Review on “Improving modelled albedo over the Greenland ice sheet through parameter optimisation and MODIS snow albedo retrievals”

OVERVIEW

This paper aims to improve simulating snow albedo by the ORCHIDEE land surface model via assimilation of MODIS snow albedo retrievals and parameters calibration through ORCHIDAS. The domain of study is the Greenland Ice Sheet (GrIS). ORCHIDEE is used in offline mode forced using atmospheric data coming from the MAR model. The optimisation is performed over three random years (2000, 2010 and 2012) taken over the period of study 2000-2017 while the improvement of snow albedo is checked over the whole period of study. An increased weight is given to pixels defining the edges of the GrIS compared to other pixels in order to improve albedo simulations over those areas crucial in term of mass loss. Moreover, a comparison between outputs of ORCHIDEE and MAR on surface mass balance, runoff and sublimation is carried out as well as a Morris sensitivity analysis of various parameters related to snow albedo and snow density relative to several outputs such as surface temperature, sensible and latent heat fluxes in addition to the previously mentioned outputs.

GENERAL COMMENTS

The paper covers the timely question of snow/ice albedo modelling over the GrIS in the context of increased melt in links with global warming. Here the chosen approach is to assimilate satellite-derived albedo retrievals, which only few studies have performed before. As such, the novelty aspects of the paper are self evident. Therefore, such work would clearly deserve to be published. Nevertheless, I have some reservations about the methodology and I especially find the paper difficult to read. As indicated by one previous referee, *“The article reads like a description of the research in the way it was conducted.”*. Unfortunately, after the first revision, this problem still remains. My main issues with the paper are the following:

- The goal of the paper is straightforward: improve snow albedo by assimilation. But why do we want to do that? In which context? Global land surface modelling, if so, why using MAR instead of ERA5 as atmospheric forcing? Or are the targets are ice sheets and large glaciers? During all the several readings I made, I asked myself what were the purposes of such study and I could not find the answer anywhere in the introduction where normally one would expect to find such information.

- The methodology clearly misses validation using independent datasets. Modelled albedos are compared with MODIS data that are partially assimilated (3 years over 18 years). Then ORCHIDEE outputs of surface mass balance, runoff and sublimation are compared with outputs from MAR, while ORCHIDEE is forced using atmospheric data coming from MAR itself. Obviously both model outputs are related, the main difference would come from the modelling approach of snow, ice and albedo in ORCHIDEE and MAR. As such, MAR cannot be considered as a reference as stated by the authors. I do, however, acknowledge that the authors raise some reservations about the limitation of the comparison in the paper.

- Several peripheral considerations (such as the test of the two minimisation approach, L-BFGS-B and genetic algorithm) degrade the readability of the paper while providing very little novelty in terms of science.

As I indicated beforehand, this paper cannot be published in The Cryosphere in its current version. Nevertheless, I do think the authors, who are renowned experts in their fields of expertise, have the ability to improve the paper to reach the publication stage. It would be unfortunate that such good science would, in the end, not be published. I list my comments and questions section by section below:

SPECIFIC COMMENTS

<u>About the objectives of the paper:</u>

- Please specify loudly the context and the purposes of the study in the introduction. Is the goal of the study to focus on ice sheets and large glaciers? If so, why? If not, do the authors focus on global or large scale climate simulations? If so, again why? Please provide also adequate references to justify your choices.

- Justify more adequately the use of MAR in relations to your goals either in the introduction and section 2.2 describing MAR. MAR is indeed a fine atmospheric model well adapted to Greenland but by using MAR, the authors make their approach less replicable to other parts of the world if their goal is to fine tuning parameters for modelled albedo for ice sheets and large glaciers as optimal parameters found are likely to depend on atmospheric inputs.

- The authors starts to talk about runoff, sublimation and surface mass balance (SMB) from L. 92-93. This should be specify in the introduction why those quantities are considered in the discussion section of the paper. Also MAR includes a modelled albedo. How does modelling in ORCHIDEE compare with MAR's? Might be worth comparing both if possible.

