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Biogeosciences Discussions

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

Anonymous Referee #2

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General comments

This MS describes the application of the DNDC model to estimate N2O emissions from grasslands in China. The paper is well-written and the subject matter would be of interest to readers of this journal. However, some attempt to estimate the uncertainty in the model predictions needs to be included. A few other technical points are addressed in the comments below.

Specific comments

The total N2O emission from grasslands in China is estimated using DNDC emission rates calculated for 18 types of grasslands. However, despite the paper including a section on sensitivity analysis, no uncertainty in the national emission rate is reported.

It would also be interesting to compare the value of the N2O emissions calculated using DNDC with the current inventory method.

Climate Data: did the use of kriging on precipitation significantly change the number of rainfall days compared to the climate stations? With DNDC the distribution of the rainfall events can be just as important as the total amount of precipitation, so if the interpolation method produces (for example) a larger number of rainfall events with less rain per event this should be noted as a possible source of error.

Modelled N2O emissions: the caption for Table 3 should note that the modelled values are averages using 8 years of climate data. Some sort of uncertainty estimate for these figures should also be included. (According to the methods section the effect of varying SOC was investigated, there's also the effect of annual weather patterns that could be considered).

Grassland categories: it looks like the N2O emission rates in Table 4 have been calculated by taking the unweighted means of the emission rates in each of the grassland types. However, it would be better to take the N2O emission rate as: (Total N2O emission)/Area, as this gives appropriate weight to each grassland type according to its area.

Section 3.6: The P-value in Figure 6 is 0.1414, so the trend is not statistically significant. This is unsurprising as 8 years is a small time-frame to expect to see significant climate change effects. It would also be useful to confirm that there are no other long-term effects that might be influencing N2O emissions (e.g. is there any major build-up or loss of SOC?). If weather differences are the major cause of inter-annual variation in N2O emissions then these results could be used to infer what changes future climate change might make to N2O emissions.

In line 22 it is stated that the precipitation and temperature "significantly increased" between 2001 and 2005. It seems unlikely that there was a significant increase in the statistical sense, so this should be rephrased to avoid confusion.

BGD

7, C444–C446, 2010

Technical corrections

Pg 1677, line 8: more up-to-date figures for the relative contributions of anthropogenic N2O, CH4 and CO2 emissions are available in IPCC 2007

Pg 1677, line 25: “The IPCC method was frequently used. . .” this sentence doesn’t quite seem complete. Who used it and when?

Pg 1688, line 20: N2O emissions have not increased “constantly” from 2000-2007. For example the N2O emission in 2005 was higher than 2006 according to Figure 6.

Section 3.2-3.4: References to Figures 3a-g when there is only Figure 3.

Table 1: The measured value in Xu(2003) is 0.296 kg N/ha/y

Page 1701: Should be “sensitivity analysis” rather than “sensitive analysis”

Interactive comment on Biogeosciences Discuss., 7, 1675, 2010.