Biogeosciences Discussions

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Reply on RC1 & RC2

Cindy Quik et al.

Author comment on "Faded landscape: unravelling peat initiation and lateral expansion at one of northwest Europe's largest bog remnants" by Cindy Quik et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-162-AC2, 2022

Dear Reviewers,

Thank you very much for the detailed attention paid to our manuscript and for the constructive comments. We think they will be very useful to improve the paper. Please find our reply to each of the points raised in italics below.

RC1: 'Comment on bg-2022-162', Anonymous Referee #1, 29 Sep 2022
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1. Overall, the paper is well written, uses a sound and well detailed methodology, and the results are overall presented nicely through a set of clearly understandable tables and figures.

Thank you.

2. The paper can be further improved taking into account the following comments, suggestions and questions, but none of these are really critical.

Thank you for these valuable suggestions, please find our reply to each below.

3. First of all, the paper lacks a short but clear overview of the variability in peatland types in Europe and which peatland type is considered in this study. You can read this a bit throughout the manuscript but a few sentences on this in the introduction could help. Also use this terminology consistently. (fens, bogs, mires, blanket peatlands, alluvial peatlands, ...). On line 74 the authors mention that limited attention has been paid to the palaeogeographical development of former extensive peatland landscapes in NW Europe. But, which peatland types are then considered? Does this also include e.g. the extensive peatlands in the Rhine-Meuse delta (coastal marshes), the large bogs and blanked peatlands on elevated areas in e.g. the Ardennes (Belgium) or in Scotland, the large mires in Alpine settings, ... ? This statement is too vague and I have the impression that the authors are in fact referring with their study to a more specific type of peatland in the whole array of European peatlands. This needs some better framing.

Peatland terminology can often be confusing and several terms are used interchangeably (see e.g. Rydin & Jeglum, 2013: Section 1.4.5 A word of caution, in: The Biology of Peatlands, Oxford University Press, p. 13).

Fochteloërveen does not fit within a single definition as it probably formed through coalescence of multiple smaller mires (see Figure 6 in the manuscript) that formed on a non-coastal and non-alluvial topographic plain. However, in the hydromorphological classification (cf. Charman, 2002: Peat and peatlands, in: Peatlands and Environmental Change, John Wiley & Sons Ltd Chichester, p. 7-15), the resultant composite peatland can probably best be described as a plateau-raised bog.
Fochteloërveen started off as a fen (minerogenous mire), but later on transitioned to a bog (ombrotrophic mire) (see Quik et al., 2022: Dating basal peat: The geochronology of peat initiation revisited, Quaternary Geochronology 72:1–22, DOI: 10.1016/j.quageo.2022.101278).
In line 74, it indeed does not become entirely clear to which peat landscape we are referring to. We propose to include the explanation above in the Introduction to clarify this, and to expand the respective sentence as follows: "So far, limited attention has been paid to the palaeogeographical development of the former extensive peat landscapes of the Northwest European mainland (Vos et al., 2020; Casparie, 1993), with the exception of coastal and alluvial peatlands of the Rhine-Meuse delta.". Please note that our statement does not refer to blanket peatlands on the British Isles, as we refer only to the NW-European mainland. We also propose to include information on the former and current vegetation type of Fochteloërveen in Section 2.1, in response to comment 17 of Reviewer 2 with the request to include more information on vegetation.

4. Also, note that the definition of peatlands having at least 30 cm of peat is one definition but others exist. For instance, the FAO soil classification scheme uses a minimum thickness of 40 cm for a histosol. Also, there is no clear definition of what peat exactly is - e.g. the minimum amount of organic matter needed or minimum thickness). It is only in the methods section that we learn that in this study a minimum % OM of 40% is used to define a peat layer. This could come earlier. What would be the impact of on the results when the definition of peat is used differently? this is especially important when results from this study are compared to other studies in peat initiation ages that potentially use different threshold levels of OM content. A short discussion on this would be nice.

We are aware that definitions of peatlands regarding the thickness of the peat layer may deviate. As stated in the Introduction, we follow the definition of 30 cm of peat, in accordance with the International Mire Conservation Group (Joosten & Clarke, 2002).
In our study we indeed used an OM of 40% or more to define the basal peat layer. This is based on thorough study of the OM gradient in peat cores from Fochteloërveen (see Quik et al., 2022: Dating basal peat: The geochronology of peat initiation revisited, Quaternary Geochronology 72:1–22, DOI: 10.1016/j.quageo.2022.101278) and in line with the Dutch soil classification system (De Bakker & Schelling, 1966: Systeem Van Bodemclassificatie Voor Nederland: De Hogere Niveaus, Stichting voor Bodemkartering, Pudoc: Wageningen, 217 pages). We believe this fits best in the Methods section.
We propose to include a note in the Discussion regarding the use of this OM percentage to define peat and related implications for comparison with other studies.

