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Dear Reviewers,
here below are reported responses to reviewers. All answer and changes related to the reviewers comments are highlighted in red (Reviewer#1) and in blue (Reviewer#2), as well as in the tracked version.

Reviewer#1 (https://doi.org/10.5194/egusphere-2025-1547-RC1)
The study aims to present a new method for mapping coral formations using geomorphological indices. The current manuscript lacks sufficient validation and clarity in its argumentation unfortunately. Key aspects of the discussion are not adequately supported by data, and the reliability, transferability, and efficiency of the new method-particularly the manual correction of artifacts-are insufficiently addressed. Also, the literature review regarding coral mapping in the introduction appears incomplete.
The paper currently sits uncomfortably between a methodological contribution and a study of coral morphology. The presented morphological results are very interesting, especially those in Figures 6,7,8, but they are not really discussed within the broader scientific literature (there is not a single paper cited in the part of the discussion dealing with this results in 5.3). On the other hand, if the focus is methodological, a deeper look into the applicability to other regions is needed. This conflict may be reflected by title and conclusion of the paper: The title suggests a method development, the conclusion is centred on the results you generate.
Clearly a lot of effort went into the paper, and it generated very interesting findings on coral morphologies. Substantial revision is necessary before publication unfortunely from my point of view. My feeling is the insights provided by the geomorphological findings may be the more novel and promising aspects of the study, but the authors may disagree of course.
We sincerely thank the reviewer for this detailed and thoughtful comment. In response to the suggestions, we have revised the structure and content of the paper to improve both clarity and focus.

Title: The title should reflect that the developed method is prevalent to coral formations only. The developed method is rather narrow (or its potential for other habitats not discussed).
We have modified the title in "**Mapping benthic marine habitats featuring coralligenous bioconstructions: a new approach to support geobiological research**" in order to explicitly indicate the focus on coralligenous bioconstructions.
In the revised title, we explicitly state that the focus of the study is on coralligenous bioconstructions. Furthermore, since the method was applied to a specific habitat (i.e., Coralligenous) and in a single pilot area, we chose to adopt a more cautious tone by using "approach" instead of "protocol".

Line 21: This is not really correct for bathymetry, which is strongly standardized by the IHO, and not entirely correct for backscatter, where several paper and reports now deal with backscatter processing (*i.e.,* https://doi.org/10.1007/s11001-017-9315-6).
We thank the reviewer for the constructive observation. To acknowledge the existing standards in bathymetry data and the emerging standardization efforts in backscatter processing, the sentence (lines 22 to 25) was revised in: "Although the acquisition and processing of bathymetric data follows standardized procedure (*e.g.,* Hydrographic Organization guidelines), and recent studies proposed recommendation for backscatter acquisition and processing, a broadly validated methodological approach, integrating geomorphometric analysis for benthic habitat mapping, is still lacking."

Line 36: What is "relatively large"?
The vague expression "relatively large temporal scale" has been replaced with a specific time range to provide clearer context in "over decadal to millennial timescale" (lines 40-41)

Line 41: I learned that the term Coralligenous is sometimes used in the Mediterranean as a noun, but everywhere else it is used as an adjective. It sounds supe strange to me as a noun. Maybe you reconsider rewording that to the broader peer reviewed literature (as you dome sometimes in the text and figures, when using coralligenous as an adjective).
We appreciate the reviewer's linguistic remark. In this paper, according to literature, the term Coralligenous was commonly used as a noun when referring the habitat or ecological system as a whole, whereas, when referring to specific benthic structures (e.g., "coralligenous build-ups" or "coralligenous bioconstructions"), the term coralligenous was used as an adjective.

Line 44: What is the ecological climax?
The sentence has been revised as follow to better clarify the concept: "Pérès and Picard (1964) and Pérès (1982) identified Coralligenous as the ecological climax stage for the Mediterranean circalittoral zone…" (line 49).

Line 55: What is "improve its knowledge" referring to?
We thank the reviewer for pointing out this ambiguity. The phrase has been revised to clarify that the purpose of the classification is to promote a more objective and reproducible understanding of coralligenous morphologies. The updated sentence (lines 59-60) now reads: "Moreover, Bracchi et al. (2017) introduced a new classification for coralligenous morphotypes on sub–horizontal substrate using a shape geometry descriptor, in order to obtain a more objective description of these morphologies…".

