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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: A new coupled system for global-to-regional downscaling of CO_2 concentration estimation" *by* K. Trusilova et al.

Anonymous Referee #1

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This article presents a new method for modeling CO2 concentrations based on merging a coarse-resolution gridded Eulerian model with a high-resolution Lagrangian particle dispersion model. This approach provides an alternative to nesting schemes within Eulerian models to resolve regional scale processes driving observed CO2 concentrations. Overall the authors motivate the problem well, explain their approach, and present forward model results comparing their scheme with the coarse Eulerian approach. This paper presents an interesting new approach and is worthy of publication as a technical note once the authors have addressed the following issues.

General Comments: It is very informative to see TM3-STILT compared w/ TM3, but it

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would seem a comparison with STILT and a data derived boundary as used previously w/ STILT (Gerbig et al., 2003 JGR; Matross et al., 2006 Tellus; Miller et al., 2008 ACP) would also be informative on whether merging STILT within a global model is worth the computation, or whether a more data derived boundary performs just as well. It should also be noted that model output has been used as a boundary condition before in a more rudimentary manner (Kort et al., 2008 GRL), representing a bridge between data-derived boundary conditions and the approach described here.

In section 3.1, the need to subtract the NF from the global model to create Cff is not clear. If the air parcels travel far enough back to be removed from the regional signal, then the global model output at those points should simply be representative of Cff. Even if air has circulated over the European continent previously, that wouldn't be captured in the STILT trajectories so having European sources in Cff would actually be important in accurately modeling Cff in those cases.

It is also unclear why the large-scale seasonal signal must be fit and removed with a harmonic function (section 4.2). Should this not be captured in the Cff computed from the global model with posterior fluxes? And if not captured by the global model, doesn't this failure suggest the global model is not capturing important variability and a data-derived interpolated boundary condition would do a better job?

Specific Comments:

p. 23191 l.4-5: The statistical error of using 100 particles is not going to be strictly 13%, as that number was derived using different wind fields over a different continent with different spatial flux distributions. The uncertainty is likely in that ballpark, but not limited to 13% as stated.

p. 23191 l.10-12: It would be useful to go one more step to show footprint and surface source equation for Cnf here, since with CO2 the authors are not using volume sources, but instead surface sources. A brief mention of what layer is considered to be in contact with the surface also should be added.

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p. 23191 l.18-20: This sentence needs to be restructured; it is currently misleading as to what a particle trajectory is.

p. 23192 l 1: 72 hours seems a short time to experience the full near field. Can the authors comment on this time period choice- does it take most of the 100 air parcels off the continent out over ocean?

p. 23192 l. 11: Care must be taken when describing the NF region as having a resolution of 0.25x0.25. The authors should distinguish specifically whether this refers to flux fields and/or driving met fields. It could also be noted that an LPDM is not strictly limited by the underlying met field resolution.

p. 23193 l.6-7: What version of the EDGAR database is being used?

p. 23196 l. 11, 12, 15: the quantity x and t get mixed up in this equation, and the harmonic function is referred to as both c(x) and c(t).

Section 5.2 The autocorrelation behavior at KAS is very confusing–would the authors comment on what may cause the course-scale TM3 model to show a more rapid drop off with time (matching the data) than the STILT-TM3 model?

p. 23200 l. 13: This paper did not demonstrate the ability to resolve temporal scales of less than 1 hour and this claim should be removed.

Technical Comments:

p. 23188 l.1-2: suggest change to “transport models used in simulating”

p. 23188 l.10-11: suggest change to “near-field contribution enabling the usage of different model types for global (Eulerian) and regional (Lagrangian) scales.

p. 23188 l.15-16: suggest change final sentence to “Autocorrelation analysis demonstrated that the TM3-STILT model captured temporal variability of measured tracer concentrations better than TM3 at most sites.”

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Interactive Comment

p. 23188 l.18-21: Suggest rewording this sentence.

p. 23188 l.23, 26: “This makes the model” the should be removed. “observation and that can be used for inferring” that can be should be removed. I will not comment further on this, but the authors should go through and remove such extraneous words throughout to add clarity to the paper.

p. 23198 l.6: Are the dots on the figure red or orange?

p. 23199 l 11-12: The sentence starting “Clearly, the” should be removed. It is redundant with the more detailed description of the figure that follows.

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