**Biogeosciences
Discussions**

Interactive comment on "Quantifying nitrous oxide emissions from Chinese grasslands with a process-based model" *by* F. Zhang et al.

Anonymous Referee #3

Received and published: 11 April 2010

1 General remarks

This manuscript reports on the generation of N2O emission maps with the DNDC dynamic ecosystem model for grasslands over China, at county-scale. The maps reveals emission patterns essentially related to climatic zones, while emissions fluxes are relatively low compared to other grassland types elsewhere in the world because fertilizer inputs are marginal. While the methodoloy used by the authors may be considered as fairly standard (examples of running DNDC on GIS data sets were reported in the literature as early as 1996), the results are original in that none such work was carried out for this type of ecosystems in China, let alone in the world for unfertilized grasslands. Thus the manuscript makes an interesting contribution in the growing body of work

on the generation of N2O inventories using biophysical models and GIS data bases. However, this manuscript needs significant improvements on 3 counts prior to be considered for publication: first it is concise to the point of being mostly elliptic, leaving up to the reader to figure out what the results really mean or how they were obtained. Secondly, the validation claimed by the authors is obscure and should be expanded for the results to bear any kind of plausibility. Lastly, an analysis of the uncertainties and errors likely generated in the upscaling process (from plot- to regional scale) would provide a necessary insight into the accuracy of the emissions simulated (to which the sensitivity analysis is a welcome starting point).

I therefore strongly recommend that the authors i/ go back to their manuscript and expand the methodology (see Specific remarks below), their analysis of the results, and the discussion section, ii/ explain and strengthen their validation with quantitative figures, and iii/ explore the major sources of uncertainty in their estimates.

2 Specific Remarks

2.1 Introduction

There is no state-of-the-art on using biophysical models to generate N2O emission maps - should be added (and will likely give interesting ideas to the authors). The authors emphasize the changes in grassland management, among which the shift to fertilization (one of the 'most adopted options' on p. 1678 l. 19), however in the rest of the paper they neglect fertilizer inputs. It seems contradictory.

2.2 Materials and Methods

§2.1.1 What do the authors make of the varying estimates in grassland area? Is the digitizing method the most accurate ? §2.1.2 Soil data set: are all the DNDC parameters included in the soil map, and what is their accuracy? Why use only the topsoil data in DNDC (does not the model simulate plant growth and its rooting system, which goes beyond the 10-cm depth) ? §2.1.3: what about solar radiation data (needed to grow the grass) ? §2.1.4: using a fixed grazing stock over the whole of China is for sure a crude assumption. Should be looked into in the uncertainty analysis (and it came out in the sensitivity test).

§2.2 Where are the results of the uncertainty analysis on soil C content? Other parameters should be included in the analysis (eg grazing stock rate). The last sentence belongs in validation (2.3)

§2.3 The methodology for 'validation' is obscure. How was DNDC parameterized for the 8 test sites in China (based on grid-cell simulations?) What about the other inputs (management, grazing, weather data)? The time-scale of the observations (these must have consisted of daily emission rates rather than annual totals)? Simply presenting a regression with annual emissions is not sufficient (especially since some of the model inputs may have been tweaked to reach a good fit). I am still puzzled by the use of the 2 US sites to validate simulations in China. Also, the alpine meadow site in China presented a serious challenge for DNDC, so is was set aside in the validation regression, but the authors do not offer any explanation as to the misfit.

§2.4 and Figure 3: it is easier to interpret the results of the sensitivity analysis when presenting variations relative to a nominal value rather than absolute values (we do not know what the nominal N2O efflux is).

2.3 Results

§3.1 to 3.4: no need for separate sub-sections of only a few sentences here, they should be merged. It is not because a model is sensitive to both management and soil/climate factors that it will necessarily provide good projections in relation to climate change (p. 1683, top).

§3.5: what is the basis for the 'ecological zoning' (no reference here)? and for the climatological zoning presented afterward ? What is the spread around the mean values for the 3 zones, and are these means statistically different ? §3.6: the increase of N2O emissions from grasslands with time (l. 15 p. 1685) is puzzling, given the strong inter-annual variations of N2O emissions. The authors offer climate change as an explanation, which would no doubt be interesting, with maps supporting the increase in temperature and precipitation in the north. Some factorial analysis might be helpful to substantiate that claim (for instance running the model with the same input data except climate for all years). The last sentence hints that some other factors seem to have come into play (eg the area of temperate grassland). The authors should be more specific.

§3.7: for sure the hypotheses on nitrification and denitrification activities could be checked by looking at simulated data (l. 3 p. 1686).

§3.7.1: I am not sure it is relevant to compare the montane grasslands of China with subalpine meadows of the US. The climate may be similar but no doubt the soils are not, and DNDC is quite sensitive to soil parameters (among which soil carbon content). The authors should look into that too to make sure their comparison is relevant. The same could be said of European grasslands (which have higher inputs, anyway).

Overall, given that the model was only run on a sample of 8 meteorological years, I am not sure its results may be generalized so far as the overall N2O source strength of grasslands in the world. What are semi-natural grasslands (l. 26 p. 1687) ?

What are 'grassland-farming' rotation systems ? (l. 6 p. 1688)

2.4 Figures

Fig. 1: explain climate classification, and locate validation sites on the map. Fig. 2: what matters in model validation is the mean deviation and root mean squared error, the regression parameters are far less relevant (see Smith et al, 1996).

2.5 Tables

Table 1 and Figure 2 duplicate the same data. Table 2 should give the range of the parameters.

3 References

Smith, J. U.; Smith, P. Addiscott, T. M. Powlson, D. S.; Smith, J. U. Smith, P. (ed.) Quantitative methods to evaluate and compare Soil Organic Matter (SOM) Models Evaluation of Soil Organic Matter Models, Springer-Verlag, 1996, 181-199