

## ***Interactive comment on “Tracking climate variability in the western Mediterranean during the Late Holocene: a multiproxy approach” by V. Nieto-Moreno et al.***

### **Anonymous Referee #1**

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This paper presents a study of sedimentation in the western Mediterranean during the last 4,000 years. It is interesting to see a marine study focussed on this very recent period, and generating output with appropriately high temporal resolution (better than centennial). It is certainly an interesting paper from which to start discussions, but I feel there are 4 major and a number of minor issues that need to be better considered before the paper can be published.

Overall, I feel that this paper requires Major Revisions.

Major Comments (Specific)

- 1) Despite trying very hard, I cannot convince myself that the cores show the same  
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histories. Fig. 3 is moderately convincing but Fig. 4 shows two completely different sets of curves. In Fig. 5 TOC shows a moderately similar signal, U/Th is quite different and Br/Al and Cu/Al are both completely different between the two core records. The data in figure 6 do not show even rudimentary similarity, and there are important differences in Fig. 7.

As the authors are presenting these signals as coherent records of regional climate signals, this poses us a very significant problem. Either the proxies are all very unreliable - in which case they should not be used! - or the cores show different histories that need to be interpreted separately. I am inclined to the first view, as even when digested into a statistical assessment of the variability in complete datasets (Fig. 8) major differences remain. Is the correlation between these datasets significant? If not, how can we believe the climate changes inferred from them?

If we are to believe that these two cores show the same climate signal, the authors MUST demonstrate their histories are the same! I am aware the authors make some effort to explain away some of these differences (Page 647, Line 7) but this is not enough. A rigorous analysis of covariance is required if the argument of climate control on these datasets can be accepted.

- 2) Why do the authors not follow Moreno in interpreting the grainsize variability as eolian dust supply? If that idea was true 6 years ago, it is true now! They at least need to demonstrate why eolian supply is NOT capable of driving the variability they ascribe to bottom velocity.

- 3) We need to see more detail (graphically) on the chronology. It would be very helpful to show some kind of time-depth plot laying out the dating errors at different levels clearly.

- 4) The data, proxy-relationships, interpretations and causality are very confused throughout the paper. The authors use the balance of illites and kaolinites as an indicator of the balance of fluvial and eolian input on the basis that a previous study

showed that the former was sourced from the north and the latter from the south. I am not convinced that ALL the palygorskite is derived from the south, but much more importantly I utterly refute that this is a fluvial / eolian proxy! It is a PROVENANCE proxy! What if the southern margin experiences high fluvial input or the northern margin eolian input during some period? Why in their statistical analysis did they find that the fluvial and eolian proxies are both in the first group, when they should be more or less independent (unless they are perfectly opposed, in which case the authors need to tell us!)?

The authors need to be far more realistic in reporting exactly what they can diagnose from their data, and I am therefore not convinced that their ultimate interpretations are robust. I recommend the authors expand Section 3, giving much more convincing arguments for why they are able to interpret the various data in the way they do.

I also recommend that the authors go through the text and remove the sections where causality is confused. For example in Section 9 line 22 they state:-

"A decreasing trend of fluvial derived-elements ..... would suggest a riverine input decline into the basin and subsequent dryness."

I think they mean that they think the reduced fluvial input is DUE TO dryness, but I am not sure. Another example (Section 9.2, Line 19):-

"Such an influx of fresh water from a large flood may have enhanced stratification of the water column, in turn increasing organic matter preservation."

Do they really mean this was a FLOOD - i.e. a single, high-magnitude event? Or do they mean enhanced ongoing diffuse addition of freshwater? I am not sure. A final example (Section 9.2, line 1):-

"The decrease of fluvial derived elements that took place during the LBA became a relatively steady pattern during this period, leading to a progressive establishment of wetter conditions."

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How can a change in fluvial derived elements in a marine sediment record LEAD TO (i.e. cause) establishment of wetter or dryer conditions? The causality here is simply impossible! Moreover, why is a DECREASE in fluvial activity being interpreted as establishment of WETTER conditions? Surely, this is backwards!

Overall, these ambiguous and incorrect statements make the paper far more confusing than it needs to be. The English is fine; but it all needs a really thorough edit for internal logic!

Minor Comments (Technical)

Page 639, line 7: This sentence does not make sense "North Atlantic cold events have been the framework of abrupt decreases of paleo-sea surface temperatures and salinities....".

Page 639, line 9; Concept of freshwater incursions into the Mediterranean are described in (Sierro et al. 2005) and (Rogerson et al. 2010). Page 640, lines 1-10; There are insufficient references in this section. Page 640, lines 11-19; There are insufficient references in this section. Page 641, lines 11-17; There are insufficient references in this section. Line 642, lines 4-7; This sentence does not make sense at this point in the paper. Page 644, lines 13; We need durations of exposure to these wash solutions. Page 645, line 3; bulloides should not be capitalised. Page 648 line 1; I thought that eigenvectors were orthogonal? In which case, the sum of all eigenvectors should explain no more than 100% of the variability? Can the authors make this section clearer please. page 647, line 20; Please define "beidellites-type". Page 647, lines 23-25; There are insufficient references in this section. Page 649, line 22; There are insufficient references in this section. Page 650, line 6; Why does bottom ventilation have to reflect dryness? It could reflect stronger cooling, or even a change in the eastern Mediterranean being transmitted to western bottom water flow via the activity of the LIW? Page 654, line 16-18; Why are you invoking a thermal effect here when 1) you ignored it 4 pages previously?

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## References

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Sierro FJ, Hodell DA, Curtis JH, Flores JA, Reguera I, Colmenero-Hidalgo E, Barcena MA, Grimalt JO, Cacho I, Frigola J, Canals M (2005) Impact of iceberg melting on Mediterranean thermohaline circulation during Heinrich events. *Paleoceanography* 20 (2):doi:10.1029/2004PA001051. doi:Pa2019Artn pa2019

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