For clarification the referee's text is written in **bold** and our replies in plain text:

Interactive comment on
"Millennial-scale precipitation variability over Easter Island (South Pacific) during MIS 3: inter-hemispheric teleconnections with North Atlantic abrupt coldevents"

O. Margalef et al.

Anonymous Referee #1
Received and published: 17 June 2015

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 (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.

The reviewer is right pointing out that an environment reconstruction and the geochemical data is already provided in Margalef et al. (2013) and (2014), as it was clearly exposed in the manuscript. However, we have summarized this part (facies and geochemical description) to make more comprehensive the reviewed manuscript. Our purpose with the submitted work is to contextualize our previous findings -that only described the main processes occurring in the island itself- with the regional pattern of Southern Central Pacific. This area, as explained in the text, can be described as a "void region" in paleoclimatic studies, since the low sedimentation rates of the ocean abyssal plains do not provide detailed reconstructions. From our point of view, a detailed contextualization and comparison with terrestrial and marine regional records is a relevant contribution to the paleoclimate field.

For the first time a Late Pleistocene record from Central Pacific is compared with precipitation patterns on Southern Hemisphere tropics and the changes in atmospheric and oceanic circulation. And for very first time the migration of SPCZ is proposed as important factor for hydrological changes during MIS3. As the reviewer #2 states: "The topic of linkages between paleoecological events the subtropical Southern Ocean and the rest of the planet is of great importance for our understanding how the ocean-climate system functions both today and in the past". We therefore defend that this is a topic of high interest for this journal.

(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).

MIS 3 is a very difficult period to study with accurate chronology because it lies beyond the radiocarbon limit, and most of the age-model construction became a big challenge. In our case, other alternative methods have been impossible to apply. There are no tephra layers to date, nor endogenic carbonates that might allow them to be dated by U-series using laser-ablation multi-collector ICP-MS and there is no choice to use OSL techniques on sand grains because of the strong matrix effect produced by peat, rich in organic matter and water. Neither tuning methods usually performed on marine records. So we have to

handle with these limitations. Despite these difficulties and considering always the existence of uncertainty, we still believe that our age-model set solid bases to allow the developed regional discussion since:

(1) The age model from the well constrained part of the MIS 3 (38.5 - 55 cal kyr BP, 4.31–8.72 m) depicts constant accumulation rates. The peat facies and depositional system inferred in the constrained part is the same as the bottommost part of the record and no evidences of drought episodes or discontinuities are revealed by any geochemical, microscopic or macroscopic observations (as is Facies D). Facies homogeneity suggests that the same accumulation and geochemical processes were in place during the whole MIS 3 in Rano Aroi. Consequently, the age-depth model can be linearly expanded through time as performed in several publications that face similar difficulties (Streett-Perrott et al., 1997, Russell et al., 2014)

(2) The three first wet events Ar1, Ar2 and Ar3, are located within the well-constrained part of the chronological model and clearly correlate with Heinrich stadials or other stadials (fact that has a logic climate mechanistic explanation). When using our age model, Rano Aroi wet events Ar4, Ar5 and Ar 6 also consistently correlate with North Atlantic cold stadials. The excellent time fitting between stadials and the well constrained Rano Aroi events is a strong argument to postulate a correlation of the older Rano Aroi wet events (beyond radiocarbon limit) with stadials during MIS3. This method is valid as correlation as commonly performed in other terrestrial or marine studies (Gasse and Van campo, 2001; Kaiser et al., 2007; Lamy et al., 2000).

Extended explanations about age model have been included as additional information in the reviewed version of the manuscript.

