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Climate of the Past Discussions

Interactive comment on "Long-term variability of droughts in the Czech Lands and largescale climate drivers" *by* Jiří Mikšovský et al.

Anonymous Referee #3

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Description

This paper addresses the understanding of the variability of droughts, temperature and precipitation in the Czech lands 1500-present from the point of view of its dependence on internal drivers (e.g. some specific modes of circulation) and also external forcing factors (volcanic, CO2, etc). For that purpose a multiple linear regression is applied having as predictand variables 3 drought indices and as predictors a set of external and internal drivers.

The purpose of the paper has value and meaningful and solid results in this direction would be worth to be published in CP. If attribution of drought variability or a meaningful step forward in its understanding in the Czech Lands would be attained I think this would be sufficiently valuable in my view to support publication. Therefore, I en-

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courage authors to pursue this line of work towards publication. At this state, I would recommend major revision of the manuscript. There are several issues related to the rationale, methodology and description and interpretation of results that in my understanding require revision. I will argue about this in the following points.

General Comments

GC1 General approach to attribution As it is described in the paper, 5 predictand series (3 drought indices and a temperature and a precipitation series) are examined using multiple linear regression as functions of independent predictors, the latter being internal and external in nature. In practice, these are 5 individual multiple regressions. Having that in mind I would suggest to consider the analysis, description and discussion of the: a) selected predictors; b) of the methodological approach; and c) of the residuals of the methodology.

a) Selected predictors. I would argue these are insufficient in both the case of the external and internal subsets.

a.1-Regarding external predictors I have no objections to the ones considered so far but the authors should discuss why important predictors like other greenhouse gases (GHGs), aerosols and particularly land use land cover (LULC) are not considered. For the case of other GHGs than CO2, it would be more elegant either to consider them or to use equivalent CO2. For the case of aerosols some arguments or strategy or implementation should be considered also. For the case of LULC, this would really be an important variable since it can have an impact on drought. If any significant trends are found, how can we attribute them arbitrarily to CO2 or to a mix of the influence of GHGs and aerosols? If there has been progressive changes in LULC in the area, in the context of this manuscript, eluding them would be really misleading for the results of this analysis.

a.2- Regarding internal predictors, the NAO is argued to be important but has not been used. Even if it has been described in previous works, it is relevant to see in this approach how much variability do ENSO or PDO account for from the residuals once the NAO has been taken into account. Do the results of the analysis concerning the presently used internal predictors change if the NAO index is used? There are some millennium long index reconstructions that would allow for this exercise. I think there is no point in looking only at Pacific indices without considering a potential larger explanatory variable like this one.

b) Methodological issues There are three ideas that I would like to bring here. One is the linear vs non-linear character of the influences that the paper tries to assess. Another one is the power of the approach used herein related to the covariance structure pursued by the analysis in view of the properties of the predictors. Finally, and related, the collinearity of some predictors.

b.1 Regarding the first one, this is commented in the first paragraph of Sec. 3. I have no reservations against the possibility of nonlinear interactions being relevant. I think it is though important and has value, to study the linear relationships. It is also important to study it in a solid way so that we minimize the danger to loosely argue that everything we cannot explain with a linear approach is due to the limited character of its 'linearity' and probably due to nonlinear interactions.

b.2 Regarding the second one, the multiple linear regression is a valid approach to analyze the linear covariance structure in the data. Now, for that purpose, the variables used as predictors like CO2 or, for that case, if additional GHGs+aerosols would (and should) be considered, since these variables present very low variability at high and mid frequencies, one has to be careful in how to handle them in terms of covariance. For instance, a positive coefficient with temperature in the instrumental period means that both temperature and CO2 show positive trends. . . but any variable showing a positive or a negative trend would show association for that matter. The limited meaning of correlating preindustrial CO2 (+GHGs+aerosols) must be commented and the limited interpretation of correlating trends in the industrial period also should be argued and improved by including other GHGs and aerosols in a meaningful way.

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b.3 Some of the predictors (eg. AMO, PDO) show covariability. How is this addressed in the analysis and how does this influence the results? Explaining which type of multiple regression approach would be important for this point.

c) Residuals This is also a rather methodological issue. If the purpose is to statistically describe drought with a multiple linear regression approach, the behavior of the residuals should be discussed. The authors should show estimation of drought variability from the predictor variables, explained variances and some convincing arguments that part of the variability is being reproduced by the predictors used.

