**Atmospheric
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Discussions**

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Comment

Interactive comment on "A mechanistic model of global soil nitric oxide emissions: implementation and space based-constraints" *by* R. C. Hudman et al.

Anonymous Referee #2

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This is an interesting paper that puts forward a number of improvements to representing soil NOx emissions in global models of atmospheric chemistry. It takes into account improved information on biome, on nitrogen made available for emission by deposition from the atmosphere, and improved information of fertilization practices. Then it also describes in a better way the effect of soil wetting on nitric oxide emissions. The authors present new, above-soil NOx emissions, and test their new emission parameterization with the GEOS-Chem model against OMI observations of tropospheric NO2 columns for a number of regions where soil NOx emissions are predicted to dominate the tropospheric NO2 content. The strength of this evaluation follows from the (virtually) independent nature of the satellite measurements, and their wide geographic

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coverage.

In spite of the high-quality set-up and execution of the study, I still have a number of major concerns with respect to the details of the satellite evaluation. First of all, the authors acknowledge that they test the above-soil NOx emissions, without taking into account the effect of any vegetation shielding or assimilating NOx emitted by soils. Because their focus is on the U.S. Mid-West and the Sahel, the canopy reduction of soil NOx emissions, may not be as important as over dense forests, but I agree with the other reviewer that one cannot simply gloss over it by stating that there is debate on the canopy effects. This is indeed the case, and therefore I suggest the authors at least try to also include estimates of how much soil NOx reaches the atmosphere, after accounting for canopy reduction. For instance Delon et al. [2009] test their soil NOx emissions with and without canopy reduction. Then, even over areas with modest vegetation or leaf area, there is strong evidence that vegetation has a shielding influence on the soil/air exchange of NOx [Pang et al., 2009], suggesting that canopy reduction might also be important for regions like the U.S. Midwest and the Sahel. In fact, I think the authors detract from their achievement by not accounting for canopy effects. To illustrate this, it is still difficult to compare their soil NOx emissions to the state-of-science above-canopy estimates from 4.7-13 TgNyr-1.

Another concern is how the authors dealt with another variable source of tropospheric NOx: lightning. The same precipitation events that trigger soil NOx pulsing are likely to have generated a number of lightning flashes leading to NO2 that then will be detected by OMI. How have the authors avoided this interference in the satellite signals? Simply filtering out measurements with cloud radiance fractions < 0.5 is not enough since the clouds may have moved out of the footprint before the OMI measurement. In that respect, it is far from reassuring that the authors report that they compare to GEOS-Chem simulations without lightning (P3570, lines 3-5), because it appears to affect 'interannual variability'. Interannual variability of what? The authors should clarify all this, and convince us why model (no lightning contribution) and OMI (soil + lightning

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NO2 contributions) can be compared.

The evaluation of the success of the pulsing scheme (section 5.3) is ramshackle. It is unclear to me how the satellite NO2 columns have been averaged over the Sahel. It cannot be just the overall average over the Sahel region, since different locations have different days of first rainfall, and, therefore, different days of pulsing, as shown in the lower panel of Figure 7. In order to generate a Figure 8, the authors must have repeated their calculations for successive days, but then how did they account for the fact that yesterday's NOx pulse from one region affects today's background for an adjacent region where first rain occurred one day later? Any information on the absolute levels (in 1015 molec. cm-2) is lacking, we only see ratios in Figure 8, so it is impossible to judge how strong the NO2 signal was, and how that relates to the satellite detection limit. Furthermore, any information on the number of samples, and on error statistics is lacking from this part. There is no discussion of interference from lightning, and last but not least, the introduction of a new product (Standard Product) to validate soil NOx pulses, while not including the DP_GC product, is questionable. In section 5.2 the authors argue for the use of both the BEHR and DP_GC products 'as a measure of uncertainty in the NO2 anomaly', but in section 5.3 using just the SP is apparently deemed sufficient. The argument that DP_GC is not available on 0.25 deg x 0.25 deg can be easily overcome by downscaling the SP to whatever resolution the DP_GC has. Evaluating soil pulsing from both SP and DP_GC would be very valuable as a measure of uncertainty, and, on top of that also, provide important information on the consistency of the retrievals.

Delon, C., et al. (2008), Biogenic nitric oxide emissions from soils – impact on NOx and ozone over West Africa during AMMA (African Monsoon Multidisciplinary Experiment): modelling study, Atm. Chem. Phys., 8, 2351-2363.

Pang, X., et al. (2009), Nitric oxides and nitrous oxide fluxes from typical vegetables cropland in China: effects of canopy, soil properties and field management, Atm. Environm., 43 (2009), 2571-2578.

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Minor comments: P3558, l7: I suggest the authors clarify here what seasonality they refer to here.

P3566, l8: I suggest to add that this concerns the N available in soils apart from fertilizer use.

P3567, l25: OMI footprints come in km2.

P3570: although the anomalies are the same (+11%), the absolute anomaly in soil N emissions estimated from the BEHR retrieval is more than 1.5 times stronger than estimated from the DP_GC retrieval. Why has the Standard Product not been used as a third retrieval to evaluate the absolute anomaly?

P3570, l26 – P3571, l1: what is the statement that 'use of a different retrieval is not pre-dictedto produce significantly different results' based upon? There are quite a number of indications in this paper even (Fig. 6) that retrievals are still quite different.

P3575, l7: the authors should clarify what cloud product has been used here.

P3575, B1. Was a cross-track bias correction not used for the Standard Product?

P3575, l22: when referring to the stratospheric slant column from TM4, it would be appropriate to cite Dirksen et al. [2011] who provide a full description and validation of stratospheric NO2 from the DOMINO and Standard products. This also relates to citing errors for the stratospheric contribution (B4), where Dirksen et al. [2011] provide updated numbers based on intercomparisons and validation.

P3575, l23: the surface albedo in DOMINO is from the combined TOMS/GOME database.

Dirksen, R. J., et al. (2011), Evaluation of stratospheric NO2 retrieved from the Ozone Monitoring Instrument: intercomparison, diurnal cycle, and trending, J. Geophys. Res., 116, D08305, doi:10.1029/2010JD014943.

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