Referee Report Egusphere-2026-5117: "An estimate of the global marine carbon flux of cold-water corals"

In the present paper the authors provide a global estimate for cold-water coral carbon accumulation and compare it to tropical coral accumulation and pelagic burial. This estimation is based on the following equation:

Coral C accumulation rate x Habitable Area x Mound covered area x Live coral cover x Exposed mounds = Global C accumulation

Although the equation presented in figure 1 makes sense conceptually, except for the exposed mounds parameter (see below), the authors' approach has several substantial weaknesses:

- Incorrect usage of published data for habitat area, mound coverage area and coral cover, partly values are not consistent with their source
- The consideration of buried mounds is not necessary
- The intersection of the different datasets provides additional problems that cannot be approximated.

None of these issues are discussed adequately.

A detailed description of all these issues is provided below.

Even if the methodological approach was correct, the conclusions seem quite biased. The global C accumulation rate provided has an uncertainty spanning over 4 orders of magnitude, and hence it cannot be used as an argument that it is one order of magnitude lower than that of Tropical Corals.

Due to all the methodological issues and the added up biased discussion, I can only recommend to reject this manuscript. I did not proceed with further detailed comments as the raised major concerns are too substantial.

We thank the reviewer for the detailed assessment of the parameterisation and for checking individual source values. We agree that the conceptual product (coral carbon accumulation × habitable area × mound-covered fraction × live coral cover, with an optional exposure term) follows the logic introduced by Dorschel et al. (2007) and otherwise uses published estimates. We also agree that several tabulated values were transferred incorrectly. Those corrections are implemented below.

We do not agree that the approach is biased, or that the width of the uncertainty invalidates the conclusion. The study was never intended to close a carbon budget. Its purpose is a first global compilation and upscaling of published local estimates, which individually have been used to infer a large role of cold-water corals in the carbon balance without a global data synthesis. Given the heterogeneity of mound provinces, the resulting distribution spans about four orders of magnitude. Within that distribution, the median coral-carbon accumulation is about one order of magnitude below the median for tropical coral reefs, while the upper tail of the cold-water distribution overlaps the lower tail of the tropical distribution. The robust statement is therefore not that cold-water corals are precisely one order of magnitude less important, but that they are unlikely to be globally significant for the modern flux of carbon from the ocean into marine sediments. We will state this more explicitly in the revised manuscript, together with the systematic biases already discussed.

Correcting the tabulated typos does not change that conclusion. Recalculation changes the global estimate materially only for the exposure term (up to about 35%). Even without that

term, the median remains well below tropical-reef and pelagic-burial reference values, inside a distribution whose width is the main result.

(I) Coral C accumulation rate

Considering this is the baseline data from which all the follow-up calculations are made, more care should have been taken to make sure data was correctly extracted from the original papers, and to check that the correct papers were being used. When looking at Table 1, for Greiffenhagen et al. (2026) all the background sediment values are wrong as they are only including inorganic carbon and not its organic fraction present in the sediment, which is outlined in the original paper. The same occurs with the data coming from Greiffenhagen et al. (2025). Additionally, some of the numbers are wrong when compared to the original paper. Almost all the aggradation rates are copied from the wrong column of the original paper and instead reflect the Total Carbonate Accumulation (Table 3 in Greiffenhagen et al., 2025). The data from Titschack et al. (2015), presents a small conversion mistake in the total accumulation of all cores.

A second aspect that should always be considered when approximating rates from sediment core data is the time averaging effect – rates decrease with decreasing resolution of the age model due to the interpolation over hiati. The values acquired from Titschack et al. (2009) are not usable here, as the aggradation rates have been derived from coral fragments with age uncertainties of 350 kyrs, most likely underestimating aggradation rates and overlooking the presence of mound development hiatuses. Data from Dorschel et al. (2007) cannot be used for the presented paper, because the core age models are only derived from 4 samples each, encompassing more than 100 kyrs and several mound formation hiatuses. The authors consider the entire cores as a unique mound formation period, completely underestimating the aggradation rates and hence, the carbon accumulation rates of this mound.

In Titschack et al. (2016), core GeoB11135-2 cannot be used. The gravity core was retrieved from a talus deposit at the foot of a submarine wall and not a coral mound, which becomes a problem when the two approaches used to get the global estimate are based on coral mounds densities and coverage. Using the data from Hebbeln et al. (2019) is also not an appropriate approach, as the authors did not measure such rates themselves, instead, they estimated them making many assumptions and likely underestimating mound aggradation rates. Both van Oevelen et al. (2009) and Cathalot et al. (2015) are dealing only with organic matter ingestion by the corals, not the carbon sequestered and stored in the sediments through calcification, which makes this data not usable to estimate global carbon accumulation. Lindberg and Mienert (2005) and Tamborrino et al. (2022) data have the same issues as Hebbeln et al. (2019).

