

We very much appreciate the **review of anonymous referee #1**. Below, we provide replies (standard) to each of the comments (italic). We considered reviewer's suggestions at several points in the text. We feel that this clarified the scope and objectives of the manuscript. A revised version of the manuscript with tracked changes has been uploaded as supplement.

*General comments:*

*The paper is well-written. The English is good. The topic is very relevant: drained peat soils are in many countries a major source of GHG gasses. Moreover peat soils are very prone to climatic change and GHG emissions from peat soils can increase dramatically in this century. However, this is not widely known by the general public and policy makers. To raise the public awareness with convincing arguments upscaling is needed to quantify in a better way the GHG emissions of peat soils. Not every researcher on the degradation of peat soils and/or on the emissions of GHG was fully aware of the importance of the groundwater table as explaining factor for the emissions of GHG. In too many papers no data is available on the (dynamic) groundwater or surface water table. The reviewed paper is filling up this gap for at least a part. The paper is very complete and only minor revision is needed, so I have only a few specific comments*

*Specific comments:*

*1. I am missing some rather obvious predictor variables:*

*- the thickness of the peat layer: the GHG emission of a thin peat layer (e.g. 40 – 50 cm) will be significant lower than of a peat layer > 100 cm. This can have a direct effect on the transformation presented in Figure 3.*

We agree that there are several more site characteristics that are known to influence GHG emissions. However, their influence is often not very well understood. For example, the reviewer points out the peat thickness as an influencing factor. However, a recent study on shallow peat layers (Leiber-Sauheitl et al., 2014) shows that at least down to annual mean water levels of -39 cm GHG emissions still show a linear increase, although in their study the peat layer had only low C contents and was only 30 cm thick. Additionally, at the large scale, information on peat thickness necessary for upscaling is often missing.

Aside from these difficulties, we emphasize that the objective of our study is to regionalize annual mean water levels with optimal accuracy in the sensitive range for GHG emissions, and not the GHG emissions themselves. The latter are influenced by much more site characteristics, in particular soil properties. It was our intention to separate the water level influence from other influencing factors. Our work, therefore, must be considered as one piece of a broader framework for the regionalization of GHG emissions that includes other site characteristics and must be developed in future research. For example, if for specific regions detailed information on peat thickness becomes available and its effect on GHG emissions can be estimated by the use of transfer functions, the map of transformed water levels (WLt) can be used as an input for this follow-up regionalization. We clarified this in the revised manuscript.

(lines 122 to 129 and 961 to 966)

*- the existence of a (thin) mineral layer on top of the peat soil: this can be an artificial layer of sand to increase the trafficability in wet periods and reduce damage by trampling and pouching. The mineral layer can also be clay deposited by flooding by rivers or the sea.*

*This can have substantial effect on the GHG emission (because the mineral layer does't contribute to the oxidation) and so a direct effect on the transformation presented in Figure 3.*

See above. Our intention was not to regionalize GHG emissions but the water level effect on GHG emissions.

- *positive or negative seepage (and/or potential of deep groundwater): can have a major effect on the WL (10 – 20 cm).*

We agree that the occurrence of positive or negative seepage can have major impacts on WL. Values of the hydraulic potential of the deep groundwater are however mostly not available at the large scale. Some countries may represent exceptions. However, there are some predictor variables in our study that are supposed to be correlated to seepage. For example if a peatland is characterized by a peat clay layer at its peat base, this may largely inhibit any seepage. Higher large-scale topographic wetness indices may indicate a higher probability for positive seepage. Furthermore, the peatland type itself may indicate seepage (higher probability of negative seepage for rain-fed bogs) . We hope that these variables partly account for seepage influence in our statistical model.

(see lines 272 to 276 and 304 to 306)

*Maybe there is no good data about these variables, however, then this should be mentioned.*

Done.

(see also lines 857 to 859)

*2. Why do the authors only use the mean annual water table. For CO2 emissions the (deepest) summer water table is probably a much better predictor variable. For CH4 are probably the highest groundwater levels important. I have the impression that good year round data is available.*

We agree that annual mean water level is probably not the best statistical measure to describe the water level effect on annual GHG emissions. However, we are not aware of well-established transfer functions that relate more complex statistical measures of water level dynamics to GHG emissions. Are highest and deepest water levels really important for annual CH4 and CO2 emissions, respectively? Our impression is that there exist several 'single-site' observations, but statistically well-founded multi-site relations of dynamic features to GHG emissions are not yet proposed. Nevertheless, it is certainly worth evaluating our approach for the regionalization of dynamic features. However, we consider data harmonization (interpolation of manual measurements) to be a crucial part of such an analysis. Our data set is however not yet harmonized. Furthermore, we believe that our study is already quite comprehensive, and the inclusion of additional models for dynamical features is beyond the scope of our manuscript.

