We would like to thank all three anonymous referees for their valuable comments regarding our manuscript. We tried to incorporate the corresponding modifications into our analysis and its presentation in the revised paper, or bring arguments in cases when we were unsure how the proposals could be implemented into the manuscript without too severe changes of its context or aim.

Major proposed changes to the analysis and its presentation in the revised manuscript:

- Results involving effect of the North Atlantic Oscillation (NAO) will be added in a quantitative form (whereas in the original manuscript, only a brief mention of NAO effects was made in the text).
- More reconstructions will be employed to represent the AMO index (2 versions in total) and PDO index (3 versions in total) and comparison with the previous results based on the reconstruction by Mann et al. (2009) will be provided. Predictor representing radiative forcing due to changes in the atmospheric composition will be altered to involve the aggregate effect of multiple greenhouse gases rather than just carbon dioxide. The solar variability predictor will be replaced by the recently published Total Solar Irradiance (TSI) data by Lean (2018; DOI 10.1002/2017EA000357), covering the entire 1501-2006 period.
- Discussion of the (potential) links between predictors will be expanded, in relation to the effects of external forcings on the activity of internal climate variability modes, as well as regarding mutual interactions of individual internal variability modes. Note, however, that these topics are extensive and in some cases (such as the causes, effects and interconnections of the Atlantic Multidecadal Oscillation) still intensely debated and far from concluded. Our manuscript can therefore only provide a limited overview of the related matters. Similarly, discussion of the physical mechanisms linking the drought indices to the explanatory variables will be extended.
- Additional results will be presented to better illustrate the properties of the regression mappings, including graphs of regression-estimated components associated with individual predictors and representations of regression residuals.
- Electronic Supplement will be added to the manuscript to hold extra materials; some of the illustrations originally in the main manuscript may be moved to the Supplement.

Please see below for specific responses to the comments of referees 1, 2 and 3 (R1, R2, R3) and the suggested changes to the manuscript.

Anonymous Referee #1

The paper analyses the long-term variability of droughts in the Czech lands based on long reconstructions (based on instrumental and documentary data). Time series of drought indices, temperature and precipitation are compared to reconstructions or time series of suspected drivers such as external forcings and oceanic variability modes. Anthropogenic radiative forcing as well as AMO/PDO are identified as influencing factors. The paper is interesting, valuable to the community and within the scope of Climate of the Past. However, I have several comments, which I think the authors should consider, before the paper can be published.

Methods

It is not fully clear which data are monthly or seasonal. Often the text mentions "seasonal and annual" or "monthly, seasonal and annual", which I found confusing. Also, the drought indices are usually calculated for individual measurement locations or grid cells. Here they are calculated for a large-scale average, as I understand. This should be made clear and explained. In the results section it then becomes clear that the seasons are analysed separately. However, what is the motivation for analysing a autumn or winter drought index?

> **Response R1-1:** The data description (Sect. 2.1) will be modified to make it more clear that the analysis was carried out on either series of annual values (i.e., consecutive values representing means for an entire year) or series of consecutive season-specific means (i.e., one seasonal value for each year). The note regarding monthly series in Sect. 2 pertains to some of the original data sources; monthly values were not directly studied in the current analysis. The nature of the drought indices as area-wide means will be more explicitly stated in the text (Sect. 2.1). Since drought data for all seasons (including fall and winter) were available and analyzed, we present the outcomes for all four seasons, to illustrate the full range of potential climate links, even though for some applications (such as investigation of agricultural droughts) spring and summer conditions may be of greater interest. It should also be considered that recharge of the underground water resources and surface reservoirs depends on the water available during fall and winter and droughts in these periods often induce major hydrological impacts in the following year.

Multiple linear regression is used to separate individual components, but fully separating external forcing from internal variability (e.g., oceanic modes) is fundamentally difficult. External forcings might operate via altering internal variability modes (e.g. solar and volcanic forcing might change the climate system via AMO or ENSO). Conversely, AMO and PDO have the imprint of global temperature rise. I see that the authors use cross-wavelet spectra, partly to assess the interdependencies, but not systematically. Partial correlation methods could be used to go into more depth here, or different models could be compared. In any case, the interpretations should be phrased very carefully.

> **Response R1-2:** Indeed, the problem of separating the strictly external forcings from the internally induced variability is a complicated one, not only at a statistical level, but also with regard to the underlying physical mechanisms. While this was not mentioned in the manuscript, we examined the mutual links between individual predictors with episodic or oscillatory components in terms of Pearson correlation and its time-windowed version.

