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Interactive comment on "Effects of mowing on N_2O emission from a temperate grassland in Inner Mongolia, Northern China" *by* L. Zhang et al.

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Reviewer's comments on BGD manuscript "Effects of mowing on N2O emission from a temperate grassland in Inner Mongolia, Northern China" by L. Zhang et al.

General comments

The manuscript describes the results of a long term (2003-2009) grassland mowing experiment in a semi-arid Inner Mongolian steppe, reporting the effects in terms of N2O emissions of different mowing heights vs a no-mowing control. The overall conclusion is that mowing only marginally decreases N2O emissions (the differences are not statistically significant) vs the no-mowing control. While the paper is competently written, and the presentation of results is appropriately illustrated with tables and fig-

ures and adequately discussed, I have a small reservation as to the general design and objectives of the experiment. Namely: the introduction states that traditional (sheep) grazing and mowing are prevailing management practices in Inner Mongolia, and that mowing is increasingly recommended (as an alternative to traditional grazing) for sustainable grassland management, with a view to restore the degraded steppe ecosystem in western China. The paper proposes then to study the effect (on N2O fluxes) of the introduction of mowing as an alternative management practice to traditional grazing. The problem is that the control treatment is not grazed, but left uncut, unmanaged.

If the aim of the experiment was to make a comparative assessment of the net N2O budgets in the traditional grazing and in the alternative mowing management, then the control should have been a grazed paddock with a realistic livestock density, and the mowing treatments at different heights should have been complemented by N2O flux measurements in the stables, or wherever the grass, silage or hay is fed to the animals. Indeed, the change in management practice for the steppes (switch from traditional grazing to mowing) presumably still aims to maintain animal production (meat, milk, wool, etc) from the ecosystem, not to turn this into a nature reserve. Thus the N2O emissions associated with off-land animal feeding operations should be quantified.

I understand that such measurements were not carried out in this study. In this case, the scope, ambition and objectives of the paper must be confined to the effect of the mowing height on the N2O emission, but no statements should be made regarding the impact of the change of management practice (grazing to mowing) on the overall N2O budget of the grassland+herbivore production eco-system. Statements such as "...the introduction of mowing as a management practice might decrease N2O emissions in grasslands..." (abstract) and "...by changing grassland management, such as introducing a proper mowing intensity, the greenhouse effects of N2O emission might be mitigated in the grassland..." (discussion) are in my opinion misleading for scientists, stakeholders and policy makers, even if stricto sensu it may be true (through not statistically significant in this study) that N2O emissions are smaller in mowed than in

unmowed grassland.

The paper may be published after a careful screening of the text to change the wording of such statements, in order to make it clear that the N2O budgets shown are in no way complete (animal emissions missing) and cannot be used by themselves to assess the overall impact of the change of management practice and production system in support of policy assessment.

Specific comments

Abstract, p19220, p22-24: "...grasslands, along with proper management practices, can be a N2O sink mitigating the rise of N2O in the atmosphere." The potential for soil N2O sink activity has been demonstrated before, but the recent review by Schlesinger (2013) suggests that this effect is a very minor one on a global scale.

p19224, N2O flux measurements: the fluxes measured in this experiment were generally extremely small, as one would expect in an unfertilized, ungrazed grassland, ranging from -32 to +67 μg N2O m-2 hr-1, ie from -9 to +19 ng N2O m-2 s-1. In some N2O flux measurement systems, many of these fluxes would not be considered significantly different from zero. Please provide an estimate of your flux detection limit, as well as the N2O precision by GC.

p19225, l18: total nitrogen (TN) was measured, but later in the text (p19231, l20) it is suggested that mineral N (NH4+ and NO3-) were also determined. If that was the case, it would be arguably more informative to show the mineral N data in Table 2 and Fig.4 than TN, as N2O production proceeds directly from mineral N by nitrification and denitrification.

p19226, l12-14: is there a substantial snow cover in winter? what is its contribution to total precipitation? Could there be substantial N20 emissions during freeze-thaw events and at snowmelt, as observed in many studies ? (eg, the Wolf et al paper, Vol 464, 8 April 2010, doi:10.1038/nature08931, reports that "...short-lived pulses of N2O

emission during spring thaw dominate the annual N2O budget ..." at their 10 Inner Mongolian study sites). In this case the N2O budgets presented here on the basis of the summertime measurements are obviously far from representative of the whole year. Please discuss.

