Response to the comments of reviewer #3:

We thank the reviewer for the valuable comments and the time examining the manuscript. We have carefully addressed the comments in the following response and provided some further analysis and statements to clarify the reviewer's major concerns. The comments of the reviewer are in black and our response are in blue.

General comments:

In this manuscript, authors assessed the impacts of climate change and artificial channel on the water retention time, salinity and stratification of the Peel-Harvey Estuary (a large chocked-type lagoon) in 1970-2016, and their evolvement under current climate projections, based on a 3D finite-volume hydrodynamic model. There are some issues which are as follows.

Major Points:

1. In introduction section, authors mainly introduced the importance of the topic and what they did. However, the related work of other researchers was not introduced.

Response: Thank you for the suggestion. It was our aim in the introduction to highlight the motivation and importance of the topic, identifying more specific research questions in the context of prior work and introducing the approach and aims. Some of the related work specific to the Peel-Harvey estuary site was briefly introduced in the introduction section (e.g. Brearley, 2005; Environmental Protection Authority, 2008; Rogers et al., 2010; Gillanders et al., 2011; Hallett et al., 2018; Valesini et al., 2019) and in the site description section (e.g. Bicknell, 2006). We acknowledge that we could further strengthen the link to prior studies of the generic impacts from climate change and anthropogenic intervention on lagoon hydrology. We will therefore undertake a further review of prior related work to further refine the introduction as part of a revision. An example refinement of the key paragraphs is drafted below:

- Globally, many studies have shown that coastal lagoon systems are vulnerable to climate change, including the factors from reduced flow and/or sea level rise (Nicholls & Hoozemans, 1996; Nicholls et al., 1999; Scavia et al., 2002; Chapman, 2012; Newton et al., 2014; Umgiesser et al., 2014). In particular, shallow coastal lagoons respond quickly to the ocean and catchment inputs whilst their geomorphological characteristics (bathymetry and especially the configuration of their inlets with the open sea) affect their hydrodynamics including circulation patterns, flushing time and water mixing (e.g. Smith, 1994; Spaulding, 1994; Koutitonsky, 2005). This attribute has meant that these systems therefore amplify the salinity and temperature changes expected from climate change relative to the open sea, and that they can serve as sentinel systems for global change studies (Ferrarin et al., 2014).

- Anthropogenic activities introduce hydrological modifications to the lagoons associated with water resource management (e.g. Hollis, 1990; Kingsford et al., 2006; Gong et al., 2008) and engineering modifications (Ghezzo et al., 2010; Garcia-Oliva et al., 2018). These enhance the need to assess lagoon hydrological function if these impacts are to be predicted and, where necessary, mitigation measures developed, in conjunction with climate influences.

- The opening of artificial channels in the lagoon systems may fundamentally alter the hydrology and the aquatic communities (e.g. Lord, 1998; Manda et al., 2014; Prestrelo and Monteiro-Neto, 2016; Garcia-Oliva et al., 2018). Changes in the connection of restricted lagoons with the ocean can exhibit a marked change in the salinity pattern or the extent of hypersaline conditions (Kjerfve, 1994; Gamito et al., 2005), which can subsequently influence the ecosystem within these lagoons (Gamito, 2006; Garcia-Oliva et al., 2018);

- Although both the effects of climate change and the creation of artificial channels have been shown to be key drivers of lagoon hydrology, and that the hydrology of lagoons has been changing at a faster pace in the past decade from a combination of human activity and climate variability (Cloern et al., 2016), to our knowledge, studies on the interaction of climate change and large engineering intervention on lagoon hydrology are yet to be explored in detail.

Full reference of the cited work are provided in the Reference section at the end of this response.

2. In the Methods section, it is better that all data used and their resources were introduced together. It seems that some data was introduced, but some not. In page 7, lines 202-203, "Gauged flow rate data for the Murray River, Serpentine River and Harvey River were applied to the hydrodynamic model whenever they are available. For the missing periods in the gauged flows and the ungauged drains, the output from the Source ...". Which data is available, and which is not available? Which periods are the missing periods? Where are the ungauged drains?

Response: Thank you for pointing out this issue and we acknowledge that the catchment flow set up can be described in more detail. We will update the locations of the ungauged flows on the site map, and draft a supplementary paragraph in the site description section as below:

"The coastal catchment of the estuary is drained by three major river systems: the Serpentine, Murray and Harvey Rivers (on average contributing to 16.4%, 46.5%, and 30.8% to the total flow, respectively), and numerous minor drains (contributing 6.3% to the total flow) (Kelsey et al., 2011). Gauged flow rate data for Murray River were available from 1970 to present, while for Serpentine River and Harvey River were available from 1982 to present. For the missing periods in the gauged flows (year 1970 and 1978 for Serpentine River and Harvey River) and the ungauged drains, the output from the Source (eWater®) catchment modelling platform (Kelsey et al., 2011; Welsh et al., 2013), operated by the Western Australia 204 Department of Water and Environmental Regulation, was used."

3. About the meteorological inputs of the model, there are various data sources: station Halls Head before 1981, climate model simulations 1981-2000, and station Mandurah since 2001. The different sources of data could influence the results.

