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# Comment on acp-2022-645

Anna Karion (Referee)

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Referee comment on "Evaluation of simulated $CO_2$ power plant plumes from six high-resolution atmospheric transport models" by Dominik Brunner et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-645-RC1, 2022

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Review: "Evaluation of simulated CO2 power plant plumes from six high-resolution atmospheric transport models", Brunner et al.

Overall, this is a very nice paper: a well-thought-out experiment was constructed to compare many atmospheric transport and dispersion models under the condition of known emissions. Presentation is very good, with clear nice figures and a lot of good additional information in the SI; it also reads well without getting too long (!) and losing the interest of the reader. I have comments and suggestions below, and have tried to note when a comment is just an optional suggestion to improve the analysis or a question that arose in my mind as I read it, especially the last comment below.

[general note: the authors should cite Angevine et al., https://doi.org/10.5194/acp-20-11855-2020] - it's different but similar in that they simulated powerplant plumes that had known emission rates.

Specific comments:

L 18 this raises the question: how did the LPDM perform?

L114 & 130 Perhaps "Table" should be spelled out here as it is in Line 97? Up to the editors.

Fig. 1, what is the underlying pixelated color? land area perhaps? elevation? Does the pixel size here mean anything? (~0.5 degrees?) relative to model resolution? (I imagine not as the models are all finer-scale).

Lines 150-170: Can the authors explicitly state whether these two versions of WRF are the same in the outer domains? (same configuration?). I see a lot of specifics on WRF-GHG configuration, and different specifics on the WRF-LES configuration. The domains are different resolutions (10 km - 2km nest vs. 5-1-0.2), so I am guessing these were different. But the first description does not include the version, and the second does not include the land surface, cumulus parametrization, radiation, etc.... so hard to compare! It would be good to know how similar the meso-scale portion of WRF-LES is to the WRF-GHG run to see if any differences are due to the outer domain or due to the LES.

Eq 1 and lines 253-260. The authors should explain what is cp(y) and its units? A is called a scaling constant, then an area integral but then it is in ppm. If A is an integral shouldn't it have some kind of other units like ppm * distance? (sigma has units of distance also right? if it is the same sigma in both parts of the equation)? Also somewhere the authors should note that "CO2" here is a mole fraction, i.e. in units of micromoles per mole of (dry?) air, or ppm. Is it a mole fraction, not a concentration, right?

L265 What is a YAML file?

Figs 2 & 3 these are very nice figures for showing qualitative model differences at a glance.

L311: Was the same constant value chosen based on the best fit to the models? Also, did a constant value fit all transects? I.e. there was not temporal evolution of the background assumed during the flight? (or altitude dependence)? [I see the altitude dependence is mentioned later in L317-319]

P12: this discussion makes me wonder if turbulence profiles (TKE for example, or just sigma_w) were compared between models (assuming measurements were not available on the aircraft). Even just comparing between the models might be interesting to understand the mixing in addition to Potential temp. (Just a suggestion)

I may have missed this further up, but were the emissions for the models the same listed in Table 2, i.e. the annual average? Is there temporal variability in these emissions? perhaps the models were using emissions reported at the hourly level (here in the US we get hourly Continuous Emissions Monitoring System data from the stack measurements for Powerplants)? We have found hugely varying emissions in time even from hour to hour, and even in predominantly coal plants here in the US. Presumably that is not the case for these two, and emissions are constant?

Fig 12 discussion: It would make things clearer if these parameters (plume width and amplitude) were referred back to the equation where they are defined, with those symbols? (for example, I woudld think that sigma was the plume width although that's not the definition in the methods section near the equation?).

Fig 12-13. (Suggestion/Comment) I realize the point here is to compare the evolution of the plume with distance for the various models, but it might be interesting to consider the integral under the plume for each model vs. observed for all the transects/distances. Presumably, this should be the same at the different distances (well only if wind & PBL are constant - so maybe not). (is this A in the equation? I'm not sure). IF the integral is not the same with distance then it points to changes in the wind, PBL, plume separation (some of it going above the ABL and advected faster), etc. that might explain deviations from the gaussian plume model.

Fig 14, very nice figures, and easy to understand the symbols etc. What are the units on the colorbar?

L504. Could the authors comment on how one would determine the wind if one was using the satellite data to determine the flux using the cross-sectional method? I.e. one would perhaps use modeled winds in the same way as was done here. But given the current "image" data is generated by the same model as is being sampled for wind, it's sort of a best (perfect)-case scenario. What if the wind from a one model was used to determine emissions from the image of a different model? That may be outside the scope of this paper, but it's relevant in determining the ability to estimate accurate emissions if the wind model is not exactly like reality, as it is in the model world here. Perhaps given the range of these wind values (the average used in the emissions estimate) from the different models, one could arrive at an additional uncertainty from this component of the analysis? (I see later the wind speed issue is addressed for the IME method).

L545-547 This is a very important point, in a current climate where satellite observations are meant to solve all our emission quantification problems -- we will still be relying on a model to do this, and the model still requires an accurate estimate of wind speed in the PBL and PBL depth.

[overall comments / musings, no need to address in the paper unless it would be easy to do!]:

I wonder if this set of very valuable model simulations could be used to understand how important dispersion and turbulence are for accurately simulating tracer concentrations (mole fractions) relative to the importance of the underlying mean meteorology (mean wind speed and PBL depth for example).

I.e., often when estimating emissions using a model, the underlying meteorology (not the dispersion) is usually evaluated: wind speed, wind direction, and PBL. If the underlying met has no bias in these quantities, the transport is considered "validated". Unfortunately, it is not clear whether there is a way to evaluate dispersion as well? I would guess not without observations of TKE or such a quantity. I wonder if the authors could comment on the ability of the models they investigated to simulate mole fractions at a given location even when performing well for wind speed, direction & PBL depth. It might be nice (if the authors agree with this based on their findings) to emphasize this issue of turbulence, mixing, and dispersion as important separately from wind speed, direction and ABL depth, which are obviously crucial but not the whole story.