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Dr. Ilja van Meerveld
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- Corrections HessD 10 C17-22, 2013

Dear Dr. van Meerveld,

attached you will find the corrected version of our HESSD paper "Impact of long-term drainage on summer groundwater flow patterns in the Mer Bleue peatland, Ontario, Canada".

We carefully evaluated the reviewers' suggestions and implemented them in the text wherever possible. An additional version of the manuscript with all changes highlighted in yellow has been uploaded.

Reviewer 1

Likewise, projections of future climate are limited to an opening statement about a drier climate with no detailed description of climate projections or documentation.

> *We added a section in the introduction, where the predicted changes in climate in Ontario are briefly summarized (Line 57-70).*

1. Provide a full description of the predrainage vegetation and post-drainage growth of the forest. Tree cores from a representative number of trees may provide some evidence of forest growth. Time series analysis of aerial or other photographs may also help.

We added a figure (now Figure 2) and text in the methods (L.146-147), results (L. 282-292), and discussion (L. 573-582) that describe the pre-drainage composition of the vegetation, as deduced from pollen analysis, and core stratigraphy on both sides of the ditch. These data show that both sides of the ditch had a similar pollen record, typical for a bog, and began to deviate from each other after the ditch had been dug.
Time series of aerial photographs for the period before 1923, when the ditch was established and for the years afterwards, when differences emerged, are not available.

2. Provide evidence of uniform surface elevation and peat properties before drainage. Peat core dating may provide data needed to document whether or not drainage had effects beyond natural spatial heterogeneity.

Regarding peat core dating see comments above. Surface elevation data from before 1923 are not available but there is strong comparative indication that the cross section that we in the paper describe in Figure 1 is not the original land surface.
An intact peatland margin of the Mer Bleue peatland has been described in terms of surface elevation previously (Fraser, C.J., Roulet, N.T., Lafleur, M.: Groundwater flow patterns in a large peatland. J. Hydrol. 246: 142–154. DOI: 10.1016/S0022-1694(01)00362-6, 2001). A cross section of from a lagg (P1) into the peatland margin located in a distance of 3 km west of our study site is shown below (Figure 1). The cross section illustrates the typical dome-shaped morphology of this bog on scales of half a kilometer. At the transect investigated in this study this characteristic surface slope is strongly altered with the ditch representing a local minimum in elevation instead.

The peat decomposition degree along the transect investigated here has previously been examined as well (Blodau, C., Siems, M., (2012): Drainage-induced forest growth alters belowground carbon biogeochemistry in the Mer Bleue bog, Canada, Biogeochemistry, 107(1-3), 107-123). This study documented that the peat near the ditch is much more decomposed than 200 m inland on the 'bog' side of the ditch. Furthermore it showed that on the 'forest' side the peat is more decomposed at the same distance from the ditch than on the 'bog' side. The remaining peat had also lost in decomposability, particularly at the forest side. Both findings suggest that peat was extensively mineralized near the ditch and particularly under the forest. These data are fully in line with a lowering of the land-surface, as documented in many other studies of peatland drainage before.

