Editor Initial Decision: Reconsider after major revisions (08 Nov 2014) by Dr. Sönke Zaehle
Comments to the Author:
Dear authors,

many thanks for providing an updated manuscript, in which you have addressed most of the concerns the reviewers raised. However, based on my own reading, I have a range of comments on the revised manuscript, which I kindly ask you to address/correct before this manuscript may be publishable in Biogeosciences.

Please add from your responses to reviewer #1 the response to the major Methods comments to the manuscript.

We added this information. For instance, at page 8 line 156-157: "We used fragmented and sieved litter mainly to standardize the litter area available to microbes." or at page 10 line 203-205 "Briefly, the fresh soil samples were adjusted to approximate 60 % of water holding capacity and then incubated for one week in the dark at 25 °C to reactivate soil microbes following the original procedure published by Vance et al. (1987)."

I wonder why you did not test whether the regression slopes statistically different across the years - or whether despite meteorological differences, the relative placing of the treatments was conservative across years? This would seem to make your study more robust.

Actually, this was tested for but not explicitly stated in the manuscript. The interaction between the so-called "treatments" and "year" effect gives the same information that the suggested test on the regression slopes. Such interaction was never significant, which explains why we did not mention this before. In the revised manuscript, we now explicitly write that the treatment were conservative over the years (page 12 line 239, page 13 line 255, line 267, page 14 line 282).

In the beginning you state your hypothesis that you would find priming. But this is not discussed at all later in the manuscript. Instead, you only discuss the effects on plant production - why? presumably because you did not find any evidence for priming? Isn't that worth noting? And if it's not - why do you mention priming then in the introduction? Please rework the manuscript to be consistent between the hypotheses you state and your answers/results in Discussion and Conclusion

We thank the editor for this important comment, which helped us to better structure our introduction. In fact, priming was not hypothesized but observed during this experiment (details are given in Xiao et al. 2014; Oikos). What we hypothesized was that nutrient availability would increase through the observed priming effect. After having re-read our paragraph we agree that this was not clear at all and we therefore completely rewrote this paragraph, stating more clearly the true objective of this study.

The use of the word resilience is somewhat inadequate. Your manipulation had low effects, which were statistically insignificant (probably partly because of the high variance in the measurements?). So is the lack of finding a response not more a sign for a low signal to noise ratio rather than an insensitivity of the ecosystem? After all, there is a systematic trend with litter addition for most variables - which does not suggest insensitivity to litter addition (or resilience). You need to define what you exactly mean by resilience. That of the N:P ratio, biomass production, or what else?

We understand this confusion are therefore included the definition of the term resilience that we adopted at page 6 line 110-112: "Here, we define the term resilience as the absence of a significant effect of litter additions on the measured variables (plant biomass, C:N:P ratio, etc.)." Holling (1973, Annual Review of Ecology and Systematics) originally defined resilience as the "measure of the persistence of systems and of their ability to absorb change and disturbance and still maintain the same relationships between populations or state variables".

Of course, to "maintain the same state variables", we must define the range where the variable is still the same. In our case, we used the statistical significance between treatment to take into account the variability due to (i) micro-environmental differences (e.g. heterogeneity of the soil inducing different N and P content between replicates) and (ii) phenotypic plasticity of plants. We consider that when a treatment was not statistically different from the control, it indicates that the state variable considered was still in the same range. i.e. not different.

Because statistical differences were almost only found in the highest litter addition treatment, which are far from plausible, we concluded that plant growth in the Chinese grassland systems is resilient to litter addition.

Minor comments:

Abstract:

"Global climate change has generally increased net primary production (NPP) which leads to increasing litter inputs."

I don't think that there is evidence for this (globally and in the past), and you do not provide any suitable reference. Future litter inputs may increase in dry climates because of CO2 fertilisation - but this is a hypothesis, not a fact. This sentence does also not link to projections of the ESMs you refer to later in the abstract. Please revise (or remove) this first sentence.

We modified this sentence as following at page 2 line 23-24: "Global climate change has generally modified net primary production (NPP) which leads to increasing litter inputs in some ecosystems."
L 35 ff: It seems more logical to start with the "more realistic" result, than the outlier

In the revised version, we presented the "more realistic" result before the results of the high litter additions

L37: remove "quite". Either it is resilient or it is not.

We removed it.

L38: Why does this highlight complexity? If you add litter within a certain range of ambient litter fall, nothing statistically significant happens (though the trends go into similar directions than with higher added litter), and you only see a statistically significant results once the perturbation becomes large. This is more an expression of the inevitable variability of ecosystem measurements (inducing noise) than the complexity of the ecosystem. Please revise or remove.

The sentence has been removed

L61: I cannot see how Hungate 2003 makes any statement about the importance of litter dynamics in terms of feedbacks between plant growth and climate - the word litter does not appear in this paper.

