Frankfurt am Main, 27.04.2022

Dear Editor, Nathalie Combourieu Nebout

Please find enclosed a revised version of the manuscript entitled ”Holocene wildfire regimes in peatlands in western Siberia: interaction between peatland moisture conditions and the composition of plant functional composition”.

We have addressed the major comments made by the reviewers, which include

1. running the Asian transfer function for testate amoeba and presenting the testate amoeba percentages into SI (File S5),
2. focussing on the relationship between forest composition and fire in section 4.3,
3. correcting any geographical misplacement of our comparisons with published records,
4. moderating the references to forested peatlands, including the removal of this term from the abstract

While we feel that these amendments were justified and have improved our manuscript, we consider now that we have reached the point where further revisions would be futile a fundamental difference in scientific opinion. Consequently, we ask that, as the Editor for our submission, you make a balanced judgement of this final resubmission.

Kind regards

Angelica Feurdean on behalf of all the co-authors

Reviewer 2

There are 3 spelling errors on the citations of the publications of Barhoumi et al., 2019 and 2021: line 331, line 863, it is not "Bourhami", but "Barhoumi"

R: Thank you we have corrected this.

Reviewer Kurina.

I have read the revised version of the manuscript cp-2021-125 “Holocene wildfire regimes in forested peatlands in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types” by A. Feurdean and coauthors. Unfortunately, I have not seen the essential corrections through the text made after the first cycle of review and comments that I expected to see. On the contrary, only some of the most disputable sentences that reviewers drew attention to were rewritten to smooth out sharp corners, after which these phrases became even more vague and uncertain, but the essence did not change despite the recommendations of the reviewers. For now as reviewer I decided to give the authors one more chance to correct the manuscript by making major, not minor, revisions. Now I list only global comments on the text of the revised manuscript. Although in addition I have found a series of minor errors and inaccuracies, which I will not mention now, because I think that after the recommended global rewriting of the manuscript, these inaccuracies may be removed occasionally and (or) new inaccuracies will appear in other places of the text. I am going to list minor errors only after major rewriting of the manuscript in according with my global comments. I repeat again, to my mind, this research contains valuable relevant precise data that are worthy to be published, although I disagree with interpretation of the results in this research. Therefore my global comments are aimed at improving the logical structure of the article and thoughtful interpretation of the data obtained in order to fit it objectively into the results from earlier publications about the study area.

R: Although we agree with some of the points raised by the reviewer Irina Kurina, and have further revised the manuscript (see details below), we also feel that these comments reflect a lack of willingness to accept our interpretation and outline of the manuscript. This perspective contrasts with the other three reviewers who have all accepted the manuscript.

1) I fundamentally insist on a change of the heading of the manuscript and data interpretation through the whole text. My main disagreement is that, in fact, authors do not investigate forested peatlands. They investigate vegetation cover (mainly tree composition) in boreal ecosystems taken together (without separating forested peatlands) in the selected study area. To reconstruct the history of vegetation cover at a long-term scale the authors take peat cores from two peatlands. This does not mean that they investigate peatlands, but this means

