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Comment on acp-2022-777

Anonymous Referee #1

Referee comment on "The carbon sink in China as seen from GOSAT with a regional inversion system based on the Community Multi-scale Air Quality (CMAQ) and ensemble Kalman smoother (EnKS)" by Xingxia Kou et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-777-RC1, 2023

Kou et al. estimated biosphere carbon fluxes over China by applying a regional inversion system to GOSAT CO2 data. The inversion was designed to provide a higher spatial-temporal resolution than previous studies. While the topic is definitely interesting to the reader of ACP, the manuscript, in its current form, is not up to the standard. My main comments are as follows

(1) The paper lacks technical rigor. The authors present the high spatial-temporal resolution (64 km and 1 hour) as the innovation of the paper, but do not provide justification that the inversion of GOSAT CO2 data can meaningfully resolve hourly data, as GOSAT observations are daily observations at the same local solar time. A reader may be interested in quantitative information on to what extent the results are affected by prior information and to what extent they are constrained by observations. In addition, the authors claimed that the inversion is verified against "independent observations". But in fact these validation data are also taken from the same GOSAT CO2 dataset. Although these observations are not assimilated in the inversion, they may well have error distributions similar to those assimilated. Hence, these data cannot be regarded as "independent" validation data.

(2) The writing needs to be improved. For example, Section 2.2 (a key section describing the inversion algorithm) is difficult to follow. The logic flow is not clear. Important information such as how the error covariance matrices are specified and updated in the ESRFs is missing. Results in Section 3.3-3.4 are not presented in a concise and well-structured way. The discussion is not focused on new findings and insight, but in many cases, reporting numbers without proper interpretation. There are several occurences where some discussion points and even exact same sentences are repeated. For example, "(the system) is sufficient to robustly constrain the control vector" appears in line 26, 416, and 624. Notation and terminologies are used inconsistently and loosely, for instance, control vectors, state vector, and state variables are all used to represent a similar concept without explicit definitions. Overall, I'd suggest to substantially shorten the paper to focus on the contribution of this study to the field. Attention needs to be paid to logic

flows and consistent terminology.

Minor comments:
Line 23, 228: What is an observational operator? It is never clearly defined.

Line 111-112. The author first claimed that "regional CTMs are rarely used in satellite carbon data assimilation" but then cite a few studies that performed regional carbon data assimilation, which appears to be inconsistent. Moreover, the authors need to clarify what are innovations in this study relative to these cited studies.

Line 140: The study uses historical GOSAT observations not "real-time" GOSAT observations

Line 150-154: Two science questions are raised by the end of the Introduction, but it is not apparent that the discussion is focused on these questions nor these questions are adequately addressed.

Line 171: Why does not CMAQ need initial and lateral boundary meteorological fields. Is CMAQ coupled with a meteorology model (e.g., WRF)? A typical regional chemical transport model like CMAQ is driven by archived met fields and does not need initial and lateral boundary meteorological fields.

Line 174: What is "'real' initial and lateral boundary atmospheric CO2 concentrations"?

Line 232: If yf and yp are "wet" CO2 concentration, you should apply (1-w)^-1 to convert "wet" concentration to "dry" concentration, instead of multiplying (1-w)

Line 245: What is BG here?

Line 265: model grid -> model grid point

Line 271-274: It is not well justified that data with |o-b|>5 ppm should be removed. How the choice of the threshold affects the inversion results?

Line 281: Non-assimilated observations cannot be regarded as independent verification data. The filtering criteria (1) and (3) are the same as that for assimilated observations, and I don't quite get what the criteria (2) is about.

Line 293: Natural fluxes are optimized/updated, not "assimilated"

Line 302-304: It is unclear whether boundary conditions are perturbed by 5% or 10%. More importantly, it is not justified whether 10% perturbation to natural fluxes is proper.

Line 312: The word "high-risk" may not be suitable here.

Line 325-334. Discussion on data coverage here is not related to either what is before or after. I do not see the flow of logic here.

Line 351: It is stated that the detector on GOSAT is "more sensitive to near-surface CO2 changes", but I don't know what this is compared to. And I do not see how this statement add to the discussion above.

Line 373: I do not find any solid analysis showing that the calculation is reasonable or effective, except for some vague discriptions and comparisons.

Line 413-414: Logically, agreement with previous estimates does not provide a strong indication that your model transport is reliable.

Line 471-472: I do not find results to support this claim.

Line 489: Any evidence shows that a smaller daily variation is "more realistic"? I doubt whether the GOSAT data are sufficient to constrain the day-to-day variation given missing data and sparse sampling?

Line 391: Where does the number -0.47 come from?

Section 3.3. The author found downward correction over forest and grassland and upward correction for cropland areas. This is an interesting finding, but no further information is

presented.