5. The distinction being made between peat initiation at the landscape and local scale (lines 64-68): is this the authors own working definition (then do mention this explicitly) or more generally accepted/used (in that case, a reference is missing).

The distinction between peat initiation at the local and landscape scale was proposed in the study by Quik et al. (Dating basal peat: The geochronology of peat initiation revisited, Quaternary Geochronology 72:1–22, DOI: 10.1016/j.quageo.2022.101278). Reference to this study was included in Figure 1, but not in the text in lines 64 – 68. We propose to add the reference there as well.

6. The authors could make the need for this particular case-study much clearer if some

existing info on the peat bog would be summarised better, e.g. in a small infographic/figure. In section 2.2 several hypothesis on peatland initiation and development by Zagwijn, Vos, Waterbolk and Fokkens are briefly discussed but this info could be summarised in a figure showing the time range suggested by the four previous studies during which the peat bog was developing. This would much better indicate the variability in existing theories and thus the need to better constrain the age of the peat bog initiation and lateral expansion.

We very much like this suggestion and propose to include a schematic figure that summarises existing knowledge on the peat remnant. In contrast to Reviewer 1, Reviewer 2 states that the entire Section 2.2 should be deleted (see comment 16). We are convinced that this section provides important information on the case study and therefore belief this section should remain, as is also substantiated by the suggestion of Reviewer 1 to expand the section with a schematic figure explaining the variability in existing theories.

7. Also, the data on archaeological finds could be added as a prior knowledge. It is also adviced to add a bit more info on the archaeological finds on the maps. These are not shown at all on figure 2 yet discussed in section 2.1. It is only in the results section on figure 7 that the location of the finds is shown, yet without details and the reader has to find it out by looking back to the description of the study area.

We propose to include the locations of the archaeological finds in Figure 2(d) so that their location is clear earlier on. We also propose to include the 'Concise description' that is listed for each find in Table 2 behind the location names in Figure 7(a), to prevent the necessity to look back to Table 2.

8. In tables and in the text, elevation values are presented in m O.D. but no details on the OD are provided except for the caption in figure 2 (it should be mentioned in the main text where O.D. is first used). Also, make clear it is mean sea level in which location ? I assume Amsterdam (NAP ?).

We have indeed provided information on the OD in the caption of Figure 2. Here it is mentioned that the OD used is the Dutch Ordnance Datum which is roughly equal to mean sea level. The Dutch Ordnance Datum is indeed NAP. We propose to move this explanation to Section 2.1, so that it is clear from the beginning of the Study Area section onwards.

9. There is an elaborate explanation in section 3.2.1 on how the covariates are calculated but I wonder whether a graphical schematic presentation showing the Pleistocene surface, the bog surface (remnant + before cutting), the present hydraulic head and the assumed original hydraulic head. This would make it much clearer to the reader how zp, H and zpH should be interpreted in relation to the peat bog surface.

This is a valuable suggestion. We propose to include a schematic figure that explains the different variables as Reviewer 1 suggests.

10. Related to this: the authors make some assumption on the shape of the original hydraulic head but do not discuss whether this is a valid assumption nor whether other assumptions on the spatial pattern in zpH would yield different results. The current hydraulic head is used for calculating zpH but is the current head not biased as only part of the peat bog remains - thus, the current topographic variability (and spatial pattern in hydraulic head) does not represent the topographic variability before peat initiation ? This is also partly touched upon on lines 365-370 to explain deviating results where the area is now forested but the uncertainty on the palaeohydraulic heads could be discussed more. It can also be considered to model palaeohydrology using the Pleistocene surface and early Holocene climate conditions to better constrain the hydraulic conditions leading to

peat initiation.

This is indeed discussed briefly in lines 365 – 369, but also more elaborately in lines 546 – 560. In lines 573 – 575 we offer the same suggestion as Reviewer 1 mentions in this comment: to use hydrological modelling to derive the natural isohypse pattern of the landscape, which could subsequently be used within the presented digital soil mapping approach.

11. A linear relation between peat initiation ages and covariates is examined, including peat thickness. Why are non-linear relations not considered ? Several studies have shown that peat growth is indeed linear at the start but then tapers off when an equilibrium height is obtained: growing peat bogs may lead to extra drainage as the bog raises higher above the surrounding landscape and hence, peat growth rates goes down. (see e.g. Morris et al 2015 GRL, Yu et al 2009 Sensitivity of northern peatland carbon dynamics; Swinnen et al 2021 Biogeosciences).

Please find our combined response to comments 11, 12 and 14 at comment 14.

