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Biogeosciences
Discussions

Interactive comment on "Observed increase in springtime surface partial pressure of CO_2 in the east equatorial Indian Ocean during 1962–2012" *by* L. Xue et al.

Anonymous Referee #2

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In the manuscript Xue et al uses recent measurements of oceanic pCO2 collected in the east equatorial Indian Ocean during 2012 and the database of Takahashi et al to explore the interannual variability in oceanic pCO2 and quantify the changes in ocean acidification. Overall I found this paper quite disappointing, it is inconsistent is sections and poorly written. Scientifically I am not convinced that the results are robust, not do I feel that the paper mounts a strong scientific argument to support the statements presented in the paper. Overall I think that the scientific question is essentially solid however more work needs to be done to convince me that there are new and novel results being presented here. At this stage I have no choice but to recommend major revisions, I would also suggest that if the authors are intended to submit a revised

version that it be edited by a native English speaker.

Major Comments

Overall the authors discount some potential mechanisms of interannual variability (not very well) but then to go onto present only a hand waving argument (at best) as to what mechanism maybe controlling variability. As the major result of the paper is comparing one cruise with the historical data, this is clearly not enough. That the paper lacks serious background e.g. what is the seasonality? doesn't help. I remain unconvinced that the interannual changes are not in part due to this. Equally the statement is made that this region acts as a strong source, but this is always inferred and never shown. The authors also assume that there is linear response in oceanic pCO2, while over this period the response is clearly not linear in the atmosphere (see Fig 4) – therefore I question the results that the strength of the CO2 source is decreasing over the study period, and also how sensitivity are these results to the 1962 values.

That said, clearly large changes have occurred in this region over the last 4 decades, without understanding how it may have changed assuming that TA has not changed over this period seems a erroneous assumption make – clearly using CO2sys or other carbonate chemistry with only changes in oceanic pCO2 is not enough. I am also concerned that the authors discount MLD based on their limited data – certainly changes in other seasons can have a profound impact on mixed layer dynamics. The authors are worried about salinity changes on pCO2 as these can only be minor (< 1%) and the salinity changes are likely a tracer of water mass changes, which is a line of evidence that such be pursued. Overall I find the presentation of the figures challenging, as they do nothing to help the arguments presented here.

Minor Comments

There are numerous grammatical errors through the text that need to be addressed.

Some of the statements made in the text are redundant e.g. Generally, seawater pCO2 increases with temperature. . ..

The introduction of OA is confusing and aragonite is not introduced at all – nor is its relationship to temperature

What does +/ - 1% of the upper range values of pCO2 equate to (line 23 , p526)

There are lots of inconsistences such as in the abstract - the EIO being a source of atmospheric CO2 but then state it's a sink.

A discussion of what pCO2 rates faster or slower than the atmosphere means does need to be included somewhere in the text.

How is MLD calculated?

Interactive comment on Biogeosciences Discuss., 11, 521, 2014.