Review #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.

These comments refer to the new uploaded manuscript (rather than the tracked changes file). I noted some discrepancies between the tracked changes and the revised manuscript, and I assumed (maybe incorrectly) that the tracked changes file was not a final version.
This was a correct assumption, apologies for the confusion.

After an initial review, some of the comments and concerns regarding this manuscript have successfully been addressed. The introduction is now more informative on stylasterid corals and the missing (but crucial) data on specimen mineralogy has been included.
We have noticed common themes between both reviewers' comments in this round of revision. One of them is the need for improved discussion of mineralogy. We have taken these comments into serious consideration and have thoroughly edited the introduction, results, discussion, and conclusion sections to all include mineralogy.

I still think this data is of high value for the scientific community, and as such it should be published. However, the discussion of this manuscript (MS) is still lacking depth and details when it comes to differences between this manuscript and previously published data which is the most important and interesting perspective this new data (and MS) provides.
Thank you for this perspective. We have spent considerable time reworking the discussion of this manuscript to highlight the differences between what we are presenting and previous work.

Introduction

There is no mention of mineralogy within stylasterid corals in the introduction. It is hard for a non-expert reader to then understand why mineralogy is tested, and the significance of mixed mineralogy discussed later in the manuscript.
We agree that the importance of mineralogy was underrepresented throughout this manuscript and the introduction has been reformatted to include its significance.

Line 41: fidelity.
Done

Line 74 "Samperiz et al. (2020) noted variability among the δ18O […] within a single growth band." This statement is not accurate. Samperiz et al. (2020) noted a variability of <0.50‰ for δ18O within a single growth band. Unlike δ13C (variability of ~3‰). This statement seems to point that variability of both δ18O and δ13C within a single growth band was similar. Need to be precise.
We see how this statement is misleading and will take care to use more precise wording for the next submission of this manuscript. We have reworked the introduction, and this statement has been removed entirely.

Methods

Line 116: Here you state that that species level identification was achieved through SEM imaging. Later, in Line 433 you state that the samples for this paper were not analysed using SEM, but other specimens were (I assume that this other specimen is where Figure S3 comes from). Need to specify this in methods. Are the samples analysed for taxonomical information from the same dredges/locations? And the sample in Figure S3? These details are important.
We agree, this was unclear as written. This has been updated to specify which samples were analyzed using SEM.

Equally, it is necessary to identify the sample ID from where SEM was obtained for future references, reproducible science, and transparency. The same applies for any other sample analysed in this manuscript that are not EA-11 and EA12 (i.e., what is the sample ID of specimens analysed for mineralogy?).
Thank you for this comment, as you have made clear the importance of transparency when describing methods exactly. It was an oversight to not have included sample names for each step, and they have been done now.

Line 154 (?): Do you apply a calcite-acid fractionation factor or an aragonite-acid fractionation factor to gas δ18O data? This information is missing, and it is important when considering offsets from equilibrium in Figure 6 (extended in my comment below about Line 384).
Please see our response below to this reviewer's comment about Line 384.

Discussion

In **line 339 "our results do not directly support those of Samperiz et al. (2020)"; line 361 "These results neither completely support nor refute those of Samperiz et al. (2020)"**. These two lines are within the same paragraph, yet they point towards different directions. Upon reading further, the reader then realises that because of sampling differences (sampling growing tips v. slices 1cm below the growing tip), comparing results of this manuscript and those of Samperiz et al. (2020) is difficult. This is correctly stated by the authors in line 371, at the end of the paragraph. Therefore, it is very hard to affirm that "results do not directly support those of Samperiz et al. (2020)" (as in line 339).
This section was unclear as written and has been reworked, see further comments below.

This comparison, and discussion on internal variability along the growth axis is necessary and definitely enriches this manuscript. For example, the fact that maximum δ18O and δ13C in EA-12 are found in different slices is an interesting observation. However, this paragraph is long, and has contrasting statements. It is not until the very end that one understands that the comparison with previously published data (i.e., Samperiz et al. 2020) is not straightforward. This need to be re-written for accuracy, acknowledging the limitations about the comparison and being very precise about similitudes and differences in both sampling and data.
We agree that this paragraph meanders and we contradict ourselves several times. The beginning was a poor choice of words to try and develop the nuance in the differences between our results and those of Samperiz et al. (2020). We have reformatted the discussion section and have clarified the comparisons with other works. We have also adjusted this part of the discussion to more

clearly state limitations introduced by sampling strategy. This new structure flows much better for the reader, and we appreciate these constructive comments.

Line 357: Is this meant to be EA-12a?
Yes, thank you for the catch, this has been changed.

