Response to Reviewer Comment 3

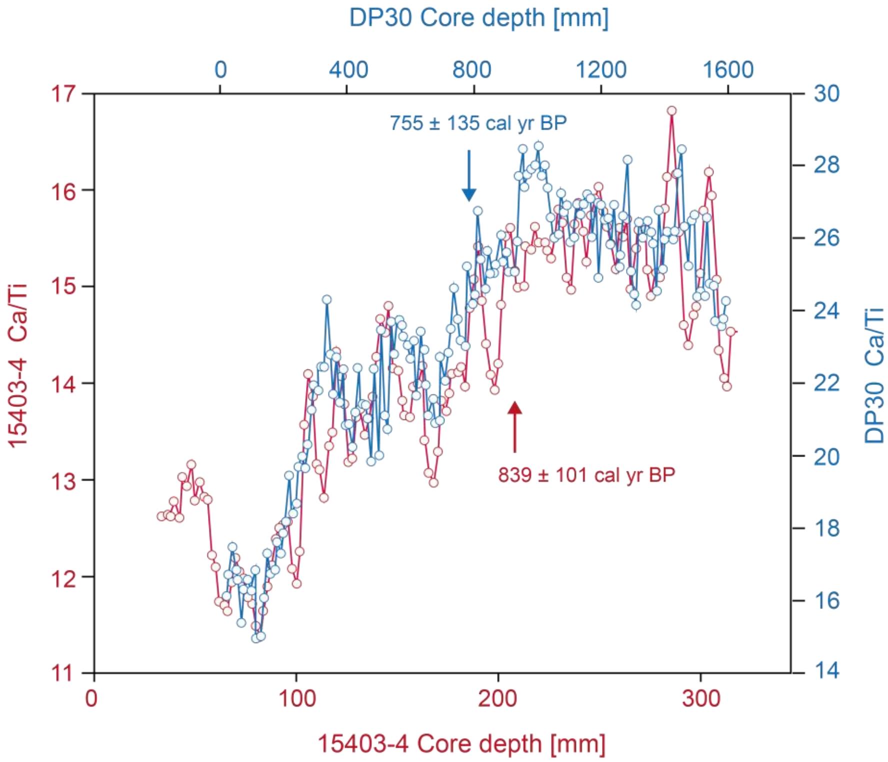
We would like to thank the anonymous referee 3 for the detailed comments and suggestions that will help to improve the manuscript substantially. Below we respond, point by point, to all comments. Referee comments are displayed in italic font, our response is written in normal font.

1) Due to the water depth of the marine sediments recovered in the cores, when the authors described the sediment they have to use the term hemipelagic sediment and not "homogeneous nannofossil-rich mud". In addition, due to the goal of the manuscript it is necessary to be more precise in the description of the sediment recovered in the cores. Are there other biota with benthic foraminifera? I know that in this area, there are also layers with ostracods and in some case, there are also pteropods. Are there evidence of this biota?

We will add the term "hemipelagic sediment" for the general characterization of the sediments. The studied core intervals are rather homogenous. Lithologically, the sediment can be correctly classified as nannofossil- and foraminifer-bearing mud. The sediment further contains traces of pteropods, ostracods, echinoid spines and other microfossils, which are not concentrated in layers but are more or less homogenously distributed over the entire interval studied. We will modify the text accordingly.

2) In my opinion, the first problem is related for the last two centuries. There are no radionuclides data (210Pb and 137Cs). Did the authors consider the radionuclides data published in Grauel et al (2013) to create the age model for the last centuries? Without radionuclides data, the authors cannot produce an age model for the last two centuries (i.e. last 150 years) to demonstrate the continuous and normal sedimentation rate in the upper most part of the core.

The maximum depth of oxygen penetration into the surface sediment of gravity core GeoB 15403-4 was identified by distinct color changes at 6 cm. The oxygen penetration depth in a multicore from the same station (GeoB15403-6) was identified at 8cm sediment depth. Furthermore, when the core was opened and sampled, the sediment surface appeared undisturbed with no indications of compression or sediment loss as seen on core pictures. Therefore, we argue that the core top represents the year 2011 when the core was retrieved. To support this, we correlated Ca/Ti data from the XRF core scanning to the adjacent sediment core DP30, which contains modern sediments at the core top as proven by ^{210}Pb dating (Goudeau et al., 2014) (RRC 3, Figure 1). Although accumulation rates at the two sites are different, the close correspondence of the Ca/Ti records (further validated by the available AMS ^{14}C ages) suggests undisturbed records and confirms the reliability of our age model. The presence of an undisturbed sediment surface in GeoB15403-4 is further supported by the continuation of the Ca/Ti curve at site GeoB15403-4 since it was sampled few years after core DP30. This information will be added to the manuscript appropriately.



