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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Accounting for the vertical distribution of emissions in atmospheric CO_2 simulations" *by* Dominik Brunner et al.

Anonymous Referee #2

Received and published: 30 December 2018

General comments:

The proposed study investigates the impact on surface-level CO2 concentrations for simulations where all emissions are presumed to come from the surface versus from a more realistic vertical distribution. The authors establish that a vertical distribution of emissions is accepted as an important characteristic in the air quality community during their simulations of other, non-CO2 species, though it has been mostly neglected in the CO2 community. This is an important observation, and the authors even point out that large regional CO2 emitters such as power plants were specifically designed to emit at a higher altitude for the purpose of minimizing the impact on surface CO2 concentrations. They thus perform simulations for each emission type–surface and vertically distributed–to compare, and they further include a simulation testing the impact

of including plume dynamics. The hypothesis being tested looks to be of high value to the CO2 modeling community. The analysis used simulations from the SMARTCARB project and compared CO2 concentrations at times when uncertainty should be at its lowest, following standard practices from the modeling community. The plume analysis agrees with an earlier study's conclusion that EMEP vertical distributions may put emissions too high for combustion processes (i.e. powerplant stacks). The authors find that, compared to the standard surface-released CO2 emission scenario, one with a realistic vertical profile has decreased dry air mole fraction values at the surface layer in the afternoon (as is commonly used in inversion analyses), especially in the winter, where their January analysis showed a 43% mean difference (25% median difference). This is a significant finding that should be shared with the community. The authors further investigate the impact on total column XCO2 concentrations and find only a small difference in the monthly means, however a potentially significant difference near individual powerplant plumes. The difference in concentration values between power plant emissions simulated at-height versus with a plume-rise model are explored with individual tracers and found to be noteworthy because of the differing atmospheric dynamics. Their literature survey is well-performed and up-to-date, and their science is impactful and presented in a clear and straightforward manner. I recommend this manuscript for publication following minor edits.

Specific comments:

Page 4, Line 23: It might be good to include, after the sentence stating the height of the lowest vertical layer, a sentence stating that the vertical allocations will be described in more detail in section 2.4. Otherwise, an interested/over-eager reader may go searching for that information prematurely (as I just did).

Page 5, Line 16: Is there a citation yet for version 3 of the TNO/MACC inventory?

Page 5, Line 19: It would be nice to have a reminder of the SNAP categories here

Page 5, Line 21: What are the 7 source categories used by the city of Berlin?

Page 5, Line 24: Please go into more detail about how this “mapping” was performed. There may (necessarily) be inherent assumptions here that the reader should know about.

Page 5, Line 28: How was this merging performed? An average of the two at each pixel? Figure 1: In the left image, what do the colors represent? Different source categories or emission fluxes? What are the units? If these really are both in units of flux, then I am wondering why there appear to be more strong point sources in the southern portion of the right image (post-projection) than the left (pre-projection).

Page 6, Lines 8-9: In what context were these flux tower fits performed to tune the remaining VPRM parameters? Was this further part of the SMARTCARB simulations? If so, could we get a little more information about which flux towers were used (or a citation)?

Page 6, Lines 14-15: When the vertical layers were divided up to add new layers, how were the emissions distributed along those layers? For example, if the 4-90m EMEP level was divided into 3 COSMO-GHG levels–4-30, 30-60, and 60-90–were the emissions in 4-30 assigned to be identical, more, or less than those assigned to 60-90?

Page 6, Line 18: Sentence starts with “This modification”; is this referring to the second of the 2 modifications? Or is this intended to read “These modifications”? If it is only referring to the second modification, then where did the 10% number come from in the first modification?

Page 9, Line 14: What is the July difference in ppm, to compare against the 1.6 ppm in the previous (winter) paragraph?

Page 9, Line 32: Is it stated somewhere how the column mean dry air mole fraction is calculated? I assume it is, as the name suggests, the mean concentration at each pixel along the full vertical column for each pixel in the domain, but it is worth stating this explicitly somewhere. Apologies if I just missed it.

Page 10, Lines 19-20: This last sentence is very confusing for me. What does the “95th percentile” refer to?

Page 10, Lines 21-22: Maybe not in this sentence, but somewhere in the next paragraph or two, it would be nice to support the “very large” differences assertion with a number (from Figure 12, presumably).

Page 12, Line 18: Similar to a comment above, I am still not clear on what the “highest percentiles” refers to.

Technical corrections:

Page 1, Line 12: Should read “The results suggest that. . .”

Page 1, Line 18: Comma should be moved, I think: “. . ., the contributions from anthropogenic and natural fluxes and their sensitivity to climate change, and political and societal drivers.”

Page 2, Lines 13-16: I suggest putting commas around “such as the WMO’s Integrated Global Greenhouse Gas Information System (IG3IS)” to make it an appositive, otherwise the sentence is a run-on.

Page 3, Line 4: “Hogue et al.” missing a year

Page 3, Line 8: “Lauvaux et al.” also missing a year

Page 4, Line 10: I am not able to make sense of the phrasing “to offload compute intensive part”. Is it trying to say “to offload computationally-intensive parts”? If not, is it possible to reword/clarify?

Page 4, Lines 11-12: The phrasing “which allows to increase. . .” feels awkward (and maybe incorrect?). I would consider rephrasing the sentence as “which allows for an increase in spatial and temporal resolution, and which greatly. . .”

Page 4, Line 16: I think it’s supposed to be “an NO2” not “a NO2”, but I could be wrong.

Page 4, Lines 19-20: “250-km-wide” should be hyphenated to act as one adjective

Page 12, Lines 3, 5, 8, 10, and 12: Might want to say “near” or “around” or some other phrase instead of “of/on the order of” so that readers do not think this is referring to an order of magnitude

Page 12, Line 11: “Bagley et al.” does not have a year.

Page 12, Line 25: Change “However, also these methods. . .” to “However, these methods also. . .” to correct awkward phrasing

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