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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 #2

Received and published: 17 November 2015

General Comments

The authors present an analysis using the Standardized Precipitation Index (SPI) to assess future trends in meteorological drought in Poland. They use high resolution climate simulations of the ENSEMBLES project of six different RCM/GCM combinations under the A1B emission scenario. The results show a positive trend of the SPI in winter and a slightly negative trend in summer. Additionally, the effect of bias correction on the trend signal is only weak. However, the spread between different model realisations introduces much more uncertainty. The paper is well written und structured. It provides information on future SPI trends and also on the very important topic of the effects of bias correction on the results. In general I would recommend publishing the paper in HESS, however, some major and minor comments are summarized below and should

be taken into account.

Major Comments:

The authors use the linear trend of the SPI time series as a change indicator for meteorological drought occurrence in the future. I think, although the trend estimator is a very robust one, that the approach introduces some uncertainty and difficulty in interpretation. In the results maps are displayed showing the slope of the linear regression of the SPI values against time, indicating whether the SPI shows a negative trend (→ interpretation is increase in droughts) or a positive one (less droughts). These plain numbers make it hard to assess the magnitude of change. The SPI is a probabilistic drought index, indicating the chance of a certain precipitation amount to occur. For the reader and also for a deeper justification of the title of the manuscript (meteorological drought) it would be worthwhile to assess future drought occurrence in a more profound way. One possibility would be to fit the Gamma-distribution of the precipitation time series only in the reference period (1971-2000), but calculating the SPI for the whole time series (1971-2099). That would enable to assess possibly changing probability of drought occurrence (e.g. SPI below -1, or even -2) in a future time period (2070-2099) compared to the reference period, which should follow a unit normal distribution. I think the manuscript would benefit, if these kind of analysis is added. For examples two figures for winter and summer might be added to the results, or even to a Discussion section, although not existing. This is an additional point I'd like to make, that I think the manuscript would benefit from adding a Discussion section, adding a critical discussion on bias correction, possible introduced uncertainties thereof and the necessity for bias correction in the light of the presented results (Maybe section 3.3 could be included in a Discussion section and also some parts of the Conclusions). There is also much literature cited in the introduction. The Discussion section should pick up the main findings of these and discuss them in the light of the apparent results.

Personally, I think no matter how large the biases from the model data are, the differences between raw and corrected SPI should not be too big, since calculating the

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SPI is some kind of quantile fitting as is the quantile mapping. As the first reviewer commented, the differences between raw and corrected SPI might come mostly from differences in the fitting of the distributions and/or differences in the extreme values, which is particularly of concern in quantile mapping.

Minor Comments:

Page 10332, Lines 1-2: Suggestion: "...drought severity in Poland are estimated applying an ensemble of six climate projections using..."; The ENSEMBLES project is described later and there is no need to introduce this abbreviation in the Abstract

Page 10332, Line 3: "...six different RCM/GCM runs..."; please also aim to avoid abbreviations in the Abstract. If it is ultimately necessary write the full name and the abbreviation in the Abstract and at that point in the text where it first appears.

Page 10332, Line 7: "... spatial resolution of 25 km for the..."

Page 10332, Line 9: delete "25 km x 25 km"; "...projection and timescale. Additionally, results obtained..."

Page 10332, Line20: change "with different" to "driving different"

Page 10333, Line 20 – Page 10334 Line4: Just state shortly what Rimkus et al. (2012) found out. Shift most of the text to the Discussion section and discuss it in the light of your findings.

Page 10334, Lines25-26: "or drought indices such as the climatic water balance, that are insufficient for adaptation purposes." Please clarify these statements: what is the climate water balance drought index? Do you mean the SPEI? Then you will have to add a reference (Vincente-Serrano et al. 2010). Why is it insufficient? Can you justify this statement?

Page 10334, Line29 – Page 10335, Line2: Merge this sentence with Page 10335 Lines 14-16, since there is much redundant information.

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Page 10338, Lines 4-10: Instead of listing all simulations in the text a small table would give a much better overview of the different runs and the RCM/GCM combinations.

Page 10338, Line 15: E-OBS is not a reanalysis in the usual climatological sense (like the ERA-40 or NCEP dataset). I would consider writing "E-OBS gridded observation data", or simply "E-OBS data". See also Line 27 on that page.

Page 10339, Line 5: Dosio and Paruolo (2011) and Gudmunsson et al. (2012)

Page 10339, Line12: Please specify the threshold you applied for wet/dry day distinction.

Page 10340, Lines 11-17: Please only cite the most important studies in the light of your investigation. This list is rather long.

Page 10340, Line 21: This is a rather sloppy formulation. Of course other distributions can be used, but what are the implications? When or where do I use other distributions?

Page 10341, Lines 10-13: This statement is not clear to me, please rephrase.

Page 10344, Lines 3-5: Delete paragraph. It is not necessary.

Page 10345, Line 4: rephrase: "...precipitation intensities are simulated by RCMs driven by ARPEGE."

Page 10346, Line 16: raw should be row.

Page 10347, Line 4: Fig. 14: Please stick to the order of the Figures referenced in the text.

Page 10347, Line 12: Why did you choose exactly this station? Could you please justify this decision?

Page 10349, Line 18: rephrase: "...depends on the climate model and month under consideration."

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Page 10349, Lines 19-20: rephrase: "...of simulated data, therefore the most intense bias correction is applied in that case."

Page 10350, Lines 22-29: Where are these results shown? (Table, Figure)

Page 10353, Line 11: Why the "first six months"? Where is the justification for this? I would rather suggest using the four "core" months of the seasons: January, April, July and October.

Page 10354, Lines 4-6: Please add a reference to this statement.

Page 10354, Line 20: Reference of Maurer and Pierce (2014): the authors of this study analysed precipitation, not a precipitation index. This is a complete different thing, so I think this reference is not valid for the given statement.

I could not find a reference in the text for Figure 7.

Figure 10 is a bit confusing. You produced a stacked bar chart, which is not appropriate in my opinion. A better way would be to draw the bars separately, grouped by month, or to have a line chart with one model representing one line in different colours.

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