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

**Anonymous Referee #1**

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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?

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.

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.

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.

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.

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.

P. 2, L. 24: instrumental precipitation series

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

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

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

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

P. 4, L. 26: strong clear?

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

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).

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

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

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