We thank the reviewer for the line-by-line check. Organic carbon was omitted from the Greiffenhagen et al. (2025, 2026) background-sediment values; adding it raises those background fluxes by less than 10% on average. These background values are not used in the global coral-carbon calculation. Coral accumulation is estimated from the coral component only, because hemipelagic background sediment is already accounted for in pelagic burial (see VII).

A full re-check shows that most Greiffenhagen et al. (2025) and Titschack et al. (2015) values used in the code were already correct. The table errors are:

- Greiffenhagen et al. (2025): total accumulation 13.6 → 13.9; sediment accumulation 3.71 → 3.72 (not used); aggradation rates corrected to 131, 61, 61 and 22 (not used).

- Titschack et al. (2015): the 0.12 conversion affected three total-accumulation entries (4.92 → 4.96, 12.36 → 12.3, 2.64 → 2.66). Coral accumulation rates were already correct.

None of these corrections changes the global coral-carbon estimate. We will correct the table and apologize for the transcription errors.

On time averaging: Titschack et al. (2009) and Dorschel et al. (2007) supply 5 of the 41 lowest aggradation rates, and both age models average across long intervals and hiatuses. We retain them. Vertical mound growth rate varies with coral population density, framework production, bioerosion and trapping (Frank et al., 2009). High rates indicate dense framework; low rates or missing coral ages indicate sparse growth and erosion, not values that should be discarded because they are low. Excluding dormant or poorly resolved intervals would raise the mean coral-carbon flux by about 70% and bring it close to tropical-reef means, but that would systematically bias the compilation toward active phases. The reported variance already covers this choice. We will state it explicitly.

We also retain the other contested sources, and we disagree that they should be removed in order to narrow the distribution. This is a compilation of published estimates, not a re-interpretation of each core's setting. Individual outliers increase the variance; given mound complexity, that is the appropriate outcome for a first global estimate.

- GeoB 11135-2 (Titschack et al., 2016) is a talus deposit. We will flag it as such. Removing one core does not change the global distribution, and we do not have the metadata to reclassify published rates systematically.
- Hebbeln et al. (2019), Lindberg and Mienert (2005) and Tamborrino et al. (2022) are published rate estimates with stated assumptions. We do not see a basis for the claim that Hebbeln et al. (2019) systematically underestimates aggradation, and we will not replace those values with our own.

van Oevelen et al. (2009) and Cathalot et al. (2015) quantify organic-matter processing, not carbonate accumulation. We will label them clearly and exclude them from the carbonate-accumulation distribution, so that the coral-carbon estimate is not mixed with a different flux. They remain relevant only as context for organic-carbon turnover, which is not the quantity upscaled here.

(II) Habitable area

The authors use the **species richness** habitat suitability model from Fragkopoulou et al. (2025) to predict the distribution of corals mounds globally, assuming that among **all suitable habitats** one of the species predicted would be a framework-building cold-water coral. This is far from being the correct approach for many reasons:

1. It is definitely not correct to assume that one of the present species would be a mound-forming cold-water coral. Why should this be the case?
2. If the authors aim at estimating carbon sequestered by coral mounds, the habitable area cannot include areas outside the continental margins and/or deeper than the current known existence of coral mounds.
3. The model used includes more than 700 species many of which are octocorals, black corals and stylasterid corals. Scleractinian corals is just one of the groups included and mound-forming cold-water corals are a very small subgroup (max. 6 – 8 species).
4. Even if it was modelling scleractinian framework-building corals, this model is predicting presence of individual colonies, not mounds.

5. It is unclear which species richness value was considered as suitable for the presence of coral mounds.

We will clarify that Fragkopoulou et al. (2025) is used as a maximum potential habitat, not as a map of mounds. Framework-forming scleractinians, in particular Desmophyllum pertusum and Madrepora oculata, are among the most widely recorded and opportunistic species in that compilation (21,730 records; Scleractinia 21.61% of the species composition). Supplementary bioregion tables show Caryophylliidae near 10% in every bioregion and Dendrophylliidae (Enallopsammia rostrata included) in every bioregion. We therefore treat the predicted habitat as the area in which these species can occur, not as the area in which mounds are present. Mound cover is applied afterwards and is only about 3.5% in large surveys. The product is an upper-bound habitat, not a prediction that mounds fill the suitable area. We will add a short table and a map comparing the two species' records with the Fragkopoulou habitat, and we will state this assumption explicitly.

