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

Anonymous Referee #1

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This is a well written paper on a relevant subject. It describes the application of a process-based model (DNDC) to the estimation of national nitrous oxide emissions from managed grasslands. The layout is very clear, the text concise, the use of the English language very good, selection of tables and figures adequate and the graphical quality of the plots excellent. Overall a nice contribution to Biogeosciences.

There are, however, a few issues in relation to the details of the paper that need to be addressed with minor revisions.

(A) Management. The only management option considered for the application is grazing, which is probably a realistic assumption for China, as (I imagine) hay production plays a minor role. The authors assume a constant grazing intensity across all of China, with stocking density estimated as total livestock divided by total area (Section

2.1.4) and number of grazing days per year and grazing hours per day as given in Tab. 2. Although there is a necessity, in this type of studies, for simplified approaches, the latter assumptions can be questioned as (likely) not all of the Chinese grasslands can sustain the same animal population. In this sense, an alternative and somehow more realistic solution would be to relate the grazing intensity to the potential productivity of the different grassland types. The latter could be estimated from climate data using empirical relations (e.g. as done in Sala et al., 1988 or Lauenroth and Sala, 1992) or running a model like DNDC without grazing. As I am aware of the required working load,I am not asking for a re-evaluation of the inventory. On the other hand, I'd like to encourage the authors to more thoroughly comment the assumptions on management in Section 2.1.4. and again in the Conclusions (things that could be improved).

(B) Model validation. As explained in Section 2.3 the model performance is tested with reference to ten grassland sites in China and United States, but the authors do not report the management at these sites, in particular whether they are grazed or not. Results of the sensitivity analysis suggest that stocking rates and grazing time are relevant for explaining N2O emission. In view of the importance of grazing for the overall assessment of N2O from Chinese grasslands, commenting on the management at the validation sites would help understanding whether DNDC performs properly with respect to the specific settings of the study.

(C) Sensitivity analysis. While the results presented in Fig. 3 are per se instructive, I am missing the motivation for a sensitivity analysis in the context of the preparation of national inventory. One logical reason could be the necessity to quantify uncertainties in the output (e.g. average annual emissions are 76.5 Gg N2O-N +/- what?) given uncertainties in the inputs. Apart from the assumptions on management, these could arise from uncertainties in original data (e.g. soil data, climate data), the preprocessing of inputs (e.g. spatial interpolation), and of course model uncertainties. A second reason could be the need to explain inter-annual variations in N2O emissions. Yet another reason could be a discussion of the implications of changes in the man-

agement regime, e.g. intensification of grazing. In any case, it is desirable that the authors provide a rationale for the sensitivity analysis and discuss the results shown in Fig. 3 from a broader perspective.

(D) National inventory. The results of this study are discussed in Section 3.7 in relation to emission estimates for other areas of the world. However, it is also important to set the work also in the context of the worldwide efforts to establish national greenhouse gas inventories. In most cases these are based on the IPCC methodology (http://www.ipcc-nggip.iges.or.jp/public/2006gl/index.html, accessed 31.02.2010). I couldn't find anything specific in this direction in the Introduction. Two aspects could be of interest. On the one hand, the authors could provide their point of view on the advantages of a spatial application of DNDC (with the specific setup of the study) as opposed to the IPCC methodology. On the other hand, they could discuss the estimated total emissions from grasslands (76.5 Gg N2O-N per year) in relation to the figures (in particular N2O and GHG emissions for the agricultural sector) reported in the Initial National Communication on Climate Change by the People's Republic of China (available under http://unfccc.int/national_reports/non-annex_i_natcom/items/2979.php, accessed 31.03.2010).

(E) Climate change and N2O emissions. In section 3.6 and again in the Conclusions, the authors emphasise the positive trend in N2O emissions over 2000 to 2007 and propose a possible link to climate change. In view of the (not quantified) uncertainties in the emission estimates and the shortness of the time period considered, I would be more cautious in speaking about trends in N2O emissions in response to trends in climate.

(F) Minor issues. (1) Please specify whether grassland and soil data were interpolated to the same grid as used for the climatic data. (2) There is an inconsistency between the number of 10 validation sites mentioned in Section 2.3 and the 11 sites listed in Table 1. (3) Please refer to all of the panels of Fig. 3 in discussing the results of the sensitivity analysis in Section 3.

References

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