First of all, the authors would like to thank reviewer 2 for such a precise and helpful review of our work. We think that the suggestions and comments made helped improve the manuscript. In addition, the questions raised also made us re-think a couple of the sections. In the manuscript, find the changes suggested by reviewer 2 depicted in blue.
Here, to be clear and precise with our answers, we used **R#-C#** and bold notation for reviewer 2 comments, and our answers appear as **R#-R#.**

R2-C1: In terms of missing references:
The sediment trap site is located in the same area analyzed by K. Schroeder, J. Chiggiato, S. A. Josey, M. Borghini, S. Aracri & S. Sparnocchia (2017). Rapid response to climate change in a marginal sea. *Scientific Reports* | 7: 4065 | DOI:10.1038/s41598-017-04455-5. It is very strange that this reference is not reported in the submitted manuscript.
In addition, concerning the oceanography of the Mediterranean is not reported as reference Pinardi et al., (2015). Mediterranean Sea large-scale low-frequency ocean variability and water mass formation rates from 1987 to 2007: A retrospective analysis. *Progress in Oceanography* 132 (2015) 318–332.
and in the following paper (Garcia-Solsona et al., 2020) one of the study station is very close to the sediment trap location. Maybe you could find additional information for the submitted manuscript.
Garcia-Solsona E., Pena L. D., Paredes E., Pérez-Asensio J.N., Quirós-Collazos L., Lirer F., Cacho I., (2020). Rare Earth Elements and Nd isotopes as tracers of modern ocean circulation in the central Mediterranean Sea. Progress in Oceanography, 185. https://doi.org/10.1016/j.pocean.2020.102340 (7) -
R2-R1: Authors appreciate the references suggested by reviewer 2. They are now included in the manuscript in **lines 152-630-708-709** (as an example).

R2-C2: ***Chapter 3.3:***
The authors report line 218 that: G. bulloides includes G. falconensis [(this is my interpretation when I see in the text Globigerina bulloides (+ G. falconensis)]. Maybe my interpretation is wrong. But, if it is right my interpretation, the question is that in the text (Table 1, or line 363) the authors consider separately these two species. Please try to control, if necessary.
In the plate is missing the picture of Turborotalita quinqueloba. Just a question: did you find only Globigerinoides with chambers lobulate (trilobus) or also with elongate and sac-like (sacculifer), and if you found both, what is the abundances of these morphotypes? Are the present in the same seasons?
R2-R2: Authors agree with this remark, it is odd. As only one individual of *G. falconensis* has been identified through the samples, we decided to remove it from Table 1 and specifiy in a clearer way that only one of this taxa has been counted in **Lines 224-225** ”Finally, only one individual of *G. falconensis* has been identified**.”** Also, now the **Table 1** caption states : ”Note that *G. falconensis* has not been included due to its scarcity (only one individual was identified).”

At first, we did not include a picture of *T. quinqueloba* to **Plate 1** because our intention was to showcase only the most abundant and representative species. However, in the new version of the manuscript, **this species has been included**.
Concerning the differentiation between lobulated and sac-type *Globigerinoides*, we mainly found individuals belonging to the first group, the sac-type individuals were scarce. The latter were identified mainly during summer and autumn. This also appears in **chapter 4.1, lines 355-357.**

R2-C3: ***Chapter 3.4:***
Line 238-239: it means that the SST data in the same site of sediment trap are also from mooring line data. Is it wright? In several figures of the submitted manuscript, are from satellite derived or from data in situ the SST data? In is not clear for me.
R2-R3: Authors apologize for this confusion. No data comes directly from the mooring line, as reviewer 2 states later in the comment, all the SST and chlorophyll-a data represented in the figures are satellite-derived. We simplified the first sentence of the chapter in order to amplify the satellite side of it. **Line 247** "To assess the possible relationship of planktic foraminifera fluxes with environmental variability, ~~key environmental parameters, namely~~ satellite-derived chlorophyll-*a* and Sea Surface Temperatures (SSTs) were retrieved from global data sets." Furthermore, the section has been divided into two new sections: **3.4 Satellite-derived environmental parameters** and **3.5. Planktonic foraminifera flux and surface sediment data from other Mediterranean settings**.