Line 383 "Samperiz et al. (2020) observed that calcitic specimens exhibited δ18O and δ13C values further from equilibrium compared to aragonitic corals": As per Samperiz et al. 2020 data (and their Figure 3), it would seem that calcitic stylasterids show more depleted values for both δ18O and δ13C, but when considering equilibrium (because it is different for aragonite and calcite), calcitic specimens are further away from δ13C equilibrium but not for δ18O. Modify this statement.
Yes, we agree that the wording was misleading regarding the isotope values relative to equilibrium. In editing this section, this sentence has been removed.

Line 384 and below "Because our calculated equilibrium δ18O and δ13C values were higher for aragonite than calcite, our data relative to aragonite equilibrium […]". When calculating δ18Oequilibrium – δ18Ocoral , it seems clear that you calculate equilibrium for calcite and aragonite (as it is different). However, it is not clear to me whether you apply different acid-fractionation corrections to your coral data depending on whether you considered it calcite or aragonite. It is my understanding that it would only make sense to compare data against aragonite equilibrium if this has been corrected with an aragonite-acid fractionation factor, and with calcite equilibrium if this has been applied a calcite-acid fractionation factor. I have mentioned this also in the methods, where it should be specified.
We thank the reviewer for this comment as it was very illuminating and interesting to read about. We agree that the acid fractionation factor (AFF) we used should be clearly stated. In our study, the acid fractionation was accounted for by measuring calcite reference materials, therefore the calcite fractionation factor was incorporated into data transfer onto the PDB scale and calculations based on those data. Regarding a strict division between aragonite AFF-corrected and calcite AFF-corrected values only being compared to their respective equilibria, there is more nuance. Whereas we agree that the aforementioned comparison is the most accurate method for interpreting stable oxygen isotope values, equilibrium/paleotemperature equations for both carbonate polymorphs often don't account for the AFF or simply assume calcite by calibrating with the NBS-19 standard (e.g., Grossman and Ku, 1986; Crowley, 2010). Such complications have been the topic of several correspondences among the isotope geochemistry community (see Lachniet, M., Pack, A., & van Geldern, R., Isogeochem listserv, April 2014; Gillikin, D., Isogeochem listserv, October 2020). To combat this confusion, we think that future works need to clearly state the AFF they use (or assume), previous paleotemperature calibrations for aragonite need to be revisited, and further temperature calibrations like that of Samperiz et al. (2020) need to be made wherein the AFF is accounted for.

Here, we consider how the different AFFs would affect the results of this study, specifically comparison to any theoretical equilibrium point in isotope space. As the acidification of carbonate with phosphoric acid only fractionates oxygen isotopes, the carbon isotopes are unaffected;

however, we see a strong linear relationship between the oxygen and carbon isotopes (Figure 5 in the manuscript). This suggests that the AFF only affects the absolute value of each $\delta^{18}O$ measurement on a magnitude not significant enough to disrupt the linear relationship between isotopes – perhaps adding noise to the relationship unsubstantially as the relationship is still clear. Further, calcite and aragonite AFFs determined by Kim et al. (2007) at 50°C (the temperature we acidified coral samples) would result in a difference in $\delta^{18}O$ values of ~0.03 ‰. This potential offset is two orders of magnitude smaller than the minimum range in $\delta^{18}O$ values (1.31 ‰) observed in either coral (Figure 4 in the manuscript). Such a magnitude of change can be significant for calibration to temperature, which is outside the scope of this work, but does not impact our interpretations of coral growth and the optimal location for sampling if one wanted to calibrate this species or genus to temperature.

References:
Crowley, S. F. (2010). Mineralogical and chemical composition of international carbon and oxygen isotope calibration material NBS 19, and reference materials NBS 18, IAEA-CO-1 and IAEA-CO-8. Geostandards and Geoanalytical Research, 34(2), 193-206.

Grossman, E. L., & Ku, T. L. (1986). Oxygen and carbon isotope fractionation in biogenic aragonite: temperature effects. Chemical Geology: Isotope Geoscience Section, 59, 59-74.

Kim, S. T., Mucci, A., & Taylor, B. E. (2007). Phosphoric acid fractionation factors for calcite and aragonite between 25 and 75 C: revisited. Chemical Geology, 246(3-4), 135-146.

Equally, throughout the manuscript, and given mineralogical results, it will be more appropriate to use calcite equilibrium (as samples seem to be mostly calcitic).
This has been done.

Line 388 "Our data are distinguished when we examine the heterogeneous δ18O and δ13C values across a coral slice". I am not sure what this sentence means. Also, not sure how it fits with what is being discussed in this paragraph.
This statement was meant to emphasize that overall, the isotopic values were within a realistic range, but were very different when considering the variability within a single slice. This was poorly written and has been removed.

