

Answer to Reviews

First of all we would like to thank Prof. Jan Lenaerts (Reviewer 1) and the anonymous Reviewer 2 for their time and thorough evaluation of our submitted manuscript. Below we would like to explain how we addressed some of the points raised during the review. We think that our manuscript has been notably improved during the review process.

Throughout the answer to reviewers **black** are the reviewers' comments, **blue** indicates our answers and **red** highlights the changes we made in the text or figures of our manuscript.

REVIEWER 1

“This study uses a regional atmospheric climate model (MAR) to study the effects of drifting snow on clouds over Antarctica, with a particular focus on cloud radiative properties. Comparing a simulation that includes drifting snow processes to another simulation excluding those, the authors show that drifting snow increases clouds (and cloud ice content) over large parts of the ice sheet, with impacts on atmospheric temperature and radiation. While the manuscript discusses an important and poorly studied subject, is relatively well written, methods mostly are sound, and the topic is relevant to GRL, I am afraid that my review of the current manuscript brought up several major issues in the results. I feel that each of those needs to be addressed in order for the manuscript to provide a substantial and robust addition to the current literature. Here below, I will highlight each of these significant issues, after which I will list some smaller items of note. “

Thank you for taking the time to read our manuscript and provide a valuable perspective on some of our results and discussion points. Below we describe in detail how we addressed the four “major issues” raised by Prof. Lenaerts.

Major Issue 1: Signal vs. noise – instrumental uncertainty

The authors start off their description of the results including uncertainties, but then leave those out in Section 3.5, which presents the comparison to station observations. Based on the instrumental error (and omitting any uncertainties arising from comparison between MAR and stations, which would further increase uncertainty) the differences between the two simulations are highly insignificant. For example, the IMAU AWS vs the Kipp&Zonen CNR instrument, which has a monthly mean uncertainty of ~5% for both downwelling LW and SW fluxes (e.g. Van Tricht et al., 2016 (Nat. Comms)), overwhelming the signal of the drifting snow reported here. I don't have specific guidance for the authors to overcome this, but I don't think the claim of an 'improvement' in surface radiation representation is justified (especially in light of Major Issue 2). Perhaps it would be useful to focus on case studies, where a strong blowing snow event passes by an AWS and the two simulations can be compared with the observed record.

In this study we focus on long-term (20 years) comparisons between observations and our MAR simulations across multiple weather stations (20 stations). During these two

decades, we do not focus on individual blowing snow cases. We also include weather stations that are not located in areas with frequent drifting snow (see new map Fig. S3 and new Fig.4D) The fact that we still see a reduction in the mean bias, i.e. more accurate radiative fluxes in the model with blowing snow, is strengthening the drawn conclusions. However, in a different publication using the exact same MAR model (Le Toumelin et al. (2021)) we already addressed the requests of the reviewer. Over an area with extremely frequent drifting snow events we show that, I) that the long-term mean bias in incoming longwave fluxes is reduced by 5.5 Wm^{-2} , 5 times more than for the average across all weather stations in our manuscript, II) that for a specific case study (2nd to 3rd Oct 2012), the difference in longwave radiation can reach more than 60 Wm^{-2} between MAR with and without drifting snow, notably outside the 5% error of the observations raised by reviewer 1.

However, we have decided to highlight that we have already looked at the impact of modelling drifting snow for specific drifting-snow areas and specific singular events in a study directly tied to our manuscript to make the results easier to interpret for the reader.

“Furthermore, using the same MAR model setup and observations it has been shown that during blowing snow events differences in LWD can reach up to 60 Wm^{-2} , far outside the uncertainty of in-situ observations (Le Toumelin et al., 2020).”

Additionally, we filtered our observations from the weather station dataset for blowing snow days based on an observationally threshold from D17 and D47 weather stations, and we are now able to show that the reduction in mean bias in incoming longwave radiation for example increased from 1.1 Wm^{-2} to 3.3 Wm^{-2} when focusing on drifting snow cases only. We have included a new figure showing drifting snow day statistics compared to observations (Fig.S2) and updated our result accordingly:

“We also compared our MAR model results to observations only during drifting snow days at the location of a given in-situ weather station (Fig. S2). We find that during drifting snow days that the reduction in the longwave biases is even more pronounced, leading to a three times higher LWD bias reduction of -3.3 Wm^{-2} , equivalent to a 50% reduction in the mean bias.”