We agree that mound-forming scleractinians are a small subset of the >700 taxa in the model, and that the model predicts colony presence rather than mounds. A species-richness threshold, or a restriction to mapped mounds only, would shrink the area and lower the flux. We do not apply such a filter, because the aim is a maximum first-order estimate. Colony presence is a necessary, not a sufficient, condition for a mound; the mound-covered fraction is the term that converts potential habitat into mound area. Global metre-scale mapping does not exist. Reducing the habitat to known mounds would most likely decrease, not increase, the flux.

On depth: only 2 of about 21,400 D. pertusum and M. oculata records are deeper than 2000 m, and about 850 are deeper than 1000 m. Excluding records below 1000 m changes the surface by about 4%. We will carry a conservative habitat-area uncertainty of <15% to cover depth and taxonomic breadth, rather than silently including abyssal area where mounds are not known.

(III) Mound covered area

Although including this term is likely a good idea, there is a need for a better explanation on how the numbers were extracted. In any case, this term can become quite problematic. How is the total area explored decided? This is crucial as the total area should be exclusively determined by the outer mounds limiting a mound province extension. If the total area is only related to the available multibeam bathymetry, side-scan sonar etc., the background area used to make the calculations is completely arbitrary. Probably the approach followed by Tamborrino et al. (2022), where he takes a radius of 1 km from all mounds to define the mound province background area, is the most adequate one. However, this approach should be applied to all other datasets.

In addition, the data extraction from Tamborrino et al. (2022) is wrong. The values from Table 2 correspond to the Median footprint area in the original paper (Fig. 6A in Tamborrino et al., 2022), not the mean.

The paper from Di Giovanna et al. (2025) makes no sense in Table 3. This paper is showing a habitat suitability model for scleractinian CWC colonies in submarine canyons and based on ROV videos. It is not talking about mounds anywhere.

We agree that the surveyed area must be defined consistently, and that a fixed radius around mounds (Tamborrino et al., 2022) is attractive. That radius is not specified in a way that can be reapplied, and the other studies do not publish the raw bathymetry needed to redo it. We therefore cannot homogenise the denominator.

What the compilation does show is that surveys larger than 50 km² converge on a mound cover of about 3.5%, whereas very small survey areas produce much higher percentages. We will drop the small-area, high-cover value so that the percentage is not controlled by an arbitrary window, and we will say so in the text. Further mapping is justified for conservation and biodiversity; it is unlikely to move the global carbon-flux conclusion outside the present four-order range.

Tamborrino et al. (2022): the values in our table are the medians (their Fig. 6A), not the means. The column header was wrong. Because the distribution is skewed, the calculation uses medians, as stated in the text. The global estimate is unchanged; the header will be corrected, and we will note where a source provided only a mean.

Di Giovanna et al. (2025) does not document mounds. It will be removed from the mound-cover table and from that calculation. Canyon colonies can still contribute carbonate within the habitat term; we will state that this is a presence of framework, not a mound province, so the two are not conflated.

(IV) Live coral cover

According to the equation in figure 1, the live coral cover is needed to estimate the area where high carbon accumulation rates occur. In principle, there are two possibilities to obtain this data: (i) An ROV or AUV mapping of an entire mound with photography and a subsequent image classification (see Lim et al. 2017); or (ii) the usage of ROV transects and a subsequent sea floor prediction map as performed by e.g. Sowers et al. (2024), De Clippele et al. (2017) and Heindel et al. (2010; not cited in manuscript). However, the usage of many datasets provided in Table 4 are not adequate due to the following arguments:

Vad et al. (2017) provides a live coral percentage based on individual colonies, the thickness ratio of living to dead part of colonies, surface percentages of reef cover on a mound were not provided.

Sowers et al. (2024) provided an aerial estimate for various ‘mounds’ habitats (slope, ridge, peak). All three habitats for a ‘mound’ and should be considered in context (relative areal contribution to the mound) instead of being used separately.

Overlap of data between De Clippele et al. (2017; subarea of 2021) and (2021). Furthermore, taken the numbers from the first paragraph of the results, live and dead framework is not separated and estimated to cover 17.6 % and coral rubble 76.5%…

De Oliveira et al. (2022) cannot be used because this study does not cover an entire coral mound. De Oliveira et al. (2021), Lim et al. (2017) and Conti et al. (2019) studied the same mound!!!

It is Lim et al. 2017, which should be taken not 2018. Lim et al. (2018) provided mound density data. The numbers provide by Conti et al. (2019) seem to be wrong. According to the paper the dead coral should be >~25 % and live coral between 3 and 4 %. Actually, only the data of Lim et al. (2017) should be taken as this is based on seafloor observation of an entire mound.