Line 395 "This contradicts results of Stewart et al. (2020) wherein authors find that stylasterid corals of mixed mineralogy exhibit less variability compared to purely aragonitic scleractinians and stylasterids". In section 3.2 of Stewart et al. (2020) the authors analyze internal variability (heterogeneity) within coral samples by calculating the difference between replicates (Table 2 of their paper). The authors only report mean differences for δ18O, not δ13C, so this statement cannot be hold for both stable isotopes. Nevertheless, the mean difference in δ18O replicates for aragonitic stylasterids is 0.14‰ (for n=16), and for mixed mineralogy stylasterids is 0.13‰ (for n=4). It will be hard to argue that this difference between mineralogies is significant and mixed are less variable without carrying out further statistical analyses. Change the written statement from "less variability" to "similar variability" in line 396 so that it correctly addresses data presented by Stewart et al. (2020). The fact that data in this manuscript contrast with previously

published data remains true, yet the differences in sampling and analysing heterogeneity should be addressed here.

We agree with this comment in that we had poorly described the variability of Stewart et al. (2020). In editing the discussion section, we removed this section because upon rereading Stewart et al., it was unclear if the mixed mineralogy specimens they present were truly mixed. The authors state that all $\delta^{18}O$ and $\delta^{13}C$ values and associated mineralogical data were from Samperiz et al. (2020). The confusion lies in the mineralogy of the E. gracilis specimens. The supplemental information provided by Samperiz et al. (2020) do not state the mineralogy of these corals, they are listed as "na". It is unclear where the mineralogical information came from for E. gracilis. Stewart et al. could have done the analyses or gotten more information from a personal communication from Samperiz et al., but it was not stated. Therefore, we chose to eliminate a reference to Stewart et al. mixed mineralogy corals.

Line 397 "[…] but they are characterized by the largest stable isotope variability.". This sentence is confusing, and it is not clear what the authors are pointing at. Re-write for clarity.

This line has been removed.

Line 407. References of data of these corals (E. dabneyi, E. antarctica and bamboo corals) needed.

This line has been removed during editing, but we will be sure to provide citations in other mentions of these works.

Line 408 "across their surfaces, perpendicular to the growth axis". This seems confusing. Is it across the surface of the coral (i.e., the outer material in contact with seawater)? Is it the surface of each cross-sectional slice? Change wording.

This line has been removed.

Line 410 "external corals". I am assuming this refers to previously published data of E. dabneyi, E. antarctica and bamboo corals but it is not clear. Re-write for clarity.

Yes, but this line has been removed.

Line 410. In the last sentence of this paragraphs the authors explain why their data is different to previously published data. I am not sure why there is still no mention of the role that changing mineralogy might be playing here. These MS is the first published data of highly resolved stable isotopes in mixed mineralogy stylasterids, while previous published data is on specimens either 100% calcitic or aragonitic. I understand the authors are defending that it is a growth pattern and not a mineralogy effect. Yet, I think adding a statement clarifying that these differences exist and referring to section 4.2.3 on to why mixed mineralogy is not the source of isotopic variability would help structuring the narrative and showing that mineralogy has really being considered.

Thank you for this comment. Because of the constructive comments of both reviewers, we have edited this manuscript to incorporate much more information about mineralogy. We have incorporated it into the introduction and added a much more thorough discussion section. We have also made changes to describe how mineralogy should be considered when examining the isotopic variability.

Line 417 "The presence of symbionts could contribute metabolic carbon and/or oxygen to the pool from which corals calcify". References for this statement.
Done

Line 418 and 419 "Depending on the relationship […] (Epstein et al. 1951)" - *Carbonate-water isotopic temperature scale*, Geol. Soc. Am. Bull., 62, 417–426: I might be wrong, but I don't think this statement (symbionts near calcification sites can shift isotopic signal to lower values) emanates from the cited paper.
Good catch, this was an artifact of previous edits. It has been changed to the correct reference.

Line 435: What sample this SEM comes from (Sample ID). The specimen used for SEM was collected alive or dead? Equally, what part of the coral is Figure S3 from (i.e. main trunk without visible polyps/cyclosystems, growing branch with high presence of cyclosystems?, center of a slice or towards the outer layer?). Information on the context is missing that can be important to interpret the data provided.
Thank you for pointing out that we are missing information. This has been added to the manuscript as well as the supplemental figure captions.

Line 443 about single coral variability as per Stewart et al. (2020): See comment above.
We understand the mistake that was made here. This section has also been heavily edited to provide more of a consideration for a mineralogical influence on the isotopic values.

Line 447 "[…] but corals from the same dredges were." As above, details and information on sample ID for this analysis is missing.
This has been edited.