12. Overall, the paper does not really discuss the various factors/processes that control peat initiation and development which is to be learned from other studies, both field and numerical process-based modelling studies. This could be added to both the introduction and the discussion section. In the conclusions section, the authors state on line 592 that geomorphic position in the landscape is of great importance. Is this not what could be expected based on previous studies and general knowledge on the processes governing peat initiation and peat growth rates ? A more in depth-comparison of the main results with other studies would improve the discussion and conclusions section.

Please find our combined response to comments 11, 12 and 14 at comment 14.

13. Also in the conclusions section, it is again mentioned that lateral expansion is taking place at a higher rate between 5500 and 3500 cal yr bp but why ? what is the hypothesis for this acceleration ?

In our study we aimed to find explanatory variables within a digital soil mapping approach that would allow us to reconstruct the pattern of peat initiation and lateral expansion within (and potentially beyond) peat remnants, and to reconstruct peat initiation ages and lateral expansion for the Fochteloërveen as case study area. As such, providing an explanation for the observed spatiotemporal pattern falls outside the current study scope. However, we do provide a hypothesis for the observed accelerated pace of lateral expansion between 5,500 – 3,500 cal y BP, which is discussed in the Discussion in lines 486 – 492. The phase of accelerated lateral expansion at Fochteloërveen appears to fit in a large-scale trend of peatland expansion in Northern Europe and North America that is associated with Neoglacial cooling (see references in the respective lines in the manuscript).

14. Finally, the authors do follow a purely statistical approach. This method can be replicated but does require for each study again a large database on peat initiation ages, original topography and hydraulic data. Are there alternatives to reconstruct the palaeoextent of the peat bog and its development through time using numerical peatland models such as digibog or other types of models ? Could the authors reflect on this a bit more ?

In our analyses we sticked with linear relationships as the amount of data (radiocarbon dates) is fairly low to apply methods that involve more assumptions. We found linear regression to be the best option as it involves only a few assumptions, which were valid for our dataset (see lines 309 – 312 and 515 – 519 in the manuscript). Also, the data

points that we used in our linear regression analyses are all basal peat dates (see lines 255 – 258 and Section 3.2.3); i.e. stemming from the linear phase of peat growth at the start of formation of a peat layer.
In the Introduction we discuss three approaches for reconstructing spatio-temporal peatland dynamics (see lines 79 – 90 in the manuscript). Unfortunately, so far no numerical models seem to be available that can model the lateral dimension of peat growth (see line 90, and the reference included there to the discussion on peat models by Baird et al., 2012). In lines 573 – 575 we propose to use hydrological modelling to derive the natural isohypse pattern of the landscape, which could then be used in the current digital soil mapping approach. If in the future numerical peatland models would be expanded with functionalities to model lateral expansion, the digital soil mapping approach that we proposed in this study could potentially be used for validating numerical modelling outputs.
In our approach the choice and construction of explanatory variables is based on process understanding (see lines 117 – 138 and lines 286 – 290). Subsequently these explanatory variables are used within a statistical approach. In the Discussion in lines 538 – 545 we reflect on this. We propose to slightly modify this paragraph in the Discussion to accentuate the combination of process-informed choices within a statistical approach.

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15. The paper is generally solid and provide new results about the history of the peatland, showing that the oldest peat is much older than earlier investigations have indicated. However, the paper can be improved and streamlined to make it more informative and easy to read.

Thank you for the useful suggestions, please find our response to each suggestion below.

16. General structure - much of the text is general background about the peatlands, which is already familiar to the potential readers of the paper such as this. For example, the introduction is too long because such basic facts like the idea for the formation of peat and the processes for peatland initiation are described. These can be deleted or at least shortened. Similarly, streamlining is needed to avoid unnecessary repetition. For example, on pages 5-6 the results of earlier studies are explained and this is repeated in Discussion on lines 473-485. All in all, the short review on pages 5-6 "Peatland development and decline in the (wider) study area" seems out of place and could be deleted.

In our view, the information included in the Introduction concisely explains concepts that are key for general understanding of the manuscript. This opinion is substantiated by comment 3 of Reviewer 1, where this Reviewer states that more background information should be added, explaining more on peatland types in NW-Europe. We propose to follow the suggestion of Reviewer 1 by briefly adding information to the Introduction regarding relevant peatland types (also see our response to comment 3).
Regarding the section "Peatland development and decline in the (wider) study area" we are surprised that Reviewer 2 suggests that this section should be removed. We belief this section provides significant information on our case study, and highlights the variability in existing theories on peat initiation and lateral expansion at Fochteloërveen. Reviewer 1 suggests to add a schematic figure to this section which summarises the existing theories, to emphasise the need to better constrain spatio-temporal development of Fochteloërveen. We propose to add a schematic figure as Reviewer 1 suggests (see our response to comment 6) and are convinced this will help to demonstrate the relevance of the section.
In lines 473 – 485 in the Discussion, we emphasize only the differences between our findings and the existing theories, without repeating non-relevant details of those theories. We belief these differences are important to stress, as they place the

development of Fochteloërveen in a new light.

