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.
We thank the anonymous reviewer for his thorough review. In particular, the critical discussion of the $\delta^{13}C$ and DCF data helped to improve the MS.
We agree that the discussion section is long. However, considering the detailed discussion of all proxies, we prefer not to shorten this section substantially.
After including the relationship between the NAO and winter temperature in the cave area in the revised MS, the indication of an influence of the NAO became even stronger. We also emphasize that all observed cycles have counterparts in the solar spectrum, which at least indicates that there may be a relationship. Thus, we prefer to leave the title as it is, in particular because we are quite cautious with our conclusions.

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: : :.)
Modified as suggested

Page 3, line 31:suggest ‘ which enabled the calculation of surface temperature’
Modified as suggested

Page 5, line 6:suggest ‘ and especially its Mediterranean component affects precipitation over..’
Modified as suggested

Page 5, line 15: suggest ‘The gallery is developed between: : :’
Modified as suggested

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.
We agree that this information is important and included a short discussion of the drip water $\delta^{18}O$ data. Drip water $\delta^{18}O$ shows only low variability throughout the year (ca. 0.2 – 0.4‰) and no seasonal signal suggesting a well mixed water reservoir above the cave and a piston-type response.

Page 6, line 20:Please check if this reference really should be Frisia et al (2006)? Should it be Frisia et al. (2003)?
This reference is, indeed, Frisia et al. (2006) and provided to document the 'history' of the development of the chronology. The first chronology for ER76 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).

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).
We do not explicitly link the 25 a-cycle to the NAO here, but provide the information that the NAO exhibits a corresponding cycle. Thus, this statement does not represent a preliminary interpretation of the data. No changes made.

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

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.
It is true that disequilibrium carbon isotope fractionation is not unusual in speleothems. However, these data are important to understand the processes affecting the $\delta^{13}C$ signals of speleothems at Grotta di Ernesto. They are also important for the discussion of the oxygen isotope data. Thus, we prefer to keep the provided details.

Page 13, lines 22 and 25: The meaning of the numbers in [] brackets after the correlation coefficient in both cases is not clear.
The numbers in brackets denote the 95%-confidence intervals for the correlation obtained from block bootstrap resampling, which takes into account serial dependence. This information has been provided already on the previous page, but for reasons of clarity, we repeat it here in the revised MS.

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'.
We clearly say that the disequilibrium effects are 'particularly pronounced for carbon isotope fractionation' in order to clarify that the effect is much stronger for $\delta^{13}C$. However, the other tests indicate that $\delta^{18}O$ may also be affected by disequilibrium processes. No changes made.

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.
We agree that this is an alternative/additional explanation for the observed seasonal signal in soil water $\delta^{13}C$ values at Grotta di Ernesto and revised the MS accordingly.

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.
We agree that these are interesting points. Cave temperature is very constant throughout the year at 6.7° C, and this information has been provided in the revised MS. We also included a short paragraph discussing that drip water $\delta^{18}O$ shows only low variability throughout the year (ca. 0.2 – 0.4‰) and no seasonal signal. This suggests a well mixed water reservoir above the cave. Thus, the effect of enhanced calcite precipitation during winter months should be mainly reflected in lamina thickness and $\delta^{13}C$, which are both modulated by degassing. Since isotope fractionation under conditions of disequilibrium cannot be completely excluded for $\delta^{18}O$, these processes may also have a (weaker) effect on $\delta^{18}O$. However, as explained in detail in the discussion of the $\delta^{18}O$ variability, evapo-transpiration suggests that major recharge of the aquifer occurs during winter months. Thus, the $\delta^{18}O$ signal most likely is a winter rather than an annual signal.

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'.
In section 2.2, we explained that cave air pCO_2 varies seasonally at Grotta di Ernesto and is modulated by air advection due to the seasonal difference between surface air and cave temperature. Cave air pCO_2 is lower in winter than in summer, which results in increased degassing of CO_2 from the dripwater, higher supersaturation with respect to calcite and, thus, higher calcite precipitation rates during winter. The effect of degassing is, thus, particularly pronounced during winter. We have included 'and cave air pCO_2 is lower than during summer' in the previous sentence to make this clear.

