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Climate of the Past Discussions

Interactive comment on "Hydrological variations of the intermediate water masses of the western Mediterranean Sea during the past 20 ka inferred from neodymium isotopic composition in foraminifera and cold-water corals" *by* Quentin Dubois-Dauphin et al.

Anonymous Referee #2

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Dubois-Dauphin et al. present new and high quality neodymium isotopic data from planktic foraminifera and scleractinian cold-water corals from three locations in the western Mediterranean. They use these data to characterise the hydrographic (not hydrological!) variability at intermediate depths in the western Mediterranean and to constrain the variability of the intermediate circulation (a key component of the Mediterranean thermohaline circulation!) in the basin through the last 20 kyr. This timespan is a valuable one, in that over the last 20 kyr the Northern Hemisphere underwent a

series of abrupt climate swings superimposed upon the transition (glacial termination I) from the Last Glacial Maximum (LGM) to the Holocene interglacial. In addition, the Mediterranean Sea featured the deposition of sapropel S1 (roughly from 10 to 6.5 ka BP) in the eastern basin and the Organic Rich Layer 1 (roughly from 14.5 to 8.5 ka BP) in the western basin and these changes appear to reflect variations in the Mediterranean circulation/ventilation (to which intermediate water circulation is central). Hence these climatic and oceanographic developments provide a rich source of information on the pattern(s) and drivers of thermohaline circulation changes in the basin. So all the ingredients are there to make the study by Dubois-Dauphin et al. a key contribution to the palaeoceanography of the Mediterranean Sea.

In summary, I think that the manuscript is certainly suited for publication in Climate of the Past, while below I identify those aspects that should be revised in order to better highlight the relevant (and novel) aspects of the study, make the data analysis/interpretation sound, strengthen the conclusions, and, in turn, make the manuscript acceptable for publication.

Major Points

Introduction. The Introduction could and should be improved and sharpened up (and the same may apply to the discussion). For example (Lines 57-65), the authors seem to build their rationale on the (potential) influence of the Mediterranean thermohaline circulation on the AMOC. But this is not the only reason for better characterising the patterns or variability and the drivers of the thermohaline circulation in this basin. The authors could also (or first) more clearly illustrate the importance of the Mediterranean circulation (an notably of the Levantine Intermediate Waters) for the deep-sea ventilation during the formation of organic-rich deposits (sapropels) across the basin (e.g., De Lange et al., 2008 – Nature Geoscience; Rohling et al., 2015 – Earth-Science Reviews and many others) and/or the more recent evidence of a link between Mediterranean circulation changes and positive phases of the North Atlantic Oscillation (e.g., Incarbona et al., 2016 – Scientific Reports). This would make the introduction section better

suited for Climate of the Past by making a more convincing case for the wide relevance of studies like the one by Dubois-Dauphin et al. to the palaeoceanography of the Mediterranean Sea and more generally to our community.

Sea Surface Temperature record. The uncertainties associated with the sea surface temperature (SST) reconstructions presented in the paper (Lines 247-255) should be quantitatively assessed. The authors state '. . .Reliability of SST reconstructions is estimated using a square chord distance test (dissimilarity coefficient), which represents the mean degree of similarity between the sample and the best 10 modern analogues. When the dissimilarity coefficient is lower than 0.25, the reconstruction is considered to be of good quality. . .". This is a merely qualitative statement; the associated with the SST record presented in the manuscript should instead be quantified.

Data analysis. I think data generated by Dubois-Dauphin et al. are of high quality, but I also think that their analysis and presentation could and should be improved. For example, could the records in Figure 3b be stacked? This would highlight the main trends in the data and help the reader to easily follow the interpretation presented by the authors (at the moment also because of a 'wordy' and fairly unfocused discussion this is not the case). Even better, a Monte Carlo analysis of the data in which both uncertainties in the neodymium isotopes and in the chronology are considered would considerably strengthen the data analysis, allow more quantitative arguments, and make this a key example fo the use of neodymium isotopes to address palaeocirculation problems.

Data interpretation. I wonder if the data presented can be so unequivocally interpreted as a reduction of Levantine Intermediate Water (formation? circulation?) during the deposition of sapropel S1 to the extent of arguing for a circulation reversal (which most quantitative analyses so far suggest to be highly unlikely). A possibility that the data cannot rule out is that the Levantine Intermediate Water shoaled rather than weakened and the core sites were bathed by a water mass with a different isotopic fingerprint (e.g., the western Mediterranean intermediate waters proposed by the authors) because of this shoaling.

Minor Points

Lines 36-39: text is not very clear; I would recommend rewriting this bit.

Lines 272-283: I think this section can be moved to the methods and merged with sections 3.3.

Lines 483-484: What do the authors mean by 'intensity changes'?

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