We would like to thank the reviewer 1 (Jesse Farmer) for an insightful review with lots of great questions and comments that undoubtedly helped us to improve the manuscript.

On the following, I will answer the comments provided by the reviewer (which are listed in blue).

Review of Alonso-Garcia et al., "Sea-level and monsoonal control on the Maldives carbonate platform (Indian Ocean) over the last 1.3 million years, by Jesse Farmer

Alonso-Garcia and colleagues present x-ray fluorescence (XRF)-derived records of carbonate source (Sr/Ca), summer monsoon intensity (Fe/K), and primary productivity (Br) from a sediment record (IODP Site U1467) in the Maldives Inner Sea covering the last 1.3 million years. The authors note a first-order correlation between Sr/Ca and glacial-interglacial cycles indicative of changes in carbonate source (periplatform vs. pelagic), which they attribute to longstanding theories of how carbonate platform productivity is affected by sea level. However, they also note discrepancies between a simple sea-level driver of carbonate production. These discrepancies are highlighted for interglacials MIS 5, 11, and 13, and are attributed to a combination of summer monsoon intensity and Indian Ocean Dipole state. Last, they note long-term changes in carbonate production reaching the Maldives Inner Sea apparently coherent with the Mid-Pleistocene Transition and Mid-Brunhes Event.

Overall, this is an exciting dataset and interpretation that is worthy of publication in Climate of the Past. With that said, I think the manuscript would benefit from major revisions and another round of review. The current draft is a bit scattered; key background information appears in the discussion, and the rationale for evaluating factors other than sea level for carbonate productivity is not entirely clear. I've included a few major comments and line-by-line edits up through the discussion. I'd be happy to look at a revised manuscript and provide a more in-depth review of the discussion at that point.

Answer: Thanks for your interest in our study. We will try to reorganize the text so the readers can understand better the data and discussion presented in the manuscript.

Major comments.

Introduction. As a non-expert in periplatform carbonate sedimentation, I felt that necessary background was missing in the introduction to distinguish Sr-rich carbonate production during highstands vs. Sr-poor carbonate production during lowstands. Later on, the first 1.5 paragraphs of the discussion (L224 – 245) were incredibly useful background context; I'd urge the authors to move this content from the Discussion to the Introduction.

Answer: ok, I am trying to give more context in the introduction of the revised version so the readers can understand better the interpretations of the data.

Oxygen isotope sea level proxies and their (quantitative) utility. Whereas deep ocean d18O-based sea level reconstructions usefully indicate the glacial-interglacial character of sea level change, they are not sufficiently precise to be employed for comparing highstand sea levels:

- Typical precision on d18O-based sea level reconstructions is on the order of ± 20 m accounting solely from uncertainty on calculated d18Osw (see Ford and Raymo, 2020 -

https://doi.org/10.1130/G46546.1, and note also that the 2sigma uncertainty in the Spratt and Lisiecki, 2016 stack is similar). This magnitude of precision is not sufficient to constrain sea level differences within or between mid/late Pleistocene highstands.

- d18O-based sea level reconstructions also appear to have accuracy problems, possibly more apparent during highstands. For instance, the Elderfield Site 1123 d18O record would suggest MIS 11 sea level of + 40 m, which is hard to take seriously given geological constraints of < 13 m; Raymo & Mitrovica, 2012 (https://doi.org/10.1038/nature10891). Additionally, d18O-based sea level reconstructions appear to greatly overestimate MIS 3 sea level (Dalton et al., 2022 - https://doi.org/10.1016/j.gloplacha.2022.103814).

While such a detailed view of d18O-based sea level reconstructions might seem tangential to the current manuscript, it is necessary because the authors' motivate their investigation of additional mechanisms using the discrepancy between highstand sea level (from d18O) and Sr/Ca (L259-260). I'm not sure this avenue of motivation holds up given the imprecision on the sea level reconstructions. At the very least, the error on the sea level estimates needs to be presented. I'd also urge the authors to consider something like a crossplot of Sr/Ca vs. sea level (with uncertainty shown) to illustrate the assertion that Sr/Ca and sea level are decoupled – such decoupling is not particularly apparent in Figures 5 and 6 to warrant a long discussion of additional mechanisms.

