

General Comment: Ernesto cave represents the only example in Italy (so far) of high resolution (annual) speleothems study and the mechanism producing calcite deposition are extremely well understood thanks to a long detailed monitoring program. Any palaeoclimatic record from this cave is then relevant for our understanding the climate in the Mediterranean area and the relation with central Europe. The text is well written, clear and references are really updated. However, I think there are many points on which a discussion should be open because Scholtz et al. give some interpretations, which are not completely supported by the data presented (and from local climatic data). Proxies presented and discussed are not always so obvious (as honestly recognised by the authors) so the paleoclimatic reconstruction are not always robust (as stated in the conclusion).

We thank Giovanni Zanchetta for his thorough review, which was very helpful in order to further improve the paper. In particular, his suggestion to investigate the relationship between the winter NAO index and winter temperature in the cave area resulted in further evidence that the area is sensitive to the NAO. This further confirms our interpretation of the proxy signals as potentially reflecting past changes in the NAO.

-The main point concern the interpretation of the data in terms of NAO. The most convincing proxies (according to the authors interpretations) are lamina-thickness (LT) and carbon isotope composition of the calcite. In the paper is reported that there is a (relatively) robust correlation between precipitation and NOA (but not show in any figure), but LT and $\delta^{13}\text{C}$ are interpreted as proxies for milder winters (so temperature). Which is the relation? Having a record starting from 1921 a possible correlation between NAO-winter temperature and precipitation should be clear? If this relation is not evident I'm just wondering if most of the interpretation proposed by the authors are sound or just speculations.

We agree that the relationship between winter temperature and the winter NAO index was missing. Thus, we re-analysed the data from the nine weather stations in Trentino and, indeed, found a significant positive correlation between winter temperature and the winter NAO index for eight out of nine stations, which is even higher than the (negative) correlation with precipitation. This suggests that phases of thicker laminae and lower $\delta^{13}\text{C}$ values (the most convincing proxies) correspond to (persistent) NAO⁺-conditions. This information has been included in the revised version of the MS and corroborates the interpretation of our record in terms of past changes in the NAO.

-The interpretation of the long term $\delta^{13}\text{C}$ as due to the progressive soil development is intriguing, but I must be honest the record does not suggest necessarily this: none look progressive in the record but more a step-like behaviour (suggesting some related to bioma changes, so I suggest to look in details the pollen diagrams in the area if there are). In additional a quite obvious correlation with texture index do not support the interpretation proposed (or at least a different interpretation can exist) suggesting changes in ventilation and degassing.

We do not agree that the evolution of the $\delta^{13}\text{C}$ signal shows a step-wise behaviour, neither do we see an obvious correlation with the texture index. Nevertheless, we do not exclude other processes, such as changes in ventilation and degassing in the MS. However, we consider these processes unlikely to be the reason for the observed millennial scale change: 'The observed long-term decrease in $\delta^{13}\text{C}$ between 8.0 and 2.5 ka (Fig. 3), however, is unlikely to be related to changes in cave ventilation and degassing as this would require a progressive reduction in ventilation. A more likely explanation for the millennial-scale decrease of the $\delta^{13}\text{C}$ values is ...'

*Furthermore, we included a paragraph discussing the potential effect of changes in vegetation association above the cave. Pollen data from nearby Lago di Lavarone (Fig. 1) show that the vegetation association consisted of broadleaf (*Fagus*), *Abies* and *Picea* between ca. 8 and ca. 2 ka and was, thus, similar to that observed today (Filippi et al., 2007). It is, thus, unlikely that the long-term decrease in $\delta^{13}\text{C}$ is related to changes in vegetation association. Prior to 8 ka, the vegetation mostly consisted of xerophytes and conifers. It is interesting that ER76 started to grow when broadleaves appeared in the biome, which suggests a relationship between deciduous trees and supersaturation of karst waters in region as suggested by Frisia and Borsato (2010).*

I recognised that the authors use a lot of caution in the interpretation so my comments are for stimulation discussion and not for necessarily support a different interpretation.

Specific comments

Pag. 912 ca line 15. Magny has recently proposed others regional scale patterns who could be useful for discussion (e.g. Magny et al., 2009 Holocene, Magny et al., 2011 JQS). In particular Magny et al., 2011 JQS should be of interest (maybe some can be seen in $\delta^{13}\text{C}$ record?).

