Clim. Past Discuss.,
doi:10.5194/cp-2016-90-RC2, 2016

Climate of the Past Discussions
Open Access
EGU

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

Anonymous Referee #2

Received and published: 22 November 2016

This paper presents a statistical reconstruction of annually-resolved winter (DJF) temperature in Jerusalem since 1750. The statistical methodology is based on principal component regression, using both instrumental data (precipitation and sea level pressure) from stations in central and western Europe and high temporal resolution records of proxy data (tree ring chronologies from Jordan). New climatic records are welcome and indeed needed for the region. However, as Referee #1 pointed out, I have serious concerns about the novelty, methodology and lack of supporting proxies.

Major Comments:

1) The authors stated that reconstructed winter temperature in Jerusalem is a first comprehensive attempt. However, Mann (2002) reconstructed annual and seasonal

Printer-friendly version

Discussion paper

patterns of temperature back through the mid-18th century through a method that combines all of the information available in instrumental, historical, and proxy climate indicators. Therefore, I couldn't truly get the point that makes the reconstructed winter temperature in Jerusalem different from Mann (2002). If the authors think that their study differs from Mann (2002) in terms of methodology, proxy sources, etc., they must include a discussion/comparison on that.

Mann, M. E., 2002. Large-scale climate variability and connections with the Middle East in past centuries. Climatic Change, 55(3), 287-314.

2) Without doubt, the authors are aware of that the most prominent influence of large-scale forcing is NAO, however, other teleconnection patterns have been demonstrated to play a key role in characterizing the eastern Mediterranean hydroclimate variability such as NCP and EAWR. In the manuscript, the authors chose long-term precipitation and sea level pressure observations located in central and western Europe indicating a highly correlated relationship with the climate of region of interest, which can largely be linked to the NAO. However, recent studies highlighted that past (up to LIA and MCA) hydroclimate variability of the eastern Mediterranean region has been controlled not by NAO forcing alone and more importantly, the character of the NAO and its teleconnections have been non-stationary, indicating contrasting spatio-temporal trends and patterns in the Mediterranean region (e.g., Roberts et al., 2012). My concerns about the methodology arise here because the authors have used a limited geographical coverage of dataset in which the dominance of non-stationary components at high frequencies of the climate signal may take place. In parallel to Referee #1, I strongly believe that including and interpreting available high-resolution paleo-records would add a contribution to the body of knowledge aimed at understanding the uncertainties coming from non-stationary climate signals.

Roberts, N. et al., 2012. Paleolimnological evidence for an east-west climate see-saw in the Mediterranean since AD 900. Global and Planetary Change, 84-845, 23-34.

Other Comments:

1) Northern Hemisphere meteorological season of winter starts from December 1 to February 28. Therefore, I am confused with the expression of "Mid-winter (DJF)". I recommend the authors just use "winter" or "mid-wet season" or "mid-cold season".

2) Pg. 3, line 42-49: Considering the importance of the extreme hydrometerological events such as 2007-2010 drought in Syria mentioned here, it would be good to analyze and discuss the extreme climate characteristics of Jerusalem. For instance, the authors discussed Jerusalem as a representative of Eastern Mediterranean climate. Is this evaluation also valid in the extreme hydrometeorological events? I would recommend the authors here include some extreme climate indices to perform the representative analysis.

3) Pg. 3, line 50-58: Given the fact that the region is located in a transition zone and under the influence of by sub-tropical and mid-latitude circulations as well as by tropical intrusions, the authors should include some other observations and/or high-resolution paleo-records in the southern parts of the region (see Major Comment #2). Please refer to:

Felis, T. and Rimbu, N., 2010. Mediterranean climate variability documented in oxygen isotope records from northern Red Sea corals – A review. Global and Planetary Change, 71, 232-241.

4) Pg. 7, line 167-174: I have some doubts and questions here:

i) Do the stations have exactly the same temporal coverage? If not, the method applied between short time series and longer ones would be misleading in the choice of reference stations.

ii) Has the methodology applied to each individual monthly series of a tested station? Or at seasonal time scale?

iii) I couldn't get the exact representative scores (mean correlations/mean significance)

in Table 2. Some of the values are different from mean correlations/mean significance result. Please check it out or am I missing something?

iv) How about the representativeness of the extreme hydrometeorological events and interannual variability? (Please see Other Comments #2).

5) Pg. 7, line 197: Why 300mb geopotential height? Please briefly explain the physical/dynamical reason for that.

Figures and Tables:

The figures are generally of good quality, however the labels and legends are difficult to read in Figure 2. It would be good to add a topographic background in Figure 1. Please check Table 2 (Please see Other Comments #4).

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2016-90, 2016.

Printer-friendly version

Discussion paper