**Atmospheric
Chemistry
and Physics
Discussions**

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Comment

Interactive comment on "Improvement and evaluation of simulated global biogenic soil NO emissions in an AC-GCM" by J. Steinkamp and M. G. Lawrence

Anonymous Referee #2

Received and published: 23 August 2010

Unfortunately I didn't manage to provide a very extensive review of this paper. Given that the other reviewer provided a very extensive review of methodological issues, I will provide a rather general feedback of the paper.

The authors revisited a 15 years old paper by Yienger and Levy regarding soil NOx emissions, and update the parameters in that model with more recent measurements and model parameters. Indeed most global models use some version of the Yienger and Levy parameterisation, and the update of this work is timely.

The work is interesting and should be published- but to my opinion the authors could improve the the presentation and organisation of the paper to make the short coming

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of the old method clearer, and how that has been addressed in the current paper. I recommend publication- trying to address the comments of the other reviewer, but also focussing on the take home messages in abstract and conclusions, which should at least contain the main results. Very often in the paper it is not clear which process was or was not included in the specific section, this is an organisational issue, that should be improved.

Specifically:

p. 16008 l. 16 what was wrong with the old land cover map?

p. 16008 l. 18 why update for fertilizer use was needed?

p. 16008 l 20 so what do you calculate, and what do top-down approaches calculate? In which regions according to your new set of calculations the soil nox is less or more important than previously thought? What are the main remaining uncertainties?

p. 16009 What kind (range) of fluxes do the top-down approaches suggest? And what are the published values in the model estimates? Do they all use an inter-active calculation or rather some version of the 'static' fluxes?

p. 16010 What was the reason for using 10 years of 'nudged' ECMWF ERA40 in an off-line fashion. What was the advantage of 10 years. I note that the same surface parameters would be provided by ECMWF on a much higher resolution for recent operational analysis.

p. 16010 the four steps (LC, LC+FIE, etc) do they correspond to 4 simulations, discussed in 4 sections? In reading the manuscript it is not always clear what simulation is discussed.

p. 16011 section 2.1 I guess this is the old model, not state-of-the-art?

p. 16012 I am surprised that the issue of CRF is not worked out better- e.g. with the canopy sub-model introduced by Ganzeveld.Also since vegetation capture seems so

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important, it must also play a role in the measurements. Can you explain how this factor has been accounted for?

p. 16012 This section tries to mimic the 'old' YL simulations. It is not clear to me, whether YL95e is close to the original, and what are the differences, just from using a different model, climatology etc.

p. 16013 It is of course good to use a newer LC database, but at least discuss what kind of issue would play when using the 'old' one. Mention what is done when no data were found for a certain LC type.

p. 16013 It is nice to know that there are different ways of fitting to the data (ignoring <0, or not), but it is not clear how this information is later used.

p. 16014 'perturbed' versus unperturbed, I am surprised that this topic is not further picked up in the uncertainty discussion. One could imagine for certain major natural ecosystems to also evaluate the 'lower' values,

p. 16015 l 7, I didn't quite get the problem ('checked'?). Do you mean information on the temporal behaviour of the precipiation was not stored for more than one time step?

p. 16015 If I understand well the only thing changed is the Land cover data? What simulations do the two number correspond two (YL95o and new?) Does this contain all parameters (pulsing, CRF, fertilizer)?

p. 16016 l. 5 Here I understand that something different is done regarding fertilizer?

p. 16017 Section 3.3 is interesting, but not very well worked out. l. 5 is the 'opposing' effect? The solution that is chosen in some model versions, is to use the highest resolution of the driving database for surface parameters calculations.

p. 16018 It would help making statements like 'our best estimate of fertilizer induced NOx emisisons is: I understand this section is still using the 'old' database? p. 16019 it is not clear to me why the 'new' VSM is in principle better. Please explain.

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p. 16019 this section is somewhat disconnected from section 3.2 where the database is introduced. This section also needs a few introductory sentences regarding why the whole section below is done I guess the problem is that many measurements are available for short periods- and you want to match equations 1 and 2? for this period only. Is this done off-line? It is not clear to me how different emission coefficients obtained from various measurements, were combined to a single coefficient per ecosystem. A simple average? p. 16021 twice as high as original YL? You always compare new with YLold?

p. 16023 What were the values from Stehfest and Bouwman?

p. 16024 CRF and previous sections: to me it is not clear whether previous calculations were with or without CRF; I suspect that the number of 9.01 was already with CRF?

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 16007, 2010.

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