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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Effects of *a priori* profile shape assumptions oncomparisons between satellite NO_2 columns and model simulations" *by* Matthew J. Cooper et al.

Anonymous Referee #2

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This manuscript addresses an interesting feature of sensitivity of NOx emission inversions to a priori profile shape assumptions in AMF of satellite NO2 columns. Authors conclude "As the difference between the simulated profile shape and the a priori profile shape increases, so do the corresponding assimilated emission errors". In the discussion section, however, the authors indicate that the adjoint inversion is less sensitive to vertical representativeness errors in cases where emissions are poorly constrained. It is noted that choice of AMF will become increasingly important to adjoint inversions as emission inventories improve. The manuscript delivers some new and intriguing messages to satellite and air quality modeling community. I think the manuscript is well written. The introduction and revisit of AMF and averaging kernel are neat and

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Discussion paper

helpful. There are some parts that need further investigation and explanation. Hope the authors revise and improve the manuscript before final publication.

* Major point Line 304-310: the manuscript deals with the "truth" emissions that deviate only 5% from the original anthropogenic NOx emissions. Here, it is written that the tests using random 15% or 30% perturbations to emissions were insensitive to the AMF. In real cases, NOx emission inventory errors are quite large (> 30%). Do the authors mean that choice of a priori shape factor is not important for most of real emission study cases? Please show the results from the random 15% or 30% perturbation tests (or other new cases if possible) and discuss more on applications to the real world problems (e.g., Qu et al., 2017).

* Minor points 1. Examples of inconsistent a priori shape factor: I do not think these days retrieval groups use SF_BL, SF_Trop type a piori. In the abstract, up to 80% increased error is based on this choice. I am not sure if readers need to take this number seriously.

2. Line 274-276: I am not sure what these mean.

3. Line 288-292: Examination of the mathematical frameworks behind two common methods for comparing simulated and retrieved columns highlights how the method introduced by Palmer et al. (2001) facilitates separation of observations sensitivity (scattering weights) from the profile shape (shape factor) enabling the model-retrieval comparison to be independent of a priori profile assumptions. In the last part, model-retrieval comparison to be independent of a priori. . .It is confusing because the main conclusion of the manuscript is that the model-retrieval comparison is not independent of a priori (in certain cases).

4. Line 307: Add "s" in the subscript.

5. Line 315-Line 320: It is good to emphasize these again. But I believe that retrieval groups are already doing this. It might be good to mention various data supported by

the retrieval groups.

6. Can the posteriori NOx emission difference (%) for highly polluted cases be shown in Table 2? This type of results will also be useful for higher perturbation cases.

7. Is the number of iterations of 4D VAR assimilations for all the test cases the same? How many iterations are requited for the tests?

8. Is there a possibility that model spatial resolutions affect the results?

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