Dear reviewers,

We would like to thank you for the insightful comments and suggestions and thank you for considering the revised manuscript for publication. Please find our detailed response regarding the comments below.

Reply to Anonymous Referee #1

General:

1. *One of the novelties of the paper I consider the test of the GEPmax dependence on leaf N content. In my view this deserves an additional exposition in the introduction and being mentioned in the abstract.”*

   Information was added in the introduction in P. 1, L. 31, P. 2, L. 28 -36.

2. *It is hard to judge how robust the response of the different PFTs to N fertilization actually is. Granath et al., 2009 suggest that some Sphagnum species behave differently and can actually adapt to higher leaf N content. Would a scenario with mosses having a higher max N tolerance in the N factor on GEPmax than in the original model also be possible? In this case mosses would simply be outcompeted by graminoids/shrubs through growth and light competition. Can you exclude this possibility?*

   Such a scenario is possible, however, we think that it is not likely. In our assumptions about the tolerance of Sphagnum to N we were conservative and considered the possible adaptation of *Sphagnum* by applying a rather high threshold value of leaf N content at 1.5 g N m^{-2} leaf area. The value is converted to N per grams of leaf dry mass with a specific leaf area of *sphagnum* that ranges from 135 to 473 cm^2 g^{-1} (Bond-Lamberty & Gower, 2007). With the specific leaf area of well-drained mosses of 135 cm g^{-1}, the N content threshold in the model was in equivalent to 20 mg N g^{-1} dry biomass. This value is higher than the N content of *Sphagnum* found in natural peatlands (e.g. Bragazza et al., 2005, Granath et al., 2009a) and at the high end of the N content in *Sphagnum* in the N fertilization experiments (Limpens et al., 2011). Toxic effects of N on *Sphagnum* were suspected to occur at extremely high N content that impaired the photosynthesis and growth of *Sphagnum* (Limpens and Berendse, 2003; Fritz et al., 2012). With the implemented current knowledge about N toxicity threshold in the model, the modeling results suggested that toxicity of N plays an important role in the decline of mosses. Other factors, such as the shading from increased shrub biomass, may have also played a significant role in the declining of mosses in the fertilized plots as well (Chong et al. 2012).

   Changes were made in P. 4 L. 27- 44 (in the word version).

3. *Reading this model study I often found myself looking up the PEATBOG model paper (Wu and Blodau, 2013) in order to find the corresponding equation. I don’t think this is a bad thing per se, regarding the complexity of the problem, but repeating the main model equations for a given*

sensitivity could greatly improve the visibility for the reader. My suggestions are e.g. the model eqs for (i) the dependence of GEPmax on leaf N content, (ii) the C/N effect on ER and (iii) the competition for N uptake.

Appendix A and B were added to specify the key equations and parameterizations.

Specific:

1. *"p. 10274, l. 17: Please add global estimates/modelling studies for future peatland C storage changes under N fertilization and dynamic peatland vegetation change, e.g. results from Spahni et al., 2013."*

   Added the following information to P. 2, L. 22-24 (in the word version):
   For example, a dynamic global vegetation model has simulated a reduction of the maximum annual NPP from 800 g C m^{-2} yr^{-1} to 450 g C m^{-2} yr^{-1} when coupling a dynamic N cycle to the C cycle (Spahni et al., 2013).

2. *"p. 10277, l. 14: missing "g N" units"*

   Information was added in P10288, L14 (in the word version P.4, L.5).

3. *"p. 10277, l. 28: Please be careful with the wording. If you define GEP (gross ecosystem production) as photosynthesis, I assume you mean gross carbon assimilation during photosynthesis. Later on photosynthesis is shown being dependent on leaf N content. But there are two parts of N allocation as you explain 10 pages later: once to photosynthetically active processes and once to biomass growth. Please be more precise in general and early on, when you use the term "photosynthesis" as this is a major part of the paper."*

   The term "GEP" and "photosynthesis" was examined and changes were made in P. 3, L. 46 and P.11 L.30 (in the word version).