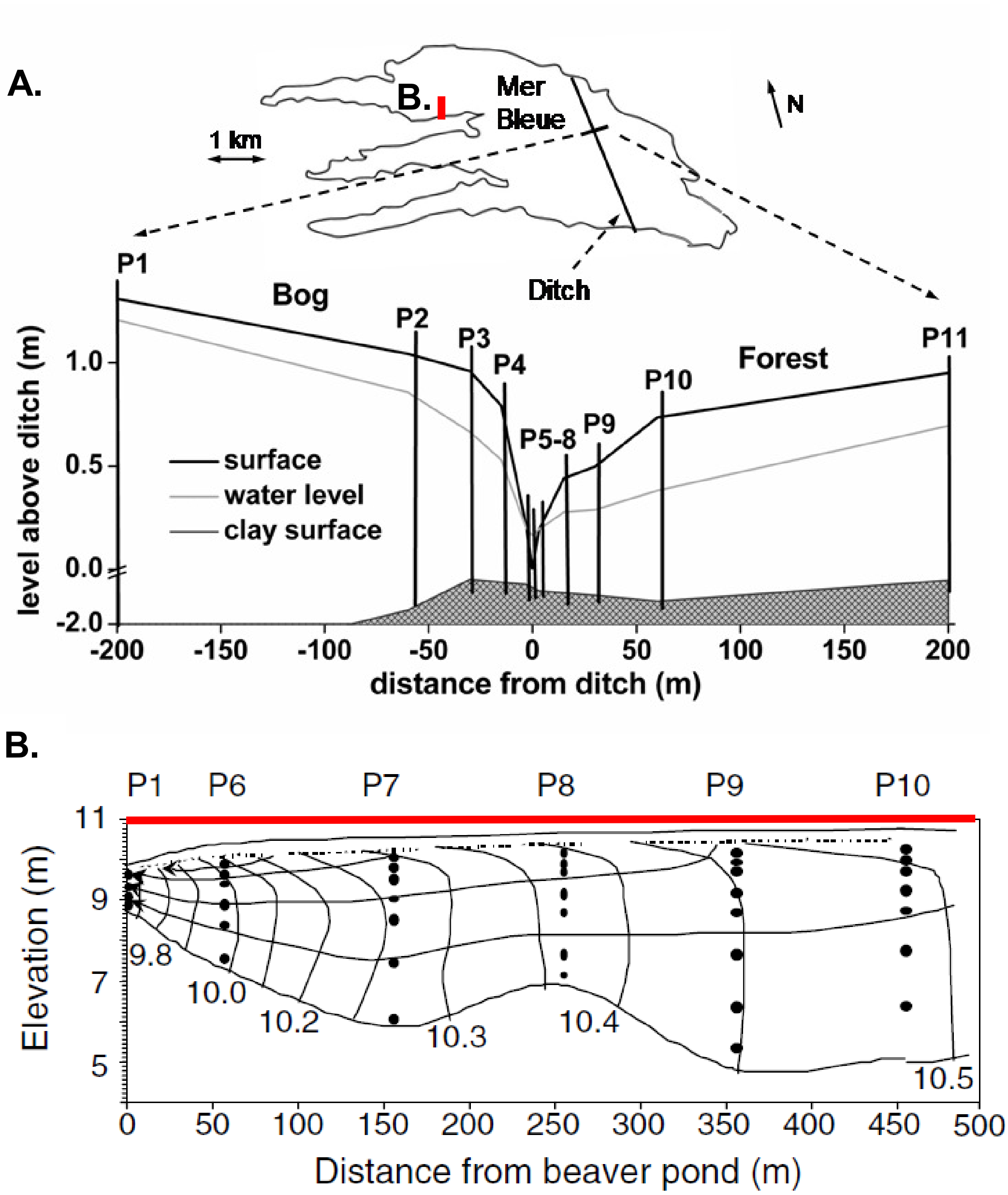

Figure 1 *Comparison of cross section of our study site (A). with the Mer Bleue peatland margin shown in red and described in Fraser et al. (2001) (B).*

3. Given that it may not be realistic to document changes in peat hydrology, there needs to be some discussion of the possibility that peat physical properties were different before drainage and that no real change occurred due to drainage. That is, discuss plausible alternate interpretations of the measured data.

There does not seem to be any other reasonable cause of systematic *variation of peat hydraulic properties than the establishment of the ditch given the genesis of the Mer Bleue peatland, the similar vegetation conditions in the Mer Bleue peatland in general and specifically in the drained area before drainage (Figure 1, Figure 2), and the data provided by Fraser and colleagues' (2001) from a similar nearby transect that is uniform in terms of peat physical characteristics. We documented before that the decomposition degree of the peat differs towards the ditch as well, which is in line with changes in the physical properties that are reported here (Blodau and Siems, 2012).*

4. Cite literature that would provide projections of local or regional climate change.

This change was implemented (L. 57-70; 563-560)

5. To provide more context, discuss the contemporary rate of change of air temperature and other relevant climatic variables, at the site or within the region.

See comment above.

6. Expand the discussion to more completely address the future climate analogue and what it all means.

We revised the discussion regarding this point (L. 550-569) and added some text in the introduction as well (see above). In our view one should not overemphasize the analogue concept, though. It does say in a proof-of concept way that drying can a) either lead to little change or large change in ecohydrological functioning and that b) thresholds apparently must exist for either development to occur. This finding is one central message other than the documentation of changes in peat hydraulic properties and flow patterns. More work is needed to follow up on this concept by investigating more field sites in case studies and by using coupled hydrological-ecosystem modelling to understand this issue in a "dose-response" and quantitative way. To do so is beyond the scope of this initial study, however.

In addition to the central premise, other aspects of the paper need to be revised. Much of the peatland terminology used in the paper is confusing. Given data that document groundwater discharge, the site does not seem to be an ombrotrophic bog. Peatland hydrological terminology needs to be properly and consistently used. Finally, to improve grammar throughout the paper, many uses of "in" should be replaced with "at".

We reviewed the paper for confusing terminology, especially regarding the hydrologic processes that are addressed. To improve clarity we now speak of the 'Mer Bleue peatland' when no specific area is addressed, and use 'Bog' and 'Forest' only in a site-specific way. In terms of the investigated area being a bog it has to be noted that the site has remained only a bog regarding hydrologic flow patterns and vegetation on the western side of the ditch. This can now more clearly be seen from the image and photographs in Figure 1. Due to the change in the flow patterns, and with establishment of trees, the eastern side cannot be classified as a bog anymore, yet it remains part of the larger "Mer Bleue peatland" system as shown in Figure 1. Discharge has been reported to occur in bogs during summer in several studies, including Mer Bleue, and has become known as a "flow reversal event" (Fraser, C.J., Roulet, N.T., Lafleur, M.: Groundwater flow patterns in a large peatland. J. Hydrol. 246: 142–154. DOI: 10.1016/S0022-1694(01)00362-6, 2001.). In our opinion occurrence of temporary flow reversals, as seen in this study, poses no problem with respect to classifying a peatland as a bog.

