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Interactive comment on "Simulating atmospheric tracer concentrations for spatially distributed receptors: updates to the Stochastic Time-Inverted Lagrangian Transport model's R interface (STILT-R version 2)" *by* Benjamin Fasoli et al.

Anonymous Referee #2

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General Comments:

This work documents the workflow of STILT simulations and presents improved physical processes for fine-scale simulations. I appreciate the authors' efforts in addressing overdue problems for the community, in particular those who use STILT extensively. I hope that the authors continue updating their work through GitHub.

I can easily follow the method and think the paper is relatively well written given the con-

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Discussion paper

ciseness in length. I have some questions/concerns in the evaluation of the improved method. In current form, the authors do not characterize the errors, in particular in surface emissions. So it is hard to evaluate the results. The model evaluation is a key result in this study, and the authors need to describe how much they know (or prescribed) the errors in surface emissions (and others if prescribed) so that we can be sure that the better results from GWD are due to the improved schemes. Please also address the detailed comments below.

Detailed Comments:

L13 - 21: STITL-R should be applicable to other tracer gases, not only CO2. The authors describe CO2 only, which seem to be strange. This is probably because the authors show an evaluation study using CO2, but this CO2 focus is limited.

P2, L21: Need to cite older work about HYSPLIT.

P2, L28 - 29: Need to mention more recent work on city-scale or regional inversion work based on multiple receptors that uses STILT extensively. Literature review here does not represent a full range of the use of the traditional STILT, which I believe is import to for the reader to understand the context, and motivation for the new development.

P3, L6: Need to include the reference for R properly. Not doing so is irresponsible because without R this work is not possible.

P3, L20: For large-scale simulations, the users have applied other types of parallelizations in running STILT, e.g., running multiple jobs (each job may represent one receptor for a give period) at the same time taking advantage of high performance computing. The authors need to briefly mention what the difference between the old method and the one introduced here would be although the method described here seems to be similar to what users have been using. Is there a new concept here?

P3, L27: Not all systems use SLURM although it is popular. Is there an option for a different job scheduling tool?

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P4. L4 - 22: In many cases, PBL heights from meteorological models (e.g., WRF) are directly used to represent z_pbl. The authors need to clarify this and describe more on the use of WRF PBL related to equations (1) and (2). For HNF simulations, WRF needs to be run at a similarly fine sale, which is really expensive? If not, what would be the impact on h = min(h',hˆ*)?

P5, L1-2: Reading this, my immediate thought was if this would require more simulation time to estimate the weighted influence. It would be nice to mention the cost.

P6, L32: Should not include a paper in preparation.

P7, L5: 24-h backward in time seems to be too short. How was the upstream boundary condition treated? I see a short description from L17. Boundary conditions are complex due to wind directions. Is the wind consistent from one direction? I would like to see a more description on this.

P7, L30: Please use rˆ2 and state which method was used in calculating r. Pearson's method? How are these rˆ2 values statistically different? The simulations from GWD is distinguishably from a different distribution from the other two so that we have more confidence in GWD? Note that in this evaluation, we want to clearly see better results from GWD. Right?

P8: L1: I think this is probably the most important single statement in this paper. I would like to know how the authors determined the uncertainty in the surface fluxes. Without precise uncertainty characterization, the results are not reliable. What if the inventory is systematically low and GWD overestimated the mole fraction, which could be shown to be closer to the observations than the other two methods? I believe that the authors have considered this point, but I don't see the details here to the level that I can clearly see the outperformance of GWD. Also we need to note that the rˆ2 values are all low and similar to each other.

P8, L6: Please be more quantitative. It is not clear what has been reproduced.

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P8, L10 - 15: The simulated mole fractions are a combined result of transport and surface flux emissions. The authors, as mentioned, need to say how much we know about the surface emissions (used here) related to this discrepancy as well as the transport arguably improved from this work.

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