I recognize this point, GC1, is rather long. It should probably be treated as independent points. Nevertheless I think it is important and would like to see the arguments for all these. Some specific comments will also follow below.

GC2 Mechanisms As it stands, the approach of the manuscript is to argue on the basis of the regression coefficients. This is quite extreme in its present state. Even in the discussion part, a relatively aseptic account of the results of other authors are provided in this sense. However a more mechanistic based approximation discussing the rationale behind the statistical relationships that may be found is needed. GC3 Temperature and precipitation What does having temperature and precipitation add in this analysis? I don't mean to be unconstructive. . . just that if it is included in the analysis pursuing a more in depth understanding of drought, the reader should understand why they are there. What gain in our understanding do we get from including temperature and precip and analyzing them as predictors? Would it be of use including them in one exercise as predictands and assess their relative influence on drought?

GC4 Section 5: PCA analysis The strategy for the PCA analysis in page12 should be described (already in the methods section), as well as it is purpose and results presented in the text. . . unless the results are rendered invalid or not useful. If the analysis provides some valuable insights within this ms, it should be shown.

GC5 Section 5: discussion The Discussion section provides a wealth of information on different results from various papers. However, in my opinion it misses a bit some purpose or direction. Actually, it also reports on results (e.g. GC4) that are not shown although they permeate to the conclusions. This is not recommendable. I suggest to pass any results clearly to the parts of the paper to make clear the objectives, methods and analysis of the results. Having a Discussion part or a Conclusion and Discussion makes sense to put the results of the present ms in view of past literature and state clearly what we learn from it. I would advise the authors to modify this section in this sense.

Specific comments

SC1 Title: '. . . large-scale climate drivers' If we understand 'large-scale climate drivers' referring to modes of circulation, shouldn't the title also include those? E.g. ' Long-term variability of droughts in the Czech Lands due to external forcing and large scale climate drivers' ?

SC2 Page 2, l 17: 'Internal forcings' I think the use of this concept is not adequate in the manuscript. We relate forcing factors to changes in the energy of the system and, therefore, external in nature. I agree with using the terms internal/external drivers or external forcings, but not internal forcings.

SC3 Page 3, l 31: 'Missing monthly precipitation figures. . .' I don't understand what is meant here by 'missing' figures in Dobrovolny et al (2015).

SC4 Section 2: Figure 1 I haven't found a reference to Figure 1 in the ms. Check on this please. Regarding this figure and the presentation of drought, in Section 2 there is some description of differences in definitions among the different drought indices used in the text. I think some comment on the available reconstructions are pertinent. There is a paragraph in page 2 (l 21-30) describing the origin of the series. Can the authors provide any thoughts on whether the different definitions really play a role or basically the same information is available, also considering the source data for the reconstructions. Can we anticipate any added value of using these three indices instead of one

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in this work?

SC5 Section 2.2: forcings I think it is desirable to place this forcing in the context of PMIP3 and PMIP4 forcings. The authors will find longer reconstructions of this forcing spanning the millennium that have been used to detect solar forcing on temperatures for instance since the 14th century (Schurer et al 2013). Maybe these can be better options for predictors than the one used in the ms (1610-present) Schurer, A., G. Hegerl, M. E. Mann, S. F. B. Tett, and S. J. Phipps, 2013: Separating forced from chaotic climate variability over the past millennium. J. Clim., doi:10.1175/JCLI-D-12-00826.1.

SC6 Section 5 L 30 '. . . the increase in the ambient CO_2 post 1850 is clearly correlated with the increased probability of drought. . . while during the pre-instrumental period such link does not manifest. The trend in CO_2 in the industrial period is just one degree of freedom. Please recall the comments GC1b

Technical corrections, typing errors,etc:

TT1 Page 6, l 8: '. . . results. . . standardized regression coefficients. . .' In an simple regression these would be, by definition, correlation coefficients. How does this differ in this analysis from correlations? Some methodological details on the multiple regression approach taken is advisable. Does it account for covariability in the predictors? Etc. . . Please provide more explanation of the relevant aspects in the ms.

TT2 Page 2, l 20: '. . . that increase. . .' substitute by '. . . that the increase. . .' This is just an example. I have found a few of those. I think the text is easy to understand in general. However, I would recommend it would be revised for editing/english

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