Specific comments

The current title does not mention climate change or future climates. A re-titled paper may better communicate the content.

Mentioning climate change may direct more attention to the paper. However, strictly speaking the paper describes what the title conveys. Some inferences regarding a drier climate are discussed, but in terms of the objectives and content we feel that the title is appropriate.

p. 34, line 3-4: The analogy needs to be properly posed. The analogue to future conditions is the drained condition not the "drainage ditch."

We changed the expression (L. 3).

p. 34, line 8: Here is the first instance in which the hydrological conditions don't appear to fit the definition of a bog. If groundwater is upwelling, these peatland areas would appear to be fens, not bogs.

We added a half-sentence that puts this unexpected finding into context (L.9-10).

p. 34, line 13: Specify the meaning of "partly."

We removed this half-sentence.

p.35, lines 20, 21, and elsewhere: Italics are needed when the genus and species are written. For example, Picea, Larix, and Sphagnum.

We checked the manuscript and changed the type of font where needed.

p. 36, line 2: Are lower rates of groundwater renewal projected for future climates in southeastern Ontario? Elsewhere, the authors invoke lower water tables due to changes in peat physical properties and evapotranspiration as the drivers of change after drainage, not changes to renewal, which presumably means reduced rain and snow fall.

To our knowledge no detailed estimates are available about future changes in groundwater renewal in southeastern Ontario. Regional climate projections suggest similar precipitation in summer as today. This prediction implies a lower rate of groundwater renewal, because evaporation is strongly linked to air temperature. To make this link we now refer to a paper by Schindler (1997) that describes the relationship between summer temperature and evapotranspiration (L. 70).

p. 37, line 3: “Supported” is vague and possibly an inappropriate word choice.

We rephrased the sentence (L.102-103).

p. 37, lines 1-2: Is it known that vegetation cover was different before drainage? How is it known that the drainage ditch affected vegetation. When was the peatland ditched and what is the age of dominant trees in the forest? Are trees ages mostly the same?

As we point out in the text the drainage ditch was dug in 1922. The palaeoecology of the transect has been described in detail in Talbot et al. (2010). We have no information about the average tree age, unfortunately. However, we added information, including a figure, about the historic development of the vegetation using pollen and peat stratigraphy (Figure 2) and refer to this information now in methods (L.146-147), results (L. 282-292), and discussion (L. 573-582). The available information shows that the density of trees, especially of Larix and Betula, strongly increased on the “forest side” after drainage and led to the current very large, 5 to 10fold differences in standing biomass between both sides of the transect. These differences can now also be seen more clearly in Figure 1, panel B.

p. 37, line 21: Replace “leave” with “leaf.”

We replaced the word.

p. 38, line 21: Consider deleting “from QE” which is redundant with the immediately following text.

We followed this suggestion.

p. 38, line 22: Provide information on the quantification of the latent heat of vaporization.

We added the latent heat of vaporization value (L_v) used (L.163).

p. 38, line 27: “Additionally” seems like a more appropriate word choice than “further.”

We replaced “further” by “additionally”.

p. 39, line 1 and elsewhere: Quotes around bog are not needed. See previous comments regarding peatland nomenclature.

In this case we respectfully disagree. We think that in this particular case the quotes are useful, as we do refer to a specific area called ‘Bog’ and ‘Forest’. The nomenclature has now been explicitly explained in the section “2.1 Study Site”.

p. 39, line 2: What criteria characterize a reference site?

A reference site here would be negligibly influenced by the hydraulic effects of the ditch. As the mentioned sites did not serve this purpose, we removed the expression.

p. 41, lines 11-18: More direct wording may better convey what was done.

We agree and changed the text accordingly (L.251-257).

p. 42, line 21: Should “equaled” be replaced with “were equal?”

We agree and replaced it.

p. 44, line 21: “pattern” is unnecessarily repeated.

We removed one “pattern”.

p. 44, lines 24-26: Runon and unclear sentence.

We rephrased the sentence for clarity.

p. 45, lines 7-9: The clauses of the sentence don’t seem to be related.

We related the clauses.

p. 46, lines 1-6: Runon sentence.

We separated the sentences and rephrased.

p. 46, line 20: Remove the extra space.

We removed it.

p. 48, line 21: Fix the spacing and dashes.

We fixed the problem.

p. 49, line 18: Why would new additions of plant material have any effect at depth? The new plant material could not have been deeply buried.

We rephrased the sentence and now refer to the roots of shrubs and trees instead.

p. 49, line 19: Instead of "of model behaviour," include the specific response variable.

We changed the sentence accordingly and now refer to hydraulic heads instead.

p. 50, line 6-7: The use of "vice versa" is not specific enough.

We rephrased the sentence.

p. 50, line 12: A grammar correction is needed, "ET was modeled."

We changed the grammar.

p. 50, line 20: Specify how the flow pattern was consistent. Over time?

This interpretation is correct. We specified the content.

p. 51, line 20: The growth of vascular plants needs to be documented somewhere in the document.