Line 448 and 4.2.3 in general: Authors mentioned the use of false-colour imaging (Figure S4). This is the first time this method is mentioned in the MS. This information needs to be included in the methods section. Is this false-colour image obtained via EDS, Raman, or any other method? Furthermore, this false-colour imaging was done on an alive or dead specimen? What was the sample ID? Is the same sample as one analysed via XRD or another third sample? There are VERY important information missing from this manuscript. Importantly, mineralogy data from the false-colour imaging differs from the one obtained via XRD. While data from XRD shows an up to 5% difference between center and external material, in the false-color image this difference is of up to 18%. The authors base their argument that mixed mineralogy is not the cause for the observed discrepancies with published data (Wisshak et al. 2009 and Samperiz et al. 2020 – both 100% aragonite) due to the small mineralogical change (<5%). Considering Figure S4, and not knowing from what sample this data is coming, it seems reasonable to question the possibility that mixed mineralogy percentages might not be similar across a given *E. fissurata* population. And if this is the case, one can question that EA-11 and EA-12 might as well have a higher percentage difference between the inner and the outer slice than those analysed via XRD. Ideally, XRD would be done on EA-11 and EA-12. But if this is not possible at all, the limitations of these observations need to be discussed together for their implications in interpreting the results.

Furthermore, it would be interesting to include what method was followed in Figure S4 to separate outer and central. Is it white versus colored coenustum? Or a random circumference of radius 1cm? This is very important to be able to compare with stable isotopic data.

We appreciate this reviewer's thoughtful consideration of this section. However, we have decided to remove this figure altogether. Because we were able to perform XRD analysis on a coral piece that was also used for stable isotopes, we considered it preferable. The image processing done here is very qualitative and based on identifying the blue and red pixels in the image and separating them. They were then divided into the outer and inner portions based on the approximate scale and the average diameter of the white coral center of the other corals. This specimen was collected from the same dredge as all the other E. fissurata presented here, but the false color analysis was done years ago before it was intended to be incorporated into this work. Additional details that would be required to fully incorporate this figure into the main text are not available. Therefore, we have chosen to remove it from this manuscript.

This manuscript feels like mineralogy effects, and the fact that these specimens show a mixed mineralogy (although likely higher calcite than aragonite) are only visited briefly and without paying much attention to it. I would like to see a deeper discussion on mineralogy, rather than rapidly brush it off within one sentence. Importantly, this point does not mean the authors are wrong about differences in growth patterns for *E. fissurata* (with higher vital effects in the outer regions of a cross- section). As growth model of stylasterids are largely unknown, it very well could be that some populations not only show differences in mineralogy, but also in the entire growth strategy they follow. These results are fascinating, and really indicate how much more we need to learn about this taxa, but needs a deeper discussion so non-expert readers can grasp the importance of the taxa and further research.

We appreciate the constructive and supportive nature of this comment. During this revision process, in preparation to discuss mineralogy in greater detail, we have been convinced that it cannot be discounted. We agree with this reviewer and have edited this section of the discussion so that a potential mineralogy impact on the coral geochemistry will not be downplayed. This was a pleasure to research as we learned that we actually do have the first $\delta^{18}O$ and $\delta^{13}C$ records from mixed mineralogy stylasterids. It is exciting to be able to discuss these results and hopefully generate excitement and further studies into how these corals calcify.

Furthermore, whenever the temperature calibration from Samperiz et al. (2020) is used across the manuscript, at the very least it should be noted that this calibration was obtained for 100% aragonitic specimens and that it is unknown whether it is applicable for calcitic (or mixed) specimens, or that a δ18O – temperature calibration for calcitic specimens could have a different slope hence the need to be cautious. I understand that due to the lack of a better fitted calibration, Samperiz et al. (2020) is used, but it is good to be clear about limitations here.

We agree.

Section 4.3 Hypothesizes large-scale calcification model for *E. fissurata*: This is just a suggestion and I leave the decision to modify it to the editor/authors of the study.

This section feels very long, and somewhat confusing. It goes for two long paragraphs on growth models of other coral taxa (Corallium and acroporids) to, just at the very end, conclude that "we do not suggest that the same crystal structures are apparent here. We instead hypothesize […]".

I think this section would benefit of being more succinct and straight to the point. Furthermore, the growth model for stylasterids proposed by Wisshak et al. (2009) is only briefly mentioned. Reconciling or comparing this previously published model, with the authors data (both mineralogy and stable isotopic data) would be a valuable insight. Instead, it is really focused on other corals taxa for no apparent reason.