- Mass loss in Greenland is not only due to snow melting but also ice dynamics in outlet glaciers that are located in many edge pixels. This should be mention somewhere as it is a limitation of the authors' work (no ice dynamics considered) in the introduction with adequate references such as:
    - Aschwanden, A., Fahnestock, M. & Truffer, M. Complex Greenland outlet glacier flow captured. *Nat Commun* **7**, 10524 (2016). https://doi.org/10.1038/ncomms10524
    - Khan, S.A., Bjørk, A.A., Bamber, J.L. *et al.* Centennial response of Greenland's three largest outlet glaciers. *Nat Commun* **11**, 5718 (2020). https://doi.org/10.1038/s41467-020-19580-5

- Darkening of the GrIS is mentioned in the introduction as a very important phenomenon for albedo. Nevertheless, this darkening is not explored anywhere in the paper. The ability of the authors' approach to capture this darkening could be studied in the result section.

- Since this paper is about albedo assimilation, a longer paragraph dedicated to that very subject would be much appreciated. Also several references are missing, among others:
    - Dumont, M., Durand, Y., Arnaud, Y. and Six, D.: Variational assimilation of albedo in a snowpack model and reconstruction of the spatial mass-balance distribution of an alpine glacier, J. Glaciol., 58(207), 151-164, doi: 10.3189/2012JoG11J163, 2014.

    - Cedelnik, J., Carrer, D., Mahfouf, J.-F. and Roujean, J.-L.: Impact Assessment of Daily Satellite-Derived Surface Albedo in a Limited-Area NWP Model, J. Appl. Meteorol. Clim., 51(10), 1835-1854, https://doi.org/10.1175/JAMC-D-11-0163., 2012.

    - Boussetta, S., Balsamo, G., Dutra, E., Beljaars, A. and Albergel, C.: Assimilation of surface albedo and vegetation states from satellite observations and their impact on numerical weather prediction, Remote Sens. Environ., 163, 111-126, https://doi.org/10.1016/j.rse.2015.03.009, 2015.

  - Geppert, G.: Analysis and application of the ensemble Kalman filter for the estimation of bounded quantities, PhD thesis, Universität Hamburg, Hamburg. Doi: 10.17617/2.2161673, 2015.

- A plan at the end of the introduction describing the sections developed in the paper would improve the readability of the paper to a great extent.

About the methodology:

- The set of parameters is optimized over the whole GrIS but albedo conditions varies greatly between the interior and the edges of the GrIS as stated in the paper. Does it make sense to follow this approach instead of a multisite approach? Also, Figure 4b shows differences between Northern and Southern parts of the GrIS.

- The Morris sensitivity analysis does not intervene in the choice of optimized parameters. It should be instead include in a discussion section after the analysis of results. Also it involves parameters that are not optimized. The Morris sensitivity analysis and the comparison between MAR outputs and ORCHIDEE outputs could be merged in a same discussion section as they tend to complete each other. This discussion should also be pointed out in the introduction of the paper. Also, the list of parameters of interest should have be provided in the ORCHIDEE land surface subsection. Since the authors focus not only on albedo per se but also on rate of density change and parameters controlling surface mass balance and runoff, authors should provide more information on the snow model employed in ORCHIDEE or at least point towards adequate references. This would help the readers to understand more clearly the authors' objectives.

- τ_{max} seems to have almost no influence for albedo according to the Morris sensitivity analysis, why keeping it for optimization?

About MODIS data:

- About MODIS albedos, it is unclear to me if there are any reliable observations during winter time. Authors exclude data from November to February in their data assimilation system but later states albedo is improved in winter times in the result sections. This is rather confusing. I suggest you exclude all albedo comparison for winter period as I do not think they make sense (see e.g. sentence L. 280-281 *"We also see that the optimisations improve the fit … in the optimisation"*). Alternatively, authors can explicit in the manuscript their methodology regarding albedo during winter time.

- How do the authors aggregate MODIS data from the original grid to the MAR grid (just an average or something more sophisticated)? That should be made more explicit in the text.