This omission was made up for, see above (Figure 1 and 2).

p. 51, line 21: The word "this" is a modifier. The dangling modifier needs to be addressed.

We checked the entire document for similar grammar problems and added a noun to the modifier.

p. 51, lines 26-27: The authors have not provided any evidence that hydraulic conductivities have increased due to drainage. They have only shown that hydraulic conductivities are spatially variable across the peatland.

This statement is technically correct. Our conclusion is only based on reasoning given the nature of undisturbed continental ombrotrophic bogs, the evidence for reasonable spatial hydraulic homogeneity of the peatland in a nearby area (Fraser et al., 2001), and the vegetation and water table history of the site the ombrotrophic system (that we have now added to the text). To accommodate the reviewer's concern about causation, we changed the wording here from "increased" to "higher".

p. 52, line 2-5: The phrase "we cannot support this idea by measurements of nutrient fluxes" needs to be fully explained.

We rephrased this sentence and now refer to specifics.

p. 52, lines 19-21: This sentence is an example of hydrological explanations that do not make sense in relation to the terminology. How can the treed area have inputs of groundwater and be a bog? This information also seems to contradict an earlier statement that implied that areas surrounding Mer Bleue were well drained and drained away from the bog (p. 38, lines 8-10). That information would indicate that there should be little or no connection of the catchment to the peatland which is a stark contrast to this sentence. These discrepancies need to be addressed by using terminology that is consistent with the ancillary information that is presented elsewhere in the document.

The reviewer is right that the terminology was misleading. "Catchment" here referred to the main bog west of the ditch, which drains towards the ditch. It does not refer to upland areas. We replaced "catchment" by "main body of the peatland".

The treed area is no longer a "bog", neither in terms of hydrology, nor in terms of micro-topography or vegetation community. With the new terminology that refers to the entire wetland as "Mer Bleue peatland", the open western side of transect as 'Bog', and the treed eastern side of the transect as 'Forest', the inaccuracy should be eliminated.

References: Fix the Baird et al. citation.

We checked the citation.

Tables 1 and 2 may be of secondary relevance and better placed in the supplemental materials.

As the tables do not take much space we left them in place. The reviewer referred to the contained data himself, when discussing the results and criticizing the conclusions, and even more so was the data discussed by Andy Baird.

Figure 1. The map and lines superimposed on the cross-section makes a somewhat cluttered image. I suggest separating these distinct informational images.

Here we cannot follow the reviewer's judgement. We asked a few colleagues and they found the image reasonably clear. We added another panel to the figure, however, to better illustrate the sampling design and the vegetation patterns in the area.

Figure 3. The lines of information above each figure panel are not well explained or readily understandable.

We added some more explanation to the Figure caption (now Figure 4).

Figure 5. None of the flowlines suggest bog hydrology. This figure is another example of terminology that is not consistent with the results. Additionally, the flowlines suggest inputs of water from outside the peatland, which again is not consistent with descriptions of watershed connectivity.

The eastern, treed area did not show flow patterns typical for bogs during the investigated period and cannot be considered being a bog any more, neither in terms of hydrology, nor micro-topography, or vegetation. The analysis of the vegetation patterns suggested, however, that also this area once was part of the bog.

The result of the transient simulation in panel (b,) is somewhat unrepresentative for the entire period as it illustrates (the interesting) time when a flow reversal occurred in the 'Bog' area at the beginning of September. The comparison with (now) Figure 3 shows that this was not the case much of the time, when the hydraulic gradient in the 'Bog' area was downward oriented.

As pointed out in the text, the west and east end of the modelling domain represent no-flow boundaries. We added this information also to the figure caption to make sure that the results do not seem inconsistent with other descriptions.

Supplemental information: What are hydrologic triangles? The text from lines 2-20 seems highly repetitious with information that is presented elsewhere. If there is some unique info written here, consider merging it into the body of the paper and deleting the mostly repetitious supplement.

We removed the expression "hydrologic triangles", which refers to the minimum hydrologic information (3 points in space) needed to obtain a local groundwater flow direction from hydraulic heads. We kept the other diagrams, though, as they provide complementary information about the occurrence of hydraulic gradients in the area.

Reviewer 2 (Andy Baird)

General comments

This paper describes a study of the sub-surface hydrology of two parts of a raised bog affected by a drainage ditch that has been in place for *c*. 90 years. The authors suggest that the drying caused by the ditch can be regarded as an analogue for a drying climate; thus, the results could give insights into how peatlands might respond to future climate change. Although interesting, I'm not sure such a argument is that convincing and would have liked a more detailed discussion of why a ditch might be expected to replicate some of the changes expected from climatic drying.

In the manuscript we have pointed out that drainage is not a strong analogue for a drier climate (L. 551-569). Unlike changes in the water balance, drainage does not act in a spatially uniform way and it also shows different temporal dynamics. Nevertheless, the impact of drier conditions on changes in hydraulic conductivity and vegetation, and associated changes in water table regime dynamics and groundwater flow patterns, can be studied. In the discussion we also point out that drier conditions will influence peatlands also through lower water table levels in the drainage network (e.g. beaverponds), which would act on the peatland in a similar way as artificial drainage by a large channel.

