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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 #1

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General Comments

This paper presents calculations using a global model to evaluate the impact of removing soil nitric oxide emissions on NOx, PAN, HNO3, O3, OH, and the methane lifetime. The coupling of the NOx and HOx cycles is nicely illustrated. And the comparison to lightning NOx provides a useful point of reference. The conceptual points early in the paper are well written but the results are not particularly novel or surprising. There is nothing particularly wrong with the paper, but we are not sure it rises to a level of importance that would justify publication in ACP.

In particular, we note 1) a complete lack of comparison to observations, Of course it is

obviously not possible to observe the Earth without soil NOx emissions. Nonetheless, the authors point out a few regions of the Earth where soil NOx represents the primary NOx source and some evidence that their model accurately reflects the chemistry in these regions would make it a much more valuable contribution.

2) the emphasis on correlation coefficients of column abundance with changes in surface sources as the primary diagnostic has the problem that it cannot possibly account for non-local effects. Such effects are important to the column (and the reason to use a 3-d model and not a box model) where air above the PBL often last touched the ground some distance away. As a result the manuscript tends to reach conclusions about effect that aren’t sufficiently justified.

Specific Comments

Model description and setup

The strength of the soil source in Tg(N)/yr should be included in this section. Additionally, more information about the soil model should be provided, such as how fertilizer application and rainfall are treated.

Results and Discussion

It is not clear that there is a “global” effect of soil NOx as almost all of the effects are nearly zonal (or so it appears from the analysis presented). It would be clearer to present the hemispheric results first and then present the globe as a sum of these effects. For example, the authors report a 6% decrease in NOx mixing ratios in DJF despite the increase of NOx (figure2a) in most of the Northern Hemisphere.

With respect to PAN, a small correlation coefficient between surface soil NOx fluxes and the difference in PAN concentrations does not justify the statement that PAN formation in the northern hemisphere is VOC limited while in the southern hemisphere it is NOx limited. The correlation coefficient neglects effects such as the relative locations of the uplift (which is a strong determinant of column PAN) and soil NOx sources

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and the influence of changes in the large scale background ozone (which affects PAN production rates). These factors may be more important than VOC in controlling the PAN-soil NOx relationships. Also, in locations where it is warm, PAN concentrations are relatively insensitive to NOx because NO2 appears in the source term while NO in the sink. Thus even in the Southern hemisphere the increase in PAN is most likely associated with the increase in OH and not a direct NOx effect. (see Cleary et al. ACP 2007 for a discussion of PAN in steady-state)

Technical Corrections

P10229 line 14: change peroxyl to peroxy.

P10234 line 21: It appears that the mixing ratio percent changes are switched for DJF and JJA.

Figure 10: A plot of the difference between the two model runs would be useful.

Reference

P.A. Cleary, P.J. Wooldridge, D.A. Day, D.B. Millet, M. McKay, A.H. Goldstein, and R.C. Cohen, Observations of total peroxy nitrates and aldehydes: measurement interpretation and inference of OH radical concentrations, Atmos. Chem. Phys. 7, 1947-1960, 2007.

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