Response to reviewers for the Wendt et al. manuscript: "Precise timing of MIS 7 sub-stages from the Austrian Alps"

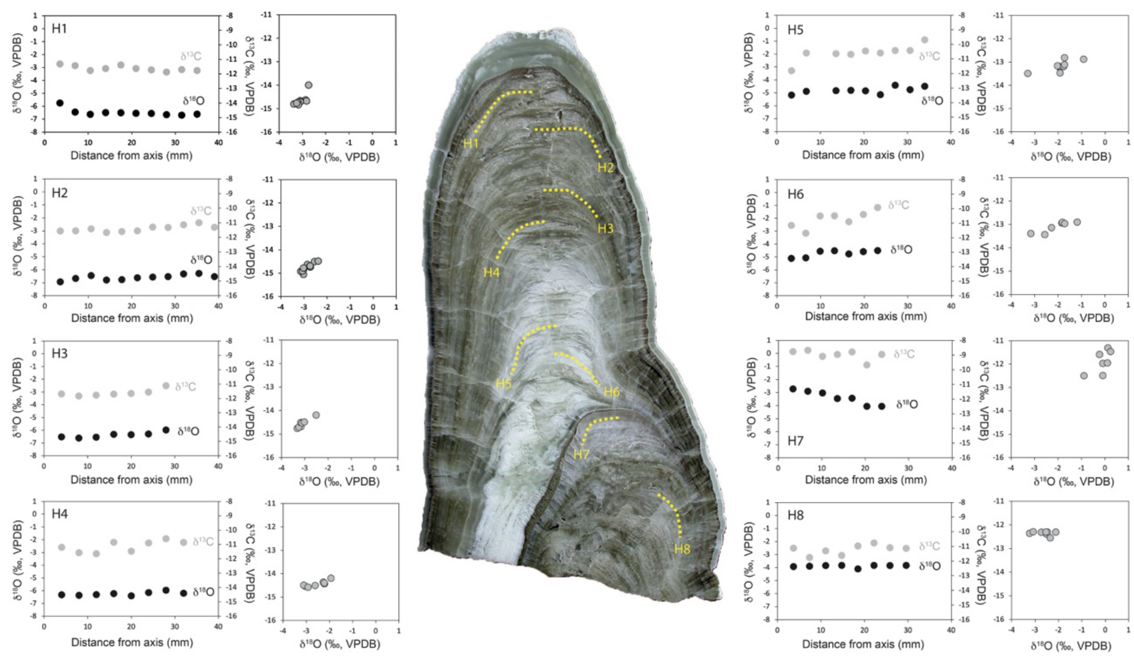
We greatly appreciate the comments and critiques made by the two anonymous reviewers. Below is a list of individual comments and questions. Our responses are in blue:

Reviewer #1

1. I am not very convinced of the data presented from the two new stalagmites, and I think the authors should consider the benefits of including them here.

We agree that our original manuscript lacked sufficient evidence to ensure that the two new stalagmites (SPA 146 and 183) precipitated in isotopic equilibrium with dripwaters. To address this issue, we have re-sampled 8 Hendy tests from SPA146 and 5 from SPA183 (see new supplementary figures below). The results present a more comprehensive picture of the two stalagmites. The lack of 18O and d13C covariation suggest that SPA 146 and SPA 183, like SPA 121, grew in isotopic equilibrium.

It's important to note that the stable isotope values from each stalagmite do not replicate each other perfectly. This may be due to several factors (see responses below for additional discussion). However, we argue that the isotopic trends generally agree and can be interpreted in relative terms as a proxy for past regional climate changes. To this end, we maintain that stalagmites SPA 146 and 183 provide valuable insight into the timing of and regional cooling at the onset of MIS 6.



