Description

The paper addresses the identification and quantitative attribution of drought variability over the Czech lands in terms of three drought indices spanning over the last 5 centuries. It targets drought-climate links comparing drought indices with climate (temperature and precipitation) reconstructions, modes of internal variability and external forcing parameters.
As I stated in my first revision, I think the purpose of the paper has value and steps in the attribution of drought variability over the Czech Lands or progress in understanding mechanisms would be valuable in my view to support publication. The revised version of the manuscript includes new analysis like NAO related links and other aspects not considered in the initial version. It also considers more reconstructions of specific indices and provides a more clear view of the difficulties to relate, at least with this technique, the variability in external forcings and large scale climate drivers with Czech drought during the last few centuries. I think the authors have made an effort to provide a more clear manuscript and the discussion of their results is fair and honest in declaring the understanding that can derive from this analysis and its limitations. I think that there are still a number of issues that have to be taken care of. In general I would say that individually taken they are not major but there is a number of them. I will leave it to the consideration of the Editor whether a new revision cycle will be needed.

General comments.

GC 1. Overall I would invite the authors to really think about what we gain from this analysis and have clear statements about the confidence and reliability of the links they report on, and report on this assessment in the conclusions as a take-home message for the reader. Some of the resulting coefficients and relationships stem from the (sometimes clearly, sometimes marginally clear) analysis of some of the reconstructions and not from the others. I think the authors do an honest job in highlighting this (and some of my subsequent comments go in this direction), although I would suggest really making an effort for a very clear assessment of how much confidence we can have on these results.

I think it is important to minimize the danger that results are cherry picked in the future using this manuscript as a reference for clear links between a mode of circulation (say PDO or AMO) and drought when in fact, it is not, and the relationship may be very much dependent on the reconstruction considered. I think this is particularly the case when the study provides coefficients resulting from a multiple regression analysis that considers data in different periods but there is no insight about the mechanisms that may support such relationships. Perhaps a message that needs to be clearly stated is that there is too much uncertainty and that even if one technique may provide relatively clear results in depicting some level of relationship (very small R2) between drought and large scale drivers and forcings, there is too much uncertainty for having confidence on the purported relationships as other reconstructions do not provide that clear link.

GC2 Would the authors have arguments to believe that any specific reconstruction from the ones considered for ENSO, PDO, AMO or NAO is better or more reliable than the others? If so, this should be clearly stated. It is actually done so to some level. At a first stage the reconstructions are considered alike to derive relationships to regional drought and then the relationships are used to decide that one reconstruction is more trustworthy or reliable or better than the other because it shows a more intense relationship with drought. I think this is a dangerous path. Indeed the manuscript steps onto this ground and I would strongly suggest revising these types of arguments. After all I think that if this manuscript gets to publication, its value for the community should reside as much in the quality of the arguments as in the strength of the statistical results.

GC3 A great deal of the material of the manuscript at this stage refers to the Supplementary Material. Actually there are more plots in the SM than in the main document. Much of the discussion part relies on this bulk of material. I really wonder if some of the results should not be promoted to the main text.

Specific comments

SC1 Section 2.1.

Various indices to characterize drought at seasonal and annual timescales are used in the paper and introduced here. The last paragraph indicates that these indices are derived in Brázdil et al (2016a). The first paragraphs of Sec 2.1 indicate the differences in the predictor variables that lead to different definitions of SPI, SPEI and PDSI. I suggest that it may be good to include here some sentences of the different information that using these three indices instead of a single one can provide in the light not only of a priori definitions but also of the results in Brázdil et al (2016a) or on what may be expected to obtain later on. This information may be of interest for the reader to have some understanding of the interpretation of the indices for this specific area based on previous experience of the authors. Are the indices very different? Do any of the three reflect any specific features? Indeed the low frequency variability in fig 1 looks very similar for the three of them. I suggest providing the correlations between each pair of the three indices available. This will allow the reader for knowing two what extent having 3 series instead of one adds information. Otherwise the reader misses some, probably justified, rationale for this set up.

SC2 Section 2.2. Page 5, Line 15-17. '…extended back to AD 1501 using CO_2, CH_4… concentrations obtained…'

Who did this extension? If obtained by the authors indicate reference. If developed by the authors, please, explain how.