The authors investigated hydraulic heads and hydraulic conductivities (*K*) to assess how flow patterns and changes in peat properties might have differed in two parts of the bog either side of the ditch. Their data set is quite large and they have certainly obtained some useful measurements. However, I have two substantive concerns about the quality of the data and hence the authors' interpretation of their results. First, the authors don't seem to have taken account of the potential effect of peat smearing around piezometer intakes on estimates of peat *K*. There are published protocols that suggest that piezometers to be used for *K* tests should be (i) constructed with intakes that have high levels of perforation (50-70%) and (ii) 'developed' to remove smeared or low-*K* skins around the intake. As far as I can tell the authors have not used these protocols so I question the validity of their *K* data.

We considered 'development' of the piezometers as normal good practice. In the revised manuscript we point out that all piezometers were pumped empty at least once before any sampling took place (L.173-179). The determination of hydraulic conductivity by slug tests was preceded by emptying piezometers on at least three more occasions for water sampling (L. 196-207). We also added a new figure that shows the response of hydraulic head to two repeated slug tests (Supplemental Information, Figure 2S.). In one piezometer the second slug test resulted in a slightly faster response, in the other case slightly slower response.

Secondly, the authors don't appear to have taken proper account of differences in piezometer responsiveness when interpreting their head data. Differential responses can lead to apparent hydraulic gradients between instruments when none actually exist. Just because piezometers show a response to rainfall doesn't mean that the response is a faithful reflection of changes in the peat around the piezometer. Therefore, I am not convinced the head patterns they observed represent the real behaviour of the bog; the patterns may be measurement artifacts (in part, at least). I think some of the data are probably of good quality, including the piezometer data presented in Figure 2, but I'd like to know more about the data collected from other instruments (Figure 3).

The problem with different response times of piezometers, i.e. with differences between real hydraulic head and hydraulic head as reflected by the water table in the piezometer, does occur when heads rapidly change. This rapid change occurs after precipitation, as illustrated in the annotated simulation supplied in the comment.
The data presented in Figure 3 (now Figure 4) were obtained on 13/8, 2/9, and 4/10. Each time several days (5 days, 3 days, 4 days) had passed from the last precipitation event to the time when we measured hydraulic heads. This is illustrated by comparing these dates with the precipitation data in Figure 2 (now Figure 3). In the simulation supplied with the comment, 3 days were sufficient for equilibration using a piezometer in peat having a K_h of 3 x 10^{-8} m/s. The hydraulic conductivities in the piezometers nests that matter for documenting the difference between 'Forest' and 'Bog' had substantially higher K_h. These are the P9 and P10 in the forest which had K_h between 10-6 to 10^{-8} m/s (Table 2). The critical hydraulic gradients, for example on September 02, (P9, P10; now Figure 4) were visible across the whole depth gradient. The corresponding K_h of the 'Bog' sites were mostly similar or larger and there vertical hydraulic gradients were also very small. Considering response times and sampling dates it is thus very unlikely that the majority of hydraulic heads represents artifacts.

To address these concerns, however, we added sections in the results that caution the reader regarding the interpretation of gradients between layers *where K_h values are small or strongly differ with depth, such as the 15m sites in the 'Bog' and the underlying clay (L.341-347).*

Given these concerns, I don't think the paper is publishable in its current form. I'd recommend a substantive revision where greater attention is given to data quality, especially in the interpretations of flow directions within the peat.

We are grateful for this advice that helped improving the manuscript.

I have other, more minor, concerns about the paper, and these are articulated in the 'Detailed comments' section below. In that section I also elaborate on the more major concerns outlined above. Where a concern is more major, I have used a plum-coloured font.

Abstract, line 17: I don't follow/understand the sentence starting on this line with "When water...".

We rephrased this sentence (L. 19-21).

Page 34, line 25. This is an old estimate of C storage in northern peatlands. More recent estimate suggest a larger store (see, e.g., Yu *et al*. (2010), *GRL*, doi:10.1029/2010GL043584).

We replaced the estimates and citation.

Page 35, line 20. The plant genus names in the sentence starting on this line should be italicised. I don't know journal policy, but the adjectival case should, perhaps, also be hyphenated ("...*Sphagnum*-dominated bog...").

We italicised all genus and species names.

Page 35, line 23. I assume years are meant here. Is before present before 1950 (some hydrological readers may not know the dating convention implied here)?

We added "years" and "1950". In Glaser and colleagues' paper this convention is not defined but since they use ^{14}C carbon dating they must have followed it.

Page 36, line 22. " insight **into**..."?

We changed the word.

Page 37, line 16. "Sphagnum" should be italicised. This point applies to botanical names given elsewhere in the document.

See above.

Page 37, line 23. "leave" should read 'leaf'.

We changed the word.

Page 38, line 21. But surely one would expect *Et* to differ between the "Bog" and "Forest", so why use *Et* estimates from single station elsewhere on the bog?

