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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Comparing atmospheric transport models for future regional inversions over Europe. Part 1: Mapping the CO_2 atmospheric signals" *by* C. Geels et al.

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General comments

The paper compares the simulation of CO_2 for July and December 1998 across Europe from 5 models run with different domains and resolutions. The comparison is made for monthly mean distributions and synoptic and diurnal variability. The aim is to assess which features of the observations are most reliably modelled to provide information for inversion studies of how to use continental data. In general the paper achieves this aim with a series of recommendations being presented in the abstract. However I wonder whether the recommendations are rather conservative - there seems to be a tendency

to reject data that isn't well modelled rather than looking at how the models could be better used to fit that data.

For example, recommendation 1 against high altitude sites may be fixed by sampling the model at a lower level than those shown here (as suggested by Fig 8). In recommendation 2 the diurnal cycle is rejected although this carries useful information about the biosphere fluxes. Perhaps it is not possible in this paper, but we need to determine whether all night-time data is difficult to model or whether it is only the extreme high concentration events. One test of this would be to plot the median diurnal cycle rather than the mean. Recommendation 3 is fine but implies the need for tall tower observations which are unlikely to be widely available. Hence instead we need to establish what would provide better simulations of near-surface data. Is it just vertical resolution? When comparing diurnal and synoptic variations you interpolated to the station altitude. Did this mean you weren't using the lowest model level for the low altitude sites? Would using the lowest level help the comparison? For recommendation 4 it would be useful to indicate what horizontal and vertical resolution you think is necessary to capture e.g. Spain since many global transport models are now being run at 1-2 degree resolution.

I find the results from the LMDZ simulation rather surprising, given that within its zoom region the horizontal resolution is not so different from e.g. HANK. (I assume that the fluxes used for LMDZ within the zoom were at the higher resolution not at the global resolution.) The implication seems to be that the vertical resolution is critical since LMDZ has the thickest surface layer and the smallest number of layers below 1500m. Perhaps this needs to be stated more explicitly. Also how do you account for LMDZ producing the correct diurnal amplitude at Pallas? Without an explanation being offered in the paper, it does make me wonder whether there is a problem with the LMDZ simulation.

This type of model-data comparison paper necessitates careful and relatively detailed presentation as has been done here. Unfortunately for the authors, it also means that my comments are likewise rather extensive and detailed.

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Specific comments

Abstract: In the second paragraph it is noted that the differences at high-altitude sites are less pronounced (which I interpret as better) but in the third paragraph the recommendation is that low-altitude sites are preferable to high altitude sites. This appears contradictory. As noted above, I think the main recommendation that you can draw about the high altitude sites is that it is not appropriate to simulate them with a model level at the real altitude of the site. As you show from the REMO example for CMN, level 4 is a much better fit to the observations. Ideally a similar assessment should be made for all models and all mountain sites and both seasons, but perhaps you do not have the necessary information saved from the simulations.

P3713, line 5: I do not believe that the partitioning between northern land regions is still controversial (as it has been) though uncertainties remain. Gurney et al. (2002) did much to explain why a large sink was found by Fan et al. (1998) which was not seen in other studies.

P3714, line 12-17: You might want to mention here that the TransCom group has an experiment underway to look at synoptic and diurnal variations between models (Law et al., 2005).

P3716, section 2.1: It is quite difficult to visualise the domains used in the different models from table 1. Would it be worth providing a figure showing particularly the high resolution regions for each model? Plotting the observing sites on the figure could also be useful.

P3718: line 13-19: I understand from this that seasonal and diurnal variations of fossil emissions were neglected but it wasn't clear to me whether the difference between 1990 and 1998 emissions was accounted for.

P3720, line 11: The statement that the lower boundary conditions were 'identical' is perhaps misleading. It would be useful to comment here on how the fluxes were re-

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gridded to the different model resolutions and that models with grid > 1 degree will lose flux peaks relative to higher resolution models, i.e. while the net flux across Europe may be the same, there will be larger spatial variability of fluxes in the higher resolution models (up to the resolution of the fluxes). It would also be good to confirm that in the high resolution windows the fluxes are at the higher resolution

P3721, line 3-6: I suggest you list/tabulate the Mace Head concentrations for each model and month, or at least indicate a range across models so that the reader has some way of assessing what the impact is of presenting the plots relative to Mace Head.

