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Dear Dr. Duursma,

we have now completed the minor revisions of our manuscript bg-2010-194 with the title "Experimental nitrogen, phposphorus, and potassium deposition decreases summer soil temperatures, water contents, and soil CO_2 concentrations in a northern bog" by Wendel, S., Bubier, J., Moore, T.R., and Blodau, C..

We thank you and the reviewer for their insightful and constructive comments. In response to the criticism and suggestions we revised large parts of the text, the statistics, and the central Figure 3 and tested if we could explain effects on soil temperature by considering biomass data on the experimental treatments. We think that the manuscript is now suitable for publication.

Please find our specific responses below:

Although the data have been collected in an appropriate way, I have many difficulties with the data analyses. It is not clear how most of the statistical analyses have been done. For several soil depths, measurements were done at only 1 replicate plot per treatment. I can understand that it was impossible to have intensive monitoring at all plots, however, also for these soil depths, differences among plots are presented as significant treatment effects! Apparently, there were consistent, but very small (see for example fig. 3 Temp 10 cm or WC 5 cm), differences between one plot of treatment A and one plot of treatment B, which resulted in a significant difference due to the numerous recordings over time. However, such a difference between two plots does not say anything about a treatment effect, it can also be the result of a small difference in topographic position. Testing for treatment effects requires replicate plots.

Reviewer 1 rightly criticized the erroneous application of $_{RM}$ANOVA using non replicated plots as treatments. As the reviewer points out a difference found between two plots in this case does not say anything about a treatment effect, but that the difference may for example be caused by small differences in topographic position or other random variation. We thus removed any reference to such results from the text as well as Table 2, which summarizes the result of the ANOVA.

In the discussion the authors give plausible explanations for the observed differences in soil physical parameters. It is clear that the differences among experimental plots are not direct

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effects of the nutrient additions, but the result of changes in vegetation structure. Why not relate the soil temperature and moisture regimes to vegetation variables as vascular biomass and leaf area index, which seem to be available? There are 18 plots (6 treatments x 3 replicates) with varying Sphagnum cover and vascular plant biomass, which should be sufficient for a regression analysis. Such an analysis would also be more interesting for ecosystem modelers who want to include the indirect effects of climatic changes through changes in vegetation composition/structure on biogeochemical cycling.

Abstract: It is not clear what you mean with the word interaction, be more specific.

We changes this section and eliminated the reference to the “interactions” between factors PK and N (Page 2, Line 10-12).

2.2 Instrumentation: At what frequency were the recordings of soil temperature and moisture and the sampling of CO2 and CH4?

Recordings of loggers were done at 30 minute intervals and sampling of CO2 and CH4 in weekly intervals. We refer to the frequency in the revised manuscript at pages 5, line 32 and page 6, line 22.

2.4 Data analysis: Why is it necessary to correct for autocorrelation? I do not understand how you can test for treatment effects when using the average of the triplicates or using the one replicate available per treatment. It seems like you used the numerous time steps as replicates. This is not how to test for treatment effects. A proper statistical analysis separates for the effects of timestep and treatment, for example timestep as the within-subject factor and nutrient treatment as the between-subject factor. The plots are the subjects.

Autocorrelation may lead to erroneous results in $_{RM}$ANOVA and was thus removed from the data set. The reviewer is right that the application of $_{RM}$ANOVA to non-replicated treatments was erroneous- this was due to an oversight by the senior authors. We accordingly removed the results referring to non replicated depths in soil temperature. As temperatures were replicated on three plots per treatment at depths of 5 and 20 cm (temperature) and 10 cm (water content) statistical comparisons could still be drawn (Table 2, Figure 3, and text).

3 Results: This chapter is poorly written. The results from statistical analyses should be integrated in the sections with temperature and water content results. The results section should describe the main results, not methodological issues as regression coefficients among the triplicates and autocorrelation. Many sentences are not clear.

We agree that this chapter was poorly structured and written. We integrated the statistical results in the presentation of the results of temperature and water contents and removed superfluous statements. Methodological aspects, such as the regression coefficients between time series among the replicates, were condensed to a minimum and better integrated (page 7, line 21 to page 9, line 20).

3.1 Temperature Do you know why control plot b behaved unusual? Was this plot also in other aspects unusual? Or was the R2 of 0.65 (not bad) the only reason to omit control plot b from data analyses? In a field experiment it is normal that plots, also within a treatment, are somewhat different. That is natural variation and that is why often 5 or more replicate plots are needed to test for the effects of treatments.

It is not clear why this plot behaved unusually in terms of its temporal temperature dynamics. Visually it appeared similar and other variables obtained from this plot did not deviate in a similar way. We agree that considerable variation between replicates is the rule and thus replication is required to identify treatment effects. We expected mainly an offset of temperatures between replicates, but not a deviation in the temporal dynamics. The temperature time series among the replicates of all other treatments were much more closely related, so that we assume that the behaviour of plot control b was somehow anomalous with respect to soil temperature.

Table 2: Why is volumetric water content included (as a covariable?) in the analysis of temperature data, and vice versa? Are in this analysis only the treatments Control, 5N, PK, and 5NPK included to test for the interaction between 5N and PK treatments?

In the analyses all treatments were included in a nested design. All temperature time series values were listed and assigned to the corresponding replicates a, b, c of each treatment, and the potentially controlling variables N and PK load, as well as potential co-variables water content (and temperature, respectively). The co-variables were included because the predicted variable may not only depend on the treatment factor effect but also on variation of the co-variable independent of the treatment effect, for example caused by random variation in decomposition degree of peat with depth, which has a considerable influence on water contents in peat at a given matrix potential.

Fig. 3: Why only compare to control and not among all treatments as in fig. 5? For such multiple comparisons among treatments usually a Tukey post-hoc test is performed.

This was indeed an omission that we rectified. We now present temperature results from the replicated plots in terms of significant differences among all treatments (Figure 2). For consistency we used paired t-tests for these multiple comparisons, as presented with respect to CO_2 concentrations in Figure 5.

Thank you very much for the detailed and helpful comments and our apologies for the weaknesses in the manuscript that should have been better addressed beforehand.

Best regards,

Christian Blodau