We had only one micro-meteorological tower available for the ET calculations, which was also bound to the site described. For this reason the ET was measured in the bog and in the forest it was estimated by inverse modelling and comparison with literature data.

Page 39, line 5. How were the piezometers installed? Was the response times of the piezometers measured (see Hanschke, T. and Baird, A.J. 2001. Time-lag errors associated with the use of simple standpipe piezometers in wetland soils. *Wetlands* 21(3), 412-421). I'm a little concerned that some of the piezometers may have had slow response times and given misleading values leading to errors in the estimated hydraulic gradients.

We reviewed the literature again regarding this issue and added more information in the manuscript that better describe the installation (L.164-168, 196-207) and the interpretation of head response (L.283-292). In the Supplemental Information we added information regarding piezometer design and response; see also comments above.

Page 39, line 13. I don't follow what was done here. Slowly-responding piezometers tend to give damped responses. Also, in any pair of piezometers between which head differences are being measured, problems can occur if the instruments respond differently to changes in head in the peat around them.

We agree. See detailed response at the beginning of the revision notes.

Page 39, line 21. It is implied here that the piezometers measured K_h. Actually, piezometers such as those used by the authors measure an undefined mix of K_h and K_v as noted by Surridge *et al*. (2005, *HP*, doi: 10.1002/hyp.5653). No information is given on how the slug tests were conducted. If the intakes were screened with mosquito mesh, did that cause smearing of the peat around the intake during piezometer installation? Were the piezometers 'developed'? Was slug injection or slug withdrawal used? All of these are potentially-important considerations as noted by Baird *et al*. (2004, *HP*, doi: 10.1002/hyp.1375).

We agree that these aspects were not properly described in the last version of the manuscript. The piezometers were 'developed' as discussed above and now described in the manuscript (L. 175ff). We added the details about piezometer design in the manuscript (L.164-168) and images in the Supplemental Information (Figure S1). We also added a figure in the Supplemental Information showing piezometer response to a repeated slug test (Figure S2). Practically the piezometers were manually filled with a conventional watering-can and the water table decline immediately recorded.

Page 39, lines 23 and 24. *How is anisotropy defined here? As K_h/K_v? More information should be given. And is it reasonable to expect anisotropy of the peat in the areas affected by drainage to be the same as in the pristine area of the bog?*

We defined anisotropy as used in the paper by Fraser et al. (2001) and added this information. As we also point out it is likely that anisotropy was influenced, for example by enhanced decomposition, in the drained area. As we do not have information in what way it may have been different we conducted sensitivity analyses with MODFLOW to investigate this issue (see L. 263 ff.). Multiplication of anisotropy in the model had little impact on model performance in terms of hydraulic heads. The uncertainty in our choice of anisotropy, which was not constrained by actual measurements but based on the values from the nearby site investigated by Fraser et al. (2001) was thus likely of little consequence for the interpretations.

Page 40, line 2. *Given that piezometers can also be sampled for their gas, I suggest rewording as follows: 'Peat pore-water was sampled from the piezometers ...'.*

We changed the sentences accordingly.

Page 41, line 15. *I'm not quite sure what is meant by "calibration targets" here.*

As model calibration implies finding a good fit between measured and modelled hydraulic heads we removed this confusing expression.

Page 41, line 21. *Why was a paired t test used? The statistical design of the study is rather complicated. Piezometers either side of the ditch could be regarded as being independent of each other, which suggests that a two-sample t test for independent samples should have been used. And, rather than do several t tests, one for each depth, it might have been better if a two-way, repeated-measures, ANOVA had been used, with location ("Bog" and "Forest") as one factor and depth as a second factor, with depth being the factor with repeated measures (if piezometers at different depths in each bank were very close to each other).*

The rationale for using the simple, paired t-test is to examine whether at the same distance from the ditch hydraulic conductivities in a particular depth *cannot be considered equal on both sides of the transect. We considered the sites on both sides of the transect as not being independent of each other but instead as being exposed to a spatially directed, diminishing impact of the ditch (with growing distance from the ditch). Thus a paired t-test seems to make sense. To conduct an ANOVA would have been a possibility. However, the factor 'Bog' and 'Forest' is not a constant, due to the diminishing impact with distance from the ditch. It thus seems reasonable not to overstretch the statistical approach.*

