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Interactive comment on "Combined North Atlantic and anthropogenic forcing of changes in the marine environments in the Gulf of Taranto (Italy) during the last millennium" *by* Valerie Menke et al.

Anonymous Referee #3

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Dear Natascha Töpfer Copernicus Publications Editorial Support

I hereby you receive my report on the MS "Combined North Atlantic and anthropogenic forcing of changes in the marine environments in the Gulf of Taranto (Italy) during the last millennium" by Menke et al The authors analysed benthic foraminifera and clay mineral data providing information on natural and the anthropogenic forcing during the last millennium in a shelf area of Taranto Gulf. This area has been intensively studied (about 19 scientific articles) by several authors (from Cini-Castagnoli et al. 1989 to Goudeau et al. 2014) using different tools. In all these scientific articles, it is evident the extreme high sedimentation rate of this area mainly over the last millennium, but it also evident the occurrence of several problem in terms of chronology (see the high-error bars for AMS14C dating, Grauel et al 2013; or Versteegh et al 2007 where it is impossible to understand the chronology and where the references reported for the chronology have no data). In most of these articles is reported a more or less constant sed-rate over the last two millennia. It is surprising to document constant Sed.Rate in a shelf area with a dynamic marine environment. 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? 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 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 tie-point, 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. 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. 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. 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? 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. 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 My overall conclusion is that the manuscript is properly constructed and is suitable for the journal but unfortunately, it needs major revision.

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