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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of modelled soil biogenic NO emissions on related trace gases and the atmospheric oxidizing efficiency" *by* J. Steinkamp et al.

Anonymous Referee #2

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The paper investigates the influence of soil NO emissions on lower tropospheric NOx, O3, peroxyaceltyl nitrate (PAN), nitric acid (HNO3), the hydroxyl radical (OH) and the lifetime of methane (CH4). To this purpose, twin simulations with a coupled global GCM/CTM are performed, with and without including soil NO emissions. The soil NO emissions are calculated using the classical Yienger scheme. The paper focuses on the impact of soil NO emissions on the troposphere’s oxidation capacity through OH, measured as changes in the lifetime of CH4. The paper is well written, and well constructed, showing the impact of soil NO on species concentrations step by step. The results are also illustrated by figures in and adequate way.

However, the results presented are not really new, In order to make this paper a valuable contribution, the analysis should further as it does right now. Some methodological weaknesses should also be removed.

In the past years, many studies have dealt with the impact of NOx emissions on the oxidative capacity [Fuglestvedt et al., 1999; Wild et al., 2001; Fiore et al., 2002; Berntsen et al., 2005; Naik et al., 2005; Shindell et al., 2005, West et al., 2007]. From these studies it is well established, that NOx emissions enhance the troposphere’s oxidation capacity, decrease the methane lifetime and its radiative forcing. The early paper of Fuglestved et al., 1999 analyses in addition regional differences in the efficiency of NOx emissions to alter CH4 levels. In this framework (but without really citing it), the present paper specifically evaluates the impact of soil NOx emissions. The authors find that globally, they contribute to about 15 % of total NOx emissions in some regions and seasons up to 26%, so these emissions are important to be taken into account. They result in an average CH4 lifetime decrease of about 10%.

If we accept that a specific study on soil NO emissions is valuable, then the presented results rise several questions, which are not sufficiently addressed in the current version :

1) Is there a specificity in soil NO emissions (SNOx), due to their preferred localisation in the tropics, while anthropogenic emissions are larger in Northern Hemisphere temperate regions?

A simulation of an equivalent decrease in total NOx emissions could be performed to answer this question. Probably simulations with regionally reduced emissions as in Fuglestvedt would be also helpful. For example, the comparison with the efficiency of lightning NOx emissions is interesting.

2) On which factors such a SNOx specificity would depend ? This is important from a mechanistic point of view. The authors indicate that the ratio between OH production from ozone photolysis and from the NO + HO2 reaction is a key factor, but they do not

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quantify this ratio. This should be done and other factors also discussed.

3) What is the uncertainty in the presented results ? What is uncertainty in the SNOx emissions calculated using the Yienger scheme ? The authors cite papers using observations to assess this uncertainty.

What are the key factors for model uncertainty in the efficiency of NOx emissions in altering CH4 and can this uncertainty be evaluated ? How does this efficiency compare with that in other models ? These points are not at all addressed in the paper, but need to be.

If these points are not properly adressed, I would not recommand the final version for publication.

Other remarks :

In section 3.1, page 10322, lines 23-26, it is indicated that correlations between SNOx and O3 and OH columns differences could be used to determine the dominating pathway in enhancing OH (O3 photolysis or NO + HO2) reaction. This argument is too qualitative. A quantitative assessment of the impact of both pathways would be interesting here (see above). Probably, this would require specific experiments changing NOx emissions region–wise.

In a general way, the analysis of spatial correlation coefficients does not allow to measure the impact of SNOx on NOx species, O3 and OH, as for some of them, their lifetime allows transport into neighbouring cells. Thus the correlation also depends on species lifetime, transport efficiency, heterogeneity of SNOx.

Minor remarks :

The unity pmol /mol used throughout the paper is may be an SI unit, but nevertheless is strange for me. Is this required by the publisher ?

Page 10236 lines 15-16 : In the vertical …. Sentence not clear.

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Page 10237 line 3: replace “nearly” by “clearly”.

In table 3, better distinguish “global” “and low latitudes” (put space between figures)

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Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10227, 2008.