

Interactive comment on “The contribution of soil biogenic NO emissions from a managed hyper-arid ecosystem to the regional NO₂ emissions during growing season” by B. Mamtimin et al.

Anonymous Referee #2

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This paper investigates the soil biogenic NO emissions from fertilized and irrigated hyper-arid ecosystem of the Tohsun oasis in NW-China and their contribution to the regional NO₂ emissions during growing season. The authors developed a so-called Geoscience General Tool Package (GGTP) to estimate biogenic soil NO emissions at the oasis. They calculated the anthropogenic NO₂ emissions based on various energy consumption activities and associated emission factors, as usually done for an emission inventory (EI). They also derived the total regional NO₂ emission values (biogenic + anthropogenic) by top-down approach using OMI satellite tropospheric NO₂ vertical column density (VCD) data, and compared them with the biogenic (from GGTP) and anthropogenic (from EI) NO₂ estimates derived by bottom-up approach. They found

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that biogenic emissions from soils of managed drylands (irrigated and fertilized) in the growing period can be much more important contributors to the regional NO₂ budget and thence photochemistry than thought before. The study is interesting and should be a welcome addition to the literature of journals like ACP. On the other hand, from the viewpoint of this referee, the paper can be improved further by adjusting the structure of the paper properly as well as by being more concise. Below are my comments and suggestions in detail.

1) I would suggest that the contents (and subtitles) of “Sect. 2 Materials and methods” and “Sect. 3 Results and discussion” be reorganized (and renamed) so that the three different emission estimates (i.e. the bottom-up biogenic NO emission estimates, bottom-up anthropogenic NO₂ emission estimates, and top-down total NO₂ emission estimates) as well as their inter-comparisons could be seen more clearly and followed more easily. For instance, Sect. 2.2 seems to have repeated contents for each of the three estimates and might be skipped with some contents being merged to the subsections for each corresponding estimate.

2) The full name for GGTP does not fully reflect the focus of this study as “the soil biogenic NO emission” is not seen. The soil biogenic NO emission model is an important part of this study, but it appears only in the sub-sub-title (Sect. 2.4.7) of Sect. 2. I would suggest updating Sect. 2.4, first by using some phrase like soil NO emission estimate (or model) for the title. Sect. 2.3 and some contents of Sect. 2.1 might be merged to it. The updated subsection needs to be more concise, e.g. by using citations (if possible) and/or Appendix (or Supplement Information) to describe the validation of soil parameters. The same strategy may be apply for “Sect. 3 Results and discussion” (e.g. Sect. 3.2–3.5).

3) I would suggest moving the contents on the biogenic emissions from soil in Sect. 2.5. (e.g. the whole Sect. 2.5.2, which introduces the FF and Q10F) to the new section describing the soil biogenic NO emissions (as suggested above). These parts are related to human activities via soil. It would better to include them in the soil

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NO model to address the soil NO emission variations by ecosystem management. If possible, a sensitivity study can be carried out to investigate such anthropogenic effect by changing FF and Q10F. At the same time, just keep the contents that are related to the anthropogenic emissions from fuel composition. It seems that all the emission factors were taken from the literatures, and thus this section can be more concise by just using citations. The formula (18), Table 1 and Fig. 15 can be omitted, and instead the contributions of industrial and traffic activities to the total anthropogenic emissions may be described.

4) I would also suggest removing the contents on the soil biogenic emissions in Sect. 2.6. Just focus on the retrieval of regional NO₂ emissions from satellite observations. Are there any advantages or disadvantages in the retrieval of NO₂ emissions from an oasis in comparison with from a city by satellite data? In addition to OMI NO₂ data, were meteorological parameters, such as wind fields, used in the retrieval of this study? The retrieval of regional NO₂ emissions from the oasis by satellite is an important part of this study and needs to be described/discussed more in detail.

Technical issues:

P34534, L7-9: It might not be necessary to emphasize the scaling from annual to monthly values. Instead, the anthropogenic NO₂ emissions from fuel composition might be mentioned.

P34534, L12: are equal to?

P34534, L25: NO_x = NO + NO₂

P34538, L11: county?

P34547, L9: replace "proposed" with "used" because so many studies are cited.

P34547, L23: I do not understand the word "space" here.

P34549, L11-12: Wagner et al. (1989) and Mallick et al. (2009) cannot be found in the

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Reference.

P34551-34552, Sect. 2.4.5: no validation and calibration are presented.

P34558, L9, the title for Sect. 2.5.3: it might not be suitable to use "assimilation" at least for the method of deriving monthly values of anthropogenic NO₂ emissions.

P34561, L27-28: there could be dust aerosols over the desert region.

P34569, L2-4: The NO fluxes as function of theta cannot be found in Fig.9.

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