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Reply on RC1

Anna Wieland et al.

Author comment on "Climate signals in stable carbon and hydrogen isotopes of lignin methoxy groups from southern German beech trees" by Anna Wieland et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-135-AC1, 2022

We thank Referee #1 for the positive evaluation of our work and for the helpful comments to improve the manuscript. All comments and requested changes were taken into account. Please note that comments by the referee are in italics

Wieland et al. present an interesting new annually resolved series of lignin methoxy d13C tree ring series. These are highly novel methodologies and promising for paleoclimate reconstructions.

While the records themselves are interesting – and should be published - I do have several concerns regarding the methods used to correct for the plant physiological effects and the interpretation of the long-term trends. I will first describe my main concern and then point out several smaller comments on the manuscript.

Main concerns:

d13C in plant material is strongly influenced by various environmental and ecophysiological factors. These include

i) changes in atmospheric d13C and atmospheric CO2 concentration

ii) changes in atmospheric deposition in nitrogen

iii) change in tree light environment

iv) change in tree height

I will first focus on the effect of atmospheric d13C and atmospheric CO2 concentration changes. The authors correct for the d13C atmosphere effect or the Suess effect. That is all fine. However, the authors then move on to correct for the effect of plant physiological responses to atmospheric CO2 (eg. change in discrimination or iWUE) using several correction factors that have been proposed by various authors (Kurschner, Feng, Treydte) and which differ almost 3 fold in magnitude. The authors also use a correction factor developed by a previous study for higher altitude Larch trees (Riechelman et al. 2016). As shown in fig. 3 these corrections result in very different upward curves since ca. 1950 with some showing very strong increases in the "corrected" d13C.

I do not disagree with the need to correct for the effect of CO2 on these series, but we do not know enough about tree responses to CO2 to know which one of these "corrections" represents the "real" tree response. None of the corrections in the literature seem to argue in a particular convincing way how trees respond to CO2 and some just fit curves that results in the highest correlations with the targeted climate variable. In addition, tree ring d13C studies show that trees respond differently between sites and species.

In short, I cannot see how one can choose from these relative arbitrary correction curves which one is the best. The authors are favouring the correction from Riechelman as that results in the highest correlation with observed temperature (fig. 4, 5 and 6), but this is somewhat circular in my opinion. You add several artificial increasing trends to the d13C and then relate it to a climatic record of which we know that is has a positive trend and find a good match. But what do we really learn from this, and secondly can you use such a record for reliable climate reconstructions?

Several of the conclusions are entirely due to this methodological choice of adding trends to the d13C curve. For example, the increase in strength of the correlation with temperature for the upward corrected curve (fig. 4,5) is simply due to the addition of a trend to the series. It is also not surprising that the series with the strongest trends added, results in the strongest inter-series correlation (lines 207 etc). And again the d13C corrected according to Riechelmann, results in a good correlation with d2H as you have two series with strong upward trends (fig. 10), but the correlations vary in reality between negative (with the raw data) to slightly positive when correcting for the Suess effect. In my view, we are not learning much from this, and I do not believe one can use these records put recent temperature increases in a longer term context. It seems to violate the stationarity principle and the correction for that is artificial. But do please correct me if i see this wrong.

One needs to know in much greater detail how CO2 truly affects plant isotope discrimination. In perspective of this and the poor correlations pre-1965, I wonder if the conclusion that "this is a suitable proxy for reconstructing high to low frequency summer temperatures" (lines 317). This is perhaps true for the high-frequency variation since 1966, but not for the low-frequency variation and not for the full period.

Authors: Following the referee's suggestion, the relationship between increasing correlation coefficients and applied correction methods as well as their limitations are now discussed in greater detail in section 4.1.

Furthermore, the increasing Rbar values and correlation coefficients between $\delta^{13}C_{LM}$ and temperature are now described as a result of the different trends added to the $\delta^{13}C_{LM}$ series. In the revised manuscript, the correction factor of Riechelmann et al. (2016) is still considered but not indicated as the 'real' or 'ideal' correction factor.

In addition, the summarizing sentence in section 5 '…the suitability of this proxy to reconstruct high-to-low frequency summer temperatures' has been modified to read '…the suitability of this proxy to reconstruct high-frequency summer temperatures. To reconstruct long-term trends with $\delta^{13}C_{LM}$ values, a further understanding of how plant isotopic discrimination changes due to increasing CO_2 concentration is essential.'

