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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 - General comment

I have read the manuscript "Stable isotope record of Eemian seasonal temperature from MIS5e tufa stromatolite; Somme Basin, Northern France" submitted by Dabkowski et al. I also read the comment posted by Referee 1 as well as the reply of the authors to this comment.

Here are my comments on the paper and the subsequent discussion. This study has a great potential to interest scientists who work with this type of deposits to try and reconstruct past climate conditions. Its forte is in the comparison between petrographical and geochemical data on a very high-resolution basis. Such studies should indeed be

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encouraged.

However, and beside these positive aspects, my general feeling is that the authors did not treat the interpretation of their data as carefully as they should. They have a tendency, for example, to not really explain apparent contradictions in their record (facies I-III and varying isotopic signal) or between their record and the Greek record they used as a comparison. There is also a lot of contradictions or unclear points within the text (and the subsequent comment), especially regarding the source of the parent waters to the studied stromatolite. The interpretation of the authors is based on a series of assumptions that, even if they could indeed be true, are still not proved beyond doubt and are not sufficiently discussed. The study lacks an evaluation of the modern system, which would indeed help clarifying the interpretation of the authors. The context of deposition is not sufficiently described. Consequently, my advice is to reject the paper as submitted. If the authors can address the points raised by both the first referee and myself, and reformulate their hypothesis in a clearer manner, I think that there may be possibilities to resubmit this manuscript in a new (and expanded) form.

Here I provide the points that I would like to see further discussed or explicated (specific comments). Further down the authors will find a few additional comments about their manuscript.

II - Specific comments

1) Dating constrain, general introduction to the study site, climatic and hydrographical context (introduction and discussion)

There is not enough detail about how the age of these deposits was ascertained. A table or figure showing these should be added to the manuscript, or a more precise description added to the text. The Eemian climate should at least be described in broad terms, especially the climatic optimum – also the stratigraphical position of the analysed section vis-à-vis the base of the sequence (1659 – 5) which supposedly shows

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this climatic optimum, should be indicated, as well as the amount of time covered by the whole tufa sequence, if known. The relation of the studied stromatolites to the river and local springs should be better discussed. This is extremely important for the interpretation of the isotopic data. It is unclear to me whereas the stromatolite could have originated from spring waters (like the tufa deposits to which it is associated) or from riverine water. This point should be discussed carefully since at present I cannot find any clear reason to associate these deposits to riverine waters, and therefore a comparison to the modern river seasonal temperature variation is pointless. Generally, the whole sedimentological / depositional context should be better described, including points raised in the authors' response to the comments made by the first referee.

2) Laminae

Laminae formation

Boundaries between laminations (described as "sharp": 1661-7) are actually not clear between facies I and IV according to Fig. 3.

I don't quite follow how the authors can compare one taxon (B. arvenensis) with two modern taxa working in biocenose (Phormidium and Schizothrix), which precipitate alternating laminae in autumn and spring, and then infer that B. arvenensis would create different laminae that would relate to winter and summer depositions. To take it a little bit further and for the sake of discussion: if no other modern analogues for stromatolites are available for comparison, and with no independent age control, isn't it just possible that the laminae reflect longer time-scales, with "good years" and "bad years" of deposition reflected into the different laminae, rather than a seasonal signal? Or "good" and "bad" decades? The authors justify in the next paragraph the choice of summer and winter seasons for the deposition of their laminae by comparison to tufa deposits, sparitic microfacies I corresponding to summer (good growth conditions) and "other microfacies" representing limited growth under less favourable conditions ("winter"). Could the authors clarify the time of deposition of facies III, which features

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"sparitic crystals including filaments and fascicules, similar to those in Microfacies I" (1660-25-26) and why it would be different from the time of deposition of facies I?

What is the difference between microfacies I in blue and microfacies I in red in Fig. 3? How can the authors explain the isotopic difference between these two "types" of facies I in light of their interpretation?

Hiatuses

(1662-6-7): the sharp transition between laminae is interpreted to possibly reflect short hiatuses: if a hiatus is present, there is no real way of telling if it was short or long. This is also contradictory to the hypothesis of nearly continuous and fast calcite precipitation, and to sentences like "constant presence of water flowing along the slopes from springs" (authors' reply to referee I). So what conditions created the hiatuses?

To widen the discussion: cannot phases of non-deposition result in the dissolution of previous layers rather than in the deposition of detrital material? It is stated in the discussion (shouldn't that be in the introduction?) that the tufa formation of Caours generally contains little detrital contents. Isn't it just due to local settings that the stromatolite lacks a detrital component?

What created the voids in facies III?

3) Stable isotopes

The interpretation of the authors of their isotopic record, although possible, is based on several assumptions that cannot be proved beyond doubt. This should be made clearer. The paper would really beneficiate from envisaging all possibilities to explain stable isotope variations further, rather than narrowing possibilities down to one so early in the discussion. ∂18O data of regional precipitation such as presented in the authors' response to the comment by Referee I should be included in the manuscript and fed into the discussion. ∂18O data and intra-annual temperature variations from local springs are crucially missing, as well as ∂18O data from the river and modern

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carbonates. In the absence of these, the authors should be careful to keep all options opened for their interpretation until they have discussed all of them in detail. A discussion such as included in the authors' response should be added to the text, presenting more clearly the reasons for their choice of interpretation.