Line 71ff: There are a quite a lot of papers on remote sensing of corals including with acoustics. Please extend your literature review accordingly, with a focus on peer-reviewed literature. Chiocci et al. 2021 seems to be a report, and the widely cited Bracchi et al 2017 is a self-citation (which is fine, but not as the only reference if there are others as well).
We thank the reviewer for this helpful suggestion. We added some works in the text and in the references (Fonseca & Mayer, 2007; Lecours et al., 2015; Brown et al., 2012; Lamarche and Lurton, 2018; Abdullah et al., 2024) to support the relevance of acoustic remote sensing application in habitat mapping (lines 81 to 84). However, it is important to note that the present study focuses on coralligenous bioconstructions, which differ significantly from coral reef from both a geomorphological and geobiological perspective. Moreover, coralligenous bioconstructions have not received the same attention as coral reef, which explain the relative scarcity of dedicated literature on the subject.

Line 94: remove "briefly"
Deleted as requested at line 103.

Figure 2: The Geomorphological Indices act on the DTM and not the raw data, or? Then you need to adapt the flowchart. How is the ROV validation feeding into the Benthic Habitat Map? I understand correctly that your method is only valid for the coral formations? It is not clear how the Geomorphometry analysis done after "Processing" differs from the one done for the "Coralligenous bioconstructions".
We thank the reviewer for these valuable suggestions. In response, we have updated Figure 2 to more clearly reflect the workflow of our methodology. The revised figure (line 115):

- explicitly shows that geomorphological-geomorphometric indices were applied to DTM and not to raw data;
- includes an arrow connection the validation of the model through visual census of ROV-video transects, indicating how ground-truth data support habitat identifications.
- separates the coralligenous bioconstructions pathway, linking it to further morphometric analyses (TPI for extraction, shape indices, thickness, volume and surface calculation), highlighting that the protocol is focused for this habitat.

These changes aim to clarify the logic and scope of the method as well as its data flow.
To better clarify how geomorphometric analysis conducted after Processing differs from the one performed for the coralligenous bioconstructions, the text in the section “2. Methodological approach” has been revised as follows:

- Lines 104-105: “The most representative morphological indices, represented by slope and seafloor roughness, were extracted from the Digital Terrain Model (DTM).”;
- Lines 118-120: “Once the spatial extension and distribution of the benthic habitat have been defined by combination bathymetric, backscatter, slope and seafloor roughness data, the extraction of coralligenous build-ups was carried out using the Topographic Position Index (TPI), according to Marchese et al. (2020).”

Finally, to clarify how the ROV–video transect contributes to the validation of the benthic habitat mapping model, the text has been revised as follows:

- Lines 121-123: “Finally, the benthic habitat mapping model was ground–truthed by visual analysis of ROV–video transect performed along specific paths within the study area.”

Line 115: Did you use the photogrammetric 3D reconstructions?
Although the optical module mounted on the ROV was designed for high-resolution 3D reconstruction of underwater targets, no photogrammetric processing was performed for this study. Consequently, to avoid confusion, we have decided to remove the relative sentence at lines 125-126.

Line 127: there is no WBMS Basic model. What kind of navigation solution did you use (EGNOS, RTK, ...)
We thank the reviewer for pointing this out. The correct model used was the Norbit iWBMS-Long Range. We have also specified that the navigation solution adopted was RTK, ensuring high positioning accuracy during the survey. The sentence in the lines 138-140 of the manuscript has been modified in “MBES surveys have been carried out using a pole–mounted, Norbit iWBMS Long Range Turnkey Multibeam Sonar System integrated with GNSS/INS (Applanix OceanMaster) operating with Real Time Kinematic (RTK) corrections, ensuring high positioning accuracy during the survey.”

Line 129: What is several.
We thank the reviewer for pointing out this ambiguity. The term “several” has been replaced with the actual number of sound velocity profiles acquired during the survey (lines 142 to 144). A total of three sound velocity profiles per day were performed using a Valeport miniSVP throughout the survey. Considering the absence of freshwater inputs and the relative stability of the water column across the depth range, this was deemed sufficient to ensure reliable sound speed correction for high-resolution MBES acquisition.

