Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-445-AC1, 2019

Biogeosciences Discussions
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EGU

Interactive comment on "Different sensitivities of litter decomposition and nutrient release to ultraviolet radiation" *by* Weiming Yan et al.

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Received and published: 8 January 2019

Responses and corresponding revisions:

Interactive comment on "Different sensitivities of litter decomposition and nutrient release to ultraviolet radiation" by Weiming Yan et al. Response: We are very grateful for your positive evaluation of our work and the constructive suggestions and comments, which have improved the manuscript. We have followed all of your advice and have revised the text according to your suggestions. Our responses to the concerns raised appear below, and the revised manuscript could be found in the Supplement.

General comments: 1: A large part of the results were presented again in the Discussion. A rewriting of the results should allow to extract more explicitly the meaning

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of the findings avoiding the need to be repeated in the discussion. The authors have made many classifications, i.e., lab/field, litter types (although not very correct and could be re-divided), durations and MAP levels, but some interesting findings were not presented. For example, why UV enhancement and attenuation had opposite effects on mass loss vs. N/P release (Fig. 1)? Why UV changes had strong effects on litter N content and C/N and lignin/N ratios (Fig. S4)? Response: Thank you for your comments. Follow your suggestion, we have rewritten the results section to more explicitly describe the findings and have revised other sections accordingly. Fig. 1 shows that UV change had effects on mass loss opposite those of N/P remaining, which indicates that mass loss exhibited a similar change as did N/P release. The concentration of N/P may increase over the course litter decomposition and result in a negative value of N/P release, which cannot be used in the calculation of the formula; thus, we used N/P remaining in the calculation. Potential reasons for the large differences among the effects of UV on leaf litter chemistry in the study include the following: 1) interspecific differences, as the magnitude of UV effects on leaf litter quality varies with plant species and type, for example, between herbaceous plants (Pancotto et al., 2003; Pancotto et al., 2005) and woody plants (Song et al., 2013b); 2) variation in environmental factors, especially precipitation, temperature, and soil nutrient content, which influence litter chemistry (the studies were performed at different sites, including dune grassland (Hoorens et al., 2004) and field sites (Pancotto et al., 2003; Pancotto et al., 2005)); and 3) variation in treatment duration, because the accumulation of variation in chemistry parameters changes with exposure time. The length of time that plants were grown under UV radiation ranged from one growth season to 3 years in the included studies. These reasons might explain the strong effects on litter chemistry.

2: The authors made a very simple regression result in Fig. S5, which, however, I think cannot help us to answer the above two questions. Instead, for the Fig. S5, why not to try to separate the control and treatment data because UV treatment did had very strong influence on litter N content and associated ratios, and this may help us to make insightful discussion. Response: Thank you for your comments. Follow your sugges-

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tion, we separated the control and treatment data in the revised manuscript (Fig. S8). The results indicated that k decay had a significant correlation with N concentration under both the control and UV-change treatments. Furthermore, the slope of the relationship between k decay and N concentration was larger under UV enhancement than under ambient environment conditions and UV attenuation. We have added this information to the revised manuscript.

3: I remember that the photo-degradation of litter decomposition was found in arid grassland (i.e., Austin 2006 Nature). Yes, the authors compared the decomposition rate and MAP, and from that figure, the regression results were interesting, particularly in arid areas with low precipitation. However, it was much different when the MAP reached at 1400 mm, so I think the regression results can be presented as bars with different categories (i.e., MAP ranging from 0-50 mm, 50-100 mm, etc.). Of course, the previous Figure 5 can be presented in Supplementary Information. Response: Thank you for your suggestion. According to your suggestion, we have presented the results as bars rather than in a regression plot and have added the comparison of k decay between the control and UV-attenuation treatments. The results showed the UV attenuation had a significant effect at precipitation levels ranging from 100 to 200 mm and from 1400 to 1500 mm. Moreover, we moved the original Fig. 5 to Supplementary Information (Fig. S6).

4: Following the above question, photo-degradation of litter decomposition may be observed in grasslands in arid ecosystems in previous years, but in recent years, there were many studies conducted in forests. Therefore, why not divide the litter type to more specific classifications (i.e., grass, herb, broad-leaved and needle foliage) corresponding to the ecosystem types (grasslands vs. forests)? By the way, I think just use "herb" in grasslands was not correct and the authors should carefully distinguish grass and herb. Response: Thank you for your comment. According to your suggestion, we have divided the litter types according to ecosystem and have added the results for specific groups (i.e., grasses, herbs, and broad-leaved and needle-leaved plants) in

Supplementary Information (Fig. S5). In addition, associated content has been added to the revised manuscript.

