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The Cryosphere
Discussions

Interactive comment on "Processes influencing near-surface heat transfer in Greenland's ablation zone" *by* Benjamin H. Hills et al.

Anonymous Referee #1

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Review Hills et al. 2018: Processes influencing near-surface heat transfer in Greenland's ablation zone, MS No.: tc-2018-51

The authors study the near-surface thermal regime of ice in the ablation zone of Greenland (GrIS) based on observations and simulations of ice temperatures. Observations essentially demonstrate that ice temperatures at ca. 20m depth (T0) are systematically higher than mean air temperatures. This corroborates earlier observations and the related hypothesis that the thermal regime in the ablation area of ice sheets is significantly influenced by other processes than heat conduction. Such is reconsidered in model experiments addressing the relative role of seasonal changes in heat content due to heat diffusion, vertical advection due to ablation, insulation by snow cover, radiation absorption and temperature gradients in the deeper layers. Progressive model

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sensitivity studies reveal that the strongest effect is related to modifications of the bottom boundary conditions, while the other (near-surface) effects tend to cancel each other. The final simulation yields T0 being significantly colder than observed ice and air temperatures. It remains somehow unclear whether this is due to principal shortcomings of the semi-idealized simulatins or having not treated process having another strong impact on the thermal regime. Some interesting observational evidence is presented indicating that episodic refreezing at depth can play a role in this context, but these processes are not treated in the simulations. The reader is left with the rather basic conclusion that in the ablation zone of ice sheets, the mean annual air temperature can not straightforwardly be used to predict the near-surface ice temperature (nor vice-versa).

General remarks: The underlying research topic is discussed since long and is still relevant in various contexts, in particular regarding the mass and energy budget of GrIS and other ice sheets and their response to regional climate changes. Experimentally guided modeling is methodically appropriate to investigate that. The demonstrated observational background is valuable and based on sound experimental methods. Presentation of measured ice temperature records and their interpretation need some reconsideration concerning coherent time and depth referencing. The meteorological data are adequate but fall a bit short due to the lack of direct measurements of solar insolation and surface temperature, both being considered as primary drivers for subsurface temperature developments. On the other hand the existing data could have been better exploited for the purpose of this study. Specifically, just one site is considered finally, while the obviously present spatial variability is poorly addressed. Supplementary information is relevant, but is not used quatitatively because the associated processes are not treated in the simulations. The 1d, enthalpy-based model approach is appropriate, but description of some important details may be improved. Specifically, this concerns the grid setup (e.g. resolution) and numerics (discretisation, solving method, initialisation, parameterization of processes related to radiation absorption and water transport incl. refreeze and runoff). The chosen simulation strategy is understood as semi-idealized sensitivity studies restricted to the mentioned five processes and is principally fine. Unfortunately, however, the lacking validation of the model results leaves a rather large gap between simulations and observations, which also hampers interpretations. Judgement of the robustness of the simulation results is largely impossible and could have been supported by a few additional runs considering diverse uncertainties (of e.g. input and parameterizations) and quantitave validation with observations (which contrasting to the author opinion must not necessarily result in inapproriate tuning). New and potentially important issues could thereby have been addressed with reasonable effort as well. Such might consider the hypothesized impact of e.g. the observed transient refreezing events or of the strong temperature inversions which govern near-surface exchange processes in the atmosphere above ice sheets. The latter aspect poses the question whether the model should not be forced by a measure of surface temperature instead of air temperature. The anticipated limitations of 1d modelling are not at all discussed. The manuscript is well structured and written, figures are mostly appropriate, exept of rather inconsistent treatment of time and depths. Achievement of an overview on backgrounds would benefit from compilation of an additional table compiling meteorological and ice temperature data. Major revision concerning the above mentioned aspects is recommended.

Specific comments: P1L21: Statement "...the five heat transfer mechanisms presented here..." may be put in actual perspective that just 4 processes are quantitatively treated P2L6: "high" refers to elevation or amount of melt? P2L11: Mention importance for interpretation of ice-cores or modelling ice flow P2L14: "van Everdingen, 1998" is rather unreproducible and incompletly given reference P2L17: The metric "depth of zero amplitude" needs to be reconsidered ("zero" in true sense is rather meaningless, e.g. Hooke 1976 refers to 1% of surface amplitude, the definition T0@-20m is rather subjective and questionable in view of the model setup (identical to bottom of the domain)). In this context, please also mention that >10m temperatures principally reflect the thermal conditions during the previous year, and in case of substantial refreeze from even before (all being attenuated though). P2L21: "forcing is constant"

