Interactive comment - **Anonymous Referee #2** - Received and published: 2 May 2017

our responses in BLUE

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.

The paper presents only original results, some are innovative some others are a confirmation of what already found, fact that underlines the goodness of the applied methods on the available dataset. Of course we strive for improving the ms. quality during the constructive review process. We compiled a cleaned large-scale network of tree-ring chronologies from the Italian Peninsula to identify signals of climate variability in indices of tree growth, and to improve models of future climate scenarios for a climate change vulnerable region. The applied methodology is the key, innovative, issue of our paper.

Specifically, the paper presents the application for the Italian peninsula of an innovative approach to climate reconstruction, firstly approved in the dendro community in 2016 (i.e., 1 year ago; Climatic Change, 2016, 137:275–291, DOI 10.1007/s10584-016-1658-5), but the climate reconstruction is not the only objective: a deep analysis of climate signals recorded by trees (RW and MXD) in a regional-scale network is performed on static periods (using site chronologies, classical approach) as well as on moving periods (using HSTC chronologies, innovative approach) in order to evaluate reconstruction potentials and possible biases in past climate reconstructions.

Briefly, rather strict passages of quality check of each individual series vs. the respective mean chronology are performed before constructing the site chronology with dendroclimatic purposes (only older than 100 yr trees, etc. ; p. 6 l. 13 and following lines). Not all the resulting site chronologies are used (see the problematic gray-shaded areas in Table 2), and for these latter sites only the individual indexed series are retained for further analyses. Finally all the 'saved' individual indexed series from all sites are initially used for the construction of the HSTC chronology (p. 6 l. 32 and following).

To our knowledge this is the first attempt performed in the Italian peninsula presenting a multispecies and multiproxy approach with dendroclimatic purposes.

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.

The added value for 2017 are the first application the HSTC approach at the regional scale in the Italian peninsula, and the previous passages for the site chronologies construction; the simultaneous assessment of MXD/TRW and TRW of broadleaf/conifers, as also recognized by Referee2; the use of high quality site specific climate data. Some other added values are hereafter reported.

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.

Our reconstruction is performed using a different methodology than Trouet 2014, and is based only on the Italian sites, thus excluding surrounding areas characterized by more continental climates (i.e., the European Alps, Balkan area, Greece and sites from the central and eastern European Alps to central Romania and

Bulgaria; p. 4 l. 20). Our reconstruction improves the one of Trouet 2014, being more representative for Italy and presenting less negative oscillations (see next heading).

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.
In the revised version we could add more information and comparisons also with other reconstructions, as suggested by Referee2. Around 1913 and in the 1970s the reconstruction of Trouet 2014 shows temperature nearly as low as during the coolest periods at the end of the Little Ice Age (around 1815), which is questionable or only partially explainable with her decision of including also sites characterized by continental climates. Our reconstruction is much less variable over the same periods. Three years after Trouet 2014, we are able to improve the reconstruction of late summer temperature for the region of the Italian peninsula (this is another added value).
Regarding the detrending method applied for the HSTC chronologies, this is widely presented in the ms. (p. 6 l. 32 up to l. 42): the HSTC are constructed starting from the indexed individual series that are obtained while applying the RCS method at each site (p. 6 l. 5 and following).

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?
No, there was no way. The first author has spent more than one year (2014-2015) in personally calling and contacting people from the Italian dendro community asking for data for the paper, and asking for metadata for at least updating the availability of dendro data at the national level. Referee2 will not be surprised that some research groups did not even pass their metadata even if already published.
The resulting dataset used and presented in the paper is what we could collect and, based on the innovative methods applied, on the high quality of the site specific climate data, and on the obtained results, we think that it is adequate for a publication in 2017.

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...
Based on this suggestion, we will add the analysis performed for Referee1 in the revised version of the ms. Moreover, we will add some more information as the ones suggested by Referee2.

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!
We thank Referee2 for the interesting comment, we will add this information in the revised version of the ms.

How did you validate your site chronologies with only 3 series? Did you use other chronologies? If so, please specify in the manuscript!