New Supplementary Figure A (to replace supplementary figure 3)

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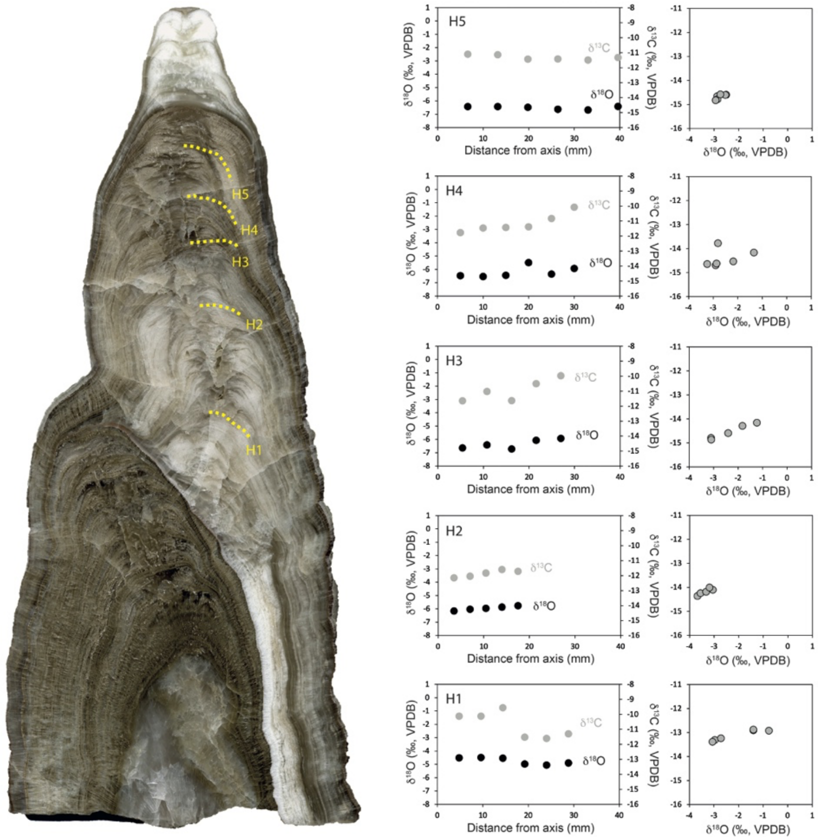

New Supplementary Figure B (to replace supplementary figure 3)

2.- Source of precipitation. The authors indicate two main sources of precipitation in this region, which can be differentiated by the d18O isotopic values. I agree with this statement, but I consider that Atlantic sourced precipitation may not be much more negative than the Mediterranean one, depending on the moisture uptake along the longer pathway. Rainout effect is sometimes compensated by the more positive recycled moisture that is being incorporated in the way from the source to the Central Alps. It is then important to take into account the moisture recharge through the long pathway as, sometimes, the result is an enrichment derived by the effect of enriched inland moisture compared to ocean moisture. See, for example, Chakraborty et al., (2016) and Krklec and Domínguez-Villar (2014).

Our original text cited a combination of site-specific modern precipitation and dripwater calibration studies that support our interpretation of Spannagel d18O. An expanded discussion of precipitation d18O in the Austrian Alps is now provided in a new section titled "climate setting"

We agree with the reviewer that, due to rainout effects, the difference in d18O of North Atlantic vs Mediterranean sourced rainfall to our study site likely minimal. We have reworded the text emphasize that temperature is the dominate control on Spannagel d18O, with only minimal (amplifying) effects related to source.

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3. Similarity with d18O monsoon records. The authors indicate several times in the discussion the high similarity with Asian monsoon records (lines 175, lines 235, etc); I think these statements should be modulated as I observe many differences in timing and pattern in Fig. 4. Both the similarities and the differences must be clearly described. For example, the time of TIIIa is completely different, also the pattern. The time of 7d as defined in Spanagel (234-216 ka) does not coincide at all with Chinese monsoon timing. Please indicate and explain potential mechanisms for those differences.

