Response to Keryn Gedan (referee 1)

General comment
Overall, I think this is a fantastic observational dataset of variation in fish communities during tidally driven variation in DO. The DO data themselves, sampled across a spatial gradient in the mangroves, are very interesting and nicely plotted. The authors recorded fish videos at an impressive number of sites and hours in this remote location, and identified a large number of fish species. The analysis is creative.

Response
We thank Keryn Gedan for her insightful comments that improved the manuscript. We carefully addressed each comment below.

Comment #1
Spatial patterns in DO within the mangrove forest: p. 12, Line 8 notes no difference between in-forest and edge sites, however in the results, p. 7, Line 20 describes a difference in the frequency of DO <50% saturation in-forest and edge environments, with low DO observed more frequently in edge (channel) sites than in-forest. This result surprised me, given that that the in-forest sites would have less water exchange than edge sites. Was this a true difference or not? If there was truly no difference, why do you think the expected pattern of lower DO in the forest than the channel was not observed?

Response
This is a very interesting point that surprised me as well. First of all, the difference in mean DO is not significant between edge and in-forest. The slightly lower DO levels found on the edge I believe are due to the fact that water decreases too fast compare to DO in-forest, therefore within the time that the in-forest is inundated, DO does not decline as much as on the edge where very shallow water remains permanently as it does not get exposed at low tide. This sentence has been added to the discussion to make this point: "There was no difference observed between DO dynamics on the edge and in-forest, however minimum values were slightly lower on the edge because water remained permanently at low tide being subjected to further decline compare to in-forest that became exposed earlier during the tide and therefore experiencing a shorter DO decline period." p.11, lines 17-20.

Comment #2
Discussion section 4.1 "Tidal migrations: stranding or hypoxia?": The text does not answer the question in the section title. Low DO was correlated with the tidal cycle, so it is impossible to disentangle fish movements in response to tidal variation/depth relative to DO. This is carefully worded in the introduction, but not the abstract, which suggests that fish migrate due to low DO, rather than DO being a factor that may drive tidal migrations. The first discussion section would be stronger with a different section title.

Response
The section title has been modified as suggested to be more accurate: "Depth and DO are both potential factors for observed tidal migrations" p.9, line 32.

Comment #3
Related, did in-forest sites bottom out during low tides? It looks as though they did in Fig. 2, for example, just after an ebb tide video period on 2/28/17. This would be an opportunity to investigate stranding patterns. Did all fish leave the site towards the end of this falling tide? That particular occasion was a relatively mild DO period (40-80% saturation), and so perhaps the effects of shallow water and low DO could be separated, at least anecdotally, if not statistically, on that date.

Response
This is a great idea! Unfortunately, I did not sample until the end of the falling tide this day, therefore I do not have the data to identify whether fish were still present towards the end of the ebbing tide. But a future targeted sampling at the end of ebbing tide could indeed provide interesting results to help disentangle the effect of low DO and shallow water depth. In Dubuc et al. (2019) we studied more specifically fish responses to depth patterns and showed that certain species did not use mangrove habitats while depth was high enough for fish to safely access, emphasising that water quality, and especially DO, is probably an important parameter to take into consideration for habitat utilisation.

Comment #4

Discussion section 4.2 “Tidal-induced dissolved oxygen variations” presents two processes that reduce DO in mangrove waters: 1) diel cycles that are the result of dominance of photosynthesis effects during daylight hours and respiration at night, and 2) tidal pumping of porewater into the water column on ebbing tides. These two processes should be better distinguished within the introduction and discussion sections. Separating discussion of the two factors and their relevance to this study (for which fish data is daytime-only) into separate paragraphs would be helpful.

Response
In the introduction, a brief explanation on how DO fluctuates in response to the autotrophic cycle and tide has been added: “The main factor responsible for DO fluctuations is the autotrophic cycle, with photosynthesis occurring during daylight hours, and respiration during nighttime hour, creating a diel-cycle in DO. Another important parameter to consider, especially in intertidal environments, is tide as it is responsible for many physical and chemical changes susceptible to impact oxygen cycle. If these two factors are considered, DO ca be partially predicted, with the lowest DO levels occurring at night or dawn at low tide, following nighttime respiration, while maximum levels are recorded in the afternoon at high tide, following autotrophic production (Kenney et al., 1988; Mazda et al., 1990; D'Avanzo and Kremer, 1994; Tyler et al., 2009).” p.2, lines 25-30.
The manuscript has been modified to clarify this point, especially the fact that fish assemblages were only collected during daytime, and therefore, part of the answer on how DO diel fluctuations impact fish assemblages is not known. p.11, lines 22-25.

Comment #5
The discussion section on “4.3 Species-specific responses to DO variations” highlights some very important results from this study. However, there is a missed opportunity that no species are specifically discussed. What can be said about the fish species, genera, or families observed in the high, medium, and low tolerance behavioral groups identified?

Response
This is an important result from the study indeed, and this discussion section has been extended and modified to emphasise it: section 4.3 p.11-12. It was interesting to notice that species observed in high tolerance group were all species commonly associated with mangrove habitats, using them extensively, while species from the other two groups are less commonly observed, even never recorded before in mangrove habitats.

Comment #6
Please add a legend to the map in Fig. 1.

Response
A legend has been added as suggested.

Comment #7
p. 3, Line 6, “implying that mangrove forests are especially vulnerable to anthropogenic deoxygenation due to their location along the coasts” – I would say, “the aquatic communities
within mangrove forests” are vulnerable, since the mangrove forests themselves are fairly resistant to deoxygenation as low DO does not negatively affect the foundation species of mangroves, and the forest itself can withstand low DO stress quite well.

Response
The sentence has been modified as follow to address this unprecise language issues: “implying that mangrove ecosystems are especially prone to experience anthropogenic deoxygenation due to their location along the coasts.” p.3, lines 8-9.

Comment #8
p. 4, Line 28, What does VLC stand for?

Response
The acronym VLC has been defined in the manuscript to address this comment p.4, line 28.

Comment #9
p. 5, Line 20, In the random forest model, what happens in the case of co-linear variables? Also, can RF account for interactions between predictors?

Response
There is no agreement on how correlated variables impact the prediction of Random Forest models (Grömping, 2009; Neville, 2013). However random forest is a very robust method, and in this study, considering the large dataset used with only few predictors that are all relevant to explain fish assemblages, we believe that overfitting is not an issue. I am not aware of any coding available to account for interactions between predictors in random forest models, but a recent paper has suggested some that could be made available in the future (Gregorutti et al., 2017). Here, we tested for the effect of correlation by building two models, one excluding depth, and the other one excluding DO, to confirm that both variables increased substantially the prediction capacity of the random forest, which was the case.

Comment #10
p. 5, Line 30, Please explain "out-of-bag error."

Response
A short definition of "out-of-bag error" has been added to the manuscript: p.9, lines 30-31.

Comment #11
p. 10, Line 16, Tidal period is generally defined as high to high or low to low. High to low would be half of a period.

Response
The sentence has been modified to use the correct term as advised p.10, line 18.

Comment #12
p. 11, Line 21, parenthetical "High tolerance pattern" is confusing. I suggest "(i.e. most species exhibited the "High tolerance" pattern)."

Response
The sentence has been modified to enhance understanding as suggested: p.11, line 28-29.

Comment #13
p. 12, Line 15, it seems like "value" of mangroves, here, implies that fish diversity or abundance is a proxy for mangrove forest value? Please clarify.

Response
To avoid confusion around the term "value" a definition has been added when first mentioned in the introduction p.1, line 14; p.1, line 31.