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Geoscientific Model Development Discussions
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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 #1

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Review of Fasoli et al.

Fasoli et al report on developments made for the Stochastic Time-Inverted Lagrangian Transport Model (STILT). They added high-level functions to make simulations using the R language. They added code to make parallelized simulations. They introduce a new method to deal with near-field emissions which have not yet been homogeneously mixed within the boundary layer. Fasoli et al. further introduce a smoothing

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Discussion paper

technique to estimate plume surface response functions that is compared to the existing approach. Finally, they show how their developments perform in an experiment in which CO2 measurements have been taken aboard a light-rail in the Salt Lake City metropolitan area.

In general I find the manuscript well written, developments and results are presented in a concise and understandable manner. The manuscript fits the scope and contains enough scientific content to warrant publication in GMD. I have a number of comments that I would like to see addressed before publication, hence I end up with "minor revisions".

My main points of concern are:

1) There is no mentioning of how this work and code repository relates to the original code repository and work at http://stilt-model.org / BGC Jena. None of the co-authors are from Jena or other developers of STILT. I can only assume that this development has been made in accordance and in agreement with the rest of the STILT developers, and that there are no licensing issues. Should be checked and stated explicitly.

2) Model performance is assessed in a very qualitative manner ("looks better") and sometimes overly positive. I suggest authors consider more quantitative assessments, and take a step back before claiming (see below) e.g., that the model represents enhancements in (individual) roadways and intersections.

3) While developing a new method to deal with incompletely mixed sources close to the receptor, the fall back to limiting mixing to the crude 0.5*PBLH formulation. There is no physical basis for that, and I urge the authors to reconsider this artificial limitaion. More on this below.

Specific comments:

p2 l31ff It is unclear to me why 1-10km and 0.1-1 hr spatial and time scales should qualify as "hyper" near-field. Unless you show that "near-field" is a common term that refers to larger spatial or temporal scales I suggest removing the "hyper", as is is hyperbole.

p3 l26ff Can this be used with other queue managers apart from SLURM?

p4 l3ff Again, "hyper near-field" sounds very hyperbole and I suggest renaming it - you are talking about the region in which the well-mixed criterion does not hold.

p4 l7ff Mixing to h = 0.5 * PBLH (p4 l7) is a crude assumption with no physical meaning - if you would wait long enough you would have mixing into 1.0 * PBLH (ignoring en-/detrainment) at the top of the BL.

p4 l15ff You then derive a more complex formulation based on turbulence theory, which you propose to be better. However in the end you use h = min(h', h*), with h* the crude approximation (see above), which effectively stops dilution at 0.5 * PBLH. This seems wrong - why not dilute up to whatever your new formula gives you, maybe cap at 1.0 * PBLH? There is no reason in reality why emissions from the ground should not be mixed further up than half the PBLH.

p4 l21ff: this spatial domain should be variable and strongly dependent on receptor location, topography and meteorolgy - this should be emphasized. In general, sensitivity studies on how this new formulation performs are required.

p4 l21ff: it should be mentioned how (whether?) this method will work with intermittent turbulence and nighttime (stable) conditions (see p7 l22 where you exclude nighttime values for such reasons).

p4 l28: this should be Figure 4.

p5 l8ff: explain better: which particles go into the sigma calculations?

p5 l11: it would be helpful for the reader to see how b enters the two-dimensional Gaussian you are using for density estimation.

p5 l22ff: "improved" is based purely on visual aesthetics ("looks more similar!") - a quantitative measure would be very beneficial here.

p5 l27ff: "compensating" it might be, but only in the case here - just concede what f is: a fudge factor without physical mean.

p7 l9: "dilution correction" refers to the fudge factor f being set to 2? Explain!

p7 l20ff: You are doing the right thing by ignoring nighttime values, but I suggest you still include the nighttime data in the plots to elucidate the magnitude of this problem - this is something that all model approaches have in common and it helps to remind people that comparing nighttime values is difficult and care needs to be taken.

p8 l7: there is no appreciable "evening rush hour" peak in this figure. Neither does the model "capture" it, as it is too low throughout the day compared to observations. Remove.

p8 l1 and Figure 7 caption: there are no consistent enhancements in modelled CO2 concentrations visible in the bottom right plot that would conincide with the individual intersections shown. I disagree with the statement that the method captures these enhancements. Rephrase and state more careful what you actually can resolve.

p8 l18ff: Careful to make sure that you are not mistaking increasing resolution with the "hyper" near-field approach described earlier - this last section just shows that higher spatial resolution can be beneficial. Might want to rephrase "fine-scale approach".

Figure 6: axis labels missing, should appear at least once for x and y

Figure 5 and 9: plot x axis from 0 to 24, add nighttime values (shade to make clear you don't use them).

Figures 4, 6 - 8: Background maps at least for Figures 7-8 seem to come from Google Earth, are you sure you have the license to use and publish them?

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