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Geoscientific Model Development Discussions

Interactive comment on "Sensitivity of simulated CO₂ concentration to regridding of global fossil fuel CO₂ emissions" *by* X. Zhang et al.

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We appreciate the referee's comments and suggestions. Our responses to the specific points are listed below. Main points: First, the authors argue that "dynamical consistency" is important. From the start (abstract) it is not totally clear what is meant by this. My impression is that the authors claim that "land/sea emission" and "land/sea mixing" should be strictly separated, and that addition of land emissions over a coarse-resolution ocean grid cell may lead to errors. This might be true due to the fact that diurnal mixing over land is distinctly different from ocean mixing. However, the authors fall short in explaining and exploring this issue in the paper. We are grateful for the

reviewer's detailed comments and suggestions.

We agree that more explanation is needed to clarify what is meant by "dynamical consistency" in our manuscript. We have modified text in the "Abstract" and "Introduction" sections.

In the "Abstract" section, we have made the following modifications: "Regridding of fossil fuel CO2 emissions (FFCO2) from fine to coarse grids to enable atmospheric transport simulations can give rise to mismatches between the emissions and simulated atmospheric dynamics which differ over land or water. For example, emissions originally emanating from the land are emitted from a gridcell for which the vertical mixing reflects the roughness and/or surface energy exchange of an ocean surface. We test this potential "dynamical inconsistency" by examining simulated global atmospheric CO2 concentration driven by two different approaches to regridding fossil fuel CO2 emissions."

We also added more explanation and modified the paragraph in the "Introduction" section as:

"Transport models typically distinguish the surface characteristics of a model gridcell in broad classes such as land versus water or urban versus rural. These classifications are important to both the emissions of FFCO2 and atmospheric transport above and/or downwind of particular gridcells. For example, modeled atmospheric transport processes such as mixing with the planetary boundary layer, convection, synoptic flow, and even general circulation are influenced by the gridcell surface characteristics (e.g. surface roughness or energy budget). Global tracer transport models usually discretize surface gridcells at a lower resolution than those of fossil fuel CO2 emission data products produced in recent years and, thus, the emissions need to be aggregated to the coarser model resolution. In this process, the transport model gridcells with less than 50% land geography are usually designated as water gridcells. Emissions present on the finer FFCO2 grid, resident within the coarser model water grid cell are thereby

mixed into the atmosphere according to vertical mixing processes characteristics of ocean or lake transport dynamics."

Only in the very last paragraph they mention "tile" approaches in Earth system models. However, the model used in the paper (PCTM) uses MERRA re-analysed winds and it would be logical to outline in the paper the way "ocean" and "land" are separated in this model, with particular emphasis on the land/ocean-surface scheme. Specifically, they might show how vertical mixing characteristics change when going from land to sea (K-diffusion profiles?).

We agree that an explanation how the PCTM and/or MERRA reanalysis differentiate the "ocean" and "land" boundary layer could provide a more logical understanding of the issue. There is a big difference between land and ocean diffusion coefficients. We have added statements in the "Method" section to explain the scheme as:

"The vertical mixing profile in PCTM includes two dynamical processes: turbulent diffusion in the boundary layer and convection. The two processes are parameterized following the MERRA model – which differentiates the vertical mixing in the boundary layer over land and ocean by using different surface heating, radiation, moisture, roughness and other physical factors in the eddy diffusion coefficient (Kh scheme) (Louis et al., 1982; Lock et al., 2000; McGrath-Spangler and Molod, 2014). Considering the purpose of this study, a check of the diffusion coefficients of the MERRA meteorology is performed. The result shows a significant difference between land and ocean planetary boundary layers, indicating the existence of different vertical mixing characteristics between the two boundaries (see figure in the supplement)."

Second (and related): Although the paper focuses on the global scale, the problem at hand plays at the regional/local scale, as illustrated in figure 1. However, the findings at station TAP are treated in a rather hand-waving way, glossing over the remarkable fact that the simulated mixing ratios in the "experiment" simulation are lower than in the control, while in general the opposite would be expected (for land stations at least,

since the emissions are transferred to land locations). A more local focus of figures 2 and 3 would therefore be of large value for this paper, e.g. highlighting the specific situation around station TAP.

We agree that the "dynamical inconsistency" impacts the distribution of emissions much more at the local-to-regional scale rather than global scale. Considering the purpose of this study is to call for the attention of this topic, in section 3.2 and section 3.3 we choose to discuss the opposing signals (shown in Figure 2 and Figure 3) generally at the global scale, instead of focusing only on one specific region. We agree that highlighting the location of station TAP in emission and concentration fields could clearly present the significant effect of "shuffling the fluxes from ocean grid to land grid" on this station. We have added an additional figure (Fig. 5) to show the phenomenon in the manuscript. We also added additional explanation of Fig. 5 in the text: "The TAP monitoring station is located in the negative portion of the emission dipole (emission difference: -24.1 TgC/gridcell/yr) corresponding to the positive emission portion on adjacent land gridcells, as displayed in Fig. 5a. Consistently, the TAP site lies in the negative portion of the annual mean surface CO2 concentration field (-6.60 ppm) opposing to the positive portion on land (Fig. 5b)."

