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Discussions

Interactive comment on "Using an integrated hydrological model to estimate the usefulness of meteorological drought indices in a changing climate" *by* Diane von Gunten et al.

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We thank the reviewers for their relevant and useful comments. We are confident that we can adequately address each of these comments and that the revised paper will gain from the discussion. Please find below our response to each of the reviewers' comment.

1 Comments of the reviewer 1

1. **What I find somewhat contradictory in this respect is the low temporal res-**

olution in the applied correlation and regression analysis (and therefore averaged dynamics). The authors use annual hydrological variables. I think that for drought analysis and better understanding drought propagation in the future a sub-annual resolution (at least seasonal dis-aggregation) would be highly desirable. Put differently, how much can we infer from an annual average relation between meteorological drought and streamflow or groundwater levels in contrast to e.g. seasonal data, especially when thinking about water management and planning issues?

   We are aware that seasonal and sub-annual scales are essential for drought prediction and water management (e.g., Kumar et al., 2016). However, we have conducted our analysis at the annual time scale in this paper because some of the drought indices such PDSI or RDI cannot be directly applied at the seasonal scale. Moreover, the central theme of this paper is the difference between the correlations coefficients (which are similar in all studied climate scenarios) and the model bias/RMSE (which depend on the climate and irrigation scenarios). To study these differences, annual time scale is adequate in our point of view. Indeed, a re-analysis at the seasonal time scale would not change our main conclusion (the need for a hydrological model) but it would unnecessarily lengthen the paper. Finally, the annual timescale is used in many of the published papers on future climate-change impacts (e.g., Kirono et al., 2011; Park et al., 2015), and we wanted to ensure comparability with previous research efforts on this topic. Hence, we have decided to present our analysis for the annual scale in this study.

2. **A concern that is somewhat related is that very little information is provided about hydrological processes in the catchment (now and changes in the future) and how they relate to differences in the linkage to drought indicators. Is enhanced ET the only factor? I would appreciate to see time series of modeled precipitation, ET, future discharge, groundwater levels etc. for a more process-based picture of the link between a precipitation**

decrease/ET increase and hydrological drought. This may also help to understand how generalizable the findings from this unique catchment are.

Thanks for this remark. Part of the information was provided in our recent paper (von Gunten et al., 2015). However, more information on local hydrological processes would help to clarify and interpret our results. We will provide more hydrological data in the revised version, such as precipitation, PET, Q, etc. In addition, we will add a discussion about how our findings are related to the hydrological regime and about the potential for generalization.

3. **Regarding the paper presentation, the paper is well written and clearly structured. However, the manuscript would benefit from shortening the methods section (suggestions see below). Although I appreciate the attempt to be very transparent, currently almost 9 pages present methods, and only 5 results and discussion, which seems a bit imbalanced.**

   We agree with the reviewer. We will shorten the method section in the revision and move some paragraphs to the appendix.

4. **"We conclude that meteorological drought indices are able to identify the timing of hydrological impacts of droughts in present and future climate." I am bit concerned about the general inference on timing between the two variables looking at annual averages. What about e.g."summer flash droughts" and intermittent heavy rainfall (likely leading to enhanced surface runoff and less recharge) versus a continuous seasonal dry period versus wetter period? Wouldn't the annual average response be similar, but the dynamics between meteorological and hydrological drought and thus water availability and implications for management be different at shorter time scales?**

   Our results are only valid for the annual timescale. We will reformulate the abstract (notably the highlighted sentence) to clarify this point. We have decided

to focus on drought effects at the annual time scales because it is the adequate timescale for analyzing long-term changes such as climate-change impacts (cf. answer to issue #1)

5. **Since you provide an overview of the methods in section 2.1 some of the later information is a bit redundant and could be heavily shortened.**

   Yes, we will shorten the Section 2.1.

6. **P 5, L 6-13: Is this needed in this detail?**

   We will shorten this part also.

7. **Study area: since you provide a detailed description of the basin a link to changes in catchment processes in the future may be interesting to pick up in the results/ discussion.**

   A discussion on this topic was already presented in von Gunten et al. (2015). Hence, we kept this part brief to avoid repeating ourselves. We will nevertheless expand the discussion for relevant catchment processes.

8. **Climate scenarios: Could this be shortened and potentially merged with the results 3.1 section (since this section contains quite a bit of methodology in my view)?**

   Yes, we will merge the two sections (3.1 and 2.4). We will also shorten Section 2.4 on the climate scenarios, and the method section in general.

9. **Irrigation scenarios: Where does irrigation water come from? Surface water, groundwater abstractions, reservoirs, are there any water transfers?**

   The irrigation water is surface water from the Yesa reservoir, which is situated about 80 km north of the catchment, at the foot of the Pyrenees mountains. This information is already given in the introduction (p.4 line 3), but we can repeat it in the paragraph on the irrigation scenarios. We did not assume any limitation on irrigation water.

