

Interactive comment on “Precise timing of MIS 7 sub-stages from the Austrian Alps” by Kathleen A. Wendt et al.

Anonymous Referee #2

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This article proposes new radiometric ages for glacial-interglacial and stadial-interstadial transitions across marine isotope stage (MIS) 7. The durations of these transitions are also presented. To date, few records offer precise ages for these events, so this ms is potentially of much interest. In summary, the main time series presented (SPA121) comprises new age data applied to a previously published isotope record. The new ages significantly improve the precision of the SPA121 time series, which is a useful advance. The series from the two other stalagmites provide additional data that support the authors' claim that the composite record reaches into MIS6 and therefore covers the MIS7a-MIS6 transition.

The main issue I have with this ms is the authors' claim that the sharp speleothem oxygen isotope (^{18}O) changes (both increases and decreases) can be used to date

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the various MIS 7 transitions. To me, this is not convincingly demonstrated because I feel the use of the terms ‘MIS 7e-d transition’, ‘termination 3’, etc. is not done with full recognition of what these important terms actually mean. I will use Termination 3 as an example. Put simply, a Pleistocene termination is a global event representing the entire period over which a large percentage of Earth’s cryosphere decays (mostly Northern Hemisphere ice sheets). Termination 1 started at around 20 ka (Denton et al 2010 Science) and spanned well over 10,000 years. Previous radiometrically based records of T3 (Cheng et al. 2009, 2016; Pérez-Mejías et al. 2017) imply shorter durations but no where near <1 kyr. Here, Wendt et al. state that T3 occurred over just ca. 700 years (table 1). Besides the potential physical impossibility for melting so much ice so quickly, this short time frame also means very large adjustments to existing age scales for marine time series (incl. LR04) covering this period (I don’t just mean wholesale shifts of the ocean record to older or younger U-Th based ages of events – this is to be expected because of limited age control in these records – but the extreme squeezing and stretching of sedimentation rates to unrealistic levels). A 700-yr termination would also drastically change the time scale of Antarctic ice cores, which although having inherent uncertainties of its own also has its limits in terms of how much accumulation rate and ice-flow modelling change can be tolerated. Termination 3a is a similar story: it is also argued to have been completed in just ca. 700 years.

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. Speleothems (and other archives) preserve the local or regional expression of climate changes associated with these transitions. Therefore, in assigning ages of MIS transitions using a speleothem chronology one must first resolve how the climate signal in the speleothem actually records such transitions and how these are linked to relevant ocean record(s). 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). To cite Termination 3 again. . . Regard-

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ing 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. The case for a link between the cave and ocean records through the whole time interval must be better developed. This needs to bear in mind that the age and duration of a given climate transition is global and based essentially on changes in global ice volume, and therefore is best resolved in ocean records. A speleothem (or lake record, etc.) will respond to this event according to local and regional climate dynamics. 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?

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.

There is an alternative explanation the authors should consider too: is the speleothem

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$\delta^{18}\text{O}$ 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 $\delta^{18}\text{O}$ values derived from strongly $\delta^{18}\text{O}$ -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 $\delta^{18}\text{O}$). This raises the question of whether the sharp increases and decreases in $\delta^{18}\text{O}$ 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.

This all sounds rather negative, but I encourage the authors to re-consider the meaning of MIS transitions, as global events with local and regional expressions. This is exceptional radiometric dating – I hope the authors can apply these precise results in a more meaningful way.

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