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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Analysis of CO_2 spatiotemporal variations in China using tower data and a weather-biosphere-online-coupled model, WRF-VPRM" *by* Xinyi Dong et al.

Anonymous Referee #2

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The paper presents simulations of atmospheric CO2 over China at high spatial resolution and comparisons against observations. Temporal and spatial variations are assessed. The manuscript needs to be revised before it can be recommended for publication.

General Comments

It is stated that WRF has been evaluated extensively with respect to meteorology, but no references are given. In this context an evaluation of the model against meteorological observations within the domain of interest is needed. If no references can be found, this evaluation should be included in this manuscript.

The authors claim that the WRF-VPRM model can be used to assess carbon budgets related to biospheric fluxes and to anthropogenic emissions. However, it should be clear that VPRM is a highly simplified light use efficiency model that represents upscaling of flux observations from eddy covariance measurements made over Europe, which would need further optimization through inverse modelling (see e.g. Kountouris et al., 2018) even for the European domain. Applying the same VPRM parameters to a different domain will result in even larger errors in fluxes. Furthermore, anthropogenic fluxes are simply used as input to WRF-VPRM, thus regional carbon budgets can directly be derived using the emission inventory data themselves.

I fully agree with Reviewer #1 in that more details are needed with respect to the description of the model setup, but also the observation sites. For example, only at the end of the discussion it is mentioned that the city of Hangzhou is located 60 km away from the Lin'an tower. This clearly belongs to the description of the data used, ideally in a specific section within the methods section, entitled for example "atmospheric observations".

Specific comments

In addition to the mean bias (MB), the normalized mean bias (NMB) does not really provide additional information, as the mean of atmospheric CO2 for specific locations/periods is always within 10% of 400 ppm. I therefore suggest not reporting the normalized mean bias.

Abstract: Line 18: "characterize CO2 dynamics" I suggest rephrasing "characterize the dymanics of CO2 in the atmosphere"

Line 23: "determined" rephrase, e.g. "dominated"

Line 44: "calibrated" -> "adjusted"

Line 45: "determine posterior flux"

Line 76: "suffer from" -> "due to"

Line 94: A reference for CT2019 is needed. From where and when were the data downloaded? See also NOAA's usage policy under https://www.esrl.noaa.gov/gmd/ccgg/carbontracker/citation.php

Line 114: "pure" I suggest using "process based"

Line 130 "samplings of CO2 surface concentrations with monthly intervals are collected through" -> "atmospheric samples near the surface are collected at monthly intervals and analysed for CO2 through"

Line 134: please provide a clear reference for the OCO-2 data. From where and when were the data downloaded?

Line 137: please provide a clear reference for the TCCON data from the Hefei site. From where and when were the data downloaded? Please ensure also that the TCCON data use policy is followed (see https://tccon-wiki.caltech.edu/Main/DataUsePolicy).

Line 166: "forest which" -> "forest, which"

Line 187: The attribution of model-observation discrepancy to the vertical allocation of emissions is not plausible. It would be required to at least state the distance of upstream (strong) emission sources that could have an impact on the CO2 profile over the site.

Line 188: "Biosphere models" please rephrase, e.g. "tracer transport models"; also: CASA is a biospheric process model providing biosphere-atmosphere exchange fluxes, to which level within a tracer transport model those are added is not prescribed by CASA. Also note that the injection height is relevant only for anthropogenic emissions of CO2 due to the associated smoke stack height or plume rise (see Brunner et al., 2019), not for biospheric fluxes.

Line 198: "Pu et al. (Pu et al., 2014)" -> "Pu et al. (2014)"

Line 204: Please reformulate, this sentence is not clear. What do you mean by "as a

process-based model"?

Line 209, Fig 4c: I suggest using daytime values at the Lin'an tower. Note that the air samples at the NOAA stations are also taken during daytime, usually in a well-mixed boundary layer. Otherwise nocturnal peaks in (modeled or observed) CO2 will simply dominate.

Line 210:"we will probe into bias" -> "we will discuss details on the bias"

Line 224: "minimums" -> "minima", "maximums" -> "maxima"

Lines 241 – 245: I strongly recommend using ppm/yr as unit for the different trends.

Line 265: "may have also estimated" I assume that there is corresponding output from WRF-VPRM with hourly biosphere fluxes from respiration and photosynthesis, such that it can be confirmed that WRF-VPRM simulates non-zero respiration during non-growing season.

Line 280: "above or underestimation" -> "above, or due to underestimation"

Line 300: it should be made clear that here the concentration footprint is meant, rather than the flux footprint. See e.g. Lin et al. (2003) for concentration footprint, and Schmid et al. (1994) for flux footprint.

References: Kountouris, P., Gerbig, C., Rödenbeck, C., Karstens, U., Koch, T.F., Heimann, M., 2018. Atmospheric CO₂ inversions on the mesoscale using data-driven prior uncertainties: quantification of the European terrestrial CO₂ fluxes. Atmos. Chem. Phys. 18, 3047–3064. doi:10.5194/acp-18-3047-2018

Lin, J.C., Gerbig, C., Wofsy, S.C., Andrews, A.E., Daube, B.C., Davis, K.J., Grainger, C.A., 2003. A near-field tool for simulating the upstream influence of atmospheric observations: The Stochastic Time-Inverted Lagrangian Transport (STILT) model. J Geophys Res-Atmos 108. doi:10.1029/2002JD003161

Schmid, H.P., 1994. Source Areas for Scalars and Scalar Fluxes. Boundary-Layer Meteorol 67, 293–318. doi:10.1007/BF00713146

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