
Response to Reviewer #2 Comments

Comment 1: The main concern I have now, and I did not pay attention to last time, is the trends evident in Fig 4. I think the authors must say what is causing this. Is it that the model has not reached steady state at the beginning? or is this spurious ERA5 trends? The latter is trivial to compute, and I think the authors should show a map of trends in ERA5 over the data period (or just over 2000-2023) and the map of trends estimated from the model. The trends shown are small ($<0.05\text{mm/yr}$), but they may well be spatially systematic and hence bias entire continents in the same direction.

Response 1: Thank you for pointing out this issue. We agree that the trend-like variations visible in Fig. 4 need further explanation. In the revised manuscript, we have added an analysis comparing the long-term trends in the ERA5 soil-temperature forcing and the corresponding TED series during 2000–2023 (new Fig. 5).

The comparison shows that the spatial patterns of the TED trends are generally consistent with those of the ERA5 soil-temperature trends. The strongest relationship is found for the 7–28 cm soil-temperature layer, with a Pearson correlation coefficient of 0.719 at the station scale. The correlation increases to 0.758 when the data are averaged within $5^\circ \times 5^\circ$ grid cells. These results suggest that the low-frequency variations in TED are mainly related to long-term variations in the temperature forcing and the associated subsurface thermal response.

We have also revised the manuscript to note that trends fitted to TED time series should be interpreted with caution, as they may reflect long-term variations in the temperature forcing and uncertainties in the input datasets.

The following paragraph has been added in Section 3.1:

“The representative time series in Fig. 4 also show low-frequency variations that appear as trend-like signals over the 2000–2023 period. To examine their possible origin, Fig. 5 compares the long-term trends in ERA5 soil-temperature forcing with those in the TED series after removing the annual and semiannual components. The spatial pattern of TED trends is broadly consistent with that of the ERA5 soil-temperature trends, particularly over continental regions with pronounced long-term temperature variations. Among the four ERA5 soil-temperature layers, the 7–28 cm layer shows the strongest station-scale relationship with TED trends (Pearson $r = 0.719$). This relationship becomes slightly stronger when evaluated at the $5^\circ \times 5^\circ$ grid-cell scale ($r = 0.758$). These results indicate that the low-frequency variations in TED are closely associated with long-term changes in the temperature forcing and the resulting subsurface thermal response.”

The corresponding new figure has been added as Fig. 5:

