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Discussions

Interactive comment on "A 500 kyr record of global sea level oscillations in the Gulf of Lion, Mediterranean Sea: new insights into MIS 3 sea level variability" *by* J. Frigola et al.

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We are very grateful to Andre Droxler for his very detailed comments and suggestions on our manuscript. We have taken into consideration most of the main points are discussed below.

General comments:

1. Most of the concerns of the referee about the manuscript refer to the age model and the discrepancies between different proxies resulting from the model, especially after 150 ka. As a consequence, after stating that he finds the manuscript is very interesting, the referee suggests a dramatically reorganization of the paper based on the

unequal chronostratigraphic control of the age model with depth. While the top 80 m (last 150 kry) of the sequence are very age well constrained, especially the last glacial period (20-70 ka time interval), the chronostratigraphic control becomes poorer down core, thus creating some discrepancies in the correlation of different records, as shown in Fig. 3 as modified by the referee. The referee suggests then that instead of going from orbital to millennial scale interpretations, we should focus the discussion on a full glacial cycle and develop the conceptual model during the time interval with the best chronology, i.e., last 150 kyr, and later test the model back in time. The referee further suggests, within text corrections, to define the age model with the LR04 benthic stacked record (Lisiecki and Raymo, 2005) instead of using the SPECMAP stack (Martinson et al., 1987). In the revised version of the paper we have re-adjusted the age model by tuning our PRGL1-4 oxygen isotopic record with LR04 benthic stacked record (Lisiecki and Raymo, 2005) and we have also included this reference record in the new Figure 3, as suggested by the referee. To improve the chronology of our record we have also correlated our planktic oxygen isotopic record with those of the Portuguese margin (Roucoux et al., 2006) and the North Atlantic region (Stein et al., 2009). Further improving the chronology of MIS 12 and 13 needs substantial additional work, which is in progress by colleagues at the University of Salamanca (Sierro and Flores), which are co-authors of this paper. Nonetheless, as suggested by the referee, extinction of P. lacunosa, which last appearance was observed at around 275 m-depth in the borehole and lowest occurrence in the Mediterranean is dated around 0.467 Myr (Raffi et al., 2006), is taken into account in the age model, which helps us to anchor the end of the succession at ∼530 ka, i.e., during MIS 13. In the new Figure 3, we have also marked with grey bars the main condensed layers (CLs) in order to make more evident their correlation with interglacial periods. This new tuning has resulted in a substantial improvement of the linkage between PRGL1-4 records and the reference records, thus supporting the results presented in the manuscript. Since the main discrepancies between records noted by the referee have been solved thanks to the new, improved age model, as described above, we think that an eventual re-structuring of the manuscript is no more justified. We are very much convinced that the more appropriate is, first, developing the sedimentary model of the GoL margin at orbital scale, whose imprints are more noticeable, and showing the general glacial/interglacial imprints on the margin evolution and, second, focusing on the last glacial cycle and millennial time-scale climate variability. Although time constrain is much better for the 20-70 ka time interval, millennial time-scale sea level oscillations, probably of the order of, or lower than 10-15 m, are still unresolved. Our results demonstrate for the first time that millennial time-scale sea level fluctuations left an imprint in the GoL continental margin, though we cannot state the exact magnitude of such sea level rises. Because of all the points exposed previously, including the relevant addition of the new, re-defined age model, we stress that the structure of the manuscript going from orbital to millennial time-scales is the most appropriate. The new Figure 3 gives further robustness to the results exposed in the manuscript.

2. The referee asks to better explain how the correlation between the seismic profile and borehole data was done in Fig. 2. This point was also mentioned by referee Jamie Austin and we have already given a detailed explanation in the reply to Jamie Austin. In short, this correlation was done visually to illustrate how the observed changes in grain-size data have a clear expression in terms of seismic reflectors. In any case, this point is not a central objective of our manuscript. Furthermore, it has been presented in greater detail in other works and PhD theses (see references in the manuscript) and is part of a new paper accepted for publication (Jouet et al., accepted). We have clarified in the text and in the figure the character of the correlation.

3. The referee finds confusing the interpretation given to explain the correlation between the observed changes in the silt/clay ratio and the identification of sequence boundaries. He states that in the Vail's et al. scheme sequence boundaries relate to sea level lowerings, while we identify sequence boundaries during deglaciations and sea level rises. The point is that Vail et al. and ourselves in our manuscript we are looking at two different things. In Vail's et al. scheme sequence boundaries are de-

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fined following seismostratigraphic criteria after analysis of a complete seismic reflection profile or a set of them (tracking a reflector general downlaping). Our interpretation is based on grain-size changes resulting from the analysis of borehole samples from a single location. While downlap or onlap configurations cannot be easily recognized in a borehole, abrupt changes in grain-size can be related to physical changes, as expressed in seismic reflection profiles. Abrupt changes in the silt/clay ratio of the upper slope sedimentary succession resulting from the migration of deltaic systems can be correlated with stronger reflectors marking sequence boundaries.