Answer: This is a very good point. I will add this information about the uncertainties of the sea-level reconstructions in the text. It is especially relevant for MIS 11, when the export of HSAC is very high. Also, I will add the error of each reconstruction in Figure 4 and a cross-plot of sea level vs Sr/Ca.
The accuracy of sea-level estimates is an important point for this study, and it would be wonderful that sea-level estimates with better precision would be available. It is true that Lisiecki and Spratt (2016) acknowledged a mean uncertainty in their sea-level reconstruction of 9-12 m (1σ) for the stack. A 2σ error (18-24 m) of this record is a rather high uncertainty for places like the Maldives Archipelago. The seawater $\delta^{18}O$ based sea-level record of ODP 1123 from Elderfield et al. (2012) presented an error of ±0.2 ‰ (which is equivalent to ±20 m). However, this is one of the best sea-level estimates for the Mid-Late Pleistocene and in their supplementary material Figure S6 they showed that for the last 250 ka there was good agreement between the sea-level estimates from coral reefs and the seawater $\delta^{18}O$ derived sea-level (see Figure 1 below).

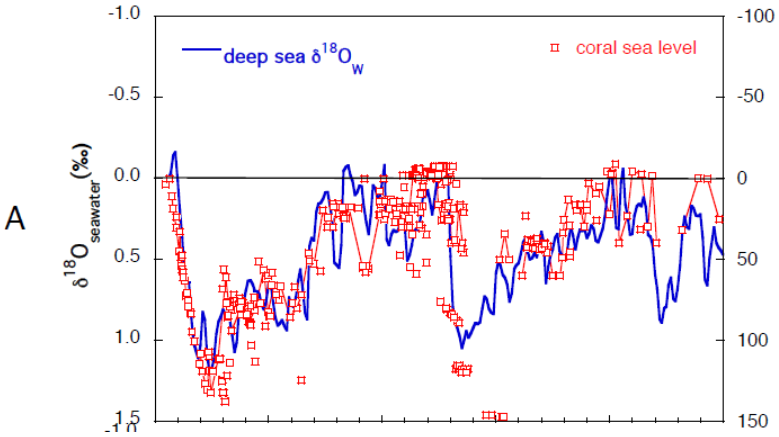

Figure 1. Partial reproduction of Figure S6 from Elderfield et al. (2012) from supplementary material.

The motivation of the study to find what else was driving the production and export of HSAC in the Maldives Inner Sea emerged from the differences that we can observe between the Sr/Ca record and the sea-level records, which was considered the main driver for carbonate production in the atolls and platforms. In order to answer the reviewer question, I made a cross-plot with the U1467 Sr/Ca and the Elderfield et al. (2012) sea-level record (Figure 2). The cross-plot (Figure 2A) shows a wide dispersion of the data and the linear regression a low R^2. Even if we only plot the values for the interglacial periods (Figure 2B) the dataset shows a wide dispersion, with very variable sea level estimates for high Sr/Ca values. This is particularly evident for the data included in the blue rectangle, which groups most of the interglacial data points. Therefore, we conclude that other factors than the sea level must be affecting the production and export of HSAC.

