Response to Reviewer 1

We thank the anonymous reviewer for their constructive comments and thorough review of this manuscript. Below are the reviewer's comments and author responses in blue italics.

The authors explore the stable oxygen and carbon isotope heterogeneity in an extremely under-studied cold-water coral taxon – stylasterids. They present high spatial resolution δ18O and δ13C results from individual corals that show highest values, closest to equilibrium seawater values, in the center of the two corals examined. This heterogeneity implies that potentially large differences in reconstructed seawater temperature could result from sampling different parts of a single coral specimen. The motivation for the paper is sound; it is clearly of great importance to understand the isotopic chemistry of coral skeletons so that they can be accurately used as temperature proxies, and we can understand their biocalcification mechanisms.

The paper uses two examples of the genus Errina, one dead and one alive. The genus Errina has been studied in prior publications e.g. Samperiz et al. (Rosenheim was a co-author on this study), and Wisshak et al., The main findings in the paper directly contradict these earlier studies, so it is particularly important that all options are explored to explain the new observations. For example, it is unclear whether the effects seen in this study are a feature local to this site, or to Errina fissurata. Given the contradictory results between Errina specimens, then it is important to avoid generalized statements about sampling strategy for temperature proxy work (e.g. centers only). Indeed, the current advice to use the white material from the center will not hold in samples which do not exhibit this coloration. The authors hint that this may be a mixed mineralogy species - mineralogy is likely a key factor for explaining the different findings between studies and needs to be addressed in more detail in the discussion.

The reviewer brings up a good point here in that we did not explore every option that could affect the stable isotopic records like diagenetic or mineralogic effects. We are editing our discussion to include mineralogical data collected on different coral specimens from the same sample collection. In doing so we are including an additional author, Dr. Noel P. James (Queen's University), who conducted these analyses. These samples were analyzed in parallel with our work, but not included in the initial manuscript because they were not from the same exact specimens. However, they are useful to address the reviewer's comments.

In terms of the generalized statement about sampling the center white portion of these corals, we understand that not every coral will have the same coloration. The reviewer mentioning this has demonstrated that we need to clarify that we only recommend this sampling scheme for the corals in this study. We will thoroughly edit our manuscript to reflect this. What is important for others to investigate, however, is whether there are similar isotope patterns in other corals.

The paper omits to reference or draw on a number of stylasterid publications – including those by the authors, that give some sense of the age (e.g. Millar et al 2014, King et al 2018, Wisshak et al 2009) needed for the modelling, and information on biomineralization and diagenesis (e.g. Black and Angus 2012, Stewart et al 2021)

These publications were not included in our growth model because we were looking for rates of radial extension for individual branches, not overall vertical/axial extension as described by these works. These publications, and others, note varying vertical growth rates from ~0.2 mm/year to 7 mm/year (King et al., 2018; Miller et al., 2004; Chong and Stratford, 2002; Stratford et al., 2001; Wisshak et al., 2009), therefore, we looked for any other records of deep-sea branching corals for realistic rates of radial extension. However, another reviewer had similar comments and suggested that we incorporate an

estimate of radial growth using the distance between radiocarbon dates and the radius of the branches from King et al. (2018). Therefore, we will include this calculation.

The recommendation is that this manuscript should be published but that the discussion and conclusions should to be revised. Below are some points to be considered.

Abstract:

Remove statement that the study identifies the optimal location for sampling for paloeceanographic studies.
We will edit this statement to clarify that we mean for the corals in this study.

Consider removing the final line, as it is not clear that the paper will motivate advanced visualization studies.
As the final line is written, we state that this work will inform the advanced visualization techniques (i.e., inform where to apply computed tomography (CT) scans), not motivate new studies. We have reworded it for clarity.

Introduction

Line 32: suggest "...fidelity of elemental and isotopic records archived..."
Done

Line 35: Simplify sentence. "...decades to millennia, which is especially useful to reconstruct ocean circulation patterns during the time that the coral was alive."
Done

Line 56: Suggest delete "...and constructed the most comprehensive model for biomineralization to date." Others may disagree that this is the most comprehensive model, and updates have been published in the intervening years.
Done

Line 58: "have a strong linear relationship".
Done

Line 59: technically corals are trending "away" from equilibrium where they would be if it weren't for biological processes.
We will clarify this sentence.

