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.