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

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Comment: 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 C1 of mangrove. They have not discussed the physiological aspect of the Fe assimilation by the mangrove.

Response: We thank the reviewer for this comment and important distinction. We agree that we do not have enough data to directly assess Fe toxicity. We will make changes throughout the manuscript to clarify that the evidence is strongly suggesting differences in water availability between sites but not necessarily Fe toxicity per se. We will modify the manuscript to use Fe in wood and sediments as an indicator of water availability and the possibility of Fe toxicity will be presented as one (of multiple) possible synergistic stressors. We will also add to the manuscript discussion about the physiological assimilation of Fe in mangroves. For example, Marchand et al. (2016) found that Fe2+ availability can lead to plant uptake and potential toxicity, however Fe2+ uptake in mangroves has rarely been described.

Comment: 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?

Response: We thank the reviewer for this constructive feedback. We have not focused on explaining why certain areas survived the dieback while others did not. Previous studies (Duke et al., 2017; Harris et al., 2017) provide very strong evidence that water availability in the Gulf of Carpentaria was extremely low prior to and during the dieback event. Here we build of this work with multiple lines of evidence suggesting that changes in sediment geochemistry were also associated with low water availability. Fe concentrations in wood and sediment do suggest that water availability was lower in dead forest areas than living areas. We eliminate elevation as a potential driver of differences in water availability because tree mortality occurred even in the lower intertidal zone of dead mangroves which are at the same elevation as the lower intertidal zone of the living forest area. Since other potential water sources: precipitation and tidal flushing are eliminated as being different between the sites, this likely suggests that differences in water availability were driven by regional groundwater flows which are highly spatially variable, For example Stieglitz (2005) highlights that the interrelationships between confined and unconfined aquifers in the coastal zone can result in localised differences in groundwater flows.

The discussion regarding climatic drivers were used to assess the likely regional drivers of the dieback as well as linking climatic variability to the observed Fe geochemistry and uptake in trees. We will make these links more explicit in the revised manuscript by clarifying this aspect of the paper. We will also add to this work evidence from a recent publication by Harada et al., (under review) which provides isotopic data of mangrove leaves in dead and living areas of forest at the same study sites. Less enriched leaf δ13C values in living forest areas suggest increased water availability and are consistent with our evidence from sediment and wood chronologies.

Comment: 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 Fe2+, most of the Fe2+ may quickly oxide to Iron oxyhydroxide due to high aeration. So, during dieback time also, despite the oxidation C2 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.

Response: We will indeed reduce speculation about Fe toxicity in mangroves and instead focus the manuscript on using Fe as a proxy of water availability, and the role of climate drivers in sediment Fe geochemistry in mangrove ecosystems. The drivers of Fe availability in mangrove sediments are complex and are discussed in detail within the manuscript. We incorporate the suggested discussion of Fe plaque formation and how this may interfere with root uptake of Fe and other minerals.

Comment: Apart from above, I have following comments: âAËŸ c 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.

Response: We will rewrite the abstract in line with the above comment. The statement regarding oceanic productivity changes associate with Fe release will be removed from the abstract.

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