

Interactive comment on “Technical Note: A new coupled system for global-to-regional downscaling of CO₂ concentration estimation” by K. Trusilova et al.

Anonymous Referee #3

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This technical note compares the modelling efficiency of the Eulerian model TM3 to the TM3-STILT Lagrangian model. Here, the system is designed for future nested inversions, including global and local scale influences of the CO₂ sources and sinks (referred as near and far fields in this paper), using consistent transport fields for both. One major point needs clarification in this paper. The TM3 model you use for the comparison is at the global scale, even if the higher resolution grid, used as input for STILT, is available. The performances of STILT are then better because you use higher resolution wind fields and flux fields as input, and you compare the results to the coarse resolution grid of TM3. Why not using the TM3 concentrations from the regional domain of interest (DoI) at high resolution? Evaluation of Lagrangian models

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are usually done by comparing the simulated concentrations by the Eulerian model to the Lagrangian model, at the same scale, using the same flux inputs. Otherwise, you compare a global model designed for large scale influence (general circulation mainly using averaged data) to a mesoscale model that will perform better at the point location. You should include results using the smaller domain of the TM3 model at higher resolution that will be comparable to STILT.

There is also another issue. In the paper, the main attempt is to show that the newly developed TM3-STILT shows better agreement with the concentration data than TM3 at low resolution. Actually, TM3 is not used to invert fluxes with hourly concentrations, but using averaged concentrations. Global scale inversions avoid misrepresentation of the local variability by averaging the high frequency temporal variability. If you expect to extract the local signal from concentrations, it has to perform better than global models but you shouldn't compare them at the same temporal resolution. The aim is to capture the local variability, giving a far field residual that is better than the averaged concentrations. If you capture half of the local variability for example, you will bias the global inversion maybe more than averaging the concentrations that subtracts 80% of the local signal (a pure guess). You should focus more on the potential improvement using your TM3-STILT system compared to actual inversions, and not concluding only that higher resolution means better model-data agreement.

P23189-3-4: add references

18-21: The Lagrangian approach includes also several issues compared to Eulerian models. Parametrization of the vertical mixing, evaluation of the footprints, amount of data for a long period of time, determination of the "surface layer",... are some of the issues of the method. Please re phrase the sentence to clarify this point.

P23190-1-3: Explain why you don't compare a regional Eulerian model to a regional Lagrangian model. Is there any justification for comparing the two methods at different scales?

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P23191-1: This sentence is very confusing. Do you use STILT coupled to the TM3 wind fields or to the ECMWF operational data?

P23191-4: "was set to 100": Unclear. Explain what "100" means (per time step? total per tower? per observation?).

P23191-16-17: what is the height of your "surface layer"? This is a technical paper, more details could be given about this parametrization of the model, or references.

P23191-1-2: How many particles are still in the Dol after 72 hours? This should be accounted as a loss of information if particles are still present in the Dol.

P23194-6-7: This is a subjective conclusion. This CFF component could potentially bias your regional inversion if the area is not dominated by the mesoscale circulation. Misrepresentation of synoptic events could lead to a biased estimate of the regional fluxes. Could you give an estimation of the errors due to the CFF component on the regional balance? This is not directly in the scope of the paper, but the potential errors due to wrong boundary conditions could affect the results of this paper, and decrease the overall performance of TM3-STILT.

P23195 (about nighttime data): Arguments 1 and 2 seem very similar. The two means that well mixed conditions are required to simulate correctly the atmospheric circulation (at least the vertical motions). One could wonder if nighttime data are of great interests considering their spatial representativity. Flux towers could potentially measure very similar footprints during the night. Inversions are interesting, first of all, because they represent a larger area at the surface than flux towers, which is not clear during the night. Is it what you mean by "are representative of the well mixed boundary layer"?

P23197-7-11: This conclusion doesn't really help the reader. What is the point you make here? Is it just saying that a $4^{\circ} \times 5^{\circ}$ is not able to reproduce the local variability? Or saying that a Lagrangian model using $0.25^{\circ} \times 0.25^{\circ}$ as input wind fields does better than a global model? The main goal of regional modelling is to capture sufficiently

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well the regional variability. Global inversions don't even pretend to, and they avoid this issue by averaging the data. Here, you have to focus on the main goal: is your model good enough to extract the regional signal? It could be worse trying to extract this signal than averaging the data. And this is the point you have to face here. If you bias the local signal, then, this will bias the global inversion too. Finally, the comparison should focus more on extracting the regional signal with hourly concentrations with TM3-STILT to extracting the global signal using averaged concentrations.

P23198-12-14: This conclusion is important. Several stations are located in mountainous areas (Puy de Dome, Schauinsland). STILT-TM3 seems not able to capture the local variability. Could you explain the major limiting factors to reproduce the local dynamics (vertical mixing, valley breeze, free troposphere/PBL exchanges)?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 23187, 2009.

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