Improving the CLASSIC (v1.8) Snow Model to Better Simulate Arctic Snowpacks

https://doi.org/10.5194/egusphere-2026-492

(Lalande et al., 2026)

Response to RC2

We thank Reviewer 2 for their careful and constructive assessment of the manuscript. We acknowledge the fundamental limitations of a single-layer snow scheme and the partly empirical nature of the proposed bulk compaction parameterization. As suggested by the Reviewer, we have made these points clearer in the revised manuscript and implemented the proposed corrections to the figures and text.

Below are our detailed responses to Reviewer 2. The original comments are shown in *italic black font*, and our responses are in blue. Line numbers refer to the originally submitted versions of the manuscript (egusphere-2026-492.pdf) and the Supplement (egusphere-2026-492-supplement.pdf).

Before addressing the reviewer's comments, we note that the revised manuscript also includes several author-initiated corrections and changes made in response to Reviewer 1, which are described in detail in our response to RC1. In particular, we corrected an inconsistency in the normalization of the individual-site snow depth and SWE markers in Figs. 6, 7, C2, and C3. This correction does not affect the overall scores or the main conclusions of the study.

Reviewer's comments

Lalande et al. investigate adjustments of the CLASSIC land surface model for simulation of snow. They note the strongly layered structure of Arctic snowpacks. CLASSIC is now fairly unusual in using a one-layer snow model; although this is not important for some applications, it means that CLASSIC cannot have an accurate representation of both bulk snow density and bulk thermal conductivity in a strongly stratified snowpack with thermal conductivity represented by a nonlinear function of density. This is mentioned in 4.3, but it should already be acknowledged in the introduction. The references (Essery et al. 2013, Augas et al. 2020 and Cristea et al. 2022) cited in the introduction in support of the statement that "Whether multiple snow layers are needed … remains an open question" all use a minimum of two or three layers, not one.

We agree that deriving thermal conductivity from bulk snow density represents an important limitation of the CLASSIC single-layer snow model and should therefore be acknowledged in the Introduction. We have also revised the paragraph discussing the studies cited above to distinguish more clearly between the number of layers required to represent internal snow stratigraphy and the ability of single-layer models to simulate bulk snowpack properties. The two paragraphs in the **Introduction** have been modified as follows (with the main changes highlighted in bold):

> *"Whether multiple snow layers are needed to accurately simulate bulk snowpack properties and the thermal regime of underlying soil in climate models, and if so, how many, remains an open question in the snow modeling community.* ***Evaluations from the Snow Model Intercomparison Project (SnowMIP) have shown no simple relationship between overall model complexity and performance, and some single-layer schemes can perform comparably to substantially more detailed models for bulk snow properties (e.g., Brown et al., 2006; Etchevers et al., 2004; Krinner et al., 2018; Menard et al., 2021). Conversely, controlled sensitivity studies have shown that increasing the number of snow layers can improve the representation of internal mass and energy transfer, snowmelt, and snowpack temperature, although the magnitude of these improvements depends on the model formulation, snow conditions, and target variables (e.g., Essery et al., 2013; Augas et al., 2020; Cristea et al., 2022).*** *[...]."*

> *"[...] CLASSIC includes a one-layer snow model that demonstrates comparable performance to the most detailed snow models in terms of bulk snow properties according to SnowMIP (Etchevers et al., 2004; Krinner et al., 2018; Menard et al., 2021).* ***Although CLASSIC uses a single prognostic snow layer, an assumed quadratic internal temperature profile provides an indirect representation of its vertical thermal structure. Nevertheless, diagnosing snow thermal conductivity directly from bulk density may constitute an intrinsic limitation for strongly stratified snowpacks, such as Arctic snowpacks, because the density–conductivity relationship is nonlinear.*** *[...]"*

Furthermore, to support this point, **we added an idealized two-layer sensitivity example in Sect. 12 of the Supplement** and expanded the corresponding discussion in **Sect. 4.4**, as follows:

> *"[...] Generally, estimating an effective snow thermal conductivity from the mean snow density can be misleading in a single-layer framework. Indeed, the relationship between density and thermal conductivity is nonlinear (e.g., Calonne et al., 2011), and thermal resistances (i.e., layer thickness divided by thermal conductivity)—not conductivities—combine in series in vertically stratified snowpacks. As a result, applying a density-dependent snow thermal conductivity parameterization to the mean snow density cannot reproduce the equivalent conductivity of a multilayer snowpack.* ***An idealized two-layer sensitivity analysis presented in Sect. 12 of the Supplement indicates that the bulk approximation underestimates the integrated thermal resistance by approximately 7, 26, and 48 % for moderate, strong, and very strong stratification (represented by depth-hoar/wind-slab density pairs of 250/350, 200/400, and 150/450 kg m^{-3}, respectively).*** *This limitation may partly explain why the model reproduces observed snow density reasonably well while still exhibiting deviations in simulated snow thermal conductivity. [...]* ***Future developments could also consider diagnosing an internal density profile within the single prognostic snow layer, similarly to the quadratic temperature profile already used in CLASSIC, or implementing a fully multilayer snow scheme.*** *"*