Line 134: Please justify a 5 cm resolution with your survey settings. I doubt this is possible in either along-track or across-track direction with your survey settings. What would be the impact of different resolution on your new method?
We thank the reviewer for raising this important point. We acknowledge that, due to an oversight, the Figure 1 originally included in the manuscript showed the approximate survey lines used during the

initial scouting phase, rather than the navigation lines corresponding to the MBES dataset processed and analyzed in this work. This has now been corrected and the revised Figure 1 includes the navigation tracks from the MBES surveys described in the paper.
Additionally, the corresponding text in section 2.1 "Bathymetric and backscatter data" (Lines 140 to 144) has been revised to reflect this update and to address the related comment regarding SVP acquisition: "Data were collected in 59 tracks with a swath overlap of 20-40 % performed at an average speed of 4.5 knots. A total of three sound velocity profiles per day were collected using a Sound Velocity Profiler-Valeport miniSVP. Considering the absence of freshwater inputs and the relative stability of the water column across the depth range, this was deemed sufficient to ensure reliable sound speed correction.".
We sincerely apologize for the confusion caused. Should the reviewer or editor require further verification, we would be glad to provide the raw navigation files privately via email upon request.

Line 147+150: Please provide units for the search radius.
We thank the reviewer for this observation. The units of measurement for the search radius (map units) have now been explicitly added in the revised manuscript and throughout the rest of the paper.

Line 172ff: I find this section much too detailed. How are is the detailed description of the onshore terraces relevant for the developed method? Potentially this could be shortened?
We thank the reviewer for the constructive comment. We agree that the original section "3. Geological Setting" included an excessive level of detail, particularly regarding the sedimentological characteristics of the onshore terraces. As recommended, we have significantly shortened this section by removing redundant stratigraphic descriptions and maintaining only the general overview of the five order terraces in the Crotone Peninsula. We believe this information is relevant as it contextualizes the extensive carbonate production due to algal bioconstructions during the Late Pleistocene, which also appear to influence the current seafloor morphology and supports the investigation of submerged coralligenous habitat in the area.
The revised version, reported from line 188 to 255, provides the necessary geological framework in a more concise form, reducing the original section by approximately 39%, as suggested.

Line 232: What are morpho-acoustic characteristics?
In the context of this study, morpho-acoustic characteristics refer to the combination of morphological descriptors (*i.e.,* slope and seafloor roughness) and acoustic properties (primarily backscatter intensity) used together to identify and describe benthic habitats. This expression is used throughout the manuscript to concisely represent the joint analysis of morphological and acoustic data. Accordingly, we have decided to change the title of Section 4.1 from "Morphological and morpho–acoustic characteristics of the seafloor" to "Morpho–acoustic characteristics of the seafloor" (line 262).

Line 237: Can you add the depths here above and below the marked break in slope?
The requested depth values have been added to the text (line 267).

Figure 4: Can you annotate the figure and mark the slope and occurrence of coral and Posidonia?
We appreciate the reviewer's suggestion. However, we chose to keep Figure 4 (line 275) in its current form, as the separate visualization of bathymetry, backscatter, slope and seafloor roughness already provides a clear and uncluttered representation of seafloor morphology. Adding further annotations could compromise the readability and interpretability of the individual datasets. The spatial distribution of *Posidonia oceanica* and coralligenous bioconstructions is instead illustrated in detail in Figure 6 (line 315), which integrate morpho-acoustic data with field observation (ROV-video transects) to support habitat identification.

Figure 5: Is the Posidonia Oceanica growing ON the rocks/corals, as stated in the figure, or in the sand patches between? I cannot recognize it on the image. Is "rocky blocks" and actual term?
In the study area, Posidonia oceanica was observed growing predominantly on hard substrates. This pattern is confirmed by ROV video analysis and supported by previous studies in the same area (e.g., Rende et al., 2008).
Regarding the use of the term "rocky blocks", we know that this is not a "standard" geomorphological classification. However, based on MBES and ROV investigations, we were able to identify the morphological presence of scattered block-like features, but not their lithological nature. Since a more detailed interpretation was not possible, we opted for the generic term "rocky blocks" to describe their appearance without implying specific compositional characteristics.

Figure 6: Can you plot a depth contour in here? The sharp distinction between corals and only blocks is striking. Do you have a video transect crossing that boundary, that could be interesting to show in the paper?
We thank the reviewer for this constructive suggestion. In response, we have updated Figure 6 (line 315) by adding depth contours, which help to contextualize the morphological distribution of coralligenous bioconstructions. Unfortunately, we do not currently have a high-resolution ROV video transect documenting the boundary between coralligenous and blocks, due to technical issues encountered during the video acquisition phase (in particular, the widespread presence of fishing nets on the seafloor, which prevented continuous and safe navigation for the equipment).