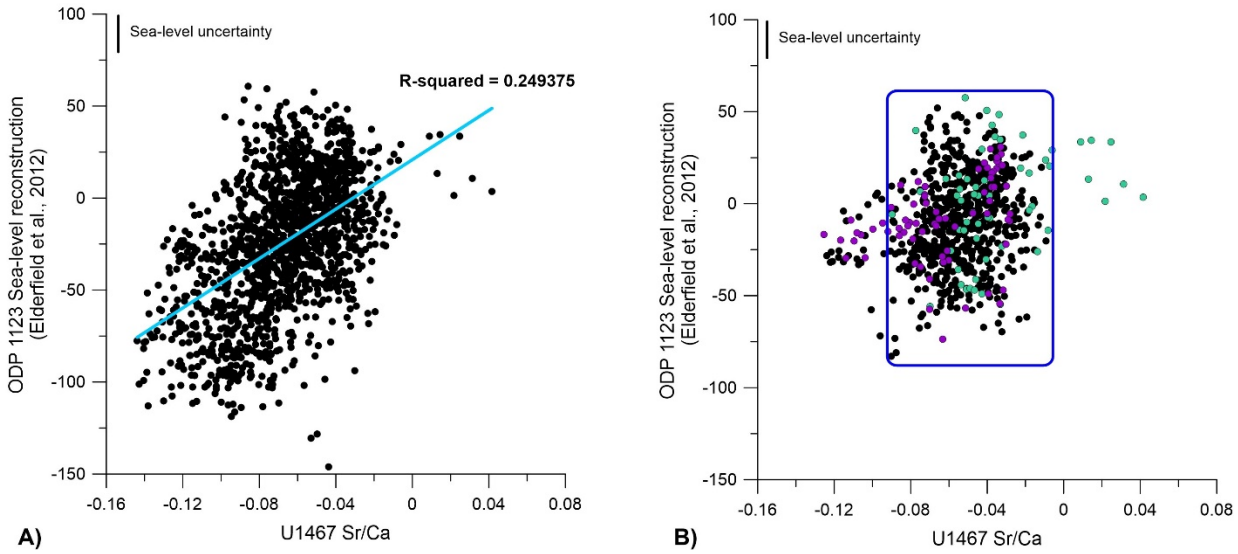

Figure 2. Cross-plot between U1467 Sr/Ca and the Elderfield et al. (2012) sea-level record showing the low correlation between both records. Panel A) shows the full dataset comparison including glacial and interglacial periods. Panel B) shows only interglacial data, green dots correspond to MIS 11 and purple dots to MIS 31. The blue rectangle indicates where most of the data is grouped.

XRF Br_n. Is this more reflective of productivity or diagenetic alteration? The most notable feature to me is the apparent "burn down" of Br in glacial maxima from MIS 2 to MIS 8, with effectively constant Br_n around small orbital-scale variations before this time. Could the small orbital-scale variations also be related to diagenesis?

Answer: Thanks for bringing up this question. We will try to clarify this point in the revised manuscript. Br is more abundant during the glacial periods in the Maldives Inner Sea (Bunzel et al., 2017 and this study), indicating higher organic carbon accumulation. According to Ziegler et al. (2008), total organic carbon (TOC) and Br content in the sediments show a clear correlation except when there is input of terrestrial organic matter. The alkenones record of U1467 does not show a strong correlation with the Br record (Figure 3), thus, the input of organic matter from the continent does not seem to be the factor controlling the Br variations. In addition, the glacial periods show higher bottom water oxygenation

based on the higher plant *n*-alcohols/*n*-alkanes (HPA) ventilation index and the ostracod assemblages (Alvarez Zarikian et al., 2022). If respiration/degradation of the organic matter would be an issue, the glacial periods would be depleted in Br and TOC but it is the opposite. Therefore, we believe the Br variations reflect sea surface productivity and that the organic matter is not very much affected by diagenetic processes. The orbital variations observed in the Br record, in our opinion, correspond to variations in sea surface productivity related to orbital parameters, mainly precession and obliquity. We will try to clarify this in the revised manuscript.

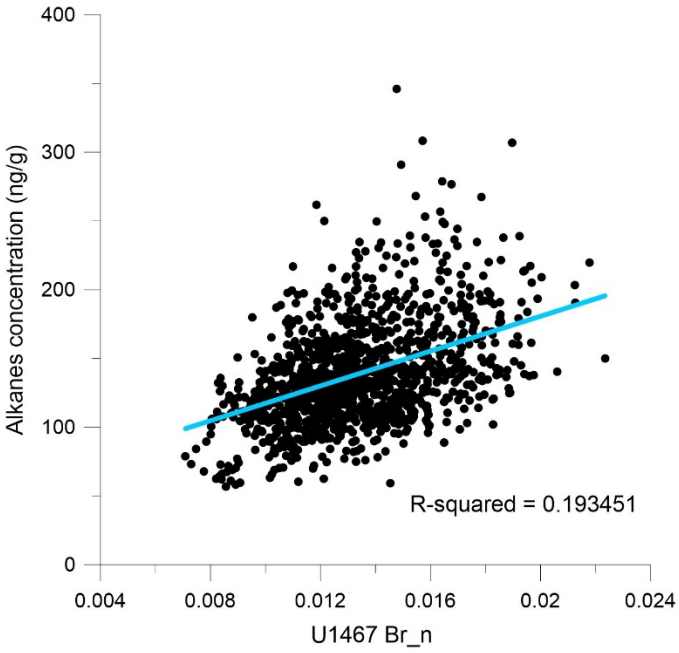

Figure 3. Cross-plot between U1467 Br normalized record (this study) and the U1467 alkanes concentration in the sediment (Alonso-Garcia et al., 2019).

Minor/line-by-line comments.

Answer: All the minor comments will be reformulated following the suggestions of the reviewer. Below is the answer to some questions included in the minor comments.

L31-33. This sentence confuses me. Don't you expect higher Sr/Ca during interglacial periods? If so, then "several interglacial periods before and after the Mid-Brunhes event (MBE, ~430 ka) indicate high carbonate production (high Sr/Ca)" would not be surprising. Perhaps this meant to say glacial, or some other dynamic?

Answer: In this sentence I meant that the interglacial periods prior to and after the MBE indicate high carbonate production. With this I mean that for example MIS 13, MIS 15, MIS 17 show similar values of Sr/Ca to MIS 9, MIS 7 or MIS 5.

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