that they used a peatland as a natural archive for extraction the palaeoenvironmental information. By the way, the authors said this idea by themselves in the Introduction section (see Line 98). Furthermore, to reconstruct the history of vegetation cover the authors apply pollen analysis, which reflects regional vegetation in general, including both forests and peatlands. The only possibility for the reconstruction of vegetation directly in the peatland, I consider through the analysis of plant macrofossils in peat as it was performed in the research by Magnan et al. (2012) (this paper is referred in the manuscript). The authors did not analyze the plant macrofossils, so they cannot reconstruct the vegetation in the peatland, but only the vegetation of the region as a whole, and they cannot separate objectively between pollen from forest and pollen from forested peatland. I emphasize the importance of this comment on the example of the research by Mikhailova et al. (2021) (this paper is referred in the manuscript), in which, for the same peat sequence, obvious differences between the results of the plant macrofossil analysis and the pollen analysis were demonstrated.
In the revised manuscript (lines 439-440) the authors postulate that most of the tree pollen deposited on the surface of the studied peatlands comes from trees in the peatlands, because forested peatlands are widespread in West Siberia. I principally disagree with this statement. In the previous comments Irina Kurina asked the authors to display the definite figures of the area, occupying by peatland ecosystems, which are available from the recommended publications. As a result the authors added the references to these publications, but they have not displayed the certain figures of the area with peatlands. Therefore I point these figures here, based on the following papers (Vompersky et al. 2011 in Contemporary Problems of Ecology; Kremenetski et al. 2003 in QSR; Liss et al. 2001 monograph Wetland systems of Western Siberia and their conservation value – in Russian; Alekseeva et al. 2015 in Bulletin of Tomsk Polytechnic University – in Russian). In the southern taiga of West Siberia forests occupy 50% of the area, peatlands occupy 30%, including forested peatlands (15%) and open peatlands (15%). It means that the area of forests (50%) more than three times greater than the area of forested peatlands. It follows from this that tree pollen from forests prevails in general pollen rain, rather than tree pollen from forested peatlands. Thus, again I strongly recommend to add into the manuscript the certain figures about area occupied by peatlands, both forested and open, and by forests (for comparison) in the study region of the southern taiga of West Siberia. And I urge the authors to correct their assumption (lines 439-440) that most arboreal pollen in their spectra is derived with trees from forested peatlands. The figures show that this assumption is wrong or at least is doubtful.
R: We have removed the words 'forested peatland' from the title and in most places throughout the revised text. While we are aware that our pollen data records the mixed composition of trees on the peatland and surrounding forest, we disagree that our reconstruction merely used peatland archives to reconstruct vegetation. Firstly, pollen gives a regional picture of vegetation, but the spatial scale of the reconstruction is strongly influenced by tree cover and canopy density. Given the forested landscapes surrounding the study sites (see Fig.1), it is likely that that pollen from the immediate neighbourhood of the coring sites dominate the sequences. Secondly, the percentages of forests, forested and non-forested peatlands mentioned by the reviewer refer to western Siberia, whereas Fig. 1 shows that at the study sites. The combination of the two points supports that a significant part of the reconstructed tree cover comes from forests on the peatlands. We have added the numbers requested and revised lines 119-122.

2) The authors pointed water table depth of 20 cm as threshold determining correlation between peatland surface moisture and severity of the fires (see lines 46 and 347). I want to note that this value in itself (water table depth equal to 20 cm) does not carry ecological meaning. Because this figure is a result of calculations made by transfer function, which is based on a training set used one-off measurements of water table depth made during field work in different mires in different periods of the growing season (different months). Moreover this value of reconstructed water table depth is obtained for the peat sample, including a mixture of testate amoebae for at least 1-2 dozens of years (it depends on time resolution for the peat core), not for a one year. Therefore logically it is not correct to compare this value with modern one-off or mean annual values of water table in peatlands. And please note that mean annual measurements now are made for sparse single peatlands. Thus, I want to emphasize that the results of testate amoeba-based water table reconstruction are used to show the dynamics of water table changes. For example, based on these values we can say that surface wetness in the peatland becomes dryer or wetter, but we should not focus on definite values like water table changed from 20 to 10 cm. I foresee that in other peatlands the other individual thresholds will be obtained, for example, 10 cm or 25 cm or any other, while in this research the threshold of 20 cm was calculated. Therefore I recommend removing the value 20 cm from the Abstract (line 46). Instead, for Abstract, the authors might say that they revealed the statistically significant or quantitative relationship between fire severity and peatland moisture.

R: We have removed the threshold values in water table depth and the statistical correlation of absolute water table depth and charcoal (GLM model) from the entire manuscript and figures (Fig. 6 c,d). In the light of new results for the DTW values, we have replaced the absolute values (cm) with standardized values (see lines 200-205; 282-290).

