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Climate of the Past Discussions
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Interactive comment on "Climate signals in a multispecies tree-ring network from central and southern Italy and reconstruction of the late summer temperatures since the early 1700s" *by* Giovanni Leonelli et al.

Anonymous Referee #2

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First of all, my apologies for delivering the review so late.

This paper gives a nice overview over climate responses in central-southern Italy across multiple species and reports a 300 year long late summer temperature record based on MXD. However, it is not really clear to me whether this paper tries to be a synthesis, a network analysis or about climate reconstruction, as neither part is performed sufficiently to justify publication in the present form. Had this been published in the 10-20 years ago the manuscript would have probably driven me to write a more positive review.

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Discussion paper

However, it's 2017 now and given the network size and actuality of the data I was actually wondering what is the added value of this publication over previous publications of Carrer et al. 2010, Piovesan et al. 2005 and Trouet 2014 apart from being the first simultaneous assessment of MXD/TRW and TRW of broadleaf/conifers? The former two of which have substantially higher site replication (Carrer et al. (2010) 55 ABAL sites and Piovesan et al. (2005) 24 FASY sites) and come up with very similar climate response patterns. Also, a big part of the manuscript is about climate reconstruction, solely based on conifer MXD data already published in Trouet 2014.

Trouet 2014 includes 6 of your 8 MXD chronologies in her Balkan temperature reconstruction, hence there is no surprise that the climate fingerprint is near to exactly the same. It's also no surprise that the temporal pattern is nearly the same. I also wouldn't say that the Trouet reconstruction is more variable in time, maybe on (multi-) decadal time-scale, but certainly not on centennial time-scale. Trouet 2014 varies around 0, whereas your chronology has a positive mean since 1850 and clearly negative before 1700. I would be interested to see actual statistics like standard deviation for such a claim (in low- and high-frequency domain), given the different amount of low frequency between your chronology is simply due to the different type of detrending used, which is discussed nowhere in the manuscript. As Klesse et al. 2015 use also RCS in Greece for an update of Mt. Olympus, a comparison of your data with completely independent data with potentially similar low-frequency characteristics is also lacking in this manuscript.

Was there no way to get Carrer and Piovesan/Di Filippo and others to provide their data to be included in this analysis? I know, Dendro people can be pretty possessive and restrictive with their data. But you cannot really call the present collection a representative network, any result is based on the screening of so little data (4 broadleaf chronologies; again, given that it's 2017 and not 2000) when there is potential for so much more. And even if the results kind of match previous publications, where is the novelty apart from applying the HSTC method?

A novelty would have been to tease apart the reasons for different strengths of climate influence, as you have done in your reply to Reviewer #1. That is what I would expect of a multi-species network analysis. The analysis and discussion presented in the manuscript is way too superficial. You could go much further and talk about which series from which sites, which species end up being highly sensitive? Is there a trend in mean climate conditions? And so on...

The authors furthermore exclude many of the in table 1 listed chronologies for the initial analysis, because they do not meet the criteria of number of samples or the required EPS threshold value. Later on, nowhere in the manuscript they state how many and which of the series in the HSTC approach come from the initially discarded sites, or which series of the initial good chronologies were discarded. Please indicate! How did you validate your site chronologies with only 3 series? Did you use other chronologies? If so, please specify in the manuscript!

Additionally, there are a couple of more chronologies on the ITRDB that fall into your region, uploaded in 2014 from P. Cherubini (your co-author). Did you exclude them because they were too short? If so, please specify in the manuscript!

Also you use RCS. How did you detrend the sites with less than 10 samples for the HSTC approach? There is no mention of it in the manuscript. And even 10 samples for a site RC is incredibly low. I am very skeptical about the use of RCS with such low replications as the ones used in the manuscript. Why didn't you just use a stiff spline detrending, or the classic negative exponential curve? What is the low-frequency gain over those approaches that are much less prone for weird sampling related trends (especially with low replication), since your chronologies are only (or >99%) composed of living material?

I challenge that the site-specific historical climatic records actually give you any real advantage over e.g. CRU, when you use correlation analysis (apart from the length of the record back to $\sim$1800). Had you reported site-specific sensitivities, i.e. as regression slopes, to a parameter given a specific mean condition I would totally agree with you.

Temporal stabilities in climate correlations for ABAL and FASY TRW have been also reported previously (again, see Carrer et al. 2010 and Piovesan et al. 2008). So the only real novelty is the analysis with MXD. Is the correlation decay in conifer TRW due to opposing low-frequency trends (possibly related to your detrending) or is it the high-frequency agreement that decays? No discussion about that in the manuscript.

Furthermore the balance between Introduction/M&M and Results/Discussion is off. Especially the whole climate reconstruction section (1.2) takes an unreasonable large part of this manuscript. The main message could be condensed quite severely. If you insist on keeping it as detailed as possible then for the sake of completeness (as you seem to count every single recent climate reconstruction of the Mediterranean region) you should include as well: Dorado-Liñan et al 2015 (Spain, PINI, temp pJASO), Klesse et al. 2015 (Greece, PINI, MJJ precip; PILE, JAS temp), Levanic et al 2015 (Albania, PINI, JJ temp), Poljansek et al., 2013 (Bosnia-Herzegovina, PINI, summer sunshine), Tegel et al. 2014 (Albania, FASY, summer temp). All of which seem to me to have much more relevance to be cited than the chronologies from Turkey/Caucasus/Jordan, which come from far more distant locations (and in part use different species).

For the amount of different analyses performed, the result section is pretty short and the discussion in the context of previous publications in southern-central Italy again very superficial.

This manuscript needs some serious overhaul in its concept, structure and depth until it is acceptable for publication. M&M and Results have been written a lot in passive voice, which should be considered to be changed. Please use more active voice, as Word tells me directly to revise the previous sentence.

Some additional things:

Abstract Line 34: climate worsening is an awkward formulation, use climate cooling instead.

Table 2: # of series; be consistent in respect to reporting number of trees or cores. Or why are there only 11 and 15 series from Lombardi et al. 2008 (Co-author here) included? In that paper they report 25 and 30 series from those sites.

Figure 3: I suspect that rows A, B, C show the correlations with T, P and S, respectively? Please make that both clearer in the annotation and in the figure. Something like: "chronologies of conifer MXD (left), of conifer RW (center) and of broadleaf RW (right) vs. Monthly temperature (a), precipitation (b) and SPI_3 (c)".

Page 6, lines 27-31: What did you do exactly? The first two sentences don't make sense. You identified your DCV and z-scored this time-series? SPI is already z-scored. And why do you then retransform them, just leave them in the original unit if you use site-specific climate data.

And why didn't you use SPI-1 instead of monthly precipitation? Monthly precip is essentially SPI-1 before transforming the measured values into a gamma distribution and z-scoring based on the cumulative distribution, so the correlation changes only maybe at the second or third value after the point. This is nitpicking, but I was just wondering why you use both variables and don't decide for one of them.

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