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Climate of the Past Discussions

Interactive comment on "Multi-century tree-ring based reconstruction of the Neuquén River streamflow, northern Patagonia, Argentina" *by* I. A. Mundo et al.

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General Comments:

The manuscript entitled "Multi-century tree-ring reconstruction of Neuquén River streamflow, northern Patagonia, Argentina" by Mundo et al. provides a high-quality, long-term dataset useful for water management planning and investigating drivers of trends and variability in a hydrologically important Argentinean river system. The paper builds substantially on previous work within the region, providing the longest reconstruction of streamflow in South America, and adding an impressive network of new chronologies while utilizing collections extending back to Holmes and LaMarche's

early work. The authors employ a chronology screening criteria and a nested PCA reconstruction methodology with the intention of enhancing the common climate/flow signal, and extending the reconstructions as far back as possible. The methodological approach is sound, and the interpretation of results is supported by the data – though the authors might consider adding a more quantitative ranking of drought event severity, magnitude, and duration (see Biondi et al. 2002, 2005, 2008). Relationships between streamflow variability and atmospheric circulation are explored using indices and geopotential heights. The documented association between Neuquén River flow and the Southern Annular Mode is striking, and speaks to the fidelity of the climatic signal (precipitation) captured by the reconstruction. Overall, the paper is well written and methodologically robust with high-quality figures. Due to the importance of this river system for regional water resources and ecosystems I expect this paper will be of broad interest within the scientific and resource management communities, and is suitable for publication within Climate of the Past with only minor revisions.

Though the manuscript is generally well written, the discussion of results presented within the paper could be substantially improved before official publication. The discussion seemed to lack a strong flow and focus, which would likely become more apparent with the potential addition of a summary figure (or two), and perhaps an additional minor analysis. For example, the start of the discussion related your new streamflow reconstruction to the early work of Holmes and LaMarche along with other regional reconstructions of streamflow and precipitation. I'm not sure if any of this data is available (it's not listed on NOAA paleoclimate), but it would be helpful for the reader if there were a stacked or compilation figure of the discussed reconstructions. If space is limiting, figures 4 and 5 could be moved to the supplemental. Also, a more quantitative ranking of drought/pluvial magnitude, intensity, and duration (e.g. Biondi et al. 2002, 2005, 2008) may better contextualize the importance of a reconstructed or observed event, and aide in the discussion of the hydrological importance of the event. This may help contextualize a discussion on the relevance of the SAM (or ENSO) in forcing major drought or pluvial events. Also, how are the SAM, ENSO, AAO all potentially

related to the dominant frequencies of the reconstruction that are shown with SSA and the Blackman-Tuckey spectral analyses? Does their influence on streamflow modulate through time? Is the same behavior shown in other regional reconstructions (e.g. the comparison figure)? And, can we infer more about the regional expression of the climate forcings? Focusing on improving the summary figures, statistics, and discussion may greatly improve the overall flow and focus of the paper, which may help convey the major points to a water management audience.

References

2008 Biondi, F., T.J. Kozubowski, A.K. Panorska, and L. Saito. A new stochastic model of episode peak and duration for eco-hydro-climatic applications. Ecological Modelling 211: 383–395.

2005 Biondi, F., T.J. Kozubowski, and A.K. Panorska. A new model for quantifying climate episodes. International Journal of Climatology 25(9): 1253–1264.

2002 Biondi, F., T.J. Kozubowski, and A.K. Panorska. Stochastic modeling of regime shifts. Climate Research 23: 23–30.

Specific Comments:

Abstract and throughout. Comparison of the 20th century condition to conditions of the past millennium is made. The major reconstruction presented here is nearly 800 years long. Are you referring to longer, unpublished results you have that include the last 1000+ years? Consider contextualizing the results within the past ∼800 years instead of 1000, unless citations to ongoing or unpublished work are made.

Pg. 3545 ln. 13-16. Consider citing the method used to classify/quantify the duration, magnitude, and intensity of drought/pluvial events, or, add a follow-up sentence describing how a drought event is defined.

Pg. 3546 ln. 12-14. Can this sentence be clarified? It's unclear exactly what is meant by "The average monthly streamflow was evaluated with tree-ring data to determine

the most appropriate months or seasons to develop the streamflow reconstruction."

Pg. 3548 ln. 7-8. What specifically are ARSTAN chronologies? Are you referring to the standardization program or the version of chronology used here? Specifically, did you use the standard (std), residual (resid) or the arstan (ars, residual chronology with pooled autocorrelation added back in) version of the chronology? What's plotted in Figure S1 looks like either the std or ars chronology but this should be clarified along with a simple justification (e.g. retention of low-frequency climate information) for using either the std or ars versions of the chronologies.

