Reviewer 1

This paper deals with improving how surface albedo is represented in land surface models by data assimilation of satellite-derived albedo. I will note that I am not an expert on data assimilation and thus, cannot really comment on the applicability of the DA approach used here. The only comment I have about the DA is I'm not sure how all the parameters in Table 1 are tuned to fit the albedo, and maybe some more explanation as to how that is done is warranted. I find the sensitivity analysis ok so no strong comments there. My comments mostly pertain to the MODIS data and result of clarify in the manuscript.

We thank the reviewer for taking the time to read and comment on the manuscript. We welcome comments from someone who is not an expert in DA since we hope to reach a wide audience with this study. This study uses data assimilation for parameter estimation instead of the more common state estimation approach. In state estimation, the model state is updated whilst keeping the model parameters fixed (see paragraph starting on L26 for a list of state estimation studies). For parameter estimation, the parameters are allowed to vary in the ranges prescribed. They are optimised together in this parameter space. The cost function is dependent on the parameters (i.e., Eq. 1: J(x), where x is a vector containing all the parameters). The cost function has two parts, one measuring the mismatch between the model outputs and the observations, and the second part the mismatch between the new parameter values and the original values. Since the model outputs depend on the parameters, changing the parameters changes the model outputs impacting the mismatch between it and the observations (in our case, satellite retrievals). Even parameters not directly involved in the calculation of the snow albedo will impact the output, as we see in the sensitivity analysis, since many processes are interlinked. For example, the ice albedo is not part of the snow albedo equations but impacts the overall albedo calculated.

We have added the following to the manuscript to clarify this:

> "To minimise the cost function, two algorithms were considered in this study. *They both work by varying the full set of parameters considered within the prescribed ranges, retaining the set of parameters at each iteration which reduces J(x) compared to the previous iteration.* The first *algorithm* is a deterministic gradient-based method… "

One general comment, it is true that under low sun angles, the albedo retrieved from satellite is less accurate, yet I find it strange to have a discussion on winter months when the sun is below the horizon. I realize that there is a need to spin the model up over full annual cycles to accumulate snow, but I found it confusing to read that Nov-Feb were omitted in paragraph starting around line 160, but previously in the manuscript it was said the albedo would be set to April values during the winter period. So what exactly is being done here?

We apologise for the confusion caused. The step of setting the winter values to the April values was not undertaken by us but rather by the Box/MOD10A1 product creators. We have clarified in the text which post-processing steps were done by the product generators and which were done by us. Full changes to the paragraph can be found in response to the next comment.

I also think more description of the MOD10A1 product is needed here. Is it a true daily albedo integrated over a full 24 hour period based on several overpasses of the MODIS instrument, or is there a certain swath that is used for a specific local time? It is unclear what time of day is being used for the optimization. The solar zenith angle varies of course with the day of the year as well as the time of day and thus there is likely a solar zenith angle dependence still in the MODIS albedo product. It is also mentioned that VIS and NIR albedo are used (e.g. Line 70), but my understanding is that the MOD10A1 is a broadband albedo product. Further there are no figures showing VIS and NIR albedo. Since the MOD10A1 data set is being used for the DA, much more information about this data set is required and a discussion of its accuracy. I'm particularly not convinced that there exists a true north/south gradient in the albedo. That doesn't fit with the known pattern of precipitation that brings fresh snow to the ice sheet. The tuned model actually seems to do a better job of expected albedo pattern (e.g. Figure 1). The MODIS pattern looks to be a solar zenith angle dependence. Is the albedo normalized by the Solar Zenith Angle? I also find the MODIS albedo product to show too high of albedos (i.e. Figure 1, Figure 4).
We agree with the reviewer that for DA it is important to understand the data set used in the assimilation and its limitations. As such, the section describing the product has been expanded as well as the discussion (see below). Firstly, let us address the different points highlighted in the comment:

- The dataset uses data from the MODIS sensor on board the NASA Terra satellite. The Terra satellite has a sun-synchronous, near-polar circular orbit which crosses the equator at ~10:30 A.M. local time (Hall & Riggs, 2016). Complete global coverage occurs every 1-2 days, with more frequent retrievals occurring near the poles. MOD10A1 is a clear-sky product, and when more than one observation is available, the value representing the best sensor view of the surface in the cell based on solar elevation, distance from nadir, and cell coverage is kept. In this study, we use the data from Box (2017), which is based on the MOD10A1 data but with additional denoising, smoothing and gap-filling. This dataset was specifically created for applications over Greenland (Box et al., 2017) and validated against ground-based measurements from the PROMICE stations (Fausto et al., 2021).