About the snow and albedo modelling and spin up:

- L. 69-71: "*For the new icy soil type, … those of the loam soil type because it is the dominant soil type in the non-ice-free reguibs around the GrIS (Fischer et al., 2008)*". Does it make sense to assume that basically, the icy soil type is impermeable (porosity and saturation are equal) for the edge of ice sheets? What is it classically used for ORCHIDEE in CMIP experiments and how do your modifications compare with the usual approach?

- I have several questions and comments on the following point "In the absence of fresh snow, snow albedo decreased exponentially from its fresh value" (L. 74-75):

  - According to Table 1, fresh snow has a fixed albedo. This is a rather crude approximation. Does it make sense? Why not instead using an increase such as the linear increase with snowfall intensity implemented by Boone and Etchevers (2001)? Could the authors reflect on that?
    Reference: Boone, A. and Etchevers, P.: An intercomparison of three snow schemes of varying complexity coupled to the same land-surface model: Local scale evaluation at an Alpine site, J. Hydrometeorol., 2, 374–394, 2001

  - Also, fresh snow occurs during winter when no MODIS data are available, how does B_{aged} can be calibrated?

  - A_{aged} is the albedo of pure ice, please indicate it in Table 1.

  - The snow albedo modelling implement an exponential decrease law that is close to the approach of Douville et al. (1995) but involves, contrary to the aforementioned paper, soil temperature. Where does this formulation come from? Has it been previously published and validated before? Has it been used for ice sheet before as well? I think the modelling of albedo deserves more explanation or justification in the paper.
    Reference: Douville, H., Royer, J., and Mahfouf, J.: A new snow parametrization for the Meteo-France climate model, Clim. Dynam., 12, 21–35, 1995.

- L. 180-182 *"All the simulations performed in this study include two years of model spin-up to allow the snow to accumulate … ensuring correct initial states"*: I assume that the two years of model spin-up are 1998 and 1999 since the period of study is 2000-2017. Am I correct? If so, please indicate it in the manuscript. Also what was the snow depth before model spin-up. Was it 0? Did you allow only snow to accumulate or was melting also occurring? Please provide more details. One key point of a scientific paper is that readers can be able to reproduce the described experiment themselves. The authors should focus on that very point. It would have avoided many questions I have listed.

<u>About the data assimilation approach:</u>

- The approach of selecting three random years (2000, 2010 and 2012) for the optimisation raises the question of the robustness of the approach. While it shows consistent improvement over the whole period of study 2000 – 2017, I wonder if selecting other years would have led to different results especially if, for example, 2000, 2001 and 2002 would have been selected. Could the authors reflect on the robustness of their approach?

- L. 120-122 *"We define the observation error (variance) as the mean-square difference between … but also the model errors"* I am partly unsettled by the authors' stance on observation errors. It breaks the underlying assumption of independence between background, model errors and observation errors of the Bayesian statistical formalism. Nevertheless, I can see why the authors adopt such approach but it deserves more justification. Basically, why using this approach instead of a fixed error variance for MODIS albedo data? Also, it induces more uncertainties at the GriS's edge than in its centre that probably comes from the modelling approach. Does this assumption really make sense?

- About τ_{max}, it was shown by the Morris sensitivity analysis that it has almost no impact. Figure 6b focus on correlations. What about variances? The cost function's Hessian at minimum can provide local sensitivity analysis (see e.g. . Also its inverse is the analysis covariance matrix. It would be of interest to study analysed variances compared to background variances and evaluate their reduction. Authors show strong anti-correlation between δ_c and τ_{max}, but associated comments should be weighted with the role of variances.

- Two minimisation algorithms are considered in this study: the gradient-based L-BFGS-B approach and the genetic algorithm. There is no added value of evaluating those two algorithms compared to what was already written in Bastrikov et al. (2018). I think this subject is very much on the side of the authors' work. They should focus instead to the heart of their study: albedo assimilation and remove anything related to the gradient-based L-BFGS-B algorithm including appendix A1.

About the validation:

- The authors acknowledge that as MAR being a model this part of the work cannot be considered as validation thus limiting their study to an evolved proof of concept although still better than a twin experiment. Also the comparison is not independent since MAR atmospheric variables are used as inputs of ORCHIDEE. The study therefore shows the difference of modelling approached in MAR and ORCHIDEE (in its various configurations). What is the objective of such comparison in regards with the objectives of the papers? Again, such question occurs because the context of the study is not stated loudly by the authors.