Line 70: this sentence needs to be revised to be clear that the sampling strategy for the stylasterid is similar to bamboo corals, not that the corals are similar.
We agree, done.

Line 74: These statements about stylasterids should be reconsidered. These stable isotope values in these corals generally fall closer to seawater equilibrium than the well-studied Scleractinia suggesting that these heterogeneities in stylasterids are relatively smaller (Samperiz et al., 2020; Stewart et al., 2020 EPSL). These findings should act as strong motivation for this study however, as understanding what is causing these small deviations from equilibrium will yet further refine what are perhaps the most promising coral temperature proxies – stylasterid d18O and Li/Mg. The authors should also consider refer to Stewart et al., 2021 Sci Rep. which highlights the contrasting calcification behavior of stylasterids compared to Scleractinia.
After reading this reviewer's comments, we see how the end of this paragraph concentrates more on the complications of stylasterid corals, and we believe the comments broaden the potential impact of this

paper. We will edit this section to include considerations for scleractinian data which demonstrate the potential for these stylasterids. We will include the publications the reviewer suggested.

Methods

Further information is needed on the sampling sites. What are the local d18O and d13C values in seawater? Are there strong gradients? I have been back through a related publication from King and Rosenheim to try and ascertain the connection between sites. I think that some of the samples are from the same cruise or even dredge, but maybe not the same samples. Why is this? Did you look at the samples from the prior publication? I was surprised not to see the age information derived from that paper not being used in this publication, and not included in the reference list. It surely needs to be cross –referenced and compared. One of the main conclusions of that paper was that changes in the water masses could be identified – can we see any of those changes in these two new corals?
Regarding the local seawater stable isotopic ratios, we don't have data directly from the coral sampling site. Instead, we've used nearby stations for $\delta^{13}C$ and $\delta^{18}O$ seawater values and the average for the depth range are listed in the supplement. We will edit the supplement to include each discrete depth to provide the reader with additional context. We thank the reviewer for their diligence as these corals were collected from the same cruise and Ross Sea dredges as the previous publication by King et al., 2018, they are just different specimens. The complication lies in that we are not certain from which dredge the corals were collected (D05, D06, D07, D08, or D09). This is because we performed the geochemical analyses after receiving the corals from a now deceased collaborator and we have not been able to locate sample collection information recorded during that cruise. We will clarify the methods.

We did not incorporate any of the age information from the previous radiocarbon work for these corals because the radiocarbon was not an independent chronometer. In the previous publication, we demonstrate that the radiocarbon record is influenced by an older water mass impinging on the corals, i.e., the reservoir age changed. The age of the corals that we calculated was only applicable if we disregard that change. If we compare the length of these coral specimens to those in our previous publication, they are similar, and the corals here may span a similar range of time, but we don't have a direct age control. Further, the temperature calibrations we present in Figure 9 shows little evidence for the incursions of Circumpolar Deep Water (which would be a warming signal) during the time the carbonate analyzed was deposited. Therefore, this work neither supports nor contradict the earlier work because it may be too old or too young to have recorded a water mass change.

A new panel or figure needs to be added showing the water column properties for the two corals, so that it is easy to identify the potential for changes in temperature and d13C over the lifetime of these corals.
As mentioned above, the seawater data used here is from a station nearby and not exactly where the corals were sampled. We will add the isotopic data through the entire water column to the supplemental material.

How old was the dead coral? A radiocarbon date would be helpful here.
An age constraint would be a nice addition to this work, but we don't think it's necessary to explain the isotopic trends observed in the corals.

Line 110: presumably the 8mm section was "d". Please say this here.
Done

Line 155. 'This variability is unexpected is we assume that each slice / cross section reflect deposition at the same point in time' This assumption does not seem to be likely, given that the corals probably live for at least 100 years.

We agree and have changed the wording here.