- MODIS has a range of albedo products integrating over different spectral bands e.g., VIS/NIR/SWIR/complete products. The reviewer is correct in stating that the MODIS product used in MOD10A1 is a broadband albedo product. However, in the ORCHIDEE model, VIS and NIR albedo are computed separately. Therefore, to be able to compare the model to the satellite retrievals, we averaged the VIS and NIR albedo model outputs. This is stated on L70 of the ORCHIDEE description section.

- As for solar zenith angle dependence, this is managed first in the MOD10A1 distribution and second in the Box (2017) distribution. For MOD10A1, pixels with solar zenith angles > 70° are masked (where night is defined as a solar zenith angle ≥ 85°). This and other adjustments to the dataset eliminate a spurious darkening trend concentrated over snow and in the northern part of Greenland (Polashenski et al., 2015). Over Greenland, a residual bias based on the sun's angle above the horizon is observed in MOD10A1 when compared to the in situ measurements. Box (2017)'s dataset corrects this bias according to time and latitude using a linear regression (see Fig. 4 in Box et al., 2017). Furthermore, in Box et al. (2017), Figure 2 shows albedo values measured at an in situ site in Greenland (KPC_L station from the PROMICE network). These values go as high as 0.9 and correlate well with the de-noised MOD10A1 product.

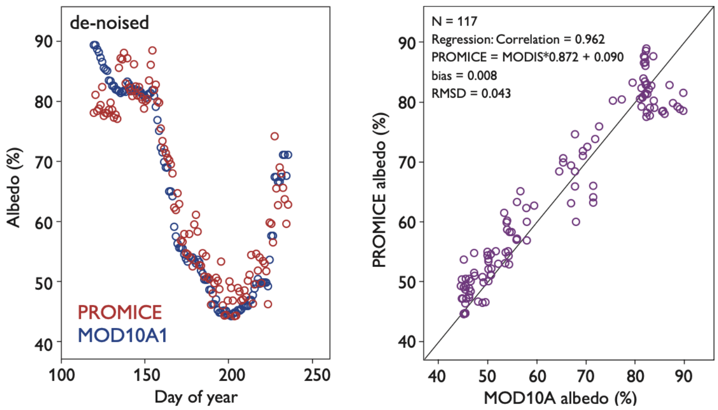

Fig. 2. Year 2013 example comparison of daily de-noised albedo from satellite (NASA MODIS MOD10A1 Collection 6 data) and the ground (PROMICE) for the KPC_L (Fig. 1) station on the north-eastern Greenland ice sheet.

Some SZA bias may remain in the dataset, and we have added lines to acknowledge this in the conclusion. However, it is also likely that the north-south gradient in albedo exists. Although there is more precipitation in the south of the GrIS, fresh snow in the south decays faster due to warmer temperatures. In addition, to melt processes, there are also other processes that impact the GrIS albedo, such as dust deposition and algae growth. For example, on the bare ice zone of the southwestern portion of the Greenland ice sheet, ice algae can account for 75% of albedo variability (Cook et al., 2020; Williamson et al., 2020).

In light of all these comments and responses, the following text has been expanded in the MODIS description section in Methods and Data:

"In this study, we used satellite-derived *snow* albedo from the NASA (National Aeronautics and Space Administration) MODIS (Moderate-Resolution Imaging Spectroradiometer) MOD10A1 product (Hall et al., 1995)*. This product uses data from the Terra satellite, which has a sun-synchronous, near-polar circular orbit crossing the equator at approximately 10:30 A.M. local time (Hall & Riggs, 2016) and providing global coverage every 1-2 days. MOD10A1 is a clear-sky daily product. When more than one retrieval is available on a given day, which is the case near the poles, the best value is kept. This best value is chosen based on solar elevation, distance from nadir and cell coverage (Hall & Riggs, 2016). In addition, pixels in the MOD10A1 with solar zenith angles greater than 70° are masked (night is defined as a solar zenith angle greater than 85°).*

