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## Reply on CC2

Angelica Feurdean et al.

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Author comment on "Holocene wildfire regimes in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types" by Angelica Feurdean et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-125-AC4>, 2021

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*Community comment 4: Irina Kurina: The manuscript "Holocene wildfire regimes in forested peatlands in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types" by Angelica Feurdean et coauthors is of great interest and raises a very relevant topic for scientific research. This research is based on a variety of evidence, it is used a multi-proxy approach to the detailed palaeoenvironmental reconstruction. Age-depth models for the studied peat cores as a necessary basis for work looks very strong. I think also that the study area related to the southern part of Western Siberia, on the one hand, is poorly studied and, on the other hand, this is the one of the most suitable places in Eurasia for such research because of the widespread occurrence of peatlands. Although the design of the manuscript (so called paperwork) leaves much to be desired. And the interpretation of the data obtained raises many questions. Therefore below I provide my comments (questions) to the manuscript.*

R: Thank you for the valuable comments that helped to improve the current version of the paper.

*Line 58 – I think it is unclear and you should explain how climate-disturbance-fire feedbacks do affect overall future resilience of forested peatlands to climate change.* R: This is stated in the previous sentence, which reads: 'However, dry conditions, particularly a water table decline below the threshold of 20 cm, will probably exacerbate the frequency and severity of wildfire, disrupt conifers' successional pathway, and accelerate shifts towards more fire-adapted broadleaf tree cover'. We do not consider that this needs further explanation in the Abstract. Details can be found in the Discussion.

- *Line 175 – I think this is incorrect heading for paragraph. You cannot apply directly reconstructed water table depth based on testate amoeba data to climate reconstruction without any explanations. In fact, testate amoebae are protists, which inhabit the upper layer of cover in peatlands. They indicate local conditions of surface wetness in a peatland. And the level of this locality is very small, which means that there are only a few centimeters around. Based on testate amoeba data from only one place (microsite) in a peatland we cannot judge surely even about the degree of waterlogging for this peatland of itself, in general. Moreover we cannot say anything about climate (hydroclimate). Before using the reconstructed DWT for climate reconstruction, you should show the clear connection between the two.*

R: We agree that the link between the water table and climate needs to be further demonstrated, but the evidence provided by our study is sufficient to suggest that dry peatland conditions are likely to burn often and /or severely. In revising our paper, we are extending the explanation on testate amoeba hydrological reconstruction slightly, but the full record of testate amoeba (TA) assemblages as well as the hydrological reconstruction and interpretation at the four sites will be published extensively in a future publication (Diaconu et al., in prep). TA assemblages record onsite (very local) changes in peatland hydrology whereas macrocharcoal in peatlands record local fire occurrence, generally strongly influenced by the local peat moisture (Blyakharchuk and Kurina, 2021; Magnan et al., 2012; Feurdean et al., 2020). Thus, although not identical, the spatial scale of the two proxies is rather local. The water table in peatlands is driven by the interplay between precipitation and evapotranspiration and that a substantial amount of water is lost via evapotranspiration, which is largely controlled by air temperature (Swindles et al., 2019; Diaconu et al., 2018; Pleskot et al., 2021). If the peatland type is oligotrophic or mezzotrophic (isolated or partially isolated from other water sources except for precipitations) then peatland moisture is a direct reflection of the hydroclimate conditions. Even when a peatland is dependent on the discharge of a water source, the quantity of the discharge is still dependent on the climatological conditions, which is indirectly reflected in the DWT. The good agreement between the high fire activity, low water table, and regional high summer temperature reconstructions, makes us suggest the existence of a link between fire, peatland hydrology, and summer temperatures (see similar findings from literature (Turetski et al., 2014; Kettridge et al., 2015). However, in revising this paper we will use peatland moisture fluctuations to define conditions reconstructed from testate amoeba.

*Lines 53-58 – you express your suggestion about possible future changes in forested peatlands in according to climate changes in the Abstract section. Next, Lines 411-417 – you mention about this suggestion in Conclusion section, but I have not found anything about this idea along the Result and Discussion sections of your manuscript. It is incorrect, when new thoughts appear in the Conclusion section, if they are not discussed before in the Discussion section or they are not mentioned in the Result section of the manuscript. I think it would be better and logical to expect moving this suggestion from the Conclusion to the Discussion section and adding the references to the published scenarios of climate changes in the future, that you mention in Line 54 and Line 412 of your manuscript.*

R: These are predictions for potential future forest-fire interactions as a response to future climate changes based on what we have learned from our palaeoecological data. We do not consider that this information is unfounded, but rather it follows logical findings from our long-term records. We, therefore, decided to keep these here.

