

## ***Interactive comment on “Sensitivity of simulated CO<sub>2</sub> concentration to regridding of global fossil fuel CO<sub>2</sub> emissions” by X. Zhang et al.***

**Anonymous Referee #1**

Received and published: 29 June 2014

This paper describes a straightforward study into the treatment of CO<sub>2</sub> emissions near coastal regions. When high-resolution emission maps are re-gridded for use in coarse-grid models, several choices can be made. In this paper, two choices are compared. The control simulation uses the standard approach in which fine-scale emissions are simply assigned to the overlying coarse-scale grid boxes. In the "experiment" simulation, the authors take the emissions from coarse cell boxes designated as "water" and distribute those to the neighbouring land grid cells. Subsequently, the simulated CO<sub>2</sub> concentration fields are presented and discussed, mainly in the perspective of "atmospheric inversion" studies.

I have only a few remarks, and several suggestions to improve the paper further.

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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. 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?).

Second (and related): Although the paper focusses 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.

For the rest, the paper was pleasant to read and very adequate for the journal.

Minor comments:

P 3577, line 20: Peylin et al. (2013): reference wrong or missing.

P 3578, line 3: convection synoptic flow → convection, synoptic flow

P 3578, line 4: "dynamic inconsistency": seems that the authors are promising a study to the interaction between emission and atmospheric flow at the km-scale. For in-

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stance, 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.

P 3578, line 2: to a coarser model gridcell. I suggest, “to the coarser model resolution”, or “to coarser grid cells”

P 3578, line 4: “the minority land geography dictates a water gridcell but with the presence of emissions”: 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.

P3579: line 4: “and the adjustment method used the regridded emissions”?? I think: “and the adjustment method used to re-grid the emissions”.

Page 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.

Page 3581, line 18:  $F_j$  is its emissions  $\rightarrow$   $F_j$  is its emission. I note in figure 1 that the “emission” is defined in units of  $\text{kgC}/(\text{m}^2.\text{s})$ . 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.

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Page 3581, line 20: whose corners intersect at a corner  $\rightarrow$  those that share a corner with the shuffled cell

Page 3582, line 2: “emissions fields” should be “emission fields”

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  $\text{TgC}/(\text{cell}.\text{yr})$  (or  $\text{kgC}/(\text{m}^2.\text{yr})$ ). 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  $yy$   $\text{TgC}/(\text{cell}.\text{yr})$  receive  $zz$   $\text{TgC}/(\text{cell}.\text{yr})$ , which is on average a  $xxx$  % increment.).

Page 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.

Page 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 figure 1?) 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  $\text{CO}_2$  concentration fields differ?). From the global plot (figure 3) it is hard

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to discern the TAP location in the “emission-difference” dipole. 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.

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