Major Issue 2: Sublimation vs. clouds and SMB impacts

Throughout the paper, the authors mention the impact of sublimation on surface temperature and humidity, but fail to discuss the impact on the radiative budget (through the latent heat flux). Instead, they focus on the impact of clouds only, masking the potential effect of sublimation. In the end, the story remains somewhat convoluted: while Figure 1 shows a decrease of atmospheric temperature, but Figure 4 shows an increase in the surface radiation budget due to clouds – leaving me with the (perhaps false?) conclusion that sublimation (enhanced latent heat flux) dominates the enhanced CRE, and total surface radiation decreases, lowering atmospheric temperature? And, lastly, there is no discussion on potential SMB impacts – how much additional sublimation occurs in the drifting snow enable MAR simulation, and is there any impact of increased/changed clouds on precipitation?

Reviewer 1 seems to be addressing separate aspects when saying that “sublimation dominates the enhanced cloud radiative effect (CRE),..., lowering atmospheric temperatures.”

Firstly, atmospheric radiation, longwave and shortwave, is absorbed at the surface where it can be turned into heat and higher temperatures. However, in our cross section plots we show the whole atmospheric column *above* the surface. Here, sublimational cooling certainly dominates because the radiation is mostly not absorbed in the atmosphere, while the surface can still be radiatively warmed.

Secondly, the fact that we see an increase in downwelling longwave radiation *despite* a cooling of the near surface atmosphere just adds to the fact that blowing snow and subsequent changes in cloud properties enhance the atmospheric longwave emissivity notably. After all, the incoming longwave radiation at the surface is a multiplication between temperature and emissivity ($T^4 \cdot \epsilon$), where in our case temperature even decreases, so the atmospheric emissivity overcompensates for lower temperatures. We therefore have added some clarification in the text emphasizing this point:

“Our results further highlight the efficiency at which drifting snow enhances the atmospheric longwave emissivity. Overall, downwelling longwave radiation at the surface is a combination of atmospheric temperature and emissivity ($LWD = \epsilon \cdot T^4$). The fact that we see a notable increase in longwave radiation at the surface despite an atmospheric cooling strengthens the conclusion that drifting snow is a notable - and often neglected - component of the Antarctic surface radiation budget.”

Apart from the additions above, we do not think that SMB or precipitation impacts of drifting snow are a reasonable fit for a manuscript that mainly discusses the radiative impact of drifting snow over Antarctica. We will have to answer these valid discussion points in a separate manuscript.

Major Issue 3: No discussion of drifting snow climatology

The results start right off with the impact of drifting snow on the atmosphere, leaving the reader (at least me!) wondering throughout the rest of results how much drifting snow the model actually produces? And are there any statistical correlations between the strength/frequency of drifting snow and its impacts on the atmosphere (in other words, do the regions/seasons/... with most drifting snow also show the strongest response – and why (not)? I believe that, as minimum, a new Section 3.1 should be added to briefly discuss Drifting snow climatology.

We have added a new panel to Figure 4 (panel D), which now shows the climatological difference of the airborne snow particle ratio in g/kg. These results show that the areas with most frequent drifting snow are in locations where the surface slopes steeply towards sea level. This has to be expected, because drifting snow transport is driven by katabatic winds, which are driven by temperature/pressure gradients and gravitational pull. We also added text to our manuscript describing this new figure panel. However, we also wanted to briefly note that our manuscript borders on being too long for GRL, so we cannot be too expansive in our additional analysis and text.

“When looking at the climatological difference in airborne snow particles caused by drifting snow (Fig.4D) we see that the snow particles ratio is mostly enhanced over the steeper surface slopes of Antarctica, where the gravitational pull accelerates the katabatic winds. These constitute also the areas where the longwave warming is most enhanced in our simulation with drifting snow.”

In respect to more direct impacts regarding individual drifting snow events we would like to refer the reviewer to published and cited work with the same model configuration over

Adelie Land in East Antarctica, which are cited at various stages in the manuscript, i.e. Amory et al. (2021) - <https://gmd.copernicus.org/articles/14/3487/2021/> and LeToumelin et al. (2021) <https://tc.copernicus.org/articles/15/3595/2021/tc-15-3595-2021.html>

Major Issue 4: CloudSat-CALIPSO

Section 3.3 discusses the use of the CloudSat-CALIPSO satellite derived cloud fraction product. There are a few important caveats to this analysis: (1) how is cloud fraction defined in the satellite product (a weighted combination of low, mid, and high clouds? A simple sum? Or something else?), and is the cloud fraction definition consistent between MAR and the satellite product? If not consistent, this is not a fair comparison; (2) Figure 3 suggests changes in the cloud fraction between the two MAR simulations, but it is unclear at what level in the atmosphere, and what driving physical process would be – especially given the fact that all drifting snow (or the vast majority) occurs below 720 m above the surface. The authors later (Line 314 and below in discussion) discuss some physical mechanisms for increased cloudiness, but fail to give evidence which of those actually shows up in the model results, and how important they are.