Detailed comments 5. Line 30. “weight loss”? And why not “mass loss”? The latter one is more widely used in litter decomposition studies. Response: Thank you for your suggestion. We have replaced ‘weight loss’ with ‘mass loss’ in the manuscript and figures.

6. Line 222. The authors declare that “UV-(A+B) attenuation: : : but showed LITTLE effect: : :”; however, “UV-B enhancement: : : showed SIGNIFICANT effect” (line 221). I think the authors should change the presentation. In fact, the RR for UV-(A+B) attenuation was less than - 0.25, but that for UV-B enhancement was only 0.04, so why did the authors say UV-B enhancement had SIGNIFICANT effect whereas UV-(A+B) attenuation has LITTLE effect? In fact, the RR for UV-B enhancement maybe not significant (overlap with zero) if the sample size was smaller. Response: Thank you for your comment. We have merged the UV-B and UV-(A+B) results into the category UV attenuation in Fig. 1 as proposed by Reviewer #2, and we have moved the original UV-B and UV-(A+B) results to the supporting information (Fig. S2). The text has been revised as follows: ‘As expected, UV enhancement and attenuation showed opposite effects on mass loss and nutrient release. UV enhancement and attenuation showed significant effects on k decay, with RRs of 0.09 and -0.41 (Fig. 1), respectively; furthermore, UV-B enhancement and attenuation showed significant effects on mass loss, with RRs of 0.04 and -0.35, respectively. UV enhancement promoted N and phosphorus (P) release, with RRs of -0.16 and -0.08 of N and P remaining, respectively. UV attenuation showed the opposite effects on N and P remaining, with RRs of 0.08 and 0.10. The effects of changes in UV radiation on C and lignin release were not significant. Both UV-(A+B) and UV-B attenuation showed similar effects on mass loss and N and P release (Fig. S2)’.

7. Lines 223-225. The authors declared that the RR was greater for k decay compared with mass loss, but I think the authors should treat the k decay and mass loss results

with caution. There are at least two reasons: First, both the k decay (assumed that the exponential models were used) and mass loss reflect one thing. Second, the sample size for UV-B attenuation on k decay was very small (n=4), so its confidence was not strong, and this result may be excluded in some cases. Therefore, I think using other sentences (i.e., Similarly, k decay: : :) would be better than ": : : greater than : : :". Response: Thank you for your comment. We agree with your statement that both k decay and mass loss reflect one thing. Following your suggestion, we have revised the sentence to ' UV enhancement and attenuation showed significant effects on k decay, with RRs of 0.09 and -0.41 (Fig. 1), respectively; furthermore, UV-B enhancement and attenuation showed significant effects on mass loss, with RRs of 0.04 and -0.35, respectively.'

8 Line 225. Why the authors did not present some results on N and P release directly? That will be very interesting because it seems that UV-B enhancement and attenuation showed opposite effects on N/P release relative to on mass loss/k rate. And then C and lignin. Response: Thank you for your comment. Fig. 1 shows that UV change had effects on mass loss opposite those of N/P remaining, which indicates that mass loss exhibited a similar change as did N/P release. We originally planned to show the N/P release results. However, the concentration of N/P may increase over the course litter decomposition and result in a negative value of N/P release, which cannot be used in the calculation of the formula; thus, we used N/P remaining in the calculation.

9. Line 225. I think "no effect" was not a very good word if we have other choice. Why not "the effects were not significant"? Response: Thank you for your suggestion. We have revised this sentence to 'The effects of changes in UV radiation on C and lignin release were not significant' in the revised manuscript.

10. Line 257. "UV enhancement had NO effect on the weight loss in the first four months"? I am confused why some RRs were not significant (marked in gray) but the 95% CI did not overlap with zero. This problem can be found in many figures. Please check it. Response: Thank you for your comment. We apologize for the misunder-

standing regarding the values in the figures. It is true that an RR was considered significant when its 95% confidence interval (CI) did not overlap with zero. The value in the figure indicates the weighted response ratio, and the error bar represents the standard error, not the 95% CI.

