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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessment of the influence of bias correction on meteorological drought projections for Poland" *by* M. Osuch et al.

Anonymous Referee #1

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1 General Comments

The authors present a trend analysis for future projections of seasonal precipitation based on the meteorological drought index, SPI, for Poland. Projections are based on an ensemble of RCM runs, providing high spatial resolution. The projections show an overall increase in precipitation during the winter and a slight decrease in precipitation during the summer, with some model disagreement. The effect of bias correction on these projected trends was evaluated and found to have a small effect, but which is smaller than the variability among GCM/RCM model combinations.

The paper is extremely well-written, clear, and easy to understand. It provides high

resolution projections and a non-parametric trend analysis of seasonal precipitation for Poland, which is worthy of publication, and asks an interesting research question – whether bias correction affects projections of the drought index, SPI. However, I have two major issues relating to the lack of a focus on drought and insufficient testing regarding bias correction. These are described below. Because of these fundamental issues, I recommend a major revision.

2 Major comments

I have 2 primary issues with the paper:

1. The paper claims to be measuring trends in drought and discusses meteorological drought throughout. While the authors use the SPI, a drought index, they measure trends across the entire range of SPI values, which includes both wet and dry anomalies. Thus, the paper really deals with trends in seasonally accumulated precipitation, or general dryness/wetness. For example, extreme rainfall (SPI > 1) events increased in severity or frequency, while drought events (SPI < -1) remained the same, the trend would show an overall increasing trend in SPI, which the authors would incorrectly classify as a decrease in droughts. While overall wetness and droughts are potentially related, they are different and do not have to respond in the same way.

   The authors cite the study by Rimkus et al. (2012) which did specifically measure droughts, looking at trends in drought "intensity", defined as the sum of negative SPI values for a region. They later begin defining drought thresholds (Page 10341, Line 1), but this is never mentioned again.

   My recommendation is either to (a) change the title and text to reflect a focus on accumulated precipitation, or (b) focus analysis on drought occurrence, either

based on area below a threshold or the sum of SPI below a threshold. The results shown here are interesting in their own right, so either choice would be acceptable.

2. The title and much of the text focuses on the effect of bias correction on trends in SPI. I have serious questions with this premise and the conclusions that bias correction has a slight effect on trends in SPI values (Page 10336, Lines 8-11; Page 10350, lines 3-8; Section 3.3). SPI is a normalized index based on quantiles, though it uses a gamma distribution rather than the empirical cumulative distribution to calculate them. Thus, SPI uses a similar quantile fitting procedure as bias correction and thus bias correction should have nearly negligible difference. This can be seen in Figure 10, where the differences in significant trend areas are generally within 10% and are generally centered around 0 (except February). The only effect from bias correction should be due to (a) distribution fitting differences, (b) differences at the very extreme values, or (c) the difference between summing months first and normalizing (no bias correction) and first normalizing, summing, and then normalizing again (bias correction). The examples provided (e.g. Maurer and Pierce 2014) deal with bias correcting precipitation, rather than a relative metric like SPI, which is a very different question.

   Comparing differences between trends in bias-corrected and non-bias corrected SPI values skips the important step of determining whether there is a significant difference in SPI values themselves between the two. Given the above explanation, I doubt there is. In order to support your claim, I recommend quantifying the difference in corrected and non-corrected SPI time series using metrics like correlation, mean squared error, or mean absolute error.

3 Moderate Comments

1. Title: Based on the above comments, I recommend adjusting the title to focus more on overall dry/wet trends, rather than on drought and bias correction.

2. Page 10341, Line 12: It would help if you distinguished between the reference period for bias correction (1971-2000) and the reference period for SPI normalization (1971-2099). It might also be helpful to add these reference periods to Figure 1 to help make this distinction.

   Tied into the issue of reference periods is your claim that it is better to use the entire period (1971-2099) to normalize SPI values based on Wu et al. (2005). By using the entire time series as a reference period, you force the SPI values to follow a normal distribution; however, it causes difficulties in interpretation when there is a detectable trend in SPI values.

   For a stationary timeseries, an SPI of 0 means that precipitation is near the median value of the reference period. But, for a non-stationary time series, this refers to the median value along the trend.

   For instance, if SPI was calculated based on a historical time series (e.g. 1971-2000), an SPI of 0 would mean that precipitation was “typical” based on the reader’s experience. But, using the full time series (1971-2099) with a linearly increasing trend, “typical” conditions should occur sometime around 2035. What the reader considers typical, i.e. historical and current climate conditions, would actually be considered drier than typical, with SPI values less than 0. As stated above, both reference periods allow for a valid analysis of trends as shown in this study, but there may be difficulty with interpretability moving forward.

