Biogeosciences Discuss., 11, C5395–C5400, 2014
www.biogeosciences-discuss.net/11/C5395/2014/

Biogeosciences
Discussions
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Interactive comment on "Carbon and nitrogen dynamics in native Leymus chinensis grasslands along a 1000 km longitudinal transect in northeast China" *by* L. Ma et al.

Anonymous Referee #1

Received and published: 24 September 2014

General Comments The paper by Ma et al. advances a relevant topic by presenting observations of several carbon and nitrogen cycling diagnostics across a rainfall gradient. The primary interest of this data set is the synthesis of plant, soil, litter, and microbial community information, which are not typically included together in such studies. While the data set is new, complete, and potentially useful, there are significant issues with data analysis, the conclusions drawn, and integration with the vast literature on this topic. Most importantly, I am concerned about the use of field observations on a single day (or week) with climatology averaged over several years to address climate control on C-N cycling. Either the climate and ecosystem parameters should be collected at the same temporal scale (e.g., several years of field data) or an argument

must be made that the samples collected are representative of some "average" condition. I am sure this is the case for the slow soil variables, but I anticipate substantial intra- and inter-annual variability in the plant pools, soil moisture, and soil microbiota. Further, there are some grammatical errors that obscure important content. Overall, I would rate the quality of this paper as poor/fair and my suggestions for improvement are detailed in the "specific comments" and "technical corrections" section below.

Specific Comments

1) There is no mention of precipitation in the title. This is an important piece of information that conveys the significance of the "1000 km longitudinal transect."

2) The abstract does not satisfactorily describe the motivation, methods, and conclusions. For example, there is no mention of the PFLA or AMF results or the redundancy analysis. Although you state "...likely due to the relative changes in temperature, soil arbuscular mycorrhizal fungi biomass and N availability," this sounds overly speculative, when you have data to support a more precise statement. Secondly, the second to last sentence beginning "It was concluded..." is not a novel conclusion, nor is it a focused description of what you observed. I would again be more precise here. Finally, I failed to understand the connection between the last sentence and your work, specifically your prediction of C and N sinks under changing climate (which I address in other comments below).

3) Introduction, first paragraph: In the motivation, there seems to be some confusion between rainfall variability or "precipitation regime" (i.e., drought, extreme precipitation events) and mean annual rainfall. Your measurements and results are presented across a mean annual rainfall gradient, while the IPCC projections of increased rainfall variability are conflated with changes in mean annual rainfall. Either discuss how increased rainfall variability will change mean annual rainfall or soil water content or remove the discussion of rainfall variability – the paper can be completely motivated from the perspective of mean annual rainfall and would reduce confusion. On Page

12161, Line 12, replace “precipitation regime” with “mean annual precipitation.”

4) Page 12161, Line 18-20: The linear relation between ANPP and water availability is not always observed – see Huxman et al. (2004) and Hsu et al. (2012) for other examples.

5) Page 12162, Line 25: replace “precipitation regime” with “mean annual precipitation” (also Line 28)

6) Page 12162, Lines 8-11: References not needed for this statement.

7) Page 12162, Lines 13-16: soil water availability often depends strongly on precipitation, especially in water-limited ecosystems. My opinion is that some of your references may offer insight into the mineralization-precipitation relationship, even if precipitation was not reported directly. Can you discuss this further? You might also consider linking precipitation, soil moisture, and mineralization in your results and discussion. What does it mean that soil moisture measured on a single day in July scales linearly with mean annual precipitation?

8) Page 12162, Lines 27-28; Page 12163, Lines 2-3; Results Section 3.3: How do you define C and N sequestration potential? In Figure 4, sequestration is the sum of shoot, root, litter, and soil (page 12167, line 19-20). Because soil C is a slow variable and intra- and inter-annual variability in litter and plant C may be important in grasslands that turnover rapidly, soil C is the best indicator of a “sequestration potential.” Regardless, “sequestration potential” is a nebulous concept and therefore I would suggest only discussing carbon storage in the context of measured soil C.