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.
We agree that the statement concerning bacterial degradation of leaf molecules was not clear. As discussed by Rudzka et al. (2011), the contribution of labile and recalcitrant organic matter to the soil gas CO_2 'cannot be distinguished from each other using $\delta^{13}C$ measurements alone', even if there seems to be 'some evidence that more recalcitrant soil carbon pools of C_3 bulk organic matter are characterised by higher $\delta^{13}C$ values compared with the coexisting labile pools (Glaser and Knorr, 2008)'. This suggests that the interpretation provided in our paper is indeed not correct. However, we want to emphasize that the $\delta^{13}C$ value of recalcitrant organic matter is, at most, 1-2 ‰ higher than that of the labile material (Glaser and Knorr, 2008; Gerzabek et al., 2001; Stemmer et al., 1999). Thus, only a small change of about 0.2 to 0.4 ‰ is expected for variable contribution of labile and recalcitrant organic matter to the soil gas CO_2 reservoir. For these reasons, we deleted the statement concerning bacterial degradation of leaf molecules in the revised MS.
Our other explanation, which suggests a progressive change in soil thickness and composition above the cave, is still valid. A mature soil containing more soil organic matter is characterised by higher soil pCO_2 values, and the observed decrease of ca. 1 to 2 ‰ is consistent with this scenario (Cerling, 1984).

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.
We moderated the statement in the revised MS and now write 'this does not confirm the previous interpretation of McDermott et al. (1999)'.
We did not shorten the discussion of the δ^{18}O data. We agree that the interpretation of the δ^{18}O data is complicated and the interpretation in terms of past climate variability is difficult – despite of the long term monitoring program. However, we consider these data as an interesting example for the complexity of climate proxy data in speleothems. Actually, one of our conclusions is that 'the δ^{18}O signal seems to be influenced by a complex interplay of several climate parameters and is, despite of the wealth of the monitoring data, difficult to interpret.' No changes made.

Page 17, line 24: Please refer to Hendy (1971) when introducing the concept of 'open' vs. 'closed' system.
The corresponding reference has been added.

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?
As explained above, the effect of variable contribution of the labile and the recalcitrant pools to total soil air δ^{13}C is relatively small and has, thus, been deleted in the revised MS. The other explanation offered (i.e., a progressive change in soil thickness, increasing soil pCO_2, larger contribution of old ^{14}C), however, is consistent with the data.

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 Mc- Dermott 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.
We absolutely agree with this comment and added a sentence referencing the results of McDermott et al. (2011). In our discussion, an agreement between ER 76 and the COMNISPA record is interpreted as dominant influence from the North Atlantic, whereas similarity between ER 76 and CC26 is interpreted as Mediterranean influence. Thus, we utilize the CC26 record as a Mediterranean signal in our interpretation.
However, we want to highlight here that the authors themselves (Zanchetta et al., 2007) and many other publications (Drysdale et al., 2009; Drysdale et al., 2005; Drysdale et al., 2004) interpret the δ^{18}O signals recorded in speleothems from Corchia cave as reflecting North Atlantic climate variability because the cave site receives its precipitation 'mostly from North Atlantic frontal systems'.

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.
Since the interpretation of the complex proxy signals recorded in ER 76 in terms of past variability of the NAO is not straightforward, we shortly repeat the key points about the NAO here. This should help the non-expert reader to understand the offered explanation.

We agree that our interpretation in terms of solar variability is solely based on the spectral analysis, which does not provide a mechanism how solar variability influences the proxy signals at the cave site. However, all observed cycles have counterparts in the solar spectrum, which at least indicates that there may be a relationship. In addition, we are quite cautious with our conclusions. Thus, we prefer to leave the title as it is.

Figure 3: Please number the 'texture code' axis in figure 3b.
Modified as suggested

Figure 7: Please show the timing of Holocene IRD events.
As explained in the MS and documented in detail in Mangini et al. (2007), the COMNISPA record shows a strong correlation with the Bond et al. (2001) IRD events. Cold phases in the COMNISPA record coincide with the Holocene IRD events. Since the figure already contains six records, includes the grey boxes highlighting the warm phases at Grotta di Ernesto and is quite complex, we prefer not to include further records/details.

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