Although some potentially noteworthy correlations appeared, none of them (other than the AMO-PDO relation) seemed strong and stable enough to warrant a specific treatment of inter-predictor links, at least not in the context of purely linear regression. Therefore, in our analysis eventual external forcing-induced components in the indices of internal climate variability modes were treated as a part of these indices; components attributed by the regression analysis to the forcings themselves were then treated as direct responses. To provide a more complete picture of the potential indirect effects of external forcings manifesting through their influence on the internal climate variability modes, results of analysis carried out with just the predictors representing external forcings (solar, volcanic, anthropogenic) will be added to the revised manuscript. Furthermore, the Discussion will be expanded to provide additional references to works addressing the influence of external forcings on the relevant internal climate variability modes.

In the case of the imprints of global temperature in the AMO and PDO (pseudo)indices, please note that the long-term temperature component (in the form of mean northern hemispheric temperature) has been removed from the data during pre-processing (as described in Sect. 2.2), and the AMO/PDO predictors therefore only encompass oscillatory variations around the hemispheric temperature series. This will be highlighted in the revised version of the text.

Due to the sheer amount of possible combinations, results of the cross-wavelet analysis were only presented for selected pairs of predictors/predictands, either those showing interesting interactions, or those intended to illustrate a similarity or contrast in behavior compared to some other pair of variables.

While we agree that partial correlations can offer additional insight into the interdependencies in a multivariable system, their use does not necessarily solve the ambiguity arising from the existence of a common, physically relevant component within multiple explanatory variables, stemming not from a one-way causality, but rather from a two-or-more-way interaction. Such a component cannot be reliably assigned by purely statistical means and since its origin is typically rather complex, we prefer to deal with its presence and interpretation during the discussion of the results. Note also that in the most prominent case of such collinearity in our analysis, related to the similarity between AMO/PDO predictors by Mann et al. (2009), we addressed this problem by application of a simple version of principal component analysis, to provide a more complete interpretation of the role of individual predictors and the components within.

The regression model itself is not explained clearly. From the text it becomes clear that different ENSO indices were used, but which model (which ENSO index) is the one shown in Figs. 4 and 5? Furthermore, only very late in the paper we learn that the explained variances are very low, below 5%. Should we even analyse regression models that have no explanatory power? Finally, the effect of reconstruction uncertainty is not discussed.

Response R1-3: The missing identification of the primary ENSO index will be corrected – it will be explicitly stated that the results in Figs. 4 and 5 are based on the ENSO reconstruction by Li et al. (2011), while the results for the ENSO data by Mann et al. (2009) are only mentioned in the main text.

The seemingly low fraction of variance explained by the regression models (R^2) is a result of dominance of inter-annual variability in the predictand series, matched in the regression mapping against predictors mostly dominated by inter-decadal variation. Formally, higher R^2 could be achieved by removing the year-to-year variations, e.g. by smoothing the series by a moving average filter (to give an example, for the period 1501-2006, 21% of variability of the annual SPEI series can be explained by the regression model if the series are smoothed by 11-year moving average; this value increases to 33% when the NAO reconstruction by Luterbacher et al. (2002) is also included as a predictor). However, since some of our explanatory factors (the episodic volcanic activity, 11-year cycle in the solar variability signal, and the NAO index in the revised version of the analysis) do exhibit faster variability, which would be largely erased by the smoothing, we prefer to perform the analysis with the unaltered series. The prominence of individual explanatory factors is evaluated through statistical significance of the respective regression coefficients, regardless of the overall R^2 – an approach that we believe to be consistent with our primary aim, i.e. identification of forcings and large-scale factors influential in establishing the drought regime of the Czech Lands (as opposed to an attempt to construct a predictive model reproducing the series with as much variability as possible). To better illustrate the actual magnitude of components associated with individual explanatory factors, time series of regression-generated components corresponding to individual explanatory variables will be included in the Supplement accompanying the revised version of the manuscript.

The effect of uncertainties tied to the results would be rather difficult to quantify reliably, as not all series analyzed come with an uncertainty estimate, and methods of its estimation differ even when such data exist. However, due to increased number of versions of some predictors in the revised version of the analysis, more attention will be paid in the revised manuscript to the robustness of the results based on different reconstruction sources.

The paper says little about the mechanisms linking the external and internal drivers to drought and hydroclimatic conditions in general. Obviously a study using reconstructions cannot explicitly address net radiation, soil moisture, temperature effects, land-surface feedbacks, atmospheric circulation effects (blocking), etc. But it would be nice to read the authors' hypotheses. The paper is rather silent about mechanisms. In the introduction Hess-Brezowsky weather types are mentioned, and later the NAO, but the NAO is not incorporated into the analyses and the discussion parts then follows another thread: Doing a PC analysis of AMO/PDO. It would be nice if the Discussion section could come back to mechanisms at some point.