- I do realize it would be hard work to use independent data to validate the authors' approach but the absence of proper validation really weakens the validity of the approach and therefore the paper in its current version. One possibility could be to use data from GRACE about Greenland mass loss as a way to validate your approach by assuming all the mass loss is carried out by melting (in direct link with SMB). While this assumption is probably excessive as it ignores the impact of outlet glaciers, the comparison with independent data coming from GRACE could solve partially my aforementioned reservations about the validation aspects of this paper.

- Another possibility would be to use in situ data from the PROMICE network as mentioned in the conclusion L. 379-380 "One solution would be to run … lead to issues of scale and representativity". In land surface modelling, it is common for example to compare modelled soil moisture at e.g. 0.25° with in situ measurements, see e.g. Kumar et al. (2019) or Albergel et al. (2020). While there are issues of scale, those comparisons are still by far important and very useful. I do not understand why it could not be done in the authors' context.

  References:

  - Albergel, C., Zheng, Y., Bonan, B., Dutra, E., Rodríguez-Fernández, N., Munier, S., Draper, C., de Rosnay, P., Muñoz-Sabater, J., Balsamo, G., Fairbairn, D., Meurey, C., and Calvet, J.-C.: Data assimilation for continuous global assessment of severe conditions over terrestrial surfaces, Hydrol. Earth Syst. Sci., 24, 4291–4316, https://doi.org/10.5194/hess-24-4291-2020, 2020.

  - Kumar, S. V., Mocko, D. M., Wang, S., Peters-Lidard, C. D., and Borak, J.: Assimilation of remotely sensed Leaf Area Index into the Noah-MP land surface model: Impacts on water and carbon fluxes and states over the Continental U.S., J. Hydrometeorol., https://doi.org/10.1175/JHM-D-18-0237.1, 2019.

- If no independent data are used, then the part of comparing ORCHIDEE and MAR should be strengthen by highlighting the differences in term of modelling for both approaches (with appropriate bibliographical literature) and this comparison should be strongly linked with the Morris sensitivity analysis and with the background context of the study that is definitely missing. Also for SMB and runoff, the difference between MAR and ORCHIDEE clearly occur at the edges of the GrIS (either with standard, tuned or optimised parameters for ORCHIDEE). The parameter optimization for albedo does not make ORCHIDEE closer to MAR (quite the contrary). The previously performed Morris sensitivity analysis could help to understand the mechanisms behind those increased differences and would make a nice discussion section.

MINOR COMMENTS AND TYPOS

L. 8 *"This improvement is consistent for all years, even those not used in the calibration step"*. Could the authors rephrase the sentence as I would expect such result otherwise the methodology would not work?

L. 14-16 *"Increased warming … algae growth (Cook et al., 2020)"*. Darkening of GrIS has already observed and expect to worsen and increased impact on GrIS melting. Rephrase accordingly. Also, several missing references, among others:

- Dumont, M., Brun, E., Picard, G., Michou, M., Libois, Q., Petit, J.-R., Geyer, M., Morin, S. and Josse, B.: Contribution of light-absorbing impurities in snow to Greenland's darkening since 2009, Nature Geosci., 7, 509-512, 2014.

- Williamson, C. J., Cook, J., Tedstone, A., Yallop, M., McCutcheon, J., Poniecka, E., Campbell, D., Irvine-Fynn, T., McQuaid, J., Tranter, M., Perkins, R. and Anesio, A.: Algal photophysiology drives darkening and melt of the Greenland Ice Sheet, PNAS, 117(11), 5694-5705, 2020.

- Perini, L., Gostinčar, C., Anesio, A. M., Williamson, C., Tranter, M. and Gunde-Cimerman, N.: Darkening of the Greenland Ice Sheet: Fungal Abundance and Diversity Are Associated With Algal Bloom, Front. Microbiol., 10, https://doi.org/10.3389/fmicb.2019.00557, 2019.

