Atmos. Chem. Phys. Discuss., 12, C448–C451, 2012
www.atmos-chem-phys-discuss.net/12/C448/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparing Lagrangian and Eulerian models for CO_2 transport – a step towards Bayesian inverse modeling using WRF/STILT-VPRM" *by* D. Pillai et al.

T. Nehrkorn (Referee)

tnehrkor@aer.com

Received and published: 8 March 2012

General comments:

The authors present a detailed comparison between CO2 concentrations observed at a tower (for one whole month, August 2006) and by aircraft (for a single day, 20 Oct 2008), and modeled concentrations using an Eulerian (WRF) and Lagrangian (WRF-STILT) transport model. The study is an important step toward establishing to what extent the sensitivities (footprints) generated by the backward in time Lagrangian model (STILT) can be regarded as the adjoint of the Eulerian transport model (WRF). The paper is well organized, and the paper is well written. The authors took care to make

the comparison as clean as possible, and methodically isolated possible causes for the observed discrepancies between the results of the two models.

One problem with the way the material is presented is that the material dealing with the month-long tower simulation results is somewhat disjointed from the aircraft case (the latter is really much more closely linked to the Pillai et al. 2011 paper). The aircraft results are interesting in their own right, but it would strengthen the paper if the authors could show some evidence that the sensitivity tests of the aircraft case are relevant to the model-model mismatches seen in the August results. For example, are the discrepancies seen in August primarily related to differences in how fossil fuel emissions, and/or emissions at nighttime are treated in the two models (both of which are important in the aircraft case)?

Specific comments:

p. 1276, line 3: 100 particles: Did you test whether results were significantly different for larger numbers of particles?

p. 1276, line 5-7: Which version of STILT was used for these runs? Please clarify whether this version switches all particles to the large domain (irrevocably) at once, or allows separate particles to use d01/d02 winds as needed?

p.1277, lines 11-25: The model-model agreement seems to be better at upper than at lower tower levels. Do you have an explanation for this?

p. 1278, line 1 and Figure 2: Please mark the location on figure 2. It appears to be right near the d02 domain boundary. Can you comment on if and how the treatment of transport across nested domains in WRF and STILT differs and how this might affect the model-model mismatch?

p. 1282, line 15-20: Could you explain what you mean by "interpreted loosely"? I understand the problem you were facing (STILT provides footprints, but they were obviously not quite right since the modeled transport of the fossil fuel sources was not

ACPD

12, C448–C451, 2012

correct), but it's not clear how you solved this (finding a source location with a WRF plume reaching the aircraft location). Was it trial and error around the general STILT footprint vicinity, or based on the location of actual sources? If the latter, it would be helpful to show maps of the sources and/or the STILT footprints.

p. 1284, line 20 - p.1285, line 9: This paragraph does not really belong in this section. Perhaps make it a separate section (something like "Sensitivity of results to model resolution"), before section 3.1.

p. 1284, line 25: I'm not sure I would agree that the results exhibit a strong sensitivity to resolution: the curves are essentially flat past 12 km, and the two data points at 2 and 6 km also don't give an unambiguous result (with a dip for the model-model r^2 at 6km).

p. 1285, lines 1-9: I'm not sure I completely follow the argument here. Extrapolation of the curves from 2km to 0 is not necessarily representative of what one would see if one actually did those computations at those very high resolutions, since the assumptions made in the formulation of the turbulence parameterization (and others) start to break down, and one would have to use different (e.g., LES) approaches.

p. 1286, line 14: I have a minor quibble with the conclusion stated here. Reasonably close agreement between concentrations simulated by the two models demonstrated here is certainly a necessary condition for using STILT as the WRF pseudo-adjoint, but not sufficient. The definitive proof of its usefulness in this role would be a successful inversion with WRF as a forward model and STILT for the adjoint.

Table 1: Since you used the K-F cumulus scheme (I assume only in the 6km grid?), convective fluxes are not used in STILT. How, if at all, does parameterized convection affect the tracer transport in WRF? Was there enough convection during August 2006 for this to have a significant effect on the results?

Technical corrections:

ACPD

12, C448–C451, 2012

p.1268, line 10: Change "during which both models" to "during which the two models".

p.1268, line 23: Change "Thus" to "This".

p1269, line 11: Change "identically equal" to "identical".

p. 1271, line 4: also mention anthropogenic fluxes here.

p. 1272, line 11: delete "is made as"

p. 1272, line 12: delete "of the"

p.1274, line 22: Change Cini to Cbg.

p. 1276, line 22-23: omit ", but ... WRF/STILT" (The fluxes described in Section 2.1 are interpolated in the same way).

p. 1285, line 26-27: rewrite this sentence to make clear that this paper only deals with the one case, not "most of the cases" (described in Pillai et al. 2011).

Fig. 1: I suggest adding "CO2" to the box describing TM3 (to make clear that meteorological i/b conditions do not come from TM3, but ECMWF as described in the paper).

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 1267, 2012.