

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 #2

Received and published: 29 November 2017

This manuscript is a case study for reconstruction of streamflow based on tree-ring growth data in Chile. Just to put things in perspective, and I do not mean any negativity here, there is nothing new in this particular manuscript with regard to dendrohydrology. There is almost 50 years of literature in this area; the same regression models, the same way of reconstruction, and the same variable (streamflow). The authors reconstructed summer flows, instead of annual flows, but reconstruction of seasonal variables rather than annual ones happened also several times before but perhaps not in Chile. So, from my point of view, there is not any aspect of novelty in this research. It is important to state this, at least to give the authors a chance to clarify in case I missed something, and I apologize if I did. The section that is most interesting to read

C1

is section 4, which the authors called “Discussion and conclusion”. In this section, the authors argued hard for the utility of tree ring-based reconstruction to identify droughts that are more severe or more frequent than those inferred from the instrumental record. This argument can be found in so many of the dendrohydrology papers published in the last decade, and again nothing here is new. However, I have a fundamental issue with the scientific foundation of the argument, and unfortunately this applies also to several other papers published in this field. The authors reconstruct almost 300 years of streamflow data and start comparing it with instrumental record of 60 years. Obviously, they find droughts in the 300 years with characteristics that are different from those in the 60 years, of course! But for water resources engineers, whatever you find in 300-year record MIGHT be a 300-year drought. It is unfair to compare it with the 60-year record. Engineers would fit a statistical distribution to the 60-year record, estimate 100 or 1000-year droughts, then fit a distribution to the longer reconstructed record, and again estimate whatever drought quantiles you want, then compare. Otherwise, engineers and water resources planners never use just the deterministic short instrumental record of flows. If you can prove, based on the analysis I suggested, that the reconstructed flows lead to significantly different frequency or severity of droughts, then you made the case about the utility of reconstruction. Other specific points: Page 2, Lines 8-10: If you have the future projections based on the CMIP5 results, why don't you investigate if projected future droughts are more severe/frequent than the past ones? Just an idea; Page 3, Line 27: How did you get the “natural regime”? Did you account for irrigation abstraction? Page 3, Line 29: This sentence is not clear. How did you use the double mass curves to determine the calibration window of time? Page 4: I feel that I miss proper information about the hydroclimatology of the region. How wet or dry is it? How much is the rain and its variation? Just provide some background information; Page 4, Line 17: You are referring to Table 3 before Table 2, please reorder the Tables; Page 4, Lines 24-26: peak over threshold is usually used for floods, but here you are doing drought analysis. Do you mean flow below threshold or something like this? You also need a reference for portion; Page 5, Line 25: What are these per-

C2

centage numbers (54.06% and 74.12%)? Do you mean m³/s? Page 6, Line 2: “that” should be “than”, and “here” is not clear. Do you mean your manuscript? If so, why is the reference, it is confusing; Page 6, Lines 11-17: These results are not really good (especially, RE of 0.36), I know they are typical in many dendrohydrology studies, but they should at least make the authors a bit more humble and lighten the assertion tone that is coming later in Section 4; Page 6, Lines 23-24: What does this sentence mean? Page 6, Lines 25-27: Is there any meaning for these windows of 5-year, 20-year,..etc? Of course, every time you change the window, you can get different results, but what are we supposed to learn from this? Page 6, Line 27: You cannot really use reconstructed flows to comment on extremely high streamflow. Look at your Figure 4 (top left) and you will agree with me; Page 6, Lines 29-33: I cannot understand this portion; Page 7, Lines 1-5: What does this argument imply? Rain and streamflow are different! So, how did you conclude that it is a pluvial system? I think you need to elaborate; Pages 7-11: Almost half of the paper came under one section called Discussion and Conclusions. This is a style and format issue that does not look good. You need to include more analysis with the Results section, then not very long Discussion section, then a separate Conclusions section, this will be better. Page 7, Line 31: Usually trend analysis is misleading. Have you looked at the trend of the entire reconstructed record? Page 8, Line 5: I got confused, was that SAM work done in this study or taken from other studies? Page 8, Lines 10-12: On what basis was this statement made? Looking at Figure 6 and the correlation numbers does not give me the same impression that the authors have; Page 11, Lines 21-24: I cannot find proof for this in the manuscript, perhaps the authors need to rewrite this; Figure 2 (but also a general comment): On what basis was the selection of Jan-Feb only? Why not March and April too? They also seem part of the low flow season, especially that it may not be a good idea to call two months a drought; The authors also need to note that averaging the streamflow of three stations may reduce the variability in individual gauges, and make the reconstruction easier (nevertheless, the reconstruction accuracy is not high anyways). So, you need to justify this; Table 1: Please report the standard deviation also or better,

C3

the coefficient of variation to see the variability of each series; Table 3: What are those LAN and VILL in the Table title? Table 4: The autocorrelation of the tree ring chronologies are quite high, and is usually transferred to the reconstructed annual flows. I find this unrealistically high for annual flows, can you comment on this and its impact on the reliability of the reconstructed flows? Can you compare it with the autocorrelation of the instrumental flows?

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-478>, 2017.

C4