10. **Drought indicators: This section could be strongly condensed. Do you really need the introductory part (P8, L13-30)? P, L23-30: this could go into the discussion section. SPI/SPEI/PDSI are all frequently used. I therefore suggest making reference to existing papers and keeping these methods brief.**

    We agree with this comment. Consequently, we will move the description of the drought indices to the appendix. This will shorten the method section considerably.

11. **Computation of potential evapotranspiration: Could some of the details go into an appendix?**

    Yes, we will shorten this part and move some of the information to the appendix.

12. **Methods of comparing the drought indices to predict hydrological variables: Which model are the future drought indicators based on for predicting the hydrological response (e.g. shown in Figure 7)? I assume it is average of the outputs of the four regional climate models as in Figure 6 bottom panel but this information should be given in this section.**

    Yes, it is the average of the four climate models. We will add this information to the revision.

13. **I would suggest presenting a relative bias rather than an absolute one. In the results you also set the absolute values into context (e.g. P9, L21:"the largest bias is equivalent to only 3.9% of the present water deficit").**

    We agree that presenting the relative bias makes our results more accessible. We will change the figure as proposed.

14. **Figure 3: There are seasonal differences, which is why I think information may be lost when only looking at annual averages for the correlation/regression analysis**

    Yes, we agree that there are seasonal differences in the correlation coefficients. But our goal was not a general analysis of bias introduced by correlation coefficients. Please refer to our responses above regarding the choice of the annual time scale (answer to issue #1). We will nevertheless note this limitation in the revised text.

15. **Section 3.2: P14, L13:"details are available in the supplementary material": where do I find this?**

    We sent the appendix with the paper. But we did not check if it was available. We will make sure that it is sent to you with the revision.

16. **Figure 5: Is the irrigation scenario a mean of PIRR and FUTIRR or just PIRR?**

    We used the irrigation scenario PIRR in the present and FUTIRR in the future. We will clarify this in the label of the figure.

17. **I don't fully agree that the correlation coefficients are all similar, as you write.**

    Thank you for pointing this out. This point was not described clearly enough. We meant that the correlation coefficients linked with a particular drought index were similar in the present and future climate, not that the correlation coefficients were similar for all drought indices. For example, the correlation coefficient between SPI and Q is similar for all climate/irrigation scenarios. But the correlation coefficient between EDI and Q is different than the one between SPI and Q. We will modify this paragraph to clarify our point.

18. **How do you explain EDI <0.5? EDI performs especially poor when considering the ETHZ model - any ideas why?**

    We have various hypotheses, but they have not been investigated in detail. A possibility is that intense precipitation events, which are common in summer in the Lerma catchment, create outliners in the effective precipitation used by EDI. These outliners have large values, so a large impact on the correlation coefficients, but they have a low correlation with drought conditions, which could decrease the overall correlation. As requested, we will address this issue in the revised manuscript in more details after additional investigations.

19. **I think if you decrease the panel size there would be enough space for including the correlation coefficients with water deficit and groundwater head.**

    We did try to plot all the correlation coefficients in the same figure before and it is indeed possible. We have provided this figure with all the correlation coefficients (Q, heads, and water deficit) in the appendix. So the proposed figure is part of the paper and the reader will have access to it. However, we did not include this figure in the main text because the figure is somewhat difficult to grasp in a short amount of time and because it would distract the reader from the major points of our study.

20. **If you start out with three hydrological variables (including hydraulic heads), I would like to see this reflected in this section but currently there is no information about hydraulic heads in the presented material.**

    The correlation coefficients between hydraulic heads and drought indices strongly depend on the position of the wells (see the Figure 1 of the supplementary material). Based on our initial investigations, the model bias is also highly dependent on the well position. The hydraulic heads of one well can react very differently compared to the heads from another well. This makes the interpretation of the

various combinations of heads and drought indices complicated. Indeed, there are 12 wells and 7 drought indices. So we need to study 84 different cases to reach some conclusions and these conclusions would only be valid for these particular wells. Hence, the analysis might not be really useful for the reader. Consequently, we have decided to not analyze hydraulic heads further.

21. **Figure 6, right panel: you write that"the relationship between SPEI and discharge is relatively stable in different climates". I find it hard to distinguish the pink from the red dots but to me the slope of the pink or red dot relation looks higher than for the present regression line? Have you considered comparing/plotting regression coefficients for the different indicators and scenarios to go beyond this one SPEI example scatter plot?**

    The goal of this figure is to show that the curves in the aforementioned case are stable *compared* to other cases. We wanted to illustrate the impact of the irrigation scenarios and the range of the possible outcome of our analysis. We will provide the regression coefficients in the revised version of the paper to simplify the comparison. In addition, the model bias (given in the next figure) can be used to compare the curve quantitatively. We will also modify the colors of the figure.

22. **Figure 7: Since you have different units for your hydrological variables and to better relate it to the present scenario I would prefer relative over absolute values for model bias.**

    We will present the relative value of the model bias in the revised version. It will hopefully help the comparison.

