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Discussions

Interactive comment on "Linking climatic-driven iron toxicity and water stress to a massive mangrove dieback" *by* James Z. Sippo et al.

Anonymous Referee #1

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Sippo et al. in this manuscript have tried to understand the reasons for massive mangrove dieback in 2015-16 along 1000 km coastline of the Gulf of Carpentaria, Australia. They have analyzed Fe concentrations and water use efficiency from a living and dead mangrove from the region and tried to link it to climate events like ENSO and other parameters such as rainfall, water vapour etc. The manuscript contains a good data set with 14C ages. However, in the end, content of the manuscript does not justify the title. From the beginning, authors have made up their mind that since the Fe content in the dead mangrove is higher than the living, it must be the reason for toxicity and hence the eventual death. From the data, it is quite clear that Fe content is higher in dead mangrove compared to living but at the same time, authors have admitted that there is no report of Fe toxicity at the reported concentration level in this particular species

of mangrove. They have not discussed the physiological aspect of the Fe assimilation by the mangrove. Also, the linkages to the mangrove mortality with climate parameters such as rainfall, sea-level, ENSO etc. comes as a forced attempt. The very fact that these two regions are adjacent to each other with no geomorphic differences (i.e, similar elevation etc.), climatic factors are likely to affect them in almost equal measures. I am not sure if it makes sense to link death of mangroves in one part of the same region to a climatic phenomenon, particularly when it is not affecting the adjacently located mangroves with similar species. Having said that, it remains a fact that mangroves have died in one part and not in the another. I would expect the authors to explore more localised reasons for this dieback. In the end, after discussing regional climate at length, authors themselves have invoked the possible role of groundwater. How the creation of aerobic and anaerobic environments in these two adjacently located patches have varied with time leading to availability of bio-available Fe and higher assimilation of Fe by mangrove remains to be looked into. Moreover, Authors have not provided the information of about the history of tidal regime in the region. Was it different between the living and dead mangroves? From the manuscript it appears that sea level receded from the region leading to oxidation of pyrite and formation of bioavailable Fe leading to assimilation. If this was the case, why only in dieback patch? Also, please keep yourself open for explanation other than Fe toxicity. I think, in general, Fe toxicity is linked to water logging and its likelihood is higher under the anaerobic conditions. Since mangroves are experiencing frequent tidal flooding, they are often anoxic and thus chance of Fe toxicity is normally high. Aeration through specialised roots and other biological activities makes rhizosphere of mangrove species often oxygenated. So, most iron is in oxidized form (Fe^{3+}), which is insoluble, forming iron plaque in roots of many mangrove species. Thus, roots of mangroves potentially have high concentration of iron than the stem and leaves. If the tidal flooding is disturbed, oxic zones in mangrove region may increase, which leads the more oxidization condition. Though it favours the oxidation of pyrite and liberate Fe^{2+}, most of the Fe^{2+} may quickly oxide to Iron oxyhydroxide due to high aeration. So, during dieback time also, despite the oxidation

of stored pyrite and subsequent increase in sediment iron concentration, availability of bioavailable Fe2+ should be less. Though Iron plaque formation prevent mobilization of toxic metals, due it is high cation affinities it can also block the mobilization of other nutrients. Considering this, during low inundation periods, formation of iron plaque could increase many folds, which in turn affect complete mobilization of other nutrients and ultimately to gradual mortality. In light of above, I would suggest that authors revisit their arguments through physiological aspects of Fe interactions with mangrove and more localized reasons for generation of different situations in adjacently located mangroves. Apart from above, I have following comments: âĂć Abstract needs to be re-written with focus on above comments. The last part pertaining to inputs to ocean and increased productivity appears to overstatement, given that you do not have data to prove so.

Material and method : âĂć This section needs a bit more detail. There are sentences which are repetition. âĂć No information about standards used. âĂć The d13C was directly done on Wood cellulose or it was performed on graphite as in 14C? âĂć CRS was used for what? How is it relevant? âĂć Data analysis contains some sentences already covered in material and methods. âĂć You have used relative concentrations for Fe but later in discuss you invoke absolute concentration level to suggest that present concentration is not enough for dieback? Do not you think that the mention of absolute concentrations would provide a good idea to reader to compare their results if they work on this problem in their region? âĂć The concept of time lag and why was it used needs to be justified. Results : âĂć As mentioned before, it would be a good idea to provide absolute concentration of Fe in wood and sediment. âĂć In Figure 3: why there are less number of data points in living forest of upper and mid intertidal? âĂć Figure 4. No explanation of figure as to how it helps in understanding the discussion. âĂć Fig 6: Here you jump to absolute concentration instead of relative. Also, it would help if you explain the how is it relevant to discussion, probably related to pyrite oxidation. This fact is not coming out clear. Discussion: âĂć Most of the first paragraph appears to be overstatement. For example, it is clear that there is greater assimilation of Fe. How do

you know that it went through the process of pyrite oxidation, particularly when you do not have any data or mechanism to show from this study. âĂć Fe in sediment section: I am inclined to suggest that Fe input to ocean part should be deleted as this is not the primary focus of the manuscript. âĂć Overall, my comments about the discussion remains as above, i.e, to focus on Fe cycling in sediments and look for a relatively localised reason for the mortality. Limitation : I am worried about exactly what authors have put it as a limitation of the study. You cannot claim Fe toxicity as a reason for mangrove mortality and be apprehensive about the whole finding as well.

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