Discussion

Figure 4 and general discussion. The authors rightly make comparisons to previously available data. These data also need to be shown on the figure. Relating to the earlier points above, it would also be helpful to compare to bamboo coral and scleractian coral data to highlight the differential amplitude of the ranges in the isotopic data.
We agree that it would be beneficial to include other data for a direct comparison to our records. We will edit this figure to include data from previous publications.

Line 225: delete space in 0.6 8 ‰
Done

Line 231: This is a crucial point that needs to be expanded. Stylasterid mineralogy can indeed have a significant influence on their geochemistry. The manuscript would benefit from outlining this in the introduction, followed by more detailed exploration in the discussion and as a requirement for analyses in the conclusions. The fact that stylasterids can build skeletons from calcite, aragonite or both is strange and further highlights the need to study these corals. The big question, though, is what is the mineralogy of the Errina fissurata specimens used in this study? Cairns and Macintyre [1992] record an Errina fissurata specimen from a similar (maybe the same?) location to that represented here, which is composed of 91% calcite and 9% aragonite (sample 40 in their paper). Samperiz et al. [2020] showed that calcitic stylasterids have generally lower d13C and d18O than aragonitic stylasterids. Therefore, this raises the possibility that variable mineralogy in these specimens (e.g. increasing percentage of aragonite toward the center of each section) could explain the isotopic trends observed. Could changing mineralogy also reconcile the new data with the contrasting trends found by Samperiz et al. and Wisshak et al.? The authors hint at some data surrounding the mineralogy of these specimens, and the manuscript would benefit from more detailed discussion of this theme.
Yes, we completely agree here. We had previously acquired some mineralogical data, but not from the same specimens that were analyzed for stable isotopes (data herein) or radiocarbon (from King et al., 2018). However, the samples analyzed for mineralogy were from the same cruise and dredge(s). After reading these comments, and similar comments from another reviewer, we feel that comparing data from two specimens is both warranted and useful. We will edit the discussion to include the mineralogical data and discuss how that affects stable isotopes. We can quantify the mineralogical change in percentage of calcite-to-aragonite from the outer portion to the center. As mentioned above, this had led us to also include Dr. Noel P. James (Queen's University) as an additional author for measuring mineralogy and facilitating the incorporation of these data into the manuscript. We can use the small change in percent calcite-to-aragonite by region (~3% more aragonite in the interior compared to outer samples of coral slices) for an interesting thought experiment. If mineralogic changes between the outer and inner sections of the skeleton we the most important factor in explaining our isotope variation across skeleton discs, the isotope composition of the new material would have to be on the order of ~-32.5‰, far in excess of any skeletal variability we observed herein. Such a negative value seems unrealistic to be the only forcing of the higher values towards the centers of each of our coral slices. Therefore, with the caveat that the mineralogical data are from a different specimen, we will discuss that mineralogy is an important consideration, however, not the main forcing of the trends we observe. Only a small amount of additional "new" calcite is needed to be within our observed range of skeletal isotope composition, however the mineralogical data do not support a larger change in mineralogy.

Line 232: see above, suggest changing “additional work needs to be done to approximate the mineralogy over the sampled discs” to focus on how mineralogy might change across the sampled discs.
Again, we agree and are including additional mineralogical analyses.

Line 255. Using a bamboo coral growth rate is not appropriate here. As the authos point out, Stylasterids fall within the Hydrazoa, whereas bamboo corals fall within the Anthazoa. They are very different organisms which calcify very differently to one another. There are some age data for stylasterid corals – including data from the authors of this paper (King et al 2018) and others. These need to be cited and used rather than drawing on bamboo corals. Likewise on Line 245 it is not clear that the data from scleractinia can support the biomineralization models proposed given the large differences between them.
As mentioned above, to calculate the calcification models we needed rates of radial extension for the coral stems and individual branches, not overall vertical extension as described for stylasterids (e.g., King et al., 2018). Therefore, we chose to rely on the bamboo coral data as they were the only we could find describing the radial extension of deep-sea branching corals. Additionally, the specific value of the growth rate doesn’t need to be completely accurate for the purpose of demonstrating the growth patterns we were hypothesizing. We only needed a constraint that was likely in the range of possible values.