Response R1-4: Note please that our study is dealing with droughts defined through the SPI/SPEI/PDSI indices, shaped by (and calculated from) precipitation and temperature series. Our interpretation of the possible links is therefore focused on the role and eventual interaction of the temperature and precipitation variability in establishing the central European drought regime expressed by the above indices. Also note that some of the responses, while statistically significant, represent rather minor tendencies, difficult to reliably assign to specific mechanisms (especially in our analysis involving pre-instrumental period, as no data exist consistently capturing global large-scale circulation over the last five centuries, making it difficult to evaluate influences related to circulation, blocking, etc.). Even so, we will try to provide more detailed discussion of the possible mechanisms in the revised text, along with additional relevant references (please see also our Response R3-7).

NAO-related effects will be included in the revised version of the manuscript, based on the NAO index reconstruction by Luterbacher et al. (2002) and multidecadal NAO variability reconstruction by Trouet et al. (2009; DOI: 10.1126/science.1166349). The results in the current Figs. 4 and 5 will be updated to show regression coefficients related to NAO in addition to the previously considered predictors; relation between NAO and external forcings (especially volcanic) will be documented and discussed.

Minor comments

Abstract, l. 14: "external and internal climate forcings". Please be careful with terminology here and elsewhere. Considering the coupled climate system "forcing" is used for external influences (subdivided into natural and anthropogenic) while internal variability is used for the dynamics of the coupled system even if unforced. When considering only the atmosphere, "oceanic forcing" is sometimes used. In any case, the terms should be defined and used consistently.

Response R1-5: Terminology in the manuscript will be modified to avoid use of the term 'forcing' for factors originating from internal climate dynamics.

P. 2, L. 5: a substantial number of studies: cite
P. 2, L. 5: A lot of work has been done on droughts in the USA. Perhaps before zooming in on Europe, you could mention that.

Response R1-6: Both comments accepted, corrected by adding new references as follows: "In addition to a substantial number of studies investigating drought indices for the instrumental period in Europe (e.g. van der Schrier et al., 2007, 2013; Briffa et al., 2009; Sousa et al., 2011; Todd et al., 2013; Spinoni et al., 2015; Haslinger and Blöschl, 2017) and other areas of the world (e.g. Dai, 2011; Spinoni et al., 2014; Ryne and Forest, 2016; Wilhite and Pulwarty, 2018), generally calculated …".

Added references:

Briffa, K. R., van der Schrier, G., and Jones, P. D.: Wet and dry summers in Europe since 1750: evidence of increasing drought, Int. J. Climatol., 29, 1894–1905, doi: 10.1002/joc.1836, 2009.

Dai, A.: Characteristics and trends in various forms of the Palmer Drought Severity Index (PDSI) during 1900–2008, J. Geophys. Res., 116, D12115, doi: 10.1029/2010JD015541, 2011.

Haslinger, K. and Blöschl, G.: Space-time patterns of meteorological drought events in the European Greater Alpine Region over the past 210 year, Water Resour. Res., 53, 9807–9823, doi: 10.1002/2017WR020797, 2017.

Ryne, S. and Forest, K.: Evidence for increasing variable Palmer Drought Severity Index in the United States since 1895, Sci. Tot. Env., 544, 792–796, doi: 10.1016/j.scitotenvv.2015.11.167.

Sousa, P. M., Trigo, R. M., Aizpurua, P., Nieto, R., Gimeno, L., and García-Herrera, R.: Trends and extremes of drought indices of drought indices throughout the 20th century in the Mediterranean, Nat. Hazards Earth Syst. Sci., 11, 33–51, doi: 10.5194/nhess-11-33-2011, 2011.

Spinoni, J., Naumann, G., Carrao, H., Barbosa, P., and Vogt, J.: World drought frequency, duration, and severity for 1951–2010. Int. J. Climatol., 34, 2792–2804, doi: 10.1002/joc.3875, 2014.

Spinoni, J., Naumann, G., Vogt, J., and Barbosa, P.: European drought climatologies and trends based on a multi-indicator approach. Glob. Plan. Change, 127, 50–57, doi: 10.1016/j.gloplacha.2015.01.012, 2015.

Todd, B., Macdonald, N., Chiverrell, R. C., Caminade, C., and Hooke, J. M.: Severity, duration and frequency of drought in SE England from 1697 to 2011. Clim. Change, 121, 673–687, doi: 10.1007/s10584-013-0970-6, 2013.

van der Schrier, G., Barichivich, J., Briffa, K. R., and Jones, P. D.: A scPDSI-based global dataset of dry and wet spells for 1901–2009. J. Geoph. Res., 118, 4025–4048, doi: 10.1002/jgrd.50355, 2013.

van der Schrier, G., Efthymiadis, D., Briffa, K. R., and Jones, P. D.: European Alpine moisture variability 1800–2003. Int. J. Climatol., 27, 415–427, 10.1002/joc.1411, 2007.