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probably means “periodic”? P3L3: Neither insulation by winter snow, nor radiation absorption or refreezing are processes unique to ablation areas P3L12: Check spelling of “Isunnguata Sermia” P3L27: Add more details about thermistor string measurements e.g. field accuracy not only concerns calibration but also depth referencing, uppermost sensors may be affected by solar heating, cables running through strong temperature gradients may affect signals through heat conduction, how was long-term drift determined (guessing that sensors were not excavated after the 3yr measurement period?) P3L28: neither HMP60 nor NR-Lite are Campbell Sci. products, please provide some more details about uncertainties of meteorological measurements i.e. comment e.g. radiation shielding of air termperature sensor (which depending on specific device used can be substantially affected), in what extent were radiation measurements reliabale concerning horizontal alignment, rime or snow? P3L32: Please add information how the notoriously noisy SR50 signal was processes in order to derive mentioned data (filtering, independent check of accumualted amount through e.g. stakes?) P4L13: Please again clarify criteria for T0 (allowed range, at what depth, which might be site dependend?) P4L14: It is not clear whether given figures are truly comparable. Please clarify period to which they refer (mean annual air temperature is clear, but according to Fig. 2 the ice temperature records have different length, is T0 calculated for same periods each?) P4L15: Temperature gradients are calculated for what depth range? P4L21: “…strings failed…”: this again points towards an inconsistent treatment of data. Question is again in what extent it makes sense to compare records of different length. Tentatively, such inconsistencies also can explain why the average profiles Fig. 2 show different gradients in the upper 10m, which despite of the partly different locations might not be expected if data refer to same period of time. Still regarding Fig. 2, please also check how in the 33km-subplotsthe max. ice temperatures can be in excess of 0°C. Please also comment this in view of uncertainties (accuracy) of measurements. Fig. 2 shall be updated accordingly and some interpertations may be adjusted then. Further a new table may be added compiling an overview on atmopsheric and subsurface conditions for the same period of time (may be simulation period).

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That may be extendend by other meteorological data (humidity, wind, radiation, snow). P4L33:" …T-14, transient heating events were observed… ", which refers to Fig. 3 at depth ca. 10m. Fig. 2 (lowest panel, left) also shows data from this site. One here may note the exceptionally high data (close to 0°C) at depths 13-17m, which presumably reflects the (really strong) impact of another transient heating event. Question is, why this one does not trace in Fig. 3 (as according to the annotations the entire measurement period is considered in both figures). Another question refering to Fig. 3 in this context is, how one could understand that the melt layer persists throughout the whole winter 2017, which according to the meteorological data was not exceptionally warm. Humphrey et al. 2012 put forward some interesting ideas in this context, which may be reconsidered regarding the ablation zon of the ice sheet. P5L5: NR-Lite does not measure net shortwave radiation (which btw. shall never be negative) P5L10: Numbers should be given with respect to a common period of data (see comment above) P5L13: "…measure almost no winter snowpack at sites 27-km and 46-km…": how can such be understood, as surface slope is rather flat and uniform (so wind drift may not make the difference), neither elevation nor horizontal distances between stations are large enough to explain such strong precipitation gradients. Potential measurement problems (SR50, see above) shall be explored or previous work be checked (e.g. data from GIMEX-91 or PARCA or diverse model output). P5L19: "…IMAU s6, the second year is more typical for this area…" How can this be judged from Fausto et al 2009 ? P5L30: " …simulates ice temperature to 20 m, a depth chosen for consistency with measured data" and "T0 , is output from the bottom of the domain for each model experiment…": Constraining the model domain that way may introduce artifical effects to the T0 simulation results.. A study shall be performed to investigate in what extent simulated magnitude and depth of T0 depends on the size (depth) of the domain. The outcome may be considered in context of redefinition of T0 (see respective comments above). P5L32:" model does not, nor is it meant to, simulate the surface mass balance." Doesn't that contradict equ. 4 (temperate) and statement at P6L24? The relation between vertical velocity and omega(H) needs to be clarified (independent or