Line 275 – Which data point to a slow down in growth rate for stylasterids, please provide reference.
The line mentioning “the available literature supports slowing growth with time” is referring to the previous paragraph where we describe that marine calcifying organisms generally slow their growth with time. We will reword this to clarify the meaning and cite appropriate references.

Line 280: The biocalcification modelling is a nice addition, and explains the results of this study, but currently this section doesn’t appear to reconcile the results of this study results with the highly contrasting findings in Samperiz et al., and Wisshak et al., who both find stable isotope values tending towards equilibrium towards the outer edge of Errina sp.samples. More discussion/explanation is needed here. Is this a sampling / species / mineralogical / ontogenetic / location effect? Not all samples have the same white / pink delineation, making the proposed sampling strategy challenging.
We appreciate this comment, but the modelling was not constructed to reconcile the results here with published results. The models were to represent the trends we expected based on those published works and how we would expect the growth to happen based on the trends we observed here. We are adding discussion about mineralogical analysis, and we will also add discussion about a possible species affect. We know that many stylasterids exhibit visible growth rings, which demonstrates the variability among the taxon. We are aware that not all are stylasterids have the white/pink delineation as well. Therefore, we will be sure to clarify our discussion where we recommend the sampling scheme for this widespread species but may not be suitable universally.

Line 301: delete extra full stop
Done

Line 304: The authors could consider comparing their suggested mode of growth – fast initial growth to form a framework followed by slower growth focused in the centers of branches – with that suggested by Wisshak et al. [2009]. Wisshak et al. show evidence for skeletal reorganisation – including dissolution and re-precipitation - during stylasterid growth, following the initial skeletal precipitation. Although the isotopic trends and interpretations of growth models may differ, overall this study and Wisshak et al. appear to be suggesting similar processes. More discussion of this here could lend more support to the authors ideas. It would also be really interesting to know whether there is any visual support for the suggested slow infilling around the initial framework, starting in the central region of each disc? It’s hard

to see from the photos in figure 3, but is there anything in terms of pore size, structure or general appearance of the carbonate which supports this theory?
Yes, we agree and will add discussion incorporating the growth described by Wisshak et al. (2009) and possible diagenetic effects as this work has been essential to stylasterid studies. We don't have any visual data but will be conducting SEM imaging soon to investigate. We will look for any aragonite crystals in different regions of the corals and compare that to previous work as well as our isotopic records. The results will be incorporated in the updated manuscript.

Line 315: While the results of this study alone imply that the best place to sample thesecorals for temperature proxy work would be the centers, this result is not applicable to all Errina specimens. Samperiz et al. (including authors from this paper), and Wisshak et al., studies find the exact opposite to be true. Until the cause of this discrepancy is established it is premature to suggest a recommended sampling strategy, especially given different banding and coloration patterns in different specimens.
As mentioned above, we will be sure to edit the wording so that we specify our sampling recommendations only apply to the species analyzed here.

Line 365: It is not clear that the new results assuage hesitation surrounding the influence of vital effects. The studies by Samperiz et al., 2020 and Stewart et al., 2020 already did this when they showed that bulk sampling of stylasterids provided highly accurate d18O and Li/Mg temperature proxies compared to the existing scleractinian coral calibrations in the literature. The current study has shown that internal heterogeneity and vital effects in stylasterids are more complicated than previously thought and more study is needed to ascertain if this is because of mineralogy / species / location etc
This reviewer has made a few comments to this affect, and it demonstrates to us that we need to clarify the language throughout the manuscript. We will be sure to assess every time we mention "stylasterids" as a taxon and be sure that they are not used as an umbrella term when we mean to refer to one species.

Figure 1: It would be useful to have the coral ID's labelled on the figure rather than the dredges. Also, there are 5 dredge sites and just two corals used in this study which is confusing. It would be very useful to compare the sites in King et al 2018. An additional panel or figure is needed with the water column data and indications of the variations we might expect in d13C relating to different local water masses
We will adjust the figure to label the coral ID's and not the dredge numbers to reduce confusion. As mentioned above, we are unable to determine the specific dredges from which the corals were recovered. We also mentioned above that we used nearby stations for $\delta^{13}C$ and $\delta^{18}O$ seawater values for our equilibrium calculations. We're not convinced that including a water column profile in the figure will benefit the readers as we won't have the values from the exact study location. We will, however, edit the supplement to include each discrete depth to provide the reader with additional context.