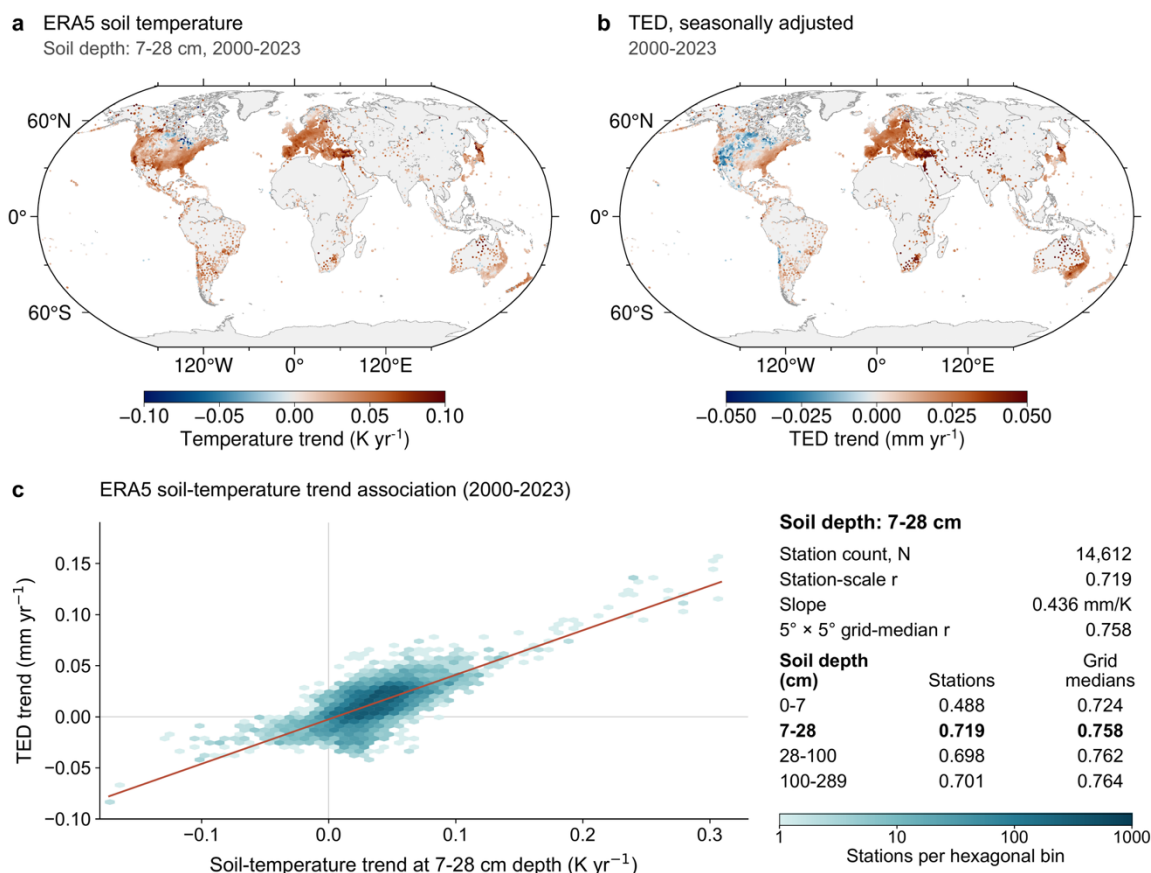


Figure 5 Relationship between low-frequency TED variations and ERA5 soil-temperature trends. (a) Linear trends of ERA5 soil temperature at 7–28 cm depth during 2000–2023. (b) Linear trends of TED after removing the annual and semiannual components. (c) Relationship between soil-temperature trends and TED trends at GNSS stations. The inset table summarizes Pearson correlation coefficients for the four ERA5 soil-temperature layers at station scale and at the 5° × 5° grid-cell scale.

Comment 2: In my original review, I suggested that data before may be most useful. The authors responded: "The limitation is not the ability of the thermoelastic model to generate TED estimates before 2000, but rather the quality and consistency of the available GNSS coordinate time series during the early period." This seems an upside-down response. Surely it is not the GPS that is validating the model, but the model seeks to improve the GPS? After all, the GPS is only showing 10% RMS reduction, which is hardly a validation. The main validation should be on either the setup of the FEM or the forcing fields. Both should be just as reliable prior to 2000 as after.

My point is that since the network is so sparse before 2000, deformations of the kind the authors model become more important potentially since they can alias into global solutions in ways that may not with a better station distribution. Anyhow, something for another time.

The authors responded: "The thermoelastic model itself can be extended to earlier

periods where sufficient GNSS observations and input datasets are available. " - GNSS observations are not a requirement for the model. And there are input datasets available for all time periods. It is ok to say that this is just something we'll consider in the future.

Response 2: Thank you for this clarification. We agree that our previous response was not clearly stated. The availability of GNSS coordinate time series is not a limitation for generating TED estimates, because the FEM_{FL} model itself only requires temperature forcing and subsurface properties as inputs.

The current 2000–2023 period was selected for this release because the dataset is designed as a station-level correction product for GNSS height time-series applications. This period provides a consistent framework for product generation, evaluation, and comparison with existing GNSS time-series analyses. The FEM_{FL} model can in principle be extended to earlier periods when the required input datasets are available. We have revised the manuscript to clarify that extending the dataset before 2000 is a possible future development rather than a limitation of the modelling approach.

We also appreciate the reviewer's comment that the physical evaluation of the model should rely primarily on the consistency between the model response and the temperature forcing rather than the GNSS WRMS reduction alone. In the revised manuscript, we have added an additional analysis comparing ERA5 soil-temperature trends with TED trends (Fig. 5) to examine the relationship between the temperature forcing and the resulting thermoelastic response.

The following sentence has been added to the Data availability section:

"Although the FEM_{FL} model can be applied to earlier periods when the required temperature forcing and subsurface information are available, the current release covers 2000–2023. Future extensions to earlier periods will be considered after evaluating the consistency of the corresponding input datasets and broader application requirements."

Comment 3: I welcome the removal of sites in regions of ice cover, although it may also have been appropriate to either include them with NaN or include them with 0.000m deformation. Perhaps the NaN would be sufficient. The absence of a file for a given station may cause downstream problems to users, but I guess NaNs in a file may do also. I wonder (below) if all figures are updated to reflect that these sites are now removed (Fig 8 and 9).

To me, it is unfortunate that the authors adopted the approach of producing a known incorrect model for the seasonal snow-covered regions. Here, they have the data presumably (or can download it) that would enable them to consider this more easily than downstream users. Assigning the temperature of the snow or zero degrees as the surface temperature in the presence of snow, rather than air temperature, would dramatically improve the model and produce a usable product that requires little editing. This presumably requires downloading some extra files but little else in terms of code modification. Next version!

Response 3: Thank you for these helpful comments. For ice-covered regions, we have excluded stations located in Antarctica, Greenland, permanent ice-covered areas, glacier-covered regions, and ice shelves from the released dataset and subsequent analyses. The

same screening criteria have been applied consistently to all analyses and figures in the revised manuscript. We have also clarified this point in Section 2.1.

