Reviewer 1

This is an interesting study which includes an unusually large number of tracers and biomarkers in order to trace the origins of organic matter in shelf and canyon sediments.

The main weaknesses are the limited number of core samples obtained across the Gulf, and the lack of data on the riverine sources of organic matter. As a result, the conclusions are overstated in places.

We agree with the reviewer that the limited number of core samples in the Gulf of Palermo included in this study limits the conclusions we can derive from them. Nonetheless, we are convinced that the use of a very ample array of proxies for organic matter quantity and origin compensates, at least in part, for this limited number of spatial data. Moreover, to further support our contentions, we have justified our interpretation based on other studies carried out in the area under scrutiny in our study. Nevertheless, conscious of the limited spatial resolution of the data, we have toned down the conclusions, replacing many affirmative statements with cautionary verbs, like “may” or “could”, making manifest the uncertainties of our findings.

Line 100: delete “scarce,”

Done

Page 4, line 2: “biomarkers and other sediment organic matter parameters” (or similar – but not just biomarkers)

We have changed “multiple biomarkers” to “multiple sedimentological and geochemical parameters” (lines 106-107 in the revised manuscript).

Line 119: so these two rivers discharge the same amount of water, on average? What about sediment loads? The latter would be more relevant than water discharges. And what about other “distal sources” which could contribute to the canyons – what might they be (other rivers further upstream etc..)

Unfortunately, no data of sediment yield is available for these rivers, nor for the majority of Italian rivers (Billi and Fazzini, 2017). We agree that this information would be more valuable in the context of distribution of terrigenous OM at sea rather than river discharge, and more work should be done focusing on the role of rivers in the land-ocean continuum.

We can not be certain about the “distal sources” that transport terrigenous OM to the Gulf, and given the lack of sampling of different rivers in Sicily (in this or other studies), we can not conclude which are these distal sources of terrigenous OM. In the text, we merely hypothesize that these distal sources could be either rivers further upstream, such as those that discharge in the adjacent Gulf of Castellammare or input from aerosol. The revised manuscript now reads (lines 529-531):

> “In contrast, terrigenous OC reaching Arenella Canyon would originate from a different source farther up-current from the Gulf of Palermo (e.g., distal rivers such as those that discharge into the adjacent Gulf of Castellammare, aeolian input, or coastal erosion).”

Line 145-155: First you write that 7 cores were collected, but later you write that triplicate cores were collected from one site in each canyon (500m) – please clarify.

We can see why this can be confusing to the reader. To clarify the sampling strategy, as well as provide complete information of the sampled sediment cores, we have added this information in Table S1 in the Supplementary Information:

Core	Location	Coordinates		Depth (m)	Sampling date	Number of cores
		Latitude (N)	Longitude (E)			
S-70	Inner shelf	38.1248	13.4018	72	15/08/2016	1
AC-500	Mid-Arenella Canyon	38.1949	13.4090	544	16/08/2016	3
OC-200	Upper Oreto Canyon	38.1509	13.4140	223	15/08/2016	1
OC-500	Mid-Oreto Canyon	38.1754	13.4281	574	10/08/2016	3
OC-800	Lower Oreto Canyon	38.1864	13.4357	770	15/08/2016	1
EC-200	Upper Eleuterio Canyon	38.1347	13.4978	242	08/08/2016	1
EC-500	Mid-Eleuterio Canyon	38.1471	13.4924	518	08/08/2019	3

Results section:

The descriptions of the data are a bit too long in my opinion. It should be possible to shorten by sticking to the main findings. All the detailed data needs to be shown in a table. The yellow to purple colour ramp used in the figures is not the easiest to interpret.

We do not agree with the reviewer. We believe that the description of the data in the Results section is the adequate length, considering the very ample number of different variables included in the study. All the data is given in a Supplementary dataset (Paradis, 2025).

A *viridis* color scheme (dark blue to yellow) was used in this manuscript since it allows perceptual uniformity of the colors, it is colorblind-friendly, and can be easily interpreted when printed on grayscale. However, we have now opted for the *batlow* color scheme which is universally good for people with tritanopia, deuteranopia, protanopia and for color-blind. This color scheme also better highlights small variations in the dataset, allowing the reader to better understand the data (Crameri et al., 2020). See example of the new figures below.

Also, there are no Results on the mixing models – this should be included here, not in Discussion.

We have added a short description of the mixing models in the results section as well as a figure of the output of the different mixing models in the Supplementary information (Fig. S4-S5). Please see lines 354-369 in the revised manuscript:

> "3.3 Source allocation through mixing models
>
> Considering the range of values of terrigenous and marine end-members, there is a general shift of $\delta^{13}C$ composition from more terrigenous to more marine with depth (Fig. S4). However, when combining the $\delta^{13}C$ values with OC/TN and $\delta^{15}N$, the trend is not that clear. Nevertheless, the fraction of terrigenous OC provided by the Bayesian Markov-Chain Monte-Carlo mixing model in one dimension with only $\delta^{13}C$, as well as in two-dimensional mixing models of $\delta^{13}C$ coupled with OC/TN, $\delta^{15}N$, or $\Delta^{14}C$ showed a general offshore decrease from 80 to 20-40%, depending on the model (Fig. S5).
>
> The spatial variations of the source apportionment were very similar between the one-dimensional mixing model and the two-dimensional mixing model with OC/TN and $\delta^{15}N$, although these models provided highest uncertainties (Fig. S5). In these three models, the

offshore decrease of the terrigenous OC fraction was interrupted by a sudden drop in Oreto Canyon at 200 m (OC-200), which presented minimum terrigenous fraction (15-19%). This low terrigenous fraction presents a stark contrast to the terrigenous OC fractions (48-60%) in sediment cores collected further downcanyon at 500 m (OC-500) and 800 m (OC-800). The sediment core collected in Arenella Canyon (AC-500) also presented a similarly low terrigenous OC fraction of 20%.

In the dual end-member mixing model with $\delta^{13}C$ and $\Delta^{14}C$ (Fig. S5), both OC-200 and AC-500 also presented the lowest terrigenous fraction, but only of 47-55% in comparison to the lowest terrigenous OC fraction of 15-19% presented by the other models."

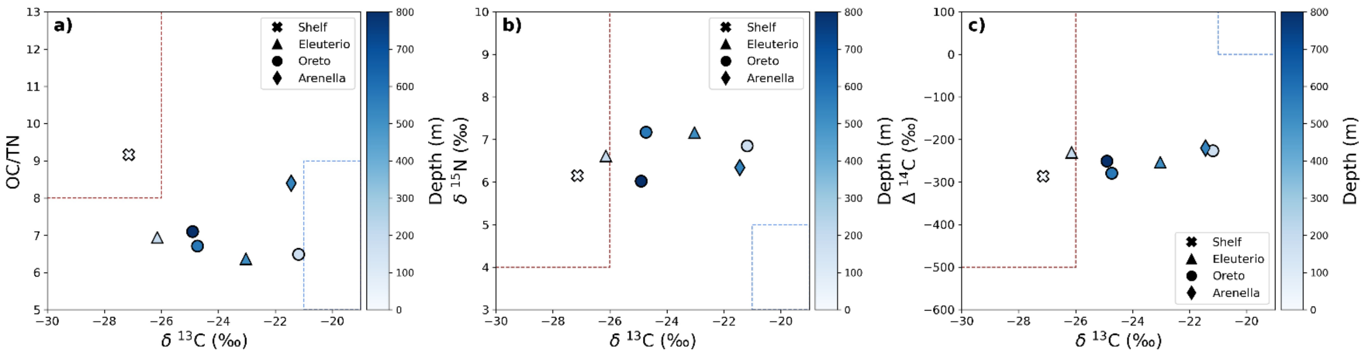

Figure S4. Scatter plots of OC/TN and $\delta^{13}C$ (a), $\delta^{15}N$ and $\delta^{13}C$ (b), $\Delta^{14}C$ and $\delta^{13}C$ (d) used for the two-dimension mixing models, showcasing the values of terrestrial (brown dashed lines) and marine (blue dashed lines) endmembers. See Table 1 for the values and sources of the different endmember values.