We agree that these two papers also discuss interesting regional scale Holocene climate patterns. We compared the ER76 $\delta^{13}\text{C}$ and lamina thickness record with the lake level record from Cerin (Magny et al., 2011), but there is no clear relationship. Even the large change at 4.2 ka is not visible in our record. Thus, we did not include the two records and references.

Pag. 913: Frisia et al., 2003, 2005 instead of Frisia et al., 2003 : : : : Frisia et al., 2005)

The CP guidelines for references do not explicitly mention this example. However, since we used the EndNote Output Style File provided on the CP Homepage, we assume that our version is appropriate. No changes made.

Pag. 916 Frisia et al., 2006 is related to a Cave in Sicily, why quoted?

It is correct that Frisia et al. (2006) present a speleothem record from Sicily. However, they compare this record with the previous version of ER76 (McDermott et al., 1999) and provide an updated age model, which is based on lamina counting. Thus, this reference is important for the 'history' of the development of the chronology of ER76.

Pag. 916 ca line 20: it is unclear: if the record started for McDermott at ca 9.1 later a correction of 600 yr is suggested the record now started at ca 8 ka. It should started before?

In combination with the previous comment, it becomes clear that our description of the 'history' of the development of the chronology was not really clear. The first chronology was provided by McDermott et al. (1999) based on U-series dating. These data suggested a bottom age of 9.1 ka for ER76. Subsequent annual lamina counting, however, was not in perfect agreement with the U-series data and suggested a younger bottom age of 8.5 ka. This updated age model was then used in Frisia et al. (2006). The new U-series determined for this study clearly show that the bottom age determined by McDermott et al. (1999) was indeed too old. The new chronology based on the new U-series data and lamina counting results in a bottom age of 8.038 ka. We clarified this in the revised manuscript.

Pag. 919: here is mentioned the correlation of some meteorological station with NAO and winter precipitation but none is said about T.

We agree that the potential relationship with temperature may also be important. Interestingly, eight out of nine stations show a significant positive correlation between winter temperature and the winter NAO-index at the 95%-confidence level. We also re-calculated the relationship between winter precipitation and the winter NAO-index using longer and more complete data sets obtained from <http://hydstraweb.provincia.tn.it/web.htm>. The new results confirm our previous results (i.e., a negative relationship between NAO and precipitation during winter months) and are even more robust (seven out of nine stations show a significant negative correlation at the 95%-confidence level).

Pag. 921 Line 24: almost significant?

We agree that the wording was ambiguous and modified it accordingly.

Pag. 924 ca. line 10. The average values of ER 76 oxygen isotope composition would be useful here.

Good idea. The mean value has been provided.

Pag. 926 Line 20 to 25. Are there any evidences on this kind of changes in soil thickness? Why should only leaf degradation? Maybe the transition at ca 7.5 ka could be related to a sudden (? Soil development is a relatively low process) soil thickness (with changes also on texture), but since then the record is quite flat. Of course the absence of the older part of the record amplified the difficulties in interpreting the records. However, we are well inside the Holocene and pollen diagram should support the view that afforestation is already accomplished (then soils should be already well developed) in the area. To support their interpretation dcf data are also illustrated but they don't cover all the record and organic matter degradation and contribution from different proportion of old/new organic matter not depend only on soil development. However, I recognize that this is not a prominent part of the manuscript and despite I found it particularly intriguing I don't want to focus too much on this.

As discussed in detail in the response to the comments of the anonymous referee, we deleted the section discussing the potential effects of leaf degradation on the $\delta^{13}\text{C}$ signal. The interpretation in terms of progressive evolution of soil thickness and composition, however, is consistent with both the decreasing trend in $\delta^{13}\text{C}$ (due to increasing soil $p\text{CO}_2$) and the increasing trend in the dcf (due to increasing contribution of 'old' soil organic matter). Thus, we did not modify this explanation.

Pag. 929 The sentence "In addition, a significant amount of winter precipitation, deposited as snow, seems not to contribute to the drip water budget" let me puzzled. I found strange this but if it is true so how can the system records any form of NAO signal? Water is the carrier of CO_2 in the system, and if a significant part of this signal is lost presumably also the others could be very noisy. Is that the reason for which at the end of the story only a tenuous temperature signal is preserved? Because on the contrary the NAO signal should dominate (and in the cave too) as precipitation and not as temperature. Probably the sentence is not clear and need to be rephrased.

