Climate of the Past Discussions

Clim. Past Discuss., author comment AC1
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Reply on RC1

Jack Longman et al.

Author comment on "Carbon accumulation rates of Holocene peatlands in central–eastern Europe document the driving role of human impact over the past 4000 years" by Jack Longman et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-65-AC1, 2021

In this manuscript Longman et al., present eight new carbon accumulation records for peatlands in the Carpathian mountains and combine this with the one existing published record. Not only is this a highly novel paper due to the location and type of peatland analysed it is also an excellent contribution to the question of the drivers of long-term carbon accumulation in peatlands. Longman et al., have done an admirable job of untangling the complex drivers of peatland carbon accumulation both in their own data and in the earlier literature. The difference in dominant drivers between the early and mid-to-late Holocene is highly convincing and matches similar trends seen elsewhere for which others have thus far failed to find such a satisfactory explanation.

We appreciate the reviewer's comments, and have attempted to respond to all all remarks and suggestions in the document below. Our responses are highlighted in italics.

I find the main message of this paper quite convincing, that message being that long term peatland C can be controlled by either climate or nutrient factors and that indeed a combination of these two drivers are important. Although I am myself convinced of the direct role dust and nutrients can have on CAR, if I am to be the 'devils advocate' the authors may wish to consider and rule out the linkage between dust and/or mineral input and climate itself which presents a conundrum as to which is the direct or dominant driver of CAR. Dust inputs into peatlands have in themselves been used to reconstruct climate, with the expectation that increasing aridity results in more dust (e.g. https://doi.org/10.1016/j.epsl.2009.03.013). This raises the question about how independent dust inputs and climate really are. In the case of my own work in New Zealand it was quite easy to discount climate warming or aridity as we would not expect this after a volcanic eruption, but for this work the separation is less clear. This dataset might offer a simple opportunity to test this given the detailed climate and dust records the authors present. It might warrant inclusion of a new regression or correlation table in the supplementary material or at the least a sentence or two considering how dust and climate may be interlinked and how the authors can separate these as dominant drivers.

This is a valuable suggestion, and we have attempted to discuss the point in the updated manuscript. We now compare the record of dust flux from the Mohos bog (selected due to its length) to all the reconstructed variables from Paleoview. These comparisons (in the form of a list of regression values) are now included in the supplementary information.

This exercise clearly shows the linkage between climate and dust deposition generally, and specifically in the earliest section of the compared record (4950 – 4000 yr BP). However, the disconnection between the two in the most recent 2000 years is also clear, suggesting the dust input is not purely climatically controlled, rather that it reflects a number of controls. These may include deforestation and human impact, thereby supporting our argument.

To reflect this point in the updated manuscript, we have added the following to the text:

"However, it has been proposed changing dust flux is purely related to climatic controls (e.g. Marx et al., 2009), and so To further investigate a potential controlling impact of local dust and erosion, we compare the model data to the reconstruction of dust flux from Mohos (Supplementary Table 3). This exercise clearly indicates the covariance of dust and climatic forcing factors (and especially precipitation) when considering the whole record. However, for the periods in which we infer local erosion and dust supply stimulating peat growth, there are either very weak, or not statistically significant correlations between climate and dust (Supplementary Table 3). Such a finding indicates the disconnection of climate and dust flux in the last 2000 years, and supports our assertion of local dust drivers in this period."

We also add the correlation table to the supplementary information (Supplementary Table 3).

In addition to this point above I have some minor comments and suggestions, see below.

Figure 2. My personal preference here would be to reverse the x axis so the time goes from modern to older, but this is just a personal preference and should be the authors decision.

We appreciate the author's comment but believe that working from left (oldest) to right (youngest) is more suitable, and so we will not be making this change.

Supplementary Figures:

The inclusion of C content and bulk density records for the individual cores would be nice to see in the supplementary figures. Particularly how often and to what degree mineral material is being washed in and also, thinking for the future, this information could be useful for future synthesis papers regarding peat properties.

All this data will be uploaded to Pangea as part of our updated submission and so will be available to download for any potentially interested readers.

Methodology:

It should be stated somewhere that the AMS radiocarbon dates are primarily on bulk peat. This has been controversial in the past, however recently I think people are less concerned about this https://doi.org/10.1016/j.quageo.2015.10.003 , however the situation could still change so it would be good to be clear about this in the methodology, not just the SI.

We have added a sentence to clarify this in the main text:

"The majority of samples were taken from bulk sediment/peat, an approach which has been shown to yield reliable age information (Holmquist et al., 2016)."

L19: Suggest for clarity changing to "mountainous peatlands"

We have made this change.

L28: This is really interesting!

We appreciate the reviewer's enthusiasm.

L43: Suggest you say "stimulates plant growth more than organic matter decomposition"

Change made.

L52: This question of early human influence is extremely interesting if a little tangential, I remember a recent paper revising the ancient human population upwards considerably. If you have not seen it I quite like the following paper about the 'lost' medieval peatlands in Flanders Belgium. https://doi.org/10.1007/s12685-011-0037-4

We agree the question of when humans began to alter their environment is a very interesting one, and appreciate the paper recommendation.

L62: I don't remember Kylander et al., 2018 talking about the inputs of mineral soil into peatlands due to deforestation and there is not any mention of trees or forests in that paper that I can find using ctrl+f. It might be worth checking that reference. It's a very interesting idea though!

We have restructured this sentence to reflect the two points being discussed:

"Finally, it is possible that increased erosion as a result of forest removal either by human actions or natural causes may lead to increased mineral in-wash (Longman et al., 2017a), with dust deposition potentially stimulating peat growth (Kylander et al., 2018)."

L72: Might be worth mentioning here how important these mires may be regionally for Romania/Serbia's carbon inventory, especially as so little carbon work has been done on them

We have added the following to the sentence:

"…one which may represent a regionally important carbon stock. "

L194: Also maybe worth mentioning that these numbers are more comparable to those from Eddy Covariance. It's also really cool that we have these high numbers now for down-core and gas measurements of C accumulation

We have added the following to this sentence:

"Further, eddy covariance data regularly indicates carbon accumulation rates greater than 100 g C m^{-2} yr^{-1} (e.g. Roulet et al., 2006)."

L194: You might also want to consider this paper https://doi.org/10.1016/j.quascirev.2019.03.022 where there was also comparably high C accumulation despite a relatively harsh climate.

We have cited the suggested paper here in the updated manuscript.

L195: The nutrients are presumably coming in in 'pulses' which seems to be quite important according to a new pre-print https://doi.org/10.31223/X5FW3J

We have added the suggested reference to this section and discuss the possibility of

pulsing nutrient supply being important.

"In each potential scenario, nutrients would likely be supplied in pulses, shown to be important for peat growth (Schillereff et al., 2021)."

L269: This could be a good place to mention the geology and soil fertility in the Carpathians. Is it exceptional in any way?

We have added the following to the manuscript:

"In parts of the study area such as the eastern Carpathians, soils are dacitic and contain large quantities of volcanic minerals, meaning they are potentially nutrient-rich (Longman et al., 2017b)."

L271: Please define the migration period, I am not familiar with this

We have defined this in the updated manuscript.

L308: This also contradicts the following highly cited paper: https://doi.org/10.1038/s41558-018-0271-1 however I can really believe this is the case given the bias towards regions with cold and continental climate

We have added this reference at this point, and altered the following sentence to reflect the fact that this observation is not just from Alaska, but across high latitude regions.

L346: I agree it is not possible to read too much into the recent changes for the reasons you have mentioned. I also recommend you remove the last sentence of the conclusion for this reason (L408)

We have removed this line from the updated manuscript.