(see lines 124 to 129)

*3. I have the impression that the ditches are only considered for drainage, while infiltration via the ditches can be very important to keep the groundwater table as high as possible. This could also be (a part) of the explanation of the impact of the total length of ditches: di len dry (250). Anyway, whether ditch water infiltrates (as in a polder situation) or not, can also be a strong key variable.*

This is true and information on water management, i.e. on whether ditches are used for drainage only or also for infiltration, could strongly improve model performance. However, there are no maps on ditch water levels and on the drainage/infiltration situation for Germany. This is already discussed in the former manuscript (lines 251 to 262 and 847 to 850).

*4. Unconsolidated rock as material at peat base: do you mean sand or clay soil? There is a major difference in saturated water conductivity of clay and sand, so making a difference would be very advisable. Note that many readers are not used to the term “unconsolidated rock”.*

We mean 'loose' unconsolidated rock, like sand and gravel sediments. We made this more clear.

(see lines 272 to 276)

*5. The word “ raster” is used. Shouldn't that be “grid”*

Thank you. 'Grid' is more appropriate.

*Page 3866, line 13: influence of grassland is reduced by a factor 0.5: why 0.5? This is a very strong reduction. My feeling is that this reduction is by far too strong.*

We agree that 0.5 is very low. We tested several factors and 0.5 showed to be the best factor. We think that the strong reduction is caused by the fact that our grassland category also includes wetter grasslands that cannot be detected with the land cover catalogue. As discussed in the land cover section, wet grasslands are only separated by tree and shrub, and 'wet soil' attributes. Other wet and extensively used grasslands cannot be detected. In contrast, arable land is certainly dry. The factor 0.5 is thus partly the result of the large scatter of WL in the grassland category.

(see lines 246 to 250)

*Furthermore: wouldn't it be more logic to have grassland as a basis and use a factor > 1 for arable land?*

Only the ratios of the weights matter. Whether 0.5 and 1, or 1 and 2 is used makes no difference. Regarding the logic: As within the 'grassland' land cover type there are not only intensive grasslands, it appears more intuitive to us keeping the '1' for arable land, signifying deep drainage.

*Page 3871, line 17: If I remember well, Berglund and Berglund (2011) concluded that sensitivity of CO2 emissions to WL was very low or not existent. Can this be somehow in agreement with the relation in fig 3. (Note: In fact I think that the relation in fig. 3 is OK!). Note that the GHG emissions of thin peat layers and peat layers with on top a (thin) mineral layer might not be well represented by the relation in fig 3.*

We are aware of the experimental observations of Berglund and Berglund (2011). They observed emissions from lysimeters at two constant water levels. CO2 emissions from the -40 water level cm lysimeter were even slightly higher than emissions from the -80 cm water level lysimeter. These results indicate a leveling out of emissions at approximately -40 cm, which is shallower than shown by fig.3. There are however other studies which we also cite that indicate still an increase of CO2 emissions at water levels deeper than -40 cm (e.g., van den

Akker et al. 2011, Drösler et al. 2011). The function of fig. 3 represents an estimated average response shape. In the revised manuscript we emphasize that we do not include soil property effects, as mineral layers on top of the peat or thin peat layers, in the response shape (see also discussion above)

**(Please note also the replies to referee #2 and #3 below)**

Kind regards,  
Michel Bechtold and co-authors

References:

Berglund, O. and Berglund, K.: Influence of water table level and soil properties on emissions of greenhouse gases from cultivated peat soil, *Soil Biol. Biochem.*, 43, 5, 923-931, doi:10.1016/j.soilbio.2011.01.002, 2011.

Drösler, M., Freibauer, A., Adelman, W., Augustin, J., Bergmann, L., Beyer, C., Chojnicki, B., Förster, C., Giebels, M., Görlitz, S., Höper, H., Kantelhardt, J., Liebersbach, H., Hahn-Schöfl, M., Minke, M., Petschow, U., Pfadenhauer, J., Schaller, L., Schägner, P., Sommer, M., Thuille, A. and Wehrhan, M.: Klimaschutz durch Moorschutz in der Praxis, Ergebnisse aus dem BMBF-Verbundprojekt „Klimaschutz - Moornutzungsstrategien“ 2006-2010, vTI-Arbeitsberichte 4/2011, 2011.

Leiber-Sauheitl, K., Fuß, R., Voigt, C. and Freibauer, A.: High CO<sub>2</sub> fluxes from grassland on histic Gleysol along soil carbon and drainage gradients, *Biogeosciences*, 11, 735-747, doi:10.5194/bg-11-749-2014, 2014.

van den Akker, J. J. H., Jansen, P. C., Hendriks, R. F. A., Hoving, I. and Pleijter, M.: Submerged infiltration to halve subsidence and GHG emissions of agricultural peat soils, *Proceedings of the 14th International Peat Congress, Stockholm, Sweden, 2012.*

We very much appreciate the **review of anonymous referee #2**. Below, we provide replies (standard) to each of the comments (*italic*). At several points, we thoroughly revised the manuscript, in particular the paragraph about the weighting scheme. We feel that this considerably improved this part of the manuscript. A revised version of the manuscript with tracked changes has been uploaded as supplement to the reply to referee #1.

`\begin{itshape}`

General Comments This manuscript contains some interesting new approaches to generate exhaustive spatial predictions in a data-scarce environment. The structure of the manuscript could be improved by starting with the methodology followed by the data and data requirements.