Wilhite, D. A. and Pulwarty, R. S.: Drought as hazard: Understanding the natural and social context. In: Wilhite, D. A. and Pulwarty, R. S., eds.: Drought and Water Crises. Integrating Science, Management, and Policy. CRC Press, Taylor & Francis Group, Boca Bayton, 3–20, 2018.

P. 2, L. 24: instrumental precipitation series

Response R1-7: Corrected as "The authors demonstrated the importance …"

P. 3, L. 10: What time window was used for the SPI?

Response R1-8: The time window used for the SPI/SPEI calculation was chosen to match the type of the series used as predictand, i.e. 12 months for the annual data, 3 months for seasonal data.

P. 4, L. 3 and 4: I do not understand this sentence.

Response R1-9: The sentence will be changed to: 'As a result, a 100-member ensemble of distributions of monthly precipitation totals for each season and the year was obtained. These distributions were then applied for calculation of indices for every year in the 1501–1803 period.'. Note, please, that this is just a substantially simplified description of the data preparation process, and full explanation can be found in Brázdil et al. (2016a).

P. 4, L. 10: "climate forming agents": rephrase

Response R1-10: Reformulated to 'climate-defining factors'

P. 4, L. 19 and 20: Omit the first part of the sentence, which is unnecessary. Start with "A large part..."

Response R1-11: Accepted

P. 4, L. 26: strong clear?

Response R1-12: Changed to 'clear'

P. 5, L. 1-3: Perhaps cite Fischer et al. GRL (https://doi.org/10.1029/2006GL027992)

Response R1-13: The reference to Fisher et al. (2007; DOI 10.1029/2006GL027992) will be added to the revised manuscript.

P. 5, L. 19: I am a bit puzzled why the authors use the Mann et al. ENSO series. As the authors write (and other authors have also pointed to that), the reconstruction varies mostly on the 8-20 year time scale. Why use it as an ENSO time series then? I would rather use other ENSO reconstructions. Similarly, for AMO and PDO it would be nice to have two indices for each (e.g. Shen et al. 2006 for the PDO, Gray et al. 2004 for the AMO).

Response R1-14: Please note that ENSO reconstruction by Li et al. (2011) was used as the primary descriptor of ENSO phase, and a basis for the results shown in Figs. 4 and 5. ENSO index derived from the Mann et al. (2009) data was only used as an alternative ENSO descriptor and was not found to be associated with any the statistically responses in the drought data. This will be more explicitly stated in the revised text; regression outcomes for Mann et al. ENSO data will be included in the Supplement.

Results based on the PDO reconstruction by MacDonald and Case (2005; DOI 10.1029/2005GL022478) and Shen et al. (2006; DOI 10.1029/2005GL024804) and AMO reconstruction by Gray et al. (2004; DOI 10.1029/2004GL019932) will be included in the revised manuscript and discussed along with the outcomes of the analysis utilizing the originally employed PDO/AMO data by Mann et al. (2009). Regression coefficients will be presented for each version of the predictors; their similarity (or lack thereof, as is the case for the PDO reconstructions) will be discussed with regard to the robustness of the results and the associated uncertainties.

P. 10, L. 34: Is the tendency for wet conditions after volcanic eruption really due to lower temperatures?

Response R1-15: This formulation is meant to reflect the fact that the tendency towards higher values of the drought indices during periods with higher amounts of volcanic aerosol coincides with significant drop of temperature, while no statistically significant change in precipitation is indicated.

P. 11, L. 34: I am surprised that Sutton and Hodson (2005) paper is not mentioned in context with the AMO effect.

Response R1-16: The reference to Sutton and Hodson (2005; DOI 10.1126/science.1109496) will be added to the revised manuscript and discussed.

Anonymous Referee #2

GENERAL COMMMENTS:

Manuscript under revision is an approach to study of drought in Czech Republic area, taking in consideration previous climatic reconstructions, already published, using these informations to generate drought indices based on instrumental records. Analysis of possible relations with different forcing factors is also made to offer general or initial explanation to drougth mechanisms for this Central Europe study area.
Main effort focused to compare rainfall indices and temperatures for long or complete periods. It's a good first approach to drougth phenomena. It open research to study specific events at higher temporal resolution, impacts and responses, etc.

SPECIFIC COMMENTS

+ Title could include temporal dimension of work of manuscript.

> **Response R2-1:** Because we are using the expression "long-term", it is probably not necessary to extend the title for the time span used.

+ Title. Expression "drought" into title is excessively general. A more correct definition of topic developped into manuscript would be "drought indices".