RRC3 Figure 1: **Comparison of Ca/Ti records of adjacent cores DP30 (blue curve; Goudeau et al. 2014) and GeoB15403-4 (red curve). The close correspondence and available ^{210}Pb data for DP30 suggest the presence of a largely undisturbed sediment surface in both records.**

For further refinement of our age model of multicore GeoB15406-4 we now use the stable oxygen isotope record of the benthic foraminifer *Uvigerina mediterranea* for correlation with the ^{210}Pb dated multicore NU04 (Grauel et al., 2013). The isotope records show a close correspondence and the revised age model exhibits deviations in the range of -19 to +16 years (standard deviation of 12.2 years). We will use this new age model, which is still in accordance with the AMS ^{14}C age for the base of our multicore and does not change the general interpretation of our proxy data.

3) The second problem is associated to the occurrence of a tephra layer related to Lipari Eruption. The authors reported as reference for this tephra, a manuscript submitted as Menke et al.. I do not think that the authors can use this tephra as tiepoint, reporting as reference a manuscript under review. In addition, the authors refer this tephra layer to Lipari eruption, but this eruption is related to Monte Pilato eruption phases (see Forni et al., 2013, Geological Society of London). This Monte Pilato volcanic phase spans from 729 to 1220 AD (see Forni et al., 2013, Geological Society of London). Why did the authors decide to associate this tephra layer to an age of 1174 yr BP (776 yr AD) that corresponds (or it is very close) to the age associated to the beginning of this eruption phase? Maybe it is wright, but if this reconstruction is based on data reported in the manuscript Menke et al. under review, I think that the authors have to improve the present version of the manuscript. Alternatively, they have to exclude this tephra from the present age model.

It was unfavorable that the cited manuscript concerning the age model of GeoB15403-4 based on tephrostratigraphy was not available at the time of the review process. This manuscript is now accepted for publication in the March Issue of Quaternary Research (Volume 89, Issue 2). For a detailed description and critical discussion of the age dating of the ash layer see Menke et al. (2018) (https://doi.org/10.1017/qua.2018.2). Several arguments favor the AD 776 Monte Pilato eruption rather than the AD 1220 Lami eruption. The Monte Pilato eruption is characterized by slightly higher SiO_2 and lower K_2O contents, which is in accordance with the geochemical analyses of the detected cryptotephra. Based on associated pyroclastic layers, the size of the Monte Pilato eruption was much larger when compared to the Lami eruption. Accordingly, the Monte Pilato eruption had at least a subplinian character, which provided the necessary size to inject sufficient volcanic matter high into the atmosphere to be transported over the required distance to the Gulf of Taranto.

4) The authors reported from line 5 to line 10 (pag 5) information concerning the undisturbed sediment in the uppermost part of the records. Without radionuclides and proxy of porosity, it is not possible to make this assumption. There no evidence to support this assumption. In table 1, it is necessary to specify if the authors run AMS14C on mix of planktonic foraminifera or on single species. In addition, it is important to indicate the thickness of the sample used for each AMS14C dating. This information is important to analyse the propagation of errors. In your age-depth profile (Fig. 2) it is important to show the propagation of errors. Because of the authors have no radionuclides data, the propagation of errors cannot be extended to top core. Please it is necessary to show the algorithm used to create the age model.

Concerning the age model of the upper part of the core see our response to comment 2). As stated in the manuscript we used samples of mixed surface-dwelling planktonic foraminifera to generate the AMS ^{14}C dates. These comprised shells of *Globigerinoides ruber* (variety pink and white), *Globigerinoides conglobatus*, *Globigerina bulloides*, *Orbulina universa* and *Globigerinella siphonifera*. We avoided shells of deep-dwelling taxa such as *Globorotalia inflata*, *Globorotalia truncatulinoides, and Neogloboquadrina dutertrei*. A description of the species that were used will be added to the methods section. Figure 2 B and C show the thickness of the depth interval that was used for dating with a horizontal line, however since the depth interval for site 03 is so small, the line in Figure 2 B is hard to see. We suggest to add a column with values for the sample thickness to Table 1.

5) Spectral analyses: I would like to suggest to use the "Intrinsic Mode Functions" (IMF) (Huang et al., 1998) to analyse the signal and to run wavelet analysis on selected IMF component. This is the correct approach when you analysis records for the last millennia. Only with this approach you can identify the stable frequency associated to your proxy and if it is continuous present along the whole study record. The single spectrum reported in figure 11 represents a mean value within the record, so that it is not representative of a possible forcing.