L. 18-19 *"This, in turn, enhances melting, creating feedback to the atmosphere"*. Missing reference to support the statement:

- Le clec'h, S., Charbit, S., Quiquet, A., Fettweis, X., Dumas, C., Kageyama, M., Wyard, C., and Ritz, C.: Assessment of the Greenland ice sheet–atmosphere feedbacks for the next century with a regional atmospheric model coupled to an ice sheet model, The Cryosphere, 13, 373–395, https://doi.org/10.5194/tc-13-373-2019, 2019.

- Box, J. E., Werhlé, A., van As, D., Fausto, R. S., Kjeldsen, K. K., Dachauer, A., Alhstrøm, A. P. and Picard, G.: Greenland Ice Sheet Rainfall, Heat and Albedo Feedback Impacts From the Mid-August 2021 Atmospheric River, Geophys. Res. Lett., https://doi.org/10.1029/2021GL097356, 2022.

L. 22: *"… it is crucial that it is accurately simulated in ~~THE~~ land surface models (LSMs) …"*

L. 39-40: *"Examples of DA used for parameter estimation … in snow modelling are less common. Bonan et al. (2014) … "*. The reference is not about snow modelling but on ice sheet initialization and DA or inverse modelling is well known in this field, see the following paragraph in Bonan et al. (2014) "MacAyeal (1992) and MacAyeal (1993) introduced control methods to infer basal drag in ice-stream models, using in particular the self-adjoint property of such models, leading to many application papers (Rommelaere and MacAyeal, 1997; Vieli and Payne, 2003), and later for full Stokes models (Morlighem et al., 2010; Jay-Allemand et al., 2011). Later on, many DA and inverse methods were introduced in glaciology. The Best Linear Unbiased Estimation (BLUE) and Optimal Interpolation (OI) methods were introduced by Arthern (2003) and Berliner et al. (2008). The Robin inverse method due to Chaabane and Jaoua (1999) has been introduced by Arthern and Gudmundsson (2010) for ice sheet models, and finally Heimbach and Bugnion (2009) presented the first adjoint ice sheet model derived automatically." Most previous references focuses on estimating basal friction or basal velocities as parameters. Regarding parameter estimation for ice sheet mass balance, you can see:

- Bonan, B., Nodet. M., Ozenda, O. and Ritz, C.: Data assimilation in glaciology, in Advanced Data Assimilation for Geosciences, Lecture Notes of the Les Houches School of Physics: Special Issue June 2012 (Edited by Blayo, E., Bocquet, M., Cosme, E. and Cugliandolo, L. M.), 577-584, Oxford University Press, 2014.

L. 48-49: "*However, with large amounts of data, … the multisite approach is common*": Debatable statement. One of the main reason of the multisite approach is commonly used is that the set of optimal parameters or parametrizations for various sites in LSMs can differ significantly due to soil properties (soil texture, water potential, hydraulic conductivity …) and land cover (vegetation variables). Please soften the previous sentence accordingly.

L. 64: authors indicate that CMIP 6 version of ORCHIDEE is used in this paper: add reference publication(s) for this version in addition to the historic paper of Krinner et al. (2005). Also authors mention in the code availability section that ORCHIDEE vAR6 is employed. Could the authors harmonize notations between both paragraphs?

L. 73-74 "*we computed the mean of albedo in both visible (VIS) and near-infrared (NIR) spectral domains*". Please indicate that this is to be in accordance with MODIS data. Also the description of albedo following this sentence does not distinguish VIS and NIR spectral domains. I do not think spectral domains intervene in the computation of modelled albedo. Rephrase sentence L. 73-74 in order to state that your model does not distinguish VIS and NIR albedo.

Section 2.2: Could the authors cite the paper(s) associated with MAR Version 3.11.4? Gallée and Schayes (1994) is rather outdated for this version.

Section 2.3: Please mention that this dataset do not include data from the Aqua satellite as explained in Box et al. (2017). People familiar with MODIS datasets tend to expect data coming from both Terra and Aqua satellites.