Site chronologies were all validated starting initially considering the whole dataset of individual raw series available. The final number of series per site is the result of the iterative selection applied to the initial datasets: we only retained series responding to the fixed criteria (p. 6 l 13 and following). No chronology from sites with a so low number of series entered in the successive analyses.

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!

Our research is based on data available to the authors and to the dendro community in 2015 (year of last dataset update; this information will be added in the revised version of the ms.).

Referee2 will agree with us that the chronologies uploaded onto the NOAA's ITRDB are data collected for many different research objectives, not only for investigating climate responses or for performing climate reconstructions. With our robust approach for chronology construction (deeply detailed in the method chapter p. 6 l. 13 and following lines), we had to discard several Italian sites, but there is no reason to make a list of the discarded sites and the reasons why they were discarded since the beginning: they simply did not meet all the requirements fixed by us for dendroclimatic analysis (most of the times they presented too short chronologies). The Cherubini's chronologies that the Referee2 is mentioning were also checked.

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?

RCS is a well approved approach for retaining low-frequency variability in tree-ring chronologies (especially the long ones), and performs better than splines and negative exponentials in this domain. Our approach was to apply the same detrending method at each site and to the whole dataset, in order to treat all data in the same way. Sites with low replication presented however long and well intercorrelating individual series: if the resulting chronology presented high values of EPS then we used it in the following climate-growth analysis, otherwise we used only their indexed individual series.

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 ~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.

The climate data used in this research are site-specific (coordinates, elevation and slope orientation), better homogenized and based on more stations than the ones used for the CRU gridded data. Most of the stations for central and southern Italy used for the CRU dataset start after 1950 and, before this date, the CRU interpolation scheme imports information from very far. We used the CRU as independent dataset for evaluating the spatial correlation pattern of our reconstruction (p. 7 l. 14 and Fig. 6).

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.

Temporal stability in climate correlations was tested on HSTC chronologies of RW and MXD, innovative aspect, and not on species. This analysis was mainly performed for evaluating the reconstruction potentials and the possible biases in past climate reconstructions. Deepening on correlation coefficient trends, as suggested by Refereee2, will be added in the revised version of the ms.

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.

We thank Refere2 for the helpful suggestions. A more balanced ms. will be proposed by moving some parts in the online materials and by focusing more on the Mediterranean regions closer to our study area, following some of Referee2's suggestions. Climate-growth publications from Italy and the Mediterranean were mainly focused on the species used in presented in our ms. (p. 2 l. 32).

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.

We will perform many changes as here above reported in our responses, and will change from passive to active voice, as suggested-

Some additional things:

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

Probably climate cooling and/or wetter conditions as MXD has proven to depend both on temperature and precipitation/drought (fig. 3). These variables, especially in summer, are also associated in Mediterranean climates (p. 9 l. 1). We will reword the sentence, we also noticed that the dates of minimum values are not correctly reported (correct values: 1698, 1741, 1814, 1913, 1938).

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.

Given the series selection method set up for this research, at each site some series were discarded if not meeting the fixed requirements (p. 6 l. 13 and following lines).

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)".

We will modify the caption. The figure order is correct.

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.

The sentence needs a rewording. Series were transformed in z-scores before averaging them between sites.

The 'interesting' climate variables identified by black-filled squares in Fig. 3 (months with significant correlations at most sites (>50 %) and with mean correlation values of $|r| > 0.25$) were regionalized and then averaged over two to four consecutive months (we called them DCV). Regional climate series were calculated

by z-scoring the monthly series and calculating regional mean departures; the series were then completed and ri-converted in original units (based on regional mean departures and their specific means and standard deviations), and finally averaged between sites. DCVs were then calculated as means of consecutive months of the regional series.

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.

We actually used the SPI calculated at several timescales (from 1 to 12 months; p. 5 l. 36) when assessing climate-growth relationships. As explained in the Results chapter (p. 7 l. 32) 'the highest correlations (for both MXD and RW) were obtained for the indices calculated at the timescales of 2 and mainly of 3 months'. We therefore decided to present only the SPI_3 results, and this is also discussed later (p. 9 l. 4). This timescale is used for modeling agricultural droughts and well fits with growth and wood density issues also in trees. We prefer leaving in the ms. also the variable of precipitation, being it of more direct readability.

The Authors, May 11, 2017