how coupled?). P6L2: Please specify grid (resolution, constant or higher resolved near the surface and the bottom, which would be reasonable?) P6L15: Equ. 1 needs some clarification resp. “temperate ice diffusivity” . Understandig that melt is treated via w and radiation absorption in the last term of equ.1, what is the nature of temperature ice diffusivity then (latent heat flux in sense of evaporation/sublimation)? If so, please justify assumption that this is order of magnitude smaller than cold ice diffusivity and what is relation to mentioned impermeability of near surface ice. Please check units in Tab. 2 given for moisture diffusivity (shall be m2/s?), and add values for diffusivity for ice and snow (instead of conductivity and Cp) P7L2: “. . .limit water content”: what happens with excess water? Treatment of water transport in the model (also in snow and with respect to refreezing (super-imposed ice?) generally needs to be better described
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P7L3 and L5: “fixed to the air temperature at the surface, “ this is a questionable assumption, because it neglects the existence of the quasi-persistent inversion conditions above ice sheets and glaciers and associated surface exchange processes. Surface temperatures are significantly colder than air temperature which is not accounted in the simulations presented here. The potential impact (uncertainty) may be addressed by introducing a correspondingly changed upper boundary condition (representative figures for the difference between surface and air temperature may be retrieved from on of the various GIMEX-91/K-transect papers). A more general question is whether in an enthalpy based model also the boundary conditions need to specified in terms of enthalypy. P7L22 and Fig.6 : Treatment of depths needs clarification also in this context. In case of ablation the grid will be reduced by ca. 3m. In Fig. 6 however, this can not be seen (all profiles still expand to 20m). Seemingly profiles are plotted upon each other without taking account for this. The effect increases for experiment with accumulation, when surface changes are even larger (ca. 5m in total). The issue also has implications on intercomparison of profiles from single simulations or different experiments and the determination of depth with “zero” termperature change, finally. Regarding the latter, however, it is dsuggested to use some other criterium like 1% of surface amplitude.

In Fig. 6a the six profiles may be colour coded according to months (presumably), as the evolution of temperature profiles is not shown elsewhere. P8L7: Please add value for ks (rho=300kgm-3) in Tab. 2 and for diffusivities, respectively. Please clarify also whether for the near-surface nodes (representing rotten ice) another value is used. P8L12: Please clarify the treatment of radiation absorption for snow (extinction coefficient, bulk approach)? P8L19 and L21: The discussed absorption coefficients are valid for shortwave radiation, while in this study treatment of radiation absorption is based on measured net radiation. It may also be considered in this context that the intensity of solar incoming radiation (mostly counting for absorption within snow and ice) is smaller than measured net radiation. As solar incoming radiation and net radiation are strongly correlated the impact of using net radiaion in the presented simulations may be tested by e.g. applying a correction factor derived from literature references (GIMEX again). P8L25: with respect "Neumann instead of Dirichlet BC" the sensitivity of simulation results (of T0 essentially) on different size of the model domain (e.g. 50m instead of 20m) shall be tested by dedicated model runs. Similar regarding different magnitude of the prescribed gradient according to the observation T-15s), which is alos valuable in oder to judge the spatial variabilty and iherent impacts on model results and interpretations. P9L5: " Because the air can exceed the melting temperature in the summer while the ice cannot". Also in this context, using surface temperature instead of air temperature to drive the model would be most interesting (see comments above). P9L20: The given numbers for T0 may be reconsidered in light of above comments (criteria for allowed variability) P9L26: Simulations are also not able to reproduce the observed rel. max. of ice temperature at ca. 10m depth. May be this is due to inconsistent periods of time (as argued above). Fig. 7 is difficult to understand, needs major clarification and revison. Questions are: how can model output yield data below -20m? What is the purpose of comparing simulation results covering one year with average profiles compiled from observational records with different lenghts and gaps? Presumably the right-most red curve refers to T16. How can this extend to -20m while according to Fig. 2 the record stops at -10m? Information about air temperature during years which are not considered in the simulation is not useful either, in particular as long as not refering to the same July-July period (as is for the simulations). P10L7: In view of the fact that specification of the bottom BC has the strongest impact on the results the according sensitivity to observed variability should be quantified. This has not at all to do with an inappropriate tuning exercise rather helps to constrain the respective reliability of the simulation results. P10L25: “than in other areas” P10L26: “the near-surface active layer in the ablation zone is small “ .. replace small by shallow P10L29: Please clarify meaning “melting dynamics are complicated by the 20-m temperature gradient” also referring to sentence before (surface processes have weaker control) P11L6: Please add other references to this recently focussed issue (e.g. Renneralm 2013, Steger 2017, Smith 2017 or Andrews et al 2014, Nature 514). Humphrey et al. 2012 appears most interesting in the overall context of the paper (although referring to firm area) and allows some aspects to be put in larger context (e.g. observations of Tair-To at elevatons below the ELA) P11L2: Please here also consider earlier comments (P4L33). In this context, too, please check and comment the argue that the event shown in Fig. 4a (depth 7m) corresponds to the one shown in Fig.2 (lowest panel left, depth 15m) P11L33: Also consider whether water bodies can be advected from higher elevations ? The results may generally be put in better context to related investigations in the accumulation area (e.g. Humphrey et al 2012) Table 1: please elevation of sites Table 2: H is not a constant. Please consider adding a new Table (climatology of average temperatures of air and ice, plus other meteo parameters). Data should be based on same period of time i.e., simulation period preferably)

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