GMD Revisions

EGUSPHERE-2025-5512

DReaMIT: A Dynamical Reanalysis Framework for Modelling Surface-Based Temperature Inversions in Cold Environments

Review 1:

[0] Generalized comment: A major issue is that the authors compare their new method with three existing approaches while assuming that readers are already familiar with these baseline methods. This may limit the readability and broader applicability of the work. I recommend generalizing the formulations of the three comparative methods (Equations 2–5) in terms of absolute/relative elevation and hypsometry, rather than simply citing previous studies. Equations 2–5 differ primarily in their definitions of the height term h, which can be explicitly summarized. Such generalization can directly highlight the novelty of the proposed method and make the contributions accessible to broader readers who may not have a background in climate downscaling.

- ➔ We added a full paragraph in the introduction to better contextualize SBIs and provide a broader and more accessible background to the work of Pozsgay and Gruber (2025). There, we highlight the broader importance of SBIs on downstream applications beyond model development.
    - LINES 37-45: New paragraph.
- ➔ The new model presented in this study, based on hypsometry and using a non-linear function, is only compared to a single model, namely the model of Pozsgay and Gruber (2025), which is labelled as the 'Dawson' model in Figure 4. This model is presented in Section 3.3.1 and the near-surface modelled temperature is given in Equation (1). Now, the 'three' models corresponding to the first 3 columns of Figure 4 are all in reality the one model from Equation (1) but with small differences: (a) 'Dawson' uses the exact fitted parameters alpha and beta derived in Pozsgay and Gruber (2025), and the figure shows it is inadequate for spatializing; (b) 'Absolute elevation' uses the same model but with a new fit of alpha and beta; (c) 'Relative elevation' is the same model but z is replaced by z_relative in Equation (1) and a new fit of alpha and beta is performed too. This is now explained with greater care at the

end of Section 3.3.1. We added a full paragraph in Section 3.3.1 to summarize this, i.e. carefully lay out the 'three' models of the first three columns in Figure 4 and highlight the novelty of the proposed method.

  - LINES 133-139: New paragraph.

- ➔ The new model is presented in Section 3.3.2 and the near-surface air temperature is given by Equation (4). The main difference with the 'Dawson' model of Equation (1) is that the variable encoding the topography is no longer absolute elevation z but hypsometry h, which is defined in Equation (2). Note that Equation (3) is not a new definition but simply gives context to the reader of the physical meaning of the bounds on the hypsometry (0<=h<=1). Finally, Equations (5) and (6) specify the function form of alpha and beta appearing in Equation (5), with the second notable difference of the model presented in this work compared to the one of Pozsgay and Gruber (2025) being the exponential structure of alpha, rather than the linear one.
  - LINES 37-45: Added more background to SBI and the model of Pozsgay and Gruber (2025).
  - LINES 133-139: Added a paragraph to better present the different 'models' we compare the new hypsometric model to.
- ➔ A clarification of the meaning of the 4 'models' in the 4 columns of Figure 4 was also added in the Results (Section 4.2).
  - LINES 241-244: Added a sentence to better present the different 'models'.

[1] Lines 140–145. Please elaborate on the formulation of the hypsometry for broader readership. For each station, is x0 the station location and z_0 the station elevation? In Equation (2), is h(x_0)a constant value? It appears to also vary with z, and the authors need to clarify this function explicitly.

- ➔ Thank you for pointing out this potentially confusing definition. Indeed, x0 and z0 stand for the position and elevation respectively of a given point, e.g. an observation station. The hypsometry of this point, h(x0), is uniquely defined when the radius d is fixed, which we do in this study. However, we only do so after having carefully analyzed the influence of such radius, and when it is allowed to vary, then h(x0) becomes a function of the radius d. We added some clarifications before and after the definition of h(x0) (Eq. 2), that we explicitly changed to h(x0, z0) to reflect the full picture. We hope that they will be helpful in adding some clarity.
  - LINES 161-162: We added part of a sentence to clarify position and elevation of the station.
  - LINE 164: We changed h(x0) to h(x0,z0) in Equation 2.

- LINES 166-168: We added two sentences to explicit the link between hypsometry and elevation on one side, and it how varies with radius on the other side.
- LINES 170-171: Added a plain-language sentence between Eqs. 2 and 3 explaining what a high versus low h value means in practical terrain terms.