Marine chronologies are based in previous publications in well recognized scientific journals. They obviously have the intrinsic uncertainties derived from any C^{14} based chronology in MIS 3. Nevertheless the even correlation is centered into major structures that last several millennial. Chronologies would need of a large displacement (several millennia) to actually change the SST gradient itself and the correlation with the Rano Aroi record. In addition, independently of the chronology, the number and stratigraphy of events would not change. The main outcome of the manuscript, as discussed in it, is that even with the imperfect but always independent chronologies, a series of events can be correlated from the equator to the subtropical region, whose connection can be well argued by known physical processes and further supported by other proxy evidences and model outcomes. Other chronologies, although less robust, could be feasible for each of the three considered paleoclimatic records in the exercise, but if they would actually change the current phase of the events, their explanation would not be feasible by the known physical connections. This study does not provide the final answer or proof to the discussed inter-latitudinal processes but we consider that it yields an additional argument to support them. It has a particular relevant value due to the source region with the inherent difficulty to provide more accurate paleoclimatic records.

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.

We would like to point out that for the entire record the Rano Aroi events are nicely correlated with the ice record curves which it is, as the reviewer points out, key to the discussion (especially for Ar2, Ar4 and Ar6, correlated with Heinrich events, see Figure 2). As indicated before, three out of the six wet periods are located in the well-constrained part of the chronological model (<54 kyr cal BP).

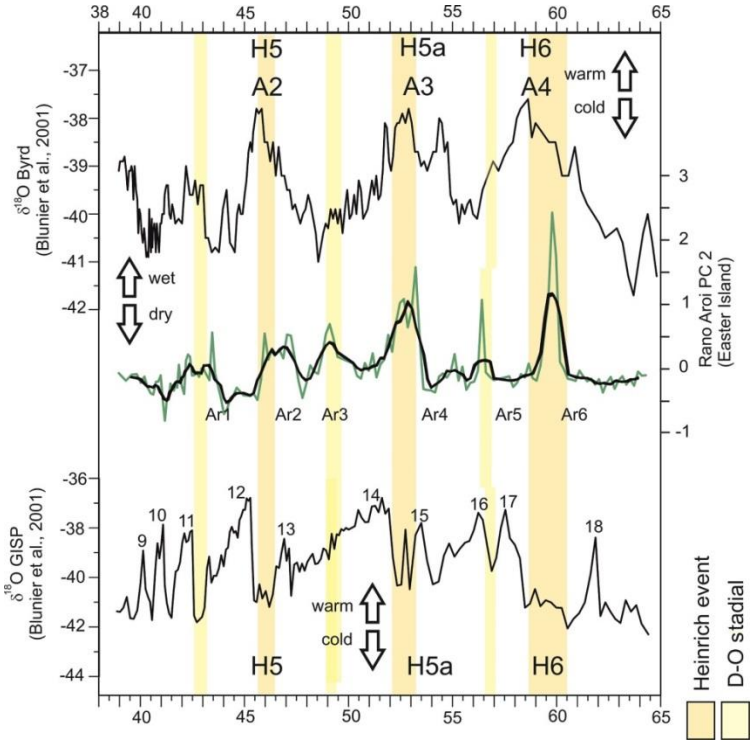

Figure 2. Rano Aroi humidity index (this study, PC2) relation with $\delta^{18}O$ curves of Antarctic and Greenland ice records (Blunier et al., 2001).

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.

We think we already answered with enough detail the first part of the question little above.

The bottommost part of the record is made of old peat (and therefore old carbon) beyond the radiocarbon detection limit (>54000 kyr BP). When this occurs radiocarbon dating commonly provides ages between 50 and 60 kyr BP with large errors that make no stratigraphical sense. These dating points were rejected. We have excluded these points from the age model figure (on the reviewed manuscript version) to avoid misunderstandings. All radiocarbon dating can be found at Table 1 and on Margalef et al., (2013, 2014).

On 20 dates I can see on Figure 2, 11 were used and 9 were rejected without any justification. Why? Such cherry-picking must be clarified.

We provided detailed explanations in Margalef et al. (2013) and (2014) for the main reasons for rejecting ages. There were ages rejected as too old because they lie beyond the radiocarbon method limit (see previous answer) whereas there were other ages rejected as too young (see the detailed explanations for the reviewer #2). All ages can be checked on Margalef et al., (2013, 2014) and on the following Table 1.