- *Line 35 – you write in the Abstract "... despite their huge extent in boreal regions". This is one of the key messages that emphasizes the importance and relevance of your research. Please add some phrases about this in the Introduction section and show the facts (figures), what is the area covered by forested peatlands in Eurasia, in West Siberia and/or in the southern border of taiga zone in western Siberia. You can take this information from different published papers and books (as examples, 1) Vompersky et al. 2011 in Contemporary Problems of Ecology; 2) Kremenetski et al. 2003 in QSR; 3) Liss et al. 2001 monograph Wetland systems of Western Siberia and their conservation value – in Russian; 4) Alekseeva et al. 2015 in Bulletin of Tomsk Polytechnic University – in Russian).*

R: Thank you, we have added a sentence acknowledging this. It reads: "Despite that Siberia contains a large extent of forested peatlands, particularly its western part

(Vompersky et al., 1994; Liss et al. 2001; Kirpotin et al., 2021) no studies have explicitly explored the interactions between peatland moisture, vegetation composition, and fire regime in this region”.

- *Sorry, it is difficult for me to understand the key idea of your research. So I read the introduction of your manuscript. Lines 60-75 – you write about wildfires in forests. Next, lines 76-89 – you write about wildfires and peatlands. Next, line 90 – you write about fires in boreal ecosystems. Next, lines 91-94 – you write about forest ecosystems. Next, lines 95-96 – you write about peatland. Then, lines 97-101 – you express the aim of your research, but it is unclear if you are aimed to study forest or forested peatlands. You have said nothing about this. Although, in fact, forest and forested peatland are not the same. They differ. It is wrong to consider forests and forested peatlands as one and the same. Please explain what exactly you are studying – forests or forested peatlands. Along the manuscript the terms of forest and forested peatland are not separated. Especially in the title and in Conclusion section you declare the research of forested peatlands, although in the Result and Discussion sections you said firstly about forests, secondly about forested peatlands and thirdly about summary of forest and forested peatlands taken together (as one) without separation. As the result, the confusion between these three different things leads to the misunderstanding of the research and the interpretation of the data obtained.*

R: We started the introduction by acknowledging the role of fire in boreal forests generally (l.60-75). We moved to recognise that a lot of boreal forests grow on peatlands and that the relationship between such forests and fire is less known (l. 76-89). We then introduced the usefulness of palaeoecological research to capture past changes in fire regime in forest ecosystems (90-91). It is not relevant at this point whether a forest occurs or not on peatland, but that an understanding of the dynamic of a forest ecosystem, which contains species that live decades to centuries, needs long-term records. Finally, the primary aim of our study is to look at forested peatlands. We believe that the introduction follows a logical path, however, in revising the paper we will make it clear throughout the other parts of the paper that we accurately use the terms forests and forested peatlands.

- *Line 170 – you write **"To determine the regional changes in forest composition, we created composite records of PFTs"**. Could you explain, how do you determine the regional changes in forested peatland composition? How do you separate the composition of forests and forested peatlands? Based on my individual experience, I cannot imagine that tree composition is the same in a forested peatland and in forests, which surround it. In most cases they are different. As confirmation, let us look at the description of modern conditions in the studied peatlands (Lines 114-117): "The local vegetation at both coring sites includes mesotrophic open sedge-Sphagnum communities with young birch trees at Rybnaya Mire and standing dead tree trunks at Ulukh-Chayakh Mire". Then if we compare this with the upper samples from pollen diagrams related to the studied mires (Figs. S4a and S4b), we can see the other picture. Both the pollen diagrams show the great abundance of arboreal pollen, belonging mainly to *Betula* and *Pinus sylvestris* taxa. We can conclude, that, based on pollen data, there are forests, consisting of birch and Scots pine, but the studied mires are open, however you mention some birch trees on mire surface. It means that the composition of trees differs in forests and in forested peatlands. And pollen diagrams reflect the summary composition of forests around and of the tree cover in forested peatlands. Someone cannot separate in pollen diagram, where is the tree composition of forests and where is the tree composition of forested peatland*  
R: We believe there is a misunderstanding of this sentence. It refers to results using the combined pollen records from the four peatlands (see their location on Fig 1) so that in the end we obtained a regional picture of forest composition and dynamics. Each peatland we cored is forested and composed of tree species mentioned at 2.1 (please note a slight modification to accommodate Loyko's suggestion). The vegetation at the coring points

was indeed slightly open as we have chosen this approach to make our coring easier, but otherwise, these peatlands are forested. The principle of pollen analysis is that it records species composition on a scale ranging from local to regional (Bennet and Willis, 2001). Given the large extent of forested peatlands we cored, we assume that the bulk of our pollen diagram indicates the tree composition on these forested peatlands. It is also highly probable that an unknown proportion of the pollen rain comes from outside peatlands. Thus, although it is not possible to separate the pollen originating from forested peatlands from pollen coming outside of the peatlands, in revising this manuscript, we will be more careful in describing the past forest composition and dynamics regarding the location of these forests and in linking the local variability in peatland hydrology (local) with forest composition (representative of the larger area likely with more variable moisture conditions).