The reference to the web is perhaps better in the figure caption? A preferable alternative is always a reference.

SC3 Section 2.2. Page 5, Line 15-17
Why wouldn't aerosols also be considered? This is an important anthropogenic component. The authors have discussed this in the response to the previous review. Please consider arguing here why including aerosols and LULC is not worth.

SC4 Figure 1
- Figure caption: suggest changing 'Fluctuations in the annual series of…' by 'Annual series of…'
- References: they are clearly exposed in the legends. I would rather suggest having them in the caption, but this is not critical. The Meinshausen one, extended… a note on this can better be incorporated to the caption, as any other feature related to the construction of the figure or source of data.
- Scale: the volcanic forcing should be negative. Units would be more clearly stated in the axis labels than in the legends.

SC5 Figure 2
- Figure caption: suggest changing 'Fluctuations in reconstructed series of…' by 'Reconstructed series of…'
- The Luterbacher et al (2002) series looks strangely flat. Can the authors please check on that one? Previous representations of this series show more low frequency variability. The resolution of the plotted series is not indicated and is confusing after reading the text (monthly, seasonal, annual… see MC7.
- Any technical details in the construction of the series added by the authors like filtering low frequency components by subtraction in the Mann et al series can, for the sake of clarity, be mentioned in the caption or a note to the main text be made.
- The reconstruction of Mann et al (2009) and MacDonald and Case (2005) seem to be in phase opposition. Maybe the authors should consider commenting on this in the main text as it can have implications for the subsequent analysis.

SC6 Figure 6
- In the logics of the text, this should rather be Figure 4.
- Please check on the Luterbacher NAO index wavelet (see SC5).
- Consider making a technical short note on the cone of influence in the caption. Also for the subsequent cross-wavelet plots.
- The numbers and labels in this figure are too small. Check size of characters also in subsequent plots.

SC7 Section 4.1, page 8.
- Line 24-25. 'A statistically significant solar related signal was also absent in all individual seasons except for SOM'
Right, and also additionally a somewhat marginal link in JJA, however they are negative! I suggest being really careful with these things. Otherwise statistical links are highlighted but they may have little

physical basis. What can be the reason for a negative relationship of temperature with solar variability?

- Regarding ENSO, AMO and PDO. It would be good if some mechanistic explanation, linking to other literature, can be provided to support the confidence on these correlations. For instance the positive correlations with wetter DJF or drier SOM… It is desirable to provide some support for these relationships on the basis of mechanisms and/or similar relationships in other studies.

  The same applies for the wetter DJF and SOM AMO situations or the influence of the PDO to dry conditions. In this last one, why would PDSI be more sensitive according to the experience of the authors? Regarding the PDO, I would be interested in having some assessment by the authors on the confidence on these results, since a) the relation is found only with the Mann et al data, and b) this reconstruction seems to have a very different behavior to Macdonal and Case and Shen, sometimes in phase opposition.

  Finally, according to Fig 4 and 5, the influence of the PDO seems to be the largest. It is important to have some assessment on confidence on these results on the basis of previous literature and the results herein, as a) these results rely only on one reconstruction and b) the resulting coefficients are even larger than the NAO. Would the authors then support a larger influence of Pacific variability on Czech drought than that of the North Atlantic?

  When reading subsequent parts of the text this is not the case, but the numbers play in this direction at this stage and some comments on this may be advisable.

SC8 Fig. 4 and 5

R^2: The explained variances shown through Fig 4,5 seem to be very weak in general. This means the bulk of drought variability is not explained by these indices. Perhaps the fraction of low frequency variability explained is larger?

Perhaps it would be advisable to do the same exercise on purely instrumental indices (ENSO, PDO, AMO and NAO), not reconstructions and have that as a benchmark of what should be expected in the frame of the reconstructions. This should be viable in terms of assessing interanual variability in the instrumental period and would place a more realistic perspective on the level of expectations we can have on the reconstructions. After all, most of the variability the study is addressing is interannual to multi-decadal, well represented in the instrumental period.

