

Review of the revision of “A new global grid-based weighted mean temperature model considering vertical nonlinear variation” by Peng Sun et al.

To the Authors,

Thank you for considering some of my suggestions. However, the manuscript needs some more revisions.

1) In the response, you answered the question (1) in my first report. However, I cannot see enough revisions to your last version manuscript. You should describe the significance of your study more clearly in the manuscript, not just in the response to my comments.

2) Again, you answered my question (2) only in the response instead of the manuscript. And I am not satisfied with your answers. What I am concerned about is the ability of your model to reflect the short-term variations of T_m . From figure 1 in your response, we can see that the time series of the T_m from your model is quite smooth. Moreover, there is an obvious systemic bias between the outputs of $T_m=0.6195T_s+103.3452$ and the radiosondes. The coefficients are constant in equation $T_m=0.6195T_s+103.3452$ so its performance varies with time, while your model is time-varying. I would like to see the comparisons between your model and the other time-varying T_s - T_m model. It doesn't matter which model performs better because the main advantage of your model is the “height-varying” compared with the T_s - T_m model. I just want you to evaluate your model more comprehensively.

3) In the abstract of the last version manuscript, you wrote “These results suggest that ... can significantly improve the accuracy of model-predicted T_m for a GNSS receiver ... (assumed to be 10 km)”. From figure 8, I think there are of course improvements, but not significant. And you evaluated the accuracies at pressure levels, so it is not proper to say “below the tropopause (assumed to be 10 km)”. I found the same problems many times in your manuscript.