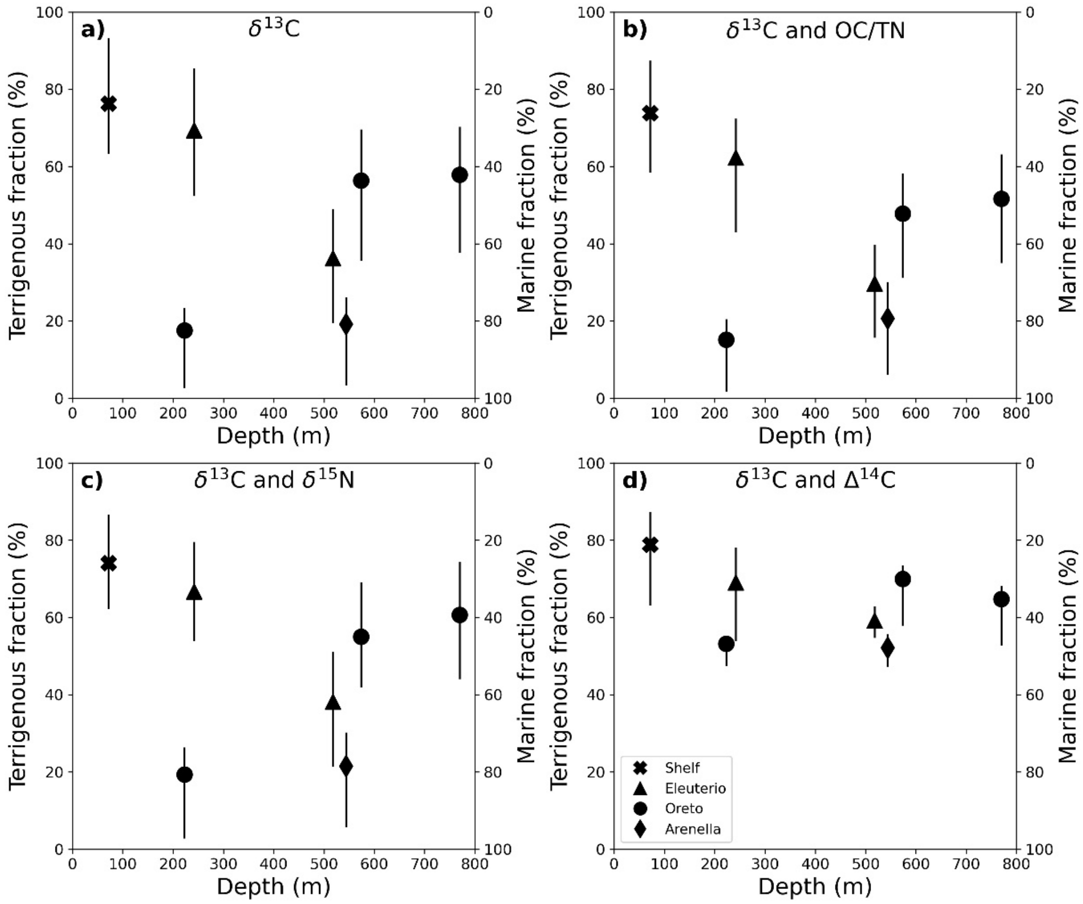

Figure S5. Terrigenous and marine OC fraction with depth obtained from a one-dimensional mixing model with $\delta^{13}C$ (a), two-dimensional mixing model with $\delta^{13}C$ and OC/TN (b), $\delta^{13}C$ and $\delta^{15}N$ (c), $\delta^{13}C$ and $\Delta^{14}C$ (d).

Discussion:

Sediment accumulation rates should be shown in the Results section first. And included in the Methods too.

Sediment accumulation rates have been extensively described and discussed by Paradis et al. (2021). In addition, since sedimentation rates are not the main point of discussion in this manuscript and they are only included in a figure to show the variation of sedimentation rates in the Gulf. Moreover, to provide the reader with a greater understanding of the variation of sedimentation rates in the Gulf, we have also added data reported in other sites. See new figure below.

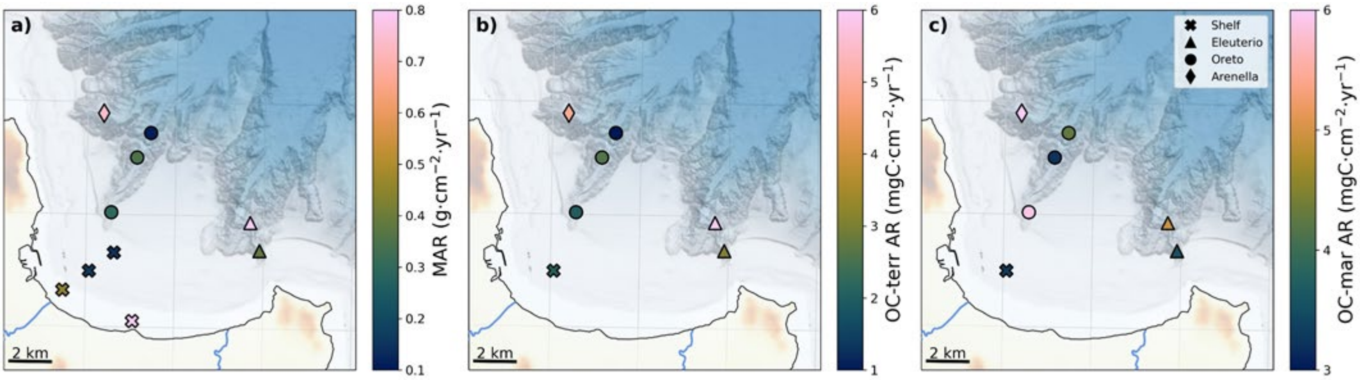

Figure 7. Spatial distribution of: a) mass accumulation rate (MAR) of the studied sediment cores (Paradis et al., 2021) and additional sediment cores from the shelf (Di Leonardo et al., 2007, 2012; Rizzo et al., 2009), b) terrigenous OC accumulation rate (OC-terr AR), and c) marine OC accumulation rate (OC-mar AR). Colour bars are adjusted to highlight the minimum, mean, and maximum values for each variable.

Line 433: what is BIT index again? Need to remind reader.

A short description of the BIT index is included in the revised manuscript as follows (lines 487-488 in the revised manuscript):

> "The BIT index, the ratio of brGDGTs and isoGDGTs, is often employed as a proxy of soil-derived terrigenous contribution, where a high BIT index (> 0.6) is indicative of high input of soil-derived OM (Hopmans et al., 2004; Weijers et al., 2014)."

The authors should be careful when stating their conclusion – after all, they are based on only 1, 2 or 3 core samples, which may not be representative of the entire canyons. This shortcoming needs to be acknowledged and the language used more careful.

We agree that the few samples collected in this study limits the assertiveness of our conclusions. On the one hand, to further support our work, we have justified our interpretation based on other studies in the area. However, we acknowledge this does not overcome the main limitation of this study, which is the number of sites in the Gulf of Palermo. Accordingly, we have also toned down the conclusions of our study, employing words like "may", "could" or "would indicate" to manifest the uncertainties of our findings.

Line 480: This pattern is consistent…

We have kept the original wording, since we already say at the beginning of the paragraph that the spatial distribution of weighted-average $\delta^{13}C$ values of HMW FAs is consistent with the eastward direction of the regional current.