We also replaced this sentence in the Conclusion:

> "*Despite inherent limitations to the CLASSIC snow model (Sect. 4), this study demonstrates the ability of a single-layer snow model to reproduce the bulk characteristics of an Arctic snowpack.*"

by

> "*Despite the inherent limitations of the CLASSIC **single-layer snow model, including its inability to explicitly represent Arctic snow stratigraphy** (Sect. 4), this study shows that it can reproduce **first-order** bulk snowpack **characteristics and the coupled snow–soil thermal response at the evaluated sites**.*"

to be more accurate. And we added in the last paragraph:

> *"[...] **Developing an internal snow-density profile could help reconcile bulk density with effective thermal conductivity within the current single-layer framework. Alternatively, transitioning to a multilayer snow scheme would allow Arctic snow stratigraphy to be represented explicitly, but would require a substantial restructuring of the model architecture, including its snow thermodynamics and mass and energy balance calculations.** [...]"*

Having a threshold wind speed for the initiation of blowing snow that only depends on air temperature (equation 6) is unrealistic but may not be significant.

We agree that the temperature-only threshold wind speed used in Eq. 6 is a simplified representation of the initiation of blowing snow. This formulation is inherited from the empirical parameterization of Gordon et al. (2006) who used the approach of Li & Pomeroy (1997) to derive the threshold wind speed. This approach uses the 2-m temperature as a proxy for the surface cohesion of the snowpack. It does not account for the actual snowpack properties and ignores the history of the snowpack.

We have added a statement acknowledging this limitation in the Discussion in the last paragraph of Sect. 4.4:

> *"[...] In addition, **both the fixed wind threshold used for enhanced compaction and the temperature-only threshold used for blowing-snow sublimation (Eq. 6) could be refined by accounting for additional controls on surface-snow erodibility, such as snow density and microstructural properties** (e.g., Liston et al., 2007; Vionnet et al., 2012; Amory et al., 2021)."*

In the present simulations, the blowing-snow sublimation parameterization has a relatively limited impact, generally reducing snow depth by only 2–4 cm at the most affected sites. Therefore, this simplification is unlikely to drive the main conclusions of the study, which are dominated by the compaction and thermal-conductivity developments. However, its influence could be greater at other sites or in spatial simulations with stronger and more persistent wind exposure; thus, a more process-based threshold should be considered in future work.

The maximum snow density parametrization with a peak around 80 cm snow depth (equation 12) looks like an ad hoc attempt to increase the bulk density of shallow Arctic snow packs, rather than a physical representation of responses to different wind exposure at Arctic and Alpine sites.

We agree that the Gaussian snow-depth dependence in Eq. 12 is empirical and should not be interpreted as a physical representation of wind slab or depth hoar formation. Still, it was introduced to allow the same compaction parameterization to improve shallow, wind-exposed Arctic snowpacks while retaining its applicability at deeper mid-latitude sites. This approach goes beyond previous Arctic adaptations that modified several parameters, including prescribing higher **fixed** values for the maximum snow density, which limit their transferability. Although developed and evaluated here within the CLASSIC single-layer scheme, the proposed wind- and snow-depth-dependent maximum density could also be tested in multilayer models, particularly for wind-affected upper layers. It would not, however, be sufficient by itself to represent the formation of low-density basal depth hoar, which requires additional processes.

We have therefore revised the manuscript to describe Eq. 12 explicitly as a semi-empirical, process-informed parameterization rather than an explicit physical representation of Arctic snow stratigraphy. We added in the Method at the end of the Maximum snow density section:

> *"[...] **This semi-empirical formulation primarily represents wind-induced densification while accounting, at the bulk scale, for the contrasting contributions of low-density basal depth hoar and denser upper wind-slab layers. It is not intended to explicitly represent the physical processes governing the formation and evolution of these layers.**"*

We also added this sentence in the last paragraph of the Conclusion:

> *"[...] **More physically based formulations of wind-slab and depth-hoar development should also be explored.** [...]"*

Several co-authors are currently involved in various projects to explore more physically based formulations for the development of Arctic snowpack stratigraphy for future studies.

