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Comment on cp-2021-125

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Community 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-CC2, 2021

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.

- 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.
- 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.
- 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.

- 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).
- 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.
- 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 peatlands.
- 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 enchanced 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 enchanced 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.

- 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.
- Lines 328-329 – you write "Warm summer temperatures likely enchanced 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.
- 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).
- 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?

- Line 71 – you cite the work by Agee (1998), but I have not found this work in the Reference List. Please add this reference.
- 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 research.
- Line 555 - the title of the work is written twice in the link.
- 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.

17. 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.