17. Study area - it is surprising that the peatland is not described at all, apart from stating its size on page 6. It would be important to describe the study site so that the readers would have at least a rough idea about its current topography, vegetation, hydrology and different types and intensity of human impact.

In section 2.1 the study area is described, including brief geological background of the area, hydrological situation of the Fochteloërveen (which is part of three catchments), and current climatic conditions. We propose to add information on the peatland's former and current vegetation type (also see response to comment 3) and present-day human impact. Historical human impacts are already described in the last paragraph of Section 2.2.

18. Results - dating is a critical part in a study such as this, so the dating results must be presented better and more in detail. It is necessary to show the calibrated dates in Table 3. In contrast, Table 1, showing the dated material is unnecessary, and could be either relocated in the supplement or the information about the dated material can be shown in Table 3.

Dating results are indeed a key part of our paper. We have included details and dating results of our samples in Table 1 and 3, following recommendations by Millard, 2014 (Conventions for reporting radiocarbon determinations, Radiocarbon 56-2:555-559, DOI: 10.2458/56.17455). According to this paper, listing the dated material is key for accurate reporting of radiocarbon dates. We therefore decided not to move this information to a supplement. We propose to add the calibrated dating results to an extra column in Table 3 as a two-sigma age range expressed in cal y BP, to meet the suggestion of Reviewer 2.

19. Chronological terminology needs to be revised. It is surprising that the authors use the old terms such as "Atlantic" or "Subatlantic". While these terms have been used in the past, they are not valid any more. One can check the official subdivision of the Holocene epoch from the International Chronostratigraphic Chart. It is important that the scientists follow the names of these officially defined units, to avoid confusion.

The 'old' terms are used only in Section 2.2 where existing knowledge on peatland development in the (wider) study area is discussed. In the cited publications, the 'old' terminology is used, but for clarity (and potential comparison with new terminology) we have also indicated the ages in cal y BP each time the 'old' terminology was quoted from these publications. We propose to add a sentence at the beginning of Section 2.2 explaining that we cite the 'old' terminology from the cited papers, and that cal y BP ages were added to ease interpretation and comparison with the new terminology.

20. Chronological expressions need to be more consistent. Now both "cal y BP" and "BCE" or "CE" are used, which is confusing. The "BCE" and "CE" ages should be indicated as calibrated radiocarbon ages to make them comparable with the rest of the paper.

Throughout the manuscript we used the cal y BP notation, and only used BCE/CE notation for the archaeological finds that are used in the validation. To ease comparison with the cal y BP ages, we propose to add a column to Table 2 in which the ages of the archaeological finds are converted to cal y BP.

21. Figures – the number of the figures is too high in relation to their information content. Figs. 1 and 3 are simple, if not naïve and could be relocated in the supplement. Figs 6 and 7 can be combined to a one figure with a, b and c panes.

We have considered the suggestions of Reviewer 2 regarding figures 1, 3, 6 and 7,

however:

- *The conceptual Figure 1 highlights the difficulties with reconstructing peat initiation and lateral expansion in the case of peat remnants. As such, it provides significant visual support to our Introduction.*
- *The cross sections included in Figure 3 illustrate our Methods and show the stratigraphical context in which samples for radiocarbon dating were selected. The radiocarbon dates play a key role in the manuscript.*
- *Both Figure 6 and 7 are currently one-page figures, where Figure 6 focuses on the modelling results and Figure 7 on the validity of these results. In our opinion multi-page figures are best prevented for reasons of legibility. As the figures also have a slightly different theme, we believe they should remain separated.*

Due to the reasons outlined above, and as Reviewer 1 states that "the results are overall presented nicely through a set of clearly understandable tables and figures" (comment 1), we are convinced that the Figures 1 and 3 should not be moved to a supplement and that Figures 6 and 7 should remain as is.

22. Table 1 – as stated, this in unnecessary as a separate table.

For reasons of legibility, we believe this table should remain separate from Table 3 as was suggested by Reviewer 2 in comment 18. To comply with the conventions for reporting radiocarbon dates (Millard, 2014: Conventions for reporting radiocarbon determinations, Radiocarbon 56-2:555-559, DOI: 10.2458/56.17455) we decided not to move this table to a supplement.

23. Table 2 does not seem to bring any additional information to the study.

We are surprised that Reviewer 2 finds that Table 2 does not bring any additional information, as it lists details on the archaeological finds that were used in the validation which are not mentioned elsewhere in the manuscript. Reviewer 1 however seems to find this Table informative (see comment 7 and our reply there).