[2] Sections 3.3.4 and 4.2. The description of α is confusing. Why is it referred to as "observed α" (around Line 190)? Given that α is a fitted parameter, it cannot be directly observed like temperature. This wording also hinders the interpretation of Figure 4. Additionally, the blue and orange points in Figure 4 appear to follow an exponential distribution rather than a linear relationship with depth, which tends to support the proposed method.

- LINE 216: alpha_observed to alpha_effective in Equation 8 and modified every mention of it in the manuscript.
- FIGURES 4, 7, and 8: We changed alpha_observed to alpha_effective.
- LINES 218-224: We added a paragraph to better explain the meaning of alpha_effective and how it differs from the modelled fitted value.
- LINES 255-257: We added a sentence to better interpret Figure 4 and explain why the new model is successful at spatializing the relationship, as opposed to the other formulations in the first 3 columns.

Review 2:

1) The authors mention that a 50 km radius yielded the best empirical fit for calculating hypsometric position, but the physical explanation of this aspect (beyond synoptic vs. local scale) remains unclear and poorly discussed. Specifically, the paper would benefit from a more explicit justification of why the 50 km radius works best for features that actually often operate on a much localized valley scale (e.g., 5 km, which hit a local minimum in performance). Does this large 50 km radius reflect the imprint of the reanalysis grid cells? or is it more indicative of the synoptic-scale boundaries of regional cold-air drainage systems? This distinction would strengthen the physical basis of the model.

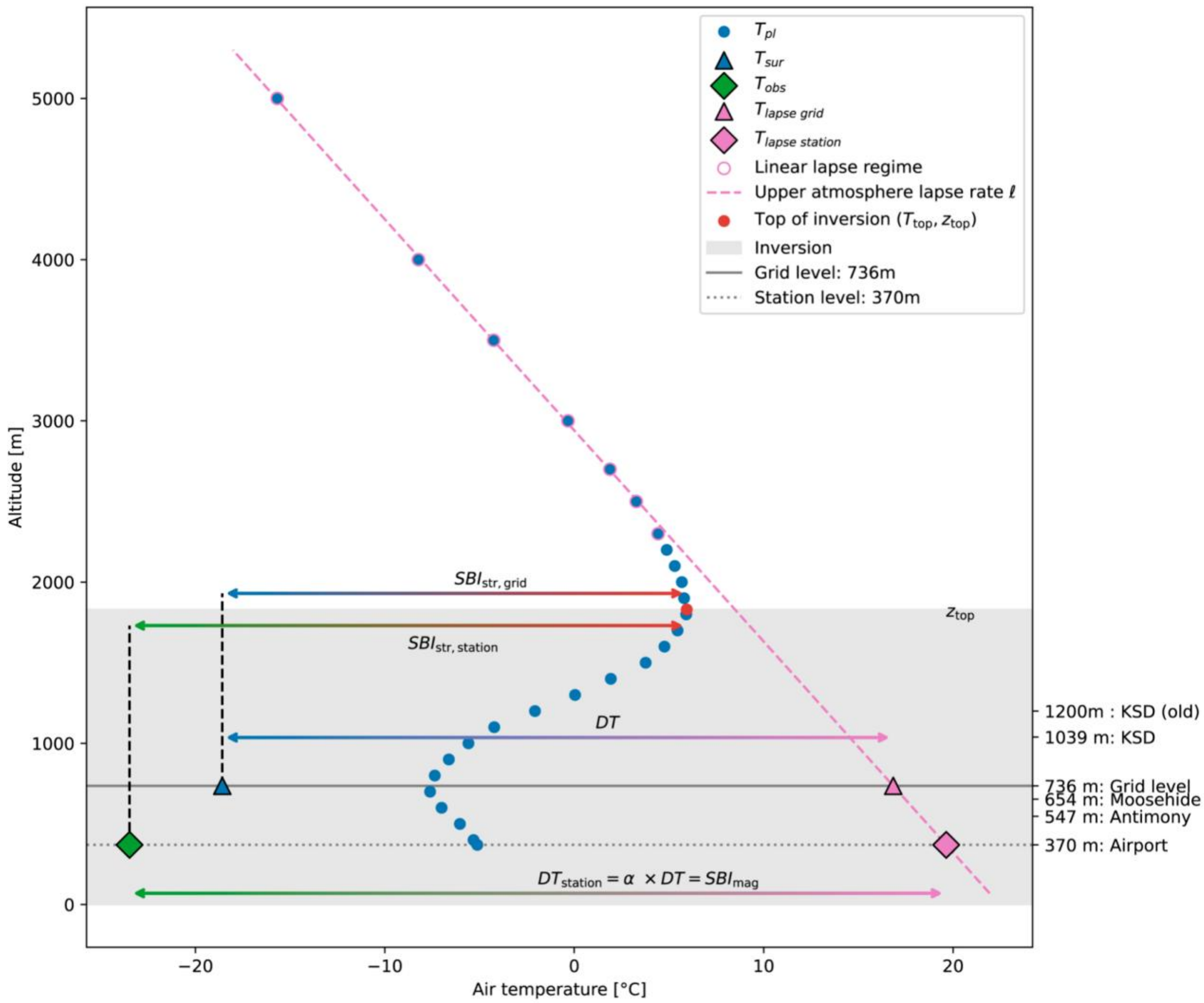
- ➔ We added a paragraph addressing the hierarchy of scales at play, linking the sustained duration of SBIs to regional valley connectivity, effectively increasing the radius due to cumulative effects.
    - LINES 381-386: New paragraph discussing the scales to calculate hypsometric position, motivating the 50km radius choice by a better-informed physical reasoning rather than a purely empirical one.