3. Figure 10: This figure is unclear. Is this a stacked bar graph? If so, each GCM/RCM combination is independent and should not be added together. If they are not being added together, then showing them stacked is confusing. A

simple line graph showing each GCM/RCM's progression through time would be more readable.

4. Discussion of the results should be expanded. The authors list several papers in the introduction that deal with climate projections and precipitation in Europe. The results show a consensus for wetter winters and generally drier summers, though there is more uncertainty in the summer. How does this compare, for instance, with Rimkus et al. 2012 or Liszewska et al. 2012? You may also compare with results from additional studies listed in the minor comments.

4 Minor Corrections

1. Page 10333, Line 10: This should be "intense", not "intensive".
2. Page 10334, Line 26: Because you have access to climatic water balance, it would be interesting in future studies to calculate trends in SPEI (Vicente-Serrano et al. 2010) and compare results to the SPI, a precipitation-based metric. This is not needed for this study, simply a suggestion for the future.
3. Page 10334, Lines 23–26: There are some additional studies that attempt to project meteorological drought in Europe, either using coarse resolution (GCM) or high resolution (GCM/RCM). I suggest you consider some of the following:
    - Blenkinsop, S. and H. J. Fowler (2007): Changes in European drought characteristics projected by the PRUDENCE regional climate models. International Journal of Climatology 27(12):1595-1610.
    - Dai, A. (2013): Increasing drought under global warming in observations and models. Nature Clim. Change 3: 52–58.

    - Orlowsky, B. and S. I. Seneviratne (2013): Elusive drought: uncertainty in observed trends and short- and longterm CMIP5 projections. Hydrol. Earth Syst. Sci. 17(5):1765-1781.
    - Stagge, J.H. , Rizzi, J., Tallaksen, L.M., and Stahl, K. (2015). "DROUGHT-RSPI Technical Report No. 25 Future Meteorological Drought Projections of Regional Climate" DROUGHT-RSPI Project .
4. Page 10335, Line 5: Hydrological drought may also refer to deficits in groundwater or reservoir storage.
5. Page 10338, Line 4: The authors should mention that the scenarios are based on AR4 SRES scenarios (presumably) and not the RCP scenarios. This is not a problem, but should be mentioned in the methods.
6. Page 10340, Lines 11-17: I appreciate the desire to cite all of this research, showing the importance of the SPI. But, I think this is citation list is a little excessive. I recommend trimming it to the most important references
7. Page 10341, Line 26: These papers discuss the use of normality testing to validate SPI values and check whether zeros cause a failure. They may be useful to cite:
    - Kumar MN, Murthy CS, Sesha Sai MVR, Roy PS. 2009. On the use of Standardized Precipitation Index (SPI) for drought intensity assessment. Meteorol. Appl. 16 : 381–389, doi: 10.1002/met.136
    - Stagge, J. H., Tallaksen, L. M., Gudmundsson, L., Van Loon, A. F. and Stahl, K. (2015), Candidate Distributions for Climatological Drought Indices (SPI and SPEI). Int. J. Climatol., 35: 4027–4040. doi: 10.1002/joc.4267
    - Wu H, Svoboda MD, Hayes MJ, Wilhite DA, Wen F. 2007. Appropriate application of the standardized precipitation index in arid locations and dry seasons. Int. J. Climatol. 27 : 65–79

8. Page 10342, Line 15: It would be good to mention in the text that the Mann-Kendall test operates based on all possible combinations of points. This is mentioned for the Sen slope (Page 10343, Line 17), but should be introduced earlier in this section.

9. Page 10347, Line 4 and elsewhere: You refer to figures out of order. In this case, you cite Figure 14 well before Figures 8-13.

10. Page 10351, Line 6 and elsewhere: Please be specific regarding the subset you are analyzing for longer duration SPI's. For instance, the SPI 12 is the annual time step, but it appears you are only considering the SPI 12 in December. The full SPI12 time series is a moving window that moves forward monthly (or daily), always looking back 12 months. I assume you are also using December for the SPI24, which should also be specified. The discussion of SPI3 is adequate, stating that you extracted values for February (DJF), May (MAM), August (JJA), and November(SON).

11. Table 1: I recommend using two column headings, one showing GCM and another showing RCM. By grouping the trends by GCM, it would be easier to look for trends among the forcing time series.

12. Figure 7: Similar to my comment for Table 1, it would be helpful if these models were organized by GCM, rather than alphabetically to see how the GCM forcings differ and how the RCMs modify the forcings.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 10331, 2015.