R2-C4: ***Chapter 4.3:***
Please in Fig. 3, report the planktonic foraminiferal species according to the % abundances: 1) G. inflata, 2) G. truncatulinoides, 3) G. bulloides, 4) G. ruber pink, 5) G. ruber white, 6) O. universa, 7) G. rubescens…….

I do not understand why the authors decided to plot in Fig. 3 the planktonic foraminiferal data also in %. I think that the authors have to plot only the flux (shells). In many cases the strong differences in shells, between the different months, produced an altered % signatures. The impact on % abundances it is possible to observe on G. truncatulinoides signal. According to the ecological niche of G. truncatulinoides is an indicative species of deep vertical mixing during the winter season, so that it is very strange to observe high abundance % of this species during the summer season of sediment trap data. Is there an explanation of this discrepancy? In fact, if you consider only the flux signals, G. inflata and G. truncatulinoides are in phase according to their ecological preferences.
Moreover, the Gulf of Lion sediment trap data, during winter season, document that the high abundance of G. truncatulinoides results almost in phase with high abundance of G. bulloides. And it has sense. In your record, this relation is not evident. Is there is an explanation for this discrepancy? The ascended of G. truncatulinolides to the euphotic zone, where it proliferates due to strong advection of nutrients from the nutrient-rich deeper layers and consequently high primary productivity could be supported by the increase in abundance of nutrient rich species G. bulloides. In the Sicily Channel this

relation seems not documented, probably related to the occurrence of other oceanographic influence. Have you an idea?
Can the authors explain the criteria adopted for the season's boundaries? I think that the spring season has to start with sample 5-april and not before and also 1-june is summer and not Spring.
R2-R4: Authors agree that the relative abundances signal (in %) can sometimes be altered by a low number of individuals in the samples and therefore produce "extreme" results and lead to various interpretations. Therefore, now **Figure 3** only shows the fluxes and the species are plotted according to their abundance order, as suggested. Furthermore, the same figure with the relative abundances has been added to supplementary material (Supplementary Figure 3).
Consequently, the results **chapter 4.3**. has been modified to be more direct and spend less to the explanation of the seasonal abundances. **Lines 411-421.**
Concerning the questions asked, in our opinion, the discrepancy between the timing and abundance of *G. bulloides* and *G. truncatulinoides* resided in the amount of nutrients and the productivity of the water masses. In the Gulf of Lions, *G. bulloides* is the main species and shows the classical "bloom" behaviour, while *G. truncatulinoides* is present more constantly and its variations are more gradual. Although the timing of the two species is different here, the response of *G. truncatulinoides* is similar across the record. Furthermore, from a productivity standpoint, the Sicily Channel is less productive than the Gulf of Lions, which in turn does not benefit *G. bulloides* abundances and, as the upwelling in our study zone is less pronounced, the timing between the two species is different. Furthermore, the upwelling conditions in the central Mediterranean are caused by the LIW flowing to the western part of the basin, which leads to reduced productivity upwelling conditions. This could explain the lack of *G. bulloides* blooms here. Therefore, we agree with the suggestion made by reviewer 2 that other oceanographic processes must be playing a major role here. This discussion is now included in the new version of the manuscript in **chapter 5.2. lines 616-633.**
Finally, the seasons have been established according to the astronomical sense of the term (i.e. equinoxes and solstices). They have not been considered within the samples (i.e., spring begins with the first sample considered to be collected during spring). As we are in a temperate zone of the ocean, with a temperate climate and we are located in the northern hemisphere, we therefore consider Winter from the end of December to the end of March, Spring from the end of March to the end of June, Summer from the end of June to the end of September and Autumn from the end of September to the end of December. This was also made to be coherent with the other datasets with which we compared our data. However, if reviewer 2 does not agree, we'll change the season's settings. The seasons description now appears in **chapter 3.3 lines 242-244: "Lastly, to describe the seasonal flux variations and to put our results into a regional context and be coherent with previous studies, each season was defined as spring (March–May), summer (June–August), autumn (September–November) and winter (December–February)."**

R2-C5: ***Chapter 5.1:***

I am very curious if in terms of benthic foraminifera, the authors found only these two species or is there a diversified benthic assemblage? If they found a diversified benthic assemblage, the identifies species are related to the same environment. Did you consider this issue?