We agree that there are additional growth structures described that are not needed and take away from the main messages of this work. We have trimmed this section down. However, when we describe the growth model by Gladfelter (1982), we are not discounting it when we say that "we do not suggest that the same crystal structures are apparent here". The Gladfelter growth model for acroporids instead prescribes very specific details about fusiform crystals followed by aragonite crystals. We do not have the data to support if the same type of calcification is occurring on that scale, but rather the broad growth patterns could explain our isotope records. Because this was not made clear, we will edit it for clarification. Regarding the Wisshak et al. (2009) growth model, we have incorporated their description, along with other evidence for the narrow, internal mesh structure on the outside of the corals and the wider canals toward the inside of stylasterid corals.

Conclusions

There is no mention of mineralogy and its role in the conclusions of the manuscript. As stated in my comment about section 4.2.3, the discussion about the role of mineralogy needs to be deeper and more detailed. As such this should also be reflected in the conclusions.

We agree, and have edited the conclusions to include the importance of mineralogy and a call to future work developing temperature calibrations for mixed mineralogy stylasterids.

Figure 5 and 6: What data is from what sample? Add circles and squares (Like in Figure 4 and 7) to differentiate between data from EA-11 and EA-12.

We have changed the symbols of Figure 5 so that the circles represent EA-11 and the squares are EA-12. At first, it seemed as though introducing more symbols to the figure would be overwhelming, but this reviewer's perspective helped us to clarify the figure for readers. We have also changed the symbols for Figure 6 to improve understanding. We have emphasized the difference between aragonite and calcite corals and made all Errina symbols the same.

Review #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.

Second Review of "Deep-sea stylasterid δ18O and δ13C maps inform sampling scheme for paleotemperature reconstructions" by King et al.,

It is good to see that many of the missing references have been included and that mineralogy has been addressed in this newest iteration of the Ms. It is a shame though that stylasterid mineralogy is still not properly addressed in the introduction, therefore the reader will be unclear as to the importance/necessity of the XRD data when they suddenly appear in the results. I still feel though that the interpretation of these mineralogical results may be too superficial, and the effects are far more important to reconciling these mixed mineralogy specimens with previous results from pure high Mg calcite and pure aragonite specimens measured before.
We have noticed common themes between both reviewers' comments in this round of revision. One of them is the need for improved discussion of mineralogy. We have taken these comments into serious consideration and have thoroughly edited the introduction, results, discussion, and conclusion sections to all include mineralogy.

I disagree with the interpretation that the centres are just 3% more aragonite mentioned in the response to review (the image in figure s4 clearly shows there is more aragonite than that – see below). Without direct measurement of these exact specimens (with spot sizes equal to that of the stable isotope analyses) it is difficult to rule out that there are *both* rate effects (as the authors suggest) AND mineralogical effects driving the trends.
We thank the reviewer for this perspective. Since the last submission, we were able to run one of the coral slices for XRD that was also run for stable isotopes. This provided additional important mineralogical information which we have worked diligently to incorporate into the manuscript. We have also changed the discussion to account for the possible influence of mineralogy and have discussed mineralogical impact on skeletal isotope ratios and the potential impact of both growth and mineralogical forcing of the isotope trends.

There are still typographic errors and mistaken figure references in text that need to be addressed. I maintain that these results are important and thought provoking and merit publication, however I still feel that the discussion needs work. My specific line by line comments are listed below.
We thank this reviewer for their patience as we work through this manuscript. We have addressed all of these errors and have polished it as a whole.

Line numbers refer to the track changed manuscript pdf file.

The abstract is still misleading. Without reading the full paper, the reader is left with the impression that stylasterid corals in general have this low to high (out to in) stable isotope distribution when the Samperiz data prior to this clearly show the opposite is true in pure

aragonite and pure calcitic stylasterids. The authors need to be clear on line 15 “growth structure of a mixed mineralogy (high-Mg calcitic and aragonite; confirmed by X-ray diffraction analysis) deep-sea stylasterid coral, Errina fissurata”.
We agree that the abstract as written would lead readers to believe that the findings we present are applicable to all stylasterids. We have edited the abstract to clarify the implications and the work done.

Line 42. I don’t think “noise” is the right term to describe vital effects. Isotope ratios could be offset from equilibrium by a consistent value of -2 ‰ for example. This wouldn’t give a noisier signal, just an inaccurate one. I would recommend deleting the word “noise” and just leave it as aka “vital effects”.
We agree and have changed the wording.

Line 63: suggest “vital effects have been invoked to explain trends in δ18O…”
Done, thank you for the suggestion.

Line 78. Consider rephrasing. This statement “these results obscure best practices for colony-scale sampling” implies that Samperiz results actually hindered progress towards understanding coral vital effects, when the opposite is true. Both this study and Samperiz study shed light the micro-structural complexity of stylasterids, highlighting the need for further work.
This sentence has been removed as the introduction has been reformatted. We agree that this read improperly as it was not our intention to imply that Samperiz et al. hindered any progress.

