RC3: Andreas Stohl

I realize my comment was a bit harsh and probably not all too polite either. However, there are simply so many papers in the literature that present little, if any, progress, and therefore I am not very sympathetic to seeing more such articles. I simply think that we should strive NOT to publish every result but instead publish more substantial advances in scientific understanding. But, to be honest, I also don't see that you have actually made a serious attempt at improving the paper in that respect, beyond adding a few sentences and adding entrainment as a possible explanation for your results. A serious attempt would definitely require additional analyses which cannot (and should not!) be done within 24 hours.

If you see differences between models, you should really explain them, not only document them, especially since the reason is - in my opinion - pretty obvious, namely that the simple trajectory models lack a process that is extremely important in the boundary layer. I am not sure why such models are used at all in a region (the boundary layer) that is beyond their design. So I don't really see the point in the comparison, since the reason for the difference should be obvious. But if you still want to compare, you should at least identify and document which process is behind and quantify its effect. At least in the model world, that should be absolutely straightforward.

How right or how wrong the different models are in comparison to reality is, of course, a much more difficult question, and I am not expecting this to be fully answered.

We thank the reviewer again for his comments. We appreciate the opportunity to clarify our underlying rationale for this study. As we previously noted, we fully agree with you about the important limitation highlighted here: pure-trajectory models do not account for sub-grid motions, convection and boundary layer turbulence, and, as trajectories enter the boundary layer at the end, where turbulence is extremely important, trajectory models may fail to capture the transport history of the air mass. We agree with your understanding of the limitations of pure-trajectory models in this context. Indeed, the sensitivity analysis in Section 3.3 pointed out that the turbulence parameterisation is causing these differences.

However, we respectfully disagree with your point that dispersion models are the “correct” tool for the Lagrangian temperature anomaly decomposition, and pure trajectory models are “wrong” simply because they do not account for sub-grid motions, convection and boundary layer turbulence. We don’t have a ground truth to perform a robust assessment of both model classes and to determine which of them is “correct”.

In this context, performing the sensitivity analysis you suggested, while physically insightful, would ultimately lead us back to the same question. Indeed, we performed a simple test using FLEXPART simulations with and without turbulence parameterisation. We computed the resulting budget only considering the parcel track from two positions before crossing the PBL to two positions within the PBL. Figure R1 clearly shows the discrepancies associated with the turbulence parameterisation regarding the contribution of adiabatic and diabatic processes. Overall, this result shows part of the effect of the turbulence parameterisations on the temperature anomaly budget due to the entrainment of air parcels in de PBL. However, it does not answer the core scientific question: *Which model class (pure trajectory or dispersion) yields a temperature decomposition closer to reality?*

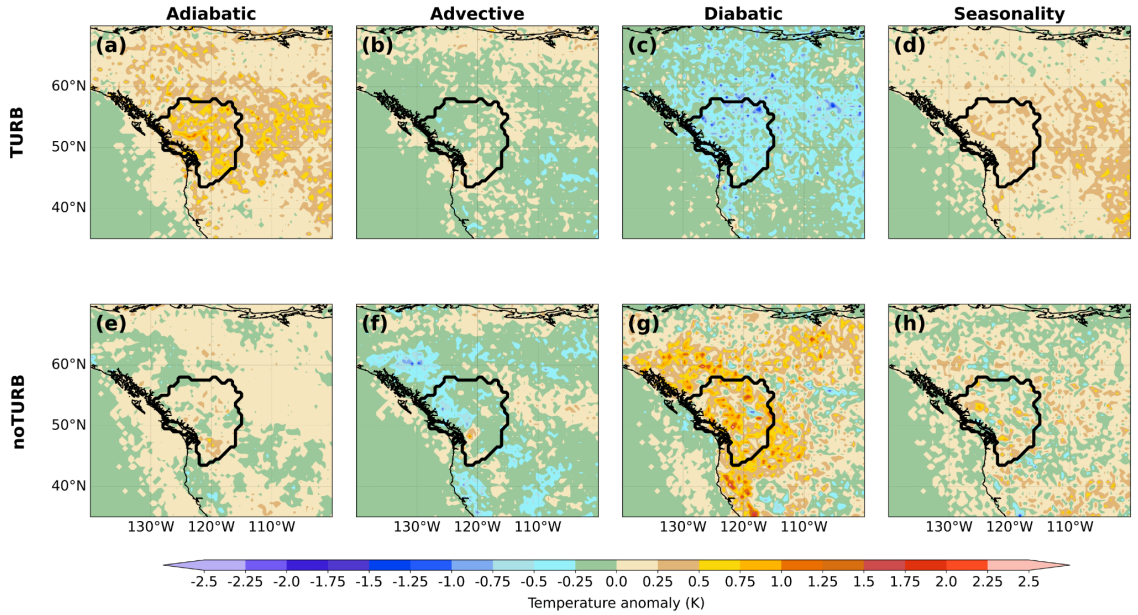


Figure R1. Lagrangian temperature anomaly decomposition considering only air parcel tracks from two positions before crossing the PBL to two positions within the PBL. Air parcels were extracted from FLEXPART simulations with (TURB) and without (noTURB) turbulence parameterization.

In general, we argue that such a sensitivity study would be highly valuable if we were comparing two different dispersion models, where understanding the different turbulence schemes is necessary. However, when comparing a dispersion model to a pure trajectory model, quantifying turbulence confirms the mathematical difference between having a parameterisation and not, without proving which approach is more valid for the Lagrangian temperature anomaly decomposition.

We believe that the next study needed is not a model inter-comparison of turbulence effects, but rather a robust analysis of the temperature anomaly decomposition compared with the synoptic pattern, soil-moisture temperature coupling, etc., to understand which resulting budget is better aligned with the overall atmospheric circulation. Because that definitive validation remains an unsolved challenge for the community, we view our current study as a crucial first step. Given the widespread and continued use of pure-trajectory and dispersion models in heatwave research using absolute terms, we believe this comparison provides useful information for the community by highlighting how the choice of model approach can affect the resulting physical interpretation. Importantly, our goal is not to present a fully resolved answer, but to highlight this significant discrepancy to the community, thereby motivating the exact future validation studies and deeper theoretical analyses that both we and the reviewer agree are necessary.

We hope this clarifies why we believe the manuscript's current scope matters to the community and why we chose not to expand the analysis into a turbulence sensitivity study.