- *Line 320 – you used Ti concentration as “possible indicator of water influx”. Could you provide any reference to the researches that confirm this idea? I am surprised to see such interpretation of the Titanium peaks. As far as I can consider from different papers (as example, Kempter and Frenzel 2008 in Science of the Total Environment; Margalef et al. 2014 in Palaeogeography, Palaeoclimatology, Palaeoecology; Hutchinson et al. 2016 in Regional Environmental Change – you cited the last reference) Ti is mainly precipitated from atmosphere. Its increasing peaks in a peat core (or lake sediments) can be caused by wind or soil erosion, by enhanced precipitation, or by increased production of the ecosystem. There are many reasons for positive peaks of Ti, but I never heard about river flood as the reason. If we look in your manuscript in Line 320 you write “The detrital element Ti, a possible indicator of water influx, was high in the bottom profiles that were rich in minerogenic material.”. Then, in Lines 326-327 you write “Proxy records from Siberia attest to warmer and drier-than-present climate conditions between 9 to 6 ka ... (Groisman et al. 2012)”. The age of bottom profiles in the studied mires with high peaks of Ti is about 8.5-7.0 ka (I take it from place Rybnaya mire, Fig. S5). So it coincided with period of drier climate conditions in Siberia. I think that flood events should be happen if precipitation increases, but precipitation was reduced at the period. How can you explain this discrepancy? I can imagine that this period of drier climate conditions might contribute to frequent fires, deforestation and enhanced soil erosion. This is just my opinion, but I think this is more reliable explanation for the Ti peaks, than river flood, that you suggested.*

R: Thank you for this comment which indicates that we need to clarify our explanation of the geochemical features of the peat profile. The higher Ti content in the basal, minerogenic portion of Rybnaya and, in fact, also at UC is not connected to floods but reflects the minerogenic substrate at the sites and therefore pre-dates the inception of the peat. We refer to subsequent (higher in the profile) Ti fluctuations as indicating possible flood events i.e., low frequency but high magnitude events which may not reflect the overall climatic trend at the time. As a lithogenic (or geogenic) indicator Ti can be seen as an indicator of detrital input reflecting the mineralogical content (in comparison to the highly organic nature most peat profiles). The source of this material will reflect both the type of mire and the events leading to this input. In an ombrotrophic context the input will be aeolian. Here the landscape position of the site means that (at this stage in the mire) a fluvial input (reflecting possible flooding or channel position change) is feasible as a transport mechanism for the delivery of such material and associated lithogenic signal (also seen in other lithogenic indicators but Ti has been selected as indicative).

- *Line 181 – you used the transfer function developed for the pan-European region (Amesbury et al. 2016) to derive the water depth from the studied peat cores. This transfer function was developed mainly (or even especially) for ombrotrophic and oligotrophic peatlands, but you applied this to the mesotrophic mires. I think this might increase the incorrectness of the reconstructed values of DWT in your study. Why did you not used the transfer function developed for Asian peatlands (Qin et al. 2021 in*

QSR), because the studied mires are located in Asia, but not in Europe? Apart from that, I can say that the transfer function developed for Asian peatlands includes more places with higher values of pH and therefore, I guess, it might be more suitable for reconstruction of DWT in the studied mires. Also I suggest adding testate amoeba diagrams from the studied peat cores to the Supplementary Materials of your manuscript. It is very important and interesting data. Furthermore, it would be very helpful to show the efficiency of the transfer function in your peat cores. There are standard statistic indexes (the chi square distance of fossil testate amoeba assemblage to the closest modern analogue from transfer function training set; goodness-of-fit statistic; the number of rare taxa and the number of absent taxa) indicating to what extent the fossil testate amoeba complexes in the cores correspond to the testate amoeba complexes embedded in the transfer function. We should avoid the situations when the half of taxa from fossil testate amoeba assemblages are absent in the taxon list of the transfer function and really do not contribute to the water depth reconstruction

R: The study of Qui et al. (2021) was not available at the time of running our quantitative DTW reconstructions and assembling our datasets for this paper. However, the pan European transfer function (TF) is not limited to European sites but includes peatlands from Siberia and the sites having a large PH variation. Although, Qui et al. (2021) states that the Asian TF performs similarly to other large-scale TF, a grouping of some taxa were less representative for our records than in Amesbury et al (2016). A full comparison of the Siberian profiles using both training sets, representative diagrams for each site, and climate-based reconstruction are in prep for publication Diaconu et al. In the revision manuscript, we will therefore retain our reconstructions but add a few sentences on the numerical performance of transfer function.

