Alessandro Amorosi - Interactive comment

General comments

This paper provides convincing evidence of multidecadal to century-scale record of sea-level and climate change from the Holocene succession of the Rhône prodelta, in southern France. Based on a multiproxy record from a 7 m-long core, the authors provide nice documentation of transgressive and highstand sedimentation, focusing on a condensed interval that spans a 4 kyr interval of time. This study clearly documents how subtle changes in the meiofauna (benthic foraminifers and ostracods) can be used as a proxy for both sea-level and climate change. In particular, though I'm not a fossil specialist, I enjoyed the way ostracods were used on very high-resolution time scales as indicators of hydrological fluctuations.

Fossil analysis is very accurate, and in general, I found the data presented and the inferences drawn to be convincing. In my opinion, this paper can be published following minor revisions. A few points that might deserve further attention are highlighted below.

Specific comments

The authors emphasize the multi-proxy character of their study, which includes sedimentology, paleontology, mineralogy and geochemistry. Mineralogical interpretation, however, is rather vague and geochemical analysis is restricted to a single element (Ti) profile, with no discussion/interpretation in text. The sedimentological and paleontological aspects of this paper are very robust and fully support the conclusions. Changes in sediment composition are very important, but reliable interpretations probably require a significantly larger data set (to the scale of the source-to-sink system) than the one available for this study. I think this part could be removed without detriment.

We agree with Alessandro Amorosi that geochemical analysis in the present study was too vague (restricted to Titanium element) and did not really support our results and discussion. Thus, we decided to remove XRF data from our paper.

Even if clay mineral assemblages appeared to be less robust than micropaleontological data, we would prefer to not remove this aspect of our paper. Indeed, we observe that changes in clay mineralogy through the core are closely related to the successive migrations of the Rhone River. It also appeared that the condensed section is characterized by a distinct change in clay mineralogy with a decrease in illite content and an increase in smectite content. Nevertheless, we agree that a larger dataset would be useful in the future to discuss in detail changes in sediments composition and allow a source to sink record and understanding of sediment composition evolution through time. But only few clay mineral studies have been carried out in the Rhone watershed and Gulf of Lions in the past. This dataset is thus considered as important even if detail interpretation of this one is yet difficult.

In terms of sequence stratigraphy, what the authors call the "maximum flooding surface" (MFS) should be termed more properly (at the core scale) the "condensed section"(CS). CS is the label that sequence stratigraphy uses to describe exactly what the authors see in their cores: a few dm-thick, condensed stratigraphic interval that marks the inversion from a transgressive to a "regressive" depositional trend. On a seismic scale, the authors can use the term "MFS" for this very thin stratigraphic interval. On the very high-resolution scale of their study, however, I feel that the term "CS" would be more appropriate (see comment below).

We modified the term "Maximum Flooding Surface" in the text. We used only the term MFS when we discussed seismic data. When we are discussing sedimentological data we changed it to "condensed section" as suggested.

I can't see "very distinct changes in all proxy records at the maximum flooding surface". There is clearly an inversion that can be seen across the condensed section along most profiles, but these changes seem to occur gradually. Several mineralogical proxies show remarkable changes a few dm above the condensed section, and not at its top. Similarly, changes along fossil profiles occur even below the CS, and not necessarily at its base. Given the definition of the maximum flooding surface, which simply marks the turnaround from transgression to regression, there is no reason to record sharp changes across this surface, while opposite trends are clearly expected above and below the MFS/CS. In this regard, the data shown by the various profiles are fully consistent with the authors interpretation.

We agree that not all proxies show a sharp change at the transition between Unit U500 and the condensed section. Except for the clay mineral assemblages which present a very distinct change (decrease in illite content and increase in smectite content) at this transition, micropaleontological proxies are rather characterized by a gradual change through this transition. Therefore we modified this sentence.

"The transition between the early Holocene deposits and the middle Holocene condensed section is marked by a gradual change in all proxy records."

Last point: the authors reject three "old" radiometric dates from the lower part of their core (a > 2m-thick stratigraphic interval), based on regional correlations of unit U500. For convincing the reader of their interpretation, they could probably expand slightly on this, stating how many radiocarbon dates from unit U500 are regionally available and, especially, which sedimentary facies did they find in other cores penetrating the same unit (was it the same deposits or something different?).

Indeed, we rejected three "old" 14C dates from the lower part of the studied core. We consider that these 14C dates are too old for seismic unit U500. Considering the age of the underlying deposits of seismic unit U400 in this area (ca. 10.5 ka cal. BP; Berné et al., 2007), we can only assume that 14C dates obtained in unit U500 are generally biased. At the regional scale, this transgressive parasequence has been described in all the studied cores as a unit made of tempestite deposits (Fanget et al., 2014). Thus, it consists of numerous silt or very fine sand laminae interlaminated with silty clay. Within these deposits, benthic foraminifera (picked for 14C dates) were mainly represented by infra-littoral (upper shoreface) species (*Elphidium crispum*). These benthic foraminifera are likely reworked by high energy hydrodynamic processes and incorporated into modern deposits. As a consequence, these 14C dates are too old. Only 14C dates from the upperpart of unit U500 (i.e. five 14C dates at the regional scale, Fanget et al., 2014) are considered as reliable. It is explained by a gradual change in lithology (increase in clay content and decrease in silt and fine sand laminae) within the upperpart of unit U500, which reflects a deepening of depositional setting and a reducing of reworking processes.

We added some information in the text to explain in more details why we rejected these 14C dates. We also added a paragraph in the discussion section (5.1.) to discuss in more details reworking processes in subaqueous deltaic environment.

"Considering the age of the underlying deposits of seismic unit U400 in this area (i.e. paleo-deltaic complex of the Rhone (ERDC), ca. 10.5 ka cal. BP, Berné et al., 2007), we assume that 14C dates obtained in this unit are generally biased, because of reworking occurring in shallow water environments. This interpretation is supported by the nature of sediments that composed this unit. Indeed, at the regional scale, seismic unit U500 is made of tempestite deposits (Facies 1, section 4.2.) which mainly contain infra-littoral (i.e. upper shoreface) benthic foraminifera (Elphidium crispum; see Table 2 and Fanget et al., 2014). These benthic foraminifera are likely reworked by high energy hydrodynamic processes. The older ages

obtained within seismic unit U500 are thus probably the result of reworking during the transgression of an underlying Younger Dryas/Preboreal delta front (for more details, see section 5.1)"