SC9 Section 4.2

- In general I agree with the description of Section 4.2. I have reservations regarding talking about periodicities. Talking about periods or frequencies in a wavelet or spectrum is fine, but I would suggest avoiding conveying the message of stable periods/cycles. Otherwise, prediction based on cyclic memory would be possible. I would rather talk about timescales of variability. Having said that, I leave that to the criteria/taste of the authors.

- The sentence in page 11 ‘ No significant match between the oscillations in the NAO index series and the drought indices was found … (it is worthy of note that this result does not imply a lack of relationship as such, merely an abscense of common periodicities…’
  I would disagree with this statement. If there is a relationship (linear) it must be appreciated in the covariability shown by crosswavelet or crosspectra. Perhaps I misunderstood the statement, but please, reconsider it, since this can be a very misleading one.

SC10 Section 5, page 12. Lines >2. ‘Even so it should be emphasized that regression … does only reveal formal similarities… . This is particularly true in the case of signals dominated by simple trends, such as the gradual rise of GHG radiative forcing… Our results should be considered a supportive argument regarding the relationship between the drought regime and the anthropogenic forcing, not a definitive proof of the causal link.’

Page 17, line 7: ‘GHGs concentration … matches the long-term trend component in the temperature sensitive drought indices quite well… Even considering that statistical attribution analysis can only reveal formal similarities… the relationship during pre-instrumental and instrumental periods and other available evidence… support the existence of an anthropogenic induced drying effect in central Europe…’

Please check the consistency of the level of reassurance of these statements with the results of the paper. The coefficients in Figs 4 and 5 somewhat support the role of GHGs, mostly in the industrial period for SPEI and PDSI and for temperature in the whole period. Seasonally, temperature is clearly positive and SPEI shows some negative response in JJA and SON. However: are temperature and SPEI and PDSI trends significant themselves? See Figure 1. Temperature trends seem to stand out of the background envelope of variability, but I would not be able to ascertain this is the case for SPEI and PDSI. Please think how to formulate attributing statistical relationships to trends that …may not be significant? First ascertain they are (detection) and then try going further.

SC11 Section 5, page 13. Lines >5. ‘While previous studies… of explosive volcanism this analysis of more <u>than</u> five centuries of data has revealed a more distinct volcanic imprint suggesting a tendency to wetter conditions following major eruptions… most prominent in summer.’

Consider also page 8 line 31: ‘The volcanism effect … precipitation is non significant… As a result the volcanism-attributed component is negligible in precipitation-only SPI, but somewhat more prominent (even still non significant) in temperature sensitive SPEI and PDSI. The season specific… during summer, when a borderline statistically significant response also appears for precipitation and both SPI and SPEI.

Page 17, line13: ‘A distinct signature of temporarily wetter conditions following major … eruptions…was detected.’

These statements suggest different levels of reassurance of the relationship to volcanic activity. I think the ‘distinct signature’ statements overstate the relationships found with drought indices. In Fig. 4 none of the drought indices or the precipitation show coefficients that significantly stand out of 0. In Fig. 5 this is also the case except for summer when SPEI, SPI and precipitation tend to show values larger than 0… but can we call that a ‘distinct signature’? Please evaluate the level of confidence on the relationships found and make sure the statements are really supported by the data and the relationships found. This applies also in general to other statements of the manuscript. See also next comment.

SC12 Regarding the volcanic response. The authors report that a lagged analysis bears no clear results. This is not strange in terms of covariance/correlations. A more appropriate analysis can be a simple epoch analysis in which the authors would synchronize the most important volcanic events in the last few centuries and the corresponding values of drought indices, temperature and precipitation. I think this would be a meaningful complementary plot to the ones shown here and would fit well to the discussion. In the summer it may show a clearer signal even.