4. *p. 10277, l. 28: To my understanding and according to Figure 1 this sentence is wrong, it should be GEP+ER = NEE and not ER+NEE = GEP.*

   This sentence has been rewritten in legend of Table 2

5. *p. 10279, l. 5: Do 10000 years reflect the basal age of Mere Bleue Bog? Is the model in a steady state regarding net ecosystem carbon accumulation?*

   10000 years was similar to the basal age of the Mer Bleue Bog of about 8000 years. After 10000 years of spinning up, the model was at a steady state regarding the ecosystem C and N accumulation and vegetation composition.

6. *p. 10279, l. 20: typo "leaf area index"*

Correction was made at P.10279, L.20 (P. 5, L. 35 in the word version)

7. *p. 10283, l. 5: I can't follow the last statements. Do sphagnum and vascular plant biomass have different properties that directly affect the rate of respiration in the model? Please clarify.*

   There was a mistake in the sentence and a correction was made in P. 20183, L. 5 (in P. 6, L. 41-42). In the model, the ecosystem respiration (ER) was an aggregation of the autotrophic respiration (AR) of each PFT and the heterotrophic respiration (HR) of labile and recalcitrant soil C (Appendix A). The autotrophic respiration rates per area were similar in all PFTs, considering the respiration rate per biomass and the specific leaf area of each PFT. The large difference occurs in the heterotrophic respiration of the labile and recalcitrant soil carbon (Appendix A, Table 1). The ratio of labile to recalcitrant biomass was set to be higher in the vascular plants than in *Sphagnum*, as a representation of their lower decomposability (Bragazza et al., 2012). The model overestimated the *Sphagnum* biomass and underestimated the vascular PAI in 2008, both leading to an overestimation of the recalcitrant fraction of the fresh litter input to the soil. Therefore, the modeled decomposability of soil organic matter was also underestimated and consequently the HR and ER were underestimated as well.

8. *p. 10287, l. 9: Is the C/N increase because of the shift in vegetation composition?*

   The increasing C/N ratio in the 5NPK, 10NPK and during the first 30 years of the 20NPK was a result of a faster net sequestration of C than N in the peat. The limited N sequestration in the peat subject to low N inputs was attributed to the moss layer that retained most of the N at the ground surface (Fig. 7b). When mosses were saturated and eliminated from the vegetation community, the N filtration failed and the N that had been retained by the mosses was transferred into the peat N pool. The C/N ratio in the peat thus started to decline after 30 years in the 20NPK treatment (Fig. 7i). Above explanations were added at P.9, L 39 – 42 (in the word version)

9. *p. 10288, l. 26: Just to get it right, for the comparisons of the modifications 1,2,3 you do not correct GEP, ER, NEE anymore as you did in Fig. 1?*

   There were no corrections for modification 1, 2 or 3.

10. *p. 10290, l. 3: Where can I find supplementary Figures?*

    The supplementary figures and tables are presented in a separate file named "supplementary information" that is downloadable from the manuscript webpage: http://www.biogeosciences-discuss.net/11/10271/2014/bgd-11-10271-2014.pdf.

11. *p. 10290, l. 8: Use 'Mer Bleue' instead of 'MB' or define it at the first occurrence of the name in the text.*

    "MB" was changed to "Mer Bleue" (P.10, L. 18 in the word version).