11. Figure 267-268. What is “control treatment”? “Control + treatment”? From Figure 1, UV treatment had very strong influence on k rate, so have you tried to compare the relationships between MAP and k rate under control and treatment conditions separately? Just like Figure 6. Response: Thank you for your comment. In the control treatment, litter decomposition was occurring under ambient environment conditions, without the enhancement or attenuation of UV. Our aims were to explore the relationship between k decay and MAP under ambient conditions and determine whether the effect of UV attenuation on k decay is affected by MAP. In addition, the first figure displays the relationship between k decay and MAP, and the later one shows the relationship between the RR of decay and MAP; we cannot compare these two regression equations due to their different parameters.

12. Lines 275-278. I am confused that how can the authors conclude that : : : was more sensitive than : : :. The slopes >1? Or compared with the 1:1 lines in Figure 6? Response: Thank you for your comment. We apologize for the lack of clarity. We have revised the text to ‘Various effects of changes in UV radiation on the RRs of remaining nutrients and weight remaining were found (Fig. 6). The slope of the RRs of remaining C and N and the weight remaining under UV attenuation were 1.31 and 1.23, respectively, however, the effects of both UV enhancement and UV attenuation on the relationships between each of C, N and P and mass loss relative to the ambient environment were not significant ($p>0.05$). Interestingly, UV enhancement significantly promoted lignin release compared with mass loss ($p<0.01$).’

13. Lines 284-289. It seems that this sentence was redundant here because this has been presented in the Introduction. We should focus on the most important findings, and some general sentences, i.e., litter decomposition is a complex process regulated

by both biotic and abiotic factors, were not very interesting for readers. Response: Thank you for your comment. We agree with your statement that the sentence was redundant and have deleted it from the revised manuscript. In response to this comment and a suggestion proposed by Reviewer 2, we have revised the beginning of the discussion section to 'In the present study, a meta-analysis was performed to assess the effects of UV exposure on the dynamics of litter decomposition and nutrient release. We found that leaf source (grassland or forest), experimental condition (field or laboratory), experimental duration, and direct or indirect effects of UV exposure affected litter decomposition and nutrient release under UV exposure'.

14. Lines 292-295. Just like the suggestion mentioned above, the sentence "UV enhancement had : : : lead to a decrease" has been introduced in the Introduction section, so we do not need to repeat it again here. Response: Thank you for your comment. We have deleted this sentence from the revised manuscript.

15. Line 302. I think "litter decomposition" should be replaced by "mass loss" here. As you declared (but I suggest to delete it) that "litter decomposition is a complex process" and this process includes many sub-process, i.e., mass loss we observed, C structure breakdown, release of N, P and other nutrients, etc. Therefore, UV treatments had opposite effects on mass loss vs. N/P release and not litter decomposition vs. N/P release. Response: Thank you for your comment. Following your suggestion, we have replaced 'litter decomposition' with 'mass loss.'

16. Line 310. I think the authors should discuss the sample size for remaining C, and not only "a different regulatory mechanism". In meta-analysis, the collected data have strong influence on our conclusion, which may be biased if the sample size is too small. This is a potential uncertainty in meta-analysis. Response: Thank you for your comment. We agree with your observation that the sample size may have affected the results of the meta-analysis. Thus, we have revised the text to 'However, interestingly, changes in UV radiation did not affect the release of C, which was a focus of our concern, may be indicated that a different regulatory mechanism other than

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UV radiation may be controlling litter decomposition, although the small sample size may have contributed to the insignificant results. Therefore, more studies needed to determine the effects of UV changes on the release of C'.

17. Figures. The authors should note that what are the meanings of the error bars, e.g., 95% CI. Figure 1. Maybe there was a small mistake for the UV-B attenuation (triangle) for "k decay" in Figure 1. Generally, the difference can be considered as significant if the 95% CI did not overlap with zero. However, for the UV-B attenuation for "k decay", the 95% CI did not overlap with zero, but it was gray and not black, although the sample size was very small (n=4) Response: Thank you for your comment. We apologize for the misunderstanding regarding the values in the figures. The values in the figure are the weighted response ratios, and the error bars represent standard error. The black symbols indicate significant differences ($p < 0.05$) between the response ratios and zero. We have revised the figure legends for clarity.

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2018-445/bg-2018-445-AC1-supplement.zip

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Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2018-445, 2018.

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