Figure 3: "largest isotopic values" should be "highest isotopic ratios"
Done

Figure 4: this figure should include previously published data.
We will add additional data to this figure.

Figure 5: Similar microsampling data by Samperiz et al., and Wisshak et al., should be included on this plot for direct comparison of absolute values between studies (e.g. Fig 7 Samperiz et al., 2020). This will highlight the contrasting isotopic results for the central part of the coral in these studies and the current study. It will also show how low the d13C values are in this study compared to the other measurements of Errina in the literature.

We will add these data to the figure.

Response to reviewer 2

We thank the anonymous reviewer for their constructive comments and thorough review of this manuscript. Below are the reviewer's comments and author responses in blue italics.

Stable isotope measurements (d18O and d13C) from deep-sea corals provide valuable paleotemperature reconstructions through the water column at all latitudes (unlike shallow-water corals, geographically constrained). Yet, deep-sea scleractinian and bamboo corals have shown complications derived from "vital effects" that deviate the environmental signal in the isotopic records. A new coral taxon (Stylasteridae) has been considered as an alternative archive, as "vital effects" in these specimens have been reported to be lower than those in other deep-sea coral taxa. This study, however, shows that "vital effects" and their impact on the skeletal isotopic composition of stylasterid corals might not be as straightforward as previous studies have shown.

This study carries out fine stable isotopic (d18O and d13C) mapping on several cross-sections of two specimens of Errina fissurata. Results show that sections closer to the growing tip of the colony present more depleted d18O and d13C than sections further down the branch. Equally, those samples located near the centre of each cross-section (branch) showed values that were closer to equilibrium than those samples from the outer areas of the cross-section. Importantly, these results contradict observations from previous papers. The authors present a growth model where an initial skeletal framework form the main structure of the branch (quick biomineralization) followed by slow mineralization of the inner sections for structural strength and argue that this is the source of isotopic differentiation across all cross-sections. This work points towards the need of a deep understanding of the growth mechanisms of Stylasterid corals (or Errina sp. in particular) in order to obtain more precise paleoreconstructions and introduces an strategy (isotopic mapping) to locate the skeletal area closest to equilibrium, and therefore the areas to sub-sample for the aforementioned reconstructions.

This is an interesting piece of work that deepens our knowledge of a newly explored paleo archive (Stylasterid corals), focuses on the need for further research regarding skeletal growth and geochemical composition and presents new information on the stable isotopic composition of skeletal material. The data presented by this manuscript is of importance for communities in the fields of paleoclimate, and marine biomineralization and calcification and as such, it should be published. However, a more thorough discussion, including data from previous publications and expanding on concepts like the role of mineralogy on the reported results should be considered and included. See below for some points that can improve the strength of the manuscript.
We noticed similar themes between both reviewers (e.g., incorporation of coral mineralogy, clarification of methods, and incorporation of previously published data) and agree that these areas need the most attention during our revisions.

1. Introduction

A more extensive literature review on stylasterid corals, and more specifically previous geochemical publications of this taxon and its positive results for reconstructions, would help making a stronger case

on why keep focusing efforts on these specimens. This can be included either towards the end of the third paragraph or in the fourth.
We agree and will add this section.

2. Methods

L. 96: It is unclear from which specific dredging the two samples come from, or whether dredging D05 to D09 was done consecutively and there is no possible way to know the exact depth of the sample. This needs to be specified.
Yes, as written, it is not clear. The dredges were consecutive, but it is unclear to us where exactly the corals were collected (from dredge D05, D06, D07, D08, or D09). This is because we performed the geochemical analyses after receiving the corals from a now deceased collaborator and we have not been able to locate sample collection information recorded during that cruise. We will clarify the methods.