We agree that wavelet analysis will add significant information on the continuity of the periodic variations in our data. Accordingly, we will perform wavelet analyses on the SIIBF and EBF data series. We will use the approach of Torrence and Compo (1998) applying a Morlet wavelet spectrum. Both, Morlet wavelet spectrum and Hilbert spectrum reveal similar energy-frequency distributions. In addition, we will perform time-series analyses using the REDFIT algorithm of Schulz and Mudelsee (2002) because it allows a proper error estimation of the global spectrum.

6) Concerning the recently scientific literature focused on the shallow water environment, the authors did not consider in this manuscript several articles (Ferraro et al., 2012; Vallefuoco et al., 2012; Lirer et al., 2014; Margaritelli et al., 2016; Di Rita et al., 2018; Oldfield et al. 2003; Bonomo et al., 2016; Di Bella et al., 2014). These articles in my opinion offer some important information for the submitted manuscript. Ferraro et al. (2012) and Di Bella et al. (2014) for benthic foraminifera, Bonomo et al. (2016) and Di Rita et al. (2018) relation between NAO and runoff/alboreal pollen, Oldfield et al. (2003) with low resolution concerning benthic foraminifera, etc. . ... Are there specific reasons for this choice? Concerning the NAO index, I *think that the article Brunetti et al (2002) focused on the winter precipitation in Italy modulated by NAO, has to be take in account. In addition, the NAO forcing has been shown also in other fossil marine sedimentary archives by several authors (Chen et al., 2011; Nieto-Moreno et al., 2013; Goudeau et al., 2015; Jalali et al., 2015). Why the authors did not consider these references from Mediterranean area?*

We appreciate the recommended literature and will consider the relevant references in appropriate sections of the manuscript.

7) In Figure 3A, the authors compared SIBF signals in the two records, but as documented in the figure for the last two centuries, the two curves have an antithetic pattern. The same framework, maybe less pronounced, is also shown for SIIBF signals. In my opinion, in the manuscript the explanation reported for this discrepancy in the last two centuries is not scientific supported. In my opinion, without radionuclides dating this problem cannot be solved. Again, I would like to suggest to the authors to plot the distribution patterns of benthic foraminifera per gr of sediment to understand or to try to interpret correctly this discrepancy. In my opinion, the differences in benthic assemblage reported for both study sites in figure 6, is not so evident. In addition, without a detailed high-resolution morphobatimetry of the study area it is not possible to propose this type of interpretation.

After applying the revised age models to our records the different trends in the abundance of SIIBF are still evident and can be attributed to spatial differences in organic matter flux rates. According to our experience, the benthic foraminiferal number BFN (Ind./g sediment) seems not appropriate to evaluate the microhabitat structure and thus ecological significance of the fauna. The BFN or BF accumulation rate has been interpreted as a general function of organic matter fluxes to the sea floor in the Pacific Ocean (Herguera and Berger, 1991). However the applicability of this approach in other oceans has been challenged since then (e.g., Schmiedl et al. 1997) because this parameter is strongly influenced by variable accumulation rates and other factors. Application of the BFN (to the bulk fauna and single taxa) appears useful when the counting sums are low in samples containing only few individuals and relative numbers would lead to misinterpretations. In our samples we have aimed at counting 300 individuals or more per sample, allowing a proper ecological evaluation as reflected by the relative proportion of foraminifera with different microhabitat preferences. Specifically, higher relative proportions of SIIBF clearly indicate increased organic matter fluxes and associated food availability at the sea-floor and vice versa.

9) The authors have to focus as follows: 1) on chronology of the last two centuries and on the determination of propagation of errors, 2) on the interpretation of benthic foraminifera per gr of sediment and not in percentages, due to the target of the manuscript. This approach could help to interpret the benthic data vs the target of this manuscript. 3) I think that the authors have to take in account also the several dams build along the rivers of the Adriatic Sea. These constructions changed significantly the sediment outflow in Adriatic Sea. 4) I would like to suggest to see also the contribution of Ofanto river. 5) It is necessary to improve the spectral and wavelet analysis carried out on the proxies. 6) If is necessary to filter each frequency and compared these with internal or external forcing. 7) Due to the target of the manuscript it is necessary to compare the study records (biotic or abiotic proxies) with proxy of river discharge.

We will revise our manuscript including most of the suggestions by the reviewer as outlined above. The chronology has been further tested and confirmed for GeoB15403-4 and will be refined for core GeoB15406-4. We will still base the ecological evaluation of the benthic foraminifera on the relative proportion of different microhabitat groups because the high counting sums guarantees a proper representation while the BFN can be biased by variable accumulation rates and other factors (such as patchiness). We will consider the other mentioned points appropriately. Specifically we will add wavelet analyses and elaborate a more on the riverine contributions of the different areas.

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