The version of MOD10A1 we used in this study was further processed by Box et al. (2017). Using data from collection 6 of MOD10A1 (Riggs et al., 2015; Hall and Riggs, 2016), Box et al. (2017) de-noised, gap-filled and calibrated the data into a daily 5km grid covering Greenland for the years 2000-2017. *This dataset was further validated against ground-based measurements from the PROMICE stations (Fausto et al., 2021) and the residual bias in the dataset based on the solar zenith angle corrected for using a linear regression according to time and latitude (Box et al., 2017). Finally, in this dataset,* when ~~MODIS retrievals are inaccurate due to~~ *there is* not enough solar illumination *to compute the albedo* during the winter months (January, February, November, and December), *Box et al. (2017)'s distribution swaps in the* April values~~ are swapped in~~.

In this study, we *used the dataset created by Box et al. (2017),* further aggregating these data to the resolution of the ORCHIDEE outputs, imposed by the meteorological forcing files (20 km)."

and to the discussion and conclusions:

"We must also remember that there are errors linked to the satellite retrievals themselves. Indeed, the large uncertainties in the winter months led us to omit them ~~during the optimisation stage of~~ for this study. For the other months, we set the observation errors to be the mean-squared difference between the observations and the prior model simulation to also account for the structural model errors. However, in practice, the true errors may be very different. *For example, although steps to correct the solar zenith angle bias in the product have been untaken, it is possible that the strength of the north-south albedo gradient observed in the data is an artefact of the product. Without clear and robust uncertainty quantification, we cannot disentangle natural GrIS processes from biases in the retrievals. There is an urgent need for data producers to provide this uncertainty, ideally at each time step.*"

More specific comments.

1. Line 68, do you have a reference to support the statement that Greenland soil type is loam?
   Citation to the HWSD global soil texture map has been added.

2. Lines 90-97 – yes the MODIS retrieval has larger errors under high solar zenith angles (SZA), but most of the ice sheet is dark in winter and thus, there is not albedo retrieved. It's not necessarily that MODIS retrievals are inaccurate, the ice sheet has no sun. Thus, this section needs to be rewritten to be more exact, and also some value of SZA for which you think the MODIS data are inaccurate needs to be stated (along with the appropriate reference).
   We thank the reviewer for these precisions. We agree that referring to the retrievals as inaccurate is not strictly correct. We have rewritten the MODIS description section to be more precise and included all the comments above about the product specifics and limitations (see above for the new paragraph in response to earlier comments).

3. Figure 1. Over which months is the albedo shown? Also, if Figure 1 is averaged from March to October like the other figures are, then the spatial pattern doesn't really make

sense to me for the observations which makes me think there is a bias in the observations. You would expect higher albedo values over the high elevation regions, not a north to south gradient as precipitation patterns do not show this north-south gradient. Future, the albedo values are too high from the observations considering this is summer albedo and the surface is melting over large parts of the ice sheet (see for example melt patterns from passive microwave https://nsidc.org/greenland-today/. For example, I'm including a figure here of the melt in 2022 and thus, you would expect an albedo pattern that loosely follows the microwave melt detection.

Figure 1 showed all months. We have replaced these with ones averaged over just the summer months for consistency. The albedo values are lower now that the figures consider only the summer albedo and this has been clarified in the caption:

> "Retrieved and simulated mean albedo over Greenland (averaged over *March-October for* 2000-2017)"

4. Figure 2 and its discussion on lines 180-185, I don't follow how you can say you see a degradation in model-data for March to October and that the improvement was only in the winter month. How is that shown? All the images in that figure are stated to be averaged from March to October, so where does one see that there was an improvement only in winter (and at a time when the satellite is not even recording albedo?)

   These results were part of the preliminary experiments used to find the ideal setup for the main experiment (see L156). These experiments helped to a) choose the optimisation algorithm to use in the main experiment, b) pick the time period required and c) the weightings required. It was during these experiments that we realised how much these winter months impacted the optimisation and decided to exclude them from the main experiment since they are not "true" observations. However, to be consistent with the rest of the manuscript, we have redone these optimisations over Mar-Oct only and included these instead.

