

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

**C. Taricco et al.**

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Replies to Referee #1

"I would have liked to see a more thorough discussion on the climatic interpretation of the isotopic data before they are compared with other proxy temperature reconstructions, which are subject to debate. Have the authors compared their record to instrumental series of temperature covering the 20th century?"

Yes: the paper has been substantially changed. Our  $\delta^{18}\text{O}$  record is now compared to alkenone-derived SSTs measured in cores from the same Gallipoli Terrace, as well as to European and Italian temperature reconstructions. The first one covers the last 500 y (Luterbacher et al., 2004) and the second one covers the last 250 y and results from the homogenisation of instrumental data (Brunetti et al., 2006).

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"They discuss the potential effect of salinity fluctuations (which might be due to precipitation anomalies), but should bring some kind of quantification of this effect. Instead, they only state that a salinity increase is "fairly unlikely" (p. 1098, l. 13), without giving a precise reason for rejecting this hypothesis."

The subject of the quantification of the precise influence of the  $\delta^{18}\text{O}$  of water (and of salinity) on the  $\delta^{18}\text{O}$  of our foraminiferal calcite record is left open, as stated now in the last para. of Section 4: "To infer more systematically, from calcite  $\delta^{18}\text{O}$  and alkenone measurements, information about past water  $\delta^{18}\text{O}$  variations, it will be necessary to measure a longer, homogeneous series of another proxy for past SSTs, for instance foraminiferal Mg/Ca."

"The authors chose "a window width of  $M=150$  points", out of a time series of 560 points. This means that there are less than 4 independent windows on which the analysis is done. I thought that (Vautard & Ghil 1989) advocated that  $M$  be larger than  $N/10$  to achieve statistical significance. Can the authors justify why the window width ( $M=300$ ) of the series of (Mann et al. 1999) is different from the one they use for their own record?"

The choice of a window width of  $M = 150$  points for our calcite  $\delta^{18}\text{O}$  series is justified by the need for separating multicentennial oscillations from the millennial long-term trend; taking a smaller window width (e.g.  $50 \simeq 1/10$  of the number of samples, corresponding to less than 200 y) would include oscillatory modes with periods of several centuries in the trend. In fact, subsequent experience, as summarized by Ghil et al. (2002), has shown that  $M$  as small as  $N/5$  or even  $N/3$  may be useful, especially if other spectral methods than SSA are used (here MTM and CTW), and comparison with other time series supports the results. Both of the two latter criteria are now satisfied. The comparison with the Mann et al. (1999) series has been dropped. Please see also related comment on  $N/M = 3.73$  by Ref. #2.

"There is no real consensus on the northern hemisphere temperature reconstructions

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proxy reconstructions (Jansen et al. 2007). Comparing their record to the one of Mann et al. might induce a bias toward Penn State University. It might be useful to make similar comparisons with records exhibiting a different type of variability."

In the revised paper, the SSA analysis of the Mann et al. (1999) series is no longer present, since we preferred to compare our data with Italian and European temperature records.

"Conclusions, p. 1099, l. 9. Is there a reference for the "commonly alleged warmth of this period"? To be the Devil's advocate, could it be possible that the authors' record is not so good a proxy for temperature, that a difference with other testimonies of warmth is not statistically or physically relevant?"

To be the Devil's opponent, we could reply that we have identified and cited in our paper several other lines of evidence for a cold period around 0 AD (Dahl-Jensen et al., 1998; Bond et al., 2001; DeMenocal et al., 2000; Issar and Yakir, 1997; Mangini et al., 2005). A whole new section (Sec. 5) has been dedicated to this discussion.

Minor remarks

"Abstract. The notion of "deep maximum" is very unusual. Please find a better adjective or remove it."

"Deep maximum" has been changed to "surprising maximum"; thank you.

"Introduction, second sentence. "Instrumental temperature series cover only a couple of centuries...". The paper of (Plaut et al. 1995) covers 335 years. Please rephrase the sentence."

The sentence "Instrumental temperature series, however, cover only a couple of centuries" in the Introduction has been changed to "Instrumental temperature series, however, only cover two–three centuries."

"Introduction, last para. The paper of (Sicre et al. 2008) should be cited here, with the

book of Martinson et al. (1995)."

We have not cited the paper by Sicre et al. (2008) in the Introduction, since the purpose of this paragraph is to highlight the advantages of using high-resolution sediment cores to investigate climate variations, in general, and not to cite particular proxy records.

"Results, p. 1094, l. 17. Is there a connection between the paper of (Mann & Jones 2003) and the list of papers cited at the beginning of the paragraph?"

Mann and Jones (2003) is no longer cited.

"Results, p. 1096, l. 5. It is not clear in the manuscript what Delta 14C is. In their paper, (Stuiver & Braziunas 1993) analyse atmospheric residual Delta 14C (whose definition is rather complicated)."

The phrase: "spectrum of  $\Delta 14C$  over the last 12 ky" at the beginning of the 3rd paragraph of Section 4 has been changed to "spectrum of atmospheric residual  $\Delta 14C$  over the last 12 ky"; thank you.

"Results, p. 1096, l. 15: "... in phase with the solar cycle." Please explain which solar cycle?"

The sentence: "is perfectly in phase with the solar cycle" in the 4th paragraph of Section 4 has been changed to "is perfectly in phase with the Schwabe solar cycle".

Replies to Referee #2

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."

In the Introduction, the sentence: "..several temperature series have been constructed

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using single-proxy, such as tree rings, or multi-proxy records" has been changed to "...several temperature series have been constructed, using a single type of proxy, such as tree rings, or using multi-proxy records".

"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 core is 3.57 m long, but only samples from the upper 140 cm could be analyzed up to now, for technical reasons (such as the "usual suspects" – time and personnel). The sentence has been modified to read "Appendix A provides details on the stable-isotope analysis, completed so far for the upper 140 cm of this core." We hope to be able to analyze the samples for the rest of the core soon.

"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?"

The revised paper now includes results from the analysis of the  $\delta^{18}\text{O}$  series not only by SSA but also by the Continuous Wavelet Transform (CWT). Other methods confirm the results described in the paper, in the sense that, taking into account the different resolution characteristics of the various methods in each frequency range, they reveal oscillations compatible with those obtained by SSA. We choose to focus on SSA results here because this method, when used in association with Monte-Carlo SSA, allows very reliable estimates of the significance of the oscillatory components.

"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

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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 attribute such a component to a non-periodic "trend".

When we change the window width  $M$  in this range, the individual components turn out to be grouped in different ways, but substantially the same oscillations are identified.

"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?"

The most stringent way to show the reliability of the detected components is to use a statistical test. We did so by using a Monte-Carlo test, with different windows; see Fig. 4 and its discussion.

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

The sentence "a 500-y oscillation also dominates: "...and is in phase with RCs 2-3 of our  $\delta^{18}\text{O}$  record." in the 3rd paragraph of Section 4 has been changed to "a similar oscillation also dominates: ..and it is in good agreement with RCs 2-3 of our  $\delta^{18}\text{O}$  record.". We include in this reply the figure showing the agreement.

"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

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the MC surrogates."

This is an interesting suggestion, but we beg to differ: the MC surrogates are pretty much subsuming the proposed way of testing the robustness of our results. Each oscillatory component in Fig. 5, as well as the trend RC (RC-1), has a much larger amplitude than 0.1 per mil. Given all the other tests and comparisons we have carried out already, we pray to be excused from one more test, interesting as it might be.

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

## CPD

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