

Interactive comment on “Stable isotope record of Eemian seasonal temperature from MIS 5e tufa stromatolite; Somme Basin, Northern France” by J. Dabkowski et al.

Anonymous Referee #2

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I carefully read the comments made by the authors following my review. I am answering here some of the points they raised in this discussion, with the hope that this will both clarify my comments and help the authors to prepare a clearer, modified version of their paper.

As I said in my review, I acknowledge the importance and quality of the authors' data. I still find the paper as presented confusing. I can only encourage that it is resubmitted in a revised, clearer form.

Specific points raised by the authors:

- references: “High-resolution investigation of tufa stromatolites is a very recent, poorly
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developed topic, which explains the small number of references on this specific topic cited in our paper”. This is indeed true, and such novel studies should therefore be encouraged. However, the topic of what controls the isotopic composition of carbonate deposits is not, and at least a few additional references were cited by the authors in response to the first referee's comments. Why not include these in your paper? That would strengthen your discussion and help limiting further criticism on your interpretations.

- dating constrains and Eemian seasonality: the paper published by Antoine et al. in 2006 is in French. It may therefore not be accessible to the wider audience, and then essential dating information should be summarized again in your paper. And even if you don't think that establishing Eemian seasonality is the aim of your paper, I for one would be curious to see, at least, if the seasonal range you find is coherent with what is expected for the Eemian (from what we already know about this period), rather than to say “Considering (...) the lack of independent Eemian data for comparison, we are unable at present to prove or disprove this possibility without further investigation”.

- I don't think I said anywhere that you assumed that the “specified temperature range from the Caours stromatolite isotopic record was the ACTUAL water temperature seasonal range during the Eemian optimum”. I merely raised the point that, since you don't know (or at least I, after reading your paper, still don't know) which kind of water your stromatolites precipitated from, you may be comparing apples with oranges when you compare your record to the modern river seasonal temperature range. This should definitely be clearer in your revised version.

- Blue microfacies I I am happy to hear that the “I” pointed at by arrows in Figures 3 and 4 within the microfacies column and a blue-coloured area are not “blue microfacies I”. This definitely makes things better regarding both the isotopic record and the transitions between laminae. However, you will agree, I hope, that these figures are highly confusing and can definitely give the wrong impression, as also spotted by Referee 1: “Fig. 3. There is a lack of reference in the figure caption to the arrows pointing the

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microfacies I. The reason for them to be blue in colour should be specified. Further description in the main text for the lack of correspondence with the isotope signature is required.” Of course we have to judge your paper by the evidence presented, and this evidence included a very seemingly “blue microfacies I”.

- concept of “morphotaxon”: yes this needs to be clarified.

- “RIVERINE tufa”: I can agree that the word “riverine” may include everything pertaining to a river (by definition actually) along the river profile, including its spring. It never occurred to me, though, that it may be used also for lateral springs that would feed an already “fully-grown” river, hence the confusion. I maintain that if you are to use the word riverine in this sense it can lead to misinterpretations, and that the context of stromatolite deposition should be clarified. The idea that you focused on riverine (*sensu stricto*) temperature range for your interpretation is enhanced by the fact that you compare your results exclusively to the modern seasonal temperature range of the Scardon river, and not to any local springs.

- I do find “the Greek record and interpretation of Brasier et al. 2010 rather contradictory to the present study”, because of the position of the transitions between laminae (/hiatuses) and within the isotopic signal, which are definitely different for both records, and which I feel should be discussed in more details. For the Greek record, “sharp lamina boundaries correspond with clear discontinuities in isotope profiles indicating non-deposition and truncation of the temperature record” (“dry summer and cold winter conditions”). For the French record, “possible hiatuses in stromatolite fabrics appear to correspond to “mid-seasonal”, non-extreme conditions”. I think this is quite a different signal that is shown by both sequences, and I wouldn’t call them “rather equivalent”. The only similarity these two sequences show is that they (probably) record a seasonal signal. I think the differences between these records are actually far more interesting than their similarities, because that implies a very interesting point that could be discussed more: it may be assumed, by comparing these records, that if one was to find a record from which the climatic context is unknown, one could determine the type of cli-

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mate (extreme or mild) at deposition time by looking at the location of transition points both between the laminae and within the isotopic curve. Maybe something that could help future studies. . . I found the one sentence proposed in the paper: “The differences between the French and Greek high resolution records are explained because Mediterranean climatic zones (Greece) experience “extreme” seasonality in terms of winter recharge and summer aridity, which are not feature of more oceanic (NW France) climatic zones” rather short as an explanation, and I would like the authors to consider the following point to widen their discussion: if climatic conditions in NW France are rather milder than those of Greece, what could have created the hiatuses in the French record?

I hope these comments will help the authors with their manuscript, and I wish them full success in their studies.

Interactive comment on *Clim. Past Discuss.*, 9, 1657, 2013.

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