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.

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.

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.

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.

Line 41: fidelity.

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.

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.

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?).

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).

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 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.

Line 357: Is this meant to be EA-12a?

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.

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 $\delta 18O_{equilibrium} - \delta 18O_{coral}$, 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.

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

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.

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.

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.

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

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.

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.

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.

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

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.

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.

Line 443 about single coral variability as per Stewart et al. (2020): See comment above.

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

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.

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.

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.

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.

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.

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.