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Climate of the Past Discussions

Interactive comment on "Mediterranean Outflow Water variability during the Early Pleistocene climate transition" *by* Stefanie Kaboth et al.

Anonymous Referee #1

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Dear Climate of the Past Editorial Board

I hereby you receive my report on the MS " Mediterranean Outflow Water variability during the Early Pleistocene climate transition" by Kaboth et al.

The authors provided new important information on Mediterranean Outflow Water (MOW) during the Early Pleistocene comparing benthic stable isotopic data from Mediterranean and Atlantic Ocean records. In particular, the authors describe the change of MOW in two time intervals, between 2.6 and 2.4 and between 2.1 and 1.8 Ma. Notwithstanding the different time resolution of the two compared study records, the comparison of oxygen isotope signals is clear and suggest new hypothesis concerning the oscillation of Mediterranean Outflow Water in the Gulf of Cadiz and possible modulation of the North Atlantic Meridional Overturning Circulation.

The manuscript is properly constructed and it is evident that the data support the interpretation proposed in the manuscript. Specific comment: Chapter Age model & Sedimentation rates It is clear evident in Fig 3 that the U1389 marine record shows a variable Sed Rate, so that it is not possible to use the term mean Sed Rate. It has no sense. Maybe it is important to associate this change in Sed Rate to particular events.

Chapter Spectral analyses I am convince that the age model is correct, but I am not convince that is necessary the spectral analysis in the paper. In addition, the possible information concerning the astronomical periodicities are marginally used in the interpretation and discussion of data. If the authors want to really used the spectral analysis I think that it is necessary to go in detail with wavelet analysis and detail comparison with astronomical insolation curve. This further analysis will improve the manuscript.

Chapter Glacial-Interglacial MOW variability at Site U1389 during the Early Pleistocene The discussion concerning glacial and interglacial period seems to be robust. I am not convince about the MIS102. The comparison between oxygen isotopes is not as clear as the other intervals. In my opinion, the authors need to support and render more clear this correlation and explain the difference. My overall comment concerning this chapter is that the authors have to describe in detail the comparison between the two study records. The authors use also the grain-size oscillation to support the different power of MOW in two study intervals. In my opinion, this proxy is weak and the % differences suggested by the authors are very weak. The authors suggest the d13C gradient between the Mediterranean and Atlantic site as proxy for different contribution to North Atlantic water masses. I think that the authors need to explain this correlation and interpretation.

Chapter Precession control on MOW strength during the Early Pleistocene: Similarities to Late Pleistocene MOW behaviour? I think that it is necessary to add in Figure 4 the astronomical parameter of precession and I think that the authors need to run wavelet analysis on proxy record of grain-size to understand the stability of precession frequency band (23 ky). Also in this chapter, the authors use the grain-size suggesting

an increase during the sapropel deposition in the Eastern Mediterranean. This datum is weak and it is evident only in two time intervals.

Chapter Did MOW contribute to the Early Pleistocene climate transition? In this chapter, the authors introduce the Neogloboquadrina atlantica left coiled and its value to monitor the southern delineation of transitional and subpolar water masses during glacial period. However, where is the distribution of this species? I think that the authors need to plot the abundance pattern of Neogloboquadrina atlantica left coiled for Mediterranean and Atlantic records. In addition, it is important to document the correlation between this species and the precession curve. The authors suggest a correlation between SST of North Atlantic South Atlantic. Probably it could be useful to propose a figure with a comparison of the SST of north and south Atlantic.

Figure: Fig. 1 – Due the issue of the manuscript I think that it is important to add a figure of the Mediterranean water masses circulation. Fig. 3 – In the upper part of this figure the authors show a correlation between U1389 site and Mediterranean record. However, for the Interval I, it is not clear the position of the third correlation lines for MIS 101. I think that is only a graphic mistake. Please try to check. Fig. 4 – It is necessary to add the precession curve, to add the arrows of the sapropels also in the part B of the figure. The ages in the manuscript are reported in Ma while in this figure in ka. Please try to use the same scale. Concerning the grain-size curve, I do not think that is correct to plot a continuous curve, mainly for the Interval I, where the sample are far from each other, (between 2.4 and 2.5 Ma). Try to use another graphic representation. Fig. 5 – Please add the red noise level in the spectra.

Table: In table 1 it is necessary to write the name of the calcareous plankton species (i.e., Globorotalia puncticulata). Be careful, Raffi et al 2006 is not reported in the reference In table 2, Raffi et al 2006 is not reported in the reference

The manuscript results properly constructed and all figures are representative and useful for this version of the manuscript.

I think that it is very important to publish these data, because of in the scientific literature there are very few papers suggesting a correlation between Atlantic and Mediterranean during the Early Pleistocene. In addition, this correlation suggests new important evidences for the contribution of MOW to Atlantic circulation. My overall conclusion is that the paper is suitable for the journal but unfortunately it needs still moderate to minor revision concerning the presentation of data and power spectra analysis. In addition, I think that a detailed correlation with oscillation of astronomical parameter is necessary. In several parts of the manuscript, the authors used the grain-size proxy, but the differences reported by the authors are too weak to support the differences of MOW in the two study time interval.

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