Line 485: need more info on distal sources etc.. see earlier comment

As the reviewer correctly states in several points, some of the interpretation is too speculative. This is one of the cases. Given the spatial distribution of the weighted-average $\delta^{13}C$ values of HMW FAs and the current direction, we can infer that the $\delta^{13}C$ signature of HMW FA discharged by the Oreto and Eleuterio rivers are similar (-29 ‰). Since the signature of Arenella Canyon, located further upcurrent, is rather different, we can only hypothesize that the terrigenous OC, in terms of HMW FA, comes from a different source, but we can not be certain of the sources. Hence, we can not elaborate further about which would be the distal sources.

Line 495: remind us what CPI is?

A short description was added in lines 543-547 in the revised manuscript:

> "In addition, the spatial variation in $CPI_{(C24-C32)}$ values of HMW FA across canyons (Fig. S3f), a metric of the degree of degradation of plant-derived OC, points to more degraded HMW FAs deposited in Arenella Canyon, slightly less degraded HMW FAs deposited in Oreto Canyon, and least degraded HMW FAs deposited in Eleuterio Canyon, consistent with the transit of terrigenous OC through the system (Fig. 8)"

Line 476-500: I think there is a bit of a jump between what the data show and the conclusions about riverine sources. This should be provided as a hypothesis rather than a firm conclusion. Temper the language and acknowledge that there are weaknesses in your study design and that other processes may be at play (such as x or y).

As stated before, we agree that with the data we present, we can not be this assertive about the conclusions of our findings, which is why we, according to the reviewer correct suggestion, have toned down the discussion. These are preliminary insights supported by our data and extensive studies in the region, but we are aware that further investigation is needed, which we have also included in this section of the manuscript. Here is a brief example of the smoothened text in the revised manuscript (lines 534-539):

> "Using the weighted-average $\delta^{13}C$ signature of HMW FAs from the shelf and Arenella Canyon as possible end-members of local riverine and distal terrigenous OC source, respectively, only 30 to 40 % of the terrigenous OC delivered by the Oreto River would be deposited in the Oreto Canyon, whereas the majority would originate from another up-current source that this canyon intercepts (Fig. 8). However, additional sampling should be conducted to further refine the endmembers used in this study area (Table 1) and provide more definite understanding of the dispersal of terrigenous and marine OC in the Gulf of Palermo, such as sampling the different rivers and collecting suspended particles to determine marine OC signatures."

Figure 8: use same orientation as Figure 1.

Although we devised Figure 8 as a more artistic illustration of the sediment dispersal mechanism, we modified the figure to keep the same layout as the other figures in the manuscript. We believe that this way, the reader will be able to quickly interpret the message. Here is the modified Figure 8:

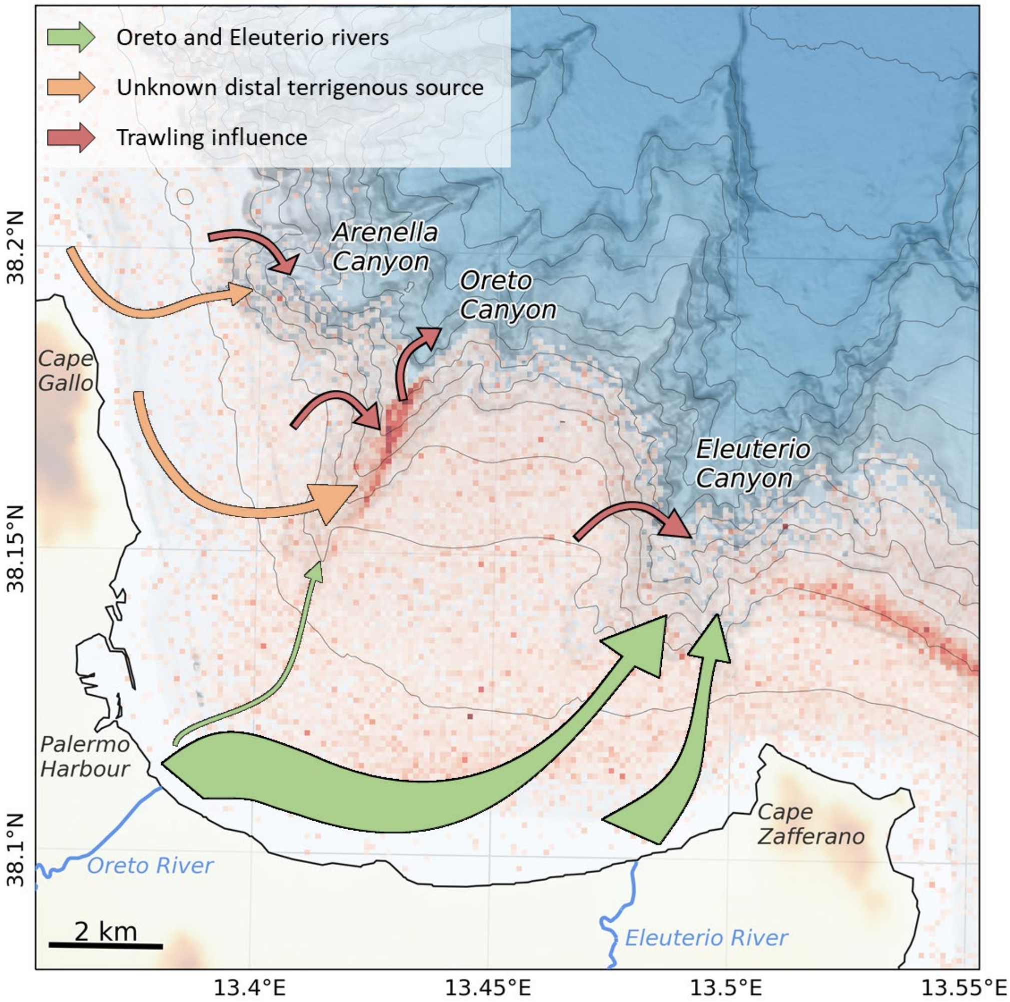

Figure 8. Schematic diagram of the dispersal pathways of terrigenous OM across the shelf of the Gulf of Palermo and its submarine canyons. The colour of the arrows indicates distinct terrigenous OM sources (Oreto and Eleuterio rivers, or unknown distal terrigenous source) or dispersal mechanism (trawling influence) whereas the size represents the magnitude of the terrigenous OM transported.

We have also included bottom trawling as a potential dispersal mechanism, since Reviewer 4 rightfully pointed out the influence of this anthropogenic activity.

Page 21 first sentence: “dire consequences” too strong language.

We modified this to “could impair ecosystem functioning”.

Reviewer 2

Synopsis

Paradis et al. presented a comprehensive study containing geochemical parameters (OC, TN, δ13C, δ15N, and Δ14C), biomarker signatures (proteins, carbohydrates, phytopigments, GDGTs, and n-alkyl lipids), and compound-specific δ13C analyses of surface sediments to assess the sources of OC deposited on the shelf and in the three major canyons in the Mediterranean Sea. A particularly interesting aspect of this study is the use of a wide of source-assignment methods to investigate the role of submarine canyons in transporting terrigenous OC across continental margins. However, due to the limited number of samples (total n=7), any observed differences should be interpreted with caution. While I have no major concerns, I offer several suggestions that could help improve the manuscript.

We would like to thank the reviewer for his/her time in reading the manuscript and providing these constructive suggestions to help improve the quality of the manuscript.

My specific comments are outlined below.

Line 16: add ", and" before "Δ14C".

Done

Lines 19-21: please specify which method was used to assess the contribution.

This has been added to the abstract, which now reads (lines 19-21 in the revised manuscript):

> "According to a dual isotopic end-member mixing model with $\delta^{13}C$ and $\Delta^{14}C$, the contribution of terrigenous OC was highest on the shelf (80 %) and decreased offshore, with contributions that ranged between 50 to 70 % across the studied canyons."