p19226, l15-16: please make it clear that the min and max T are for the N2O measurement period only (summer), but please also provide the temperature range for the whole year.

p19227, Temporal dynamics in N2O fluxes: having determined your flux detection limit in the M&M section, please indicate the fraction of the flux distribution that is not significantly different from zero.

p19228, l21-22: it is not clear here and in Fig. 4 whether the data from all treatments were pooled for this analysis; please provide details of the analysis.

p19228, 24-26 "...above-ground biomass ... and microbial nitrogen...": the R2 values provided are calculated from fitted non-linear (seemingly quadratic) regression curves, the shapes of which are not convincing, unjustified and counterintuitive; for example why should N2O emission increase as microbial biomass nitrogen tends towards 0? I suggest to remove such regressions for microbial N, above ground plant biomass and total phosphorus, as the date do not indicate any clear relationship or trend. On the other hand, I wonder why the N2O data are not plotted in Fig. 4 versus microbial biomass carbon, temperature, and mineral N if this was measured ?

p19230, l11-12: "...stepwise regression analyses show that microbial biomass nitrogen was a major controlling factor in N2O fluxes...". Related to the above comment, where in Fig.4 a quadratic regression was performed for MBN: was a quadratic regression also used in the stepwise regression mentioned here? I expect not. I am not overall convinced of the usefulness of such stepwise regressions in the case of N2O emissions, because the shapes of the relationships of the different variables to N2O can be very different (for example, linear or exponential for temperature, gaussian for soil mois-

ture, etc), whereas a stepwise regression might apply linear regressions automatically and in blind fashion.

p19229, l19-21: please change the sentence to : "Our observations, showing a decrease in N2O EMISSION with mowing height, generally supported the hypothesis that A LONG TERM MOWING MANAGEMENT would decrease the grassland N2O emissions into the atmosphere." I believe it is useful to change the wording from "mowing" to "long term mowing management", in order to distinguish the event (the act of mowing or cutting the grass on a given day, which might well trigger an N2O emission pulse - see Fig.3 in Flechard et al., Global Change Biology (2005) 11, 2114–2127, doi: 10.1111/j.1365-2486.2005.01056.x), as opposed to the grassland system resulting from the management practice.

p19229, l24: it would appear that "Grazing-induced reduction of natural nitrous oxide release from continental steppe" by Wolf et al. (2010) was delivering a different message from yours, if you argue that a mowing management leads to a reduction of N2O emission compared with the traditional grazing system of Inner Mongolia? Please explain.

p19230, 23-28: the reasoning is sound (senescence, high temperature, mineralisation, rainfall are conducive to N2O emissions), except that according to Fig.3, the N2O fluxes in August are not clearly larger than in other months, but indeed much lower than June in 2008 and much lower than May-June in 2009.

p19232, l20-21: the moisture effect is a well documented one.

p19232, l14-21: for this concluding paragraph, it could be useful to indicate that the paper does not present a complete N2O budget (fluxes for the growing season only, no emissions from herbivores and from freeze-thaw cycles) and that the results need to be complemented by other studies to assess the overall impact of the change of management practice (grazing to mowing) on the total N2O budget. However, given the revised scope of this paper, it can be interesting to reflect on which mowing height

(and, why not, the timing? the frequency?) might have the best potential to reduce field emissions within a mowing-based animal production system.

Technical corrections

p19221, l22: change to "Denitrification is one of the key ecological processes that determine N2O production...".

p19221, l27: (Cavigelli and Robertson, 2001)

p19222, l8: (Yamulki et al., 1998)

p19226, l23: "... biomass and litter measureD in 2009..."

p19227, l9: "... were significantly different ..."

p19227, l21-22: "...variation in the monthly cumulative N20 flux, with a range of 0.48 to -0.35 kg N2O ha-1 ..." : the data shown in Fig.3 show a much larger range in monthly cumulative fluxes, from -4 to +5 kg N2O ha-1, which surely indicates a unit error in Fig.3 . Please correct Fig.3.

p19230, l17: change "bound to" to "associated with"

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