Petrographical and stable isotopic data are only coherent IF: for the laminae the signal is seasonal and microfacies I only is associated to summer conditions and, for the isotopes, the signal reflects mainly water temperature fluctuations. All these can be argued to be reasonable assumptions, but remain assumptions. The word "demonstrate" should therefore be avoided.

I can only deplore, together with Referee 1, the lack of more precise data about the modern system, particularly seasonal isotopic and temperature variations for both the Scardon river and local springs. The authors compare the range of palaeotemperatures obtained from their calculations to the seasonal temperature variations of the modern Scardon river. However, it seems very likely that the studied stromatolite originated from spring or mixed water. Spring temperatures can be quite stable (or at least more stable than those of a river) throughout the year, especially if they relate to deep aquifers. Is it likely that spring temperature will change to such an extent during the year as to create the signal observed? And if the signal is correctly interpreted in terms of temperature variations, does that make sense to compare it to a river signal, which is more likely to be exaggerated by comparison to springs? In this case the 3 degrees difference is irrelevant.

This point is hinted at in the text, where the authors state that "water precipitating this tufa may thus have been a mixture of palaeo-Scardon waters and spring waters with a nearly constant temperature". The point is further stressed by the authors in the discussion following Referee 1's comment, where they state that: "Such configuration characterises spring tufa formations" and later "The facies associations thus demonstrate a constant presence of water flowing along the slope from springs at the time of the studied stromatolite precipitation". In this case I found that the authors conclusion

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that a clear RIVERINE palaeotemperature signal is recorded by this stromatolite rather baffling.

Also I don't quite follow the point about discussing significant water mixing in aquifers (to exclude the water composition as a parameter of influence to the isotopic composition of carbonates) if the stromatolite was deposited from riverine water. I join Referee 1 in the opinion that for a river, not only aquifer discharge from springs but also runoff could be a major component in the source of the waters, therefore impacting their isotopic composition alongside temperature variations.

Problems about the isotopic composition interpretations therefore include the following points that should really be discussed in detail: - if the stromatolite precipitated from spring waters, and the springs originate from deep aquifers with good water mixing, how good are the chances that water temperature will strongly vary throughout the year? And how relevant is a comparison with modern riverine seasonal temperature variations? - if the stromatolite precipitated from spring waters with short residence times in the aquifer, then seasonal variations in the water composition may become important - if the stromatolite precipitated from riverine waters, as also mentioned by Referee 1, seasonal variations in the water composition may well be non-negligible. This can only be ruled out by an isotopic study of the modern system, to really constrain major parameters that would influence the isotopic composition of the parent waters.

It doesn't make any sense that hiatuses are associated to "intermediate conditions". What would these be, that would create hiatuses in the deposition? I find the Greek record and interpretation of Brasier et al. 2010 rather contradictory to the present study, and the reason why this could be should be explicated further.

There are bits and pieces of information known about the Eemian climate seasonality. The authors should check the work by Kühl and collaborators (Kühl et al. 2007; Kühl and Litt 2007).

Wider discussion and conclusions:

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Following the points mentioned above, there is no clear reason to assume this stromatolite will provide accurate palaeo RIVERINE temperature data. There is no real explanation provided for the difference between the Greek and French records. How can hiatuses be created in intermediate conditions? What is going on with facies I and its varying isotopic composition? It should be made clearer that the authors' conclusions are based on several assumptions, including that: - the laminae are seasonal - the considered seasons are "winter" and summer - the source of water is assumed (and the authors should make it clearer which water they think created their stromatolite and why; at the moment it is far from clear) to be the river - the river is essentially fed by aquifers with a long-term recharge (a point not clarified yet by modern isotopic studies of the river or an in-depth study of the surrounding areas)

References:

I was a bit surprised by the small amount of references cited considering the complexity of the topic broached by the authors.

III - Additional comments

1659-13: please clarify if the reference cited (Brasier et al. 2010) represents another investigation of this type of deposits or of similar deposits – the sentence is unclear to me. 1659-23: can or did? This sentence should be rephrased. 1660-11: "No detrital material was seen in thin section" This sentence should be rephrased. 1660-22: "where present and better preserved in Microfacies I": unclear, should be rephrased – contradictory with first part of the sentence: "are not clearly visible". 1661-1-3 contradictory: facies lacking internal lamination, but containing thin sparitic layers? 1662-5-7: needs rephrasing. 1662-16: "this morphotaxa is thought to represent": unclear, needs rephrasing. 1662-24:"the clear preservation" contradictory to 1660-20 "are not clearly visible". 1666-2-3: please rephrase. 1666-13-15: please rephrase. Fig. 3: what is the signification of the arrows? What is the difference (petrographically) between facies I in blue (bottom of the sequence) and the other facies I in red?

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References included in this review:

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Norbert Kühl, Thomas Litt, 16. Quantitative time-series reconstructions of holsteinian and Eemian temperatures using botanical data, In: Frank Sirocko, Martin Claussen, María Fernanda Sánchez Goñi and Thomas Litt, Editor(s), Developments in Quaternary Sciences, Elsevier, 2007, Volume 7, Pages 239-254

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