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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.

C. Geels et al.

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Rachel Law has made a very thorough review of our paper, with many relevant comments. We reply to each in turn in the following:

General comments by the reviewer: The paper compares the simulation of CO2 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

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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?

Answer: We thank the reviewer for the very constructive suggestions about the recommendations. Based on this we have reformulated them as follows:

"Main recommendations resulting from the study for constraining land carbon sources and sinks using high-resolution concentration data and state-of-the art transport models through inverse methods are given in the following.

1) High altitude stations are difficult to represent in present state-of-the art models due to the relative coarse horizontal and vertical model resolution which gives an insufficient representation of the surface topography. Furthermore, the flow fields around mountainous sites are extremely difficult to simulate and therefore the transport patterns from the source areas to the mountain site are difficult to catch. The recommendation is therefore that low altitude stations presently are preferable in inverse studies. If high altitude stations are used then the model level that represents the specific sites should be applied.

2) The modelled height of the PBL has substantial influence on the concentration levels. This parameter is nevertheless very difficult to simulate correctly and this is one of the main sources of uncertainties in transport models. Especially during night time the

height of the PBL can be uncertain with several hundred percent. During day time the PBL height is better resolved by the models and hence less uncertain. Furthermore the parameterizations of the PBL height are mainly designed for day time applications. The recommendation is therefore that afternoon values of concentrations can be represented best by current models and should be used to constrain large-scale sources and sinks in combination with transport models,

3) The vertical CO2 profile is difficult to simulate, especially near the ground due to the surface exchange. In general the models results tend to be well mixed below the PBL height and therefore the model results tend to represent tower measurements sampled several hundred meters above ground better, compared to surface measurement. The recommendation is therefore to emphasize the use of tower data in inverse studies.

4) The traditional coarse resolution transport models can not resolve CO2 distributions over regions of the size of for example Spain and thus seem too coarse for interpretation of continental data. Our results indicate that a horizontal resolution of max. 1 by 1 degree combined with a vertical resolution of max. 100 m for the lowest layer, should be able to capture such distributions. The recommendation is therefore to use higher resolution models in future studies."

Reviewer: 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?

Answer: When we setup this intercomparison we decided to select the model results at the station altitude. For the low resolution models this will be the lowest model level at most low altitude sites. For the higher resolution models it might be higher levels at some stations. What we have learned is surely that for each site one has to check which layer to use and we want to make that a clear message of the paper.

Reviewer: 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

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transport models are now being run at 1-2 degree resolution.

Answer: Good suggestion. We have included this in the recommendations, see above.

Reviewer: 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.

Answer: LMDZ has about the same resolution as HANK model over Europe but the vertical resolution in the PBL is much coarser (4 levels below 1500m against 10) and the parametrisation of turbulent diffusion is different. This appears to be critical to represent diurnal cycle of CO2 at continental sites. This will be mentioned in the final version of the text.

For Pallas station we agree with the reviewer that the fact that LMDZ was producing a much different diurnal cycle than for other sites was suspicious. Indeed, after checking all the analysis chain, we found that the model level extracted for Pallas in LMDZ was systematically the first level (ground) and not the level given by altitude interpolation (standart level). Therefore, it was overestimating the diurnal cycle compared to other sites in LMDZ. This was due to a test we performed with LMDZ. This test should not have appeared in the final plots. We corrected this error and re-generated all the plots where Pallas was involved (figures 4,6,7,9,10,11,12 & 13), using the standard level of the model.

Specific comments by the reviewer: 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. 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.

Answer: We fully agree with the reviewer and have now included this in the recommendations. However, we have not been able to include this fully in this paper.

Reviewer: 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.

Answer: We agree and will rewrite this.

Reviewer: 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).

Answer: Good idea, this will be mentioned.

Reviewer: 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.

Answer: We made the description of the models short and have chosen to visualize the results on a common domain for easy comparison. We have also chosen to leave out e.g. plots of the domains, since the domain characteristics are less important here. We instead included references to papers where this information can be found. This is also done for the observation sites - in order to leave space for the detailed description of the results.

Reviewer: 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.

Answer: Unfortunately we did not account for this difference.

Reviewer: 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-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

Answer: We agree that the term "identical" can be misleading here. We will rewrite this and include some comments about the fluxes in the high and low resolution domains, respectively.

Reviewer: 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.

Answer: This is a good idea - we will add the range across the models for these values.

Reviewer: 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

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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?

Answer: The reviewer is right; the reference to 993.5 hPa level was a mistake. In Figure 2 we have plotted fields at 11 hPa above the ground - but had not yet properly changed the titles. Sorry for that, we have changed the title now. The interpolation was carried out from the different sigma/hybrid levels in the models to the 11 hPa level. Otherwise the different lowest nominal levels of the models lead to artificial differences in the concentration fields.

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

Answer: This is true - we will rewrite this in the final version of the paper.

Reviewer: 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.

Answer: Good point - we will reformulate this as suggested.

Reviewer: 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.

Answer: We agree and will rephrase this.

Reviewer: P3724, line 14-16: For December, the difference between the REMO and

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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?

