

Interactive comment on “Holocene climate variability in North-Eastern Italy: potential influence of the NAO and solar activity recorded by speleothem data” by D. Scholz et al.

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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 cli-

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matic 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). -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. -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.

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?).

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

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

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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?

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

Pag. 921 Line 24: almost significant?

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

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 recognise 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.

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 CO₂ 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

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as temperature. Probably the sentence is not clear and need to be rephrased.

Pag. 930. Line 12: I found clear the correlation for four and not for five. Note that in the fig. 7 and 3 the d13C has the axes inverted so please be consistent between figures.

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

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.

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?

Pag. 933 lines 23-23 "of this extraordinary climate. . . ." please explain the meaning.

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".

Pag. 935. Then I cannot accept the first sentence ". . .allow a robust interpretation" and I suggest to start with the second sentence of the section "High lamina . . ."

In conclusion the paper is intriguing and all the discussion is based on the many ob-

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servational 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 singal (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.

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