9) Page 12162, Lines 28-29: The hypothesis is not convincing or testable as stated. The first part, that precipitation is the primary driver of C and N dynamics in temperate grasslands is not completely supported by your literature review. Precipitation effects on C and N dynamics have been studied extensively (as you reference). How is your study different? How are temperate grasslands different from other ecosystems? What

do you mean by “primary?” In my opinion, the second part of the hypothesis regarding soil properties and microbial communities is an important contribution of this study. I would suggest emphasizing precipitation as a driver of C-N dynamics and that your study aims at teasing apart the state variables that contribute to those dynamics across a precipitation gradient.

10) Materials and Methods:

a. Climate: with respect to your interest in precipitation regimes, seasonality and the daily-scale structure of rainfall will likely affect the soil water balance and therefore soil water availability. If the climate data is at a monthly or higher frequency, I would suggest investigating how precipitation variability changes across the precipitation gradient. Again, according to the data in Huxman et al. (2004) and Hsu et al. (2012), or the theoretical studies of Porporato et al. (2003) and Daly et al. (2004), mean annual precipitation may not be the ideal choice of independent variable.

b. Plant biomass and litter mass: When were the measurements obtained and why? Again, throughout the paper the connection between measurements on a single day and average climatology needs to be addressed. What is the seasonal and inter-annual variability of plant biomass and litter mass? Are the grass communities annual or perennial? What is the litter decomposition rate? Were your measurements obtained in the wet or dry season? Would this variability obscure your results?

c. Page 12165, Lines 4-6 and Page 12167, Lines 5-7: Why did you not report the ratio of fungal to bacterial PLFA?

11) Results, Section 3.1: If the canopy is closed and the roots and soil moisture are shallow, I do not find it surprising that root biomass did not change across the climate gradient. Indeed, on page 12169, you note that this is “consistent with those of studies on effects of increased precipitation on root biomass at local and regional scales.” Also, in Figure 2c, the root biomass looks more non-linear than any of the models claimed to be quadratic.

12) Results, Section 3.3: How was C and N mineralization measured? This is not covered in the methods. Also, from Figure 4, the quadratic relationships are not well supported (also page 12169, lines 27-19). How do linear models perform with this dataset? Does the quadratic model provide any additional explanatory power? You may also try a segmented regression.

13) Page 12170, Lines 27-29: This sentence is difficult to understand.

14) Page 12171, Lines 1-3: I don't see a saturation in heavy fractions of C or N in Figures 3H and 3J.

15) Page 12171, Lines 15-22: These conclusions are not very well supported by the data or your analysis. How would changes in global precipitation regimes strongly affect ecosystem C and N dynamics? Again, what is your definition of "precipitation regime?" Why do the "grasslands of northeast China exhibit tremendous potential for enhancing C and N sequestration at the regional scale?" You haven't discussed how precipitation is expected to change in this region or how sequestration could be enhanced. Further, the conclusions that precipitation, temperature, etc. play a role in ecosystem C and N dynamics is not new and needs to be focused to represent the specific conclusions drawn from your dataset.

Technical Corrections

1) Abstract, first line: The words "unprecedented" and "profoundly" are not quantitative and rather strong – consider replacing or omitting.

2) Abstract, line 4: what do you mean by "complexity in precipitation?"

3) Abstract, line 19: remove the word "on" after "influence"

4) Line 25: "covering" should be replaced with "cover"

5) Page 12162, Lines 5-6, Lines 7-8, Lines 25-27: these sentences need to be rewritten.

6) Pager 12166, Line 15: "were all linearly increased" should be "all linearly increased"

7) Page 12166, Line 26: "long" should be "along" (and page 12170, line 29)

8) Page 12167, Line 6: what do you mean by "style?"

9) Page 12168, Line 24: "drives to impact" should be reworded

10) Page 12171, Lines 15-16: This sentence needs to be reworded

References

Huxman et al., 2004, Nature: doi:10.1038/nature02561

Hsu et al., 2012, Global Change Biology: doi:10.1111/j.1365-2486.2012.02687.x

Porporato et al., 2003, Advances in Water Resources: doi:10.1016/S0309-1708(02)00094-5

Daly et al., 2004, Journal of Hydrometeorology: doi:10.1175/1525-7541(2004)005<0559:CDOPTA>2.0.CO;2

Interactive comment on Biogeosciences Discuss., 11, 12159, 2014.