P3721, line 16: the reference here to the 993.5 hPa level (and in Fig 1 caption) seems to be confusing since you are interpolating to 11 hPa above the ground and presumably the surface pressure is very variable across Europe due to altitude. Since the model vertical coordinates are sigma or hybrid, perhaps you should indicate what sigma/hybrid level you interpolate to?

P3722, line 10-11: the argument about NEE and diurnal rectification is fine here but I think you need to be careful about generalising this to a difference 'between regional and global models' since TM3 shows large positive concentrations in July similar to the regional models.

P3723, line 15: I would drop 'horizontal rectification'. I interpret rectification as when you get a non-zero average concentration from an average zero flux. Here you are only showing day-time concentrations so talking about rectification doesn't seem relevant. Your explanation of the process seems fine though - drainage of night-time high concentrations over the ocean in the morning and relatively little mixing or sink removal over the ocean.

P3724, line 9: 'missing in the coarse resolution model simulations'. Again I think you need to be a bit careful in your generalisation here since the zoom region of LMDZ is reasonably high resolution in the horizontal.

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P3724, line 14-16: For December, the difference between the REMO and LMDZ simulations looks to be larger for the fossil component than the NEE component despite the respiration source being larger than the fossil source. Do you have any idea why this is?

P3724, line 17-22: 'For the NEE component ...' I don't understand this sentence or the next one - is it that the diurnal rectification in July means that there is little difference between July and December averages in REMO but the smaller diurnal rectification in LMDZ means that there is a larger difference between July and Dec? Perhaps if you indicated an example region and gave the approximate concentrations in each case, it would be easier to follow. Also if there were big differences between models in the monthly concentrations at MHD which you subtract each month, would this alter the reasoning?

P3725, line 5-6: In the figure caption it says the MHD data were from 2001. In order to calculate the difference in observations, how was the difference between 1998 and 2001 accounted for? I assume that no observations are shown for CBW, JFJ, and TVR because they were not available for 1998. Given that you are focussing on differences from MHD, could another year be used, at least to give an approximate observed value?

P3725, line 20-23: The sentence 'In Fig 4 it is apparent ...' seems too positive - I interpret 'reproduce correctly the fossil fuel rise' as getting the sign and the magnitude right. Your next sentence qualifies this to note that it is only the sign that is correct but I think you should modify the first sentence.

P3726, line 12: overestimated except by LMDZ - how do you interpret this result given that other results suggest that LMDZ has too much vertical mixing at night? Are the fossil emissions too large for this region in summer?

P3726, line 17: Also note higher concentrations at HEI because night-time (only one model is shown for HEI, are the others off the plot?)

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P3726, line 27: ‘strong vertical mixing’ - this seems surprising to me, I would assume more vertical mixing weakens horizontal gradients and gradients between the surface (mhd) and altitude (prs/jfj) sites. Perhaps this component is harder to interpret when plotted relative to MHD because MHD is nearest to the sink and may therefore show most variability across models. Also how might that variability be influenced by the boundary/initial conditions?

P3727, section 4.2: Figure 5 is very difficult to read which makes this section hard to assess. Line 27 indicates that error bars are shown for LMDZ but should it be TM3? If it is TM3 why is the lowest error bar lower for TM3 than DEHM when the surface layer thickness is similar for the two models? The LMDZ model line appears to end at 3000m. Is this correct? Perhaps use colour.

P3729, line 5: I think the Hungarian tower has data from different levels on the tower. Which height is used here? Would the model results compare better with other levels on the tower? Would this support your recommendation for a better agreement with data $\sim$400m above the surface?

P3731, line 11: The flux resolution perhaps contributes as well - how would the mesoscale model results compare if they were forced with fluxes at the resolution of TM3?

P3732, section 5.3/p3755-3756 Figures 12 and 13: These figures are difficult to read - when the relative standard deviation is low, it is hard to read the correlation value. Since it has already been demonstrated that the high altitude sites are not well simulated at the selected model levels, I suggest removing prs and sch from this plot. I also think it would be more useful to put all the model results for one site on the same plot so that a more direct comparison can be made.

P3733, line 10: the fact that the results are broadly similar for the hourly data and the day-time selected data is rather sobering. It suggests that although the model agreement may be better for the day-time data, the signals of interest are that much

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smaller that the better agreement may not be of as much value as first thought. I think this finding may weaken your recommendation 2 in the abstract.

