The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-175-RC1, 2020

The Cryosphere
Discussions

Interactive comment on "Physics-based modeling of Antarctic snow and firn density" *by* Eric Keenan et al.

Charles Amory (Referee)

charles.amory@uliege.be

Received and published: 31 July 2020

General comments

The paper presents an evaluation of the 1D model SNOWPACK equipped with a drifting-snow compaction routine against a large set of observed density profiles and 10 m depth temperatures scattered over Antarctica. I really enjoyed reading the paper which is nicely formulated and timely as the role of drifting-snow compaction is a currently lacking process in most of snow transport models that parameterize surface density following semi-empirical formulations designed to reproduce observed density already resulting from post-depositional processes. This is moreover an important topic for estimations of ice-sheet contribution to sea level rise since density is needed

for converting altimetry-derived ice volumes to mass. I think however that the paper could benefit from substantial improvements before publication.

Major comments

Important model information is generally lacking notably about initialization conditions, number of snow/ice layers, vertical discretization, treatment of concurrent deposition of snowfall during drifting snow, layer aggregation, and more generally on the actual influence of the inclusion of the drifting-snow compaction process on the model density products. The evaluation of forcing wind speeds taken from MERRA-2 reanalysis, which is the driving force behind drifting snow and related compaction, could be more exhaustive and adapted to more relevant characteristic time scales of the process studied. Drifting-snow compaction is only able to densifies snow at the surface (« 1m, which is by the way a coarse definition of the real surface layer affected by drifting snow in natural environments), so the only process by which drift-induced density anomalies can be buried is by accumulation. It's not clear to me what is the added-value of including drifting-snow compaction in the representation of density profiles. In many places the paper focuses on discussing the representation of density at depths at which drifting-snow compaction is not active, so the contribution of overburden pressure alone to the good agreement with observations and more generally to density profiles cannot be assessed. This could however be easily done by running the model without the drifting-snow compaction routine, as suggested in my comments. By doing so, the interesting question of "To which depth drifting-snow compaction impacts density profiles?" could be answered and help to shape our understanding on that poorly documented process while improving the scientific significance of the paper.

Minor comments

- L25, "drifting snow": Many authors have referred to drifting (or blowing) snow for describing different processes (saltation, combined or not with suspension, wind-driven snow transport > 2 m and/or < 2 m, local erosion combined or not with horizontal advection, etc..) leading to a potential confusion of the actual meaning of this term when not properly defined. Could you describe which specific(s) process(es) you refer to?

- L26, “wind-driven compaction”: please elaborate a bit on the physical mechanisms behind drifting-snow compaction (mainly through rounding and fragmentation) as it is a key element of the paper.

- L51: I don’t understand where in the paper SNOWPACK is applied to the 9 AWS locations, and what would be the objective of doing so if only meteorological observations are available there.

- L69, “both ice sheets”: I suggest to add “both the Greenland and Antarctic ice sheets”.

- L70-71: the SEB is not a process. Could you please reformulate?

- L92: Have you investigated the sensitivity of your results to the choice of the roughness length or the neutral atmosphere assumption? 2 mm is a rough value that do not necessarily fit with observations all over the AIS (see for instance Amory et al., 2017; Vignon et al., 2016), and the Antarctic ABL is more generally statically stable in the ice-sheet interior, requiring the use of stability correction functions which can be a significant source of uncertainty in the computation of u* (Vignon et al., 2016).

- L99: If Q is a saltation mass transport rate, then phi is simply a saltation (drifting snow) mass flux.

- Fig. 1, and elsewhere in the paper : “Drifting snow erosion” and “Drifting snow redeposition” sound like a pleonasm, as drifting snow is the very process by which surface snow is eroded and redeposited. Consider simply using erosion and redeposition.

- L102, “are distributed before erosion”: Distribution involves erosion. Not sure what do you mean here.

- L107: Why just not saying here that phi and Q only accounts for saltation? Due to

the one-dimensional approach, a missing aspect is horizontal advection of snow which can significantly contribute to the local saltation mass flux. Even though the objective of the paper is not to parameterize explicitly (tri-dimensional effects of) snow transport, this could lead to overestimation of drifting-snow compaction if all the saltation mass flux is attributed to local erosion.

- L109: “fresh surface snow” can be confusing since this equation has been developed to account for deposition of snow that has been transported by the wind only (see Groot Zwaaftink et al. 2013, p337), while “fresh snow” could refer to snow originating from clouds and that has reached the ground for the first time. It is maybe preferable to stay in line with the semantics of Groot Zwaaftink et al. (2013) and just remove “fresh”.