Finally, section 3.5 'Comparison of $\delta^{13}C_{LM}$ and $\delta^{2}H_{LM}$ chronologies' was deleted from the manuscript. As the referee mentioned, the correlation coefficients between the two isotopic series are mainly a result of the applied correction procedures. This study showed, that $\delta^{2}H_{LM}$ values are mainly affected by large-scale MAT changes, whereas $\delta^{13}C_{LM}$ values are predominantly controlled by local summer temperature. Therefore, both isotopic ratios are not predominantly controlled by the same climate parameter, and we can thus not assume a strong correlation between the two series.

My other main concern is that other factors that affect d13C are poorly discussed. This includes above mentioned effects of eg. Nitrogen deposition (see Leonardi et al. 2012), and effects of tree height and light (Brienen et al 2017, Vadeboncoeur et al. 2020). For these beech trees these may be very important factors that control tree isotope discrimination, but it depends on the size and age of trees. Such information needs to be added to this article and discussed. In fact, changes of climate responses with tree height could also well explain the poor relationship between d13C and temperature before 1966. For example, Trouiller et al. 2019 find that large and small tree differ in their growth response and one could thus also expect that the response of d13C will differ.

Authors: Good point! Further information on how additional factors could affect $\delta^{13}C_{LM}$ values were added to the manuscript in section 4.1. In addition, the effects of an age-related trend, including tree size and age, were excluded by calculating a site-specific linear regression model between trees with different ages over a common period (see section 4.1).

Minor comments:

In the introduction in lines 51-61 … Can you expand the section on d2H a bit more and say where the signal comes from (source water, leaf enrichment or both), if this is known.

Authors: Recently, Greule et al. (2021) provided detailed information about the biosynthetic pathway responsible for δ^2H fractionation between precipitation and lignin methoxy groups in tree rings. In this study, it is considered that precipitation accumulates to soil water and further to xylem water with no substantial isotope fractionation. Isotopic fractionation occurs in mainly two biosynthetic processes, the transfer reaction of serine to the CH_2-unit (ranging from 0 to -50 mUr) and the hydrogen atom transfer by certain flavoproteins with a depletion of -580 mUr down to -780 mUr (for further information, please refer to the study by Greule et al. (2021)). Based on the mentioned constraints, δ^2H_{LM} values are directly connected to the source water and likely not influenced by further isotope fractionation such as leaf water enrichments.

Further information about the origin of the δ^2H_{LM} signal has been added to the revised manuscript.

Section 71-79: Some of the statements are a bit over assertive: Do we really know that much about mesophyll conductance and the effects fo Ca on photorespiration to make these statements? Be more careful here as there are large uncertainties with the variables in eq. 1.

Authors: We added further information in the revised manuscript.

Line 82: better to say .. "stomatal control limits photosynthesis" (cannot say gs is higher than the rates of photosynthesis)

Authors: Change applied.

Lines 80-86: perhaps also mention post-fractionation processes?

Authors: Further information about post-photosynthetic fractionation was added.

Fig. 1: add proper units to the precipitation axis that can be understood… eg. mm per day or mm per month.

Authors: Change applied.

Lines 183: Bravais Person?? Pearson correlation coefficient

Authors: Change applied.

Lines 211: What is the low frequency series? The LM_R as in figure 5? Why is the LM_RL a low frequency series?

Authors: The low-frequency series is referred to the $\delta^{13}C_{LM}$ values. However, the referee is correct that this declination might be misleading in this context. For clarification, the term 'the raw $\delta^{13}C_{LM}$' was added to the manuscript and the subtitle of Figure 2 was adjusted.

Equations 3 and 4 are not clear. They are the same but for different periods or is this for different series? Please explain.

Authors: Equations 3 and 4 contain the same period but summer temperatures were modelled by two different series the $\delta^{13}C_{LM\_RL}$ following Equation 3 and the $\delta^{13}C_{LM\_high-frequency}$ series following Equation 4. The equation results from the linear regression between the isotope ratios ($\delta^{13}C_{LM\_RL}$ or $\delta^{13}C_{LM\_high-frequency}$) and the summer temperatures during the period 1916-2015.

Line 341: Higher compared to what? To other species? Are you talking about higher mean discrimination, or higher changes in discrimination over time (i.e. a steeper increase in discrimination)?