L. 106-108 *"Finally, in this dataset, … in the April values"*. This statement is rather confusing, please rephrase.

L. 114-120 *"Bayesian statistical formalism (Tarantola, 2005)"*. The formulation of the cost function can also be seen as an optimal control problem without any assumption on probabilistic distributions. This has given the basis of 3D and 4D-Var approach, see for example Nichols (2010). Could the authors rephrase L.114 to L.120 to play down the emphasis on the Bayesian statistical formalism?

- Reference: Nichols, N. K.: Mathematical concepts of data assimilation, in: Data assimilation: making sense of observations, edited by: Lahoz, W., Khattatov, B. and Menard, R., Springer-Verlag, Berlin, Germany, 13–40, 2010

L. 163 *"they correspond to ablation areas"* Most parts of Greenland nowadays experience ablation during summer. Edges are where strong ablation occurs. Please rephrase accordingly.

L. 168 *"… into ~~THE~~ ORCHIDEE, where … "*

L. 174-176 *"They were also the pixels with the largest errors when compared … with RMSD greater than 0.1"*. When this calculation is performed? Before calibration or after? Can the author explain where does this number comes from? It can be simply done by referring to a subsequent section of the paper.

L. 181 *"… to allow ~~THE~~ snow to accumulate …"*

L. 209 *"Bayesian framework"* see previous comment on the Bayesian term

About Figure 2: *"currently operational ORCHIDEE version"* By currently operational ORCHIDEE version, did the author mean ORCHIDEE with parameters set at default values (as in Table B1)? To my knowledge, the way albedo is modelled and the new "icy" type cannot be called "operational" yet. Could the authors modify the legend of Figure2 to reflect this point?

L. 223 "… affect by ~~THE~~ albedo parameters …"

L. 297 *"Bayesian framework"* see previous comment on the Bayesian term

L. 302 I think the text should refer to Figure 4a instead of Figure 6a here.

L. 308 Replace *"omega"* by ω and *"beta"* by β.

L. 320 *"… different parameter* setS *on modelling …"*

L. 326-327 *"Compared to MAR, the manually … ORCHIDEE performs best at simulating SMB"*. Best performance does not really make sense in the context of comparing two models. This sentence and the rest of the section should be rewritten keeping this fact in mind.

L. 347 *"Bayesian optimisation"* see previous comment on the Bayesian term

L. 347-348 *"However, we overfitted to albedo with no other data"*. This statement raises the question of prescribed observation error variances. Would other prescribed values have made the impact of optimized parameters for ORCHIDEE more in line with MAR? Or is this question related to modelling differences? Could the authors reflect on that question in the paragraph?

L 360-361 "*When we cannot further improve … this can point to structural deficiencies in the model"*. I tend to disagree with this statement. Parameter estimations can sometimes hide structural model deficiencies, i.e. you may obtain the right results but for the wrong reason. Could the authors weight on that comment?

L. 361-362 "*For example, we cannot capture the different albedos in the north and the south of the ice sheet with the current processes represented*" This problem might also come from that the author assume the same set of parameters for the whole Greenland. A multisite approach may have reduce this problem (perhaps for the wrong reasons). Could the authors reflect on that question?

L. 375 "*There is an urgent need for data producers to provide this uncertainty, ideally at each time step*". I could not agree more. The authors can mention this following reference to strengthen their statement:

- Merchant, C. J., Paul, F., Popp, T., Ablain, M., Bontemps, S., Defourny, P., Hollmann, R., Lavergne, T., Laeng, A., de Leeuw, G., Mittaz, J., Poulsen, C., Povey, A. C., Reuter, M., Sathyendranath, S., Sandven, S., Sofieva, V. F., and Wagner, W.: Uncertainty information in climate data records from Earth observation, Earth Syst. Sci. Data, 9, 511–527, https://doi.org/10.5194/essd-9-511-2017, 2017.

L. 379-380 *"One solution would be to run … lead to issues of scale and ~~representativity~~* REPRESENTATIVENESS".

L. 395 Rephrase the sentence to replace *"better"* by "more consistent with MAR outputs"

L. 398-404 I have some reservations with the statements written in the paragraph. Would the idea behind using all these satellite datasets be to replace the modelling of the Greenland ice sheet ice dynamics? Could the authors temper those statements?