- L112-113: Here the assumption is made that all the eroded snow is redeposited. How are the internal layers of the snowpack affected by deposition of new snow layers of different densities? How is the mixing with snowfall treated to compute the density of the surface layer in case of concurrent snowfall?

- L119-121: I don’t understand why the release latency and update should constitute an argument since you focus on a past period (1980-2017) over which RCM outputs are already available (see for instance van Wessem et al., 2018, Agosta et al., 2019).

- L122-124: The good agreement reported by Gossart et al. (2019) is demonstrated from mean values of mean values and thus remains valid for discussing the mean climatology of Antarctica, But that reference could hardly be used to demonstrate the ability of MERRA-2 to represent climate variables at the 6-hourly time scale or at least at the characteristic time scale over which ephemeral processes such as drifting-snow compaction is active.

- L125: Could you justify why do you prefer the monthly scale when evaluation at the daily scale, or even less (SNOWPACK is forced at hourly intervals), could be similarly performed to better highlight MERRA-2 ability at reproducing the meteorology, moreover required as input in Eq. (1)? Strong wind events, during which most of drifting-snow compaction occurs, are completely smoothed out at the monthly scale.

- L130: The performance of MERRA-2 at reproducing the Antarctic near-surface meteorology (i.e., « month) is still poorly known. While this is certainly the subject of another study and lies beyond the scope of the paper, still you have all the materials required to do it, and this could be a real added value to your work while reinforcing the evaluation. This is also, again, more consistent with the time scale of drifting-snow compaction. At least could you give more statistics, i.e. RMSE and r2, which are better indicators (when combined together) than just a mean bias, to support your assertion. Moreover, I get that these 9 AWSs are not assimilated in MERRA-2 so they are all good and independent evaluation products. But why so few AWSs when many others (>200, see Mottram et al., 2020) are available through other public sources and not necessarily assimilated in MERRA-2 (see https://gmao.gsfc.nasa.gov/pubs/docs/McCarty885.pdf)? Antarctic is vast and diverse. Most of the AWSs used here are located in DML and is a rather small sample of the climate conditions encountered across the continent. Could you add more AWSs to your analysis, or at least discuss the representativeness of these locations regarding the Antarctic climate conditions, also given that the evaluation using boreholes T and density profiles is mostly done at locations significantly away from the AWSs?

- L133: Do you rather mean -15.1 W/2, so the applied correction correspond to the mean bias as done for ILWR? If not, where does this value come from?

- L139: Important information are missing here, such as the initialization, the number of ice/snow layers, the vertical resolution of SNOWPACK and aggregation of new snow layers. You must elaborate on this.

- L171: Defining the surface as the 1st meter is quite coarse regarding the actual thickness of the layer affected by surface post-depositional processes. For instance, Groot Zwaaftink et al. (2013) consider the first 10 cm. Specifying the timing at which you compare model with observations can be of significant importance for these surface

layers (« 1m) depending on the recent occurrence of melt, snowfall and drifting-snow events, more importantly given that the interest of implementing a drifting-snow compaction routine partly relies on improving representation of density at the surface at the time of drifting-snow events. I would expect more details about the comparison methodology. Do you compare observed profiles with mean modelled profiles ? for which period ?

- L189, "almost perfect" : Sounds a bit too emphatic. I would advise to remain neutral when describing your results. A bias value alone, even low, is not a self-consistent argument to speak of almost perfect agreement when RMSE still amounts to 2.36 ° C (indicating individual bias values of several degrees in some locations).

- L199: Did you follow the same spin-up procedure for your sensitivity analysis?

- L200: Why did you choose these specific locations? Please justify and give coordinates.

- L211-212: This is another strong argument for exploring the sensitivity of density to the derivation method for u* (z0, stability correction function) as well as for evaluating MERRA-2 wind speeds at shorter intervals.

- L214: This may be because wind maxima which control drifting-snow occurrence and thus drifting-snow compaction, are smoothed out at such a low (monthly) temporal resolution. Does this stay true if you perform a statistical evaluation of wind speed at higher temporal resolution?

- L215-216: Again, then knowing the sensitivity of your results to the derivation method of u* would be particularly interesting.

- L227-229: I couldn't agree more. This is another argument in favor of an evaluation of wind speed at higher temporal resolution. Another explanation to this result might be that drifting-snow compaction is mainly active over layers thinner than 1 m. What would the correlation become by decreasing the size of the surface layer consider here while working over shorter time scales?