12. *p. 10291, l. 18: How does the model handle competition for N uptake exactly? Assuming a bog ecosystem with limited N availability: is there a priority rule for PFTs accessing nutrients? Would that change in the case for N fertilization and thus increased GEP?*

    We did not implement a priority rule for N uptake other than a moss filter. Instead the uptake is conceptualized as the result of differences in N uptake kinetics between PFTs. The N uptake of PFTs was modeled by considering the filtration of deposited N by mosses and the uptake of N by vascular roots. Mosses account for 95% of N uptake before N transfer is no longer limited by uptake into mosses. When N concentration is larger than 20 mg N per g biomass, larger quantities of N enter the soil water and become available to vascular plants. The competition for dissolved N among the vascular PFTs is controlled through the architecture of the roots and the affinity for the three N sources (NH_4^+, NO_3^- and DON), whose concentrations is calculated in each soil layer. The Michaelis-Menten equation is used to calculate N uptake specific for soil layer, PFT, and N species. The maximum uptake rate (V_{max}) is larger and the half saturation constant (K_m) lower for NO_3^- than for NH_4^+. However, V_{max} and K_m were set to be the same for shrubs and graminoids. The uptake of N is then regulated by the concentration of N in the PFT, which in turn is determined by the N use-efficiency and N turnover in the PFT. Therefore, the priorities of the N uptake of vascular plants are not hard-coded in the model but instead internally controlled by the N cycling within the PFTs. In general, the modeled shrubs showed a higher affinity for N uptake than graminoids in the upper layers due to their shallower rooting depth and large amount of roots in the shallow peat. This was the case prior to N fertilization. When N increased in the soil water, graminoids were able to take up more of the element.

    The differences in GEP in the three modifications can be explained by a vegetation shift with regards to the amount of leaf biomass and the turnover rates. In the model, the maximum GEP was set to be equally high for graminoids and shrubs at about four times the level of mosses. Hence a shift from mosses to vascular plants changes the total GEP (Fig. 9d), but the relative abundance of shrubs and graminoids does not. The modeled GEP differed greatly between modification 1 and 2 (Fig. 9f, 9g) despite a similar vascular foliar biomass (Fig. 8f, 8g). In modification 1, graminoids dominated resulting in more C assimilation over the same time period.

    Information about the model algorithms used in plant competition was added in P.4 L. 27 – L. 44. Equations of N uptake in PEATBOG were added in Appendix A.4.

13. *Table 3 caption is missing units: g C m-2 ?*

    The unit has been added (P.24, L.1 in the word version).

14. *Fig. 1 caption has a typos: “The green dotted lines in? represent weekly averaged CO2 flux ...”, and “Note that P was K not constrained in the model. “ Please correct.*

    These expressions were corrected (P. 25, L. 5 in the word version).

15. *Fig. 4: Figure shows “FPT” instead of “EPT”*

    “Biomass” has been changed to “C pools” in the caption of Fig. 4 (P.23, L. 15 in the word version)

16. *Fig. 8: Please write out ’sh’ and ’tr’ in the line description. I guess ’sh’ means shoot not shrub. Also correct ’shurb’.*

    These unclear abbreviations were corrected in Fig. 8.

Reply to Anonymous Referee #2

Major comments

1. *"The methodology is fairly hard to follow. The manuscript would greatly profit from a concise description of the PEATBOG model (possibly including a conceptual figure) in terms of what it does and what it does not simulate. In the current version of the manuscript, some of this material is not present (e.g. how do the vegetation dynamics work, how is production and nutrient uptake modelled?), or it is spread in the introduction (p102754 l 26) and the results section (p10284 l1ff), which makes it hard to appreciate what the model does. Of course, the model has been published elsewhere, but a reader should be able to understand what a model does without having to refer to the model description paper frequently."*

   A paragraph in the introduction (P. 2, L. 26 –L. 38) and a session in method (2.2 P.4, L. 11) were added to explain how plant competition for light and nutrients is modeled in PEATBOG.