3. Results

A table in results summarising average d18O and d13C from each sample (or section as described by the text) coupled to the average environmental data used for both samples (Temperature, d18Osw and d13Csw) would help the reader to quickly grasp variability (or lack of) of the data between samples and the environmental conditions. I might be wrong, but I think seawater temperature is not specified before Figure 9 (the very end of the MS) and would help contextualise the environment and the discussion later on when comparing with work in the literature if included in the results.
We agree that adding a table would be an easy way for potential readers to make those comparisons and will include one. The data are compiled in the supplemental information, but we will provide averages as suggested by the reviewer. This reviewer is also correct in that the seawater temperature is not mentioned before Figure 9 and we will be sure to include that in the section about sample collection as well.

4. Discussion

4.1. Isotopic disequilibrium

This is a nice section that sets the argument for consecutive discussion on growth models and paleo reconstructions. However, I feel that a deeper comparison of the data of this manuscript with published d18O and d13C from stylasterid corals needs to be addressed (beyond the minimum offsets from equilibrium and d13C–d18O slopes). Samples here show a wider range of both d18O and d13C than those in Samperiz et al. (2020) and Wisshak et al. (2009) for aragonitic specimens. Importantly, the d13C here reach levels similar to those of calcitic samples. Whether this is an effect of much finer sampling, mixed mineralogy, or other potential artifacts, it needs to be discussed more deeply in this section.
We agree and will be enhancing the discussion section as per this reviewer's and the first reviewer's comments. We plan to include previously published data (from both Samperiz et al. (2020) and Wisshak et al. (2009)) into Figure 4 and Figure 5. In addition to this we will be incorporating a discussion on mineralogy as well.

L. 205: What is the lifespan of stylasterid corals? This information can be added either here or in the introduction. For reference, observed axial growth rates from King et al. (2018), Miller et al. (2004), and/or Wisshak et al. (2009) would extrapolate to lifespans of >100yr (and up to 400yr) for Errina sp.

colonies. Despite this method presenting several caveats, it is useful for the reader to understand that these colonies can be long-lived.
We agree and will add that information.

L. 225: Maybe I misunderstood. 0.68‰ and 3.95‰ are offset values from calcite or aragonite equilibrium? It doesn't change the observation but would be good to specify. Especially since the comparisons with other data (Samperiz et al. (2020) and Wisshak et al. (2009)) are made for aragonite equilibrium, and they show that calcitic samples show a larger offset from equilibrium for d13C.
The offset values are from calcite equilibrium. We agree that this should be clarified and will adjust the text to be sure the proper comparisons are made.

L.226: The minimum offset from Samperiz et al. (2020) is from bulk sampling or cross-section analysis within one specimen (similar to this study?). Worth being specific here.
These values were compared to the cross-section analysis, but we will specify.

L. 231: Authors comment that they have evidence for mixed mineralogy of these specimens. What is this evidence? This needs to be expanded. Work by Samperiz et al. (2020) and Stewart et al. (2020, 2022) have shown how sample mineralogy have a great impact on elemental and isotopic composition. This sentence is the only mention to mineralogy in the manuscript; however, it is known how sample mineralogy is one of the main caveats to the use of stylasterid specimens. A more thorough discussion needs to be included on the potential effects of mineralogy on these results, especially if mixed CaCO3 polymorphs have been observed. This is an interesting point that needs to be considered and will enrich the manuscript.
We completely agree here. We have some mineralogical data, but it was not generated from the specimens that were analyzed for stable isotopes, so it was excluded. However, the samples analyzed for mineralogy were from the same cruise and dredge(s). After reading these comments, and similar comments from another reviewer, we feel comfortable including the data we have. We will edit the discussion to include the mineralogical data and discuss how that affects stable isotopes. We do have evidence for a mineralogical change in percentage of calcite-to-aragonite from the outer portion to the center. In adding to the discussion, we are going to include an additional author, Dr. Noel P. James (Queen's University), who conducted these analyses. We have also done some preliminary calculations based on the small change in percent calcite-to-aragonite by region (~3% more aragonite in the interior compared to outer samples of coral slices). This change in mineralogy could only explain the isotope values we observe if the oxygen isotope ratio of the new calcite was ~-32.5‰. This seems unrealistic. Therefore, with the caveat that the mineralogical data are from a different specimen, we will discuss that mineralogy is an important consideration, however, not the main forcing of the trends we observe. Only a small amount more of new calcite would have to be precipitated to bring isotope values to within our observed skeletal variability, but such proportions would not be parsimonious with our mineralogic data.