In addition, the authors considered the benthic species a result of resuspended sediments process. The question is as follows: In the seasons where you find benthic species, did you also find altered assemblages in planktonic foraminifera (also change in size and/or not well preserved planktonic foraminifera)? I think that this is important to verify the reliability of the samples containing benthic foraminifera. The sediment trap is located ca. 400 meter deep and I can image that the suspended foraminifera could be related to strong LIW activity. Is it possible?

R2-R5: Reviewer 2 asks an interesting question. When we decided to discuss the presence of benthic foraminifera as a possible indicator of resuspended material, we focused on the population variability. Overall, the two species described in the manuscript dominate the benthic assemblage. We also identified some *Uvigerina mediterranea* and *Lagenidae* type individuals (infaunal and epifaunal genus and species), although the number of the latter were much lower. This is also specified in **lines 509-511.** Overall, benthic foraminifera mainly appeared during early April and early June, specifically in 4 samples (see Supplementary material), overall, around 3723 planktonic foraminifera individuals were identified against only 141 benthic individuals.

Concerning the preservation state of the planktonic foraminifera in the samples that contain a relatively high amount of benthic individuals, they were well preserved, in the same state as the remaining samples. Despite not having measured the dimensions of the foraminifera, visually, the deep dwellers (*G. inflata* and *G. truncatulinoides*) seemed a little bigger. However, in those samples, we identified a higher amount of detritic material: mica flakes and framboidal pyrite. Those samples also contained a higher amount of *G. bulloides*, which is supposed to be the dominant species in the seabed sediment and to come from the MAW influence. As the questions mentions, the influence of the LIW during this period, which starts to affect the Channel in April but grows in intensity during summer, could be a factor affecting the distribution of benthic foraminifera. However, as the amount of benthic foraminifera presents some variability and is maximum in three samples that do not belong to the period of maximum LIW intensity (summer to early autumn) and neither to the maximum intensity of the MAW (winter), we do not consider their presence nor relative abundance as a reliable proxy for MAW/LIW intensity. We then interpret this increase of benthic foraminifera as a punctual increase of the current speed in the Sicily Channel.

We included this discussion in **chapter 5.1. lines: 515-525**.

R2-C6: ***Chapter 5.4:***

I have several doubts concerning the possibility to compare the sediment trap data with information from coretops from MARGO database. The age of these coretops is strongly different from the analysed sediment trap short time series. In fact, the authors reported that in seabed sediments *Globigerina bulloides* represents the main species.

Data on planktonic foraminifera (Margaritelli PhD thesis, 2016, Perugia, Italy) from a special gravity core system SW104, that allows the recovery of undisturbed and well-