P3734, line 25-28: I think it is useful to acknowledge that only using afternoon data discards valuable information in the observations that could be used for budget studies. While high-resolution models around each site might be necessary, I think there are still improvements to be gained using current generation models if the model output is used carefully. However this probably does require site-by-site assessment of model capability rather than applying generic criteria to all sites. This is a large task and will, I suspect, require a cooperative effort between modellers and those running measurement programs.

P3750, Figure 7: This figure is difficult to read - is it necessary to show the whole month or would 7-10 days be sufficient? Are the data plotted relative to a monthly mean of zero? Would plotting relative to day-time average be better?

Technical corrections

P3711, line 10: suggest ‘flasks sampled on aircrafts’ instead of ‘flask samples sampled on aircrafts’

P3711, line 21: drop ‘in’?

P3712, line 3: Insert ‘The’ before ‘Main’

P3712, line 10: ‘robustly’ instead of ‘robust’

P3714, line 14: suggest Gurney et al 2003 is the better reference than 2002 because it includes the comparison of forward simulations from each model

P3716, line 19: The LMDZ zoom is listed in the text as 0.5x0.5 but in Table 1 as 1.2x0.8. Which is correct?

P3717, line 8: should be Christensen et al, 2004

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P3717, line 20: should ‘Simmons’ be a reference - if so it is not listed.

P3719, line 24: insert ‘a’ before ‘few’, delete ‘in’ before Aalto reference

P3720, line 9: delete ‘pr’ from fossil units

P3723, line 12: suggest ‘reduced further for daytime sampling at a few hundred meters above the ground ...’

P3723, line 13: LMDZ also shows this behaviour around Italy but is not mentioned.

P3723, line 24: I had to look up where the Iberian Peninsula was, perhaps you should add Spain/Portugal in brackets for those of us whose European geography isn’t so good.

P3726, line 1: I don’t think you need the $\pm$. From the graph 8-10ppm seems to cover the full range.

P3726, line 2-3: Swap LMDZ (4 ppm) and HANK (14 ppm)

P3726, line 16: Suggest adding ‘day-time’ before biotic.

P3726, line 22: suggest ‘diurnal mixing’ instead of ‘the diurnal rectification’

P3727, line 11: should this either be the ‘underestimation of the depletion’ or the ‘over-estimation of concentration’?

P3731, line 6-7: replace ‘mid-day selected and daily averaged’ with ‘day-time averaged’

P3733, line 10: is ‘standard deviation’ meant here rather than ‘amplitudes’?

P3735, line 22: space between ‘carbon’ and ‘dioxide’

P3735, line 27: Ramonet not Remonet

P3736, line 4: Should be ‘Le Quéré, C.’

P3736, line 21: atmospheric misspelled

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P3736, line 22: presumably page range should be 8543-8567

P3737, line 1: ‘atmosphere’ not ‘atmsophere’

P3738, line 11: ‘uptake’ not ‘uotake’

P3738, line 21: atmospheric misspelled

P3738, line 27: some author initials are incorrect. Should be Taguchi, S., Taylor, J. A., Trudinger, C. M.

P3739, line 18: should be Rayner, P.J., Enting, I.G., Francey, R.J. and Langenfelds, R. L.

P3741, Table 1: It would be helpful if you could add a note to the table to give an indication of how the resolution in degrees and km compare, for example ‘At European latitudes 0.5 deg $\sim$ n km’. This would facilitate comparison between models. The resolution for the LMDZ zoom region is different from that given in the text.

P3743, Table 3: It might be useful to list the sites in the same W-E order that they are plotted in Fig 4.

P3748, fig 5 caption: Should it be TM3 not LMDZ. Last line ‘at’ instead of ‘a’. Figures 7, 9, 10, 11: In the caption you should perhaps indicate what Bg and NBg stand for in relation to the observations.

P3755, 2nd last line of caption: ‘off’ instead of ‘of’

P3756, caption line 5: ‘off’ instead of ‘of’

References

Fan et al., A large terrestrial carbon sink in North America implied by atmospheric and oceanic carbon dioxide data and models, Science, 282, 442-446, 1998.

Gurney et al., Towards robust regional estimates of CO_2 sources and sinks using atmospheric transport models, Nature, 415, 626-630, 2002.

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Interactive Comment

Law, R. M., Peters, W., Rodenbeck, C., Bruhwiler, L., and TC modellers. (2005). An intercomparison of the diurnal and synoptic behaviour of global transport models. In: 7th International Carbon Dioxide Conference: abstracts, Broomfield, Colo. Boulder, Colo.: Committee of the Seventh International Carbon Dioxide Conference. 2 p.

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