5. Line 185-186, I'm not sure what is meant by that statement. I also do not know which figure the paragraph that follows refers to.

   We acknowledge that this statement about local minima comes a bit out of left field for people unfamiliar with DA. To find the best set of parameters, we are minimising the cost function (Eq 1) with respect to the parameters. If we think about our cost function as a simple curve (i.e., J(x) on the y-axis and parameter x on the x-axis), within the range allowed for the parameter, there will be a minimum value (where the gradient is zero and positive on either side). However, if the curve is not a smooth bowl in the given range but rather an undulating function, this will not necessarily be the only place where the gradient is zero with positive values on either side. All examples where this is true are called "local" minima, and the "global" minimum is the minimum of all these. The gradient-based algorithm uses the negative gradient values to find how to reduce the cost function. If there are only positive gradients around a point, then the gradient-based algorithm becomes stuck. We are at a minimum but not necessarily the global minimum. When manually tuning a model, the same can be true - we only change a parameter manually when the RMSD decreases, the same as a negative gradient. We have added the following text:

> "Since the prior model used was already extensively manually tuned, it is likely that we started very near to a minimum *(i.e., somewhere where the gradient is close to zero surrounded by positive gradient values)*. *However, this is not the global minimum since we have been able to reduce the cost function further when using a different algorithm (i.e., in the GA case). Since gradient-based algorithms rely on negative gradient values to minimise the cost function,* ~~As such, since~~ the gradient-based algorithm is unable to leave local minima, *and therefore,* the cost function is hardly minimised."

For the following paragraph, we have added a reference to Figure 2 to the text as well as titles to each panel of the figure for clarification.

6. Line 212-214, there is no observed albedo in the winter months, so how can you talk about fitting to observed values during winter? Even in mid-January very little of the ice sheet is illuminated. Thus, much more discussion on what is meant by winter and the fitting is needed. I do not necessarily believe that filling in winter values with April values is accurate as the albedo will vary strongly as a function of precipitation and this is completely neglected if you are replacing winter albedo values with those in April.
The reviewer is correct that we need to be more precise with the language used and not talk about fitting the albedo in winter. To simplify matters, we have removed the winter months from the analysis altogether. Again, we stress that setting the winter values with April was not done by us. This is a feature of the product used. Nevertheless, by removing the winter months from the analysis, we should avoid dealing with these issues.

7. Figure 4, generally new snow has an albedo around 0.85 and thus it is clear that the MOD10A1 values are too high. How does this impact your results and should you really be fitting to something that is unrealistically high?
In the optimisations, we include uncertainty in the cost function (matrix R, see L109 and Discussion). This term helps stop overfitting and takes these types of errors into account. In future works, we could consider having a variable uncertainty to take into account the larger uncertainty during winter. However, the Box distribution of the MOD10A1 collection 6 product does not have uncertainties included, so it would be hard to accurately determine an uncertainty time series to use at each pixel. This is an ongoing issue with remote sensing and land surface modelling - it is extremely hard to quantify these uncertainties. When uncertainties are provided with remote sensed products, they tend to be uncertainties due to the sensors or retrieval algorithm used. Data users need to also think about the uncertainty around the representativity of ground-based measurements and different resolutions. As discussed in the conclusions, we set the uncertainty to be the mean-squared difference between the observations and the prior model simulation. This tends to be quite a conservative way to set these uncertainties and is widely used in data assimilation studies. Nevertheless, we have expanded the

discussion to highlight the importance of these uncertainties and as a recommendation to data providers:

> "*Without clear and robust uncertainty quantification, we cannot disentangle natural GrIS processes from biases in the retrievals. There is an urgent need for data producers to provide this uncertainty, ideally at each time step.*"

8. Figure 7. I'm not sure which refer to edge vs. middle as there is no hatching shown in the actual figure.

   The caption of the figure has been expanded to contain a clearer explanation of how to distinguish between the middle and the edge sensitivities, and the hatching in the legend changed to be filled in instead.

   > "In each case, the sensitivity of the parameters is shown for simulated quantities at the edge of the ice sheet *(shown by the filling at the edge of each box)* and in the middle of the ice sheet *(shown by the filling in the middle of each box)*."

References

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