Answer: We believe that this is related to the distribution of the emissions/fluxes. The local fossil emissions can be larger than the biospheric fluxes (which are smeared out) and LMDZ tends to have a larger vertical mixing, which leads to a relatively lower bio/fossil signal than in the REMO model.

Reviewer: 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?

Answer: We agree that it is not really clear what we mean here. We will therefore reformulate as follows:

" For the NEE component the difference between summer and winter is small at some inland regions (e.g. North of the Black Sea) when using REMO. This is believed to be caused by the rectification effect and is in agreement with the damped seasonal cycle observed near the ground at continental low elevation sites (Bakwin et al., 1998). The seasonal difference will, however, depend on several model parameters and it will in particular depend on the PBL-free troposphere exchange both regarding magnitude of the exchange and the seasonal contrast. This is reflected in the modelling results based on LMDZ for which the difference between July and December is larger. "

Reviewer: 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

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1998 and 2001 accounted for?

Answer: The figure caption was in fact unclear. Year 1998 was used for CO2 observations, but 14CO2 observations were not available for 1998 and year 2001 was used instead for MHD. This only makes a small difference for 14C as there is no large trend in the atmosphere like for CO2. This will be mentioned in the paper and the figure caption is now clarified:

"Fig. 4. The monthly averaged West to East longitudinal gradients across nine European monitoring sites displayed relative to the marine background conditions at MHD. Based on daytime values, except at HEI where nighttime data are used. Four panels are shown: 1. the fossil fuel CO2 component as simulated and observed (based on 14 C observations), 2. the simulated biospheric component, 3. the simulated oceanic component, and 4. observed and simulated total CO2. Note that for MHD 14C data we have used the year 2001 as no data are available for 1998. Note also that the scales are different for each component."

Reviewer: 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?

Answer: For total CO2 at CBW-JFJ-TVD, data were not available in 1998. Using another year to get an approximate observed value would require to subtract MHD for the same year because of annual increase of atmospheric CO2 (˜1.5 ppm/yr). This would really complicate the comparison between the stations as the year to year variation in total CO2 concentrations are also quite large. We thus choose to display only the observations when available for total CO2.

Reviewer: 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.

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Answer: True - we will reformulate this.

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

Answer: For the biosphere plot and the model at HEI: The other models were out of the chosen range (-8,4), with values much higher (above 10 ppm). This was intended to emphasize on the model to model differences at other sites. We agree that it is somehow misleading and we thus change the plot and increased the y-range to include all models at HEI. Note that we have chosen the night time values for HEI because the 14C data were only available at night. LMDz is thus an outlier as it does not reproduce the night-time accumulation see in the other models (both for the biosphere and fossil components). In summer we agree that the results at HEI suggest that the fossil emissions are too large for this region in summer. This will be mentioned in the paper.

Reviewer: 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?

Answer: We agree with the reviewer that this explanation of “strong vertical mixing” is not clear. The possible explanation should probably be found from a combination of several processes and the way they are described in the different models. We will therefore reformulate the text in the paper to “Relatively small longitudinal gradients ($<$2.5 ppm) are seen for the ocean component and the gradient tend to be most pro-

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nounced in LMDZ in July. In December the corresponding gradients are quite similar in the model results with respect to the ocean component."

Reviewer: 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.

Answer: Thanks for pointing out some mistakes in this plot - we have corrected them now. The plot looks better now, when printed.

Reviewer: 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 _400m above the surface?

Answer: Hegyhatsal has 4 levels above ground (10m, 48m, 82m, 115m). We used 115m level as representative of a larger area than lower levels. Lower levels are more sensitive to local sources that are not really reprensented with the model grids used (typically 50-300 km). Models have also generally 1 to 2 layers below 100 m, which makes it difficult to perform a relevant model/obs comparison with lower levels of HUN tower. We will clarify this in the final version of the paper.

Reviewer: 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?

Answer: It is a good suggestion for an experiment, but we haven't tried it in this study.

Reviewer: 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.

Answer: We agree that these plots include a lot of information, which makes them somewhat hard to read. Removing prs and sch will not change the plots much and we do not think that putting all the models results for one site on the same plot will be an advantages. It really depends on what you wants to compare - these plots gives, in our opinion, an overview of how the different models represents the different sites.

Reviewer: 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 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.

Answer: It is important to bear in mind that the statistics related to hourly data will be strongly influenced by the pronounced diurnal cycle. In general the spread in between models and between models and measurements is reduced when only including afternoon values. This is why we recommend the use of such data for budget studies.

Reviewer: 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.

Answer: In the conclusion we say that “CO2 data from the afternoon hours are therefore found more appropriate for budget studies.” We agree that we should add that we

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thereby discard important information included in the full time series. We will do that in the final version of the paper.

Reviewer: 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?

Answer: For technical reasons we have chosen to plot the data relative to the monthly mean - in order to have them on the same scale. It is true that the figure is somewhat “busy”, but again it depends on what you want to show with the figure. We think that it is worth showing the whole month in order to illustrate the variability of the CO2 concentration at different time scales.

Technical corrections - will all be included in the final version of the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 3709, 2006.