23. **What about displaying model bias for groundwater head in Figure 7 in addition? What can you infer from the analysis of this variable?**

    For the hydraulic heads, the main conclusion is that the response largely depends on the well localization (see issue #20). Therefore, we should show the model bias for the 12 wells to provide accurate information. It would be a lot of information for one figure. Hence, we prefer to restrict our analysis to discharge and water deficit.

24. **Section 3.4: I am curious about the underlying drivers of the differences between models regarding drought intensity. It seems worthwhile to add some explanations into the discussion section.**

    This is a very interesting question but a detailed analysis would go well beyond the scope of our study. Therefore, we will only add some comments on this subject in the discussion, based on the available literature.

25. **General: To condense the results section you could omit a few sentences repeating/ explaining methods or introducing figures since the figures are well readable (examples are: P14, L28-31; P15, L11-13).**

    We will shorten these particular paragraphs.

2 Comments of the reviewer 2

1. **However, giving details ended up with a long Methods Sections. As seen, the Methods Section (Section 2) consists of 9 pages of the 19-page paper. Hence, one of the recommendation is moving the whole sub-parts of "Drought Indices (2.6.X)" in the Appendix.**

    We agree with the reviewer. The method section is indeed too long. We will follow the suggestion to move the definition of the drought indices to the appendix and we will shorten our method section during the revision

2. **These are my recommendations, the authors may (or not) follow these:**

    - **The formula of the Penman-Monteith equation may be given in the Appendix.**
    - **I am not sure how much do we need the details of the Person's correlation coefficient. If the authors want to give it, it may be given in the Appendix.**

    We will add the Penman-Monteith equation to the appendix. It is a central equation in this paper and it is therefore useful to ensure that the reader has access to it. We will also shorten the paragraph on the Pearson's correlation coefficient and move it to the appendix.

3. **I was curious about the current irrigation usage, and noticed that the irrigation usage is enormous. The irrigation from the Aragon River collected at the Yesa reservoir in 2011 is 2* 10^6 m^3. Size of the irrigated portion is 3.54 km^2 from von Gunten et al. (2015). Hence the irrigation depth is 593 mm. On top of this number, mean annual precipitation (MAP) is 400 mm. The runoff, from Figure 6, with SPEI, is 2-3 m^3/s which is equivalent to 23-35 mm for the entire basin. If I assume no deep drainage from irrigation, water usage is roughly 1,000 mm per year. This number intrigued me in a lot. First, is this irrigation sustainable over the long-term period? 600 mm of irrigation within a 400 mm of MAP environment makes the farmers, the ecosystem very dependent on this irrigation, or headwater sources, the Pyrenees. Secondly, this value seems somewhat upper limit for maximum irrigation. Because the ecosystem is approaching towards the PET which is 1300 mm. Another saying from water-limited to energy-limited. I am not sure whether or not the authors agree with me, but I definitely encourage the authors write a few sentences into the Conclusion or the Discussion part about the sustainability of this current land-cover transformation. The demand for water due to PET changes of future climate (as seen drier outcomes of ETHZ) is much less significant than those of current land-cover**

transformation.

Studying the impacts of the irrigation onset is a major topic of the current research in the Lerma catchment. It is obviously a very political, sensitive, and important issue, even if it is somewhat outside of the scope of this paper. We entirely agree that the impact of land-cover transformation has more impact locally than the impacts due to climate change (e.g., von Gunten et al., 2015). We also agree that deep drainage is usually small. Hence, the agriculture in the Lerma (and in the Bardenas region in general) depends on irrigation, and therefore on the headwaters from the Pyrenees. Moreover, the percentage of irrigated land is expected to further increase in the region and the Yesa reservoir is being modified to store more water. Hence, the regional agriculture will very strongly depend on the availability of irrigation water in the future. Is this sustainable? It largely depends on the future hydrologic conditions in the Pyrenees, particularly in the catchment of the Yesa reservoir, and on our estimation of the ecological need of the Aragon River (from which the irrigation water is diverted). However, in any case, irrigation in the future will need to be appropriately planned, and farmers will have to adapt, for example by changing the type of crops or by upgrading to more efficient irrigation systems. Hence, the sustainability of the system is questionable from our point of view. We will add a short note in the conclusion of this paper on this subject. But it would deserve a more in depth discussion, which would be outside of the subject of this particular paper.

4. **P4. L5. Wording. I recommend forcing only for meteorology.**

   Thanks, we will modify this.

5. **P8. L17. Please cite"Table 1" before citing"Table 2". It may be good to cite"Table 1" in Section 2.1. Or you may reorder the Tables.**

   Thanks, we will modify this.

6. **P19. L10. Please change ...project 'is' to ...project 'are'. Data may use as a singular or plural, however in two previous sentences you used as plural, hence to ensure consistency.**

   Thanks, we will modify this.

7. **Figure 6. Can you ensure the y-scale similar for both figures? I think the limits are [0 0.08] or [0 0.07]. And definitely, y-value (discharge) must be truncated at zero. Moreover, it needs a better colour selection.**

   Thanks, we will modify this.

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