Line 23: It would be better to add "relatively" before "lowest".

We agree with the reviewer, and this has been added.

Line 30: Are you referring to the Arenella and Oreto Canyons, which are not connected to rivers? If so, could you explain the potential sources of terrigenous OC into these two canyons?

We are actually referring to all three submarine canyons, which neither of them are connected to rivers. We have clarified this with the following sentence (lines 31-34 in the revised manuscript):

> "This study provides further evidence that even non-river connected submarine canyons, such as Arenella, Oreto, and Eleuterio canyons in the Gulf of Palermo, are important sites of terrigenous OC sequestration and transfer to deep-sea environments, and that bottom trawling activities within submarine canyon environments can contribute to its resuspension and dispersal towards deeper regions."

Lines 48-49: What about differences in marine primary production, which also influence the relative proportion of terrigenous OC.

The reviewer is right in pointing that we focused the introduction on the dispersal of terrigenous OC but did not address the dispersal of marine primary production, which also influence in the accumulation of OC and the relative proportion of terrigenous OC. We have included this in the revised manuscript (lines 51-55) as follows:

> "Higher marine OC tend to accumulate in submarine canyons incising continental margins with high marine primary productivity (Pusceddu et al., 2010), whereas the proportion of terrigenous OC in submarine canyons can be very variable depending on the proximity of riverine sources, their suspended sediment yield, and the magnitude of littoral and along-margin transport (Alt-Epping et al., 2007; Pasqual et al., 2013; Kao et al., 2014; Romero-Romero et al., 2016; Prouty et al., 2017; Gibbs et al., 2020)."

Lines 72-73: distinguish between specific sub-pools of terrigenous OC (e.g., vegetation, soils, and fossil OC).

This has been modified.

Lines 157-159: What is the carbonate content? Could it affect the mean grain size of the terrigenous sediments?

Carbonate content in surficial sediment samples range between 17 to 33 %, and it is correlated to the mean grain size of sediments as explained by Palanques et al. (2022).

Line 216: the CPI index formulae is wrong, please correct it.

We appreciate the reviewer's sight in identifying the typo in the formula, which has now been corrected. Since there was only a typo in the written formula, this does not affect the CPI values provided in the manuscript.

Line 224: rewrite the δ13CCH3 to δ13CMeOH and consider adding a sentence explaining how the δ13C value of HMW compounds (n>24) was calculated.

This has been modified and a short explanation of how the weighted-average $\delta^{13}C$ of HMW FA was calculated has also been included (lines 236-238) in the revised manuscript):

> "Since $\delta^{13}C$ of the FAME fractions could only be measured on specific carbon chains (C_{16}-C_{28}), weighted-average $\delta^{13}C$ values of HMW FA were calculated as follows:

$$\delta^{13}C_{HMW\ FA} = \frac{\sum(\delta^{13}C_{24-28} * C_{24-28})}{\sum(C_{24}-C_{28})}\text{"}$$

Table 1: please also include the reference for the marine end-member values.

The reference of both the terrigenous and marine end-member values are included in the table. The marine end-member values refer to phytoplankton analyses in suspended sediments from the Gulf of Lions (Harmelin-Vivien et al., 2008).

Lines 275-293: What about the relative proportions of these OC classes based on OC content rather than sediment mass? They could provide insight into the reactivity of OC. Or is it already normalized to OC content?

We initially showed the contents of proteins, carbohydrates, lipids and phytopigments in terms of sediment ($mgC \cdot g^{-1}$) since this is how these parameters are often reported (Dell'Anno et al., 2002; Pusceddu et al., 2005; Moccia et al., 2019; Paradis et al., 2019). However, in light with the other biomarkers presented in this study, we normalized the contents if these compound classes on the OC content. Despite the variable OC content across the sites, this normalization did not modify the spatial patterns.

With regard to the reactivity of OC, we actually measured protein turnover rates, and the data was shown in the supplementary appendix (Paradis, 2025), but not discussed in the first manuscript to simplify the message. Since this is a topic that Reviewer 4 also pointed out, we have included it in the revised manuscript and in a figure of the supplementary information.

Below is the description of the turnover rates in the Results section (lines 306-311 of the revised manuscript), along with the modified Fig. S2:

> "Protein turnover rates in selected sediment samples showed a general increase with depth and distance from shore (Fig. S2c). Lowest turnover rates were measured in Eleuterio canyon head (EC-200, 0.85 ± 0.20 yr^{-1}) which increased with depth (EC-500, 4.69 ± 0.99 yr^{-1}). In Oreto Canyon, turnover rates also increased downcanyon where highest turnover rates were observed in the canyon mouth (OC-500, 2.52 ± 0.54 yr^{-1}; OC-800, 17.5 ± 1.8 yr^{-1}). Finally, Arenella mid-canyon had the highest turnover rates in comparison to the other sediment samples located at a similar depth-range (AC-500, 12.1 ± 2.7 yr^{-1}).3.3 Lipid biomarkers"

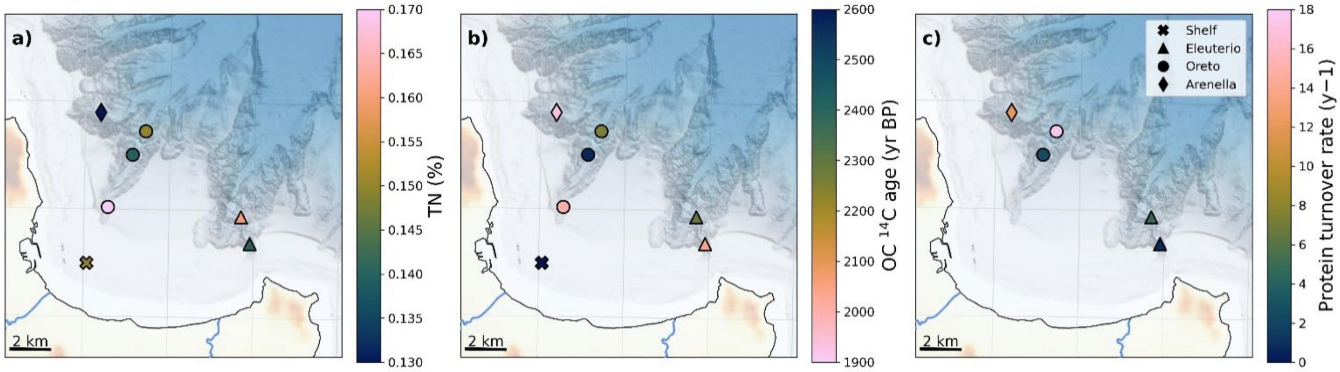

Figure S2. Spatial distribution of bulk parameters: a) TN, b) radiocarbon age, and c) protein turnover rate. Colour bars are adjusted to highlight the minimum, mean, and maximum values for each variable.

The importance of this variation in protein turnover rate, as a proxy for OM reactivity, has also been included in section 4.1 of the discussion (lines 415-419) in the revised manuscript):

> "Interestingly, the spatial distribution of OC sources is somewhat related to its reactivity, estimated as protein turnover rates (e.g., Soru et al., 2022, 2024). Highest protein turnover rates were observed in Arenella mid-canyon (AC-500), which had one of the highest marine OC fractions, whereas lowest protein turnover rates were observed in Eleuterio canyon head (EC-200), which had one of the highest terrigenous OC fractions (Fig. S2c). However, we pinpoint here that other factors may also be contributing to the reactivity of OC (see section 4.3)."