> **Response R2-2:** Accepted and also with respect to a comment of Referee 3 changed to "Long-term variability of drought indices in the Czech Lands and effects of external forcings and large-scale climate variability modes"

+ Lines 28-30. Seasonal and annual precipitation for 1501-1854 is reconstructed from "document-based precipitation indices". Dobrovolvy et al., 2015. Could explain in a short description general characteristics or contents of these "documents"? How was developed previous analysis. Just to have a connection between original information and present results generated into manuscript. IF drought is analyzed, at least public must know about historical documents used for reconstruction, temporal resolution of information obtained, locations or regions with available information, aspects of natural process and/or and human impacts detected/evaluated....etc.... I understand manuscript can have restrictions of extension, but this short overview would be useful for public.

> **Response R2-3:** This comment and several following remarks of Referee 2 concern details related to the documentary data used. We would like to stress that the primary aim of the analysis is the study of forcings and large-scale climate drivers reflected in series of drought indices, described in detail in the paper by Brázdil et al. (2016a). Because their calculations are based on reconstructed temperature (Dobrovolný et al., 2010) and precipitation (Dobrovolný et al., 2015) series, in which a detailed information of documentary data used with their types, examples and critical evaluation are given (as well as the reconstruction uncertainties), it would bring not too much new information to the merit of this article. But looking at the comments of the Referee 2, we included several additional sentences in this direction with hope to fulfill at least partly these requests by the change of the fifth paragraph of Section 2.1 as follows:

"Long-term seasonal and annual series of these three indices, dating from AD 1501 in the Czech Lands (Brázdil et al., 2016a) were used in the current study. They were derived from 500-year temperature and precipitation reconstructions based on a combination of documentary data and instrumental measurements. Documentary data were represented by descriptions of weather and related phenomena coming from different documentary evidence of individual as well as institutional character as annals, chronicles and memories, weather diaries (non-instrumental observations), financial and economic records, religious records, newspaper and journals, epigraphic sources etc. Corresponding data in the Czech Lands cover with a different density particularly the period from AD 1501 to the mid-19th century, but they continue even to the recent time. Also the spatial density of such data is changing with time depending on the availability and extraction of existing documentary sources over the Czech territory. All collected data were critically evaluated with respect to possible errors in dating or spatial attribution and were used for interpretation of monthly weighted temperature and precipitation indices in the 7-degree scale, from which series of seasonal and annual indices were created (for more details of the use of documentary data, their critics, analysis and interpretation as well as creation of indices series in historical climatology see Brázdil et al., 2005, 2010). Such data were further used as a basic tool for temperature/precipitation reconstructions. Firstly, Dobrovolný et al. (2010) reconstructed monthly, seasonal and annual central European temperature series, partly based on temperature indices derived from documentary data for Germany, Switzerland and the Czech Lands in the 1501–1854 period and partly on homogenized instrumental temperature series from 11 meteorological stations in central Europe (Germany, Austria, Switzerland, Bohemia) from 1760 onwards. This temperature series is fully representative of the Czech Lands. Subsequently, seasonal and annual precipitation series for the Czech Lands were reconstructed from documentary-based precipitation indices in the 1501–1854 period and from mean precipitation series calculated from measured precipitation totals in the Czech Lands after 1804 (Dobrovolný et al., 2015)."

+ Bibliography used on work is complete and well updated.

Response R2-4: Thank you.

+ Effort to offer a background or general overview about drought events is not so complete as we would like find. For example, justification of study of drought. It's a present or potential problem for Czech Republic?, any previous strong event justify this study? How they are drougth conditions in Czech Republic?

Response R2-5: To fulfill this comment, the first paragraph of Introduction will be changed as follows: "Droughts, among the most prominent manifestations of extreme weather and climate anomalies, are not only of great climatological interest but also constitute an essential factor to be considered in the assessment of the impacts of climate change (Stocker et al., 2013; Trnka et al., 2018; Wilhite and Pulwarty, 2018). This is valid also for the territory of the Czech Republic where droughts, besides floods, represent the most important natural disaster with significant impacts in different national economy sectors as, for example, agriculture, forestry, water management, or tourism/recreation. Since the Czech Republic lays on a continental divide with rivers flowing out of its territory, it depends on just the atmospheric precipitation for its water supply. Although we know some extreme droughts with important socio-economic and political impacts from the past, such as drought of 1947 (Brázdil et al., 2016c), the studies from the recent years show increasing dryness of the Czech climate in the past 2-3 decades expressed in higher frequency of extreme droughts with significant consequences (e.g. Brázdil et al., 2015b; Zahradníček et al., 2015). The abundance of long-term instrumental meteorological observations in the European area has provided a basis for a number of recent drought-focused studies, revealing complex regional drought patterns and a richness of features observed at various spatial and temporal scales (e.g., van der Schrier, 2006, 2007; Brázdil et al., 2009; Briffa et al., 2009; Dubrovský et al., 2009; Sousa et al., 2011; Spinoni et al., 2015). Along with more rapid variations, these also include long-term variability, such as a distinct trend towards drier conditions, prominent especially during the late 20th and early 21st centuries (e.g., Trnka et al., 2009a; Brázdil et al., 2015b). "

Added references:

Brázdil, R., Raška, P., Trnka, M., Zahradníček, P., Valášek, H., Dobrovolný, P., Řezníčková, L., Treml, P., Stachoň, Z.: The Central European drought of 1947: causes and consequences, with particular reference to the Czech Lands. Climate Research, 70, 161–178, doi: 10.3354/cr01387, 2016c.

