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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.

J. Steinkamp et al.

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We thank both referees for the constructive comments, suggestions, and information. Detailed initial responses to both are below, along with indications of how the paper will be revised for consideration for ACP. We hope these changes will strengthen the main points which have arisen from our analysis and which we make in this manuscript: 1) SNOx plays a significant role in atmospheric chemistry; 2) its influence is not limited to NOx and ozone, but it is also important for the concentration of OH and the atmosphere's oxidizing capacity, reflected e.g. in the lifetime of CH4; 3) there are notable non-linearities in the response of the photochemical system to the SNOx source, with NOx levels even increasing in some regions when the SNOx source is decreased or removed; 4) due to the large uncertainty in the SNOx source magnitude (from about

4 to 21 Tg/yr), these results stress the need for a realistic representation of SNOx in atmospheric chemistry and climate model analyses.

Referee #1:

General comments

point 1.) "lack of comparison to observations":

The algorithm of Yienger and Levy (1995) is the most widely used method in global atmospheric chemistry-transport and chemistry-climate models to calculate NO emission fluxes from the soil. It is based on measurements in different ecosystems worldwide that were available at that time. The global pattern of the NO emission in our modified version is still similar to the original algorithm, but with a slightly higher total emission (Ganzeveld et al., 2002). In a preliminary study we have compared results from a simulation at an intermediate resolution (T63) versus measurements from the literature covering the period from 1990 to 2000. We plan to include a summary of the comparison and perhaps a few of the key figures from this preliminary study in the revised manuscript. However, we do not plan to go into further detail at this stage, since we are working on a follow-up evaluation at a higher resolution with the original emissions and with "tuned" emission factors to improve the agreement with the observations.

point 2.) "correlation coefficients of column abundance [...] has the problem [...]":

The referee is right that the change in the surface layer will provide a more direct signal of the effect of SNOx (at least on NOx itself). Our intention in including the LT column density changes was to show the larger-scale affect of SNOx. In the revised manuscript we will include, compare and discuss both the correlation to the mixing ratio in the surface layer, as well as the current discussion of the changes in the LT column (with the additional perspective of how this compares

to the changes at the surface).

Specific comments

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Model description and setup:

We will follow the referee's advice to include a brief description of the Yienger and Levy (1995) algorithm, and the modifications which were made in our algorithm.

Results and discussion:

The statement concerning the stronger dependence of PAN formation on VOC in the Northern Hemisphere in our work does not mean "limited". We combined this statement with words like "likely" or "suggest", since as mentioned the PAN calculation in our model shows some discrepancies

with observations. To further soften this statement, we will exclude the sentence "This is [...] (Singh et al., 1986)" on page 10232, line 6-7. We will also include a sentence about the strong temperature dependence of the PAN decomposition, and the note that the O3 and OH concentrations (which in turn depend on NOx) play a role in the formation of precursors for PAN.

Referee #2:

We will clarify the importance of our paper's position amongst other papers that have a focus on the influence of NOx on the atmospheric oxidizing efficiency. While all of the papers mentioned by the referee deal with the NOx-O3-OH-CH4(-VOC) interaction, none of them deals with NO emission from soils, which has generally received less attention in atmospheric modeling than other NOx sources such as biomass burning. None of the publications, except for Labrador et al. (2005)

and Stockwell et al. (1999) (both considering lightning NOx), discusses the nonlinearity that we point out for the regional increases in NOx, despite lower emissions (or vice versa).

point 1.) "Is there a specificity in soil NO emissions [...]":

To show the different behavior of NOx from anthropogenic and other sources compared

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to the soil source, we are planning to include results from another simulation, in which we decrease all surface NOx sources (or perhaps all sources except soil NOx) by the same amount as the SNOx emission are reduced in our present sensitivity simulation.

point 2.) We will implement the diagnostics to identify the dominant source reaction of OH in our new simulation for the revised paper.

point 3.) see answer to referee #1, point 1

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