Biogeosciences Discussions, 2, S382–S385, 2005
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European Geosciences Union

Biogeosciences Discussions

Interactive comment on "Inventories of N_2O and NO emissions from European forest soils" *by* M. Kesik et al.

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Received and published: 2 August 2005

General comments:

This well-written manuscript provides an analysis of the application of linked process models for the regional estimation of NO and N2O emissions from forest soils. The argument is presented that highly parameterized models may yield regional estimates of soil emissions of these gases that have some advantages over more empirically based methods of scaling up field studies. Of course, numerous approaches (mechanistic modeling, empirical modeling, simple extrapolations) can be complementary. I see this paper as a significant contribution to this complementary approach to assessing regional emissions. I have a few specific areas of concern that I believe could be improved in the final publication.

Specific comments:

1. Just as oxygen varies on a microsite scale and required innovative modeling methods, such as the "anaerobic balloon," pH also varies on a microsite scale. This small-scale variation in soil pH probably profoundly affects rates of chemodenitrification. If the algorithm introduced in this paper does not address this microscale variability of soil pH, then that should be discussed as an area of needed model development and a potential source of error in the simulations presented here. This may be relevant to points below about the unusually high NO emissions simulated for areas such as Sweden.

2. I don't understand how it is possible that the Monte Carlo approach yielded lower simulated emissions than the MSF approach, which was designed to yield a minimum estimate based on the extreme values of each input variable that yields the lowest flux. I suspect that I am simply misunderstanding the last paragraph of the methods section, but perhaps this section could be revised in improve clarity.

3. In Figure 6, points 10-15 are above the line and 38-41 are below it. Isn't there a mull/moder difference between these two sites that might explain this result and that is consistent with literature discussions on this topic? Similarly, the sites with the highest NO emissions (points 16-20, 44-45) both have moder soils, which may be related to your point about the probability of NO consumption prior to escape of the NO from the forest floor surface. Because this moder/mull dichotomy has been discussed in the literature with respect to whether fluxes are dominated by NO or N2O, it might be appropriate to discuss how the model deals with it, either directly or indirectly.

4. It would be interesting to overlay your simulations of NO and N2O to develop a map of the ratio of simulated NO:N2O emissions. It would be instructive to see how this ratio covaries spatially with soil texture, pH, N deposition rates, precipitation, the difference between PET and AET (related to soil wetness), and the moder/mull dichotomy. This analysis would complement the mechanistic approach of the CASA model, which uses the "hole-in-the-pipe" conceptual framework. I believe that we can learn a lot about mechanisms by examining variation in NO:N2O ratios.

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5. In addition to the point raised about the pH sensitivity of chemodenitrification, another suspicious structure in the model is the tendency for soils high in SOC to have higher N2O emissions. I can understand that large N stocks might reflect higher rates of N cycling, including emissions of NO and N2O, but that would apply only to soils with both high SOC and a low-to-moderate C:N ratio. Soils with high SOC and a high C:N ratio may have very conservative N cycles and low N2O emissions. Soils with high C:N ratios may also be acidic, which leads us back to the chemodenitrification topic regarding NO emissions. Both the acid soils and the SOC-rich soils are common in Sweden, where, unfortunately, there are no validation data except perhaps the Johansson measurements of NO fluxes (as you correctly point out). I agree with your statement on page 803 that NO consumption may be one of the processes poorly represented in the model, but I believe that pH effects on chemodenitrification (due to microsite variability in pH) and the SOC driver of N2O emissions may be equally suspect and meritorious of further study and model development.

6. The presentation of a range of 13-350 kt N/yr for NO emissions from European soils from the Simpson et al paper makes it appear that the Davidson and Kingerlee approach is woefully inadequate. While I agree that our approach has its limitations, as do all approaches, I feel that the Simpson et al application of that approach was rather hastily done. They simply took our estimate of NO emissions from temperate forest soils that had not been significantly affected by N deposition and multiplied that by the area of all European forest to get their estimate of 13. They then multiplied the same area by our estimate of NO emissions from N-deposition-affected areas to get the 350 estimate. Surely, this range could be narrowed by attempting to estimate what fraction of forest has been significantly affected by N deposition and what fraction has not, thus eliminating their simplistic all-or-nothing approach estimating the area of N-affected forests. Of course, this definition of "N-affected" forest is somewhat subjective, so there will still be a significant range of uncertainty, but not nearly so bad as presented by Simpson et al. I really think their estimate of the "Davidson and Kingerlee range" is not worthy of citing, although it would be useful to compare your estimate with one that

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used a simple extrapolation approach to scaling to the region, based on some spatial stratification of N saturation.

7. Figures 6 and 7 include both temporal variation (years within sites) and spatial variation (differences among sites). I wonder if it would be more useful to compare differences among sites by taking the mean of all years of observations and simulations for each site and basing the regressions in these figures on only one point per site. As it now stands, the regressions are dominated by the fact that two or three sites have multi-year records, which have a large influence on the regression slopes. For an analysis of differences among sites, each site should have equal weighting. Interannual variation within sites might be investigated by normalizing each year's estimate by its multi-year mean. Separating temporal and spatial variation may thus yield greater insight into the model's strengths and weaknesses.

Technical comments: I hope that the final published versions of the tables and figures will be much larger. I could barely read them when I printed them out.

Respectfully submitted,

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Interactive comment on Biogeosciences Discussions, 2, 779, 2005.