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Climate of the Past Discussions

Interactive comment on "Two millennia of climate variability in the Central Mediterranean" *by* C. Taricco et al.

Anonymous Referee #2

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General comments

The paper discusses the extraction of long-term components from a highly-resoluted paleoclimate record using advanced spectral methods, and compares these components with results from other proxy studies. The sediment core used is unusually well dated due to its proximity to a region with active volcanism; the historically perfectly marked major eruptions show up as sharp pyroxene peaks in the record; in addition, it turns out that the sedimentation rate has been constant over the two last millenia, providing an equally spaced temporal record of $\delta^{18}O$ values when cutting the core into slices of equal thickness. The achieved resolution is 3.87 years for a time span of 2200 years (560 data points). This time series is then investigated using Singular System Analysis, by now a well-established data-adaptive decomposition technique,

advocated e.g. by the second author of this manuscript. Application of this technique is straightforward given an equally spaced time series without any gaps. Not surprisingly, the authors find a number of significant components in the centennial range and the sunspot cycle in addition. This reassures that the $\delta^{18}O$ record is a suitable proxy for (sea) temperature; the next step in interpretation - to treat a single-proxy series from a Mediterranena region as a representative for the temperature of the Northern Hemisphere as a whole - is less obviously correct.

The null model they use is the simplest non-trivial one - AR(1) noise, which is also the common choice. At the 98explain a surprisingly low portion of only 40attribute the remaining 60

The paper proceeds with a comparison to other, well-known temperature records covering the last millenium which could be considered as benchmarks in this context. They are different in character, since they are multiproxy assemblages believed to represent the Northern Hemisphere annual mean temperature more properly. These series include the famous "Hockey stick" in its original version. The authors do not mention the lengthy debate on this particular reconstruction by any means, but they are presumably aware of it (indicated by the sentence "The original...is also shown...for reference purposes"). As these long-lasting discussion shed doubt on the validity of this selection of proxies, a comment including some key references is in order here.

The authors of the current study could be the first to perform an SSA on the hockey stick data (if not, a reference is missing in the article), which is a logical step. Unfortunately, the parameter setting (window length) is set to 300 years and thus excludes the possibility to detect the 350-year oscillation, which is the second most significant component of the sediment record. This makes comparison of the different proxy records a tricky business; the authors thus discard the two leading and the fourth and fifth periodic components from the record and compare the rest only. The agreement obtained is good to excellent, leading to a tight linear relation between temperature and $\delta^{18}O$ values. This relationship is then used to extrapolate a further thousand years back,

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although the $\delta^{18}O$ then clearly leaves the regime of the most recent 1000 years, and shows its absolute minimum around year 0. This is unfortunately at odds with common knowledge that the Roman Classical Period had rather warm temperatures. The authors continue with a lengthy discussion that evidence for this last statement is weaker than commonly believed, and that an extended dry period could have increased salinity to the extent that the observed high $\delta^{18}O$ values are a mixture of temperature and salinity, i.e. one would underestimate the temperatures in the RCP. If this is a correct conclusion, why is the more recent part of the series not affected by salinity, as indicated by the almost perfect match with the hockey stick reconstruction? Has precipitation been fluctuating (strongly) a couple of centuries and then stayed constant the last thousand years? This seems implausible.

In conclusion, the paper presents a paleoclimate record of impressing high quality, which is analysed using advanced statistical methods. Their application, however, is not completely satisfying, and the next-to-arbitrary selection of some of the significant components raises the suspicion that it was done in order to maximize the match between this record and the benchmarks. This is highly problematic, in particular if a further goal is extrapolation of the temperature-proxy relationship by more than 100

A suggestion to improve the manuscript would be to restrict the data set to time covered by the hockey stick, redo the analysis with the same window length (300 years, if still feasible since then one window contains only 77 data points), NOT to exclude any significant components, and then compare. This would be a reliable procedure; if the comparison no longer works (a linear relationship, or any simple relationship for that matter, could not be obtained), this would be a hint that the sediment core $\delta^{18}O$ values are affected by salinity and a plethora of other (e.g. geological) processes, a conclusion which would be hardly surprising. However, extrapolation could be impossible in this case.

Another issue of concern is the limited precision of the isotope measurements (cf. specific comment to App. A below).

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As these suggestions are equivalent to redoing the whole analysis and potentially affects all the conclusions, they qualify as "major revision". But as the time series is well described and of excellent quality, and the method chosen is surely appropriate, the paper should be published after reconsideration.

Specific comments

Introduction:

Comment: to call tree-ring series "single proxy" gives the incorrect impression that these series exist as a single univariate time series. They are in almost every instance stitched together from many individual records, and the collation of them is far from being a trivial issue.

Results:

- the core is 3.57 m long, but only the upper 140 cm have been used. Why? The authors refer to Appendix A here, but this question is not taken up there.

- the comparison with other methods (mentioned are classical Fourier analysis and the MEM estimate) is very meaningful, but the authors do not present any results apart from the statement "results were confirmed by other methods". Given the methods do not do the same, the frequency resolution is different etc., this can't be true in all respects. What were the differences?

SSA comes with one basic parameter to be chosen by the analyst, the window length. The authors used 150 values, or 580 years, leading to N/M=3.73, which is in the range of recommended values for this ratio. In addition, they claim that results were not affected when varying the window length from 120 to 200 values. The reviewer doubts the validity of this statement. On one hand, a component just at the left margin of the spectrum (in the lowest frequency bin) usually moves further to the left when increasing the window length. Its presence is often due to spectral leakage of a component of an even lower component not resolvable by the current settings. It is problematic to

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attribute such a component to a non-periodic "trend". On the other hand, with a window length of 580 years, is it possible to reliably detect a component with a period of 595 years? This is not a mathematical impossibility due to the fact that MEM was used to estimate the periods from the full RCs, but are these estimates robust when changing the window length from below the period to above the period?

- for two other records, a 500 year oscillation is mentioned, "in phase with RCs 2-3 of our $\delta^{18}O$ record." But the latter only has a 600 (595) year period, how could that be in phase for over 1200 years?

Appendix A:

- the precision of the isotope analysis is given as 0.1 per mille. The total range of the record is thus only fifteen times the precision. Constructing surrogate series by adding white noise with a standard deviation of 0.1 per mille MUST lead to (very) different results. But this would be a classical way to investigate the uncertainty of the results by "error propagation". Needless to say, this is something different than the MC surrogates.

Technical corrections

- the Multi-Taper Method is just referred to as "MTM". Provide the full name at its first occurrence. - on p. 1098, replace "foraminineral" with "foraminiferal"

Interactive comment on Clim. Past Discuss., 4, 1089, 2008.