We thank Nereo Preto (Reviewer #1, RC C1190), Troy Rasbury (Reviewer #2, RC C1352) and Silvia Frisia (Handling Editor, EC C1361) for their helpful comments on the manuscript. In this response letter, we go through both reviews in detail and explain how we intend to revise the manuscript. Further intended manuscript changes: (1) we thank both reviewers in the acknowledgements and (2) we update references Scholz et al. (in press) and Fohlmeister (submitted manuscript).

Reviewer #1, RC C1190

This manuscript tackles a integration of timescale errors in paleoclimatic time series. This was rather neglected in previous studies. As a method paper, it is welcome and well written. Methods are described with sufficient detail to reproduce the proposed algorithm, and are sound. I have only a minor concern about the discussion of methods. The authors apply two strategies to create an age model, StalAge and iscam, the second assuming good correlation between roughly coeval proxy records. This is however quite a bold assumption, especially where stalagmites from different caves are compared, and introduces an element of circular reasoning when the proxy records are discussed (e.g., the d18O records may have consistent trends in some time intervals, but such trends were already implicitly assumed consistent for age calibration). This is not discussed in the manuscript but it could be worth to note. It is unclear to me how extensive this problem could be for the general pool of paleoclimatic time series. Sure enough, it needs to be discussed here, where the time coincidence of a d18O oscillation between three stalagmites is taken as proof that a climatic event exists. How relevant this time coincidence could be for the iscam-calibrated d18O series? Probaly not much, as the proxy oscillation seems to be there also in the StalAge-calibrates series, but a discussion is necessary.

Response. Thank you very much for this clear remark! We completely agree that we should alert readers of the danger of circular reasoning (i.e., iscam presumes a correlation between different records to optimize the age model). We also agree that we should point out that this danger is, on the other hand, not excessively large for the datsets analysed in the paper. The major reason for this is, as stated by Reviewer #1, namely the agreement with the results obtained from timescale-construction using StalAge.

Manuscript changes. One point to be added to Section 5 (Conclusions).

The manuscript then goes on discussing a interpretation of the proxy records, assuming that calcite d18O in these stalagmites is a proxy for temperature. The possibility that other parameters along with temperature might influence the d18O from stalagmite calcite is discussed too briefly. This is in my view the weakest part of the manuscript. In general, the d18O of speleothem calcite is considered a proxy for precipitations (Fairchild and Baker, 2012). For the specific case of Bunker Cave, monitoring did not get as far as concluding that a paleotemperature could be inferred from the d18O in speleothems (various references, cited in the manuscript). The whole discussion about paleoclimatic implications should be revised in the light of this uncertainty on the meaning of oxygen stable isotopes.

Response. (1) Strictly speaking, we do not assume that “that calcite d18O in these stalagmites is a proxy for temperature”; we rather assume (manuscript, Section 2.3) that d18O changes are a proxy for changes in both temperature and precipitation. (2) We do not agree with the assertion that “the d18O of speleothem calcite is considered a proxy for precipitations (Fairchild and Baker, 2012)”. Based on our comprehensive experience, the climatic interpretation of stable isotope proxy signals in speleothems depends considerably on the studied system (region, cave) and timescale (as noted by Fairchild and Baker (2012) on p. 321). In the ”data paper” on the Bunker Cave stalagmites (Fohlmeister et al. 2012, Section 4.3 therein; cited in manuscript), the authors clearly say that both temperature and precipitation changes are reflected by speleothem d18O changes; and that this climate–proxy relation holds for a rather large regional area (i.e., including the Atta cave, where stalagmite AH-1 was sampled). We agree that the interpretation would have been easier if the proxy signal had been clearer in one direction. However, we feel that we have already dealt adequately with this point throughout the manuscript, in particular because we always write ”warm/wet” and “cold/dry”.

Manuscript changes. None.

Minor comments:

P1974 L16: not excessively large

Could use a less vague phrasing, e.g.: "are one order of magnitude smaller than other errors"

Response. Since the effects of dating errors vary to some degree between records and with time (manuscript, Section 4.2: 20.5%, 14.6%, 8.2%, 2.4%, 11.3% and 0.4%), we intentionally used a vague phrasing.

Manuscript changes. None.

P1974 L20: Our analyses cannot unequivocally support the conclusion that current regional winter climate is warmer than that during the MWP.

This conclusion should be avoided: the three records show all possible combinations of relationships between the MWP and the Recent, hence, they provide no relevant information at all. Furthermore, the d18O is not a proxy of temperature.

Response. Strong disagreement. (1) Indeed, we use d18O as a proxy for temperature changes (see above). (2) The sentence (p. 1974, l. 20) is factually correct. The situation after the analyses and discussion, in which we unfortunately did not find unequivocal results regarding the MWP, is clearly different than before doing the analyses: namely, we have added evidence from three high-resolution archives. In addition, our results highlight the general complexity of obtaining quantitative statements from climate proxy records, and are, thus, of interest of the paleoclimate community and the speleothem community in particular. Should one publish only unequivocal results?

Manuscript changes. None.

P1975 L5: later part of the Holocene

substitute with "late Holocene"

Response. Holocene is roughly the interval 0–11 ka; our paper analyses the interval 0–8.6 ka. We want to communicate to readers that we do not study the whole Holocene, but only the middle and the later later parts.