The inclusion of both automatic and manual snow depth measurements at all sites and snow cover duration based on snow depth is likely to give this arguably less important variable high weight in the overall score metric (equation 20), but it is commendable that the physics modifications are evaluated without optimization and consideration is given to the transferability of optimized snow compaction.

We acknowledge that including automatic and manual snow-depth measurements, together with snow cover duration, gives snow depth greater weight in the aggregated score. However, snow depth and SWE are key controls on snow insulation and therefore on the simulated soil thermal regime. Giving substantial weight to these variables also helps avoid apparent improvements in soil temperature arising from compensating biases in snow amount. We did not further add bulk snow density to avoid adding more weight to SWE and snow depth, which already have more weight and are directly related to snow density.

Furthermore, as clarified in response to Reviewer 1, the overall score is more used as a diagnostic aid for parameter tuning and is not used blindly for experiment ranking. We also favored physical consistency between the improvements across all variables, as we further strengthen in Sect. 4.1 of the discussion (see our response to Reviewer 1 for more details).

The spin up is a lot of computation to equilibrate carbon pools that are then barely discussed in the paper. To focus on differences in representations of snowpacks, would it not be better to prescribe realistic vegetation heights at each site without CTEM?

Indeed, prescribing site-specific vegetation properties could provide a more controlled framework for isolating snow process developments. However, CTEM was retained because the study was initially designed to evaluate the snow developments within the fully coupled CLASSIC configuration and to explore their potential impacts on Arctic carbon fluxes. The required carbon pool spin-up was not so computationally demanding in the present 1D setup (dozens of minutes for each site on a personal laptop), as it was performed only once per site and reused for all experiments.

The carbon flux analysis was initially intended to receive greater emphasis. However, further model developments were added throughout the project, including all the Sect. 2.3 *Snow model: physics improvements,* which were not initially planned, but we considered them necessary before applying the Arctic adaptations. This required further effort and added length to the manuscript; therefore, we did not want to drown the reader in too much information and decided to place greater emphasis on snow developments while still providing initial insight into carbon fluxes. A more robust carbon-cycle assessment will require a detailed evaluation of Arctic carbon parameterizations, uncertain winter flux observations, and processes not yet represented explicitly in CLASSIC, including moss and lichen carbon dynamics, which will be retained for future studies.

We also tested vegetation sensitivity at several sites. Simulations with vegetation or bare ground produced only negligible differences in snowpack evolution, although vegetation affected albedo at the beginning and end of the snow season and the amplitude of summer soil temperatures. Two examples are provided here for reference:

- Umiujaq: https://github.com/mickaellalande/SnowC2-CLASSIC-1D-analysis/blob/main/CLASSIC-1D/SnowArctic/umt/outputs/run_Ref_nopeat.ipynb
- Col de Porte: https://github.com/mickaellalande/SnowC2/blob/main/CLASSIC/in_situ/SnowMIP/FR-Cdp_test/outputs/run_bg.ipynb

These sensitivity tests suggest that our snow model developments and their main conclusions are unlikely to be substantially affected by the use of the coupled CLASSIC configuration with CTEM enabled.

Minor corrections:

63: *"fail in simulating"* Corrected.

*It is unfortunate that the **Figure 2** legend obscures a value specifically referred to in the text (line 268).*

We have slightly reduced the size of the Fig. 2 legend so that it no longer obscures the value referred to in the text.

302: *None of the samples in Calonne et al. (2011) are Arctic snow, so this might be considered as "physics improvements" rather than "Arctic adaptations".*

We agree that the Calonne et al. (2011) parameterization (CL11) is not Arctic-specific in origin and can be considered a general snow physics improvement. We nevertheless retained it within the Arctic adaptation developments in a practical way, as we needed sufficiently large soil-temperature amplitudes to assess its effects, which occurred mainly at the Arctic sites (whereas soil temperatures at most SnowMIP sites remain close to the zero curtain). In addition, we first needed reasonable simulated snow depth and density before assessing thermal conductivity, as snow depth, density, and thermal conductivity jointly determine snowpack thermal resistance. We therefore did not include CL11 among the initial physics developments until we had reasonable snow depth and density across the sites, to limit potential compensating biases.

