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Climate of the Past Discussions

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

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The authors are grateful to the second referee for his review. Since there are only a few days left before the open-discussion ends on June 14th, we won't propose here a detailed reply to this review but it is helpful that some general aspects should be discussed.

After reading both referees' comments, we actually think that the general purpose of our paper has been misunderstood. High-resolution investigation of tufa stromatolites is a very recent, poorly developed topic, which explains the small number of references on this specific topic cited in our paper, as noticed by the second referee. This stroma-

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tolite and the laminated Greek tufa from Brasier et al. (2010) are the first records of this kind for the European Pleistocene (for a review, see Andrews and Brasier, 2005). We thus want to show their high potential to reconstruct seasonality and to encourage their investigation and development of the method. To make this clearer, we could change the title of our paper to "Stable isotopes from tufa stromatolite (Somme Basin, Northern France; MIS 5e): exploring the potential record of Eemian seasonal temperature".

We have not suggested that the specified temperature range from the Caours stromatolite isotopic record was the ACTUAL water temperature seasonal range during the Eemian optimum. We carefully wrote that "we are unable at present to prove or disprove this possibility without further investigation" (Discussion Paper-DP, p. 1665, l. 24-25) and concluded that studying a larger stromatolite (i.e. with more lamina) will "allow covering more signiïňĄcant periods of time of a few decades" (DP, p. 1667, l. 8-9): the present record represents a few years and is obviously too short to be representative of the 15 ka long Eemian. As the aim of our paper is not to discuss Eemian seasonality, there is no need to discuss extensively how the Caours tufa has been dated; in any case the detailed data has already been published by Antoine et al. (2006), as mentioned in our paper.

To answer more specifically some of the second referee's more substantive remarks: First, there is NO blue microfacies I. In Fig. 3, numbers "I" in the lower blue Microfacies IV label the (red) arrows as shown in Fig. 2 and explained in the text; it seems that our figure as well as its caption should be modified to make this clearer. Consequently, transitions between lamina ARE sharp (see pictures on Fig. 2) and there is NO contradiction between petrography and the isotopic record (RC, p. C1037 and C.C1038-1039).

Secondly, it appears that the concept of "morphotaxon" needs to be clarified. A MORPHOtaxon has no biological significance. It refers to facies of biogenic calcite and is defined on micromorphological/petrographical criteria. Those criteria are the only available to describe fossil biofacies in tufa as organic matter, or DNA analysed by biologists

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and identified with algal/bacterial communities, has decayed. Pierre Freytet has listed the biofacies he observed in numerous fossil and modern tufas and then defined most of the morphotaxa we now know (as Broutinella observed in Caours; Freytet, 1997, 1998). When he had sufficient data from both petrographical and biological analyses, he suggested association between morphotaxa and modern biological species or genera. Our interpretation of the lamina succession in Caours comparing morphogenera Broutinella to the Phormidium/Schizothrix biocenose is mainly based on this work and strengthened by the other cited publications (Freytet, 1992; Freytet and Plet, 1996; Janssen et al., 1999; etc.).

We don't suggest that the Caours tufa precipitated exclusively from Scardon river water by calling it "RIVERINE tufa". The word "riverine" refers to anything pertaining to rivers as opposed to lakes. In the context of freshwater calcareous deposits, it typically defines deposits formed at ambient temperature as opposed to thermal deposits usually called "travertines", regardless of their position along the river profile (from spring to main stream). The word riverine has been used in tufa studies for well over 20 years and has not so far caused confusion of this sort. We have actually demonstrated in our paper and replied to the first referee that the Caours tufa shows morphologies and facies specific to spring deposits, which is consistent with our interpretation of the isotopic signal. We will ensure the revised paper is very clear on this issue.

Finally, the referee finds "the Greek record and interpretation of Brasier et al. 2010 rather contradictory to the present study" and reasonably asks that we explain further the reasons for such a contradiction. We would be happy to if the referee can develop his point of view on this. We think that the Greek and French records are rather equivalent even if different in the shape of the isotopic curves. We have actually discussed these differences in our paper and assigned them to the distinct seasonal patterns of Oceanic vs. Mediterranean climate, so we are not clear what we are asked to say further on this topic.

We do appreciate the time taken to make these comments and we will try to ensure

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that we achieve more clarity as we revise our manuscript.

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