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Interactive comment on "Mid-winter (DJF) temperature reconstruction in Jerusalem since 1750 with some regional implications" *by* Assaf Hochman et al.

Anonymous Referee #1

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The manuscript by Hochman et al. presents a statistical reconstruction of winter temperature in Jerusalem at annual resolution since 1750, where the observational record extends back to the year 1865. The major problem I have with this study is that the reconstruction is based almost solely on long instrumental records of precipitation (and SLP) from stations at teleconnected sites in central and western Europe. Such teleconnections are prone to non-stationarity in teleconnection strength and sign, especially under boundary conditions different than today (e.g., during the Little Ice Age). Furthermore, the authors ignore a previous temperature reconstruction for the Near East/Middle East region that used a similar approach, but based on multiproxy network reconstructions from teleconnected sites outside the region (Mann, Clim. Change

2002), and basically shows a similar temperature evolution back to 1750, which limits the novelty of the study by Hochman et al. What the community actually needs are annually-resolved temperature reconstructions from proxy archives or documentary data from the Middle East region itself, which are not potentially affected by the non-stationarity of teleconnection strength and sign compared to those reconstructions that are based on remote sites that are located outside the region. Hochman et al. fail to demonstrate that the published tree-ring chronology from Jordan they use, which is their only predictor from the Middle East region itself, significantly improves their Jerusalem temperature reconstruction. Moreover, the same tree-ring chronology has been originally (Touchan et al., 1999) and recently used to reconstruct Middle Eastern drought/precipitation evolution back in time (Cook et al., Sci. Adv. 2015, Cook et al., J. Geophys. Res. 2016), which is not mentioned or discussed in the present manuscript. The observational T and PP records from Jerusalem are not new, and have been widely discussed in the Israeli and wider literature, including the cold-wet vs. warm-dry pattern, that might be time scale dependent (e.g., interannual opposite to decadal and multidecadal variability). The last two sentences of the abstract are not supported by the data/analysis, as there have been agricultural communities in this region of the Middle East prior to 1882-1904 and, in particular, the Jerusalem precipitation record does not extend beyond the year 1865. Given the overall style of this work (sometimes it reads like a master thesis, numerous dissertations are cited, although there might be more appropriate references in the international peer-reviewed literature), I am not convinced that this manuscript by Hochman et al. meets the criteria for publication in the journal Climate of the Past.

Detailed comments: Line 42-44: The information regarding what occurred 12,000 years ago is a little bit out of context here.

Line 50-54: The study of Mann (Clim. Change 2002) is missing here, and there have been some more high-resolution studies from a variety of archives (lakes, tree-rings, stalagmites, corals) in the Eastern Mediterranean region.

Line 81-83: This sentence highlights that the scope of this study is rather regional than large-scale.

Line 105-109: Give WMO station numbers, as there are 2 stations in Jerusalem, at least for rainfall. Which one was used in the present study?

Line 117: More specific information on the database where the proxy data has been downloaded is needed here (not just the standard ncdc/noaa web address). Furthermore, you need to cite the original references for the tree-ring chronologies used.

Line 120: Provide more information on the kind of historical information that has been used, and do not cite dissertations that are not accessible to the readers of Climate of the Past.

Line124-125: Which parameter did you sue (NCEP/NCAR reanalysis), 300 hPa GPH?, and why?

Line 125-126: It is odd to use the outdated Kaplan dataset for sea surface temperature in your analysis. There more recent SST products (HadISST, ERSST4) that are more appropriate to use.

Line 167-174: Give at least the periods of the individual records here (see also comment regarding Table 2).

178-179: What are your criteria for “high significant correlations”?

181-182: The NAO has at least some influence on eastern Mediterranean climate on interannual to decadal time scales (PP, T), with a rather complex spatial anomaly pattern in the eastern Med for PP. This should be at least mentioned or discussed here.

188-191: This is not new, and has been described elsewhere, and reflects at least partly the influence of the NAO on the Mediterranean and adjacent continental regions.

Line 196-200: What about the positive correlation over the eastern Med/Caspian Sea region.

Line 198-203: This is all well known, see publications by Eshel, Rimbu, Felis for the eastern Med/Middle East, and by Xoplaki for the larger eastern Med region.

Line 220-223: Is the correlation significant on both interannual and decadal time scales?

Line 226-231: Referring to a dissertation is not appropriate. You just present 14 cases out of more than 100 years for higher/lower than average winter T. What is your threshold here?

Line 237-241: Why is this notable here?

Line 241-242: Is this evident in the observational record as well (proof of concept)?

Line 245-246: You cannot say this, as you do not have a PP reconstruction back to 1750.

Line 250-265: These statements are not robustly supported by the data, and what about significance?

Fig. 2: These patterns bear similarities to the NAO/NAM pattern. Provide information on the significance of your spatial correlations. Why did you use subperiods for your analysis?

Table 1: The standard NCDC/NOAA website is not a sufficient reference here. Provide more information and cite original references for the proxy records used here.

Table 2: Show the individual records in a figure. Give their periods in Table 2. These records are likely relatively short or have gaps. How does this influence the overall scoring/representation procedure?

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