Reply to reviewer #2 comments:

Thank you very much for agreeing to review this paper and for your comments that have improved the quality of the manuscript.

Most of your comments have been taken into account in the revised version of the manuscript.

Please find below a point-by-point reply relative to your comments.

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.

The introduction has been modified by integrating the importance of intermediate and deep water circulation during the formations of organic rich deposits. However, the evidence of a link between Mediterranean circulation changes and positive phases of the North Atlantic Oscillation has not been added as it is relevant only on a decadal timescale, which is not the target of our paper.

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.

The uncertainties associated with SST reconstruction have been plotted on figures 2 and 3. Additional information has also been added in the Material and methods section in order to better quantify the SST reconstruction.

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.*

Although both sites in the Balearic and Alboran Sea are likely bathed by the same water mass (LIW), εNd records are based on different archives (i.e. cold-water corals and planktonic foraminifera). Furthermore, the age model is different as core SU92-33 is based on ^{14}C measurements while CWC are dated by the U-Th method.
On the other hand, data obtained from CWC from the Sardinia Channel display only specific time slices instead of a continuous record over time.
For these reasons, we do not think that a Monte Carlo analysis and/or a stacked record would be relevant for this study.

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.

This alternative hypothesis is now presented at the end of the discussion.

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

The sentence has been slightly rephrased.

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

This section has been re-organised following also recommendations of the reviewer #1

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

We mean changes in LIW production (enhanced or reduced). The sentence has been slightly modified to make it clear.