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Interactive comment on "Two types of North American droughts related to different atmospheric circulation patterns" *by* Angela-Maria Burgdorf et al.

Anonymous Referee #2

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Review for Burgdorf et al., "Two types of North American droughts related to different atmospheric circulation patterns"

Burgdorf and colleagues, motivated to better understand drought forcing in North America, use the LBDA and EKF400 datasets to relate multiyear droughts in North America (via LBDA) to their synoptic circulation drivers (via EKF400) over a sufficiently long record to make robust claims.

The authors rely on clustering analysis of multiyear drought events (5-yr running mean on the standardized PDSI values) to identify their prevailing spatial patterns. They find two dominant modes of soil moisture anomalies (consistent with previous findings),

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Discussion paper

and building on work, are then positioned to link those patterns to their atmospheric drivers via the EKF400 data assimilation product. They find (generally consistent with the previous literature) that particular configurations of ocean-atmosphere variability select for different drought types.

Overall the paper appears to be in a position to make a nice contribution. I have a few larger comments and some minor ones the authors might find helpful in a revision.

Major comments:

1. How does the spatial domain presented (page 5, line 4) influence the clustering of the drought events and thus the spatial patterns presented? Presumably the clustering is quite sensitive to the domain selection and its odd that Figs. 1 and 5a use a constrained North American domain to show the drought patterns, while the rest of the analysis puts North America more fully in perspective. The LBDA v1 on which the central analysis is based, encompasses all of North America, so I wonder why the authors chose to constrain their analysis in such a way, particularly given their emphasis on both pattern identification (which is likely domain-sensitive) and synoptic scale circulation on such drought events. I recognize the authors' point at page 3, line 31 about version 2 being more limited spatially. But since the 20th C. drought is dropped from the central analysis, why not expand the domain to encompass all of NA, or at the least, all of CONUS (as in Fye et al.)?

2. I wonder about the comparison of EKF400 anomalies to the LBDA anomalies: do they share the same standardization intervals? I *believe* the LBDA is standardized relative to the instrumental period of 1931-90 (could be wrong here), and then the authors here take the 5-year running mean to define a drought event (plus a spatial scale threshold). The GPH analysis is just relative to +/- 5 years centered on the drought. I wonder if the atmospheric fields should first be centered to the same interval as the LBDA and then those EKF400 anomalies can be composited with the +/- 5 year approach. This may make some more consistent GPH, T2M, and SLP patterns

emerge.

3. Are the EKF400 T2M data representative of ocean skin temperatures? Certainly SSTs and T2M should share the same variations over climate timescales, but some kind of validation of that, or just using SSTs over ocean basins would be more sound for making claims about oceanic forcing.

4. It strikes me as a pretty large missed opportunity to not also leverage the PHYDA in this work as a check on EKF400 results, given the uncertainties in the latter that the authors concede (e.g., page 6, line 33) and the authors' search for robustness. As I understand it, EKF400 simulations are forced with SST reconstructions that use a number of the same proxies that are also then used in the data assimilation process itself, which seems potentially problematic. The authors' ability to make robust claims about wave trains, jet positions, and SSTs would be greatly enhanced if there is consistency across more than one atmospheric reconstruction, which is now publicly available.

5. Updating the Fye et al. paper seems to be a central motivation in this work and there are places where contrasts are drawn between the findings here and those in Fye, which is interesting, but it would be great to have the reasons for those differences explained or hypothesized about a bit more.

6. Finally, the outlier pattern associated with the most recent drought is really quite compelling as the authors suggest this one is anomalous based on their pattern clustering. Are there any droughts in the original two clusters (Dust Bowl and 1950s) that look somewhat like the modern drought? Some more validation of that finding would be really great. Could it be a methodological artifact due to its being in version 2 and not 1, and the need to put v2 (PMDI) on equal footing with v1 (PDSI)? It might be easier to drop this from the paper and do a more rigorous treatment of it in a separate analysis.

Minor comments:

1. P2, L27: You cite internal variability here; a recent Cook et al. 2018 paper ("Revisit-

ing the Leading Drivers of Pacific Coastal Drought Variability in the Contiguous United States," Journal of Climate) shows that there are numerous ocean-atmosphere configurations that can give rise to the same drought pattern in the West Coast of North America.

2. P5, L5: is the PDSI < -1 consistent with Fye?

3. P5, L10: point to the Supplemental Figures here?

4. P6, L9: You discuss two drought types, but in this and the subsequent sentences you reference three.

5. Online it's fine, but in a print out, Fig. 2's color bar is difficult to discern.

6. P6, L29: Are these statistical tests on the patterns of droughts or the jet positions? As written it's not clear. (Seems like it should be on the jet positions.)

7. the quotes around "Dust Bowl" and such are upside-down(?); usage of e.g. requires a parenthetical, etc.; please just check the manuscript for the typos throughout.

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