Line 270: What is Coralligenous sensu stricto? It appears here and, in the discussion, but is not defined.
We thank the reviewer for this comment. The expression "Coralligenous *sensu stricto*" refer to coralligenous bioconstructions that are not intermingled with Posidonia oceanica. In the revised version (line 299-300) of the manuscript, we have explicitly defined this term where it appears first.

Line 284: Can you show an image of false positive for the different causes you mention, and how obvious they are to remove? You cannot have validation data for all of them. Given that this is a method to be repeated in other areas, what was the time requirement for cleaning the data. Given that corals and Posidonia appear to be mixed, that may be very tricky to differentiate. An example would be appreciated.
We thank the reviewer for this constructive and insightful comment. To better illustrate the types of false positive encountered and how they were addressed, we decide to include a new Figure (Figure 8 at line 357) relative to the paragraph 4.2 of the revised manuscript.
This Figure shows representative examples of artifacts caused by the presence of *Posidonia oceanica* (A), bad roll correction (C) and artifacts concentration on DTM boundaries (E). Panels (B), (D) and (F) respectively show the same areas of the model after manual removal of these false positives.
The identification of artifacts was based on specific spatial patterns, inconsistent with expected coralligenous morphology, and their removal was carried out manually as part of the data cleaning process (lines 330-331).
As for the time required for the cleaning phase (lines 331 to 334), it is not possible to provide a fixed estimate, as this strongly depends on the quality of the survey execution, the geomorphological complexity of the study area and the experience of the operator performing the cleaning. These factors can significantly influence the extent and efficiency of manual artifact removal.
Concerning the distinction between Coralligenous and *Posidonia oceanica* in the mosaic area (lines 335 to 339), we acknowledge the challenge. In our case, the separation was based primarily on backscatter signal characteristics. Specifically, Posidonia is associated with a moderate, speckled acoustic texture, while coralligenous bioconstructions exhibit more complex and spatially structured signature. Naturally, these interpretations were supported by targeted ROV video transects.

Line 285: I don't think the type i) artefact could be avoided with higher quality data - you do correctly measure the Posidonia.
We agree with the reviewer's comment. Accordingly, we have revised the manuscript to clarify that only artifact related to bad roll correction and DTM boundaries (type ii and iii in the manuscript) can be reduced by improving survey parameters (lines 327 to 329).

Line 331: How do you know they have been transported by gravitation from above? Could it not be simply the local basement?
We thank the reviewer for the pertinent observation. The hypothesis that these sub-spherical rocky blocks may be results from gravitational processes is primarily based on their rounded morphology, which is generally considered indicative of detachment and downslope transport, rather than part of the local basement. Furthermore, this interpretation is supported by geomorphological evidence observed in the emerged portion of the study area. Specifically, clear scarps are presents and affect the 4th order marine terrace deposits, which outcrop directly upslope from the surveyed seabed. These scarps suggest past gravitational instability that may have contributed the transport of material (lines 452 to 456).
We acknowledge that this remains a plausible hypothesis (hence the use of "possibly" in the manuscript). However, the morphological characteristics of the blocks and the geomorphological context provide a reasonable basis to suggest a gravitational origin linked to the nearby marine terrace.

Line 309: This I would no longer consider state of the art or standard. You are not considering a huge number of studies using Neural Networks to classify multibeam (i.e., for boulders and pockmarks, nodules...), and also miss some papers dealing with OBIA and geomorphometry.
We sincerely thank the reviewer for this insightful and constructive comment. We agree that recent advances in deep learning and object-based approaches have significantly expanded the frontiers of seafloor classification, particularly in the context of MBES data interpretation. In response, we have substantially revised lines 378 to 399 of the manuscript to acknowledge and integrate recent studies employing Convolutional Neural Networks (CNNs), Fully Convolutional Neural Networks (FCNNs) and Object-Based Image Analysis (OBIA) for geomorphological mapping of the seabed. Relevant references have been added to support this updated discussion, including Arosio et al. (2023), Garone et al. (2023) and Stephens et al. (2020). We hope this revision adequately reflects the state of the art and clarifies the intended scope of our statement.

Line 315: Is this only meant to apply to geomorphological classification?
We thank the reviewer for pointing out this ambiguity. To avoid confusion and possible misinterpretation, the portion of text corresponding to this comment has been revised and clarified. The revised formulation explicitly states that the lack of a standardized methodology refers specifically to geomorphological classification. This clarification is included within the modified paragraph already addressed in the previous comment.