We agree that our original text lacked a full-picture explanation behind the interpreted similarities between the Chinese monsoon and Spannagel records. For example, we highlighted the similarities between variations in the Spannagel $\delta^{18}O$ and Chinese Monsoon $\delta^{18}O$ on sub-orbital timescales, but did not explain why the two locations are decoupled on other timescales (e.g. orbital) which may cause confusion to readers. We have now expanded our discussion of Spannagel $\delta^{18}O$ vs Chinese Monsoon $\delta^{18}O$ and revised Figures 3-5.

Regarding the timing of 7d: we agree with the reviewer that Fig. 4 and its related text may render confusing without a clear reiteration of age uncertainties associated with both records. For example, the period of maximum isotopic depletion of MIS7d in Spannagel (229.2 ka) appears early, but is actually within the ~1 ka age uncertainties of the Chinese Monsoon record (228.2 ka). Nevertheless, our original text lacked an emphasis on the respective age uncertainties of each record. We have reworded these and similar statements to include a comparison of age uncertainties.

The end of MIS 7d is marked by the onset of TIIIa, which according to Cheng et al. 2016 occurs in the Chinese monsoon record at 217.1 ±0.9 ka. The timing of TIIIa is therefore well within uncertainties of both records. To further emphasize this, we have expanded the TIIIa discussion section and revised Figure 4.

- Line 26. I miss one or two references here to support this statement.
We added the citation PAGES (2016) as a summary reference for MIS 7 substages.

- Line 140. Replication just happens during very short periods of time, if any, and the values and trends are not so well reproduced. I would not use those criteria for discarding kinetic effects.
We have re-sampled multiple Hendy tests for SPA146 and 183 in order to provide a more robust test for possible kinetic effects (see response to first comment). It is true that the absolute d18O and d13C values from each stalagmite do not replicate each other perfectly. This may be due to several factors (see response below). However, we argue that the isotopic trends generally agree between stalagmites, particularly in their trend towards depleted values at the onset of MIS 6. Returning to the reviewers' point, we no longer argue that replication is the main line of evidence against kinetic effects; instead, we point to the evidence provided by the new Hendy test analysis.

- Line 147. This just applies for SPA21, the other two stalagmites display more negative values. Please, explain why.
Spötl et al (2008) noted that prior calcite precipitation (PCP) likely influenced the stable isotope values of SPA121. Two lines of evidence in this study supports this hypothesis. First: SPA 121

grew very slowly relative to SPA 146 and 183, suggesting a slow drip rate (and thus, higher likelihood of PCP). Second: low d18O intervals recorded in SPA 121 coincide with high d13C (e.g. during the MIS 7a-6 transition), suggesting the influence of a kinetically-controlled process, such as PCP, during cold intervals. We thank the reviewer for raising this question and have added a more detailed explanation to the text.

- Figure 4. I would suggest adding to this figure the duration of MIS7 substages (lines or shaded squares) to really see when they start and finish, not only the "peak" indicated by the name in Fig. 4D.
Done

- Figure S3. I think these data correspond to two different laminae in every stalagmite. Please indicate it in the graph or caption.
We have replaced Figure S3 (see response to first comment)

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Reviewer #2

In my opinion, the authors misuse the terms 'transition' and 'termination'. The point I want to make is that speleothems do not preserve terminations or other MIS transitions per se. Ocean sediments do

We agree that our interpretation and use of the term "termination" was used too liberally in our original text. As the reviewer correctly pointed out, this paper is a study of d18O changes speleothems from the central European Alps. Although evidence suggests that Spannagel d18O is highly sensitive to changes in the North Atlantic realm, speleothems (as with all terrestrial archives) cannot be used to directly infer large-scale changes in the ocean-cryosphere system. To this end, we have revised the manuscript to avoid such sweeping statements. We also expanded the first paragraph of our discussion section to emphasize that Spannagel d18O records the regional expression of climate changes associated MIS 7 sub-stages and terminations.