Manuscript changes. We replace "later part of the Holocene" and similar expressions by "mid-to-late Holocene" and define this interval at first usage as 0–8.6 ka.

P1975 L11: Estimates without error bars are useless.

I see your point but this sentence carries no useful information and is substantially false: cases of estimates without error bars that are meaningful could be conceived.

Response. It is clear and understood by Reviewer #1 that our sentence is meant as an exaggeration: numerous studies have been published that make bold statements on guessed climate numbers without any indication on how large the uncertainty of such a guess is. We wish to keep the sentence for didactical purposes, but: Editor's voice welcome!

Manuscript changes. None.

P1981 L23: with high (low) _18O values indicating dry/cold (wet/warm) conditions. It is unclear here whether P and T are correlated o independent. This makes a lot of a difference, because if, e.g., wet-cold combinations are possible, then oxygen isotopes alone can't be used as a proxy for T as long as another variable is given that is a proxy for P.

Response. The paper discussing the paleoclimatic interpretation of the records in detail (Fohlmeister et al. 2012) argues that high d18O values are associated with dry and/or cold conditions and that low d18O values are associated with warm and/or wet conditions. Wet–cold is rather implausible.

Manuscript changes. None.

P1986 L3: Note that the time series plots (Figs. 2, 4, 5, 6, 7 and 8) show conventionally time t on the horizontal and time series value x on the vertical axis.

Drop this sentence, it's all in the figures and everyone can read.

Response. We thought it convenient for CP readers with less training in mathematical sciences to be reminded that the typical plots in time series analysis have other axis names ("x" is on the "y"/vertical axis and "t" is on the "x"/horizontal axis. Editor's comment welcome!

Manuscript changes. None.

Figure 1:

If some lines are invisible on a proof, print full page. In any case try to avoid the comment in the caption about invisible features. Please place the name of samples (stalagmites) directly on the plots, e.g., on the bottom right corner.

Response. The recommendation to place sample names in the plot violates standard conventions. The ”invisible features“: again, we thought it convenient if CP readers are alerted that they do have to consult the high-resolution figures and do considerable zooming to see what we say. Editor’s comment welcome!

Manuscript changes. None.

Reviewer #2, RC C1352

The manuscript “Effects of dating errors on nonparametric trend analyses of speleothem time series” by Mudelsee and others uses existing climate proxy records (oxygen isotopes) combined with radiometric age constraints with statistical techniques to evaluate three stalagtite deposits from Germany. Two existing programs and modifications of these programs, are used to produce trends in these proxy records through time. It is illustrated that the error envelope is larger, though the trends are identical when uncertainties on the ages are considered.

Overall the paper is a thoughtful contribution but could benefit from a discussion of the radiometric dating techniques and their uncertainties. For example, assumptions are made for both U-series and radiocarbon with regards to starting conditions. It appears that if an age doesn't agree with other ages it is thrown out, but what if the age that is thrown out is correct and the others are in error? For example, for radiocarbon, what if some of the samples experience reservoir effects, or for U-series, what if the assumptions for correcting for initial 230Th are invalid?

Response. We agree that the effect of potentially wrong assumptions for the dating methods may have an effect on the individual ages as well as the final age models. We also agree that a discussion of these effects would be generally interesting. However, these effects and in particular the detection and treatment of potential outliers have already been discussed in detail in the original publication presenting the age modeling algorithm StalAge (Scholz & Hoffmann, 2011). The purpose of this paper is to study the *effect* of using different age modeling algorithms on nonparametric trend analyses and not to discuss the algorithms themselves.

Manuscript changes. None.

As for the proxy data itself, it is admitted that the changes in oxygen isotopes can reflect temperature and or rainfall, such that it isn't possible to put absolute numbers on these changes. However, one profound problem with oxygen isotopes in cave deposits is that it has been demonstrated that they can form well out of equilibrium with their drip waters. It is significant that the three records show a similar pattern of winter conditions of warm cold warm between 6500 and 5100 years ago. The fact that all records are similar suggests this is meaningful. It seems dicey to make much of the records that are not in agreement with each other. Could there be differences in kinetic fractionation, etc? Could the opening to the caves have changed such that there isn't a good connection to the atmosphere?

Response. We agree that changes in kinetic fractionation could have played a certain role in the composition of the d18O profiles. We cannot exclude this entirely and like to notice that it is rather difficult to quantify the degree of kinetic fractionation for past time intervals. However, the artificial opening of the caves does not seem to have had much influence on the d18O of stalagmite calcite. For Bunker cave this was shown by Fohlmeister et al. (2012; cited in manuscript. We assume that a similar behaviour is valid for the nearby Atta Cave. If changes in kinetic isotope fractionation occurred, they should have resulted in a different signal between AH1 and both Bunker Cave stalagmites. Bu1 and Bu4 should then show more consistent d18O signals with each other because they grew in the same cave—and indeed, we found that the correlation between the d18O series from Bu1 and Bu4 is larger than the correlation between the series from AH1 and the series from Bunker Cave. This is a hint that changes in kinetics in both caves altered the drip water and consequently the rain water signal to some extent.

Manuscript changes. We will include one paragraph about changes in kinetic fractionation in the revised version of our manuscript in Section 4.4.

In summary, this manuscript fits well in the special issue "Advances in understanding and applying speleothem climate proxies" and is a significant contribution. I recommend it's inclusion in this special publication.