Line 319: This should refer to Figure 2.
We thank the reviewer for pointing out this oversight. The figure reference has been corrected in the revised manuscript (line 400).

Line 339: Maybe I have missed that comparison - what are the morphological characteristics that lead to your conclusion the area is part of the submerged 5th terrace? The paragraph at the moment reads very speculative.

We thank the reviewer for this insightful comment, which allowed us to clarify the morphological reasoning behind the interpretation. In the revised manuscript, the section corresponding to lines 441 to 451 has been substantially rewritten to clarify the morphological basis supporting the interpretation of the surveyed area as part of submerged 5th order marine terrace.

Line 350: Which observation is that referring to? I cannot flow your argumentation on hydrodynamic control or interglacial cycles. Please extend this part of the discussion incorporating your results and the literature.
Following the reviewer's suggestion, we have clarified that the "observation" refers to the spatial arrangement of coralligenous bioconstructions, which tend to align sub-parallel to the shoreline and appear associated with relatively pronounced seabed structures. To address the reviewer's request, we have extended the discussion and formulated two hypotheses (lines 462 to 473), supported by Bialik et al. (2024) and Varzi et al. (2022) respectively, that may explain the observed pattern.

Line 325-338: I think this is results, or? It could go to 4.1, where you already mention the detected habitats and could describe them further.
Line 346-350: This is again results.
We thank the reviewer for these observations. Following the suggestions, we have revised the manuscript structure by relocating the descriptive content into section 4.1 "Morpho-acoustic characteristics of the seafloor" (lines 301 to 312), which now provides a more comprehensive account of the observed benthic habitat.
In turn, the section 5.1, now retitled "Spatial distribution of benthic habitats and seafloor morphology" (lines 436 to 473), has been restructured to serve as discussion.

Line 357-372: This again is mostly a method description, lacking the actual discussion of your data. Please discuss for example the implications of different threshold values for detection? Is it transferable to other regions? Is there a way to find the best threshold for a region? Is the dataset of Marchese et al comparable to yours regarding the presence of the three artifact types? With the time reduction, is it feasible as a standard method for all new multibeam data?
We thank the reviewer for these observations. In response, the section 5.2 "TPI-based feature extraction" has been significantly revised and expanded to address the methodological implications of the threshold selection and the broader applicability of the proposed approach. While the first part of the paragraph (lines 476 to 491) was retained to introduce the rationale behind the method, which we consider a general methodological commentary, the rest of the section (lines 492 to 508) aims to address the questions raised by the reviewer.

Line 375-377: This is a method description.
We thank the reviewer for the comment. In the revised manuscript, the original sentence has been reformulated to better suit the discussion section. The revised sentence (lines 513 to 516) now introduces the quantitative analysis of the morphometric parameters in an interpretative context. Specifically, it serves as an opening statement to Section 5.3 and highlight the spatial distribution, morphotype variability and growth patterns revealed through the scatterplots presented in Figure 9 (formerly Figure 8).

Line 384: The figure says R^2=0.35
We thank the reviewer for pointing out this error. This was a mistake, and the R^2 value in the text has been corrected at line 523.

Fig 8B: How comes there is so little scatter for Volume/Thickness of the banks compared to the discrete reliefs. Is this related to the identification method you use? All other graphs show comparable scatter between the two features.
We thank the reviewer for this observation. In our view, the reduced scatter observed for the volume/thickness relationships of the banks in Figure 9 (formerly Figure 8) is not a result of the identification method, but rather reflects the consistency of the hypothesis regarding the different growth dynamics of the two morphotypes. In particular, the broader variability observed in discrete reliefs supports the idea of a more important lateral growth component, whereas the banks tend to follow a more uniform vertical growth pattern.

Line 387: Any references for that from other coral fields?
Line 374ff: General comment to 5.3: I a missing completely a discussion of the literature here. Is this comparable to other coralligenous fields? Why do bank and discrete features appear mixed over short distances if they have different growth types?
We thank the reviewer for these important comments. To date, no previous study has provided morphometric analysis of coralligenous build-ups based on quantitative extraction of 2D/3D parameters (e.g., area, thickness, volume, shape indices) from high-resolution MBES data. Therefore, a direct comparison of our results with other Mediterranean coralligenous fields is currently not possible. However, a broader study applying the same methodological approach to coralligenous build-ups from different Mediterranean settings is underway. In particular, a morphometric analysis of the Amendolara Shoal (Ionian Sea, Calabria, Italy) is currently in preparation, while a second study focused on the Lamezia Terme coastal sector (Tyrrhenian Sea, Calabria, Italy) is awaiting ROV video transects to obtain ground-truth data for model validation.
Following the reviewer's suggestions, paragraph 5.3 "Morphological development of coralligenous build-ups" has been revised (from line 538 to 546) to include a discussion on comparable studies and the possible environmental controls explaining the observed spatial coexistence of different coralligenous morphotypes.