We removed the reference

L63: Please update Houghton et al. 2001 to something more up-to-date. It is inconsistent to quote IPCC 2013 for the impacts, but rely on 12 year older scenarios, which have not been used in IPCC 2013

We modified the sentence at page 3 line 61-64: "The increase of atmospheric carbon dioxide (CO2) concentration within the next 100 years (Meinshausen et al., 2011) due to continued anthropogenic carbon emissions is generally predicted to increase net primary production (NPP) (Todd-Brown et al., 2014)."

L66: here and in the following: CO2 has a subscript "2".

Done

L67: remove "therefore" - this does not lead logically from the previous sentence.

We removed this word

L81: Why is here a quotation mark (and where is its end?)

Sorry it was a typo mistake we removed it

L105: Here or somewhere else in the introduction you need to make the link between increased litter input from elevated CO_2 (which generally does not increase the input of N and P), which you expect for your ecosystem, and the fact that when adding litter you add also new N and P to the ecosystem. Particularly, because in L 110 you expect to observe a priming effect - but how do you separate this from the (trivial) increase in the pool sizes because you have added N and P to the system - essentially providing fertiliser (even if in an undecomposed form).

We add this sentence to inform the reader that the effect of elevated CO2 on litter stoichiometry was not taken into account in our experiment at page 5-6 lines104-106 : "The litter added was obtained under current atmospheric CO2 concentration and therefore, the impact of elevated CO2 on the litter stoichiometry (Cotrufo et al., 1999) was not represented in our case."

Concerning the priming, the objective was not to identify the extra mineralization of N or P due to priming but we only present this process as possible explanation for an increase of N or P pools available to plants. Regarding the results we obtained, priming played a minor role in the N or P release.

L110 better use hypothesise?

We modified the sentence.

L159: I would not consider the predictions of Earth system models "very realistic" to make statements about the changes in a particular ecosystem. Use "plausible" instead.

In the entire manuscript, we changed "realistic" to "plausible".

L162: Please be consistent between representing ecosystem stoichiometry either as C:N:P (L 138) or C:N, C:P, N:P

We modified this part and we now present C:N:P ratio for soil and litter.

L178: Clarify: was this the end of the growing season? - use of the May-September precipitation earlier suggests otherwise. If it was not the end of the growing season - would this affect your conclusions about plant growth?

No it was not the end the growing season, which occurs between May and October. But the litter additions were made in October, at the end of the growing season (see page 8 line 151). We measured in August because it is a period of high growth and we expected that the effect would be easier to detect.

We modified the sentence at page 9 lines180-182: "Field sampling of above-ground biomass, root biomass, litter and soils was conducted from August 1st to 3rd in 2009, 2010 and 2011. We choose this period because August is the peak of the growing season."

Section 3.3. They may not be significant, but it's probably worth mentioning that there was still an notable trend with litter addition in most variables.

We added this sentence at page 13 lines269-270: "Even though C, N and P contents were significantly different for the highest treatment, for almost all the pools, they tended to increase when litter was added."

L246: decreased.

OK, corrected.

L 281ff: I do not understand why you single out temperature and precipitation here, because surely these ESM simulations do account for changes in CO2, temperature and precipitation, so the predicted 10-60% increase already includes climate change. If this was not the case, I would expect a more elaborate discussion as to where your estimate of 10-60% comes from.

We single out precipitation and temperature here to explain that climate change may not only modify NPP but also decomposition. We clarified the sentence as follows at page 14 lines 290-291: "The NPP in our study sites is expected to increase between 10 and 60% due to climate change and atmospheric CO2 increase (Arora and Boer, 2014; Todd-Brown et al., 2014)."

L 287: Not really. There was also an increase in the other treatments - it was only not significant.

We modified the sentence at page 15 lines 296-297: "Results show that availability of N and P were significantly modified only for the two highest inputs treatment."

L292: here and in the following remove "quite"

OK, corrected.

L292: I do not understand the use of "only" here. Please reword.

We removed the word only that was indeed not well used.

L293: remove "indeed".

The word was removed

L 294: remove "but".

The word was removed

L296-L300: repeats more or less word-by-word the preceding sentences. Remove.

This paragraph was removed.

L300f: This would not be surprising at all

We agree that immobilization of nutrients in the microbial biomass decomposing the added litter is very likely a dominant process explaining our observations. What's a bit more surprising is that the observed priming of soil organic matter did not result in higher plant nutrient uptake and growth, except in the highest litter treatment.

L307: Given that plants do not decompose organic matter - is this really a surprise?

It is a surprise, because litter and organic matter mineralization is an extra-cellular process, so plants can compete for mineral nutrients in soil solution with the microorganisms (Kuzyakov and Xu, New phytologist, 2013)

L 311: is there any evidence for this? I do not follow why low increases in microbial biomass are necessarily a sign for changed carbon-use efficiency. The time-scale of your experiments does not allow for such a statement, because for instance, a large fraction of the microbial biomass that decomposed the litter was already dead when you took your measurements, a long time after litter addition. Since you do not provide any data on soil respiration and remaining litter, how can you be certain that the microbial carbon-use efficiency changed. What about changes in composition?