Sample Name	Depth (cm)	Dates	Calibrated ages (yr. BP)
ARO 01-01 03	238	*2580±30**	*2730±30 **
ARO 01-01 20	255	9460±50	10690 ± 120 BP
ARO 01-01 50	285	12150±60	13995±153
ARO 01-01 70	305	12880±70	14505±483
ARO 01-01 92	327	13800±60	16910 ± 180 BP
ARO 01-01 103	338	*7440±50**	*8270±80**
ARO 01-01 143	378	26960 ±150	31742±80
ARO 01-02 23	421	*12070±60**	*13923±138**
ARO 01-02 31.5	431.5	34000±500	39431±1051
ARO 01-02 53	453	35300±600	40190±970
ARO 01-03 57	552	37600±600	42210±520
ARO 01-04 62	662	*33900±500*	*39310±2570**
ARO 01-05 67	760	45000±2000	48710±2570
ARO 01-06 72	872	49000±3000	54600±5000
ARO 01-07 83	979	52000±4000	Too old ages
ARO 01-08 83	1083	>50000	Too old ages
ARO 01-09 88	1181	53000± 4000	Too old ages
ARO 01-10 88	1288	>49000	Too old ages
ARO 01-11 88	1380	49000±3000	Too old ages

Table 1. Radiocarbon ages of Rano Aroi record (ARO 06 01). Rejected ages are marked in red. Detailed explanations can be found in the text.

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 inter- pretations/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.

We are showing geochemical, biological and stratigraphic evidences of wet events on the island in both, the well constrained part of the record and also beyond the radiocarbon limit. We are arguing facts, fiscussing them considering the age and interpretation uncertainity and using them to formulate hypotheses to explain the possible teleconnections of Easter Island past climate. We would like to point out again that the main goal of this work is to contextualize in a regional Southern Pacific framework the hydrological patterns observed in Easter Island during the MIS 3. Nevertheless, if the reviewer believes that we have missed key references that must be cited and discussed in the framework of this study, we will be glad to incorporate them to the general discussion.

As it stands, the interpretation is mere speculation because of the uncertainties associated with age models of the data presented in Figure 5.

We think we have clarified this matter in former questions. We think we have clearly separated in the text what is based on a robust chronological framework, what are hypotheses and what are more open discussions of the data trying to find the most likely explaining framework.

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.

We emphasize that we consider synthetic figures important contributions to understand the regional climatic patterns.

Figure 1 is informative, but its review character clearly means the authors have interpreted their results a priori.

As the reviewer points out the figure 1 was initially allocated the last one, to be the conclusive one. But as we wanted to situate the records used in the manuscript in their geographic situation –e.g. in the study site- it becomes the first to be called. We have solve this including the geographic situation of the most mentioned records on a table (Table 1 of the revised Manuscript) and putting the review figure (Formerly Figure 1) as the last one (Figure 6) to avoid confusion.

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.

Indeed we use all published datasets because the main objective of this publication was to put our research into a general framework. The role of high latitude dynamics over the tropical paleoclimate and the inverse effect has been a matter of intense debate and increasing interest (Skinner et al., 2010). Inter-hemispheric and latitudinal tele-connections are new challenging frontiers for paleoclimatology. In this manuscript our aim is to compare our subtropical record and what can be extracted from it (El Niño-like pattern during Heinrich events, the important role of the SPCZ etc…) with changes described over high latitudes, that contribute with positive feedbacks to control the occurrence of these events.

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.

The submitted manuscript is the result of long and paused thoughts in order to extract the best possible paleoclimate information of a complex record with a difficult age model. Nevertheless, we are aware there still is room for improving our work, and we want to thank the reviewer for the useful insights that have allowed us to improve it. The main features (age model, geochemical information) of this site make the study of this record a great challenge. However, we must remind the reviewers that this paleoclimate record lies in the middle of a large 'paleoclimate void' and, hence, it is worth its detailed climate characterization and correlation with other records. We believe that with the included changes this work will be of interest for the paleoclimatic community.

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