As well as in section 4.3 (lines 569-578 in the revised manuscript):

> "Furthermore, the continuous sediment resuspension and erosion at this site due to repetitive bottom trawling promotes a reduction of OC contents in surficial sediment (Tiano et al., 2024), either associated to erosion or degradation of OC. Given the high sedimentation rates in this site (Paradis et al., 2021), the reduction of OC associated to bottom trawling in this site may be dominated by enhanced degradation of OC, potentially due to sediment mixing (e.g., (Middelburg, 2018)) and oxygenation (e.g., increasing oxygen exposure time of OC (Hartnett et al., 1998))depleting the most reactive OM components such as phytopigments from the seafloor (Fig. 3d). This process shifts the OC

source toward less marine and more terrigenous OC, which tend to be less reactive, as seen by the low protein turnover rate in this site (Figs. 3d, S2c). This process leads to older (i.e., more ^{14}C-depleted) and less reactive OC on surface sediments, which could impair ecosystem functioning (Danovaro et al., 2008) in this area, ultimately affecting benthic community composition and abundance (Pusceddu et al., 2014; Good et al., 2022)."

Line 299: When discussing the concentration of each GDGT, please remember the data are semi-quantitative. It would be more appropriate to present relative abundances rather the absolute.

Yes, which is why we provided measures of the relative abundance of crenarchaeol concentrations in comparison to all isoGDGTs. Moreover, we compare concentrations among samples, as a relative measure of where we find higher/lower concentrations of each measured compound.

Figure 4: Specify if HMW FA include all compounds with C≥24 or only even-numbered ones?

In addition to specifying that the HMW FA concentrations only include even C chains, we also remind the reader about the ranges of C number chains for both LMW and HMW FA in the figure caption:

"Figure 4. Spatial distribution of concentrations of a) crenarchaeol, b) LMW FA $(C16\text{-}C18)_{even}$, c) HMW FA $(C24\text{-}C32)_{even}$, and d) spatial distribution of $\delta^{13}C$ signature of HMW FA $(C \geq 24)_{even}$. Colour bars are adjusted to highlight the minimum, mean, and maximum values for each variable."

Line 320: The CPI value is quite low, could please double-check the calculation? Even in highly degraded, sandy sediments, CPI values typically ranged between 4 and 5.5 (See Wei et al. (2025), https://doi.org/10.1016/j.chemgeo.2025.122712)

Yes, the CPI value in our dataset is quite low, which has prompted to revising the calculation several times, but the values obtained are correct. We appreciate the reviewer for providing us with an additional study site to compare our CPI values to.

A closer comparison of the high CPI values reported in the North Sea (4-5.5; Wei et al., 2025), the East China Sea (3.4-4.7; Tao et al., 2016), and in the Laptev Sea (3.7-5.9; Bröder et al., 2016), also coexist with relatively high HMW FA contents in the North Sea (173–1150 µg/g OC; Wei et al., 2025), the East China Sea (400-900 µg/g OC; Tao et al., 2016), and in the Laptev Sea (300-7200 µg/g OC; Bröder et al., 2016). This suggests that the higher HMW FA contents in these margins may be related to a better preservation of plant-derived OC, whereas the limited plant-derived OC (18-35 µg/g OC) that is exported to the Gulf of Palermo are inherently highly degraded ($CPI_{(C24\text{-}C32)}$ 1.8-2.4).

Also note that the Helgoland Mud Area of the North Sea is located relatively close to the Elbe and Weser rivers, which have an average discharge of 760 $m^3 \cdot s^{-1}$ and 350 $m^3 \cdot s^{-1}$, respectively, which is 1000-3000 times higher than the discharge of the Oreto and Eleuterio rivers. Note that we are only comparing river discharge and not suspended sediment yield of these rivers, since the latter metric is not available for the Oreto and Eleuterio rivers. This contrasting river discharge could have a substantial influence in the preservation and degree of degradation of land-derived plants in these systems, since the majority of Mediterranean rivers are ephemeral torrential rivers.

All of this has been included in lines 547-552 of the revised manuscript as follows:

"It is important to note that these $CPI_{(C24\text{-}C32)}$ values (1.8-2.4) are lower than those observed in other continental margins with significant riverine input as well as high HMW FA contents in surficial sediments, such as East China Sea (400-900 $\mu g \cdot g^{-1}$ OC, $CPI_{(C24\text{-}C32)}$ 3.4-4.7; Tao et al., 2016), the Laptev Sea (300-7150 $\mu g \cdot g^{-1}$ OC, $CPI_{(C24\text{-}C32)}$ 3.7-5.9; Bröder et al., 2016), and the Helgoland Mud Area in the North Sea (170-1150 $\mu g \cdot g^{-1}$ OC, $CPI_{(C24\text{-}C32)}$ 4-

> 5.5; Wei et al., 2025), indicating that the limited plant-derived OM deposited in the Gulf of Palermo is already considerably degraded, which could be a characteristic of continental margins affected by ephemeral, torrential rivers."

Line 484: Are there any specific rivers?

Given the direction of the regional current, possible rivers that could provide terrigenous OC into the Gulf of Palermo are the small torrential rivers that discharge in the adjacent Gulf of Castellammare, such as the Nocella River and San Bartolomeo River. Also per request of Reviewer 1, we added these possible sources in the text (lines 530-531 of the revised manuscript):

> "[…] (e.g., distal rivers such as those that discharge into the adjacent Gulf of Castellammare, aeolian input, or coastal erosion)."

Reviewer 3

Paradis and co-workers have investigated the distribution and sources of organic matter in sediments from submarine canyons in the Gulf of Palermo, using a combination of bulk sediment properties, a suite of biomarkers, and (compound-specific) stable and radiocarbon isotopes. They find that surficial sediments in the canyons contain between 50-70% terrestrial OC, but that the source of this OC differs between canyons, where some canyons receive OC discharged by nearby rivers, and others receive OC from up-current sources. They also find that bottom trawling has a negative influence on the amount of OC preserved and promotes down-canyon transport.

The manuscript is well written, and the data that is presented is sound. The main remark I have on this work is the limited number of samples (n=7) and the lack of samples from local soils, river(bank) sediments, and marine algae or SPM to serve as endmembers in their calculations to determine contributions of terrestrial OC to submarine canyon sediments. Endmember values are now derived from the literature, and even though the authors used Monte Carlo simulation to compensate for this in their mixing models, the endmember assumptions still introduce uncertainty in the terrestrial OC estimations, and thus the importance on global scale carbon cycling.

We agree with the reviewer that one of the main limitations of our study is the limited number of sampled locations (7) and the lack of sampling of the local endmembers such as terrigenous sources (Oreto and Eleuterio rivers) and marine sources (marine algae or suspended particulate matter). To compensate this, we searched in the literature for the most appropriate end-member values based on their location and similar climate, soil type, and riverine hydrogeomorphology, leading to marine end-member values from the Gulf of Lions (Harmelin-Vivien et al., 2008), as well as terrestrial end-members from Catalan rivers (Sanchez-Vidal et al., 2013) and Sicily soils (Lawrence et al., 2020). We believe that these end-members are sufficiently, although not exhaustively, representative of the isotopic composition of OC in the study area.

Despite such a limitation, our approach is a substantial improvement to previous studies who take global average end-member values rather than local values (e.g., Di Leonardo et al., 2009, 2012; Pedrosa-Pàmies et al., 2013).

In addition, the relatively low number of samples limits comparison of terrestrial OC contributions to sediments inside and outside the canyons to assess e.g., sediment focusing, carbon sequestrations, or verifying dispersal patterns and tracing back OC courses.

We agree that the canyon-centric sampling strategy limits the conclusion of the distribution of OC across the Gulf, and we can only refer to the dispersal of OC in each canyon. However, comparison of heavy metal concentration inside and outside the canyons point to the sediment focusing capacity of these submarine canyons (Palanques et al., 2022), whereas the compilation of more data of sedimentation rates in the Gulf point to a general decrease in sedimentation rate with distance from shore only on the shelf, whereas these sedimentation rates then increase again in submarine canyons due to their funneling capacity.