*Lines 328-329 – you write "Warm summer temperatures likely enhanced evapotranspiration and consequently lowered peatland water levels, leading to drier surface conditions". You explain this for the period of "a temperature and moisture optimum between 6 and 4.5 ka BP (Groisman et al. 2012)" in Siberia. Although, if we look at the pollen diagrams from the studied mires (especially at Rybnaya mire Fig. S4a), we can see the increase of conifer pollen (Pinus sylvestris, P. sibirica) at this period and the decrease of Betula pollen. We can consider that conifers spread when precipitation exceeds evaporation (a prerequisite for the existence of the taiga). How can you explain this discrepancy when likely conifer trees indicate increased moisture (precipitation exceeds evaporation), but testate amoeba based DWT in mire indicate low levels (evaporation exceeds precipitation)? In general, I can conclude it looks very strange that reconstructed water levels in your peat cores are not coincided directly to the climate changes, which you take from the monograph by Groisman et al. (2012) for Siberia. I think this is an additional argument that the reconstructed DWT values from the studied peat cores do not reflect hydroclimate changes of the study area.*

R: The temporal succession in the main tree taxa at our sites is in good agreement with other pollen records from western Siberia. There is also a good agreement between our and the few other charcoal records from the region in indicating a high fire activity between 8-4.5/ 5 ka. Wildfires occur predominantly during the growing season (spring to autumn) but are most severe in summer associated with dry soils or peatland conditions. Peatland conditions must have been dry at this time (8-4.5 ka), to allow such high fires to occur, and lower local moisture conditions are also what TA show. In revising this paper, we will look more into agreements/disagreements between vegetation composition, TA hydrological conditions, and regional Siberia climatic conditions.

*Along the manuscript you compare the forest composition and the DWT values reconstructed for mire. I think this does not make sense because DWT is related to mire condition, although the forest composition is related to forests (not to mire).*

R: Forest density and composition respond to changes in peatland hydrology. What is true, however, is that local hydrological conditions vary across peatland and this, in turn, is influencing the forest composition. In revising this manuscript, we will more be cautious in linking the local variability in peatland hydrology (local) with forest composition (representative of the larger area likely with more variable moisture conditions). Please see also our response at point 4.

*Line 107 – when describing the "typical forests" for the study region you list tree composition of forested peatlands mainly and cite the researches "Berezin et al. 2014; Rybina et al. 2014", where peatlands (not forests) were studied. It is very strange to call these trees as light taiga, because in reality it is not forests, it is mire. I consider forested mire should not be called taiga. And why do you mention nothing about poplar, because it is one of the most abundant tree in forests from the study area?*

R: Thank you for this observation, please also see our response to Loyko, we will fix this in the revised version of the paper.

- *Line 71 – you cite the work by Agee (1998), but I have not found this work in the Reference List. Please add this reference.*

R: Added

- *Line 70 – Why does *Betula pubescens* belong to the group of invaders and to the group of endurers. In paper by Wirth (2005) the group of endurers includes *B. pubescens* from only northern taiga. Therefore it is incorrect to say that *B. pubescens* is endurer in your*

R: The fact that *B pubescens* can behave endurer only in northern taiga does not come out in Wirth 2005 However, we only mentioned its possibility to behave as endurer did not say this is the case of the study region.

- *Line 555 - the title of the work is written twice in the link.*

R: Thank you, duplicate removed.

- *Line 502 – in this reference 2020a is pointed after author's names. But in the next reference (Line 506) 2020b is pointed in the end of link or probably this is a part of DOI. What is the right variant of design? I think this detail should be uniform. If you have 2020a, then you have 2020b.*

R: The year of publication should be placed at the end of each citation. We have corrected this, thank you.

- *Line 298 – Rudaya and coauthors in their paper (2020) studied two lakes from the Steppe Altai. Indeed, this is Altai, but it is not a mountainous region. Their study area is related to the southern part of West Siberian Plain (lowland). Please correct this phrase in your manuscript. By the way (Line 95) you cite this paper again as example of research conducted in Siberian boreal forests. It is incorrect, because steppe zone is not boreal.*

R: Thank you, corrected in both locations.