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Interactive comment on "Anthropogenically induced environmental changes in the northeastern Adriatic Sea in the last 400 years (Panzano Bay, Gulf of Trieste)" *by* Jelena Vidović et al.

Anonymous Referee #1

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This paper deals with long-term environmental assessment in the northeastern Adriatic Sea, based on benthic foraminiferal and geochemical analysis. Benthic foraminifera are one of the most useful meiobenthos, because they can be used not only as paleoenvironmental indicator but also as recent environmental indicator. The authors discuss anthropogenically induced environmental changes over the last 400 years by many statistical analyses. The manuscript is generally well written, but I think some discussions should be added before its acceptance.

1) Many statistical analyses make the manuscript complex and confusing. Indeed,

the result of NMDS is not fully discussed. What foraminiferal species are related to NMDS 1 and 2, and what can we learn from the temporal changes of NMDS 1 and 2? Figure 7 is not fully used in discussion. So, I think there is no need to show figure 7 in this manuscript. I think that figure 6 (b) and (c) are enough for the conclusion of the manuscript. It is also same to figure 8. What is PC1 in figure 8? I think PC1 is the result of PCA of geochemical data (Fig. 4). However, the values are not equal.

2) The authors concluded that the foraminiferal community has adapted to naturally elevated trace element concentrations, but such adaptation cannot be evaluated from this study because anthropogenic impact is found even in the bottom of the core.

3) There are no explanations about dash line, solid line, and grey circle in figure 2. The explanations are needed in caption. I think dash line means the range between maximum and minimum ages of shells. If so, the range of ages between 90cm and 120cm are very wide (i.e. it shows "modern" to "old" ages). Moreover, calculated sedimentation rate between 120cm and 140cm is very high (ca. 2cm/yr). The concentration of Al decreases abruptly during this period. This may indicate the change of depositional environment. Evaluations about these points are needed. The authors argue that short term decline of Ammonia sp. in the latest 17th century may have caused by the increases of pollutants, because Ammonia sp. is sensitive to pollutants (p. 17 line 410 to 413). However, this short term drop is only one sample, and the drop of Al during this period may indicate the change of depositional environment as mentioned above. It may be the taphonomic effect. Indeed, it seems that the change in grain size distribution occurs simultaneously with Al drop. So, we cannot discuss the decrease of Ammonia in relation to anthropogenic impacts. Moreover, the authors describe that the increase of Ammonia sp. during the late 20th century is correlated with the increase of persistent organic pollutants (p. 21 line 511 to 516). These two interpretations of Ammonia species are inconsistent (Ammonia sp. is sensitive or tolerant to pollution? Ammonia sp. increases after 1950 when some pollutants increase rapidly.).

4) Major foraminiferal change during 1700s to 1800s is fluctuation of Valvulineria

species (Fig. 5). The authors argue that the distinct peak of Valvulineria in the early 19th century coincides with the coldest and most humid phases of the LIA by citing previous study. However, the same distinct peak of Valvulineria also occurs in the early 18th century. The authors also describe that Valvulineria is adapted to large seasonal variability of organic matter, periodic hypoxic conditions, increased fluvial runoff and increased turbidity. However, nutrient concentrations and grain size are relatively stable rather than variable. The authors do not discuss this point.

5) The authors describe that high abundances of Non keeled Elphidium, Valvulineria, and Ammonia during the 17th to 19th century suggest strong seasonal variations of river runoff and organic matter input based on the result of RDA (p. 17 line 425 to 429). However, I think that RDA results are strongly influenced by top 20cm (20th century) data of the core, because N. tot is relatively stable below 20cm. Gradual decrease of N. tot may indicate decomposition process. Indeed, if my understanding is right, high positive correlations occur within top 20cm (Figs. 4 and 8).

6) The authors propose the three hypotheses in introduction section, so inspection result of the hypotheses should be written in conclusion section. Especially, hypothesis three (relationship between foram diversity and pollutants) was not incompatible with the result. Many previous studies have already suggested that early phase of eutrophication cases increase in foram diversity.

7) p. 18 line 453 to p. 19 line 473: The authors associate the increases in the abundances of Miliolinella, Triloculina, and Haynesina with enhanced microalgal biomass (mainly diatoms) as a consequence of nutrient enrichment. However, certain Elphidium species feed diatoms and prefer organic rich sediments. Ammonia tepida decreases during this period, but A. tepida is herbivorous and tolerant to all kinds of stress conditions, including organic enrichment as the authors describe in the manuscript. So, this faunal change cannot be explained only by enhanced microalgal biomass and nutrient enrichment.

9) p. 20 line 504 to p. 21 line 506: The authors describe that the presence of PAH is probably related to industrial activities, and it started to increase from the middle of the 20th century. However, it seems that PAH concentration start to increase from the latest 18th century, although it increases rapidly from the middle of the 20th century. So, industrial activities was advanced from the latest 18th century.

10) Foraminiferal discussions in subsection 5.1 and 5.3 are same although different pollutants are described in each section. So, subsection 5.1 and 5.3 should be combined to avoid confusion.

Figure 4: In the text, the authors describe “the first two axes explaining 75.8% of the variance”, but the value of PC2 is 14.8% in the figure 4 (sum of axes is 74.8%). Which is correct? Names of each arrow are piled and indistinct. Please redraw in the clearest way possible.

Figure 5 : “sp.” is not italic.

References: Di Leonardo et al. (2006) and Solis-Weiss et al. (2001) are not cited in the text. There are discrepancies of publish year between the text and the references. P. 18 Line 440: Naeher et al. (2014) P. 11 Line 270: R Core Team (2015) P. 4 Line 104: Xuschin and Piller (1994)

Best regards,

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