Zahradníček, P., Trnka, M., Brázdil, R., Možný, M., Štěpánek, P., Hlavinka, P., Žalud, Z., Malý, A., Semerádová, D., Dobrovolný, P., Dubrovský, M., Řezníčková, L.: The extreme drought episode of August 2011–May 2012 in the Czech Republic. International Journal of Climatology, 35, 3335–3352. DOI: 10.1002/joc.4211, 2015.

+ Historial dimension of drought is not analyzed. Just index values from previous research considered as approach to climatic patterns related to low values of reconstructed precipitation. Drought is not studied by itself as climatic/historic phenomena. This aspect is not negative nor positive. Just it would require any extension of explanations about drougth as climatic pehnomena in introduction of work.

Response R2-6: As explained above, our manuscript concentrates on the explanation of effects of external forcings and large-scale climate drivers on long-term drought indices variability in the Czech Lands. This means that we are really not analyzing "historial dimension of drought" as the referee correctly states because it does not fit to the concept of this paper.

+ No specific drought events are mentioned. No description at least for one event is included into manuscript. Complexity of drought events and related impacts is not described/evaluated. May be authors are preparing other papers with these specific topics?

Response R2-7: The description of any "specific drought event" does not fit to the paper context, analyzing rather effects of external forcings and large-scale climate drivers in long-term drought indices series. Descriptions of specific drought events in the Czech Lands can be found, for example, in Brázdil et al. (2013) or Brázdil et al. (2015b). Moreover, we are currently preparing a paper with working title "Extreme droughts and their human responses in the Czech Lands in the pre-instrumental period" for another journal.

+ No explanation about drought as climatic phenomena. How is considered drought in Czech Republic, what criteria are applied, what instrumental thresholds, duration/extension/severity, different concepts/definitions of drougth, affectation of agriculture.... Any explanation would be useful to understand characteristics and effects for public unknowning these specific details.

> **Response R2-8:** We would like to stress that we are not concentrating in this paper primarily on "drought as climatic phenomena" or "what criteria are applied, what instrumental thresholds, duration/extension/severity, different concepts/definitions of drought, affectation of agriculture", because it was analysed already in many other papers related to the territory of the Czech Republic (for comprehensive overview see e.g. Brázdil et al., 2015b). We are just trying to find how fluctuations in series of drought indices in the Czech Lands are influenced by external forcings or large-scale climate drivers.

+ If drougth is defined only from specific indices (SPI, SPEI...), when we work in historical time, out of instrumental data availability, this topic must be taken with more introductory explanations. A more complete and informative approach about how documentary records detect and define droughts, what they record, what transmit....

> **Response R2-9:** We would like to stress that drought indices are not primarily derived (calculated) from documentary data, but from temperature/precipitation reconstructions based on documentary-based indices series and overlapping instrumental series. For this reason we are of the opinion that comment "A more complete and informative approach about how documentary records detect and define droughts, what they record, what transmit...." could be difficult to follow in the recent concept of our paper.

+ If manuscript is based on previous reconstructions, focused on reconstructed values of mm. rainfall, by total monthly/seasonal/annual resolution, authors must consider they cannot analyze all dimension of droughts. Rainfall indices with positive aspect can cover important drought events, when dry periods are interrupted by strong rainfall events. Knowing what tipe of drougth is under study, these singular aspects could be differenced, generating a better and deeper study.

> **Response R2-10:** We agree with the opinion of referee 2 but we are not analyzing drought on the base of precipitation indices. Precipitation reconstruction was used only as one of two basic series which were used to calculate series of drought indices.

+ Manuscript doesn't show a clear relation of type of documents and type of information rescued and anallyzed.

> **Response R2-11:** As mentioned in Section 2.1, we analyse effects of external forcings and large-scale climate drivers in long-term variability of drought indices series, calculated from reconstructed series of temperatures (Dobrovolný et al., 2010) and precipitation (Dobrovolný et al., 2015). Both these papers contain detailed information about types of documents and information rescued and analysed. Calculation of drought indices was explained in detail in the paper by Brázdil et al. (2016a). From these reasons we do not see as necessary to repeat in detail all these aspects in the present paper.