2. *The authors state that their model "consistently emphasises mass-balance principles" (which I think is trivial), but then they modify the modelled GEP and RE estimates by some empirical scalar to compare this to chamber measurements. It is unclear to me whether this correction occurs after the modelling, or whether the model has been rerun with the correction applied. They then state that (p 10282 l 2ff) these corrections accounted for the model's LAI biases, but it is unclear, how this estimate was derived, and whether this means that the LAI biases of the model were corrected. In particular p 10279 l 20ff left me puzzled as to what as actually done.*
   A particular concern is that the authors have applied a specific GEP factor for each fertilisation experiment, effectively compensating for a tendency of the model to overestimate the growth response to fertilisers. Applying a fertilisation specific calibration for the dominant process somewhat reduces the utility of using a process-based model to simulate the effect of fertilisation on foliar N and production. If the response had to be calibrated specifically for each treatment, this inspires little confidence into the predictive capacity of the model.
   Step four in the model development only really become clear when one hits Figure 8. I strongly recommend to move this section to the model description and also outline the reasoning and main effects there. Importantly, it also remains unclear whether the results in Figures 1-7 are based on version 1 or version 3.

   The methodology was rewritten in section 2.3 (P.5 L.37 to P.6 L.15) following the reviewer's suggestions. In the new version the procedures used should be better comprehensible.
   We have conducted the study in four steps with the first two steps on the short-term effects simulations and the last two steps on the long-term simulations. The first step we simulated short-term effects of N on the C pools and fluxes. As a second step, we examined discrepancies of the simulations for the "short-term" simulations and identified key controlling factors of the results. In the third step we simulated long-term effects of N on the C cycle and the fourth step experimenting on the key controlling parameter and the variability of long-term results. The simulation results in figure 1-7 were based on the original model parameterization without any modifications. Only figure 8 and 9 were produced from modified model.

3. *"The authors state that they inferred GEP from NEP and RE, but how is it then possible that there are more points of GEP than of NEP and RE in Figure 1 and 9?"*

   The observed NEP was measured at full, half, and quarter of the incoming radiation and in the dark and ER was measured once at mid-day with PPFD larger than 1000 umol m^{-2} s^{-1}. The data were collected approximately bi-weekly during the summer. Then the sum of NEP and ER was related to PAR level and obtained a correlation of GEP and PAR for each plot for each year. GEP that was used for comparisons with the model output was calculated from obtained relations between GEP and PAR and using measured PAR from the flux tower as a driver in the transfer function. We did it this way to have a continuous record of observed GEP data over time. NEE that was used for comparison with model output was calculated by subtracting ER from the GEP that was calculated as described above.

4. *"The authors state that the 2001 GEP is overestimated by the model. To me it seems that rather there is an jump in GEP between 2001 and the remainder of the time series. Why blame the model?"*

   We suspected that the "jump" was mainly a result of the discrepancies between the starting status of the model and the observations. Another contributing factor was that 2001 was an exceptionally dry year and the observed CO_2 uptake was much lower than the other years (Lafleur et al., 2003).

Minor comments

1. *"Abstract line 11ff. At this stage, the reader does not yet know that you mean leaf N concentrations and its effect on GEP -please make this clear."*

   Information was added in the abstract (P.1, L.31 in the word version).

2. *"Introduction: P 10274 L 3-5: these estimates are based on assuming no air-pollution control, whereas the newer scenarios assuming air-pollution control suggest declining N deposition over Canada. Locally this may be different of course, but here and in the discussion there should be a reflection as to how realistic the simulations and experiment in assuming a quadrupling of N deposition are. See Lamarque et al. 2011"*

   Information was added in P.10, L. 18 (word version)

3. *"P 10274 L 10 -15. This text confuses time-scales. Peatlands have arguably been a large C sink during the holocene, but their contribution to the current net terrestrial C uptake is likely rather small, given the small NEE and limited geographic extend."*

   Information was added in P.2, L. 17 (word version)

4. *"P 10274 L 24ff. Yes, but see Janssens et al. 2010"*

   The sentence was rewritten in P.2, L. 40 (the word version)

5. *"P 10275 L 26ff: This is unclear, partly because one does not know what the PEATBOG model is actually doing. I would recommend to create a new Section 2.1, in which the model is briefly described (see comment above), and this explanation is integrated"*

   A new section in method was added in the PEATBOG model description (P.4, L. 11).