Here we were talking about the carbon use efficiency observed at the community level and not at the individual level. A change in the carbon use efficiency at the community level may be explained by physiological modifications at the individual level but also by a change of the microbial community composition. We added some details at page 16 lines 319-324: "Such modification of the carbon use efficiency might be due to physiological modifications at the individual level, to a modification of the microbial community structure or to both mechanisms combined. With our data, we can't estimate the carbon use efficiency at the individual level but based on the same experiment, Xiao et al., (2014) showed that the microbial community structure was different between treatments."

Concerning the microbial death, we can't totally exclude this explanation. Nevertheless, after starvation, microorganisms generally reduce physiological activity and are considered as non active or dormant but not dead (Blagodatskaya and Kuzyakov, 2013, Soil Biology and biochemistry) and with the technique used to measure microbial biomass we are not able to know the fraction which is active and the fraction which is dormant or inactive. Therefore, the microorganisms which used the litter as substrate few weeks or month before measurements may be still alive and are detected by our methods.

L320f: On that note, you should discuss that rain fall in 2009 and 2011 was only 2/3 of the average - a low change in biomass may be associated with this.

We fully agree, we rephrased to clarify at page 16-17, lines 332-337: "Furthermore, when litter additions significantly affected plant biomass, aboveground biomass and belowground biomass were higher in 2010 than in 2009 and 2011. The years 2009 and 2011 were dry compared to the year 2010. Thus, a water stress may have limited the plant growth in 2009 and 2011. Moreover, soil moisture was likely higher in 2010. Such more favourable soil moisture conditions may have caused the higher soil nutrient availability via accelerated litter decomposition."

L323: Seems to belong to the following section. Remove "indeed".

Since this sentence was redundant with some part of the section 4.2, we removed it.

L325: This sentence is repeated word-by-word in the following section. Remove here.

We removed this part.

L336: This sentence repeats the content of what was said in L330. Merge or remove.

Both parts were merged.

L339: competition for what? why is this relevant?

We talked about competition for light. We simplified the sentence at page 17 lines 347-348: "The plastic response of increased allocation to shoots corresponds to theoretical predictions (Tilman, 1988)."

L340 A higher photosynthate concentration leads to a higher C:N and C:P not to a decline. A decrease in C:N cannot explain increased photosynthate concentration - rather the opposite is true. What are you trying to say here?

Thank you and sorry, of course we were talking about C:N and C:P ratio increases. We corrected this.

L360: Your study does not support this statement. The litter you added is qualitatively not the same as the litter to be expected under elevated CO2. Furthermore, the addition of litter has different consequences than the increase in litter fall under elevated CO2, which would mostly result in increased C:N:P (as long as other N and P inputs do not change) and therefore further reduce nutrient availability.

We removed this sentence.

L365: This is not a conclusion from your study and should thus not appear here, but in the introduction (if it was necessary at all, which I don't think it does as your manuscript does not talk about N and P fertilisers as such).

This part was removed.

L368: Why surprisingly?

We expected a more important response to the litter additions when the experiment was planned. Anyway, we removed the word "surprisingly".

L377: Note that these models do largely not account for either N nor P. I'd therefore refrain from calling them "realistic".

The word realistic was changed to plausible.

L379: I'm confused here: why do you state increased in "NPP with nutrient imbalance", when the increased input had the C:N:P of litter from this ecosystem. This does not seem to be an imbalance by itself.

We used imbalance because the C:N:P ratio of the litter was higher than the C:N:P ratio of plant and soil. But we agree that it is a bit confusing with the imbalance expected due to the CO2 increase not related to inputs of N and P. We removed this part of the sentence.

L380: Again, I don't follow why you make a distinction between the effects of temperature, precipitation and CO2 here. See my comment above. Note that your experiment does not mimic the effects of elevated CO2, because it assumed that the litter stoichiometry was unaltered.

We changed plant to ecosystems in the sentence because modification of temperature and precipitation will modify the nutrients release due to soil organic matter decomposition.

L603 (and the following figure captions): " in a steppe community of northern China"? So these results are not from your experiment, but some other place? Please simply remove (unless you really do not show the experimental data)

This part was removed

L 604: Please be more precise what you mean by year-effect? I assume that this means that for all litter treatments, there were consistent differences between the years?

We rephrased as follows at page 31 lines 613-614: "The soil inorganic N and the available P contents were different each year ($p<0.01$) but no interaction occurred between litter addition and year."

L614 and in the following: refer to figure 2 for this information - do not repeat the same information four times.

The figure legends are corrected in the new version.

Figure 1: It is difficult to spot the differences amongst the years. Consider representing smoothed temperatures and precipitation in a super-imposed way.

Figure 2-5: please add letters to identify panels. Note that the text suggests that these labels exist.

Figure 4 and 5 are illegible. Both axes and statistical relationships are too small. Note that there are also considerable errors in the labelling of X and Y axes, as well as in the placement of the significance letters. I also do not follow why in some panels of Figure, the letters have been omitted (as well as in Figure 5 also some regressions). I'd advise to remove the regression statistics from the Figure and provide these in a different form.

The figures were modified following the editor's comments; the regression statistics are now presented as a supplementary table.