Line 80: one of the most interesting things about stylasterids is that they are made of aragonite, hi mg calcite or in some cases both polymorphs of carbonate. This needs to be introduced before the “Here we present..” statement as it is a key factor when interpreting difference between Samperiz work and this study.
Yes, we agree and have done this.

Line 83. Recent boron isotope results in Stewart et al., 2022 (Sci Reports) show that stylasterids have a fundamentally different biocalcification strategy to (well-studied) scleractinia. This study shows that they do not upregulate internal pH, so it is still unclear how stylasterids precipitate aragonite in undersaturated waters. This supports the need for further research and is useful to mention here.
Yes, we agree and have done this.

Line 90. If the importance of mineralogy is introduced earlier, the XRD work can be mentioned here in the “here we present” section as an important tool to help understand differences in δ18O and δ13C results between stylasterid taxa.
We have done this as well. After incorporating this reviewer’s feedback above, it flowed naturally.

Ling 149: The importance of mineralogy needs to be laid out in the intro and mentioned in the here we present section before “2.3. Mineralogical analysis” is described. The reader needs be

clear why these analyses are needed – i.e. to establish if this is an aragonitic Errina like that of Samperiz or if there are mineralogical differences across the growth bands.
This has been done, see responses to similar comments above.

Line 153: this line has not been completed. "They were analyzed on a XX diffractometer with a wavelength of xxx."
We apologize as this was an artifact of the "Tracked changes" version. The line was completed in the uploaded manuscript.

Line 161: overuse of word "employ"
Good catch, thank you.

Line 225. Again, this intro to stylasterid mineralogy is too late into the Ms. This is also an oversimplification as some stylasterids are pure aragonite, some pure Hi-Mg calcite, and some are "mixed mineralogy" as mentioned here. These three modes of calcification within a coral family is strange and a key reason why we need to study these organisms and their geochemistry.
We agree that we didn't spend enough time discussing the mineralogy of these corals and should have brought it up much sooner than this portion of the results section. We have enhanced the introduction with much more information about stylasterid mineralogy and have described the XRD results here. The stylasterid mineralogy has become a much more prominent consideration in this manuscript.

Line 239: the statement implies results do not support that of Samperiz, then on line 245 results from specimen EA-11 do support Samperiz. This is confusing to the reader unless the work of Samperiz is clearly laid out in the introduction, stating how the new study is different, and what exactly is being tested (i.e. the first mixed mineral specimen, but same genera, sampled in a different way). The results of this study are important and thought provoking, just their place within the context of previous work hasn't been properly established.
We agree that this was a poor choice of words to try and develop the nuance in the differences between our results and those of Samperiz et al. (2020). We have reformatted the discussion section and have clarified the comparisons with other works. This new structure flows much better for the reader.

Line 246: Do you mean "Specimen EA-12 was characterized by δ18O and δ13C minima nearest the tip (**EA-12a**)," rather than EA-11a
Yes, thank you for the catch, this has been changed.

Line 253: I agree here, Figure 4 to me seems to suggest that there was effectively no difference in terms of δ18O and δ13C between samples closer to tip or base. They all have similar average and variance. The discussion above here is focused on the extreme outliers of each sample section and adds little for determining if the "tip" is lower than the main stem in its stable isotope ratio. Indeed, with no tips actually sampled in this study im not sure that this can be assessed.
We have restructured the discussion as both reviewers have aptly pointed out its confusing nature and that this is not a direct comparison. We have changed this section to clearly state the

limitations introduced by the sampling strategy. We appreciate the constructive nature of these comments.

Figure 4. Remove "analytical uncertainty" error bar from top left and just say that error was smaller than data points. At the moment it looks like the bulk average with its 2sd is the only one that had any analytical error. The legend is also poorly arranged and it is difficult to tell that there are two specimens (11 and 12) plotted here. Suggest two columns, one for EA11 and one for EA12.
These changes have been made in addition to changing the color scheme to improve clarity.

Figure 5. D. cristagalli is now Desmophyllum dianthus.
Thank you for the catch. Because of this comment, we also noticed that Lophelia is now listed as unaccepted on the World Register of Marine Species and has been replaced by Desmophyllum. All mentions of the outdated nomenclature have been corrected, and we have made distinctions between D. dianthus (previously D. cristagalli and Desmophyllum sp. (previously Lophelia).

Additional Reference:
Addamo, A. M., Vertino, A., Stolarski, J., García-Jiménez, R., Taviani, M., & Machordom, A. (2016). Merging scleractinian genera: the overwhelming genetic similarity between solitary Desmophyllum and colonial Lophelia. BMC evolutionary biology, 16, 1-17.