We would like to put Fig. 5 here:

Fig. 5. Regional fluxes difference and simulated annual mean surface CO2 concentration differences (experiment minus control) and the location of TAP monitoring site. a) flux difference; b) concentration difference. Blue stars mark the location of TAP site.

Minor comments: P 3577, line 20: Peylin et al. (2013): reference wrong or missing. We have corrected this reference.

P 3578, line 3: convection synoptic flow → convection, synoptic flow. We have modified it.

P 3578, line4: "dynamic inconsistency": "seems that the authors are promising a study

to the interaction between emission and atmospheric flow at the km-scale. For instance, they write: “the global tracer transport models used in this study do not attempt to resolve transport dynamics over urban vs. rural areas”, thereby suggesting that the models do attempt to resolve transport dynamics near coastal areas. This might be the case, but it requires explanation of the way the dynamics in the model is driven, e.g. how does the surface scheme deal with mixed land-sea grid cells.”

We have added more explanation in text of the “Introduction” section regarding this point. We also added a figure in the supplement that shows the difference of the diffusion coefficient between land and ocean to clarify the transfer of vertical mixing from ocean to land gridcells. The detailed statement is as:

“Transport models typically distinguish the surface characteristics of a model gridcell in broad classes such as land versus water or urban versus rural. These classifications are important to both the emissions of FFCO2 and atmospheric transport above and/or downwind of particular gridcells. For example, modeled atmospheric transport processes such as mixing with the planetary boundary layer, convection, synoptic flow, and even general circulation are influenced by the gridcell surface characteristics (e.g. surface roughness or energy budget). Global tracer transport models usually discretize surface gridcells at a lower resolution than those of fossil fuel CO2 emission data products produced in recent years and, thus, the emissions need to be aggregated to the coarser model resolution. In this process, the transport model gridcells with less than 50% land geography are usually designated as water gridcells. Emissions present on the finer FFCO2 grid, resident within the coarser model water gridcell are thereby mixed into the atmosphere according to vertical mixing characteristics of ocean or lake transport dynamics.”

P 3578, line2: to a coarser model gridcell. I suggest, “to the coarser model resolution”, or “to coarser grid cells”.

We have modified the sentence.

P 3578, line 4: “the minority land geography dictates a water gridcell but with the presence of emissios”: unclear. Do you refer to gridcells with less than 50% land? If so, what do you mean with “dictates”? Do you mean that the emissions that occur over land overwhelm the emissions that occur over sea (e.g. shipping)? Also: what do you mean with: “with its accompanying ocean or lake transport dynamics”? Do you mean that the surface characteristics that drive e.g. PBL dynamics are characteristic for water? Maybe say so, because I was confused by emissions from the transport sector (shipping). Anyhow, it might be good to spend a few words on “shipping” emissions, and how these are treated in the reshuffling procedure.

Yes. For “the minority land geography dictates a water grid cell but with the presence of emissions”, we mean that the grid cell with less than 50% land geography is dictated in water grid cell in PCTM. Since the emissions from ship over the sea are not included in FFDAS used in this study, we didn’t mean to compare the emissions over land to those over sea. For “with its accompanying ocean or lake transport dynamics”, we meant that the water grid cell with minor land geography enter a vertical mixing process of in water gridcell with characteristics of ocean or lake transport dynamics.

We have revised the sentences more clearly, as follows:

“In this process, the transport model gridcells with less than 50% land geography are usually designated as water gridcells. Emissions present on the finer FFCO2 grid, resident within the coarser model water gridcell are thereby mixed into the atmosphere according to vertical mixing characteristics of ocean or lake transport dynamics.”

P 3579: line 4: “and the adjustment method used the regridded emissions”?? I think: “and the adjustment method used to re-grid the emissions”.

We have modified the sentence.

P 3580, line 20: The simulation is run for four years, driven by 2002 MERRA meteorology... Maybe it is good to explain why for this study a three year spin up

is necessary. If I understand well, only fossil fuel emissions are simulated, so you expect a linear increase in mixing ratios. However, the fossil fuel signal has to propagate to the remote atmosphere, I guess.

We have added the following sentence as explanation:

"In the model simulations, tracers are propagated in the atmosphere to reach a state of equilibrium under the applied forcing. This is achieved with a four-year simulation in which the first three-year period is used for spin-up and the last year is used for analysis."

P 3581, line 18: Fj is its emissions →Fj is its emission. I note in figure 1 that the "emission" is defined in units of kgC/(m2.s). Is the amount that is shuffled in the same unit? If so, how do you assure conservation of total emissions? It might be good to spend a bit more words on this issue.

The number in Figure 1 should represent emission amount. We made a mistake in stating the unit of emission when plotting the figure. Actually, we use the mass amount in the shuffling process.

We have corrected the figure.

P 3581, line 20: whose corners intersect at a corner → those that share a corner with the shuffled cell.

We have modified the sentence.

P 3582, line 2: "emissions fields" should be "emission fields".

We have modified it.