The authors do refer to ocean records in the ms (LR04, MD01-2444: Figure 4) but do not determine exactly how the Alpine speleothem 18O profile links to these records, apart from references to alpine warming coeval with SST increases (and the converse)… The case for a link between the cave and ocean records through the whole time interval must be better developed.

We agree that an expanded discussion of this topic is needed. North Atlantic sediments show clear evidence of SST warming and deposition of ice-rafted debris associated with the rapid transition of TIII and TIIIa (e.g. Martrat et al. 2007; Channell et al. 2012). The onset of these oceanic changes occurs within age uncertainties with an abrupt enrichment in Spannagel d18O. The close coincidence between marine and terrestrial records suggests that warming temperatures in the North Atlantic realm drove warmer winter temperatures in the central Alps during these time periods. However, is it also clear that Spannagel d18O does not remain coupled with North Atlantic SSTs throughout MIS 7, such as during MIS 7d, which suggests that there are multiple driving forces that influence winter temperatures in the central Alps. We have added a discussion of the complex link between the North Atlantic and central Alps in the first paragraph of the discussion section and added a new section called "climate setting" to provide greater context.

Regarding the interval of stable d18O values between 247 and 242 ka: to which part of the ocean record does this correspond? Is it the 'late MIS8 glacial' before the termination actually starts (it would seem so, based on the authors' claims of a short termination that starts after this isotope plateau), or is it really part of the period of ice-sheet melting associated with the termination, as implied in Cheng et al. 2016 and 2009, and Pérez-Mejías et al. 2017? If the latter, which in my opinion (based on all the evidence) is more realistic, the quoted ages and durations of T3 and potentially other transitions listed in table 1 have little meaning.

We agree - our record cannot rule out the possibility that the depleted isotope plateau between 247 and 242 ka is associated the onset of ice sheet melting associated with Termination III isotope plateau. It is therefore incorrect to define the timing and duration of TIII in the strict terms outlined in our first draft. To avoid confusion, we have eliminated table 1.

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From what I can determine, it seems that the speleothem did not even capture all of T3, if you take into consideration previously published speleothem records (Cheng et al. 2009 and 2016 and Pérez-Mejías et al. 2017). It obviously captures all of 7e, the 7e/d transition and the 7d/c transition (T3a), etc. but exactly how do the boundaries of these transitions in the ocean record tie to the speleothem 18O?

If one defines the main portion of TIII as the period of maximum IRD deposition in the North Atlantic between 243-240 (Channell et al. 2012), then the period between the onset of speleothem deposition at 247.3±0.2 ka and the abrupt 3‰ increase of Spannagel d18O from 242.5 to 241.9 (±0.3) ka is well within uncertainties of the IRD deposition event. This time frame is also consistent with the abrupt changes with vegetation productivity in the Iberian Peninsula (241.6–240.7 ±1.6 ka; Pérez-Mejías et al. 2017) and Chinese Monsoon intensity (242.8–241.01 ±0.9 ka; Cheng et al. 2009, 2016). Spannagel may not cover all millennial-scale events leading up to TIII, but we disagree with the reviewer that Spannagel does not capture TIII.

We have added a section titled "climate setting" to provide further details on speleothem 18O.

In the context of the above, I would like the authors to carefully consider exactly what the abrupt speleothem 18O changes mean at this high altitude cave? For instance, are the abrupt increases examples of Bølling-Allerød-like or YD-Holocene-like events? Hard to say – age uncertainties, although small in percentage terms, are still too large to test whether the true duration of these events are comparable. But this is tantalising and really important because it implies that T1 was not alone with its two rapid NH temperature jumps, and that T3 likely had at least one comparable rapid warming (at least in this part of the N Hemisphere) well after it started. We know from T1 that the BA transition occurred ~5 kyr into the termination.

