Biogeosciences Discussions

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Comment on bg-2022-162

Anonymous Referee #1

Referee 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-RC1, 2022

The manuscript on 'faded landscape: unravelling peat initiation and lateral expansion ...' by Quick et al is a detailed case-study on of the larger peat bogs in northwestern Europe that is largely gone due to historical peat cuttings. It combines a dataset on radiocarbon based peat initiation ages from the peat remnants with a statistical analysis using local environmental parameters to reconstruct the former peat extent and temporal development of this bog. The analysis shows that the initiation of the peat bog is older than often considered thus providing new info which can be important locally eg for heritage. The developped methodological framework, though very much linked to this specific case study, can also be applied to other sites and can thus be of interest to a wider scientific community.

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.

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

* 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 palaeogeogrpahical development of former extensive peatland landscapes in NW Europe. But, which peatland types are then considered ? Does this also include eg the extensive peatlands in the Rhine-Meuse delta (coastal marshes), the large bogs and blanked peatlands on elevated areas in eg 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.
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 - eg the miniumum 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.
* 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 authors could make the need for this particular case-study much clearer if some existing info on the peat bog would be summarised better, eg 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 developping. 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.
* 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 seciton 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.
* 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 ?).
* 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.
* 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 discusse more. It can also be considered to model palaeohydrology using the pleastocene surface and early holocene climate conditions to better constrain the hydraulic conditions leading to peat initiation.
* 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 of 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 eg Morris et al 2015 GRL, Yu et al 2009 Sensitivity of northern peatland carbon dynamics; Swinnen et al 2021 Biogeosciences).
* 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 dicussion 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.
* 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 ?
* finally, the authors do follow a purely statisticial 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 ?