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Atmospheric Chemistry and Physics Discussions

Interactive comment on "COMET: a Lagrangian transport model for greenhouse gas emission estimation – forward model technique and performance for methane" *by* A. T. Vermeulen et al.

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General comments:

The paper addresses the problem of simulating atmospheric greenhouse gases concentrations at measurement sites located over continental areas, where fluxes and their spatiotemporal variability are strong. The COMET model described in the paper is a trajectory model, in which air masses are represented as columns with a time dependent circular horizontal extension, and a simple budget is done for trace gases in the mixed layer and a residual layer (the reservoir layer above the mixed layer). The model is compared to two methane datasets (Cabauw, Netherlands, and Mace Head, Ireland). Using the Cabauw data, the dependence of model performance on model parameters is investigated.

It is amazing to see how atmospheric transport can be reduced to a simple trajectory budget, where turbulent and convective transport is neglected, and also airmass history “older” that 24 hours doesn’t seem to play a significant role. However, I have the suspicion that the insensitivity of the model-data comparison to the different assumptions in the model (size of influencing regions, reservoir layer height, etc.) is site specific to Cabauw, with its very strong local fluxes and not a lot of contributions from further away sources. This local influence is evident from the results for shorter trajectories, where a significant drop in model performance at Cabauw was only observed for times as short as 24 hours (Table 6).

For sites other than Cabauw this might be quite different, so that the model can’t easily be generalized based on the Cabauw site comparison only. Given that the model lacks some of the basic transport processes (e.g. convection), I would strongly recommend to validate the model including sensitivity test also at other sites. Otherwise I don’t really see how the model can be regarded as representative.

The paper is partly hard to follow, and it would help if some further work would be invested to enhance the readability. I hope some of the specific comments can help in this regard.

Christoph Gerbig

Specific comments:

The introduction lacks some references to other relevant work on regional scale inverse modelling, including Lagrangian modelling. To give some examples of groups using Lagrangian models for inversions: S. A. Denning (CSU), J. Lin (U. Waterloo), and also myself.

Pg. 8729, line 24: “A method independent of the current so called bottom-up invento-

ACPD

6, S3410–S3416, 2006

Interactive Comment

ries that can verify these emission estimates top-down over large areas would be very helpful, and may even allow to reduce the emission uncertainties." There are methods already currently being used for this, so there is no need for the subjunctive. Later the authors refer to the work of Bergamaschi et al. [2005], but in a different context. Here I suggest to appropriately cite previous work on methane inversions.

Pg. 8731, line 2: "by employing the new continental measurement data." These new data should be introduced, e.g. by using a reference. I assume the authors are referring to the tall tower observatories in Europe within the CHIOTTO project.

Pg. 8731, line 18: Regarding spatial representation error there has been significant work also by Lin et al., 2004 as well as Gerbig et al., 2003. They use airborne CO2 observations to derive the subgrid variability. Since the authors currently refer to a paper describing tropospheric Ozone, which is photochemically produced, the Lin et al. work might be more relevant since it deals with a greenhouse gas that is emitted more similar to CH4.

Pg. 8732, line 20: The authors seem to confuse irreversibility of atmospheric mixing with the ability to derive a source-receptor relationship from a backward in time model (or adjoint model). For clarification, I would encourage to read the work on this by Thomson 1987.

Pg. 8732, equ. 3: Equ. 3 doesn't look like an equation. Also, the index n and the variable y are not explained.

Pg. 8735, line 5: "This last approach will also be investigated in this research in what will be called the uncertainty trajectories analysis." I suggest providing the name and number of the paragraph rather than the name of the analysis.

Pg. 8735, line 15: "The LPDM approach may very well introduce more suppression of coherent structures than actually observed.": Just because the implementation of turbulence might have a bias, doesn't mean that it is better to not have it implemented.

A better approach would be to address and remove the bias.

Pg. 8735, line 22: Replace “well-mixed layer“ with “mixed layer”

Pg. 8737, line 5: “the mixed-layer is build up or broken down with a typical time scale in the order of minutes” I don’t see how the boundary layer would build up in minutes, given that the driving meteorology is 3 hourly ECMWF fields.

Pg. 8737, section 3.3: The authors should better motivate the requirement of the hard to understand scanning procedure. In case of coarse emission inventory data and small AoI (small influence radii) there is no need for scanning since the emissions are homogeneous within the AoI. In case of large AoI compared to the resolution of the inventory there is also no special scanning required, since many different grid elements from the inventory contribute to the AoI. The only case where a scanning procedure might be required is in case of comparable sizes of inventory grid element and AoI. In that case, not using a scanning procedure will lead to a horizontal displacement of emissions, which in the end has to be compared with uncertainties in the trajectories. Even in that case I don’t see the necessity for the scanning procedure.

Sections 3.3 - 3.5: Can this be explained simpler as a tracer budget? This is really hard to follow.

Pg. 8737-8741: The effect of subsidence on the vertical mixing doesn’t seem to be taken into account by the approach. Subsidence leads to entrainment of residual layer air into the mixed layer, even when the mixed layer height is constant. Subsidence rates at 1-2 km (i.e. at the interface between mixed and residual layer) can be on the order of 0.5-1 km per day.

