

## ***Interactive comment on “A 500-year multi-proxy drought reconstruction for the Czech Lands” by Petr Dobrovolný et al.***

**Anonymous Referee #2**

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Dobrovolny et al. presents a new multiproxy hydroclimate reconstruction for the Czech Republic spanning back to 1501 AD, based on four different proxy types: tree-ring width chronologies from oak and silver fir, grape harvest dates and historical documentary evidence. While most of these records are subsets of published data that have previously been used in single proxy reconstructions to infer past hydroclimate/rainfall variability in the region, no attempts to synthesize moisture-sensitive proxies with different climate retention characteristics have, to my knowledge, yet been made for this particular region. This type of work has therefore the potential to be compelling and of scientific value for the paleoclimate community, as it has the possibility to provide a more accurate reconstruction at a range of time scales and also help to quantify the uncertainty in the individual proxies in a rigorous manner. Given this potential, I found

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however some of the methods selected in the current study not entirely justified. I also found the manuscript to be a bit untidy when it comes to the details. There are thus a few very important points that would be strengthened through revision.

It is not obvious in the manuscript what additional information that are gained by combining the different kinds of proxies into a single hydroclimate reconstruction compared to the previous published reconstruction efforts using the individual proxy records separately? For example, the abstract concludes “The new multi-proxy drought reconstruction demonstrated progress beyond previous single-proxy attempts at establishing the strength of hydroclimate signal.” The authors should be more explicit, what is the novel contribution of this new reconstruction and in what way does it move the field forward? The explained variance of 70% is indeed impressive, but as far as I can see not superior to the amount of variance explained when using the GHD record separately to reconstruct regional SPEI (P3/L31). Also, the target season seems to be shortened when combining multiple proxy records. Another example, page 2/L21 reads “To overcome some of the problems of the single-proxy approach, to identify possible seasonal biases and finally to provide more rigorous estimates of past and present hydroclimate variability, this contribution attempts to synthesize available precipitation-sensitive proxies from the territory of the CR and to utilize them in the first multi-proxy reconstruction of droughts in the area.” These objectives does not match the rest of the paper; I cannot see that the authors have identified and provided a solution to the seasonal mismatch among the proxies. Neither is there any in-depth test or even discussion provided on the advantage of the current multi-proxy reconstruction compared to the previous single-proxy attempts. Perhaps is there an oversight from my part, but the only reconstruction uncertainty estimates that I can find in this work are based on the regression error (figure 5). I suggest that the authors either rephrase the introduction/objective section to match the methods and the results, or explicitly identify the problems of the single-proxy approach and state what test they have designed in this study to overcome these issues.

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My other concern relates to the very short calibration/verification period used to build the reconstruction model. Reviewer no. 1 has already raised some critical points concerning this issue, for example the possibility for overfitting the model which has the effect of underestimating the prediction error. Another potential problem is the fact that early meteorological observations are utilized for the calibration/verification exercise. Because of the scarcity of observations, the early instrumental data cannot usually be satisfyingly checked for their quality and homogeneity (even for a leading climate parameter such as temperature, and for a region such as central Europe). Before applying the early instrumental data in proxy calibration it should be considered to what extent they reflect real climate (homogeneity) and to what extent the observations are representative for the proxy sites (spatial representation). It is not clear how these issues have been addressed in the current work. Perhaps an oversight from my side, but I cannot find any information in the manuscript on which and how many stations the authors have used in the current work to compute the Z-index, which selection criteria were applied? Is the calibration data some kind of a regional mean for the whole domain? If so, how many stations are included and where are they located (I would suggest to add their location to fig 1)? Is the number of stations changing through time? These issues may potentially have an important implication the calibration/verification statistics and should be at least be mentioned in the text.

Some of the details in the data and method sections are at times rather swiftly handled and details are missing that would help the readership to understand the rationale for why certain approaches were taken. Specifically: P2: How were the different tree-ring data standardized? From eyeballing fig 2 I get the impression that all four proxy time series contain different amounts of low and high frequency variability, autocorrelation and sometimes the variance is conspicuously instable (the GHD chronology). How is the final reconstruction affected by these issues? This is an important problem that must be considered when applying a multi-proxy approach. Please provide at least a discussion around this issue. L1/P22: "While reflecting a considerable amount of inter-annual spring-summer drought variability, the new Z-index reconstruction does not capture any

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long-term trends beyond the multi-decadal domain." Reading this paragraph the less well-informed readership might get the impression that the region has not experienced any long-term changes in hydroclimate beyond the multi-decadal domain, whereas the absence of any long-term trend in the reconstruction in fact is more likely related to the limited ability of the proxy data to encode climate variability at the centennial and longer time scales. Please rephrase to make this clearer.

I have noted a few instances where the text requires revising or minor revision to clarify particular points, see my comments below. I hope my comments will help the authors to improve the manuscript.

P2/L4: change to " will become" P2/L5: "... partly corrected with hydroclimate proxy reconstructions" I am not sure that "corrected" is the right word to use in this context P4/L21: clarify over which period the PCA was performed, is it performed separately over the calibration and verification periods? Also, I cannot find in the ms how the different chronologies are loaded on the principal component, which chronologies contribute the most to the reconstruction and is the contribution stable over the calibration and verification intervals? P5/L1: "The mean of the target data was added to the PC scores to ensure the same mean for both proxy and target". Over which period were the statistics computed? P5/L26: "tendencies" – change to variability or trend (suggestion) P6/L18: please check that the R2 value reported is consistent throughout the ms, for example P1/L19 – 70%, P6/L10 – 67.8%, P6/L18, 69.7%, table 1. P7/L21: "The driest year (1616) had already been identified in numerous studies analyzing documentary evidence (Brázdil et al., 2013b; DobrovolnĀj et al., 2015) and tree-ring chronologies (DobrovolnĀj et al., 2018) covering CR territory." Yes, but are these data independent from the one used in the current study? At least the tree-ring chronologies in DobrovolnĀj et al., 2018 seems to overlap with the current study. P8/L19: It is unclear from the text whether the Old World Drought Atlas and the current reconstruction share some of the predictors. Table 1: full calibration – do you mean 1805–1854 ? Figure 1: I would suggest adding the location of the meteorological stations to the map. Figure

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5: is this the regression based reconstruction or the variance scaled?

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Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-160>, 2018.