6. *"P10279 L 7ff. To remove all doubts, this paragraph should end with, "the results presented in Fig 1-9 are based on version X, unless stated otherwise.""*

   Information was added (P. 5, L.10 in the word version)

7. *"P10280 L 1: This paragraph would follow more logically the first paragraph of this section. Maybe move the model strategy paragraph to a separate section, in which the PEATBOG model is also briefly explained?"*

   This paragraph has been moved to 2.3 following the model description section 2.2.

8. *"P 10280: L 17: I'm left puzzled as to why the ER data were corrected linearly and by year. Why would one not assume that the bias between day-time and daily average RE was constant with time?"*

   The mistake was corrected. The correcting factor was not distinguished by year but by treatment. The observations of ER occurred in the mid-day about every two weeks during the summer. Therefore it represents the values at the highest daily temperature. The model on the other hand, was driven by daily average temperature and generated daily average ER. In order to have more comparable observations and simulations, we scaled up the modeled daily average ER by a factor that in principle represents the difference between daily average and maximum temperature. This factor is chosen to be the slope of ER and the air temperature, i.e. the correlation between ER and temperature. We differentiated the ER-T slope of each fertilization treatment to exclude the effect of other factors on the ER-T relation, such as leaf area index and biomass. Information was added in P.5 L.45 - 47.

9. *"P 10283 L 7: As this refer to the fertiliser response, it should read "with increasing N inputs/fertilisation"."*

   Corrected (P.7, L.18)

10. *"P 10284 L 1: This mentioning of model details would be more suitable within the Methods section - comes a bit out of the blue here"*

    This part is moved to the method. (P.6, L.2 – L.7)

11. *"P 10285 L 11 I cannot see this in Figure 5, possibly due to the large variability there?"*

    We've added data points for 11-year averages over the length of the repeated input data in Figure 5.

12. *“P 10293 L 24: This is one of the places where an introduction to the PEATBOG model would really help to understand what is going on.”*

    An introduction of the PEATBOG model has been added section 2.2 P.4 L.13 – L.46.

13. *“P1ß296 L 3-5: Would one not rather assume that the model converges to a new equilibrium without a phase of C losses? Please explain why you think that in the long term N fertilisation would cause C losses.”*

    Discussion was added in P. 9, L 23 to P.10, L. 5. As autotrophic respiration approaches a new steady state that corresponded to the new vegetation cover, heterotrophic respiration is critical to the overall C budget of the peatland. On the one hand, heterotrophic respiration was strongly linearly related to labile carbon content of the peat, which changes little after the ecosystem has reaches new stable states after about 20 years. On the other hand, heterotrophic respiration showed strong negative relation with C/N ratio of peat when mosses disappeared from the system. Also the model showed that most of the fertilized N was retained by peat without the “moss filter”. Therefore the C/N of peat will decrease and subsequently the heterotrophic respiration will increase, as long as N fertilization sustained. Ultimately it is also possible that when the peat becomes N saturated, heterotrophic respiration will approach a maximum level. However, the trend of N saturation did not show this within the time period of the simulation. Therefore, based on the modeling results on a time scale of 80 years, we suspect a C loss from the peatland to occur.

14. *“P 10296 L 19: How dod you extrapolate to 700 years. This is not clear.”*

    Information was added in P. 10 L. 23 to L. 24.

15. *“Table 3: I think that it’s worth highlighting in the text that despite tuning the GEP, PEATBOG overestimates PEAT storage by a factor of three, even though the vegetation cover seems to be appropriately simulated. Why?”*

    Information was added in P.7 L.42 to L47. There are two factors contributing to this overestimation. One is the hard-coded bulk density in the model that overestimated the bulk density and thus the C pools in the given 10cm depth. The other factor is that the model considers litter also as peat. The large increase of the modelled C pool in peat was in line with a large increase in the litter accumulation observed in the fertilized plots (Bubier et al., 2007).

