

Interactive comment on “Millennial-scale precipitation variability over Easter Island (South Pacific) during MIS 3: inter-hemispheric teleconnections with North Atlantic abrupt cold events” by O. Margalef et al.

Anonymous Referee #1

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The article by Margalef et al. deals with a MIS3 record of precipitation in Easter Island, and connects periods of rainfall increases to Heinrich events (or D/O stadials). Easter Island is among the very few islands lost in the SE Pacific area, so having a precipitation record from there is worth a try.

I however have concerns about (1) the relevance of the paper and (2) the quality of the reconstructions used, which I try to detail below.

(1) There are no new data as compared to what has been published (sometimes twice) in Margalef et al., 2013 (Global and Planetary Change) and Margalef et al., 2014 C732

(Palaeo-3). The authors use the same statistical treatment and focus only on MIS3, but the statistical treatment is basically the same than in the previous publications. The main conclusions are unchanged (probably wetter conditions during Heinrich events).

The question of whether a re-use of previously published data without any re-interpretation is appropriate to the journal has to be raised. I think the question is a fair one, as I feel the manuscript does not provide a worthwhile contribution of new information, and the decision as to whether such kind of study is within the editorial guidelines should be done by the editor.

(2) There is some attempt to link what is interpreted in terms of "rainfall changes" to changes in the E-W equatorial Pacific SST gradient. The assignment of the PC2 (the "rainfall signal") rapid variability to periods concomitant with Heinrich events is however based on a very poorly constrained chronology. The same is true for the two other marine records (Dannenmann and Pena age models have no age control point over the time interval studied). The assignment of the "wet periods" is hence more than acrobatic, as there is simply no chance to have a chronology accurate and precise enough to assign the black and green lines of figure 5, which is key to the discussion, to Heinrich events and/or antarctic warm events.

Also, the age model has been changed a little since the 2013 and 2014 publications, but not significantly improved, and considering all radiocarbon dates below 800 cm suggests the sedimentation rate is indeed infinite during MIS3. On 20 dates I can see on Figure 3, 11 were used and 9 were rejected without any justification. Why? Such cherry-picking must be clarified.

Based on point (1) I find hard to submit a clear recommendation for publication to the editor. But if the editor is opting for giving the authors a chance to re-submit a revised version of the article, there is a real need to refocus the article, as the interpretations/conclusions are not supported by the data presented - rather, it is based on the authors' thoughts about how rainfall may have affected their site given that it is

highly expected, thanks to an abundant literature, that rainfall in southern tropics may increase during Heinrich events. As it stands, the interpretation is mere speculation because of the uncertainties associated with age models of the data presented in Figure 5. Figure 6 is an attempt to have a broad, synthetic view of all the processes at play during MIS3 in the southern hemisphere and the tropics. Figure 1 is informative, but its review character clearly means the authors have interpreted their results a priori. Off-topic discussion concerning AAIW formation, Nd isotopes, and CO₂ in the last paragraph suggests me that, instead of working on their own results, the authors just pile up concepts and published datasets one on top of another to get one more publication using the same dataset. I recommend the authors to pause and go back to basics prior to extracting new valuable information from their site which is, I recognize, extremely interesting for the broad paleoclimatic community.

Interactive comment on Clim. Past Discuss., 11, 1407, 2015.