Reply to Referee #1 on "Sensitivity of simulated CO_2 concentration to regridding of global fossil fuel CO_2 emissions"

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 CO_2 emissions ($FFCO_2$) 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 CO_2 concentration driven by two different approaches to regridding fossil fuel CO_2 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 $FFCO_2$ 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 CO_2 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 $FFCO_2$ 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 CO_2 concentration field (-6.60 ppm) opposing to the positive portion on land (Fig. 5b)."

We would like to put Fig. 5 here:

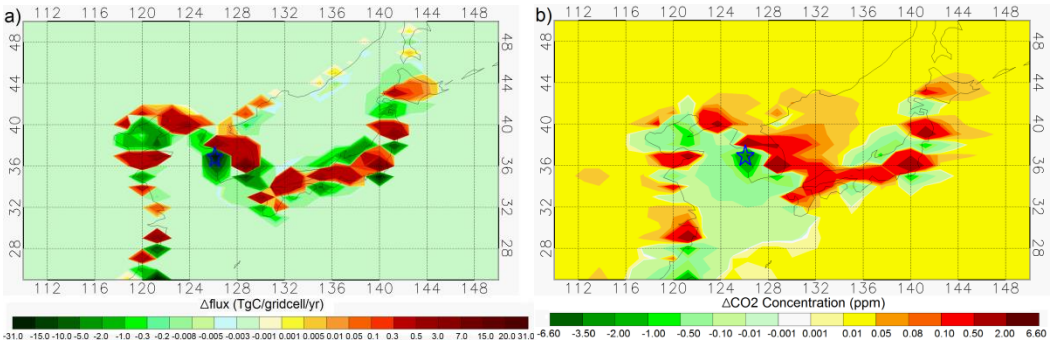

Fig. 5. Regional fluxes difference and simulated annual mean surface CO_2 concentration differences (experiment minus control) and the location of TAP monitoring site. a) flux difference; b) concentration difference. Blue stars mark the location of TAP site.

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 $FFCO_2$ 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 CO_2 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 $FFCO_2$ 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 $FFCO_2$ 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 CO_2 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."

Reply to Referee #2 on "Sensitivity of simulated CO_2 concentration to regridding of global fossil fuel CO_2 emissions"

The manuscript "Sensitivity of simulated CO2 concentration to regridding of global fossile fuel CO2 emissions" by X. Zhang et al. presents an analysis of the impact of the misplacement of fossil fuel emissions to eater gridcells when regridding from a fine-scale grid to a coarse-scale grid on simulated atmospheric CO2. The authors compare two different reridding methods: a 'traditional' method where the emissions on the fine-scale grid are simply aggregated on the coarse grid and a 'reshuffling' method where emissions on the fine grid are displaced to the nearest coarse land gridcell in case the fine grid cell lies in a coarse water grid cell. The authors highlight this dynamical inconsistency as a potential problem for atmospheric CO_2 inversions.

The reshuffling of emissions is indeed an interesting approach and worthwhile to follow up but there are several problems with the current manuscript. The authors claim that this reshuffling of the emissions ensures dynamical consistent results. However, it is not clear what they mean by 'dynamical consistent'. I assure this refers to the different vertical mixing and boundary layer behavior over land and water grid cells and that land fossil emissions in a coarse grid water grid cell would be advected differently than in a coarse grid land grid land grid cell. This needs to be discussed in the paper.

We thank the referee very much for the comments.

We agree that more discussion is needed to clarify the "dynamical consistency". We note that Referee 1 made a similar comment. For this purpose, we have modified text in the "Abstract" and "Introduction" sections.

In the "Abstract" section, we have made the following modifications:
"Regridding of fossil fuel CO_2 emissions ($FFCO_2$) 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 CO_2 concentration driven by two different approaches to regridding fossil fuel CO_2 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 $FFCO_2$ 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 CO_2 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 $FFCO_2$ 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."

On the same topic, it is not clear how the meteorological driving fields from MERRA are treated. If the MERRA data have to be regridded as well to match the PCTM grid, then there is the same problem with the treatment of meteorological field if data from a fine grid land cell ends up in a coarse grid sea cell or vice versa. This may not be a problem in this particular case if the MERRA met forcing is already on the PCTM grid but it is certainly a problem for many other atmospheric transport and inversion systems. In fact this may actually be a much more important bias and is not limited to CO2.

The MERRA meteorological data and the PCTM have exactly the same resolution.

But the major problem with this study is that it is only half way done. Since the authors claim that this is potentially an important problem for atmospheric CO2 inversion the questions are now: What is the impact on the estimated surface fluxes when using the reshuffling method in atmospheric CO2 inversions? And how do we know that this results in more accurate flux estimates? It needs to be shown that this different way of regridding really results into different flux fields. But the second question is probably even more important because the reshuffling process may create artificial biases and shifts potential natural sinks/sources from water to land gridcells as the overall carbon budget needs to be balanced. So this reshuffling regridding may just move a bias from a dynamical transport process to balancing bias.

The reviewer raises several points here which we need to separate.

First the study is incomplete because we have not considered the impact on atmospheric inversions. This is true. Of course most studies are incomplete; our task here is to note the magnitude of an effect. We also note that atmospheric inversions are not the only purpose for simulations of fossil fuel-like tracers; many studies in atmospheric chemistry have the same need and consequently the same problem. But the study also does do something of direct use for an inversion. The fossil fuel is part of the prior flux. So in an atmospheric inversion this term represents a systematic uncertainty in the mapping of fossil fuel flux into the prior mismatches (prior simulation of concentration – observations). We see that the effect is widespread and large compared to the measurement uncertainty usually used. This is enough to demonstrate significance for an inversion.

The second point is whether the reshuffling simply transfers errors from one place to another. The comment has some merit. For example reshuffling emissions away from an oceanic gridpoint may leave a station in that grid cell further from emissions than it really should be. This is possible of course. We can only investigate this by separating the transport and relocation effects by using an on-line model as suggested in another comment. We don't agree though that this could introduce land-ocean biases. Fixed fossil sources are almost entirely land-based. Putting them in ocean gridpoints seems far more likely to introduce land-ocean biases as the inversion tries to correct a poorly transported signal from the wrong environment.

And finally it is not clear how fossil emissions from planes and ships should be treated. These data are available now as well on resolutions higher than typical transport models. How are they accounted for in such a reshuffling process?

We have not addressed this problem yet. Airborne emissions are unlikely to be strongly impacted by this problem since the differences in atmospheric physics between land and ocean decrease once above the boundary layer. While emissions from shipping do potentially suffer from this problem the fraction subject to misallocation will be small so the total problem is a small fraction of a small fraction.

Reply to Dr. David Calvin on "Sensitivity of simulated CO_2 concentration to regridding of global fossil fuel CO_2 emissions"

Comments: The CO2 concentration difference between two cases (control and experiment) is mainly caused by allocation hand emissions on ocean grids. It is necessary to improve the understanding of regridding issue with an additional case by changing all ocean grids as land grids on the coastline in PCTM modeling.

This is a very interesting idea. Unfortunately we can't do it in this study because PCTM is an off-line model, which is it takes pre-defined meteorology. We believe the contribution of the paper as it stands means the delay incurred repeating the study with an on-line model before publication would harm rather than help the field. We certainly hope to test this idea in a follow-up study and have added comments to this end in the discussion.