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Atmospheric Chemistry and Physics Discussions

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 #1

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The authors present a novel parametrization to calculate soil NO emissions in a global chemical transport model and they compare their results to satellite derived tropospheric NO_2 columns. The novelty of the parametrization includes 1.) a smooth dependence on soil moisture and temperature between 0°C and 30°C and 2.) treatment of available nitrogen from atmospheric deposition and fertilizer. This reflects more natural processes than did previous algorithms in atmospheric chemistry models. And this is relevant for all atmospheric chemistry models and fits well into the scope of ACP. But I have major objections and recommend publication after the authors have addressed these issues.

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Major comments:

1. "mechanistic" in the title is misleading, since at least a.) the emission factors, b.) pulsing, c.) the differentiation between arid soils and elsewhere are all empirical. The title should be changed to something similar to "Major steps towards a mechanistic . . . ". This should be the case throughout the article.

2. Although the authors try to justify not using a canopy reduction, I cannot accept their argument. Chaparro-Suarez et al. (2011) say in their conclusions "If we transfer such indications from laboratory measurements to the natural environment we would conclude that forests do not release NO_2". The results of Raivonen et al. (2009) are, in my opinion, highly uncertain and speculative:

   - Their deposition measurements exhibit a lack of data during daytime (see their Fig. 1).
   - They measured at treetops only.
   - The measured shoots were never exposed to rain, therefore nitrate on the leaf surface was not removed by rain.
   - They give neither any uncertainty estimation nor detection limits of their instruments.

   NO emitted from the soil is rapidly oxidized to NO_2 and this NO_2 is subject to wet and dry deposition within the canopy. I would recommend either applying the old canopy reduction as previously implemented in GEOS-Chem or even better integrate the deposition velocities derived by Chaparro-Suarez et al. (2011) into a new reduction factor for temperate forests. But applying a canopy uptake is in my opinion indispensable when comparing to satellite derived NO_2 columns. If you find something more convincing to ignore the canopy reduction, I would review the arguments again.

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Minor comments:

1. Also mention the smooth temperature dependence between 0°C and 30°C in the abstract.
2. How good is the representation of soil moisture in GEOS-Chem compared to the real world?
3. Page 3557, line 9: change "...to predicting atmospheric composition and to understanding ..." to "...to predict atmospheric composition and to understand ...".
4. Additional reference that you might want to include in the introduction to highlight the effects of (soil) NO_X on ozone, OH and aerosols could be Dentener and Crutzen (1993), Andreae and Crutzen (1997), Martin et al. (2003) and/or Steinkamp et al. (2009) and for the earth's nitrogen cycle it could be Galloway et al. (2004) and/or Phoenix et al. (2006). van der A et al. (2008) also fits well into the introduction, since this shows a dominant soil source of NO_X in parts of the Sahel region.
5. What scientific questions do you address with your new implementation, please be more precise at the end of the introduction.
6. Consistently abbreviate Yienger and Levy (1995) as YL95.
7. Mention that the model setup and description of the satellite data is in the appendix
8. Page 3559, line 5: DNDC (Butterbach-Bahl et al., 2009) does account for pulsing.
9. Page 3560, line 3: add "and soil moisture state." behind "...distinguish between vegetation type".

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10. Page 3562, equation 3: where does the number 0.103 come from.

11. Page 3562, line 16: It should also be mentioned that at high values of water filled pore space diffusion is hardly possible.

12. Page 3560, line 9; Page 3565, lines 14ff and Fig. 3: In the previous implementation 2.5% of applied fertilizer were emitted, whereas in BDSNP are 0.68% were emitted. However, in Fig. 3 the emissions of BDSNP higher than the original GEOS-Chem emissions. Please explain this phenomenon.

13. Page 3560, line 18: Change “Chaparro-Suarez et., 2011” to “Chaparro-Suarez et al., 2011”

14. Page 3564, line 7 and Fig. 4: Please mention and if necessary discuss the variability in the calculated mean of the start/end of the growing season. It would be also nice to add a picture of the calculated growing season length.

15. Same paragraph: What happens in region that have two growing seasons a year, or for example regions in China with a rice-wheat rotation?

16. Page 3566, section 4: Where are the soil NO emissions more sensitive to the emission factors by Steinkamp and Lawrence (2011) and where to the new algorithm of BDSNP? Is a new algorithm justified? This could, in my opinion, be a highlight of the manuscript.

17. Page 3566, line 21: Also displacement of NO enriched air in the soil by water after a rainfall event can lead to pulsing, as well as, but rarely, Chemodenitrification. Since these processes also play a role, they should be mentioned.

18. Page 3567, line 1: The convective precipitation (see page 3569, line 8) is not mentioned here, which lead to 50% increase in the emissions.

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19. Page 3567, line 6: The 0.5 Tg(N) yr^{-1} from atmospheric deposited nitrogen are responsible for 5% of the total emission. The spatial contribution might not be evenly distributed over the whole globe. Please discuss this a little further. And it should be emphasized more in the abstract and conclusions. And this should be one scientific question (see point 5).

20. Page 3567, line 16: The difference between the arithmetic and geometric mean does not say anything about the variability. This is a large logic mistake.

21. Page 3568: Section 5.1 mentions OMI in the title but not in the text anymore. Please rename the section.

22. Section 5.1 and 5.2: Please clarify, that you used daytime values of GEOS-Chem only to calculate the model statistics. This is too important to be mentioned in the appendix only.

23. Page 3568 and Fig 5: A "soil column" is in my vocabulary a column within the soil. Please find another term like "soil derived NO_2 column", this can be abbreviated since it is used more often.

24. Page 3569, line 14: Change ". . . deviations from . . . " to ". . . deviations for June of each year from . . . "

25. Page 3570, line 2: Although the relative anomaly is the same for DP_GC and BEHR, the absolute difference seems to be large. Please add one sentence as explanation.

26. Page 3570, line 18: See point 17.

27. Page 3571, line 9: What are the 0.025mm? Soil water or precipitation, and what was the old value?

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28. Page 3571, line 26: Do your regions of dominating soil NO emissions match to results of van der A et al. (2008)? And why or why not?

29. Please remove the borders of the US states in the global maps.

30. Please remove the county borders (or whatever it is in Fig. 6). US states are sufficient.

31. Page 3586: Steinkamp and Lawrence (2011) also included the above canopy fluxes of the arithmetic mean (8.61 Tg(N) yr-1). Please list this in your table.

32. Page 3590, Fig. 4: Is there really a straight line of growing season start in the Amazon basin?

33. Page 3593, Fig. 7b: A continous colorbar for nominal data is not suitable, please change the colorbar.

34. Page 3593, Fig. 7b: "... following at least 60 days of $<$2 mm day^{-1}." Is "smaller than" correct, if this should depict the beginning of the wet season? Is it a running mean or must each day fullfill the condition?

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