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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Sensitivity of simulated CO_2 concentration to sub-annual variations in fossil fuel CO_2 emissions" *by* X. Zhang et al.

Anonymous Referee #1

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This paper studies the effect of time variations in fossil fuel emissions on simulated CO2 concentrations. The paper does not use atmospheric observations and earlier papers have studied the subject to some extend. Nevertheless, the paper provides a nice addition to earlier studies and quantifies and compares the effects of time variations to rectifier effects expected from biosphere CO2 uptake and extends the subject to total column CO2 measurements (for which the effects are understandably less pronounced).

I have little remarks about the paper, although at some points the results need to be checked carefully by the authors. Furthermore, the results should be compared to the results of earlier studies where possible. Proper credit is given to earlier studies, but I do not read whether this new study confirms of falsifies results from earlier studies.

The authors should include a brief paragraph that outlines this.

Figure S1 in the appendix seems to show emissions at a time resolution of 3 hours, while the text (page 20684, line 14) mentions hourly time resolution. Furthermore, the normalized diurnal cycle of the BLH for the biospheric fluxes (green dashed) pronouncedly differs from the cycles calculated for the three LSRs. This might be due to the sampling of BLH variations at different latitudes or the inadvertent inclusion of oceanic grid boxes. The authors should better analyze and explain this. Moreover, the mean of the dashed curves to not seem to be produce the expected zero value. The authors should make clear how these curves are calculated and maybe also should produce non-normalized values.

Figure S2 presents daily emission differences (caption mentions emissions). Moreover, the units seem wrong to me since an area unit is missing. Probably the values represent emission units per grid cell. This needs to be corrected.

Figure S3: here again the mean value seems larger than 1, which is not expected for normalized emissions. As the authors note, the Chinese values seem strange (with a large jump between December and January), and I sincerely question inclusion of the results in the paper. Maybe simply note the inconsistency and refrain from further discussion here?

I do not understand the results presented in paragraph 3.5 and presented in figure 5. The Monday values show pronounced negative values right over the source regions. The authors claim that this is due to "downwind transport" of the lower emissions during the weekend, but in my opinion a downwind effect would look rather different. I firmly believe results should look more like figure S2, which shows the underlying emissions. The authors should further scrutinize their implementation of the emissions in the model, and the subsequent analysis ("Monday" sampling is not trivial for models that normally use UTC). Anyhow, the hand-waving argumentation at the end of section 3.5 should be substantiated with further analysis.

Minor points:

Page 20682, line 2: line break should be removed

Page 20683, line 15: you will not reach equilibrium in concentrations, but rather reach equilibrium in concentration gradients. Please specify this more clearly.

Page 20685, line 10: 20to → 20 to

Page 20688, line 12. Here and in other places in the manuscript, the terms "concentration" and "emission" are intermingled. A "concentration difference" between "varying emission fields" should be rephrased to make clear that concentration differences are caused by different emissions run through a model.

Page 20691, line 25: "strong negative surface perturbation" should be "weak" since the BLH is deep. At line 27, "weaker" should be "stronger". Or did I misunderstand the point here?

Page 20692, line 20-22. I do not understand the argument made here ("for studies influences by the diurnal cycle"?). Please clarify.

Page 20694, line 5: see point above: use of "concentration" and "fluxes".

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20679, 2015.