3) Because the authors have no data on the composition of trees exactly in the forested peatlands, it makes no sense to compare directly tree composition and peatland moisture. Therefore, please, remove such comparison through the text of the manuscript. I consider the arboreal pollen data in the research mainly reflects tree composition in surrounding forests, although water table in the forested peatlands differs from water table in forest ecosystems.4) The authors are aimed to study the interactions between climate, fires, tree composition of forested peatlands and peatland moisture. I will comment this logical sequence in detail: Climate. In fact, the authors did not reconstruct regional climate changes and did not make an attempt to fit their research to the available information about regional climate from the earlier publications (as example, Borisova et al. 2011; Groisman et al. 2013 – referred in the manuscript). And this looks strange, because the authors received the pollen data and might apply this to climate reconstruction, at least, as qualitative description, not quantitative estimation of climate parameters. Moreover, please, note that available regional palaeoclimate information are based on pollen data (Borisova et al. 2011; Groisman et al. 2013). If the authors do not reconstruct regional palaeoclimate using the pollen data, they should at least to compare their pollen diagrams obtained with the other earlier published pollen diagrams from the study region. It is especially important, because possible similarity with other pollen records will allow concluding that pollen data surely reflect a common regional signal of vegetation succession and regional palaeoclimate changes. I strongly recommend to do this and to discuss the results of this comparison in the Discussion section of the manuscript. Furthermore, to my mind, your pollen records from the two studied peatlands are too different to be combined into one composite sequence. This also applies to data on the reconstructed water table depth and even to the fire dynamics. I think, it is better to consider the data from the two studied peatlands separately and to highlight their dissimilarity between each other, rather than similarity. I suppose that two studied sites (Rybnaya and Ulukh-Chayakh) related to different climatic regions (subregions), despite they are 200 km apart. In fact, if the authors compare their pollen records with the available pollen records from the studied area, they will notice that pollen record from the Rybnaya peatland is more similar with pollen records located north-westward in the southern taiga of West Siberia (please see the pollen diagram from Bugristoe mire in Blyakharchuk and Sulerzhitsky 1999; pollen diagram called Tom-river-mouth in review by Zang and Feng 2018). In these diagrams, note the clear positive peak of Abies pollen (or at least other dark coniferous pollen) in the Early Holocene (between 7.0-4.5 ka BP). The other pollen record from the Ulukh-Chayakh peatland is more similar to pollen diagrams taken southwards and south-eastwards (as examples, diagram from the Kirek lake and Chaginskoe peat in Zhang and Feng 2018; Teguldetskoye peatland in Blyakharchuk 2012; Zhukovskoe peat in Borisova et al. 2011; Pinchinskoye mire in Mikhailova et al. 2021). In these pollen records, there is clear positive peak of Abies pollen in the Mid Holocene (between 4.0-2.5 ka BP), although the peak of Abies pollen is absent between 6.0-5.0 ka BP. I recommend the authors to read the papers by T. Blyakharchuk and other palynologists and to pay attention to the principles of climate reconstruction based on pollen data. You can see that for the southern boundary of taiga belt in West Siberia an expansion of boreal forests to the south is limited mainly by humidity of the climate, rather than by temperature, unlike for the northern boundary of taiga belt. It follows from this that increase of precipitation amount causes movement of southern taiga to the south, while decrease of precipitation leads to the retreat of the taiga to the north. In the pollen record taken near the southern boundary of taiga belt this pattern appears as increase of dark coniferous tree pollen (considered as greater climate humidity) alternating with increase of Betula pollen (considered as less climate humidity). This means that increase of dark coniferous tree pollen indicates greater climate humidity. To my mind, peaks of dark coniferous tree pollen are not synchronous in your two records from Rybnaya and from Ulukh-Chayakh. This difference highlights the difference in local climatic conditions at these two places. Also pay attention to the dynamics of flooding events in the Holocene demonstrated in the research by Mikhailova et al. 2021 (this work is referred in the manuscript, see Fig. 9 there in). You can see that great floodings (considered as reflection of increased climate humidity) were observed at different periods in the Holocene for the records from Siberian taiga and from more southern forest-steppe. Fires. I would underline a similarity between fire severity and trees composition revealed by pollen data. At the Rybnaya record most severe fires were observed at 7.5-6.0 ka and at 4.5 ka BP, in the both these periods severe fires correspond with increasing of dark coniferous tree pollen (Picea, Abies, Pinus sibirica); while at the Ulukh-Chayakh place severe fires were documented before 6.0 ka and at 3.5 ka BP, in the first period this may corresponds with increased abundance of Picea pollen (not yours, but regional pollen data from earlier publications), in the second period fire peak corresponds with increase of Picea and Abies pollen in the Ulukh-Chayakh record. Pay more attention to the research by Barhoumi et al. (2021) (this is referred in the manuscript). The authors found a correspondence of fire dynamics in this research at the Baikal Lake region to the own research. Barhoumi and coauthors claimed they expected to find more severe fires at drier climate conditions, rather than at wetter climate. Although as a result they concluded that the severity of fires in the Holocene is apparently related to the tree composition of forests (more exactly the crown structure of different trees), rather than to climate conditions. This conclusion was mainly based on the fact, that in the Early Holocene they reconstructed the most moisture palaeoclimate, coincided with increase of dark coniferous tree pollen and with *period of severe crown fires. So I recommend the authors of the manuscript to make similar conclusions that clearly follow from the results of the research: the fire severity is more influenced by the tree composition of the forest than by climatic conditions. Moreover, the authors can enhance these conclusions if they follow the next logic sequence: increase of climate humidity leads to the spread of dark coniferous trees, which facilitate, in turn, more severe (crown, not surface) fires. Tree composition. I have explained already that the authors cannot consider separately tree composition in forested peatlands, although they can discuss noteworthy interactions between tree composition of boreal ecosystems taken together and fire severity.*