+ It would be interesting focuse efforts on variability and extreme events of the same variable before to compare with variability of others proxys.

Response R2-12: Aspects reported by the referee (variability, extreme events, …) were dealt in a great detail already in the paper by Brázdil et al. (2016a). We feel it redundant to repeat it here again because it does not fit to the context of the present article.

Anonymous Referee #3

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 encourage 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.

> **Response R3-1:** It is true that using just CO_2 concentration as an approximation of anthropogenic influence oversimplifies the setup. In the revised version of the analysis, aggregate radiative forcing of multiple GHGs (including CO_2, CH_4 and N_2O) will therefore be used instead. As for the inclusion of the effects of (tropospheric) aerosols, their regional effect is difficult to consider in an analysis such as ours, due to high temporal and spatial variability of their concentrations, differences in behavior of different aerosol species and still high uncertainties regarding their effects. Note also that from a standpoint of a

regression analysis, the predictors with and without the aerosol forcing are usually almost identical, as the respective time series are very strongly correlated. For instance, using the Meinshausen et al. (2011; DOI 10.1007/s10584-011-0156-z) global annual concentration and forcing data over the 1765-2005 period, the CO_2 concentration series is correlated with total anthropogenic forcing (representing the aggregated effect of various greenhouse gases as well as aerosols) at 0.995. There would therefore be almost no change of the regression results if different versions of the predictor representing anthropogenic forcing were applied (despite the obvious differences in the physical effects involved). This will be mentioned in the revised manuscript, along with a more detailed discussion of the correlation between GHG forcing and the drought indices and the caveats of its interpretation.

Regarding the Land use land cover (LULC): We are working with drought indices for the whole Czech Lands calculated from reconstructed temperature and precipitation series. The calculation procedure of none of these indices includes information about LULC. Although it can be an important factor deciding about drought severity and particularly its impacts, effects of LULC on country-wide temperature and precipitation should be limited. In this study oriented on long-term temporal changes it seems to be not an important factor helping us as predictor in the regression analysis of drought indices series.

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.

Response R3-2: Our original intention was to concentrate on mid-to-long-range variability in the drought series, i.e. oscillations typically slower than the dominant variability of NAO. Moreover, the strong relation between central European drought regime and NAO phase has been established by various prior studies, hence we considered it to be less interesting for the current analysis. Since both Referees 1 and 3 expressed their interest in the NAO-related effects, in the revised version of the paper, results involving NAO reconstructions by Luterbacher et al. (2002) and Trouet (2009; DOI 10.1126/science.1166349) will be included and discussed (please see also our Response R1-4).

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.

> **Response R3-3:** We seem to be in agreement with the referee; the mention of nonlinear approach was meant to provide a methodological context while also giving rationale for using linear version of regression.

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.

> **Response R3-4:** This is definitely true, and admittedly under-explained in the original manuscript. The inclusion of a trend-like variable (CO_2 concentration in the original version of the manuscript, composite GHG forcing in the revised one) was meant to provide a predictor potentially approximating long-term evolution observed in the drought indices. Naturally, despite the similarity in shape (and thus statistical significance of the link detected for some of the drought indices), the formal relationship does not prove causal relation. While we commented on this in the original version of the text ('Even considering that statistical attribution analysis can only reveal formal similarities and cannot verify the causality of the links detected ...' in the Conclusions, page 13, lines 13-17), and referenced supporting evidence pointing to a physical link between droughts and anthropogenic forcing (the second paragraph of Discussion), the potential for mis-attribution will be more explicitly emphasized in the revised manuscript.

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.

> **Response R3-5:** For the AMO and PDO representations based on the Mann et al. temperature reconstruction, this was actually addressed (in the Discussion section) by employing a simple form of principal component analysis, allowing to better assess the role of the common component in the predictors and of their difference. In the revised manuscript, the respective results will be shown in more detail, including the graphs of the regression coefficients and cross-wavelet spectra illustrating the relation between the drought indices and the principal components.
>
> Please note also that (multi)collinearity of the predictors results in increased variance inflation factor for the regression coefficients (and thus wider confidence intervals). Since this is an inherent feature of multivariable regression, we did not comment on it specifically; the effect can, however, be seen from Figs. 4 and 5 in the original manuscript.

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.

> **Response R3-6:** The analysis of regression residuals was performed when designing the optimum setup for the moving-block bootstrap. The only noteworthy feature (aside from the approximately AR1-consistent persistence structure) was a presence of a weak and rather unstable 22-year-period oscillation (possibly an imprint of the 22-year cycle in solar activity, but inconsistently present throughout our analysis period). This will be mentioned and discussed in the revised version. Graphs illustrating the residual variability will be included in the Supplement, along with residual wavelet spectra.
>
> As for the explained variances and evaluation of the regression results, additional results will be included and discussed in the revised manuscript, including graphs of regression-estimated components pertaining to individual predictors – please see also the second paragraph of our Response R1-3.

