Atmos. Chem. Phys. Discuss., 10, C10149–C10155, 2010
www.atmos-chem-phys-discuss.net/10/C10149/2010/

Atmospheric Chemistry and Physics Discussions

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

J. Steinkamp and M. G. Lawrence

joerg.steinkamp@mpic.de

Received and published: 23 November 2010

We would like to thank the two referees for the extensive, detailed and constructive comments, which will help to improve the quality of our article. Some of the referee comments brought up new questions, which will need more time still to address adequately. Thus here we reply to the major points of the two referees, and we will provide a more detailed final response together with the revised manuscript, when we resubmit it.

Referee #1:

a.) We agree with the referee that the reduced emissions in "rice producing regions"

can be considered a weak point of the manuscript. There are not enough measurements in rice paddies to either accept or reject this reduction in our compilation of measurements. We have restated this in the manuscript.

Furthermore, the suggestion of considering different fertilizer usage per crop type led us to attempt to calculate the emission factor per crop type separately; the results of this will be included in the revised manuscript. However, a full implementation of this as a combination of consistent global maps of natural landcover, crop types (e.g., Monfreda et al., 2008) and the fertilizer use per crop provided by FAO (2006) would introduce many new uncertainties to the resulting emissions, and would go far beyond the scope of this study (though it could provide the basis for an interesting future article). A particular challenge in such a future study would be to examine the dependence not only on the amount of fertilizer, but also on the fertilizer type and how it is applied. As long as detailed information like this is lacking, we prefer to make use of the simple implementation as it is now, though with the calculation of emission factors per crop type we provide the basis for such future work.

b.) Although it would have been nice to have been able to apply a simpler algorithm, the measured fluxes unfortunately spanned several orders of magnitude, so that we could not find a simpler algorithm that would be sufficiently accurate. We used the modeled time series of soil temperature and moisture to calculate emission factors during each corresponding period of the measurements. The emission rate is not proportional to Aw, since the dependence on the soil temperature is not continuous. If we were to have had enough time series data of the measured emissions, then we would have been able to use the simulated soil moisture separately for the periods classified as dry or moist to determine the two emissions factors separately. At present this is not yet possible, but left as a suggestion for future updates, when more data becomes available.

c/d.) We have also now included the calculation of the arithmetic mean of the emission factors as suggested. However, this raises the yearly global SNOx up to 32.8 Tg(N) without the reduction in "rice producing areas" and to 28.0 Tg(N) with the reduction, resulting in an above canopy flux of 27.2 Tg(N) and 23.2 Tg(N) respectively. This is substantially larger than most other studies of SNOx (including the provided reference of Wang et al., 2007), so that we find it more sensible to apply the geometric averaging for our recommended emissions parameters; this will be discussed in more detail in the revised manuscript.

Referee #2:

"What was wrong with the old land cover map?"

The old ecosystem map as it was implemented showed some incorrect classifications, especially in dry regions. Furthermore, it was overlaid by the fraction of agriculture derived by Lex Bouwman, which resulted in a reduced emission flux in non-agricultural ecosystems, which, according to the old ecosystem map, was agriculture. In addition, the new ecosystem map contains more recent information, is at a higher resolution and is available for different years from 2001 onwards.

"Why update for fertilizer use was needed?"

We employed a yearly varying fertilizer usage, since the fertilizer consumption increased in the eleven years of our simulation by 18%.

"What are the main remaining uncertainties?"

Several of these are outlined in the discussion above, for example, the lack of sufficient data to determine the emissions factors for wet and dry seasons individually. A brief discussion of these will be added to the revised manuscript.

"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?"

We will add a brief listing of the fluxes concluded by published top-down studies, and

compare these to the simulated fluxes in a few representative modelling studies.

"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."

We choose the period of 1990-2000 since most measurements of our compilation were performed during this period. The 11 years give enough time to observe interannual variability, as well as the impact of the trend (18%) in fertilizer application during this period. Although it would indeed be possible to use the ECMWF data directly for this study focused specifically on the emissions, our main purpose is to lead to improvements in atmospheric chemistry models; thus, we prefer to perform this study in the framework of a atmospheric chemistry GCM (including the typical resolution for full-chemistry simulations), so that the results will be directly applicable to such models. We also note that the computation of soil moisture, one of the key parameters in determining the emissions, is comparatively crude in the ECMWF model (Drusch et al., 2009).

"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."