4.2. Isotopic trends and calcification models

I enjoyed reading through this section. It is clear that new research needs to be directed towards modelling of stylasterid growth to clarify vital effects patterns. The addition of a simplified model with three scenarios (regular growth, ontogenic decrease and increase of radial growth) is helpful for the reader.
Thank you!

L. 256: The authors use published radial growth rates from Bamboo corals in their models, justified by the lack of data on growth rates from Errina sp. (or even any Stylasteridae coral). Assuming the authors still have access to the samples dated in King et al. 2018, would it be possible to roughly extrapolate radial growth across two dated points within a branch and differences in diameter? This data, although rough, could shed light on whether growth rates of Stylasteridae are similar to those employed here. This information, coupled to lifespan of the colonies (see above, L. 205) will be useful and interesting for future research.
This is a great suggestion to get a radial growth rate for these corals. We will include a calculation of radial growth based on the distance between radiocarbon dates and the radius of the branch.

L. 274-275 "…as the available literature supports slowing growth with time.": Can you specify the literature (add references)? Is this referring to Stylasteridae or marine calcifiers in general?
Yes, we will add those references. The statement was referring to marine calcifiers in general.

L. 300 and below: Building the growth of Errina sp. on growth models from scleractinian corals (or Acroporids) can be problematic. Scleractinians have shown to calcify from centres of calcification/amorphous crystals/ fusiform crystals, from where aragonite needle-like bundles grow (e.g., Gladfeiter 1982). In Scleractinia, these calcification areas show distinct isotopic signature (e.g., Adkins et al. 2003). However, these centres of calcification or growth framework has not been observed in Stylasteridae corals and therefore is hard to argue they are the cause for isotopic differentiation.
Whereas the calcification described by Adkins et al. (2003) applies to the solitary coral Desmophyllum cristagalli, the calcification described by Gladfelter (1982) is for a branching scleractinian coral. From our understanding, the centers of calcification in the D. cristagalli are different from those of the A. cervicornus. We don't argue that there is the exact same calcification mechanisms, only that there is a change in calcification rate: faster near the outer edges and slower near the center. We can adjust the wording to be clearer in this section.

Wisshak et al. (2009) discussed skeletal architecture and skeletal reorganisation and it is an important source to cite and consider when studying structural growth of Errina sp. This work needs to be included in this section. Wisshak et al. (2009) explain structural growth of Errina dabneyi based on a 2-step model also, with the coenosarc canal network in the middle area of branches being simultaneously dissolved (wider-canals) and infilled (secondary precipitation) as the skeleton thickens. Although several questions remain on the nature of this secondary material, this growth model needs to be considered in this section of the discussion.
We agree and will incorporate this into the section.

Is there imaging showing the two-step infilling process described in this manuscript? SEM images similar to those in Figure 6 from Wisshak et al. (2009) would be helpful to discern the two-step growth. Maybe observations made during the SEM analysis. But white-light images could be useful too. Wisshak et al. (2009) describe how ampullae are more common in the outer layers, while old ampullae towards the centres of branches could be seen infilled. By the pictures of Figure 3 it would seem like that is the case (no ampullae in the inner sections), but a closer inspection could be beneficial. Just a few sentences signalling whether any of the observations made by Wisshak et al. (2009) are visible on these specimens (or not at all) will be valuable information contributing to the understanding of Errina fissurata growth.

We agree with this too and have made arrangements to take some SEM images looking for such features. There are a few coral slices that appear to have more ampullae in the outer layers, but we will take more comprehensive images and incorporate these results into the updated manuscript.

L. 304 "We posit that this model accurately described the stylasterid coral growth…". Disagree. This growth (outer framework and later infilling of the centre) does not explain observations by Samperiz et al. (2020) and Wisshak et al. (2009), therefore stylasterid coral growth is still largely unknown (L. 311).
We will reword this section to clarify that we posit the growth model could apply to the specimens in this study specifically.