2) I wonder if the authors can discuss (or even better, propose) a threshold of hypsometric range beyond which DReaMIT breaks down? This is motivated by the limitations in flat river dominated regions.

- ➔ We do not think we can give such a threshold as we do not seem to see a clear pattern suggesting this. We would first like to point out the discussion of Tulita, Old Crow, and Norman Wells, that we think is useful but does not imply that the model "fails" generally in low-relief areas. The dominant controls on near-surface temperature there are not primarily terrain-controlled cold-air pooling. In these locations, river influence, floodplain energy balance, weak relief, and possible reanalysis/grid elevation artifacts may dominate over the hypsometric signal. This distinction helps define the model's domain of applicability more constructively.
    - LINES 354-356: We added a sentence in Section 5.3 to better lay out the dominant controls for the discussed stations where the model doesn't perform as well, highlighting the fact that the limiting cases do not show failure of the model but rather that SBIs are sub-dominant there.
    - LINES 404-406: We also added a plain word sentence at the end of the Conclusion to make the conclusion more actionable for future users of the model.
- ➔ Furthermore, DReaMIT is also correcting reanalysis data artifacts below grid level (see Figure below, taken from Pozsgay and Gruber (2025)), and we believe we need to gather more experience in the precise effects of the model in low range

environments before giving definitive thresholds.

Fig. 2.



3) It would be useful to define SBI in a sentence for a non-specialist reader. One sentence goes a long way.

- ➔ This element was indeed missing from the manuscript, and we added a couple of sentences in the Introduction to fill that gap.
    - LINES 37-40: Added a non-specialist sentence to define SBI.

4) Related to comment 3, the model by Porzsgay and Gruber (2025) is insufficiently summarized. I think adding a paragraph explaining it would provide the reader with a better background to value the present work more.

- ➔ Together with comment 3, we added a paragraph providing a better background for the reader.
    - LINES 37-45, 48-49: Added more background.

5) The abstract is not self-contained, referencing aspects such as "Exponentiation of the elevation variable" without sufficient background or context. Perhaps revise for completeness. Also, is it really an exponentiation of the "elevation" variable or the hypsometric position?

- ➔ We agree that this sentence was not appropriate for an abstract and we reformulated it to mention going beyond a simple linear model.
    - LINE 6: We got rid of "Exponentiation of the elevation variable", giving preference to a vocabulary that is more in line with an abstract style.

6) Introduction (Line 17): define the acronyms ERA5 and JRA-3Q. Also, in Line 19, perhaps use "multiple atmospheric pressure levels"

- LINES 17-18: Acronyms defined.
- LINES 20: Added 'atmospheric' to pressure levels.

7) Introduction (Line 21): "hydrological processes"?

- LINE 23: Added 'processes' to hydrological.

8) Line 42: what is mean by "normal" lapse rates? Is this meant to say "linear", "dry adiabatic", "moist", etc..

- ➔ We added some clarification to what we mean by normal lapse rate in this situation, based on the way reanalysis data is extrapolated below the grid level using local lapse rate with only the lowermost atmospheric pressure levels.
    - LINES 53-54: We specified what we meant by 'normal' lapse rate.

9) Line 94: correct “since since 1947”

- ➔ LINE 105: Done

10) Line 215: “second column” not second row

- ➔ LINES 240-257: Done (4 modifications)

11) Line 266: “lapse” not laspe

- ➔ LINE 297: Done