major source of these differences in meteorological conditions over orography is resolution and we have added a section to the discussion on this, quoted below. Reference has been made to the papers highlighted by the referee (Orr et al., 2014; Heinemann et al., 2021) that show the sensitivity of important orography-driven atmospheric processes such as foehn and katabatic winds to resolution. The paper by Orr et al., 2021 (https://doi.org/10.1002/qj.4138) is also referenced, which shows foehn wind sensitivity to resolutions at km and sub-km scale over Larsen C. The important influence of foehn and katabatic winds on climatology over ice shelves (typically in the close vicinity of mountainous terrain) is mentioned and reference made to papers in the literature that explore this, including: Bromwich 1989 (https://doi.org/10.1175/1520-0477(1989)070<0738:SAOAKW>2.0.CO;2); Cape et al., 2015 (https://doi.org/10.1002/2015JD023465); Lenaerts et al., 2017 (https://doi.org/10.1038/NCLIMATE3180); Datta et al., 2019 (https://doi.org/10.1029/2018GL080845); Elvidge et al., 2020 (https://doi.org/10.1029/2020JD032463)).

The high-magnitude localised systematic differences over mountainous terrain present in direct comparisons between the high/low resolution MetUM simulations (Fig.B1d, B2d and B3d) are highlighted. Further detail is provided for Fig.B2d where the difference in the mean near-surface air temperature, due to resolution, clearly extends over ice shelves such as the interior of the Amery ice shelf, which is a well-known katabatic wind confluence zone (Parish and Bromwich, 2007). It is suggested, that greater magnitude systematic differences in melt shown in Fig.6 compared to Fig B3(d-f) indicate the importance of different surface schemes over resolution in influencing variability across the particular ensemble of outputs studied. Finally, as mentioned in the referee's comment, we specify that even at 12 km resolution climatically important terrain-induced atmospheric processes, such as foehn/katabatic winds, are likely not being realistically resolved and this is explored further in Orr et al. 2021 where output from the MetUM RCM at 4 km, 1.5 km and 0.5 km during a foehn wind event on the Larsen C ice shelf show no obvious convergence towards observations during the event.

Section added to discussion: "At finer, more localised scales differing resolution is shown to create significant differences in the mean and seasonal/residual standard deviations for the monthly time series of each variable, see Fig. B1, B2 and B3 (d-f) in the appendix that show direct comparisons between the high and low-resolution MetUM simulations. The magnitude of differences in snowfall and near-surface air temperature due to resolution are greatest over regions of sharply varying topography, such as: the Transantarctic mountains; the coastal slopes of the ice sheet; and the Antarctic Peninsula. The representation of atmospheric processes occurring over mountainous regions including foehn winds that occur over the Antarctic Peninsula and katabatic winds occurring over the coastal slopes of East Antarctica are known to be resolution dependent (Orr et al., 2014; Heinemann and Zentek, 2021; Orr et al., 2021). Foehn and katabatic winds have been shown to impact climate over ice shelves, which are often in close vicinity of steep terrain, and are an important driver of surface melt (Bromwich 1989; Cape et al., 2015; Lenaerts et al., 2017; Datta et al., 2019; Elvidge et al., 2020). In Fig. B2d the difference in the mean near-surface air temperature, due to resolution, extends over ice shelves such as the interior of the Amery ice shelf, which is a well-known katabatic wind confluence zone (Parish and Bromwich, 2007). Despite this influence of resolution on the climatology over ice shelves, greater systematic differences in melt shown in Fig. 6 compared with Fig. B3(d-f) indicate the potentially more significant importance of differences in surface schemes across the ensemble of RCMs studied. It is expected that even at 12 km resolution climatically important terrain-induced atmospheric processes, such as foehn/katabatic winds, are not being realistically resolved as is shown in Orr et al. 2021 where output from the MetUM RCM at 4 km, 1.5 km and 0.5 km during a foehn wind event on the Larsen C ice shelf show no obvious convergence towards observations during the event."

Further specific comments

Line 8: "suggested" here is too weak. Suggest instead "Our results imply that…"

This is agreed and line 8 is updated to "Results imply that ... ".

Line 26: "The primary method of ice shelf retreat is through oceanic basal melting". I think this statement requires further qualification. Specifically, adding the word "currently", and something like "with the notable exception of some of the ice shelves on the Antarctic Peninsula (Pritchard et al., 2012)". Recent climate/ice-sheet modelling studies indicate that atmosphere-driven hydrofracture has in the distant past been, and will in the future be, the principal cause of Antarctic ice-shelf collapse (e.g. DeConto et al., 2021, 10.1038/s41586-021-03427-0; Pollard et al., 2015, 10.1016/j.epsl.2014.12.035).

It is agreed further qualification is beneficial here and the paragraph corresponding to line 26 has been updated to specify that this is true for the current climate and when considering the ice sheet as a whole, with notable exceptions being specific ice shelves such as Larsen A and B. In addition, a sentence towards the end of the paragraph is added specifying the critical importance of hydrofracture in distant-past and near-future SLR contributions from Antarctica, as is recommended by the referee.

