Comments on "Evaluation of WRF 4.5.1 surface layer scheme representation of temperature inversions over boreal forests"

Sunday 29th October, 2023

General:

The manuscript by Maillard et al. evaluates the performance of two simplified surface layer schemes (extracted from the WRF model and modified) in reproducing the surface-based inversion over forest areas in the Arctic winter. The authors designed a simple two-layer analytical model to capture the temperature gradients (e.g., air-canopy, canopy-surface, and air-surface temperature differences), and then compared these conceptual models with modified simplified WRF surface-layer schemes to investigate the relationship between temperature gradient and wind speed based on a long-term *in situ* measurements. The modified models correct the limits on turbulence collapse under strong stable conditions to some extent. The paper provides some insights into the limitations of the common surface-layer schemes in WRF model and accordingly proposes to improve their performance in representing surface temperature inversions over boreal forests. Despite the paper offering good research idea and valuable insights, there are still some presentations require significant refinement to match the publication standards. Given the concerns, I recommend a major revision.

Major comments:

1. Conceptual model (section 2.1.1, section 2.1.2 & Appendix A).

Please double check and re-derive the conceptual model (Eqs.1,2,4,5) carefully. I have derived three times and found that the Eq.4 is incorrect and lacks coefficient Λ_s for both ΔT_{ac} and ΔT_{cs}. The correct expression should be:

$\Delta T_{ac} = \frac{\Lambda_s(T_a - T_g) + Q_i[1+\Lambda_s/(4\sigma T_a^3)]}{4\sigma T_a^3 + 2\Lambda_s}$ and $\Delta T_{cs} = \frac{\Lambda_s(T_a - T_g) - \Lambda_s Q_i/(4\sigma T_a^3)}{4\sigma T_a^3 + 2\Lambda_s}$

Despite it does not affect solving the total temperature gradient ΔT_{as}, it has a large impact on ΔT_{ac} and ΔT_{cs}, thus affecting the correctness of the results, as shown in Fig.2 and the relevant values in the text (e.g., Lines 122-123: ΔT_{ac} will becomes positive unless the snow depth is less than 1.8 cm when the lost coefficient Λ_s is added, instead of 8 cm).

Equation (1) is correct, I derived the same form. However, I have derived several times and have not gain the Eq.2 in the text. The expression I derived is:

$-Q_i - \Lambda_s(T_a - T_g) + \Delta T_{ac}[4\sigma T_a^3 + \Lambda_s + \rho C_p C_{D,a} U_a] + \Delta T_{cs}\Lambda_s = 0$

i.e., the term of ΔT_{cs} does not contain $(1 - \epsilon_c)4\sigma T_a^3$. Perhaps I was missing in some where.

2. Using only two short paragraphs (lines 365-375) to analyze the final validation of the modified SL models raises doubts about the reliability of the model. Therefore, it is advisable to provide a more comprehensive analysis and discussion of the results obtained using all "full measurements" as inputs to

the model (as shown in Figure 8). I recommend including a figure (similar to Fig.7) to present the final simulations based on the data used in Fig.8. This will strengthen the overall analysis and the reliability of your model.

3. The paper mentions the "S" shaped relationship between ΔT and U many times but does not provide a visual representation of this relationship using raw observations. I'm curious what exactly does the raw observations look like? It would be beneficial to include this content in the text. Adding a scatter plot of ΔT versus U using all the raw observations, with colors indicating the value of the stability parameter (z/L or Ri_b), would help illustrate whether the stability regimes align with the wind speed divisions mentioned in the text. Additionally, it is important to explain why the criteria of $U_a < 2$ m/s and $U_a > 4$ m/s were used to separate stability regimes. Clarifying whether this choice is based on Figure 3c or other considerations would strengthen the methodology. Providing this context will enhance the understanding of how the stability regimes were defined and improve the robustness of the methods employed.

4. The paper does not provide a detailed discussion of the limitations of the conceptual model, which is based on a number of assumptions (e.g., ignoring shortwave radiation and latent heat flux, ...) and has its validation based on curated observations (filtering data with some criteria, e.g., snow depth > 10 cm, clear-sky, downward shortwave < 30 W/m^{-2}, etc.). It is important to consider whether the model would still perform effectively in all real situations if all raw observations were used as model inputs instead of filtered data? This issue should be considered, as the last paragraph mentions the modified models will be applied to the WRF framework (the real scenarios beyond the filtered observations).

Minor comments:

* The title of the paper could lead the readers into thinking that it is an evaluation of the performance of the SLM in the WRF model. You actually did not run WRF, so it is recommended to modify the title for more appropriate.

* Line 56, It's better to give the full name of the abbreviation LMDZ.

* Lines 81-83, Could you please explain why the derivation of Eq.1 ignores shortwave radiation and latent heat flux? Is it because this study focuses only on conditions during Arctic winter?

* Figure 1, It would be better to mark the energy balance equation like Eq.A1 at the interface between each two layers in Fig.1, which well help the reader understand the derivation of the formula in the texts. This is a recommendation only and is not mandatory.

* Line 109: "...the weakly and strongly stable limits.". How to define these two regimes?

* Caption of Figure 3: better "(a)" than "Panel a:"; "(b, c) Same as (a), but for ΔT_{cs} and ΔT_{as}, respectively." than "Panel b: same, for ΔT_{cs}. Panel c: same, for ΔT_{as}". In addition, please keep figure labels consistent – it's better not to use "panel" for some figures and "a, b, c" for others.

* Figure 5c: Colorbar is missing. And, why did you choose to use a histogram instead of a scatter plot with raw data?

* Lines 245-250: The shape of ψ of WRF looks similar to the measurements when $z/L < 20$, with the exception of the range $3 < z/L < 10$. So, the statement of "...more gradual than WRF function" appears to be inaccurate.

* Line 257: Fig.5a shows the fitting curve deviates significantly from the observations when $z/L > 3$. Questions that could be addressed include: What's the number of valid data points used for the fitting shown in the graph? Is the fitting function statistically significantly? What is the standard deviation of the fitting coefficients?

* Line 284: What am I missing? I did not see a comparison of the calculated turbulent diffusion coefficients with outputs from actual WRF runs. It is recommended that such a comparison be included, perhaps as supplementary material, to validate the modified model's performance is consistency with WRF runs.

* Line 310: How are the specific threshold values of 50 and 60 W/m^2 for Q_i determined in results analysis (e.g., Fig.6 & Fig.7)? What is the rationale for using these values (50 and 60) for data grouping?

* Line 331: "see Fig. 5a" than "see Fig. 3a".

* Caption of Fig.7: Where the specific values for T_a, T_g and Λ_s are obtained from?

* Fig.7: the red "oMYJ" in the legend should be corrected to "oMP". Why does the "mMP" overestimate ΔT_{ac} under weak wind speed in Fig.7a?

* Line 433: What's expression for H_a? $-\rho C_p C_{Da} U_a (T_c - T_a)$?

* To enhance the clarity of the derivation process in the Appendix, it is recommended to include the missing details regarding the approximation $|T_a - T_s| << T_a$ and the use of the equation $T_s - T_g = \Delta T_{ag} - \Delta T_{ac} - \Delta T_{cs}$, and so on.