Regarding point (1) we have added a clarification in the Methods section of our manuscript:

“CloudSat/CALIPSO data was checked for cloud detection on a profile-by-profile basis. A positive cloud ID (meaning: cloud in this profile) requires a cloud thickness of 960 m (480 m for low clouds below 2.75 km). CloudSat data below 720m a.s.l. are excluded due to surface clutter. Each individual profile is flagged this way as cloud/no-cloud, and the total cloud fraction is calculated as the number of cloudy profiles divided by the total number of profiles within the 2°x2° grid cell.”

Regarding point (2)

We think that we could have done a better job explaining this issue, but the point raised by the reviewer that the definition of clouds between CloudSat-CALIPSO and MAR are different is exactly the point we want to make. One cannot use most satellite cloud cover products to validate a model once blowing snow is taken into account. In our MAR model, the airborne ice/snow particles, once they reach a certain optical thickness, are considered to be clouds, often occurring in the lowermost 100m of the atmosphere. However, CloudSat-CALIPSO data used here *does not even report* clouds below 720 m due to surface cluttering issues, rendering the cloud cover product less helpful to improve model cloud cover when dealing with drifting snow particles, regardless if we would use a satellite simulator such as the COSP cloud simulator

(https://journals.ametsoc.org/view/journals/bams/92/8/2011bams2856_1.xml). We have emphasized this point in the manuscript, however, we also already discussed these issues in the first version of the manuscript:

“Further, below 2.75 km Cloudsat-CALIPSO data requires a minimum cloud thickness of 480 m in vertical extent, notably limiting the usefulness of active satellite data for comparison with regional climate models that include blowing snow. Conversely, biases in cloud cover between satellite observations and our regional climate model could also be caused by different definitions of what constitutes a cloud. However, we conclude that even if we would include a satellite simulator in our model (such as COSP), we would not be able to compare our model output to observations in a meaningful way, because data below 720 m is excluded in the observations due to surface clutter, the height in which drifting snow clouds most frequently occur.”

Additionally, we would like to refer the Reviewer to our cross-section plots to see where the cloud cover differences are coming from in terms of relative height compared to the

surface of Antarctica.

Other comments

L25: 2.17 W/m² – is this 0.17 W/m² a significant number? What are the uncertainties?

We use MAR here in a “perfect forecast” mode, where we force observed atmospheric boundary conditions via the ERA5 reanalysis data. However, in order to assess the underlying uncertainty of our simulations we would have to slightly disturb the observation-based reanalysis data and create an ensemble of MAR simulations over Antarctica. However, due to the computational expenses of such an ensemble this was not feasible at this point in time. However, in previous work we have demonstrated that the influence of drifting snow events on the longwave radiation at the surface can exceed more than 60 W/m², far outside the observational uncertainties (i.e. LeToumelin et al. (2021) <https://tc.copernicus.org/articles/15/3595/2021/tc-15-3595-2021.html>)

L26: missing – needs more context (missing where?)

We agree with the reviewer that this wording is ambiguous and therefore removed the word “missing”.

L39: Therefore... - I think this claim is not justified just based on the results presented here.

We agree that we do not discuss mass balance changes and sea level rise in our manuscript and subsequently changed “Therefore, we conclude that accurate sea level rise projections need to account for drifting snow.” to “**Therefore, we conclude that accurate Antarctic climate projections need to account for drifting snow.**”

L68: reorder words: “...all clouds, mixed-phase clouds can still exist above the boundary layer in the Antarctic interior”

We agree and changed the wording as indicated by the reviewer.

L147: ‘low height gradients’ – do the authors mean ‘shallow surface slopes’?

We agree that “shallow surface slopes” is a more accurate description here and changed the text accordingly.

Figure 2: what are the relative changes? Might be more interesting than the absolute changes, especially for LWP and IWP.

We already discuss the changes for IWP in the first version of the manuscript, e.g. “These changes in cloud ice water path correspond to a +10.3% increase over the grounded ice and a 10.2% increase over the ice shelves.” We have omitted the relative changes in LWP because the absolute values are so close to zero that a percentage change often becomes relatively meaningless on a local scale.

Section 3: if there uncertainties given, where do they come from?

These uncertainties are the spatial variability of the trends or differences in the data. We have indicated this now explicitly in the manuscript as “**note: throughout the manuscript uncertainties are given as the mean spatial variability as ± 1 spatial standard deviation**”

L307: units are missing

Added, thank you for capturing this.

