Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-478-RC1, 2017

Hydrology and Earth System Sciences
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Discussions

Interactive comment on "Dendrohydrology and water resources management in South-Central Chile: Lessons from the Río Imperial streamflow reconstruction" *by* Alfonso Fernández et al.

Anonymous Referee #1

Received and published: 24 October 2017

I found the study very interesting. The reconstruction of river flows is important by itself, providing a long historical record that improve statistics. Similar mega-droughts in the past can provide insights about drivers when these findings are compare with similar behaviours in other regions. I mean, the water management importance can be true, but is not fundamental for the acceptance of this work. According to my experience, the manuscript should be accepted with minor revisions.

Major comments

- Is the Rio Imperial streamflow variability representative of the entire SCC? I think it is not. Please provide evidences about this point, especially for summer. It is an

important issue due the title of this paper include a large region from 35°S to 42°S in Chile.

- How an accurate calculation of natural streamflow variability can help to anticipate possible consequences in the water management? This is the justification or motivation of the analysis. My point is the following. If you proved that streamflow variance (or extremes) in the past was larger than the expected by models for the future, how this information is useful for water management? Perhaps, if droughts were more severe in the past, without a major extinction or decrease of vegetation, you can ask why we expect a major problem in the future? The major uncertainty is related to in any case with the water demand, but not with the natural or anthropogenic origin of the droughts. It is the minimum ecological discharge the variable benefited by this study? If so, I guess the water management issue is restricted to the decisions depending on this variable. Can you focus on this point?

- It would have being very instructive if you were count with data until 2014 or 2015 with the aim of calculating the return period of droughts or the recurrence rate of drought events of the mega-drought mentioned in the work of Garreaud (2015). According to this author, the mentioned mega-drought 2010-14 is the largest on record. Can these statistics used in your work shows the extreme nature of the mega-drought? (at least for the instrumental record).

- I understand the use of the Southern Oscillation Index (SOI) instead the Niño 3.4 SST anomalies, due the longest record of atmospheric pressure data in Tahiti and Darwin. In fact, SOI is not usually used in climate studies, due its large "noise" in the intraseasonal timescale, compared to the more smoothed evolution of the SST index in the central equatorial Pacific. Anyway, I expected the used of the SOI directly calculated from stations but you used the NCEP-NCAR reanalysis. What is the justification then?

- About equation (1) and Fig. 3. I am not dendronologist, so it is surprising the low covariance shared between these reconstructions, even when the samples are larger.

The trees are not responding in the same way to the atmospheric forcing? (water availability for instance). This explain equation (1)... but still I need an explanation in the text for this behaviour. Looking at figure 4 (upper-left panel) the reconstruction looks good.

- Fig. 4. It is clear from the comparison between reconstructed and observed streamflow that the reconstruction captures the low frequency but not the interannual variance, although the coherence shows a peak on 2.8 years. Based on this finding, why the authors can expect a reliable comparison with ENSO? Because it is the most important driver at interannual timescales? In fact, what is the reason for not using the Interdecadal Pacific Oscillation instead ENSO? Why SAM? Please provide references that reinforce your thoughts about possible drivers.

- Page 7, lines 25-26. The sentence is misleading, because Garreaud (2015) and Boisier et al (2016) define the mega-drought since 2010, exactly when your information stop. Have you considered the possibility of interdecadal variability? IPO changes to its positive (warm) phase at the end of 70s, changing to a negative (cold) phase ant the end of 90s. This can be seen as a negative trend since 1980... Please, provide some discussion about this possibility.

On the other hand, the positive trend of SAM can be related to any trend, even without a physical explanation. In your results, when you remove the linear trend the correlation fall to near-zero values, so what is the reason there is a relationship between SAM and streamflow at 38°S? SAM it is just a long-term trend? Why there is not relation at other timescale? Do you know what is controlling the SAM trend? That is a key answer to make.

I think you should read the following paper: http://www.scielo.org.mx/pdf/atm/v25n1/v25n1a1.

Minor comments

- Page 2, lines 10-11. This values were taken from the figures? If so, how accurate is

that?

- Page 2, lines 12-15. You have written 3 times "in this region" in few lines. I think it can be improved.

- Page 4, line 17. Where is mentioned Table 2?

- Table 2. What instrumental streamflow record have you used?

- Page 4, line 15. I am not expert on dendronology, so I can not questioning the methodologies employed to construct the index based on three tree-ring chronologies. About equation (1), however, there is something intriguing to me. I assume that water availability affects in the same way same species of trees. Why coefficients are opposite in sign for PAG (+2.69) and LYV (-1.97) at the same time (t-1)?

- Page 4, line 24. It is written "the return period or extreme low flows..." Did you mean "the return period of extreme low flows"?

- Page 5, line 21. You defined summer ad January-February for streamflow. So, what are the previous months for rainfall and what is the value for the not simultaneous correlation"?

- Table 2 is analysed in page 5. I suggest to exchange numbers with table 3.

- Page 5, lines 26-27. Clearly, streamflow as precipitation exhibits a positive skewness in southern Chile, which is a normal behaviour taking into account that at most, there will be no rainfall (0 mm) as the lowest values. This kind of distributions are typical also for wind speed. So, I do not understand the point of this sentence.

- Page 6, line 8. Define VIF.

- Fig. 4. It is very nice the percentiles at the bottom of the figure. Easy to interpret. I wonder what would be the percentile for the period 2010-2015? This information is available, why you do not have used?

- Page 8, lines 33-34. The summer of 1999 is part of La Niña, not El Niño. In fact, the winter of 1998 is one of the most dry winters in instrumental record.

- Page 10, lines 24-25. The restriction of power supply occurred in 1996, at least in Santiago. It was different in Temuco?

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