In addition, comparison of $\delta^{13}C$, OC/TN, and $\delta^{15}N$ isotopic composition of surficial sediment in the submarine canyons with an adjacent surficial sediment sample from the open slope (Di Leonardo et al., 2009) actually reveals that the contribution of terrigenous OC inside the canyon is higher, pointing to the preferential transport of terrigenous OC into submarine canyons.

This has been added in the revised manuscript in section “4.1 Contribution of terrigenous and marine organic carbon in the Gulf of Palermo” (lines 439--454 of the revised manuscript) as follows:

"Although no data of mass accumulation rate is available from the continental slope, the rapidly decreasing accumulation rates on the shelf with distance from shore (0.84 to 0.15 $g \cdot cm^{-2} \cdot yr^{-1}$) to values that are considerably lower than in submarine canyons (0.35-0.82 $g \cdot cm^{-2} \cdot yr^{-1}$) indicates that sedimentation rates on the adjacent slope will be considerably lower than in the canyon axis, as observed in other incised continental margins (Buscail et al., 1997; Sanchez-Cabeza et al., 1999; Masson et al., 2010; Paradis et al., 2018). Moreover, surficial sediment from a sediment core collected in the open slope between Oreto and Eleuterio canyons at 712 m depth (Di Leonardo et al., 2009) did not present any sign of trace metal contamination, whereas sediment cores collected along the canyon axis had significant trace metal contents, indicating a preferential downslope transfer of sediment and pollutants into submarine canyons (Palanques et al., 2022). In fact, this same sediment core on the slope also presented higher $\delta^{13}C$ values (-22.7 ‰; Di Leonardo et al. (2009)), similar to marine end-member values, than those in the afore-mentioned canyons (-24 to -25 ‰), which tend toward more terrigenous end-member values. This further supports the notion that submarine canyons transfer terrigenous OC deeper and farther offshore than would occur in their absence.

The observed high accumulation of both marine and terrigenous OC in these submarine canyons confirms their role as important sites of OC sequestration, as shown in other canyon systems (Masson et al., 2010; Maier et al., 2019; Baudin et al., 2020). However, the contrasting accumulation of terrigenous and marine OC in each canyon suggests that even in closely spaced submarine canyons, the main source of the OC can greatly differ."

Specific comments:

L80: Replace Damsté by Sinninghe Damsté

Done.

L81: note that crenarchaeol is not often the most abundant isoGDGT in marine archaea, as its abundance is temperature dependent. At 'cold' sites, GDGT-0 will be more abundant than crenarchaeol.

We thank the reviewer for this clarification. Hence, we have modified the sentence to be more specific (lines 86-87 of the revised manuscript), as follows:

"BrGDGTs are generally found in soil bacteria, whereas isoGDGTs are common in marine archaea, with crenarchaeol often the most abundant isoGDGT in temperate environments (Sinninghe Damsté et al., 2002)."

L83: the first study to show brGDGT production in marine sediments is Peterse et al., 2009 Organic Geochemistry.

Thank you for this clarification. It has been included in the amended manuscript.

Method section: there is quite some information on contaminants in the study area, but this information is not further used in the discussion. Reconsider its necessity.

The information on contaminants in the study area (Section 2.1) has been removed to focus only on the sediment dispersal mechanisms in the canyon.

L134: This line mentions 'naturally high sedimentation rates'. Can you add numbers to provide context? There are a few instances later in the manuscript where sedimentation rates are mentioned and can use some specification. Please check.

The natural sedimentation rates in the canyon axes have been added as follows (lines 136-138 of the revised manuscript):

> "This configuration facilitates the transport of suspended sediment from the shelf into Eleuterio Canyon, leading to naturally high sedimentation rates (0.52 $cm \cdot yr^{-1}$) along its axis at 200 m in comparison to natural sedimentation rates in Oreto Canyon at the same depth (0.11-0.16 $cm \cdot yr^{-1}$) (Paradis et al., 2021)."

L140: this is one of those locations: what kind of rates resemble 'background sedimentation'?

The trawling-derived sedimentation rates in the canyons have also been added as follows (lines 140-144 of the revised manuscript):

> "The continuous contact of demersal fishing gear with the seafloor has contributed to sediment resuspension and its posterior transfer into these three submarine canyons, causing sedimentation rates to increase by up to an order of magnitude (0.73-1.38 $cm \cdot yr^{-1}$) in comparison to natural (i.e., pre-1980s) background sedimentation (0.11-0.13 $cm \cdot yr^{-1}$) (Paradis et al., 2021; Arjona-Camas et al., 2024)."

L172: …stable isotope RATIO mass spectrometer…

This has been corrected. Thank you for pointing it out.

L204: I appreciate that the authors assess the sources of brGDGTs even though the BIT index values they present are already very low (<0.05), indicating a primarily marine OC source. Note that the IIIa/IIa ratio used here to assess soil vs marine sources of brGDGTs is not supported by any biophysiological mechanism and also contains a temperature component by capturing part of the degree of methylation of brGDGTs.

Since the BIT index is already substantially low, indicating a primarily marine OC source, as the reviewer well points out, we have removed the calculation of the $\#rings_{tetra}$ ratio and the IIIa/IIa ratio, since they don't provide additional information. This way, the text is simplified and is easier to follow.

L206: Please add what the cut-off of 0.21 for #rings based on. The soil endmember for this ratio is generally based on #rings in local soils and likely differs per location/catchment.

As pointed out by the reviewer in the previous comment, the use of these additional proxies has been removed to simply the text. Instead, we simply focus on the comparison of the brGDGTs composition to the global database of soils and peats (Dearing Crampton-Flood et al., 2020).

L392: fraction of marine and terrestrial OC depends on marine primary productivity and export to the seafloor -> I fully agree with that. What can you say about marine production in the study area? After all, the Mediterranean Sea is known of its (ultra)oligotrophic conditions. Do you expect large contributions of marine OC at these sites? Especially in comparison to the other canyons mentioned in the text and possible influence of (high discharge) rivers?

Indeed, the Mediterranean Sea is oligotrophic in comparison to other continental margins, and this also affects the OC quantity and composition accumulating in Mediterranean submarine canyons in comparison to canyons incising other margins (e.g., eutrophic Portuguese canyons). This was further expanded in the revised manuscript (lines 429-433 of the revised manuscript):

> "We acknowledge, though, that the fraction of terrigenous and marine OC deposited in continental margins also depends on marine primary productivity and the consequent flux of marine OC to the seafloor which is lower in the Mediterranean margin in comparison to other continental margins: this leads to the accumulation of lower OC contents in several Mediterranean continental margins in comparison to submarine canyons incising other margins (Pusceddu et al., 2010; Pasqual et al., 2011)."

L397: add numbers to the 'high sedimentation rate' at this site.

This has been added.

L401: I miss some discussion on the possible implications of different OC sources per canyon. Why does this matter?

The purpose of this study is to quantify the source of OC (marine vs. terrigenous) and, using biomarkers, identify the specific sources of marine and terrigenous OC. Finally, with a general understanding of the sedimentary dynamics of this Gulf, we also discuss the dispersal of OC and its transformation. Hence, we have structured the discussion as follows:

4.1 Contribution of terrigenous and marine organic carbon in the Gulf of Palermo

4.2 Sources of terrigenous and marine organic carbon in the Gulf of Palermo

4.3 Dispersal of terrigenous organic carbon in the Gulf of Palermo

Addressing all this is crucial to understand carbon cycling in marine sediments and the role of submarine canyons in accumulating different sources of OC. This latter aspect is essential to be able to distinguish between locally produced OC (marine OC) and allochthonous OC (terrigenous), which is important for carbon accounting purposes. In addition, distinguishing these sources can also serve to understand the preservation potential of OC in marine sediments, since marine OC tends to be more reactive than terrigenous OC. Hence, the latter will be more efficiently preserved.