Pg. 8740, line 12: I would suggest to use the term “reservoir layer top” rather than “reservoir layer height”

Pg. 8741, line 11: What “loss of information” do the authors refer to? Is it the information loss related to mixing (i.e. growth of the entropy)?

Pg. 8747, line 2: I wouldn't describe the concentration data as input for the model, they are rather the validation data for the model.

Pg. 8747, line 2: What was the estimate for the temporal variation of emissions based on? Is there a reference for this? The authors should elaborate on this.

Pg. 8747, line 7: "Moreover, an assessment was made of differences with emission estimates from the National Communications of the different countries." Where is this assessment?

Pg. 8747, line 10: What is meant by NW Europe? Which countries are included?

Pg. 8747, line 22: "In the next chapter, the results of a sensitivity analysis for the COMET parameters to determine the best performance of the model will be shown." Which chapter is meant? 5.2, which is the next, doesn't show this. Obviously the authors mean "this section", i.e. section 5.

Pg. 8750, section 5.1: It should be discussed why the theoretically best case (case 12, with high resolution and time dependent emission data) has a slope bias of more than 10 % (a=0.882ppm/ppm).

Pg. 8752, section 5.6: I think these results should be discussed more deeply. What the results mean is that it doesn't matter to have a perfect vertical mixing in the troposphere every night (a mixing up to 10 km rather than 3 km causes the reservoir layer to be mixed to the top of the troposphere).

Pg. 8749, line 4: It is not clear at this stage that this is the cause for the smaller correlation coefficient for Mace Head. This should be revisited in section 5.3 when discussing the residual profile.

Pg. 8763, Table 3, Row 4: Is case 4 not also a 3D trajectory as case 1-3?

Pg. 8751, line 10: "Figure 12 shows the performance of COMET for the standard simulation as a function of the hour of the day.": I assume the authors mean "Figure

ACPD

6, S3410–S3416, 2006

10".

Pg 8751, line 16: Variability increases also due to the strong day-night contrast. Therefore it is not clear why the explained variability (r-square) should decrease rather than increase.

Pg. 8752, lines 2-7: The sampling scheme should be explicitly explained, or a reference should be given that explains the scheme. The reference to the ftp site doesn't provide easy access to this information. I actually see a benefit in a sampling scheme that reveals shortcomings of the model (especially the change in slope, i.e. a bias). The benefit of continuous data is that they allow isolating such deficiencies better.

Pg. 8752, line 9: "parametrisation of lateral diffusion through the circular AoI's in the COMET model" May be better to talk about "assumptions about lateral diffusion", since it is not really a parameterization.

Pg 8764: May be Table 4 can be replaced by a figure, since it is hard to read.

Pg. 8754, line 14: The use of the term "footprint" is unclear. Usually "footprint" is used to describe source-receptor relation ship.

Pg. 8754, line 18-11: There might be many more reasons for the poorer model performance at macehead, such as weaker near-field emissions etc. Vertical profile information within the boundary layer is only one factor.

Pg. 8756, line 13: Methane doesn't "dissolve" in the atmosphere, I suggest using something like "mixed"

Pg. 8756, line 27: The ECMWF model resolution has been changed from T511 (˜ 0.35 degrees) to T799 (˜ 0.35 degrees).

Technical corrections:

8728, line 26: Add comma: "e.g. Broecker (1997), and these were linked also"

ACPD

6, S3410–S3416, 2006

Interactive Comment

Pg. 8737, line 12: drop “the” in “In the this version”

Pg. 8750, line 11: “three nested grids of Table 1” I assume the authors meant “three nested grids of Table 2”

Pg. 8763, Table 3: Parameters a, b are not explained (neither in the caption, nor in the text).

Pg 8750, line 19: I would suggest replacing “predicted variability” by “explained variability”

Pg. 8758, line 19: Replace “ftp://ftp.cmdl.noaa.gov, ccg/co2/GLOBALVIEW” by “ftp://ftp.cmdl.noaa.gov/ccg/co2/GLOBALVIEW”

References:

Lin, J. C., Gerbig, C., Daube, B. C., Wofsy, S. C., Andrews, A. E., Vay, S. A., and Anderson, B. E.: An empirical analysis of the spatial variability of atmospheric CO2:Implications for inverse analyses and space-borne sensors, Geophysical Research Letters, 31 (L23104), doi:10.1029/2004GL020957, 2004.

Gerbig, C., Lin, J. C., Wofsy, S. C., Daube, B. C., Andrews, A. E., Stephens, B. B., Bakwin, P. S., and Grainger, C. A.: Toward constraining regional-scale fluxes of CO2 with atmospheric observations over a continent: 2. Analysis of COBRA data using a receptor-oriented framework, Journal of Geophysical Research-Atmospheres, 108 (D24), 2003.

Thomson, D. J., Criteria for the selection of stochastic models of particle trajectories in turbulent flows, J. Fluid Mech., 180, 529- 556, 1987.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 8727, 2006.

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6, S3410–S3416, 2006

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