16. *“Figure 5 & 6: The long-term trends are difficult to spot because of the large interannual variability. I recommend smoothing these results using an 8 year running mean.”*

    We’ve added trend lines of 11-year averages in Figure 5 and figure 6. The input data was repeated every 11 years.

The following references were added:

Aerts, R., J. T A. Verhoeven, and D. F. Whigham: plant-mediated controls on nutrient cycling in temperate fens and bogs, Ecology, 80, 2170–2181, doi:10.2307/176901, 1999.

Chong, M., Humphreys, E.R. and Moore, T.R.: Microclimatic response to increasing shrub cover and its effect on Sphagnum CO2 exchange in a bog. Ecoscience, 19: 89-97, 2012.

Evans, J. R: Photosynthesis and nitrogen relationships in leaves of C3 plants, Oecologia, 78, 9-19, doi: 10.1007/BF00377192, 1989.

Granath, G., Strengbom J., Breeuwer, A., Heijmans, M. M., Berendse, F. and Rydin H.: Photosynthetic performance in *Sphagnum* transplanted along a latitudinal nitrogen deposition gradient, Oecologia 159, no. 4, 705-715, DOI: 10.1007/s00442-008-1261-1, 2009a.

Granath, G., Wiedermann M. M., and Strengbom J.: Physiological responses to nitrogen and sulphur addition and raised temperature in *Sphagnum balticum*, Oecologia 161, 481-490, doi: 10.1007/s00442-009-1406-x, 2009b.

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Hikosaka, K., and I. Terashima.: A model of the acclimation of photosynthesis in the leaves of C3 plants to sun and shade with respect to nitrogen use, Plant, Cell & Environment 18, 605-618, doi: 10.1111/j.1365-3040.1995.tb00562.x, 1995.

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Wang, M., Murphy, M. T. and Moore, T. R.: Nutrient resorption of two evergreen shrubs in response to long-term fertilization in a bog, Oecologia, 174, 365-377, doi:10.1007/s00442-013-2784-7, 2014.

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Bond-Lamberty, Ben, and Stith T. Gower, Estimation of stand-level leaf area for boreal bryophytes, Oecologia 151, no. 4: 584-592, DOI 10.1007/s00442-006-0619-5, 2007.

Bragazza, L., Limpens, J., Gerdol, R., Grosvernier, P., Hajek, M., Hájek, T., Hajkova P., Hansen. I., Iacumin, P., Kutnar, L., Rydin, H. and Tahvanainess, T.: Nitrogen concentration and δ15N signature of ombrotrophic *Sphagnum* mosses at different N deposition levels in Europe, Global Change Biology 11(1), 106-114, 2005.

Fritz, C., G. Van Dijk, A. J. P. Smolders, V. A. Pancotto, T. J. T. M. Elzenga, J. G. M. Roelofs, and A. P. Grootjans.: Nutrient additions in pristine Patagonian *Sphagnum* bog vegetation: can phosphorus addition alleviate (the effects of) increased nitrogen loads, Plant Biology 14, no. 3, 491-499. doi:10.1111/j.1438-8677.2011.00527.x, 2013.

Granath, G., Strengbom J., Breeuwer, A., Heijmans, M. M., Berendse, F. and Rydin H.: Photosynthetic performance in *Sphagnum* transplanted along a latitudinal nitrogen deposition gradient, Oecologia 159, no. 4, 705-715, DOI: 10.1007/s00442-008-1261-1, 2009a.

Granath, G., Wiedermann M. M., and Strengbom J.: Physiological responses to nitrogen and sulphur addition and raised temperature in *Sphagnum balticum*, Oecologia 161.3: 481-490, DOI: 10.1007/s00442-009-1406-x, 2009b.

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On behalf of all co-authors, yours sincerely,

Yuanqiao Wu, Christian Blodau