An expanded discussion of sample mineralogy (as specified above, L. 231) coupled to the two-step growth model will be beneficial in here (as a paragraph or a new section within the discussion by itself). While Samperiz et al. (2020) and Wisshak et al. (2009) confirm their Errina antarctica and Errina dabneyi samples are 100% aragonite, authors hint at a mixed mineralogy here. I appreciate mineralogical mapping (e.g., Raman) or even bulk XRD might not be possible for these specimens of Errina fissurata in this manuscript. However, the possibility of the centre infilling to be mineralogically distinct from the initial framework needs to be considered as a source of discrepancies between results in this study and others published, gaining a more thorough discussion on the growth of Errina sp. Both Samperiz et al. (2020) and Stewart et al. (2020 and 2022) noted geochemical differences among calcitic and aragonitic Stylasteridae.
We agree. As mentioned above, we have mineralogical data that we will incorporate. This consists of some XRD measurements in the center and outer region of the corals. We will include these data and expand the discussion to include the mineralogy and SEM discussions. See replies for L. 231 above for more details.

4.3. Considerations for paleoceanographic reconstructions

L. 314: Please, specify that the white centre is the ideal region to sample for paleotemperature reconstructions "in the samples of this study". Other samples in the literature show the opposite behaviour, and therefore this cannot be extrapolated to every Errina sp. specimen. This might be species specific effect, or site-specific, or even specimen-specific.
Yes, we will do this. This comment echoes those from another reviewer on the common theme that we need to specify this study.

L. 338: "If finer-scale samples were informed with CT scanning methods…". Maybe I have missed it, but it is not clear to me what finer structures I should look for in the CT images to improve reconstructions. Is this denser or lighter skeleton because it would be an indication or more or less secondary infilling of the initial framework? A sentence here clarifying would be useful to guide future work.
The CT scanning methods could illuminate structures like growth rings that are invisible to the naked eye or microscope. Additionally, the density differences between calcite and aragonite could be determined spatially. We will mention this and discuss it within the context of the mineralogy work that will be incorporated into the updated manuscript.

L. 342: "We recommend sampling of the white centre using more spatially precise micro-milling methods…". As mentioned above, sampling the white centre would work for these specimens, but not for other published data. In addition, the white centre limits the application of this technique to specimens showing distinct coloration on its cross-section. As an example, the coenostum of Errina

dabneyi sampled by Wisshak et al. (2009) was pure white, potentially not showing a distinct branch centre. I would be very precise specifying that this technique cannot be universally applied to every Errina sp. However, a fine spatial analysis on a cross-section will be useful to inform on isotopic distribution of new samples, regardless of skeletal coloration. In my opinion this is a very important point that this manuscript raises.
We agree and will improve the section so that we are very specific about applying this technique to this species of coral and our recommended sampling scheme is not universal.

L. 355: I would also suggest including literature of CT imaging of stylasterid corals (Stylaster sp.) showing skeletal structures (e.g., Puce et al. 2011). We know that these are structurally very different from Corallum sp., and it is not certain they follow the same growth pattern. Furthermore, skeletal structure seems to differ even between Stylasteridae genera. CT imaging will be very useful to discern growth patterns before reconstructing temperatures indeed.
Thank you for this recommendation, we will incorporate this into the discussion.

L. 356: would improve “and?” allow an even closer approach…
Great catch, we will fix this line.

5. Conclusions

L. 380: Please, change “we recommend sampling along the centre, white region where the infilling has allowed for calcification closest to seawater equilibrium…”. In my opinion the evident recommendation emanating from this study is the need for spatial sampling to localise the skeletal region closest to equilibrium, in contrast to what was proposed for example in Samperiz et al. (2020) or Stewart et al. (2020) (i.e., bulk or surface sampling).
We agree and will adjust the text.

Figure 4: Add circles and squares for data from each specimen of this study (similar to Figure 5 and 9). This will help quickly localise differences across samples (or lack of thereof). Equally, including in this figure data from Samperiz et al. (2020) and Wisshak et al. (2009) would be beneficial to framework what is discussed in this section (i.e., the offset from equilibrium, differences in isotopic signal across literature sources and what might be caused by).
We agree and will add the data and make the adjustments to the figure.

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