The importance of OC source and preservation has been included in the manuscript by comparing them to protein turnover rates, a measure of OC reactivity (data previously given in the appendix, but not discussed in the manuscript). The following text has been added to section 4.1 of the discussion (lines 415-419 of the revised manuscript):

> "Interestingly, the spatial distribution of OC sources is somewhat related to its reactivity, estimated as protein turnover rates (e.g., Soru et al., 2022, 2024). Highest protein turnover rates were observed in Arenella mid-canyon (AC-500), which had one of the highest marine OC fractions, whereas lowest protein turnover rates were observed in Eleuterio canyon head (EC-200), which had one of the highest terrigenous OC fractions (Fig. S2c). However, we pinpoint here that other factors may also be contributing to the reactivity of OC (see section 4.3)."

L413: Thaumarchaeota are now named Nitrososphaerota.

This has been modified.

L428: ..age OF OC...

This typo has been fixed.

L435: I think the comparison of BIT index values can be a bit more nuanced. Also, Kim et al and Yedema et al only found high(er) BIT index values directly at the river mouth. After that, BIT index values decrease very rapidly with increasing water depth (e.g., Sparkes et al: www.biogeosciences.net/12/3753/2015). A high(er) BIT index can only be found in coastal regions receiving substantial terrestrial input from rivers, which seems to be relatively limited at the shelf site included in this study.

Indeed, as mentioned in the text, the BIT index of surficial sediment in the Gulf of Palermo is substantially lower than in other continental margins that receive substantial input from rivers, which is why we also limited the discussion of this proxy to the fact that there is limited contribution of soil OC in the system.

L520: Make sure to not overstate the role of canyons in sequestering terrestrial OC when the study does not present data from sediments

To further justify the role of canyons in sequestering terrigenous OC, we now added data of the isotopic composition of the adjacent open slope at a similar distance from shore. This sediment core shows less depleted $\delta^{13}C$ indicative of lower contribution of terrigenous OC and a higher contribution of marine OC in comparison to sediments from the canyons, supporting our interpretation that submarine canyons have a higher capacity of sequestering terrigenous OC than the open slope. Unfortunately, sedimentation rates were not estimated in the open slope, but we can expect them to be substantially lower than in the canyon axes.

Reviewer 4

Paradis and co-workers present organic geochemical data for 7 cores collected by multicorer from 1 shelf site and 3 submarine canyons in the area of the Gulf of Palermo, Sicily. The three investigated canyons differ with respect to morpho-sedimentary characteristics and their catchments. The major aim of the study is to assess the sources of OC deposited on the shelf and in three canyons.

The authors have produced an impressive dataset including geochemical parameters (OC, TN, δ13C, δ15N, Δ14C), biomarker signatures (proteins, carbohydrates, phytopigments, glycerol dialkyl glycerol tetraethers, and n-alkyl lipids), compound-specific δ13C analyses of surficial sediments as well as application of mixing models. However, the spatial coverage based on the relatively few sampling sites is rather low – in particular in the Arenella Canyon area, where there is only one sampling site.

Although the spatial coverage of the study area with sampling sites is rather poor, I think the authors could better exploit their comprehensive data sets available for the studied sites. Furthermore, in some parts of the manuscript the discussion and some related statements are still very vague and general.

We appreciate the reviewer's time reviewing the manuscript and for his/her comments and discussion that help improve the quality of this study. Yes, we agree that the number of sampling sites is rather limited, as pointed out by the other reviewers. To overcome this, we dive into a rich and extensive dataset of different proxies, and complement our dataset with published data in this study area.

The manuscript/study has a strong focus on the downslope transport of terrigenous organic carbon (OC) within the canyons. In this context I would then suggest to elaborate a bit more – maybe in the Introduction - on why it is important to assess how much terrigenous carbon is transported downslope within canyons. What about the OC from marine sources? From my point of view, it would be similarly important to assess how canyons also act as transport routes of marine OC downslope – in particular in light of the observation that the fraction of marine OC increased downslope. It has been shown that sediment transport in canyons is a rapid process and sedimentation rates at the sites of sediment deposition are high. As has been shown (e.g. Müller et al., 2025; Biogeosciences) sedimentation rate exerts one of the key controls on OC preservation. So, I would assume that in such depositional environments also marine OC would be relatively well preserved.

Yes, the reviewer is right in pointing on the importance of the accumulation of marine OC in submarine canyons. This was also raised by Reviewer 2, and we have added a few sentences on this in the introduction (lines 51-58 of the revised manuscript).

> "Higher marine OC tend to accumulate in submarine canyons incising continental margins with high marine primary productivity (Pusceddu et al., 2010), whereas the proportion of terrigenous OC in submarine canyons can be very variable depending on the proximity of riverine sources, their suspended sediment yield, and the magnitude of littoral and along-margin transport (Alt-Epping et al., 2007; Pasqual et al., 2013; Kao et al., 2014; Romero-Romero et al., 2016; Prouty et al., 2017; Gibbs et al., 2020). These contributions can also vary temporally, with enhanced sediment transport and burial of both marine and terrigenous OC triggered by natural energetic events such as storms, or by anthropogenic sediment resuspension and its posterior downcanyon transport caused by mobile demersal fisheries (Pedrosa-Pàmies et al., 2013; Liu et al., 2016; Paradis et al., 2022)."

We have also expanded on the importance of the high sedimentation rates in submarine canyons in the preservation of OC within them (lines 42-45 of the revised manuscript):

> "Shelf-incising submarine canyons can also intercept materials entrained in along-margin sediment transport, funneling large volumes of terrigenous particles towards the canyon's interior (Puig et al., 2014). These high sediment fluxes and mass accumulation rates within submarine canyons lead to efficient OC sequestration within their interior (Masson et al., 2010; Maier et al., 2019; Baker et al., 2024), making these sites hotspots for OC burial."

Both of these aspects are then highlighted in section "4.1 Contribution of terrigenous and marine organic carbon in the Gulf of Palermo" of the discussion as follows (lines 439-454 of the revised manuscript):

> "Although no data of mass accumulation rate is available from the continental slope, the rapidly decreasing accumulation rates on the shelf with distance from shore (0.84 to 0.15 $g \cdot cm^{-2} \cdot yr^{-1}$) to values that are considerably lower than in submarine canyons (0.35-0.82 $g \cdot cm^{-2} \cdot yr^{-1}$) indicates that sedimentation rates on the adjacent slope will be considerably lower than in the canyon axis, as observed in other incised continental margins (Buscail et al., 1997; Sanchez-Cabeza et al., 1999; Masson et al., 2010; Paradis et al., 2018). Moreover, surficial sediment from a sediment core collected in the open slope between Oreto and Eleuterio canyons at 712 m depth (Di Leonardo et al., 2009) did not present any sign of trace metal contamination, whereas sediment cores collected along the canyon axis had significant trace metal contents, indicating a preferential downslope transfer of sediment and pollutants into submarine canyons (Palanques et al., 2022). In fact, this same sediment core on the slope also presented higher $\delta^{13}C$ values (-22.7 ‰; Di Leonardo et al. (2009)), similar to marine end-member values, than those in the afore-mentioned canyons (-24 to -25 ‰), which tend toward more terrigenous end-member values. This further supports the notion that submarine canyons transfer terrigenous OC deeper and farther offshore than would occur in their absence.
>
> The observed high accumulation of both marine and terrigenous OC in these submarine canyons confirms their role as important sites of OC sequestration, as shown in other canyon systems (Masson et al., 2010; Maier et al., 2019; Baudin et al., 2020). However, the contrasting accumulation of terrigenous and marine OC in each canyon suggests that even in closely spaced submarine canyons, the main source of the OC can greatly differ."