Regarding seasonal snow-covered regions, we agree that snow conditions can affect the thermal boundary condition and introduce additional uncertainty in thermoelastic modelling. In this study, the upper thermal forcing is derived from ERA5 soil temperature rather than air temperature; therefore, part of the thermal influence associated with seasonal snow cover is implicitly included in the forcing. However, snow depth and snow thermal properties are not explicitly represented in the current subsurface model. We have clarified this limitation in the revised manuscript, and improved representation of snow processes will be considered in future updates of the dataset.

The following text has been added/revised in Section 2.2:

“Because ERA5 soil temperature is used rather than air temperature as the upper thermal forcing, part of the surface thermal effects associated with seasonal snow cover is implicitly included in the forcing. However, snow depth and snow thermal properties are not explicitly represented in the current model, which may introduce additional uncertainty in regions with strong seasonal snow cover.”

The corresponding limitation has also been revised in the Discussion and conclusions section:

“The model also uses static shallow subsurface properties and does not explicitly represent time-varying soil moisture, snow depth and snow thermal properties, freeze–thaw processes, or permanent ice conditions. Although ERA5 soil temperature is used as the upper thermal forcing and may implicitly include part of the snow-related thermal effects associated with seasonal snow cover, the lack of explicit snow properties in the subsurface model may introduce additional uncertainty in snow-covered regions.”

Comment 4: I thank the authors for clarifying what happens in lake or coastal regions. Does this mean that a site on land, because of ERA5 resolution, but falling in a lake pixel, would have no model result? Whereas it could have used the nearest valid cell? If there is a GPS it is on land! Anyhow, again this for future improvement. The L186-188 is not clear on what exactly happens - are such sites excluded also?

Response 4: We agree that the previous description was not sufficiently clear regarding the treatment of coastal and lake-side stations.

In the revised manuscript, we have clarified that only stations located in Antarctica, Greenland, permanent ice-covered regions, glacier-covered regions, ice shelves, and their surrounding buffer zones are excluded from the released dataset. Stations located in coastal or lake-side environments are retained as long as valid land-based temperature forcing and subsurface information are available. For stations close to coastlines or large lakes, invalid ocean and lake grid cells are not used for the thermal forcing; instead, only valid land grid cells with soil-temperature values are considered.

The following text has been added/revised in Section 2.1:

“In contrast, stations located in coastal or lake-side environments are retained as long as valid land-based temperature forcing and subsurface properties are available.”

The description of the temperature forcing in Section 2.2 has also been revised:

“For stations close to coastlines or large lakes, ocean and lake grid cells are not used for the upper-boundary forcing. Only valid land grid cells with soil-temperature values are considered.”

Comment 5: In L98 please add that this is 1D.

Response 5: Thank you for the suggestion. We have added “one-dimensional” to clarify the FEM framework used in this study.

Comment 6: L203 "ice shelves" are floating and hence part of the ocean. No relevant GPS are on ice shelves.

Response 6: Thank you for pointing this out. We agree that ice shelves are floating ice and should not be treated as land regions in this context. Since no GNSS stations in the dataset are located on ice shelves, we have removed “ice shelves” from the station-selection criteria in the revised manuscript.

Comment 7: Figure 8: please define what the cutoff is for high latitude. Given most ice-covered sites are removed now, I wonder why there are so many stations plotted in this panel? Same for Figure 9. I think these are likely legacy figures and not consistent with the new methods.

Response 7: Thank you for pointing this out. We have clarified the definition of the high-latitude group in the revised manuscript, which is now defined as stations with absolute latitude $\geq 60^\circ$.

We also confirm that Figures 8 and 9 have been regenerated using the updated station set after applying the ice-covered region screening described in Section 2.1. Therefore, the stations shown in these figures represent ice-free high-latitude regions rather than the previously included ice-covered sites.

Comment 8: Figure 13 rows 2 and 4 do not comply with the response that the paper will focus on the TED effect. Here we see the combined effect of various loading displacements and TED.

Response 8: Thank you for this comment. We agree that the lower panels of Fig. 13 include the combined effects of TED and other environmental loading components. These panels were not intended to evaluate the correction performance of TED alone, but to illustrate the relative timing and amplitude of different environmental displacement components at representative stations.

To avoid possible misunderstanding, we have revised the description of Fig. 13 and clarified that the direct assessment of TED correction is based on the TED-only WRMS analysis in Fig. 11 and the FEM_{FL}–homogeneous FEM comparison in Fig. 12.

The revised caption of Fig. 13 is:

“The lower panels compare the raw GNSS height series with the series after removing

the combined TED, HYDL, NTAL, and NTOL corrections. These panels are shown only to illustrate the relative timing and amplitude of different environmental displacement components, rather than to evaluate the correction performance of TED alone.”