Cordes et al. (2023) estimated the coverage also from ROV and submersible surveys. It is unclear if all habitats were covered and if the percentages of the relevant habitats in the region were considered relatively.

Based on the comments above, it becomes clear that the sources to estimate live coral cover of coral mounds are extremely heterogenous and several studies are actually not usable for estimating the live coral cover of a coral mound.

The available observations are heterogeneous. That heterogeneity is a result, not a reason to retain only one mapping method. We will say this clearly. We also add Heindel et al. (2010) and Lim et al. (2017). Adding them raises the median live-coral percentage by about 0.5%, which is small relative to the variance.

Vad et al. (2017) reports colony-scale live fraction, not mound-surface cover. We will label that limitation. Removing it does not change the mean, median or variance materially, so we retain it as an independent observation and do not treat it as equivalent to a full-mound map.

Sowers et al. (2024): we will replace the separate slope, ridge and peak values with a single mound-representative mean of 3.67%.

De Clippele et al. (2017) and (2021b) are not the same site (Mingulay Reef versus Rockall Bank, about 380 km apart). De Clippele et al. (2021b) report 3% live framework and 45% dead framework. We will keep both and cite those figures explicitly.

De Oliveira et al. (2021), Lim et al. (2017) and Conti et al. (2019) include the same mound. We will flag the geographic overlap. Each study is an independent estimate of live cover; collapsing them to a single “entire mound” map would reduce variance by sampling choice, not by new evidence. Lim et al. (2018; doi:10.1016/j.margeo.2017.09.008) does report live-coral cover, in addition to the 2017 map. Conti et al. (2019) is internally inconsistent (abstract: ~2% live and ~12.5% dead; main text: ~3.5% live and ~8.5% dead). We will report both and use the main-text values, noting the discrepancy.

After these additions the median live cover is 16.0%, close to Lim et al. (2017) at 18.35% and Lim et al. (2018) at 16.38%. Restricting the compilation to Lim et al. (2017) alone would therefore not change the upscaling.

Cordes et al. (2023) is a statistical analysis of live Desmophyllum pertusum cover on Blake Plateau mounds, which matches the parameter definition. We retain it.

(V) Coverage of exposed and buried mounds

The consideration of buried mounds does not make sense to me. Completely buried mounds will not be considered in morphological detections of coral mounds and mound density calculations (see methodological approach of e.g. Tamborrino et al. 2022), parasound or seismic data would be needed for their detection. For partially buried mounds only the part of the mound above seafloor would be detected and only this part might be partially covered by living corals. In terms of the equation provided in the paper and considering the explanations given above, the authors were never accounting for buried mounds to start with, as their coverage/density approaches are only accounting for exposed mounds. This parameter is thus, completely unnecessary. Finally, none-buried, as well as partially-buried mounds might not have any living corals on top in an entire province (Tamborrino et al. 2022) or only some of them might not have living corals as indicated by the data presented in Wienberg et al. (2018). Consequently, the consideration of buried mounds in the presented equation (Fig. 1) does not make sense. Even worse, the occurrence of coral mounds with and without living

corals on top within the same coral mound province presents an unsolved problem in any upscaling approach of carbon(ate) accumulation rates.

We agree that the present text is contradictory: line 174 says buried mounds are not considered, while an exposure factor is then applied. We will remove that contradiction.

We do not agree that burial is irrelevant. Buried mounds occupy habitable area in which living framework cannot accumulate new coral carbonate. Morphological mound counts, however, usually see only the exposed part, so multiplying those counts by a second exposure factor would double-count burial. In six of the eight mound-cover studies, buried mounds were already excluded by the method (bathymetry, video, or an explicit exposed-only density). Huvenne et al. (2003) is mainly buried (3-D seismic). Hebbeln et al. (2019) published both a total density (11.1 km^{-2}) and the exposed density used here (2.4 km^{-2}). Iacono et al. (2014) mix exposed and buried mounds and will be flagged.

We will therefore present two cases. Case 1 uses only mound-cover values that already refer to exposed mounds and drops the extra exposure factor. Case 2 applies an exposure correction only where buried mounds were included in the source area. The exposure factor lowered the global accumulation by about 35%. Case 1 is the primary estimate; Case 2 shows the sensitivity. We will also state the reviewer's point that mounds can be exposed and still lack live coral, which is why live cover is a separate term and why province-scale ratios of live to dead mounds remain a real, unsolved uncertainty.

