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Climate
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Discussions

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.

Anonymous Referee #2

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General Comments:

Aside from the Discussion section that is too long (11 pages) and therefore rather difficult for the reader to follow, this manuscript is well written.It provides an update on new work that has been carried out by the authors on stalagmites ER76 and ER77 from Grotta di Ernesto, a well studied cave in north-east Italy.The authors confirm and refine a previously published chronology for ER76.Importantly, the authors demonstrate that very similar dˆ13 C and dˆ18 O signals are preserved over a short overlapping period (c. 300 years) when both stalagmites were deposited contemporaneously.This suggests strongly that the stable isotope variations in both stalagmites primarily reflect climatic

signals and not merely variations in drip-specific hydrological routing effects.The title of the paper indicates that both NAO and solar signals may be present in the data, although as discussed below, the attribution of the observed variability to particular climate drivers remains somewhat speculative.The evidence for solar forcing is not strong.The authors should be cautious about interpreting the data in terms of solar cycles.Similar cycles could be driven by the NAO/AMO.The fact that the 11 year solar cycle is apparently not found despite the relatively high resolution sampling (c. 1.7 years per analysis) for the stable isotope measurements may be important and should be mentioned.

Specific Comments:

Abstract, Line 1:please indicate that the nine meteorological stations are located in Trentino (e.g. Data from nine meteorological stations in Trentino show. . ..)

Page 3, line 31:suggest ' which enabled the calculation of surface temperature'

Page 5, line 6:suggest ' and especially its Mediterranean component affects precipitation over..'

Page 5, line 15: suggest 'The gallery is developed between. . .'

Page 5, lines 30-31: There is a discussion here about the hydrology of the ER76 drip site and it is mentioned that there is a two month delay between the drip-rate response and aquifer recharge.Some important questions then arise.Is the delay simply a reflection of a piston-type response or is there more complex mixing of the waters taking place.One useful piece of information that should be provided for the reader if possible is whether or not the dˆ18 O of the drip waters change through the year, i.e. to what extent is the drip water buffered (mixed) with respect to dˆ18 O, or does it change seasonally in phase with rainfall dˆ18 O?This could have important implications for the subsequent interpretation of the speleothem dˆ18 O data.

Page 6, line 20:Please check if this reference really should be Frisia et al

(2006)?Should it be Frisia et al. (2003)?

Page 11, lines 1 & 2:Linking the 25 year cyclicity to the NAO here is an interpretation, not simply data description and therefore should be moved to the Discussion section (section 4).

Page 11, line 24:suggest 'discussed extensively' instead of 'extensively discussed'

Page 12, lines 20-30:This section could be shortened somewhat.It is probably not unusual that dˆ13 C values in speleothems are higher than predicted from simple equilibrium fractionation factors given the unidirectional nature of the degassing process.

Page 13, lines 22 and 25:The meaning of the numbers in [] brackets after the correlation coefficient in both cases is not clear.

Page 14, lines 1-3:The statement that 'all approaches suggest an influence of stable isotope fractionation under conditions of disequilibrium' may overstate the case for disequilibrium with respect to oxygen isotopes.It was stated on the previous page that 'oxygen isotope fractionation occurred close to isotopic equilibrium'.

Page 14, lines 15-16:The shift to more negative soil water dˆ13 C values on a seasonal basis at Ernesto could presumably in part reflect greater root respiration (more active vegetation) during the warmer season, in addition to the enhanced temperature sensitivity of bacterial decomposition of soil organic matter.

Page 14, lines 24 and 25:'About 80% of the annual calcite precipitation at Grotta di Ernesto occurs during winter months when surface temperature is lower than the cave temperature (Miorandi et al., 2010)'.It would be helpful for the reader to clarify the significance of this finding in terms of the isotopic signals likely to be captured by a speleothem growing mostly in winter.This is linked to the query about any seasonality in drip-water dˆ18 O.Another question is whether the cave air temperature remains relatively constant through the year or whether seasonal ventilation results in significant cave air temperature changes at theses speleothem sites?For example, in a hypothet-

ical case of invariant drip-water dˆ18 O (a well mixed drip) and constant temperature, the significance of mainly winter precipitation of calcite in terms of the isotopic signal captured by the speleothem would be reduced.

Page 14, lines 26 and 27: It is not clear to me that the content of lines 26 and 27 follow logically from the preceding sentence (lines 24 & 25) as implied by the word 'thus'.

Page 15, lines 3 to 15, figure 3 and section 4.1.5:An important observation is that the temporal shift to lower (more biogenic?) dˆ13 C through the course of the Holocene is accompanied by a shift to higher dcf values.At first sight this appears contradictory.However, the explanation offered (i.e. a change in the 'quality' of the soil organic matter undergoing digestion as a result of soil development during the Holocene seems to be reasonable.One confusing statement here (line 10) relates to the mention of 'bacterial degradation of leaf molecules'.This is confusing because it is not clear whether the authors consider 'leaf molecules' to be representative of labile or recalcitrant carbon pools. As shown by Glaser and Knorr (2008) and also argued by Rudzka et al. (2011), it is the labile (not the recalcitrant) pool that typically displays slightly lower dˆ13 C.It would be interesting to calculate if the slope of the data on the dcf – age diagram (figure 3a) because some of this could be explained by simple ageing of the soil organic carbon during the Holocene at this relatively cold site where soil carbon may be stored.

Page 15, line 30 and page 16, lines 1 & 2:The statement that 'this contradicts the previous interpretation of McDermott et al. (1999)' may be too strong because it later becomes clear that the authors have difficulty in interpreting the oxygen isotope data in terms of a single climate variable, and it may be that rainfall amount has some role to play (e.g. see discussion in lines 8-13 on page 17).Overall the discussion of dˆ18 O is rather difficult to follow and should be shortened.The main climate interpretation is based on growth rate arguments and dˆ13 C which is much clearer.

Page 17, line 24:Please refer to Hendy (1971) when introducing the concept of 'open'

vs. ‘closed’ system.

Page 17, line 31 and page 18, lines 1 & 2:The interpretation of increasing dcf in terms of a greater input of ‘old’ soil-derived carbon seems fine, but is not easy to reconcile with the simultaneous trend towards more negative dˆ13 C.The explanation offered (changes in dˆ13 C in labile vs. recalcitrant pools) appears to be the wrong way around as mentioned above?

Page 19 and figure 7:It is unsurprising the Corchia stalagmite CC26 shows quite different trends compared with ER76 (and Savi & COMNISPA) because as shown by McDermott et al. (2011), its oxygen isotope data reflect a predominantly Mediterranean (not N. Atlantic) source.Unlike Savi, the CC26 dˆ18 O data plot well above the low-frequency Atlantic-European longitude-dˆ18 O regression trends defined by McDermott (2011) throughout the Holocene, indicating a clear Mediterranean vapour source.

Page 21, lines 11-24:Some of this text repeats points made about the NAO at an earlier point in the manuscript and can be shortened or omitted.

The authors should consider that the Atlantic multidecadal oscillation (AMO) has a cyclicity of c. 65-80 years, similar to the identified 60-70 year peak.Overall, the evidence for a solar signal is not very strong and it could be argued that the title of the paper already overstates the case for solar forcing.

Figure 3: Please number the ‘texture code’ axis in figure 3b.

Figure 7:Please show the timing of Holocene IRD events.

Interactive comment on Clim. Past Discuss., 8, 909, 2012.