Figure 6. This is an important figure, but needs to be clearer. Some sort of clear distinction between published and new data (e.g. open and closed symbols), and calcites vs aragonites (e.g. blue and red symbols) and also those samples that are Errina (e.g. circles). This should clearly show that Samperiz's aragonites are all much higher δ18O and δ13C than this study. This is then a good figure to refer to when introducing the work. The caption of figure 6 also refers to a figure 6 – please check what is meant here.
Thank you for the advice. This figure proved difficult to illustrate a clear comparison among all the stylasterid coral data. We have incorporated your suggestions and modified the figure. There is now a clear division between aragonite (empty symbols) and calcite (filled symbols) corals. I have also made each Errina coral a circle, and denoted more clearly which data points are new from this study. I have also made a distinction between specimen EA-11 and EA-12 samples using circles and squares to be consistent with the rest of the figures.

Line 267: I do not follow the reasoning here. Samperiz found that calcites had lower stable isotope values than aragonites and were therefore further from equilibrium. That is the same as what is shown in Figure 6. Regardless of one's choice of equilibrium seawater value (arag or calcite) the blue dots from this study are all much lower in δ13C (further from equilibrium) than the 0,0 point and similar to the calcites in Samperiz. Sure, the offset is increased if an aragonite equilibrium is chosen, but I don't think that is the right approach for a predominantly calcite organism and the darker blue dots just make the figure less clear.
These few sentences have been removed as the reviewer is correct, and it is very unclear and the incorrect approach to displaying and describing the dataset. This entire discussion has been

reworked and with the additional XRD analysis we were able to do, we removed any mention of the aragonite equilibrium.

Line 278: this misrepresents the Stewart et al., 2020 study. All δ18O and δ13C values in Stewart et al., and bulk measurements come from Samperiz et al., 2020. Although Stewart et al., table 2 may give the impression that variance is smaller between replicates in mixed and hi-mg calcites, this is likely due to undersampling (only 4 and 2 specimens). For that reason, no such observation is made in the discussion section of that paper (stylasterids, in general, are merely compared to scleractinia and found to be less heterogeneous). If anything, the opposite is true when Sr/Ca and Li/Mg data are considered which show large discrepancies between replicate bulk analyses in mixed mineralogy specimens.
We agree with this comment in that we had poorly described the variability of Stewart et al. (2020). In editing the discussion section, we removed this section because upon rereading Stewart et al., it was unclear if the mixed mineralogy specimens they present were truly mixed. The authors state that all $\delta^{18}O$ and $\delta^{13}C$ values and associated mineralogical data were from Samperiz et al. (2020). The confusion lies in the mineralogy of the E. gracilis specimens. The supplemental information provided by Samperiz et al. (2020) do not state the mineralogy of these corals, they are listed as "na". It is unclear where the mineralogical information came from for E. gracilis. Stewart et al. could have done the analyses or gotten more information from a personal communication from Samperiz et al., but it was not stated. Therefore, we chose to eliminate a reference to Stewart et al. mixed mineralogy corals.

Line 285: D. cristagalli is now Desmophyllum dianthus.
See response to similar comment above for Figure 5.

Line 293: Citation needed for E dabneyi and E antarctica data here so show they are not from this study.
This line has been removed during editing, but we will be sure to provide citations in other mentions of these works.

Line 315: the wording in this section should be revised. These are azooxanthellate corals, therefore referring to their symbionts here is confusing. I don't think the barnacles that live on them are symbiotic, but rather epiphytes. I agree though with the premise that this is an unlikely cause of sample contamination.
Done, thank you.

Line 341: Again, reference to Stewart here is not valid and these selected bulk samples taken from the Samperiz dataset do not properly reflect the variance that Samperiz found between tips and bulk.
We understand the mistake that was made here. This section has also been heavily edited to provide more of a consideration for a mineralogical influence on the isotopic values.

