

Atmos. Chem. Phys. Discuss., referee comment RC1
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Comment on acp-2022-214

Anonymous Referee #2

Referee comment on "Towards monitoring CO₂ source-sink distribution over India via inverse modelling: Quantifying the fine-scale spatiotemporal variability of atmospheric CO₂ mole fraction" by Vishnu Thilakan et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-214-RC1>, 2022

Review of the paper "Towards monitoring CO₂ source-sink distribution over India via inverse modelling: Quantifying the fine-scale spatiotemporal variability of atmospheric CO₂ mole fraction" by Thilakan et al. The authors have tried to estimate representation errors (REs) for model resolution, using a 10x10 km simulation by WRF-VPRM simulations. They also briefly discuss CO₂ concentrations from global models.

I have found this version of the ms reads better than the previous version. The paper still lacks clear direction. Impacts of inversion results are less than expected at the end. At the minimum the paper require major revisions before consideraion for publication in Atmos. Chem. Phys.

Specific comments:

Line 145ff : I feel you do not need this paragraph, the introduction have talked about the rationale well. Go straight to model description

Line 214-2015: this is very strange, why cannot you do continuous simulation? you are loosing some 20% of computing time and may be some inconsistency in simulation with repeated initialisation + plus may be some discontinuity in transport.

Line 284: India specific study: Kulchala et al., Spatio-temporal variability of XCO₂ over Indian region inferred from Orbiting Carbon Observatory (OCO-2) satellite and Chemistry Transport Model, Atmospehric Research, 269, 106044, 2022.

Line 299: there is a paper discussing these statements here (Patra et al., ACP, 2011)

Line 387ff: I still don't know what this means or if it is needed here? If true, why are you doing this exercise anyways? What are the relevance to your WRF simulation analysis? Any comparison or cross checking?

I am still not convinced what the authors are aiming at with this analysis (Figs. 2 & 3). there are measurements from CONTRAIL if you want to get a true picture of the global model uncertainties using model-observation comparison please check Patra et al. (2011) for a methodology. i think the readers need a bit more clarification about this analysis

Line 417: is this a novel finding of this study for the this study area ??

Line 422: what is the basis of plausibility ?

Line 440: Why gap filling is needed ? You cannot replace measurement by model, if you believe the satellites are doing something right!

Line 475ff: When you say representation error in CO₂, I assume fossil-fuel component is also included in the analysis. I wonder why the cities or power plants hotspots are not revealed as the areas of high representation error ?

Line 481 : I was expecting about an order of magnitude lower representation error for XCO₂, compared to REs in CO₂ at 200 m. Can you give an equation how you calculated XCO₂ ? also could you please show the REs in CO₂ in the supplement, say at 2 km and 5 km altitude ? That will give an idea to the readers how REs propagate upwards

Line 510ff: I like fig. 7. I think this is the most novel result of this study. But i am curious if you can quantify the REs arising from flux smoothing or transport error ? In TransCom continuous experiment Patra et al. (GBC, 2008) ran two sets of experiment to make such separation (please ref to their Fig. 8 associated discussion). Of course their method of estimation of REs is different from yours, but nevertheless the flux vs transport REs is an important information to derive from such OSSEs

Line 534: Again I would like to draw your attention to one paper here by Chandra et al. (ACP, 2017). Think there is not too much scopes or need for speculations. The Indian monsoon domain is well studied for dynamic and now for chemical species

Line 559ff: Not sure how you get these numbers for RE fractions. I am a bit concerned about the low contrast in REs with latitude or longitude over India, there are forest

ecosystems, agricultural land, semi-arid land and deserts. I cannot explain the distributions of REs I see in Fig. 4 or Fig. 5. How good is the VPRM ? Any testing has been done? At the very least can you compare your results with global model results for seasonal cycles, e.g., in Fig. 2b ??

Line 596 : how can you explain higher CO₂ sink in Nov compared to July over India (Fig. S2b suggest otherwise). also other published literature suggests higher uptake in July than November (Patra et a., ACP, 2011)