Note: The conclusion is mostly on the habitat characteristics of the area (not the method/d). This is fine (I think the results to be the most interesting parts of this study, not the method itself), but contradicts the title.
We agree with the reviewer's observation, and thanks to revisions and suggestions throughout the manuscript, the conclusions is now better aligned with the main focus of the article.

Reviewer#2 (https://doi.org/10.5194/egusphere-2025-1547-RC2)
This is a well-researched paper with a robust methodology and as a technical note a useful addition to the present literature related to coralligenous bioconstructions and marine habitat mapping. There is a strong rationale which is clearly explained in the study. (Lines 62-74). I think it should refer to some standards which have in fact been developed such as the GEOHAB backscatter manual- "Backscatter measurements by seafloor-mapping sonars. Guidelines and Recommendations" (https://zenodo.org/records/10089261) and look at the framework proposed in the recent terminology paper by Jardim et al. (https://onlinelibrary.wiley.com/doi/full/10.1002/aqc.70121) as well as authors' own previous work on terminology. I feel the title could be clarified to replace "functional to geobiological researches" to something more methodological as it is unclear what geobiological researches means by the general audience. The quality of the figures is high and it is a useful technical note utilising the latest GIS tools. I would recommend publication after few changes suggested above.

We thank the reviewer for the positive feedback and helpful suggestions. As requested, we have revised the section 2.1 "Bathymetric and backscatter data" to clarify that MBES-backscatter data were processed according to the GEOHAB Backscatter Working Group guidelines (Lurton et al., 2015), which are now explicitly cited in the manuscript. The updated version (lines 154-159) now reads:
"Backscatter data were processed using QPS Fledermaus, based on time series data and applying standard corrections for sonar configuration (e.g., source level, beam pattern, receiver gain) and environmental factors (e.g., absorption, slant range, footprint geometry). The processing was performed according to the general principles outlined in the Backscatter Working Group guidelines (Lurton et al., 2015), which provide detailed recommendations for the acquisition, correction, calibration and processing of MBES-backscatter data. The final output, exported as an 8-bit raster file with a 0.05m cell size, was used to extract morphological and acoustic patterns of the seafloor."

In the "Introduction", a reference to the recent terminology framework proposed by Jardim et al. (2025) has also been added, highlighting the current terminological uncertainty but clarifying that the term "coralligenous bioconstructions" is widely used in the geobiological literature, as supported by several citations. Furthermore, additional references to recent studies (Ingrosso et al., 2018; Ferrigno et al., 2024) have been included to reinforce the adopted terminology and contextualize his definitions within the broader scientific literature. The updated version (lines 48-52) now reads:
"Although recent studies highlighted some terminological uncertainty in the definition of coralligenous habitat (e.g., Jardim et al., 2025 and references therein), within the geobiological literature the term coralligenous bioconstructions is widely and consistently adopted to indicate these biodiversity-rich, three-dimensional biogenic structures characterized by several layers of encrusting coralline algae (e.g., Ingrosso et al., 2018; Bracchi et al., 2017, 2022; Basso et al., 2022; Cipriani et al., 2023, 2024; Ferrigno et al., 2024)."

With regard to the suggestion to modify the title, we acknowledge reviewer's observation concerning the potential ambiguity of the expression "functional to geobiological researches". However, we have chosen to retain the title "Mapping benthic marine habitat featuring coralligenous bioconstructions: a new approach to support geobiological research", as the term "geobiological" reflects an emerging interdisciplinary approach that integrates geological and biological perspective in the study of marine benthic habitat, such as coralligenous bioconstructions. This research field has become increasingly relevant in recent years, and the proposed approach has been specifically developed to address the methodological requirements of this discipline, as also documented in recent literature. We believed that the current title adequately represents both the conceptual and methodological framework of the manuscript and is consistent with recent trends in the discipline.