We also agree that CL11 does not include specific Arctic depth-hoar samples, which could make its classification as an Arctic-specific adaptation misleading. We therefore reformulated the paragraph in the Methods section to be more accurate:

> *"This new parameterization has the advantage of having been calibrated using high-resolution three-dimensional images of snow microstructure, covering a wide range of seasonal snow types, including wind slabs and depth hoar **samples formed under controlled temperature-gradient conditions. However, it does not include natural Arctic depth-hoar samples representative of strongly stratified tundra snowpacks.** The sample size in Calonne et al. (2011) is smaller than in Sturm et al. (1997), **whose dataset includes a larger number of high-latitude snow samples.** Nevertheless, the Calonne et al. (2011) approach provides a more physically based and potentially more reliable estimate of thermal conductivity by directly resolving the three-dimensional microstructural geometry. **In contrast, the Sturm et al. (1997) relationship is based on heated-needle-probe measurements, which are subject to known methodological limitations and may substantially underestimate thermal conductivity, particularly for fragile and highly anisotropic depth hoar (Fourteau et al., 2022).***
>
> ***Although grouped with the Arctic adaptations, the Calonne et al. (2011) parameterization is not intrinsically Arctic-specific. It was evaluated at this stage because its thermal effects could be better constrained at the Arctic sites, where soil temperatures exhibit larger winter amplitudes, and only after obtaining reasonable snow depth, SWE, and bulk density to limit potential compensating biases in snow thermal resistance."***

We also added a paragraph to the Discussion in Sect. 4.4, complementing our previous addition on the limitations of the single-layer framework:

> *“**The improved performance of the CL11 experiment should therefore be interpreted within the context of the CLASSIC single-layer framework rather than as evidence that the Calonne et al. (2011) parameterization is universally more appropriate for Arctic snowpacks. Under the assumptions of the sensitivity analysis (Sect. 12 of the Supplement), explicitly representing the stratified layers using the same density–conductivity relationship would reduce the effective snowpack conductance relative to that obtained in the successful bulk CL11 experiment. Reproducing this bulk conductance would therefore likely require layer-scale conductivities at least as high as, and potentially higher than, those predicted by the Calonne et al. (2011) parameterization. This result is directionally more consistent with the higher conductivities predicted by Calonne et al. (2011) than with the lower values predicted by Sturm et al. (1997), although it does not establish the universal validity of either relationship for natural Arctic depth hoar. Conversely, the better performance obtained with the Sturm et al. (1997) parameterization in some previous Arctic studies can also partly reflect compensating biases when snow depth and/or density remain inadequately represented, as a lower conductivity can compensate for a snowpack that is too thin or too dense without improving its overall physical representation (e.g., Dutch et al., 2022; Damseaux et al., 2025). Our sequential evaluation, in which snow depth and bulk density were improved before modifying thermal conductivity, was specifically designed to mitigate this risk. More generally, density alone is insufficient to characterize snow thermal conductivity, which also depends strongly on snow microstructure. This limitation is particularly important for depth hoar, whose pronounced anisotropy and evolving grain and bond structure can produce substantially different thermal conductivities at similar densities (e.g., Satyawali et al., 2008; Löwe et al., 2013; Barrere et al., 2017). Further evaluation using vertically resolved conductivity measurements, multilayer snow models, and microtomographic measurements of representative natural Arctic depth-hoar samples is therefore needed. Future parameterizations could incorporate microstructural descriptors, such as anisotropy or grain and bond geometry, in addition to density (e.g., Domine et al., 2011; Löwe et al., 2013; Calonne et al., 2019; Fourteau et al., 2021).** In addition, future CLASSIC developments could diagnose an internal density profile within the single prognostic snow layer, similarly to the quadratic temperature profile already used in the model, or implement a fully multilayer snow scheme.”*

314: *Table 1 contains no PFT information.*
Thank you for pointing this out. The PFT information is provided in Table S1 of the Supplement. We added it to the caption of Table 1:

> *“**Table 1.** SnowMIP (cdp, rme, snb, swa, sap, sod, wfj) and Arctic (byl, umt, tvc) sites. Additional information on site setup, **including the plant functional types (PFTs)** prescribed in CLASSIC, soil initialization, and the observational variables available for model evaluation is provided in Supplement Tables S1, S2, and S3, respectively. [...]”*

We still added a brief description of the observed vegetation at each site directly in Table 1. We also corrected several erroneous latitude, longitude, and elevation values identified during this revision.

