

Interactive comment on “Environmental controls on leaf wax δ D ratios in surface peats across the monsoonal region of China” by X. Huang et al.

Anonymous Referee #2

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I have read the manuscript several times but found it hard to follow as it was poorly organized. General speaking I don't think this manuscript merits publication in Biogeosciences. Many conclusions seemed arbitrary and overstated.

The authors apparently neglected the species effect. The manuscript was targeted to address the effect of environmental factors on leaf wax n-alkane distributions and hydrogen isotopes. However, only species, Sphagnum was specified, while all the rest of higher plants were mystery to readers. The sites of peat bogs chosen covered latitudinal shift from 25 to 42°N, i.e. a very big latitude range. The types of higher plants, say, C3/C4 plants, gymnosperms/angiosperms, etc are expected to vary significantly from south to north, and so were the distributions of n-alkanes. Surprisingly, the authors did not analyze any samples of leaf waxes from “leaves” but arbitrarily attributed the

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differences in CPI and ACL among different peat bogs to pH, conductivity, etc. The input ratios from different plants (including Sphagnum) actually determined the ACL and CPI. For example, the alkane distribution in Fig. 2A (Zoige) was characterized by two centers, suggesting at least very different types of input. A calculation of ACL in Zoige peat bog based on such distribution and then relating it to pH, ORP etc is actually misleading. The authors simply piled a bunch of R2 in Tables 2-5 without giving data of n-alkane distributions. Are there coexistences of alkenes indicating aquatic sources? The authors ought to characterize and quantify n-alkane from different sources before any link to environmental factors.

Fig.2 also showed large amplitudes of dD values of a single compound within one given peat bog. Such phenomenon can be attributed to the fact that distributions of alkanes in a peat bog were indeed NOT homogeneous, which are distinct from lake sediments. Except the Shiwangutian, the amplitudes of dD values of a single compound in the other 3 peat bogs are so big that the differences in this manuscript could be simply due to sampling strategy.

Authors listed six sites of peat bogs for this study but only presented some of them. Such selection seemed arbitrary as authors simply wanted to present “good correlations”. In addition, even with such arbitrary selection, almost all correlations between pH, conductivity or ORP with dD of long chain n-alkanes were actually poor, with almost all $R^2 < 0.5$. However, the authors consider them as strong correlations. For that size of samples and such weak R^2 , I feel that the conclusions were too overreached. The authors did not provide any explanation how pH, ORP or conductivity affects ACL or dD values of a single compound. What is the mechanism? For example, can microbial activity significantly modify the CPI/ACL and reduce C23 and C25 abundances in the peat samples, and how was such activity affected by pH, ORP? The manuscript did not provide any insight for the community, but more like a bunch of data piled up and listed weak correlation with so-called environmental factors.

Currently the authors took for granted that distributions of n-alkane input to each bog

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were the same but ONLY those environmental factors changed their composition so that they considered the R2 between pH, conductivity or ORP and ACL/CPI. That is certainly not the case. The environmental factors such as temperature, precipitation actually controlled vegetation type and affected distribution of alkanes. Furthermore, the formation of peat bog is a process of long time. However, the data of temperature and precipitation given in this paper (for example, Fig. 3) were from a given year.

In the last sentence of the Abstract “the dDalk ratios of n-C29 and n-C31 alkanes as sensitive paleohydrologic proxies on millennial and larger timescales.” How do authors know such proxies can be applied in such larger time scales?

I also have a big concern on the calculation of dalk/p. In the Method the authors stated $\delta A_d DP$ was estimated from an online calculator. It is generally OK, but for those peat bogs located at altitudes from 900 to 1700 m asl, Rayleigh fractionation could cause large fraction in precipitation. The samples were collected in a two year period. Did authors think about the change in the amount of precipitation during the two year period? Amount effect would lead to the variations of precipitation dD values.

In summary, there are too many arbitrary statements and they are simply not convincing to me.

Specific comments: 1) Equation (1) was completely wrong. It is a fundamental concept and should have not been wrong! 2) There are quite a bit colloquial expressions. For example, Page 15163, Line 4, “Either way, the new and previous results suggest caution”

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