Each of the four steps builds on the preceding step, starting with LC. Each one also represents a separate offline simulation, using the same soil moisture and temperature derived in the YL95e "online" simulation. We will try to make the references to the individual simulations clearer in the revised manuscript and call the original publication by Yienger and Levy (1995) "YL95", the version developed by Ganzeveld "YL95EMAC"

"I guess this is the old model, not state-of-the-art?"

With "state of the art model" we meant the emissions algorithm as it was implemented in the version of EMAC before our improvements. Since this can be confusing, we will revise the terminology (see above).

"I am surprised that the issue of CRF is not worked out better- e.g. with the canopy submodel introduced by Ganzeveld.Also since vegetation capture seems so important, it must also play a role in the measurements. Can you explain how this factor has been accounted for?"

Although it might have been interesting to see what kind of effective canopy reductions are computed by a multi-layer canopy model, unfortunately the Ganzeveld module has not been implemented into the base model that we work with (ECHAM5/MESSy), only into the previous version (ECHAM4/CHEM), and the implementation would be an extensive effort. Furthermore, it would have little impact on our main results, since nearly all of the measurements we used were chamber measurements close to the surface, for which the canopy interaction should play a minor role. The canopy reduction factor is only needed for comparison to top-down inventories and the chemical processes of the atmosphere. Here, as noted above, since our primary goal is to provide an algorithm that is directly applicable in our and other atmospheric chemistry models, most of which will also not have a separate multi-layer canopy module, we think the general canopy reduction factor is most appropriate to use here, and will add this to our overview of major potential further improvements in the future.

"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."

The YL95e method was adapted from YL95 by Ganzeveld et al. (2002, 2006) and merged into the ONLEM submodel (Kerkweg et al., 2006). The differences to the original YL95 algorithm are: 1) a different ecosystem map; 2) usage of the soil water column instead of the precipitation history to distinguish between wet and dry soil; 3) usage of the soil temperature instead of the 2 meter air temperature; 4) usage of the data from Bouwman et al. (2002) for agriculture and fertilizer induced emissions; and 5)

a different underlying GCM. Though these are mostly noted in the paper by Ganzeveld, We will include this explicit listing to make it more clear in the revised version.

"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."

The differences between using the old LC database and using the new database are shown specifically in the difference plot in Figure 5. We are not sure why this did not come across clearly, but we will try to state this more clearly in the revised manuscript.

"'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,"

This is an interesting point. Instead of only picking this up in the discussion, we have also explicitly calculated the emission factors for the natural land cover under perturbed and unperturbed conditions, to attempt to differentiate these. As it turns out, for woody savannah this results in an increase by 0.5 Tg(N)/yr; however, this is almost exactly compensated with slight decreases in other land cover classes, so that with the separate treatment, the total global flux increases only insignificantly, from 9.37+-0.17 to 9.40+-0.17. Where available we will recommend the "unperturbed" emission factors for future usage.

"It is not clear to me why the 'new' VSM is in principle better. Please explain."

In the YL95e version of the model the soil column of the model can only hold a certain amount of water (in m) against the gravitational force (field capacity of soil). This is not allowed to be exceeded. If the soil water content of a model surface layer grid cell (which is equivalent to the soil column for a single-layer representation of the soil moisture) does go above a given threshold (here 0.1m), then the soil is classified as wet, no matter how deep the soil layer is and what the field capacity is. By an explicit calculation of the volumetric fraction of water in the soil column we improve the differentiation between the soil moisture states. This is a step towards a better representation of the soil moisture state, which will become more important when land surface models coupled to GCMs have several layers for the soil moisture, and only the soil moisture of the upper layer, where the NO is produced, can be used to distinguish the two soil moisture states (or perhaps in a smooth function between the extreme states).

"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?"

All numbers before section 4 were without CRF, since the CRF is only important for the comparison to the inverse modelled soil emissions derived with satellite products, as discussed above. We will note this explicitly at the beginning of section 3 to avoid confusion

Drusch, M. et al.: Towards a Kalman Filter based soil moisture analysis system for the operational ECMWF Integrated Forecast System, Geophys. Res. Lett., 36, L10401, doi:10.1029/2009GL037716, 2009.

FAO: Fertilizer use by crop, FAO fertilizer and plant nutrition bulletin 17, Rome, 2006.

Monfreda, C. et al.: Farming the planet: 2. Geographic distribution of crop areas, yields, physiological types, and net primary production in the year 2000, Global Biogeochem. Cycles, 22, GB1022, doi:10.1029/2007GB002947, 2008.

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