R: 4.3 focuses now on the relationship between PFTs and fire regime (most of the feature highlighted by the reviewer have already been addressed), with only a slight touch on the possible relationship between the water table and dominant PFTs. It is beyond the aims of this study to provide a pollen-based climate reconstruction or large -scale synthesis of pollen diagrams. Most (none) of the suggested pollen diagrams have no charcoal counted; thus, an extensive comparison between various pollen records will not strengthen the fire-vegetation relationship but merely provide similarities/divergences between vegetation/forest composition at a large scale, which is outside the scope of our study. However, we have included the recommended references. (Please see 4.3). It should be noted that composite pollen, charcoal, and water-table have been widely used to obtain regional pictures of vegetation, fire, and water table level beyond local trends.

Peatland moisture. In addition to the recommendations above, I propose to strengthen the main conclusions with data on the moisture on the peatland surface. I have mentioned already that peatlands (both forested and open) occupy 30% of the area in the southern taiga of West Siberia. It might be concluded that in one case when the peatland moisture increases (in this research the reconstructed water table depth less than 20 cm), peatlands do not burn themselves and prevent the spread of fires in surrounding forests. Thus peatlands localize and stop fires, which in most cases are initiated in forests (not in peatlands). In other case when the peatland moisture decreases (in this research the reconstructed water table depth exceeds 20 cm), fires may attack peatlands and burn them. Thus, in the contrary, peatlands become fire spreaders

R: 4.2.1 The influence of peatland moisture on fuel type and flammability We have removed the threshold values in water table depth. We have also strengthened the link between peatland moisture and probability of fire (l. 347-353).