(VI) Intersection of datasets in the global carbon accumulation equation (Fig. 1)

In the equation presented in figure 1, several datasets are used, each for itself presents important information for the evaluation of CWC reefs as carbon sinks but the intersection of these datasets is difficult. For example, the Habitat Area can be modelled and the Mound Covered Area can be calculated with bathymetric surveys but coral mound provinces are patchy and do not cover homogenously the Habitat Area.

Currently, the coverage of coral mound provinces in the Habitat Area cannot be estimated.

The same problem accounts to the Mound Covered Area and the live Coral Cover. Both datasets can be produced and datasets are available for both but there is no information on how many coral mounds have a living coral cover within a coral mound province and if this ratio varies from province to province (is to be expected) or is rather stable.

We agree that mound provinces are patchy and do not fill the habitat uniformly. The mound-covered fraction is the term that expresses that patchiness. Nine surveys is a small sample, but it is not "no information": large surveys give 3.4–3.5% cover, with a variance we propagate. An uninformed prior would be any value between 0 and 1; the surveys narrow that range. We disagree that the intersection cannot be estimated. It can be estimated only as a percentage with a wide uncertainty, which is what the paper does.

The same applies to live cover within mound provinces. Several studies measure that ratio. Combining them gives a first statistical estimate and its variance, not a claim that the ratio is spatially constant. A province-by-province intersection of habitat, mound cover and live cover would be valuable. It is not required to show that, once the published fractions are multiplied, the global coral-carbon flux is unlikely to be significant and is uncertain by orders of magnitude.

(VII) Carbon cycle discussion

Fossil fuel combustion transfers carbon from the lithosphere to the atmosphere. Main focus of carbon cycle research is the evaluation of atmospheric CO_2 sinks, hence the partitioning of carbon between the short-term reservoirs: atmosphere biosphere and hydrosphere (short-term carbon cycle; e.g., IPCC report; Friedlingstein et al. 2020), as this exchange processes are extremely relevant to reduce atmospheric CO_2 concentrations on short time scales (years to hundreds of years).

On a long run, only the carbon transfer back into the lithosphere (Holmèn 2000) balances the anthropogenic fuel combustion.

Therefore, an improved understanding and budgeting of carbon, organic and inorganic (carbonate) carbon, accumulation is essential.

Consequently, the cold-water coral reef carbon accumulation rates should consider organic and inorganic carbon (see Greiffenhagen 2025, 2026) and should be compared rather to the other long-term carbon sinks (reefs, peat, etc.) that transfer carbon back into the 'lithosphere' reservoir than to the partitioning rates between the short-term reservoirs.

Additionally, ignoring the hemipelagic background carbon(ate) because '*it represents part of the general pelagic sedimentation associated with marine primary production rather than carbonate production by CWCs*` (p. t, lines 129 – 130) is in my opinion wrong.

The background sediment accumulation rates in coral mounds exceed often significantly the sediment accumulation rates of the adjacent seafloor (in Greiffenhagen et al., 2025; 2026 they can be 1 order of magnitude higher in coral mounds with respect to the surrounding seafloor).

Consequently, also the background sediment carbon accumulation rate is enhanced and should be considered. Finally, as carbon is also transferred on the adjacent seafloor into the lithosphere, only the additional carbon accumulation should be considered.

We agree that fossil-fuel carbon is a transfer from the lithosphere to the atmosphere, and that the policy-relevant question on annual to centennial scales is partitioning among atmosphere, biosphere and ocean. This paper estimates a different quantity: inorganic carbon fixed by cold-water coral calcification and retained in intermediate sedimentary storage. It is a contribution to the active marine carbon cycle, not a closure of the long-term lithospheric budget. Weathering, carbonate burial over geological time, and tectonics dominate that long-term balance and are too slow to offset industrial emissions or to describe the ice-age cycle. We will make this distinction explicit and will not compare the estimate to peat or other long-term lithospheric sinks as if it were an atmospheric-drawdown term.

We disagree that hemipelagic background carbon should be added to the coral flux. In mound provinces, on-mound sediment is often laterally advected, baffled or winnowed material that was produced elsewhere, not a new carbon source. Adding it would double-count carbon already included in pelagic burial. On-mound rates can exceed adjacent off-mound rates because of baffling, but off-mound rates are themselves a poor measure of regional supply: the same currents that deliver sediment also cause non-deposition, erosion and hiatuses, and coral ages and matrix ages are frequently offset (Hebbeln and Wienberg, 2025; Wang et al., 2021; Dorschel et al., 2005; Eisele et al., 2014). Those processes are why a reliable "additional" background flux cannot be separated with the existing cores. We therefore restrict the upscaling to coral carbonate, state the omission as a systematic boundary of the estimate, and do not treat the potential background enhancement as coral-produced carbon.

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