Pg. 3550. The defined method for ranking intensity, duration, and magnitude characteristic of a drought is insufficient. The rankings only appear based on what would classically be defined as the intensity of the drought. See the above Biondi et al. references for a formal statistical test and ranking procedure related to these three parameters of a drought and consider applying this method in your final analysis.

Also, you have adjusted the mean and the variance of your nested reconstructions to match the "best" reconstruction. The "best" reconstruction, however, has slightly different statistical properties (particularly variance) than the instrumental record. Do you see any problems with making direct comparisons to droughts in the reconstructed record to droughts in the instrumental record? Some caveats should be placed on the discussion comparisons made between droughts in the reconstructed vs. observed streamflow record.

Pg. 3551 Ln. 13-15. The method of pre-whitening then correlating two autocorrelated time series together is one approach to estimating accurate p-values. Pre-whitening largely destroys low-frequency information in both time series, which could have the effect of artificially inflating of deflating the actual r value. In the future, one way around this potential problem is to simply calculate the p-value using the effective degrees of freedom(N) in the time series with autocorrelation. For example, see www.atmos.umd.edu/∼ekalnay/notes4a.pdf.

Pg. 3552 Ln. 7. Table 1 should be cited here, correct?

Pg. 3553. This section on drought ranking needs more work (see above). Pointing out the major intervals discussed here on the reconstruction in figure 3 might also be helpful.

Pg. 3554. Consider listing correlation coefficients and p-values in the text alongside mentioning their significance. Any expectations of what may have altered correlation strength between the SAM and streamflow through time? Do you get more or less the same results if non-prewhitened time series are used? Consider discussing/exploring potential modulating roles of ENSO or the AAO.

Pg. 3555. Composite maps during years of insignificant correlation with the SAM could be used here to explore if atmospheric structure resembling other known major modes of circulation (e.g. ENSO, AAO) are important when the SAM is not. If this is a transitional basin between major atmospheric modes of circulation (as you mention in the following discussion), you may be able to elicit both when and where other circulation patterns have a large influence on the Neuquén River flow.

Pg 3556 Ln. 23-27. The discussion of ENSO and the AAO here seems to come up unexpectedly since their potential role in driving drought and streamflow variability is downplayed in the results section. I like this discussion, but it also justifies spending a bit more effort investigating the potential influence of these circulation modes.

Pg. 3558 Ln. 7-9. The discussion on the QBO seems to appear unnecessarily here. Within the paper its potential influence wasn't investigated so it may be advisable to delete the sentence.

Pg. 3558 Paragraph 3. The discussion on the influence of the SAM and the AAO should be more clearly articulated. As the paragraph is currently structured it hops from one index to the next without clearly explaining the relevant influence of either. It may be helpful to diagram an idealistic schema of the circulation patterns associated high

and low streamflow and precipitation across the region and then focus the discussion within the text. For example, I provided a similar diagram for the Northern U.S. Rocky Mountains in a recent paper, which was extremely helpful for structuring the text. See: Pederson, Gregory T., Stephen T. Gray, Toby Ault, Wendy Marsh, Daniel B. Fagre, Andrew G. Bunn, Connie A. Woodhouse, Lisa J. Graumlich, 2011. Climatic Controls on the Snowmelt Hydrology of the Northern Rocky Mountains. J. Climate, 24, 1666–1687. doi: 10.1175/2010JCLI3729.1

Table 2. Explain in the table caption why certain reconstructions are shown in bold typeface. It won't be clear to readers who skim the methods section that these are the reconstructions you retained and used.

Figure 1. Nice maps and an impressive array of chronologies.

Figure 3. Why are the calibration and residual panels smaller and offset to the right of the rest of the figure? Enlarge these to match the rest of the graphic, or make the graphic a 2-column graphic with the calibration interval on the left. If more space is needed, the CE and RE statistics for the split calibration and verification intervals could be dropped from the graphic.

Also, double-check your code for the mean and the variance scaling of the reconstruction and reconstruction error. The error bars appear to get smaller over the earliest part of the reconstruction when they should be increasing substantially.

Figure 4 and 5. Consider moving to the supplemental if more space is needed to add a nice summary figure or two that supports the discussion. Nice figures though. . .

Figure 7a. The instrumental SAM and streamflow records need a key or to have their colors defined in the legend.

Interactive comment on Clim. Past Discuss., 7, 3541, 2011.