

## ***Interactive comment on “Southwestern Tropical Atlantic coral growth response to atmospheric circulation changes induced by ozone depletion in Antarctica” by H. Evangelista et al.***

### **Anonymous Referee #3**

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Southwestern tropical Atlantic coral growth response to atmospheric circulation changes induced by ozone depletion in Antarctica.

Evangelista et al.

I know nothing of corals so have not commented on the biological side of things, only the aspects of the manuscript concerning atmospheric circulation changes. I found the conclusions section rather difficult to read and it contained a lot of unnecessary information about the effect of ozone depletion on Southern Hemisphere (SH) high latitude climate which is not directly relevant to its effect at the coral site at 20°S, even if they co-vary. I felt the processes linking ozone depletion to changes in SSTs at the

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coral site could have been analysed in far more detail and there isn't a great deal of original research in the paper outside of the coral data. This is a shame because, like reviewer #2 I think there is an interesting hypothesis to be tested here.

Major points:

There are a few issues with the English, which I think are sufficiently minor for an editor to deal with.

Conclusions: most of the conclusions section is actually discussion and needs to be labelled as such. I found it very difficult to read as it lacks a coherent logic and flow.

Figures 2 and 3 have several sections but not all of them are mentioned explicitly in the text and therefore it's difficult to know what point the authors are trying to make with them. Each panel should be referenced in the text together with why what they are showing is important.

Figures 3a/3b: There are many papers (including Marshall 2003 that the authors cite in this manuscript plus Hines et al. 2000; Bromwich et al. 2007 etc ) that clearly demonstrate that NCEP reanalysis data at SH high latitudes are essentially rubbish before 1979. Thus, the results for high latitudes shown in Figure 3a/3b are also rubbish. Unfortunately, all reanalyses are poor in this region before 1979, which is when satellite sounder data over the Southern Ocean first became available to be assimilated into numerical weather prediction models. Before that there are no data to constrain them. It also doesn't state how far back the NCEP data go that you use (1948?).

Having said that it's not at all clear what information I'm supposed to be taking from Fig. 3a/3b that's directly relevant to the corals anyway; it may well be that at the much lower latitudes of the coral site then the NCEP data is fine?

One aspect the authors do not address is the seasonality of the ozone hole, which only occurs in austral spring, with its effects on the surface circulation likely being limited to the summer. Although the SAM has also become more positive in fall this is generally

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not thought to be driven primarily by ozone loss and is more likely to reflect intrinsic climate variability.

It would be interesting to see how long the effect of the ozone depletion took to impact the SSTs at the coral site by looking at seasonal or monthly data SST/wind stress data. This may also help explain the exact mechanisms involved, which are currently just described with no analysis done other than basic correlations.

If the current 'Conclusions' becomes 'Discussion' then it would still be useful to have some conclusions taken from the original work in this paper too.

Minor points:

Page 13195, Line 2: could be changed to something like 'Recent Southern Hemisphere (SH) atmospheric circulation, predominantly driven by stratospheric ozone depletion over Antarctica, has caused changes in climate across much of the extra-tropics'

Page 13195, Line 14: the Marshall (2003) reference is not appropriate in the context of corals

Page 13196, Line 15: impact of GHGs more to do with circulation changes rather than, specifically, any increase in temperatures; e.g. much of Antarctica has cooled in response to the more positive Southern Annular Mode (SAM) that partly results from increasing GHGs

Page 13196, Line 21: the Zazulie et al. (2010) reference is not the right one to cite here: use one or more of Polvani et al. (2011), Thompson and Solomon (2002) and Thompson et al. (2011)

Page 13196, Line 25: change 'by' to 'using'

Page 13196, Line 26: Change 'They' to 'Kang et al. (2011)' and remove reference from later on in the sentence

Page 13197, Line 11: Change 'combined here' to 'here combine'

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Page 13197, Line 13: Give a lat/long for the Arolhos National Park

Page 13198, Line 24: Here you introduce the PDO without explaining anything about what it is and why you are interested in seeing how it might affect coral growth and indeed which PDO dataset you use

Page 13199, Line 4: The ozone hole has not been gradually recovering since the implementation of the Montreal Protocol in 1989. It got significantly worse during the 1990s (as indeed is shown in Fig 3d) and has only just begun (arguably) to recover; e.g. I think 2015 was the fourth largest ozone hole on record according to NASA

Figure 2: Surely the figures should go from a to e top to bottom? Also only Fig 2d is mentioned in the text. All need to be described or removed if not necessary

Page 13200, Line 5: Again, the AMO is introduced without any reason given as to why it might affect Brazilian corals. All it says is that it reflects a warming of North Atlantic SSTs

Page 13200, Line 9: I couldn't find anything about a 'significant increase in the westerly winds by the end of the 1970s' following a quick glance through the Polvani et al. (2011) paper although I may have missed it. But, significant compared to what? If you assume the SAM approximates to the strength of the westerly winds then the Marshall SAM data suggest that the winds around about 1960 were on average stronger than the end of the 1970s. Basically, I don't think there is a change in the SAM that matches the decline in wind anomalies in Fig 3e.

Page 13200, Line 10: Most researchers now use the SAM rather than AAO. As this paper demonstrates clearly, the SAM impacts a much greater region than simply Antarctica.

Page 13200, Line 13: The Marshall SAM index is updated monthly. I suspect the authors are looking at an old website as it does seem to change from time to time. The current website for this dataset is: <https://legacy.bas.ac.uk/met/gjma/sam.html> It's

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not clear to me whether this dataset is used as the 'ozone depletion area', which is mentioned later on in the paper, or is that something different?

Page 13200, Line 17: There is too much written about the increase in the westerlies at high latitudes here. Surely the authors should be focussing on the more important factor for this research, which is the increase in the easterly component of the winds at mid-latitudes (~30-40°S) that is also associated with the ozone depletion (more positive SAM)?

Page 13200, Line 26: Change 'seaside' to 'coastal'

Figure 3: the Cataldo et al (2012) reference doesn't appear in the main list of references.

Page 13201, Line 18: although the Hadley Centre HadISST is certainly widely used, there are other SST databases available. If you are going to claim that HadISST is the best then you at least need a reference to justify this statement

Page 13202, Line 7: I think the reference to Fig. 5 here should actually be Fig. 4

Page 13202, Line 10: Similarly, I think Fig. 4 should be Fig. 5

Page 13203, Line 16: Ozone recovery is only likely to push the SAM more negative in austral summer: models suggest that whether it does or not will depend on the amount of GHGs emitted, as the authors allude to.

Comment on Reviewer #2's comments

I disagree with the assertion that the westerly wind changes would have occurred with or without the ozone loss. The fact that there are no significant trends in the SAM in austral winter or spring, when there is no ozone effect, suggests that this may not be the case. Modelling and empirical statistical studies have compared the relative impact of the two forcings and ozone is always found to have had a far greater effect (e.g. Polvani et al. (2011) suggest it is 2-3 times larger). Thus, in my opinion, I think the title

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is fine as it stands.

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Interactive comment on Biogeosciences Discuss., 12, 13193, 2015.

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