In the revised manuscript the authors made an attempt to compare the results of water table depth reconstructions, based on testate amoeba data. I think this comparison is extremely incorrect. Lines 377-378 – the authors mentioned drier period between 7.5 and 5.5 ka BP and referred to Kurina et al. (2018) and Kurina et al. (2021). I insist on clarification that in the research by Kurina et al. (2018) aggregated (combined from different records) drier period was observed between 7.0 and 6.0 ka BP. I have not found the reference Kurina et al. (2021), although there is reference of Mikhailova et al. (2021), where the drier period was revealed between 7.5 and 5.1 ka BP. BUT this research is not related to the taiga zone of West Siberia. This is forest-steppe zone of East Siberia. Line 374 – the authors wrote that the research by Mikhailova et al. (2021) was conducted in West Siberia. Again, this is wrong. This work is related to East Siberia. Lines 385-387 – the authors registered a similarity of their research with other investigations from West Siberia. Here they referred to Mikhailova et al. 2021 and to Blyakharchuk and Kurina (2021). Again, this is wrong assumption. I have said already about research by Mikhailova et al. (2021). Moreover, the research by Blyakharchuk and Kurina (2021) is carried out in southern Central Siberia (not West Siberia) and in the mountain region (not the part of West Siberian Plain). Next, for these two records the authors mentioned similar wet period at 5.1-1.4 ka BP. This period is represented completely only in the work by Mikhailova et al. (2021), while the record by Blyakharchuk and Kurina (2021) covers only the last 2500 years, so it cannot represent adequately a peatland moisture between 5.1 and 1.4 ka BP. I consider, all the mentioned regions (southern taiga of West Siberia, forest steppe of East Siberia and mountains of southern Central Siberia) have specific regional climate conditions. I suppose that all the coincidences between these records are probably occasional. In addition, to my mind, these records are as similar as they are dissimilar. In other words the similarity between these records is subjective and doubtful. In general, I tend to expect, with an increase in climate humidity, the surface wetness of peatlands also increases, to which testate amoebae response; and vice versa. In quantitative reconstructions, it is customary to calculate the water table depth according to testate amoeba data, although in reality testate amoebae respond to surface wetness, rather than to water table depth in itself. Based on this, it is not clear to me, why in the Rybnaya record for the period at 7.5-4.5 ka BP drier conditions on peatland surface were reconstructed, while the pollen record demonstrated increase of dark coniferous tree pollen indicating more humid climate. In the Ulukh-Chayakh place the picture is even more complicated. As example, in the period at 5.0-2.5 ka BP pollen data displayed an increase of dark coniferous tree pollen (considered as more humid climate conditions), while the reconstructed water table depth reflected drier conditions on peatland surface between 5.0 and 3.5 ka BP and then wetter conditions between 3.5 and 2.5 ka BP. To my mind, this looks very strange and unclear. Unfortunately, the authors inflexibly refuse *to show the initial data on the distribution of testate amoeba taxa in their peat sequences, on the basis of which the reconstruction of the water table depth was made. So, as reviewer, I have to blindly appreciate how correctly the authors calculated water table depth in their peat sequences. In this case, I have no reason to confirm the correctness of the calculations. Moreover unexpected discrepancy between pollen and testate amoeba data makes me suspect possible errors in the calculations of water table depth. Now I cannot judge surely. Therefore I have to insist that the authors show additional evidence of the correctness of the calculations of the water table depth in the peat sequences.*

R: There are two independent transfer functions (TF) showing similar trends in hydrological conditions at all four sites. The Asian TF includes calibration sites from Siberia. In File SI 5, you can view the results of the two TF and the complete list of testate amoeba percentages. We do apologize for misplacing some of the TA records; this was corrected in the revised paper. The timing and directions of the hydrological shifts were accurately presented. Please note that Mikhailova et al. (2021) paper also presents a synthesis of palaeohydrological records from the southern part of western Siberia to which we made our comparisons. Generally, there seems to be divergent trends in moisture conditions reflected by pollen versus other hydrological proxy for the 8-4.5 ka.