That 'a significant amount of winter precipitation, deposited as snow, seems not to contribute to the drip water budget', follows from the comparison of the drip water and rainfall $\delta^{18}\text{O}$ values and is the likeliest of the three discussed explanations (see extended discussion on the previous page). We agree that this may result in a lower signal-to-noise ratio and be one reason for the problems with the climatic interpretation of the $\delta^{18}\text{O}$ signal. On the other hand, however, such a scenario could result in an amplification of the NAO-signal due to the observed relationship between winter rainfall amount and the NAO (see the discussion in the preceding paragraph). Unfortunately, as explained in the MS, both hypotheses cannot be tested with the cave monitoring dataset, which is too short for a robust statistical analysis. No changes made.

Pag. 930. Line 12: I found clear the correlation for four and not for five. Note that in the fig. 7 and 3 the d^{13}C has the axes inverted so please be consistent between figures.

We see the relationship for five phases. The exception is the period at 4.9 ka, which exhibits a trend to more positive values. We added this information to the revised MS.

Pag. 931 line 10 an? I think it should "a NAO"

If the individual letters are pronounced separately, "an N A O" is appropriate. No changes made.

Pag. 932. lines 10-13. Interesting point and I agree that sapropel chronology is controversial (also from a cave point of view: Zhorniak et al., 2011 QSR). However, Siani et al., 2001, Science, or Siani et al., 2004 QSR should have well demonstrated that this interruption is related to the so-called 8.2 event.

The Zhornyak et al. (2011) reference has been added. We also agree that there may be a relationship between the interruption of sapropel deposition and the 8.2k event. However, both papers do not explicitly mention a causal relationship. Thus, we have not included these references.

Pag. 933. In Renella the climate anomaly is centred at ca 4.0 ka rather than at ca. 4.2 ka. Be honest I'm always wondering in case of very minor (and frequent) oscillations if precise correlation can be done. However, Magny et al., 2009, Holocene, suggested that this phase is quite complex, so mention this paper could be useful. In addition which is the criteria to define

prominent some peaks and their meaning. In fig. 7 they are highlighted when there is a correlation (not always obvious) with d13C signal, but there are some peaks in the LT, which show prominence but not match the d13C: then?

The timing of the climate anomaly at Buca della Renella has been corrected. The, indeed, interesting reference of Magny et al. (2009) has been added in the revised version of the MS. We used a clear criterion to define prominent peaks in lamina thickness (i.e., thickness >100 µm), which has already been provided in the MS (p. 930, line 8). No changes made.

Pag. 933 lines 23-23 “ of this extraordinary climate: : : : :” please explain the meaning.
This is related to the sapropel phase, which is a period of extraordinary climate during the Holocene in the Mediterranean. For reasons of clarity, we now use 'pluvial' phase.

Pag. 934 Line 5. I'm not expert of spectral analyses but sometime the impression is that there is an overinterpretation. There is no evidence from the data presented in which way NAO should affect the record. If LT is related to temperature over the Alps or at least for the Trentino the authors did not report any evidence of the relation between NAO and T, whereas is strong (but not in all station) with precipitation, but most this signal seems to be lost and not affecting the drip. So the conclusive sentence at ca line 20 is difficult to sustain in a robust way: so I agree in the use the term “may be”.

We agree that the results of spectral analysis are no evidence for a relationship between the NAO and climate at the cave site. Therefore, we wrote ‘... suggests an influence ...’, which is – in our opinion – not a too definite statement. However, in the revised version of the MS, we included an analysis of modern temperature data from the area, which, indeed, suggests a relationship between temperature and the NAO. No changes made.

Pag. 935. Then I cannot accept the fist sentence “: : : :allow a robust interpretation” and I suggest to start with the second sentence of the section “High lamina : : : :”

We admit that this sentence may give a wrong impression taking into account the problems interpreting the δ¹⁸O signal and modified the sentence in the revised version of the MS.

In conclusion the paper is intriguing and all the discussion is based on the many observational data from current monitoring program. However, the paleorecord appear quite complex (as climate in the area) and most of the conclusion are in term of “suggest, may be” and a very climatic picture does not emerge. This is not necessarily a limit for the manuscript but a good examples of the natural complexity. Maybe not all the speleothems records contains strong climatic signal (or at least not all the proxies). Overall the manuscript (after a discussion of the point suggested) is well suited for Climate of the Past.

We agree and honestly admit in the MS that the interpretation of the proxies recorded in ER76 in not straightforward - despite of the available monitoring program. In particular, the interpretation of the δ¹⁸O signal seems to be very complex. However, we believe that this result is also important and of interest for the speleothem community.

References

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