Specific comments The structure of the manuscript would benefit from starting chapter 2 with paragraph 2.3 first describing the general methodology followed by the data used. The current structure requires referring to paragraphs later on in the manuscript.

`\end{itshape}`

We also see that at some points we refer to the methodology during presentation of our data on target and predictor variables. To resolve this problem, we considered shifting the general methodology to the beginning of chapter 2 as suggested by the reviewer. However, several aspects of the model building and weighting scheme are the consequence of the detailed characteristics of the target and predictor variables and thus require the data background for discussion. Regarding the more complex discussion during the presentation of the model building, we preferred to present first the characteristics of the data. Given the comment of the reviewer, we did however shift the more detailed crosslink about data gaps from the data presentation section to the model building section. Further, we now provide first the data weighting scheme before we present the performance criteria in which the weights are used. We hope that this helped to minimize the crosslinks to an acceptable level.  
(see lines 338 to 345)

`\begin{itshape}`

On page 7 line 19 you state that boosted regression trees allow for data gaps, but I can't find an explanation on how predictions are made when predictors are missing.

`\end{itshape}`

This is a good question. The detailed handling of missing values (i.e. NA values) is not well documented in the `gbm` and `dismo` package documentations. Given this lack of documentation, there are several online discussion about this question. It is clear that the `gbm` package handles missing values in predictor variables by introducing surrogate splits. When applying the function `pretty.gbm.tree` to the `gbm` object, it can be seen that each split consists of 'LeftNode', 'RightNode' and a 'MissingNote'. By own tests, we found out that the mean target value belonging to the predictor NA values is attributed to these surrogate splits during model building. In case there is no NA value in the predictor variables, the mean of all target values is stored in the `MissingNote`, which is then applied during prediction when NA values in the predictor variables appear.  
(see lines 338 to 345)

`\begin{itshape}`

The weighting scheme discussed in paragraph 2.3.3. is presented as common practice yet there are no references in the entire paragraph. The mathematical formulation for

Wi gives three different formulas (5,6,7) all using the same symbols, this indicates that there are in fact three different Wi-types, please make this clear from the mathematical notation or give only the formula used. Formulas 8 and 9 require a reference or some mathematical backup.

\end{itshape}

We thank the reviewer for pointing out inaccuracies in the way we presented the weighting scheme. Our weighting scheme is not common practice. We are not aware of a 'common practice' for heterogeneous target and predictor data situations similar to our study. Our feeling is that despite the obvious importance of data weighting, there are no clear guidelines how to deal with it in complex modeling problems and that there is always a degree of subjectivity involved (see also Francis et al. 2011). This is emphasized in the new manuscript. We revised the weighting paragraph thoroughly. We clarify on which statistical principles we rely our weighting scheme (missing references and mathematical background is now provided) and how we combine them to a new weighting scheme for our modeling problem. The inaccuracies of the mathematical formulations (former equations 5,6,7) have been corrected.

(see lines 433 to 562)

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Ad a formula number for the BIAS.

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Thank you. Done.

\begin{itshape}

Notation in formulas (3, 4, 4b) is not concise: i is used both as subscript and superscript, it should be subscript.

\end{itshape}

Correct. Our mistake.

\begin{itshape}

Simulated or estimated values are commonly indicated with the hat-symbol, individual observed values can be denoted with the subscript i.

\end{itshape}

Here we prefer to keep our notation which is also frequently used, e.g. Gupta et al. 2009

\begin{itshape}

In paragraph 2.3.2 the weights Wi are used, but the paragraph on weighting comes later on, start with paragraph 2.3.3.

\end{itshape}

Done.

\begin{itshape}

The reference (Gaast., 2009) is missing in the literature section

\end{itshape}

It is "van der Gaast et al. 2009", and it is already in the references.

Kind regards,  
Michel Bechtold and co-authors

References:

Gupta, H. V., Kling, H., Yilmaz, K. K., Martinez, G. F, Decomposition of the mean squared error and NSE performance criteria: Implications for improving hydrological modelling. *J Hydrol*, 377, 80-91, 2009.

Francis, R. I. C. C.: Data weighting in statistical fisheries stock assessment models, *Can J Fish Aquat Sci*, 68, 6, 1124-1138, doi:10.1139/F2011-025, 2011.

We thank for the **review of anonymous referee #3**. Below, we provide reply (standard) to the comment (*italic*). A revised version of the manuscript with tracked changes has been uploaded as supplement to the reply to referee #1.

`\begin{itshape}`

The paper is of general good quality and should be accepted for publication soon. Maybe, minor revision are necessary, as they are already listed in the reviewer's comments nr 1 (supplement). The paper would benefit, if the method 'boosted regression trees' would be explained in more detail.

`\end{itshape}`

We extended the introduction to boosted regression trees. Next, it would be necessary to include also schematic figures. However, the concept has been presented in several papers to which we refer, and we think the degree of detail as presented in the revised version is sufficient to understand the methodology of our model building.  
(see lines 317 to 347)

Kind regards,  
Michel Bechtold and co-authors