Page 42, section 3.2. Some of the *K* values are low and may have been caused by smearing of peat around the piezometer intakes, leading to low rates of head recovery during slug tests. Low recovery rates also mean that head readings from some of the piezometers cannot be regarded as reliable. To illustrate this point I modelled a situation where two piezometers of the same design as that in the study were installed in peat. I assumed that one piezometer had an effective *K* of 3 □ 10^{-8} m s^{-1} and one an effective *K* of 3 □ 10^{-7} m s^{-1}. By 'effective *K*' I mean the *K* of the peat controlling the flow of water into or out of the piezometer. This could mean the smeared peat around a poorly-installed piezometer or just the naturally-low *K* around the instrument. I assumed a flow system in which there was no vertical head change and assumed water tables and hence heads at all depths increased by 10 cm over a 17-hour period in response to rainfall, after which they remained stable. I also assumed that, prior to the head increase, both piezometers were in equilibrium with the prevailing head (100 cm). The results are shown below, where the head is shown as a solid black line, the higher-*K* piezometer by a dashed blue line and the lower-*K* piezometer by a dashed red-brown line.
98.0100.0102.0104.0106.0108.0110.0112.00.00.51.01.52.02.53.0head or piezometer head reading (cm)time (days)
From the figure it can be seen that there is an apparent large head difference between the two piezometers, suggesting flow between the higher-*K* instrument and the lower-*K* instrument. However, the difference is entirely an artifact of the differential response time. That is why it is important to understand response time when interpreting head data from banks of piezometers. More on this matter can be found in Hanschke and Baird (2001) as previously referenced. The values of *K* used here are within the range reported by the authors, so I suspect similar artifacts are present in their data and need to be properly accounted for in a major revision of the paper.

We are grateful for this thorough analysis, which we have responded to above. We added reservations about the uncritical interpretation of gradients in the result section of the paper, see also earlier responses to your comments.

Page 43, line 22. I agree, but the piezometers shown in Figure 2 had similar K values ("Bog"), so would be expected to show similar types of response, or relatively high *K* values ("Forest") so one would expect similar responses. It would have been interesting to have seen results from piezometers such as B200-1.0 and F15-0.5 that had lower *K* values than used in my analysis above. I'm afraid I would not trust the data from those instruments because response times for these would have been of the order of days, not even hours. Overall, I'm unconvinced by the results in section 3.2.

We completely agree that this is a valid concern and that our assessment was not thorough enough. As we pointed out above, the hydraulic heads were examined several days after rainfall, which minimizes the chance of artifacts, also in comparison to the piezometer response simulated by you. As well, the critical gradients that document a difference in vertical flow between 'Bog' and 'Forest' (Forest P 8-10) occurred across depth profiles that had a similar, and not excessively low, K_f*. Regarding the other, more critical cases and locations, such as 'Bog' 15m (P4), we added a note of caution in the Results, as well as regarding the layers with very low hydraulic conductivity in the underlying clay.*

Page 43, line 23. Okay, but the piezometers concerned had relatively high *K* values. What was the situation with piezometers like B200-1.0, B15-1.0, and F15-0.5 that had low *K* values; how did they respond?

How they responded over time and immediately after rainfall we do not know since we have to rely on manual measurements carried out on the given dates. Indeed it would have been interesting to know more about their individual response, but we could not know, as we determined k_f *later and concentrated on not measuring head immediately after rainfall, when response is delayed. For the purpose of the study, however, the precautions taken and the data obtained seem adequate.*

Section 3.4 and Figure 3. Until the response-time error of the piezometers is properly addressed I don't think firm conclusions regarding flow directions and the magnitude of hydraulic gradients can be drawn from the piezometer data.

I hope we could alleviate the concerns in the revised version of the paper.

Page 45, line 10. Conjunctive adverbs like 'however' should be preceded by a longer pause/stop than a comma; I suggest a semi-colon or a full stop (period).

We changed the comma and checked elsewhere.

Page 46, line 5. I cannot agree. I'd like to see a comparison of data from higher-*K* piezometers and lower-*K* piezometers. I suspect the results will show something similar to my simple model results above – i.e., apparent hydraulic gradients that are better explained by differential piezometer responses to the same head change. Even slowly-responding piezometers can show a response to rainfall (changes in head) but that does not mean that they have registered the change in the system adequately (see the example of the lower-*K* piezometer in my analysis above).

> *We agree that the discussion of potential artifacts were inadequate. In addition to the information added and discussed above expanded this section (L 394-406) to address these concerns*.

Page 46, line 28. But were they significantly lower?

> *We did not do a test here, as differences in the shallow were quite large and appeared obvious.*

Page 47, line 11. Okay, but this is not known for sure. I guess much will depend on how the bog has developed over time in different places. I can imagine plausible scenarios where the profile was the same (as suggested by the authors) but also those where the profile was very different. Much depends on the developmental history of the bog.

> *We agree. To this end we added more information about the developmental history of the bog using pollen records and testate amoebae (now Figure 2) and the corresponding information in the Methods, Results and Discussion sections.*

Page 48, lines 12-21. This is plausible, but is there any evidence of the tree roots in the peat at the two depths on the "Forest" side?

> *The tree cover is dense in this section (see new Figure 1). Thus it is likely that tree roots contributed to the difference in hydraulic properties, as we discuss later. We have no hard evidence, however, how deep the roots of trees historically grew after drainage.*

Page 49, line 9. But Rosa and Larocque used piezometers with highly-perforated intakes and also undertook 'development' (Baird *et al*., 2004).after piezometer installation to help remove any low-permeability skins or smeared peat around the piezometer intake. However, it appears that the authors did not adopt these protocols for their tests, so I cannot agree with their statement that their results were as reliable as those of Rosa and Laroque.