For reconstruction of water table, the authors use the pan-European transfer function (Amesbury et al. 2016), which is based on training set from the European area. Not, it is important; this training set does not include samples from Siberia territory. Among experts on testate amoebae, the question remains whether it is correct to apply the transfer function developed for one region to a peat sequence from another region. Therefore the testate amoeba specialists recommend using the transfer function from the same region. Fortunately, for the area you are studying, several different transfer functions have been developed: pan-Asian transfer function by Qin et al. (2021) and three local transfer functions by Kurina and Li (2019) developed from ombrotrophic, minerotrophic and combined mixture of mires. I urge the authors to reconstruct the water table depth in their peat sequences using the different transfer functions proposed and to demonstrate the results from different transfer functions are similar with the results from the pan-European transfer function. I consider it is incorrect to state that if Willis et al. in their work (2015) applied four transfer functions from other regions to the Siberian peat sequences (because at that time they did not find a transfer function covering the Siberian territory!) and obtained similar reconstruction results, then the authors will get same way. An additional complication is that, in contrast to the Willis et al.'study (2015), which applied transfer functions developed for raised bogs to peat sequences from raised bogs, the authors in this study use a pan-European transfer function developed for ombrotrophic peatlands for peat sequences from mesotrophic peatlands. Meanwhile the research by Kurina et al. (2020) evidenced that the application of the transfer function for ombrotrophic peatlands to peat sequences including minerotrophic peat can lead to significant distortions of the reconstructed water table depth compared to the results of reconstruction using the transfer function for minerotrophic peatlands. Do not say that other transfer functions are not available for you. The truth is that they are available. You can email the authors of the pan-Asian transfer function and they help you to make reconstruction. All necessary data on the three transfer functions by Kurina and Li (2019) are free and uploaded at Mendeley DataBase. You can find it using the combination of the author name (Kurina) and key words (testate amoebae) and apply to your peat sequences by yourselves. I know that the calculation of water table depth is easy and fast (it will take no more than one working day from you). In addition, I ask the authors to represent in the manuscript or in the supplementary materials the certain figures of summarized relative abundance by those testate amoeba taxa from each peat sample, that were really involved into the calculation of water table depth, i.e. these taxa are present in the training set of the transfer function. These figures will help to reveal the transfer function, which mostly corresponds with your peat sequences. And representation of these figures will be more objective argument to confirm the suitability of transfer function to peat record, rather than only saying this is suitable and this is not suitable without any evidence as the authors did after the first cycle of review-

Corects wets Siberia !L374 this sentence does not placed Michaolve study in wetsner Siberia but say tata Lines 377-378 – the authors mentioned drier period between 7.5 and 5.5 ka BP and referred to Kurina et al. (2018) and Kurina et al. (2021). I insist on clarification that in the research by Kurina et al. (2018) aggregated (combined from different records) drier period was observed between 7.0 and 6.0 ka BP. I have not found the reference Kurina et al. (2021), although there is reference of Mikhailova et al. (2021), where the drier period was revealed between 7.5 and 5.1 ka BP. BUT this research is not related to the taiga zone of West Siberia. This is forest-steppe zone of East SiberiaLine 374 – the authors wrote that the research by Mikhailova et al. (2021) was conducted in West Siberia. Again, this is wrong. This work is related to East SiberiaLines 385-387 – the authors registered a similarity of their research with other investigations from West Siberia.

R: We have run two independent transfer functions, one developed for the pan-European region (Amesbury et al., 2016) and the other for Asia (Qin et al., 2021). The Asian TF includes calibration sites from Siberia. Following literature recommendations (Amesbury et al., 2016; Swindles et al., 2016; 2019; Qin et al., 2021), we standardised the reconstructed water-table depth values to Z scores. The two independent transfer functions (TF) show similar trends in hydrological conditions at all four sites; therefore, this revision does not impact our previously major interpretation of the hydrological conditions In File SI 5, one can view the results of the two TFs and the complete list of testate amoeba percentages (see more see l. 200-204, l. 282-290) and apply as many TFs as pleased.