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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.

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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 extent. 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.

We have carefully checked the results. Thanks very much for pointing out the faults

in the supplementary material in the following paragraph. We did the modification and responded to the reviewer's comments point-to-point in the following paragraphs.

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.

Thanks for the comments above. This study confirms the results of earlier studies. We added two sentences to state this consistency in the following.

In line 5, page 20696, we added: "These seasonal and synoptic effects are very similar to those presented in Peylin et al. (2011) at station scale." In line 13, page 20696, we added: "The synoptic-scale impact is comparable to the results in Peylin et al. (2011), which found an impact of $\sim$5 ppm."

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.

We actually downscaled the emissions from a daily total to a three-hour model simulation resolution. We added this statement in the sentence, as follows: ". . .and then distributing the daily total according to a three-hour model simulation resolution according to the hourly diurnal fractions from TIMES."

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.

Thanks for pointing out the mistake in the BLH. We carefully checked code and found that some oceanic boxes were included when processing the land BLH. We modified the processing code and recalculated the BLH for biospheric fluxes. The corrected BLH is plotted in Figure S1. Moreover, the mean of the dashed curves to not seems 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.

We corrected the normalized value and modified the plots using the correct value in Figure SI.1 and SI.2.

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.

Yes. The figures show the emissions differences between the weekly-cycle emissions and flat emissions with the unita as per gigatons carbon per grid cell. We corrected the captions and the units in the figure.

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?

The unusual seasonal pattern of Chinese FFCO2 emissions is primarily due to the inaccurate energy consumption data, which is not out of the scope for this study and is discussed in the Gregg et al. (2008) paper. Given the fact that this pattern has an effect on the simulated CO2 concentrations, and that this monthly fraction is likely used in inversion studies and forward transport model simulations, we think showing this result in the paper can be helpful and lead researchers to further explore this unusual pattern. We are sure that it will help the community to pay the attention to this unusual pattern, and then solve this problem. We already corrected the normalization of emissions, and plotted it in Figure S3.

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 dur-

ing 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.

Thanks for pointing it out. We totally agree that ‘downwind effect’ is not an appropriate description of the pattern. The negative values on Monday over the large source regions result mainly from the effect of low weekend emissions that dominate those from large Monday emissions. Thus, it is a residual effect of the low emissions from Saturday and Sunday. We replaced the phrase ‘downwind transport’ with phrase ‘residual effect’ in the text. We have modified the sentences of paragraph 3.5 as follows: “In contrast to other weekdays, Monday shows positive values only in narrow portions of East Asia. The other large source regions show negative surface FFCO2 concentration difference values. This spatial pattern results mainly from the residual effect of the lower weekend FFCO2 emissions, which dominates the effect of the large Monday emissions over these regions. This coherent FFCO2 concentration difference dissipates after 24 h and is then dominated by the higher weekday FFCO2 emissions from Tuesday to Friday. The weekend FFCO2 concentration shows negative values over large source regions, due mainly to the dominant lower weekend FFCO2 emissions over the residual effect of the larger weekday emissions.”

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C9049/2015/acpd-15-C9049-2015-supplement.pdf

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