4. The referee states that “one could argue during time intervals when the water depths were lower than the depths we observed today and other max sea level intervals, the DSWC could be even more frequent”. Certainly, DSWC as physical oceanography process could occur not only during highstands s. str. but also during periods of relatively high sea level when the continental shelf or a substantial part of it was flooded. DSWC is mainly controlled by the cooling of surface waters due to wind action, which is related to climate. However, oscillations in our silt/clay ratio do not show any correlation with periods of intensified wind-stress, e.g., glacial stages or cold Greenland Stadials. By contrast, silt/clay ratio increases occur during warm periods. It must be taken in mind that the capability of dense shelf water to cascade involving large amounts of coarse particles is strongly dependent on the volume of cascading water involved and on the accommodation space on the shelf, which change as a direct function of sea level. Therefore, it makes sense that during periods of sea level lower than today DSWC had also a reduced transport capability of coarse particles. Furthermore, during lowered sea levels rivers mouths open at shorter distance to the upper slope, which should result in an increased supply of fines, diluting the load of coarse particles accumulating in the upper slope. In conclusion, independently of DSWC frequency, oscillations in our PRGL1-4 record are mainly showing fluctuations in the mechanisms involved in particle transport, which are ultimately controlled by accommodation space, sea level and climate.

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5. The referee writes that “this conceptual model looks too simplistic in particular when only two end members lowstand and highstand scenarios are considered, and transition intervals of time, deglaciation – sea level fall, are not included”. In our opinion, the conceptual model considers both sea level falls and lowstands, and sea level rises and highstands. Nonetheless, transitions associated to abrupt sea level rises and deglaciations, and gradual sea level reductions after interglacial stages cannot be better solved with PRGL1-4 data due to low resolution reached during interglacial stages. This could be solved only if sedimentary successions with much higher temporal resolution during highstands would become available.

6. The referee has a doubt about the planktic oxygen isotope record and the age model, which refers to the lightest values observed during the beginning of MIS 6. This ïĄď18O excursion relates to a relatively wet period that occurred during the MIS 6.5 event that corresponds chronologically to the deposition of the sapropel event 6 (S6), which is in agreements with previous studies based on deep-sea sediment cores in the Western Mediterranean Basin (Bard et al., 2002).

7. The referee suggests that the sensitivity of the silt/clay ratio to sea level fluctuations during MIS 3 could be related to the fact that sea level oscillated between minus 60 and 80 m. This is a useful observation and we have included a short paragraph about it in the discussion section.

Specific comments:

Some of the specific comments on the manuscript are related to the general comments already discussed above. Most of them and other specific comments suggested by the referee within the text have been considered in the new version of the manuscript.

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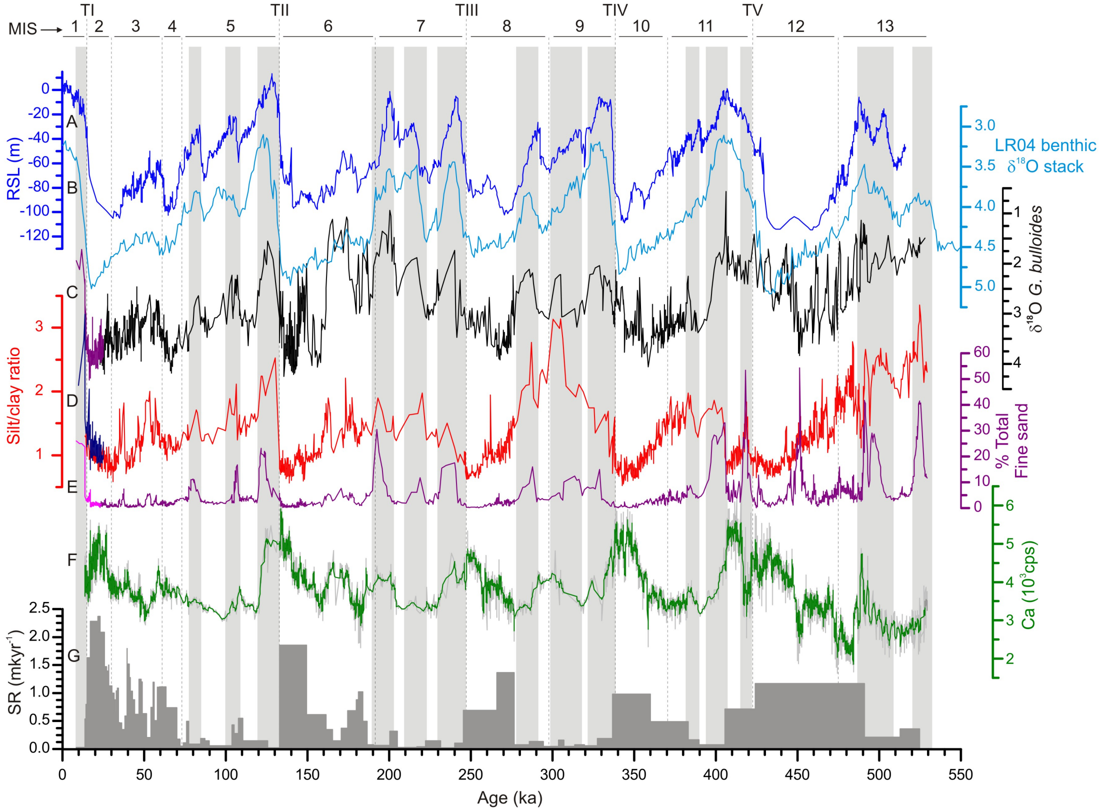

Fig. 1. Figure 3 REVIEWED