The discussion of the emission fields (experiment versus control) is interesting. Especially the comparison with country totals, or percentage of the global total emissions is clarifying. This makes me wonder why the authors show the emission increments as TgC/(cell.yr) (or kgC/(m2.yr)). The first unit depends on the model resolution (did

they test different resolutions?). Also the fractional increase of the land gridcells in the "experiment" emissions remains hidden, while this seems a relative quantity. Now the authors only present the globally integrated values that are compared to country totals. I realize that a downside of showing fractional changes is that regions with small emissions will also have large fractional changes. But one could try to present the "experiment" and "control" emissions along coastal boundaries as a histogram, with differences by emission range (e.g. coastal land cells with emissions between xx and yy TgC/(cell.yr) receive zz TgC/(cell.yr), which is on average a xxx % increment.).

Given the aim of the study is to promise the "dynamical inconsistency" issue, we chose to show the map of absolute emissions difference because we believe that it is much more informative on the magnitude and geographical distribution of the impact, since we believe that the modeler and measurement people concern how much and the impact is and where it happens the most.

P 3582, line 29: It is unclear why the city of Groningen (not a coastal city) is in the example list. What is also interesting is the fact that in tropical latitudes the impact seems to be smaller (hard to judge though from the figures). This might possibly be due to the stronger vertical mixing in the atmosphere, but this requires further quantitative analysis. Anyhow, an interaction between concentration impact and atmospheric stability would be expected and it would be useful to explore a bit further.

As we have double-checked, the monitoring site Groningen is located in a land grid cell in PCTM. Thus, a relatively large concentration impact is obtained on this monitoring site.

Indeed, tropical America and Africa show smaller magnitude compared to tropical Asia and coastal regions in Northern Hemisphere. This is mainly caused by the relatively small FFCO2 emissions in the two tropical regions associated with less energy consumption. The strong vertical mixing in the tropics might also play a role in the small signal. We agree that an exploration between concentration impact and atmospheric

stability would be helpful to understand this regional phenomenon. A future study focusing on quantifying this interaction will be expected.

P 3584, line 17: Concerning the TAP station. “The TAP monitoring station is located in the negative portion of the emission dipole displayed in Fig. 3”. This would imply that the TAP station is allocated to an ocean/lake grid cell? I think it would improve the paper further if a figure (maybe use figure1?) is added to outline the specific case for TAP (where is the station? How are the emissions from large cities shuffled? How do the detailed CO2 concentration fields differ?) From the global lot (figure 3) it is hard to discern the TAP location in the “emission -difference” dipole.

Thanks for the suggestion. We agree that an additional figure that highlights the location of station TAP in emission and concentration fields could be helpful in showing the significant effect of “shuffling the fluxes from ocean grid to land grid” on this station. We have added an additional figure (Fig. 5) for this purpose.

We also added additional explanation of Fig. 5 in the text: “The TAP monitoring station is located in the negative portion of the emission dipole (emission difference: -24.1 TgC/gridcell/yr) corresponding to positive emission potion on adjacent land gridcells, as displayed in Fig. 5a. Consistently, the TAP site situates in the negative portion of the annual mean surface CO2 concentration field (-6.60 ppm) opposing to the positive portion on land (Fig. 5b).”

What is also noteworthy is the change in behavior of the TAP time series in figure 4b. The earlier part shows a high frequency behavior that disappears in the later times.

We thank the reviewer for finding this and we have modified the figure. We double-checked the original afternoon timeseries and found that we made mistake in plotting the figure. We have corrected the mistake in the new Figure 4. The synoptic-induced variation can be seen through the year in the corrected figure.

We have modified the sentence that states this phenomenon: “High-frequency signals

are also shown in the hourly timeseries through the year for the TAP site, indicating the impact of atmospheric transport synoptic-scale.”

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C1532/2014/gmdd-7-C1532-2014-supplement.zip

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Interactive comment on Geosci. Model Dev. Discuss., 7, 3575, 2014.

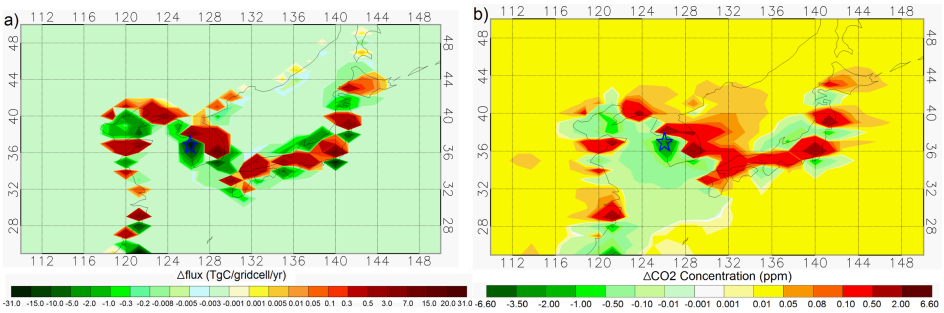

Fig. 1. Fig. 5. Regional fluxes difference and simulated annual mean surface CO2 concentration differences (experiment minus control) and the location of TAP monitoring site. a) flux difference; b) concentrat