> *We may not have spent as much effort on protocols as Rosa and Laroque but we have used good practice in terms of 'development' of piezometers, assessing response etc.. We hope we could convey this statement in this revision.*

Page 50, line 18. Vertical-upwards flow can be expected near a ditch, when water is discharging from a soil. Such upwards vertical flow doesn't necessarily imply recharge from an underlying substrate.

We agree. The discharge, however, does not only occur near the ditch but also deeper into the forest (Figure 4), and it is in agreement with chloride concentrations there as well (Figure 6).

Page 51, line 8. I think the authors are right to question the usefulness of long-term drainage as an analogue for a change to a drier climate, but this limitation is perhaps something that could have been foreseen. In any revision of the paper (see my overview), it may make sense to change the rationale of the paper and to remove the ditch-as-dry-climate analogue.

We have had both opinions from reviewers- it may be useful to remove it and it may be useful to strengthen this aspect (reviewer 1). We decided to keep it in the text with some additional explanation, such as asked for by reviewer 1. We pointed out in the manuscript that we have reservations ourself; however, it may be useful to draw the attention of the wider community to the outlined potentially diverging impacts of hydrologic change for ecohydrological functioning of these systems. To us the important point is not to overstretch the conclusions.

Reviewer 3

My review will be short, since my comments are in good agreement with the other reviewers.
The objectives are clear and the hypothesis pertinent, but I believe that using so little real data (only three sampling dates of water heads, one transect only, simple bulk density and somehow imprecise in situ saturated hydraulic conductivities) to make a very precise simulation is not the most appropriate approach to demonstrate, with great confidence, the validity of the hypothesis.

See comment by Andy Baird – he referred to the data being adequate. We agree that the data base could have been more comprehensive if it had not been obtained in an intensive summer campaign while first and last author were based in Germany. Yet we also believe that together with previous information presented in Talbot et al (2010) and Blodau and Siems (2012), as well as the improvements that we made in the manuscript, a good case is made that drainage caused vegetation, and ecohydrological processes and in consequence groundwater flow patterns to change. We also think that that it is clear that both sides of the ditch very likely strongly differed in *their response, which highlights the need to further investigate thresholds where such responses occur.*

Bulk density and porosity taken in the field were possibly the most relevant information concerning the physical change of the peatland following drainage, but the real data is only very shortly described in the text.

The bulk density data have only little influence on groundwater hydrology, which is controlled by hydraulic conductivities and water balances, and thus we only briefly mention the data here. Other changes in the peat, in particular its decomposition degree, have been addressed in a previous manuscript by Blodau and Siems (2012).

The in situ hydraulic conductivities were not determined using the most appropriate methodology, thus leaving some reasonable doubts about the validity of the data in the mind of attentive readers.

The hydraulic conductivities were determined by appropriate methodology, they were apparently just not presented in a way that made this clear, since we assumed that our protocols are normal good practice. We have extensively responded to the detailed and insightful comments by Andy Baird regarding this issue and significantly improved the way the methodology and its limitations are presented.

Water heads were also measured with a strange protocol since piezometers were emptied a day before sampling. This procedure was appropriate for water sampling protocols, but inappropriate for water head measurement in very low K soils. It is very probable that some piezometers would have equilibrated within 24 hours while some others might not. Differences in heads for two piezometers could therefore be due to protocol artifacts.

This view is a misunderstanding. At all times, first hydraulic heads were measured and subsequently, on three occasions, piezometers were pumped empty and water samples obtained the following day. We added information in the methods section that clarify this procedure. Hydraulic heads were only ever measured at least 6 to 10 days after emptying out the piezometers and slug tests were carried out at the end of the measurement period in order not to interfere with other measurements, such as the determination of hydraulic heads. We now point this out in the manuscript to avoid such misunderstandings.

More links should be made between the occupancy of the neighboring of piezometer nests by vegetation. A simple Google Earth visit of the site, guided by the Fig.1 map, shows that it is not so evident to see a drastic change in vegetation between the bog and the forest and that the vegetation in the forest can be very variable spatially.

We are thankful for this comment. Clear changes are only visible in the northern end of the ditch where the cut-off eastern section is reasonably small. Further south it makes sense that the differences are smaller and more variable because there both sides of the transect are more similar in width. To clarify the nature of the transect we added a new overview image where these differences and also the nature of the vegetation community are clearly visible (Figure 1, panel (b,))

The authors should try to reformulate the objectives and hypothesis to aim the flow simulation part, since the information gathered in the field is good enough to enable a pertinent simulation, if the uncertainties of values are fully documented and considered.

All reviewers had differing views on what the focus of the manuscript should be. We regard the flow simulation only as a tool to better understand the differences in flow patterns and potential reasons for the ecohydrological change that we observe in the area. The flow simulations do have their own limitations and we believe that the way they are used now is appropriate regarding the objectives. We did, however, change the last sentence of the introduction that refers to the modelling exercise.

On behalf of all co-authors I would like to thank all reviewers for their thorough criticism of the science and writing and their suggestions that certainly helped to improve the manuscript.

Best regards

Christian Blodau