- L280-281: I'm wondering to which depth drift-induced compaction exerts an influence on the mean density profile, given the fact that only the surface layer receives momentum from the atmosphere and is likely affected by drifting snow. This is a very interesting question, I don't have the answer and your work is among the first to focus on this aspect. But you could give an element of response by running the model without the drift compaction routine and see how it affects density profiles (and which layers are most impacted) according to SNOWPACK pre-existing physics by comparing it with the run including drifting-snow compaction.

- L301: No new results in this section, but these are good elements of discussion. This should be entirely part of a Discussion section, or mixed with the Conclusion re-entitled Discussion and Conclusion.

- L317: "new snow density": specify if new=deposited (snowfall) or redeposited (drifting snow). Maybe consider staying in line with Groot Zwaaftink et al. 2013 in which "new snow" is defined as redeposited.

- L331: This section is out of the scope, of limited interest and with no scientific results. Everything here could be moved to the conclusion and resumed in one sentence informing on the availability of SNOWPACK products for other possible applications.

- L350-352: You need a comparison between runs with and without the compaction routine to clearly highlight improvements and state that drifting-snow compaction is the process behind this. Besides, it would be a very interesting results that would strengthen the scientific contribution of the paper.

Technical corrections

- L5, "wind-driven, drifting snow": either wind-driven or drifting snow, but a combination of both is redundant.

- L179, "absent of very rare": Please add a reference.

- L224, "perhaps surprisingly": Avoiding subjective wording is strongly recommended.

- L263: remove "at depth".

- L273, "off": on?

- L325: correct "comapred".

- L336,"valide": "evaluate" is more appropriate.

Agosta, C., Amory, C., Kittel, C., Orsi, A., Favier, V., Gallée, H., van den Broeke, M. R., Lenaerts, J. T. M., vanWessem, J. M., van de Berg, W. J., and Fettweis, X.: Estimation of the Antarctic surface mass balance using the regional climate model MAR (1979–2015) and identification of dominant processes, The Cryosphere, 13, 281–296, https://doi.org/10.5194/tc-13-281-2019, 2019.

Amory, C., Gallée, H., Naaim-Bouvet, F., Favier, V., Vignon, E., Picard, G., Trouvilliez, A., Piard, L., Genthon, C., and Bellot, H.: Seasonal Variations in Drag Coefficient over a Sastrugi- Covered Snowfield in Coastal East Antarctica, Bound.-Lay. Meteorol., 164, 107–133, https://doi.org/10.1007/s10546-017-0242-5, 2017.

Gossart, A., Helsen, S., Lenaerts, J. T. M., Broucke, S. V., van Lipzig, N. P. M., and Souverijns, N.: An Evaluation of Surface Climatology in State-of-the-Art Reanalyses over the Antarctic Ice Sheet, Journal of Climate, 32, 6899–6915, https://doi.org/10.1175/JCLI-D-19-0030.1, 2019.

Groot Zwaaftink, C. D., Cagnati, A., Crepaz, A., Fierz, C., Macelloni, G., Valt, M., and Lehning, M.: Event-driven deposition of snow on the Antarctic Plateau: analyzing field measurements with SNOWPACK, The Cryosphere, 7, 333–347, https://www.the-cryosphere.net/7/333/2013/, 2013.

Vignon, E., Genthon, C., Barral, H., Amory, C., Picard, G., Gallée, H., Casasanta, G., and Argentini, S.: Momentum and heat-flux parametrization at Dome C, Antarctica: a sensitivity study, Bound.-Lay. Meteorol., 162, 341–367, https://doi.org/10.1007/s10546-016-0192-3, 2016.

van Wessem, J. M., van de Berg, W. J., Noël, B. P. Y., van Meijgaard, E., Amory, C., Birnbaum, G., Jakobs, C. L., KruÌĹger, K., Lenaerts, J. T. M., Lhermitte, S., Ligtenberg, S. R. M., Medley, B., Reijmer, C. H., van Tricht, K., Trusel, L. D., van Ulft, L. H., Wouters, B., Wuite, J., and van den Broeke, M. R.: Modelling the climate and surface mass balance of polar ice sheets using RACMO2 –Part 2: Antarctica (1979–2016), The Cryosphere, 12, 1479–1498, https://doi.org/10.5194/tc-12-1479-2018, 2018.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-175, 2020.