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.

> **Response R3-7:** In the revised version, we will pay more attention to the (potential) mechanisms behind the observed links. Note, however, that most of the connections highlighted in our analysis represent rather weak (albeit statistically significant) tendencies, which cannot be unambiguously assigned to specific mechanisms (especially considering that no observational data exist that could be used for analysis of circulation patterns over the period of the last five centuries, and that dynamical models are still rather unreliable in capturing some of the relevant factors, including the sources of multidecadal climate variability). Still, we will try to suggest plausible mechanisms that can be used as initial hypothesis that could be tested by follow up studies, e.g. in the regions (periods) with more comprehensive datasets.

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?

Response R3-8: Temperature and precipitation data were used for calculation of the drought indices themselves (as explained in Sect. 2.1), and their behavior is therefore crucial when discussing their combined effect in the drought descriptors.

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.

Response R3-9: We did not mention PCA in the Methods section, as it was only used as a supporting technique in a small part of our analysis (and we assumed its general principles to be a common knowledge, thus not requiring introduction). In the revised version, use of PCA will be mentioned in the Methods section; the results based on analysis of principal components will be presented directly instead of just mentioned in the text (please see also Response R3-5).

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.

Response R3-10: The results originally just mentioned (but not shown) in the manuscript will now be included fully, either in the main paper or in its Supplement. The Introduction and Discussion will be modified to paint a clearer picture of our main objectives: to assess the existence of links between Czech drought indices and climate forcings or activity of large-scale internal variability modes, and to investigate the properties of the existing reconstructions (for both the target and explanatory variables), especially with regard to the uncertainties tied to these series in the pre-instrumental era. To this end, additional results and references will also be included in the revised manuscript (some of them detailed in Responses R1-4, R1-14 and R3-6).

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. ' Longterm variability of droughts in the Czech Lands due to external forcing and large scale climate drivers' ?

Response R3-11: Based on this comment and suggestions of Referee 1, we changed the title to "Long-term variability of drought indices in the Czech Lands and effects of external forcings and large-scale climate variability modes"

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.

> **Response R3-12:** The terminology will be changed in the revised version of the text.

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).

> **Response R3-13:** Corrected to "Missing monthly precipitation totals in …"

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 in this work?

> **Response R3-14:** In order to express various sides of droughts, there exists a great number of different drought indices. SPI, SPEI and PDSI represent those which are used in description and quantification of droughts most frequently, and they are also used for estimating impacts of agricultural and hydrological droughts. While there are obvious similarities between the respective time series (due to precipitation sums being the key factor shaping all of them), each of the indices represents slightly different approach. As mentioned by the referee, these are briefly summarized in Sect. 2.1, along with references to more comprehensive sources. Based on the differences found during our regression analysis, their individuality seems strong enough to justify inclusion of all three indices.
>
> Reference to Fig. 1 will be added to the text, to the Drought indices section.

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.

> **Response R3-15:** We are grateful for the suggestions; in the revised version of the analysis, a recently published TSI reconstruction by Lean (2018; DOI 10.1002/2017EA000357) will be used to represent solar activity (providing full coverage for the 1501-2006 period). The previously employed TSI data by Coddington et al. (2016) will be retained as an alternative solar-related predictor.

SC6 Section 5 L 30 '... the increase in the ambient CO2 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 CO2 in the industrial period is just one degree of freedom. Please recall the comments GC1b

> **Response R3-16:** True, but note please that while we mentioned the existence of a correlation, we did not interpret it as a causal relation, only noted the possibility of one (please see also our Response R3-4).

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.

> **Response R3-17:** Indeed, in simple linear regression, standardized regression coefficient corresponds to correlation between predictor and predictand (and, in absolute value, to square root of the coefficient of determination). In multiple regression, however, no such straightforward relation exists for individual predictors. Standardization of the coefficients is used to make them more comparable mutually (among different predictors as well as among predictands), thought, admittedly, this representation does not directly convey information about the magnitude of the responses. In the revised version, the responses will therefore be also shown in the form of predictor-specific time series generated for selected regression configurations (the respective graphs will be included in the Supplement of the paper).
>
> As for covariance of the predictors, its effect is reflected in the size of the confidence intervals for individual predictors (please see also response R3-5). This aspect of multiple linear regression will be emphasized in the revised version of the manuscript, within the expanded discussion of the effects of (multi)collinearity of the explanatory variables.

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

> **Response R3-18:** Selected example corrected. The English of the manuscript was checked and corrected by a native speaker, Mr. Tony Long. The language correction will be repeated in the revised text once again.