SC13 Section 5. The discussion in pages 14 to 16 is well organized regarding the structure and the use of literature. I quite like that. There are however, two additional features that I find odd and would advise differently.

a) One is the systematic use of supplementary material. A considerable bulk of supporting evidence relies on it. It can be a matter of style but having more figures in the SM than in the main text and these figures being so relevant for the interpretation of results suggests to me that some of these figures should be included in the main document.
b) There is an issue with the interpretation and discussion of different reconstructions and how they provide or not evidence for variability of regional drought. One specific case is that of the AMO PDO indices and their differences. I understand this is somewhat an evolution of the PCA analysis in the first version of the manuscript. I would not say it is wrong, but as it is presented it reads like playing with numbers. The other reconstructions do not support that and all of a sudden the differences between two specific reconstructions of the same type (that have already been through a considerable filtering process) renders some correlations. It is hard to have confidence on these results and bear they are really representing some differences between Atlantic and Pacific variability. I think overstating those numbers is dangerous. This results permeates to the conclusions, with cautious phrasing, that is true… but I do not think there is good ground for it if it is not supported by some serious

rationale based on literature or mechanism based arguments. How can the authors provide some confidence that these are not numbers obtained just by chance?

SC14 Section 5. Page 15, Lines > 21. See also SC8. '… this role appears to be played by interannual variations associated with weather changes closer to synoptic time scales and tied to local climate…'
Still, it is strange that only NAO would not for instance account for a larger percentage of variability. And if there are other (European) local modes that account for more variability, shouldn't these actually the ones that should be considered then in this analysis?
I would advocate for having a benchmark of correlations with instrumental period indices that would then support to look at the indices selected in longer timescales.

Minor comments

MC1 Section 2.2, page 5, line 9: '…with notable oscillatory components in Fig 6.
This is the first time Fig 6 is mentioned. The second in Page 6, Line 8. The previous figure to be cited is Fig 3. I think that the logical sequence of the text asks for moving Fig. 6 to the 4th position. This would make a more logical flow in the text.

MC2 Section 2.2, page 5, line 9: '…with notable oscillatory components in Fig 6.'
Why 'with notable oscillatory components'?. Better indicate why wavelet spectra are used… for actually all series.

MC3 Section 2.2, page 5, line 18. 'Variations in solar activity typically leave no clear imprint on the climatic conditions of the lower stratosphere'
Check consistency with detection/attribution chapter in IPCC 2013.

MC4 Section 2.2, page 5, line 24. 'The effects of major volcanic eruptions … but exhibiting just <u>inconclusive</u> <u>local</u> imprints during the instrumental period'
Really?. To what area does this statement refer to? Please, check consistency with detection/attribution chapter in IPCC 2013.

MC5 Section 2.2, page 6, line 3-4. 'Since the primary focus... oscillatory behavior associated with internal climate variability…'
This may read a bit misleading because the focus of the study is also considering external forcings that influence drought. Perhaps would it be a better argument here that the external forcing signal is disregarded from the Mann et al series by subtracting the 70 yr moving average of the NH mean temperatures?

MC6 Section 2.2, page 6, line 3-4. 'Since the primary focus... oscillatory behavior associated with internal climate variability…'

Line 15-16. ‘Again, due to the presence of a strong trend component in the Mann et al series, detrending…. ‘
Did the authors in this paper do this or was the detrended series obtained from elsewhere? If so, please include a reference.

MC7 Section 2.2, page 6, line 24-32. ‘For the purposes of this study, it was also analyzed in the form of annual NAO index values, extended to the year 2006 by… Jones et al (1997)’
I found the last comment regarding Jones et al (1997) confusing, but maybe it was my misunderstanding. I suggest that the text includes clear statements on the strategy to address drought for different seasons/timescales in coordination/correspondence with those of the predictors used. I see that more clear statements are included in Section 3, page 7, lines ~10. I just suggest making this as clear as possible to the reader.

MC8 Section 4.1, page 8, line 15. ‘… or by total anthropogenic forcing including the effects of man-made aerosols’
Is this really so? Typically the effect of aerosols delays that of GHG because of their relative cooling. Thus a better correspondence between temperatures and anthropogenic forcing can be achieved when aerosols are considered. Check IPCC 2013 and perhaps rephrase argument.
In any case I understand the trends of drought are quite small and the effect would be difficult to discern between GHG and aerosols, as also the authors have commented in their response.

MC9 Section 5, page 12, line 31. ‘… it may be speculated that the responses in the seasonal data are tied to inter-annual…’
Wouldn’t this be evident in the crosswavelet analysis?