preserved water–sediment interface, show that the main species over the last 100 century (according to radionuclides chronology published in Margaritelli et al. 2020) is first *Globorotalia inflata* followed by *Glibogerinoides ruber* white variety, while *G. bulloides* represents the third abundance species. This discrepancy with coretops MARGO data is mainly related to the low resolution chronology of the coretops due to the missing of radionuclides ages (in my opinion). It means that you compare present day data with a mean signal over the latest part of the Holocene.
Incarbona et al 2019, analysed the planktonic foraminifera over the last four centuries and it is evident that the Sicily Channel is a complex system (from west to the east part). Anyway, the analyzed site (Site 342) in Incarbona et al. (2019) shows over the last century, high abundance values of G. ruber, G. bulloides and last G. inflata, conversely, Site 407 shows high abundance of G. inflata, G. ruber and last G. bulloides. The chronology of these sites is based on radionuclides ages and these data seem to support that the comparison with MARGO database could be questionable. Did you consider also the sites published in Incarbona et al 2019 (Paleoceanography and Paleoclimatology https://doi.org/10.1029/2018PA003529)?
I am reporting these data to stress the fact that the coretops database is useful to reconstruct glacial/interglacial sea surface temperature but, in my opinion, it is very difficult to use this database when comparing with sediment trap record. If the coretops data have a strong chronological control, obviously the comparison could be possible. Is the chronology of the MARGO coretops based con radionuclide ages?
The proposed interpretation concerning the discrepancy between sediment trap data and MARGO database could work but I am convincing that the comparison is questionable due to the different chronologies of the compared records.
General questions:
In Mallo et al. (2017) the authors reported that the most common size fraction of living planktonic foraminifera from bongo net is 150–350 µm and in the Sicily Channel they found the highest percentages of > 500 µm tests. In the submitted manuscript did you find the same size intervals from sediment trap record? And if no, could you suggest an explanation on this issue?
Concerning the size fraction used for the analysis, >150 micron, useful also related to MARGO database, it is clearly evident that according to this choice you did not consider in planktonic assemblage most of T. quinqueloba, G. glutinata and N. incompta. Did you try to consider the impact of this choice on the planktonic foraminiferal diversity, mainly related to productivity signal?
R2-R6:
Authors acknowledge the lack of dating control on the seabed sediment used in the previous version of the manuscript. Authors also agree with the bibliographic input from reviewer 2, it was very helpful to redesign **chapter 5.5** (ex chapter 5.4) To make the latter more complete, the data from Incarbona et al., (2019) has been added to the discussion and the work from Mallo et al., (2017) has been used as a comparison work for our time-series. Then, **chapters 3.5, 3.6 and 5.5** have been modified accordingly. Overall, we have discussed the differences between the seabed sediments datasets as the result of both oceanographical and environmental changes.

Figure 7 now features the data from Mallo et al., (2017) and from the two box-core sites studied in Incarbona et al., (2019). Also, one MARGO site was removed from the figure, the most different one.
Also, note that all the discussion around the MARGO database has been reduced and moved at the end of **chapter 5.5., lines 994-1019.** Also, note that **introduction** and **conclusions** have been modified accordingly**, lines 43-45 and 1058-1060.**
Concerning the question about Mallo et al., (2017), specifically, the most abundant size they found was 350-500 micron followed by >500 micron. This is likely due to the fact that the main species they identified were *G. inflata* and *O. universa*, which are considered "big-sized" species. In our samples, the sizes distributions were similar, most of the species were in the vicinity of the 500um. Also likely because *G. inflata* and *G. truncatulinoides* dominated the samples.
Concerning the size selected for the analysis, we choose 150 microns for the sake of comparison between the different sites both in the Mediterranean, but also with other time-series and seabed sediment across the world as it is the most widely used size-fraction for planktonic foraminifera. However, we acknowledge the point raised buy reviewer 2, *T. quinqueloba* and *N. incompta* (amongst others), are small-sized species. According to Chernihovsky et al., (2023), most of the *T. quinqueloba*, *G. tenella* and *G. rubescens* individuals are in the 120-135micron range. As we feel this is an important point, it is now included in **section 3.3, lines 227-231.**

R2-C7: Please consider to use the term Sicily Channel and not Strait
R2-R7: Authors agree to use the term Channel. The whole manuscript has been updated. Find some examples in **Lines 148-167-525-654.** Note that for readability, not all the "Channels" have been coloured in blue.

R2-C8: Fig. 5: I think that for the reader it is much clearer if close to the terms Tropical/Subtropical, Temperate/Subtropical and Deep dwellers the authors report also the term group1, 2 and 3, as in the text of the manuscript.
R2-R8: Authors agree. Figure 5 now also displays the groups mentioned in the manuscript.