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

Anonymous Referee #2

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The manuscript " Mediterranean Outflow Water variability during the Early Pleistocene climate transition" by Kaboth and others is a nice contribution to already existing paleoceanographic reconstructions focusing on the MOW. In particular, the authors apply stable isotopes on benthic foraminifera and grain size analyses to determine MOW variability and its impact on the Atlantic Meridional Overturning Circulation (AMOC) during the early Pleistocene. It is nicely written, easy understandable and fits into the scope of Climate of the Past. However, as I have a few comments this MS should be considered for publications after moderate revisions. Please find below my comments:

Major comments:

1) The question how the MOW influenced AMOC is still not understood and needs further investigation and consideration. The present dataset may shed some light into

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this debate. However, I would suggest that the authors not only compare their data to the SST reconstruction by Lawrence et al. but also to other records (e.g. Naafs et al. 2010, Khélifi & Frank 2014, Lisiecki 2014)? I think to make such a statement the authors should compare more thoroughly their records to other published records.

2) The authors use two different foraminifera Planulina ariminensis and Cibicides ungerianus, which are considered to be both epibenthic species and thus record ambient seawater characteristics. I wonder why the carbon isotopic composition of P. ariminensis shows a smaller spread than C. ungerianus? There are studies that show similar values for P.ariminensis and the Cibicides group. Maybe the authors could highlight the samples of cold and warm periods differently in the x/y plot? Is that a respiration change of the foraminifera? Is there a change in $\Delta\delta$13C gradients between these two species? To solve this problem is of great importance for subsequent studies using these species, especially in the Gulf of Cadiz. Furthermore, the authors decided to restrict their δ13C record to P. ariminensis. They mention a paper in prep by Kaboth including a interspecies-correction and an unpublished dataset of Lourens et al. and refer to this as an end-member for their recorded signal. However, this record is based on C. ungerianus δ13C values. How does that work? Can the authors please clarify?

3) This study shows that MOW variability was probably paced by precession and semi-precession cyclicity during the early Pleistocene. In fact this was already shown for records of the eastern Mediterranean Sea (ODP site 967 and 969) and have been linked to North Atlantic records during a similar time period (MIS 100, Becker et al.). I think the authors should include this in their discussion.

4) Vertical movements of the different branches of MOW have been discussed in detail in many studies for the late Pleistocene (e.g Schönfeld et al.; Zahn et al.). To my opinion the authors should add more discussion about vertical movements of the MOW in the Gulf of Cadiz. Is it really possible to disentangle these different effects with δ18O gradients? It is not really clear how you can make this statement from δ18O values? Do you infer density from that according to Lynch-Stieglitz et al. 1999? This section

needs some more explanations.

Minor comments in the text:

Title: I am not aware of any Early Pleistocene climate transition. It is either the Mid-Pleistocene-Transition (1.25 to 0.7 Ma, Elderfield et al. 2012) or the Plio/Pleistocene transition (boundary). Major oceanographic changes during the early Pleistocene occur around 1.7 Ma (e.g Khélifi & Frank 2014, Hodell & Venz-Curtis 2006; Lisiecki 2014, Martinez Garcia et al., 2010, Ravelo et al., 2004). Therefore, I think the authors should consider to change the title.

Line 51: Can you please give more information about the reference site Singa/Vrica?

Line 112-118: How do you explain the large differences in sedimentation rates varying from 0.1m/kyr to 1.2m/kyr?

Line 216: You discuss the abundance of the extinct planktonic foraminifera N.atlantica (polar species) during cold periods? So I guess the black stars in Interval I (Figure 4) are the occurrences of. N. atlantica?

Line 263: The hiatus extends from 2.4 to 2.1 Ma, so how can you state that AMOC increased at 2.4 Ma from you data?

Please check throughout the text and figure captions for the superscription in δ13C and δ18O.

Figures: Figure 1: A detailed map of the study location is always necessary, however, in this case an additional map showing the exact location of MOW in the water column would be very helpful to fully understand the MOW flow in the Gulf of Cadiz.

Figure 2: This figure indicates the (non)correlation of the oxygen and carbon isotopic compositions of the two different analysed benthic foraminifera. However, this figure needs some improvement. As mentioned above the different stratigraphic/climatic intervals should be highlighted. Furthermore, it is not clear which figure (a or b) shows

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carbon or oxygen. The reader can only guess from the data.

Figure 3: Replace commas with dots

Figure 4: This figure is confusing as it intuitively suggests that the Lawrence record covers the similar interval investigated in this study. Please change accordingly and add also other records.

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