L329: a more local analysis? What does this mean? (cf. Major issue 2)

We agree and have changed this to a more explicit wording of “**Note however, that a regional analysis of MAR in coastal Adelie Land suggests...**”

Reviewer 2

Reviewer #2 (Comments to Author (shown to authors):

This study uses a hindcast climate modelling approach to estimate the impact of drifting snow upon cloudiness and the radiation budget over Antarctica. To achieve this, It uses two sets of simulations from the polar regional climate model MAR forced by ERA5: one with the drifting snow functionality turned on, and the other with drifting snow disabled. The study then compares the results of these two simulations, with particular emphasis on the change in cloud cover (also assessed in reference to remotely-sensed cloud cover) and the radiative budget (with validation according to in-situ AWS observations). The study concludes that it is important to include drifting snow: biases compared to the in-situ radiation budget are reduced and there is a slight increase in the radiation budget of similar magnitude to current anthropogenic warming.

The results of this study provide more key evidence that developing sophisticated cloud schemes in regional climate models are essential to understanding ice sheet mass balance under future climate projections. It is clear that in regions where drifting snow is widespread such as Antarctica then the interaction of snow with the atmosphere and the radiation budget has to be captured to reduce biases in model simulations.

The study generally reads very well and is concise. The scope of the study is clearly defined. Figures are of high quality. The methods and datasets are employed appropriately. I have no major comments.

We would like to thank Reviewer #2 for taking the time to read our manuscript and provide valuable feedback on our work (and also for the encouraging words). We hope to have addressed all of their comments below.

Minor comments:

L73-89: this reads more like conclusions than an introduction. In particular, (1) my sense is that there is too much detail for this point in the manuscript on the methodological uncertainties associated with the remotely-sensed cloud cover. These issues are covered well later on in the Methods and Results. (2) I do not fully concur with the final statement that 'not accounting for drifting snow...might significantly bias [SLR projections]' - the use of the word significant usually implies statistical significance, of which I find no evidence in the manuscript - perhaps some softening here to bring it more inline with the statements made in the Discussion?

We fully agree with the reviewer here. We have removed the discussion of the remotely-sensed cloud cover from our introduction because, as stated by the reviewer, we already address it later in the manuscript. Specifically we have deleted the following sentence from the introduction: "However, current satellite cloud cover products over Antarctica often do not report values in the lowermost 100 m of the atmosphere where drifting snow is frequent and therefore neglect drifting snow and render model cloud cover verification difficult."

We also would like to apologise for the misleading use of the word "significantly" in this context. We actively tried to avoid this when preparing the manuscript by using "notable"

as an alternative, but in this case it has slipped under the radar. We removed “significantly” and used “notably” instead. Additionally, we also removed the word “sea level rise” from the introduction as our study is concerned with the climate of Antarctica, not mass flux changes - to soften the introduction a bit more.

L103-107: consider adding a brief note on how the model scales snow erosion with wind shear stress once the initial threshold value is exceeded.

We already have an explicit sentence in our submitted manuscript that all the details on how the model scales snow erosion with wind shear stress can be found in Amory et al. (2021) - <https://gmd.copernicus.org/articles/14/3487/2021/>. We think that if the readers would like to see the full description of the blowing snow module in MAR it is best described in Amory et al. (2021), also the scaling that the reviewer refers to.

L133-135: As currently written, this sounds like it could be referring to a different product/dataset to the one that is introduced earlier in the paragraph. Yet my understanding is that it is the same one? In which case, replacing 'the widely-used CloudSat-CALIPSO cloud fraction product' with, simply, 'it' could suffice.

Agreed and changed.

L217-226: this might fit better in the Discussion.

We would like to keep this quite detailed discussion here with the results where it is most closely related and fresh in the minds of the readers. We would like to reserve the general discussion at the end of the manuscript for slightly more “bigger picture” related topics.

L228: I'm not sure that this is the correct/intended meaning. The comparison to remotely-sensed data doesn't in itself tell us about how adding drifting snow changed the modelled cloud cover - so just remove L228 up to the first comma on L229.

Agreed and deleted.

Section 3.5: how about also comparing to surface temperatures?

Due to the general focus of the manuscript on drifting snow, clouds and the surface energy budget and the relatively tight length constraints in GRL we would like to reserve the broader discussion of skin temperatures of the surface for a future analysis. However, in Fig.1 we discuss the vertical changes in temperature between the two simulations.

Figure S1. Consider adding a location map overview of where these AWS are located, to ease comprehension of the figure for those who are not familiar with Antarctic AWS.

We think this is a good point and we have added the location of the weather stations in the main part of the manuscript in Fig.4D, but also in more detail in the supplements (Fig.S3).