Authors: Compared to other species. The correction factors of Feng and Epstein (1995) and Treydte et al. (2009) were determined to mostly evergreen conifers. Riechelmann et al. (2016) used larches, which are deciduous coniferous trees. Secondly, we mentioned a higher mean discrimination. The correction factor of 0.032 mUr $year^{-1}$ is constant and therefore independent of increasing CO_2 concentrations.

We have made this clearer in the revised manuscript.

Line 343: strange statement … "It has been shown .. "

Authors: Sentence has been revised.

Line 351: why is this not due to a decrease in gs due to increase in RH or VPD with increasing T? Are we also seeing a positive T response in the tree ring widths? Please discuss this.

Authors: The referee is correct, the relative humidity (RH) correlates with the temperature and the vapour pressure deficit (VPD) and directly controls stomatal conductance (gs).

The temperature response in $\delta^{13}C_{LM}$ values could be at least part of several indirect signals since factors like RH, soil moisture status, and drought stress also strongly correlate with temperature. However, RH, soil moisture status, or drought stress are also strongly correlated to antecedent precipitation, and we cannot see any precipitation signal in our $\delta^{13}C_{LM}$ series. Thus, we interpreted the strong temperature and weak precipitation signals as an indication that $\delta^{13}C_{LM}$ values are predominantly controlled by the photosynthetic rate.

Further information was added in the revised manuscript.

We observed only weak correlations between temperatures and TRW data (for example correlations between high-frequency series: $r_{summer} = -0.08$; $r_{MAT} = 0.14$; $r_{prev.Sep.-Aug.} = 0.10$). This might be explained because the study site (Hohenpeißenberg) is situated at mid-latitude and rather low elevation environment. Typically, good correlations between TRW and temperature have been reported for trees at growing boundaries, at high latitude or far north.

Lines 357-363: Explain this a bit more. Trees were supposedly younger, smaller in pre 1966, Could that explain the change? Trees were perhaps below the canopy and limited by other factors? Please discuss further.

Authors: Age-related trends, including tree age and hight, could not be reported in this study (Further explanation see section 4.1 in the revised manuscript). Further explanation of how different environmental factors might have influenced the $\delta^{13}C_{LM}$ series before 1966 were added to the revised manuscript.

Lines 368-369: drought stress i only mentioned here for the first time. Why? include this possible mechanism also in earlier sections. It is not just Assimilation that affects d13C. And you might be able to check if d13C is controlled by A or gs when you also include analysis of ring width. If ring width increases in line with d13C then it must be assimilation controlled, if it is the opposite then it must be controlled by gs.

Authors: Drought stress is now mentioned as one environmental factor that influences carbon isotope fractionation. This part has been moved to section 4.1. Moreover, comparison between tree ring width and $\delta^{13}C_{LM}$ values support our assumption that $\delta^{13}C_{LM}$ values are dominantly controlled by the assimilation rate. We added further information on this in section 4.2 and included a figure showing the relationship between TRW and $\delta^{13}C_{LM}$ series in the supplemental.

Line 370: you mean overcorrect the original (raw d13C)?

Authors: Correct. For clarification purposes 'raw $\delta^{13}C_{LM}$ values' was added.

Line 371: indeed a lot of uncertainties that can move your recent trends in d13c any direction depending on the uncertainties.

Authors: Yes, we agree. The multitude of uncertainties potentially affecting $\delta^{13}C_{LM}$ values have now been adequately highlighted in the revised manuscript.

Line 375: inter-series inconsistencies in the early part of the record again indicate that other factors than climate affect d13C.

Authors: Further explanations were added to section 4.2.

Line 379-380 "additionally .. " explain a bit more. What is soil sealing?

Authors: Additional factors were added, and soil sealing explained in more detail.

Line 397 .. intensified anthropogenic warming .. this is not clear. What do you mean why do you say intensified? Is that in comparison with the temp increase? The trends in temp and in d2H look pretty similar to me, and no need perhaps for other factors to be involved than simply temperature.

Authors: To avoid any confusion but also not to overinterpret the impact on additional environmental factors we have removed the section dealing with drought from the manuscript.

References:

Greule, M., Wieland, A., Keppler, F., 2021. Measurements and applications of δ2H values of wood lignin methoxy groups for paleoclimatic studies. Quat. Sci. Rev. 268, 107107. https://doi.org/10.1016/j.quascirev.2021.107107