343: *It could be worth noting that NRMSE > 1 means that a model has no more predictive skill than just taking the mean of the observations.*
We have added the following clarification right after the definition of NRMSE in the Method section: *"**An NRMSE greater than 1 indicates poorer RMSE-based performance than using the observed temporal mean as a constant predictor.**"*

403: *"Overall, snow albedo (third column) is reasonably well simulated, except at several sites where it is overestimated by about 0.1–0.2 toward the end of the snow season (e.g., panels c, m, r, and w)" – that is four out of the five sites at which albedo measurements are available!*
Thanks for noting that. We have revised the sentence to read:

> *"Overall, snow albedo (third column) is reasonably well simulated (mean RMSE of 0.077 and correlation of 0.64 across the **five** sites where observations are available), **although it is overestimated by about 0.1–0.2 toward the end of the snow season at four of these five sites** (panels c, m, r, and w). This bias may be related to the lack of representation of light-absorbing particles (LAPs) in the simulations and is further discussed in Sect. 4.2."*

Figure 4: *"Obs (man)" in the legend should just be a point without a line.*
Done (with all related figures).

578: *"with respect to observations"* Corrected.

586: *It is not clear that "at this site" refers to Swamp Angel, not Senator Beck.*
We have replaced "*this site*" with "***Swamp Angel***" to remove the ambiguity.

590: *Regarding sites known to be affected by LAPs, note that Swamp Angel and Senator Beck are reference sites for the Colorado Dust-on-Snow program (https://www.codos.org/#codos).*
Thank you for this useful information. We have added Senator Beck and Swamp Angel to the examples of sites affected by LAPs and cited the long-term dust and radiation measurements conducted at both sites by Painter et al. (2012) and Skiles et al. (2012):

> *"For example, snow albedo tends to be overestimated during the ablation period at Col de Porte, **Senator Beck, Swamp Angel,** and Sapporo (Fig. 3c, m, **r, and w**), which are all known to be affected by LAPs (Niwano et al., 2012; **Painter et al., 2012; Skiles et al., 2012**; Réveillet et al., 2022)."*

707: *"simulated snow depth"?* Corrected.

719: *There are recommendations for bias correction of precipitation measurements at Canadian sites including TVC in https://tc.copernicus.org/articles/10/2347/2016/*
The precipitation forcing provided by Dutch et al. (2022) had already been corrected following Pan et al. (2016). We have now stated this explicitly in Sect. 2.1:

> *"[...] we relied on the forcing data from Dutch et al. (2022), which span a two-year period* ***and include precipitation corrected for gauge undercatch following Pan et al. (2016).*** *[...]* ***Despite the prior gauge-undercatch correction,*** *the initial simulated snow depth was too low and the snow density was underestimated relative to snow-pit measurements. We therefore adjusted the snowfall rate in Dutch et al. (2022) to match the snow depth at peak snow accumulation in our simulations including our new Arctic snow model developments [...]"*

We have also revised the corresponding paragraph in the Discussion to improve clarity:

> *"At TVC, large uncertainties arise because the station itself may act as a local snow accumulation zone (Brampton Dakin, personal communication, 2023), further complicating the interpretation of snow depth measurements.* ***In addition, the site lacks direct snowfall measurements and, although the precipitation forcing had already been corrected for gauge undercatch following Pan et al. (2016),*** *it remained inconsistent with the simulated snowpack evolution, as discussed in Sect. 2.1. [...]* ***This additional empirical adjustment*** *introduces uncertainties into the analysis and may bias the tuning applied at TVC in the ALL_TVC experiment.* ***Residual discrepancies may arise from precipitation-phase partitioning, remaining measurement uncertainties, local snow trapping, and wind-driven snow redistribution, which cannot be resolved by a gauge-undercatch correction alone. A wind-dependent adjustment representing lateral snow transport and local deposition, conceptually similar to that proposed by Lackner et al. (2022), could be explored as an alternative to the uniform snowfall scaling.*** *However, because snow drifting can occur even in the absence of precipitation, it remains challenging to determine which correction approach is most accurate. In this context, the forthcoming 32-year dataset at TVC (Woolley et al., 2024; Tutton et al., 2025) will be particularly valuable for better constraining these model developments and ascertaining whether the two snow seasons currently available from Dutch et al. (2022) are representative, or whether snow redistribution processes compensate over longer time scales."*

Figure D2: *All of the thermal conductivity measurements in Dutch et al. (2022) were made in March 2018 or 2019, so should not be shown as continuous bars.*
Thank you for pointing this out. We have modified Figs. D2 and S1 accordingly. We also clarified the figure captions by adding:

> *"The density ranges combine the March and November 2018 and January and March 2019 field campaigns and are extended over the full period for reference. The thermal-conductivity ranges combine the March 2018 and March 2019 campaigns and are shown only during those months."*

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