I also found the parts of the discussion and conclusions that deal with the potential impact of bottom-trawling fisheries (e.g. Zhang et al., 2024) much too vague and general. Do your data allow you to assess how bottom trawling has altered the total OC composition and reactivity? How susceptible/vulnerable is the OM of the different origins/sources to resuspension and oxidation? In other words: How is both the ratio of marine to terrigenous OC as well as the reactivity of OC altered during downslope transport and during (potential) repetitive resuspension induced both by natural processes and anthropogenic activities – namely bottom-trawling – considering that marine organic carbon is more available to remineralization / vulnerable to oxidation when resuspended/re-exposed to oxygen-rich waters.

Also the use of the term „ecosystem functioning" is much too vague and general. Please, specify what you mean or refer to in detail because I have no idea what precisely you refer to.

We agree that this section was very limited and needed to be expanded a bit more.

With regards to the influence of bottom trawling to reactivity, we indeed see a reduction in the protein turnover rates, a proxy for OC reactivity (data previously given in the appendix but not discussed in the previous manuscript) in the site with highest fishing effort (OC-500).

With the term "ecosystem functioning" we refer to the ability of ecosystems to let trophic webs to work properly (sensu Danovaro et al., 2008 Current Biology). Accordingly, in the amended manuscript we refer to the possible impairment of benthic biodiversity and biomass caused by bottom trawling activities, especially in deep-sea settings (Pusceddu et al. 2014).

We have expanded this section further and modified the text as follows (lines 557-578 of the revised manuscript):

> "The dispersal of terrigenous OM is not only affected by the regional currents, but also by trawling-derived sediment resuspension, both of which displace large amounts of sediment from the shelf and slope into these submarine canyons (Paradis et al., 2021; Arjona-Camas et al., 2024). This transfer of sediment into submarine canyons has not only increased sedimentation rates within all three canyons since the industrialization of the bottom trawling fishing fleet in the 1980s (Paradis et al., 2021), but it has contributed to the dilution of heavy metals accumulating in the canyons (Palanques et al., 2022). Hence, this anthropogenically-induced sediment transport could also be affecting the dispersal of terrigenous and marine OC in the Gulf of Palermo submarine canyons, delivering more resuspended OC into the canyons (Fig. 8). In addition, the higher sedimentation rates in submarine canyons associated to sediment resuspension by bottom trawling activities on the flanks could be increasing the preservation potential of OC within submarine canyons, and further studies should address this.
>
> Bottom trawling activities could also be affecting OC content and composition. This would be the case in the Oreto Canyon, where bottom trawlers continuously fish along the canyon axis and highest fishing effort of the region have been recorded in this canyon (Fig. 8). Here, the repetitive resuspension and down-canyon transport of sediment and OC could explain the down-canyon increase of terrigenous OC and plant-derived (HMW FA) OC along this canyon (Figs. 4c, 6). Furthermore, the continuous sediment resuspension and erosion at this site due to repetitive bottom trawling promotes a reduction of OC contents in surficial sediment (Tiano et al., 2024), either associated to erosion or degradation of OC. Given the high sedimentation rates in this site (Paradis et al., 2021), the reduction of OC associated to bottom trawling in this site may be dominated by enhanced degradation of OC, potentially due to sediment mixing (e.g., (Middelburg, 2018)) and oxygenation (e.g., increasing oxygen exposure time of OC (Hartnett et al., 1998))depleting the most reactive OM components such as phytopigments from the seafloor (Fig. 3d). This process shifts the OC source toward less marine and more terrigenous OC, which tend to be less reactive, as seen by the low protein turnover rate in this site (Figs. 3d, S2c). This process leads to older (i.e., more ^{14}C-depleted) and less reactive OC on surface sediments, which could impair ecosystem functioning (Danovaro et al., 2008) in this area, ultimately affecting benthic community composition and abundance (Pusceddu et al., 2014; Good et al., 2022)."

Minor/Specific comments

Line 377: Maybe rephrase to „In contrast to …"

We prefer the current phrasing of this sentence.

L. 380: … had decreasing terrigenous OC contribution „with depth“

This has been added.

L. 394: Do you mean „sediment“ or „OC“ mass accumulation rates here?

We mean OC mass accumulation rates. This has been clarified.

L. 448: What precisely do you mean with „very distinct composition“ here? Can you specify a bit more.

As shown in Fig. S8 (see below), the composition of brGDGTs of the surface sediment of the Gulf of Palermo in terms of tetra-, penta-, and hexa-methylated brGDGTs are substantially different than those of global soil and peat, indicating that brGDGTs are not originated from soils but rather produced by bacteria in marine environments.

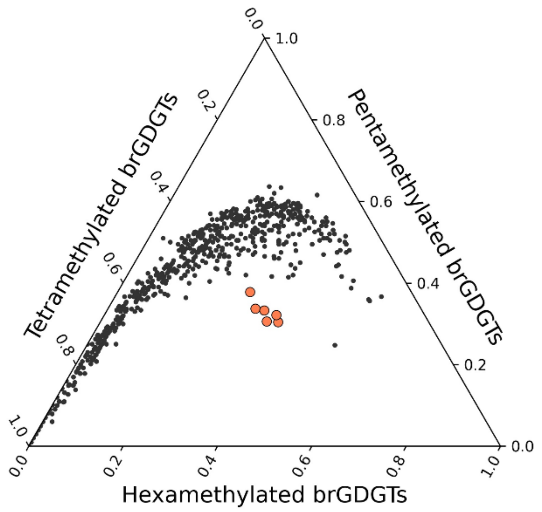

Figure S8. Ternary diagram showing the tetra-, penta-, and hexa-methylated brGDGTs in surface sediments of the Gulf of Palermo (pink circles) plotted together with the global soil and peat dataset (black dots) (Dearing Crampton-Flood et al., 2020). The clear offset of the Gulf of Palermo samples points to brGDGTs produced in-situ rather than being representative of soil-derived GDGTs.

We consider that it is not necessary to provide this much detail in the main text, as Reviewer 3 also points that it is already quite clear that brGDGTs are produced in-situ.

Ls. 510 ff.: Can you expand a bit more here how/by which process and conditions resuspension enhances/promotes the degradation of OM … please also cite the relevant references – e.g. Hartnett et al. (1998) … maybe see Zonneveld et al. (2010, Biogeosciences) for a review.

As shown in our previous reply, this has been expanded.

Ls. 512 ff.: needs to be „direct“; Which type of "ecosystem function" precisely do you refer to here? This sounds very vague and needs to be specified – otherwise delete this part of the sentence. Can you also elaborate a bit here how the total amount of OC varies?

As mentioned earlier, this has been clarified with the following sentence:

> “[…] which could impair ecosystem functioning (Danovaro et al., 2008) in this area, ultimately affecting benthic community composition and abundance (Pusceddu et al., 2014; Good et al., 2022).”

Ls. 516/517: Maybe rephrase to: "… and shown to be primarly of phytoplankton origin"

This has been rephrased.

Ls. 529 ff.: What about the total amount of organic carbon? Based on your data does trawling reduce the amount of organic matter transported and deposited in the canyons? How susceptible is the terrigenous OC to trawling-induced resuspension and transport – compared to the OC of marine origin?

As mentioned earlier, this has been expanded and clarified.

L. 531: Again, what precisely do you mean or refer to when you speak of „ecosystem functioning“? Please, explain and specify. Otherwise delete.

As mentioned earlier, this has been clarified.