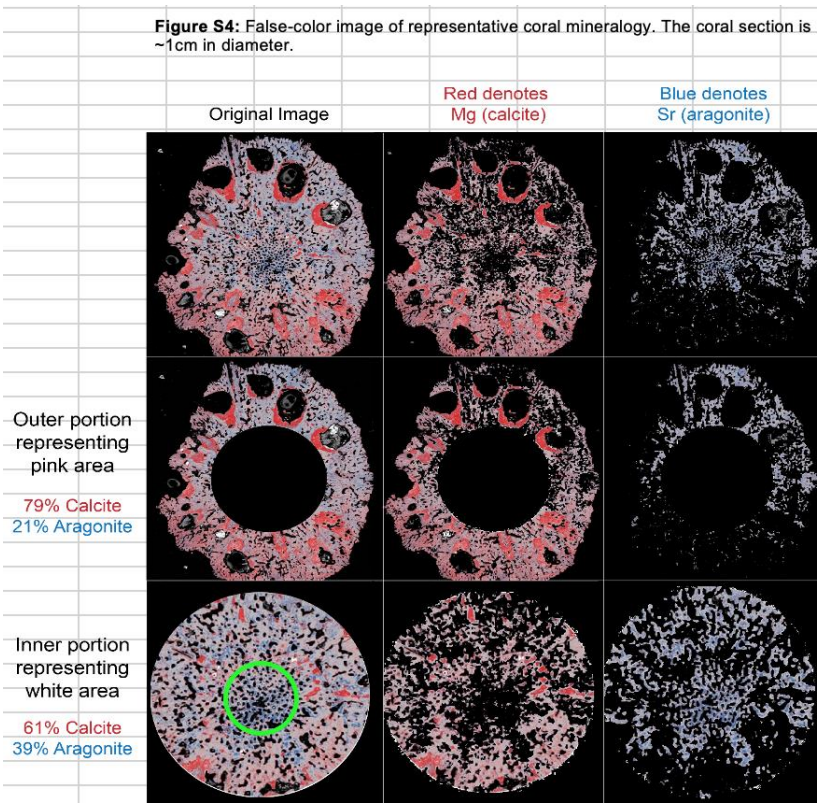
Line 342. Be clear that specimens of the same species from the same dredges were measured for XRD. This all needs to come sooner and worked into the intro, methods and results sections before discussion here.
We agree, and these edits have been made.

Line 345. Mostly calcite, therefore a calcite equilibrium value should be used for these data in figure 6
We agree, the aragonite equilibrium values have been removed from all figures.

Line 347. Space needed between to and 77%
Good catch.

Line 349. I disagree that we can rule out that the mineralogical differences across the specimen are not the driver of high δ18O and δ13C values. Aragonites should be naturally higher in their equilibrium stable isotope values (~+2 ‰) therefore an aragonite core to the specimens would give the higher values seen in this study. Both specimens in table 3 have small increases in aragonite at the centre (+3% and +5%). I agree these are small, but I question whether the XRD sampling was at the same resolution as the spot sampling for stable isotopes. It is curious that the values given in Table 3 do not represent those shown on the image in Fig S4. This supplemental fig suggests that the inner portion of the coral is +18% more aragonite. This could be a lot more if you select a smaller circle to denote the sample inner (e.g. the green circle I have added to the figure). Without measuring the samples in the study for XRD I don't see how a predominantly aragonite core to the specimens can be ruled out as driving the data. This could be acting in conjunction with the growth rate effects suggested by the model. This is not a bad thing. This is the first example of detailed stable isotope chemistry across mixed mineralogy specimen (compared to Samperiz et al., who only measured pure calcite and pure aragonite) and it shows that perhaps a different sampling strategy is needed. Stylasterids are not as simple as we first thought and their mineralogy is very important and must be considered.
We appreciate the constructive and supportive nature of this comment. During this revision process, in preparation to discuss mineralogy in greater detail, we have been convinced that it cannot be discounted. We agree with this reviewer and have edited this section of the discussion so that a potential mineralogy impact on the coral geochemistry will not be downplayed. This was a pleasure to research as we learned that we actually do have the first $\delta^{18}O$ and $\delta^{13}C$ records from mixed mineralogy stylasterids. It is exciting to be able to discuss these results and hopefully generate excitement and further studies into how these corals calcify.

Figure S4: False-color image of representative coral mineralogy. The coral section is ~1cm in diameter.



I think this image needs to be included in the main text. The green circle shows a much bluer (majority aragonitic) core to the samples than is represented in the text.

We have decided to remove this figure altogether. Because we were able to perform XRD analysis on a coral piece that was also used for stable isotopes, we considered it preferable. The image processing done here is very qualitative and based on identifying the blue and red pixels in the image and separating them. They were then divided into the outer and inner portions based on the approximate scale and the average diameter of the white coral center of the other corals. That is why the center portions are much larger than the green circle provided here. This specimen was collected from the same dredge as all the other E. fissurata presented here, but the false color analysis was done years ago before it was intended to be incorporated into this work. Additional details that would be required to fully incorporate this figure into the main text are not available. Therefore, we have chosen to remove it from this manuscript.

Line 435. Check figure numbers. I think this section should be referring to fig 10. It would be fascinating to know if these brighter pixels denote more aragonite.

Good catch, fixed.

Line 477. Conclusions should also address mineralogy e.g. In the case of *E. fissurata*, a mixed mineralogy taxon, we…"

We have included this.

Line 477. This result contradicts growth structures hypothesized in the current body of literature based on observed stable isotopic trends that have only focused microsampling on purely high-Mg or purely aragonitic specimens.

We have included this, thank you for the suggestion.