Updated paragraph: "The primary method of ice shelf retreat, when considered across the entire ice sheet, is currently through oceanic basal melting (Pritchard et al., 2012; Paolo et al., 2015), although notable exceptions are recent and dramatic collapse events, such as the disintegration of the Larsen B ice shelf in 2002, which are linked to anomalous atmospheric conditions through the process of melt-induced hydrofracture (Scambos et al., 2000; van den Broeke, 2005; Bell et al., 2018). Anomalously high near-surface air temperatures (leading to enhanced melt events), as well as low accumulation (leading to reduced pore space of surface snow), result in greater lateral propagation of melt water into crevasses across the ice shelf, which then deepen due to increased hydrostatic pressure (Kuipers Munneke et al., 2014). This process reduces the structural integrity of the ice shelf and, in addition to fractures created through supraglacial lake filling and drainage, can eventually lead to collapse (Banwell et al., 2013; Kuipers Munneke et al., 2014). Recent ice sheet modelling studies indicate the critical importance of atmosphere-driven hydrofracture events in distant-past SLR variation (Pollard et al., 2015) and near-future 2100-2300 SLR estimates, particularly under high-emission scenarios (DeConto et al., 2021). Comprehensive spatiotemporal estimates of near-surface air temperature over Antarctica, as well as the accumulation of snowfall and quantity of melt water, are thus important for SLR predictions and are typically provided by RCMs (van Wessem et al., 2018; Agosta et al., 2019; Mottram et al., 2021)."

Figure 3c: Why are the interiors of the Ross and Filchner-Ronne ice shelves masked out?

The reasoning for this is mentioned on line 220 in the section of results on correlation and the same reasoning is assumed for subsequent results sections: "grid-cells where the ensemble 40-year average monthly melt is less than 1 millimeter water equivalent per month (mm w.e m-1) are masked as these regions only experience sporadic and insignificant magnitude melt events, essentially equating to numerical noise in the simulations.". Over the interior masked regions of the Ross and Filchner-Ronne ice shelves the ensemble average melt is less than 1 mm w.e. m-1 and so systematic differences across the ensemble are assumed to not be reliable/consistent and not of primary interest in comparisons. A sentence has been added to the captions of Fig. 6 and B3 specifying that grid-cells where the ensemble 40-year average monthly melt is less than 1 mm w.e m-1 are masked.

Figures 4-6: From neither the text nor the figures is it totally clear to me whether the difference is model - ensemble, or ensemble - model. I'd expect it to be the former, and that is indeed my impression from the text. However, "different to ensemble average" implies to me the opposite. Please make this clear, in the text where these figures are first referenced, and in the figure captions.

The text when first referencing the figures has been updated to include a sentence clarifying this: "Differences for each model are then plotted relative to this reduced ensemble average (model-ensemble avg.)". The captions of Fig. 4, 5 and 6 have also been updated for clarification: "The difference to the ensemble average (model-ensemble avg.)...".

Line 446: "The primary sources of large-scale, systematic differences between the simulations, for all variables and components, are identified as deriving from differences in: the model dynamical core; the surface scheme; parametrisation and tuning." In the discussion, the sensitivity of melt to the subsurface scheme is highlighted, and justification is given for the "secondary" importance of the factors listed in the subsequent sentence (driving data, resolution, domains etc.). We may then assume by way of elimination that the model dynamics and physics are the primary sources of systematic differences. I'm not sure though that this reasoning is actually stated, and I think it should be, somewhere in the Discussion section.

This is indeed the reasoning and it is agreed that it should be stated explicitly within the discussion section, therefore the following has been added to the end of section 5.1: "In this section, features including the domain specification, ice mask applied, digital elevation model and boundary conditions applied are argued to not be the primary contributors responsible for the large-scale systematic differences between the ensemble of model outputs. This result, in addition to the previously discussed secondary contributions of resolution and driving data towards large-scale differences, by way of elimination gives that the joint influence of choices in model physics, parametrisation and tuning is the primary factor influencing large-scale systematic differences across the ensemble."

Line 456: "Therefore, as concluded in Mottram et al. (2021), there is an importance on observational campaigns to correct for biases." Do you mean there is demand for new field observations with which to constrain model physics parameterisations? Or for (post-processing) model bias correction? This statement needs expanding on.

It is meant that greater observational spatio-temporal coverage and quality is important for both improving the tuning and updating the model physics and parametrisations, as well as to use and reduce uncertainties in post processing bias correction techniques. The sentence has been expanded upon to clarify this as suggested: "Therefore, as concluded in Mottram et al. (2021), there is an importance on observational campaigns to correct for systematic differences. Improved coverage and quality of observations will provide greater constraints with which to both tune and update the model physics and parametrisations, as well as to use and reduce uncertainties in post-processing bias correction.”.

Line 458: “2100 SLR” A bit specific. Suggest simply ”future SLR”. The same applies in abstract, line 3.

This has been changed both in the abstract (line 3) and in the conclusions (line 458 - original document) to use “future” rather than “2100”.

Technical corrections/suggestions

Line 5: Suggest italicising “Seasonal and trend decomposition using Loess”, and also perhaps capitalising the T in trend, to make it clear that this is what STL stands for.

The text has been updated to be in italics and trend has been updated to have a capital T.

Line 168: Suggest italicising “Seasonal and trend decomposition using Loess”

Italics have been added.

Line 201: RMSD: This abbreviation is defined in the abstract, but should be defined when first used in the main text also.

The abbreviation is now also defined in the first instance of RMSD in the text.

Line 220 and other instances: “mmWEqm-1”... I think there should be spaces between the units, so perhaps ”mm WEq m-1”. Or “mm w.e. m-1” as I’ve seen this notation used before.

Adjustment of all instances of “mmWEqm-1” in the text have been made to “mm w.e. m-1”.

Line 299: Remove repeated number 1 before “mm”

The correction has been made.