Comparisons of the millennial scale shifts in North Atlantic climate have been previously examined in Pérez-Mejías et al. 2017 and Cheng et al. 2007. For example, Cheng et al. 2007 define the timing of weak and strong monsoon intervals: YD-III and BA-III. Pérez-Mejías et al. 2017 suggest that the S8.2 IRD event in the marine record triggered Heinrich Stadial-like conditions in southwestern Europe, similar to Heinrich Event 1 prior to TI and Henrich Event 11 prior to TII. Our findings support the timing of these shifts in North Atlantic climate. However, we agree with the reviewer that further comparison and discussion is needed in our manuscript. We have expanded the discussion of millennial scale events leading up to TIII and TIIIa in discussion section. We also highlighted the intervals of Heinrich Event-like events in Fig. 4 and added an explanation to its associated caption.

There is an alternative explanation the authors should consider too: is the speleothem 18O acting like an 'on-off' switch, i.e. does it represent binary swings between (i) periods when the glacier is present above the cave (when basal meltwaters with low 18O values derived from strongly 18O-depleted glacial or stadial snowfall occurring 1000-1200 m higher than the cave itself, near the Hintertux glacier summit ~3500 m a.s.l.) and (ii) periods when the glacier retreats during interglacials and interstadials and exposes the cave recharge area to direct infiltration (at ~2300

m) of isotopically enriched rainfall and in situ snowfall? This could explain the almost square-wave form and amplitude of the speleothem isotopic series (otherwise for the MIS7a-MIS6 transition, for example, we must consider 20 deg C or more of temperature depression plus a little extra for possible changes in moisture source, given the >6 per mil decrease in speleothem 18O). This raises the question of whether the sharp increases and decreases in d18O are really a local effect of ice retreat, whose phasing with respect to regional warming and cooling (e.g. the rises and falls in SST in MD01-2444) is not as closely coupled as the authors think.

The reviewer proposes an interesting hypothesis. However, several lines of evidence argue against this. As outlined in Spötl et al (2008), there is strong evidence that the cave was continuously buried by the local glacier during MIS 7 (and actually most of the Pleistocene – see Spötl & Mangini, EPSL 2007). The sampling location of SPA 121 was underneath glacier ice as recently as the end of the 19th century and very close to the ice margin still at about 1920 AD, i.e. well within an interglacial.

Another line of evidence is that the C isotope composition remains within the limited range of host rock values over tens of thousands of years (except for some of the cold periods when we see the high, and likely kinetically controlled, values which we interpret this to be a reflection of partial freezing of the karst). If the area above the cave would have become deglaciated e.g. during MIS 7a, we would expect colonization of alpine vegetation above the cave (within a few decades as shown by many recent observations), resulting in a drop in d13C values. This is not observed.

Intermittent waxing of the glacier, as suggested by the reviewer, may also result in meltwater and sediment pulses. There is currently no evidence in the petrography of the stalagmite nor in the d18O values for melt pulses.

Finally, it is incorrect that the precipitation that fed cave dripwaters fell 1000-1200 m higher. The highest summit of Hintertux glacier (Olperer) is 3476 m, whereas the gentle glacier basin (i.e. main accumulation area) is located at about 2800-3000 m. This is only a few hundred meters above the speleothem sampling site.

Regarding the large decreases in d18O: TII records from Spannagel show a shift of about 3.5 to 4 per mil (Spötl et al., Geology 2002; Holzkämper et al. GRL 2004). This amplitude is similar to the shifts observed in SPA 121 during MIS 7, with the exception of the 4.5 per mil decrease at the end of MIS 7a. Following the end of SPA121 growth, an additional 1.5 decrease in d18O is recorded from stalagmite SPA 183 – resulting in a 6 per mil decrease in d18O between MIS 7a and 6. The difference in absolute d18O values between SPA 121 and SPA 183 was previously addressed in reviewer 1's responses. In short, we do not interpret the cumulative 6 per mil decrease to be a climatic (or temperature) drop alone – but instead a function of different kinetic components between stalagmites.