Response: We agree that meteorological inputs from different sources would influence the results due to site specific biases they may have. The major meteorological factor that affects the hydrology is the wind, through its impact on evaporation and circulation; we have therefore undertaken a further investigation into the wind regimes from these three sources. As shown in the figure below, the wind regimes of these data sources showed similar distributions in wind magnitudes and directions, though the average wind speed in the Mandurah station record is slightly smaller when compared to other two sources. We will add the above analysis as appendix material to the manuscript and indicate the changing selection of meteorological sources could introduce some bias into the results in the selected modelling years.

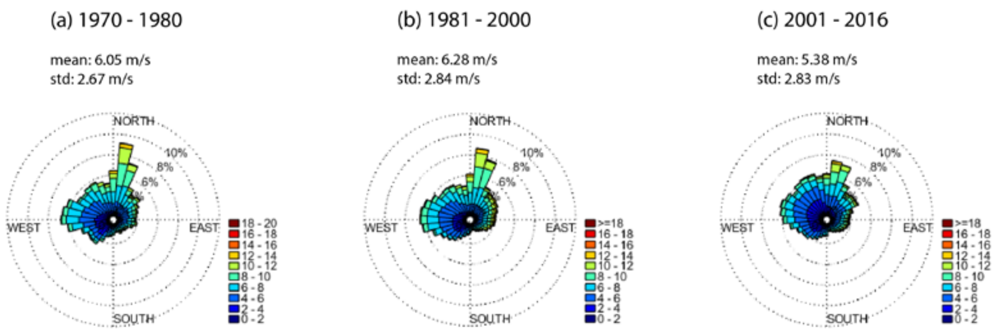

Figure 1. Rose plot of wind condition in years of (a) 1970-1980, obtained from the Halls Head weather station; (b) 1981-2000, obtained from the WRF weather model; and (c) 2001-2016, obtained from the Mandurah weather station.

4. There is no calibration of the model parameters.

Response: This is a similar comment as raised by Reviewer #2 which we reply to here also.

Ideally, we could adopt an automated calibration approach, aiming to minimize error in key model predictions using an objective function and a pre-determined acceptable criteria for model acceptability (e.g. Doherty and Johnston, 2003; Arhonditsis et al., 2008; Bahremand and Smedt, 2010). This approach has yet to receive widespread up-take in the hydrodynamic modelling community, particularly where 3-D models are employed to resolve variability in stratification, due to considerable computational burden running these models hundreds or thousands of times. Given the complex nature of the model domain that we wanted to adopt to resolve the river reaches to the tidal limit, and an individual run-time exceeding one-day per year, we therefore could not adopt an automatic optimization approach.

Instead, we adopted a structured hierarchical approach to calibration, similar to those described in Muleta and Nicklow (2004) and Hipsey et al. (2020), to manually calibrate the model. We first identified the key parameters of importance to the hydrology in the current study based on literature review and prior expert knowledge. In this stage, the key parameters were identified to be the bottom drag coefficient (which can vary spatially), the light extinction coefficient, bulk aerodynamic coefficients, and the mixing scheme options associated with the vertical turbulence model (in this case this is parameterized through the GOTM plugin). In the second stage, we evaluated a matrix of simulations, each with pre-determined parameter vectors and model options, by assessing model performance of each simulation against the observed salinity and temperature data at six stations within the estuary (at both surface and bottom levels), and the water elevation at the center of the Peel Inlet. For these we tabulated a summary of error metrics R, NSE and PBIAS, and used this to identify the final parameter options used for the validation simulations that were presented in the paper. The assessment was targeted, and included comparing performance of mixing models, against metrics relevant to the analysis such as stratification strength and hyper-salinity associated with evapo-concentration We also acknowledge issues in boundary condition data may affect the calibration, and we therefore spent considerable effort on the data quality control of the time-series of tide, weather, and catchment inputs to the model. In addition, the sensitivity of predictions to the selected environmental factors of air temperature, sea level mean height, and the bottom drag coefficient were performed.

We acknowledge that this approach is not necessarily providing the most optimum parameter set from a mathematical point of view, however, given other uncertainties in the spatial maps of vegetation (and therefore benthic drag) and potential error or bias in some of the assumed boundary conditions, it is our view that the model performance is close to the optimum and sufficiently accurate for the scale of our assessment.

To address this in the paper, we therefore propose to add in the revised version a brief summary of the calibration approach, and the results as supplemental material to this manuscript, and provide an improved discussion that describes the known uncertainties and limitations of the model in this regard.

5. Topography is also an important factor affecting the hydrology of an estuary. Between 1970 and 2016, how did the topography of the lagoon and the natural channel change and affect the hydrology? There is no anything about this.

Response: We agree that changes to the lagoon bathymetry could be a factor that could lead to changes in the hydrology of the estuary, and further explanation is required to clarify the potential significance of this. Unfortunately, there is not successive survey data to allow us to fully assess this. Hence, we used the latest morphology dataset from the Western Australia Department of Water, obtained in year 2016 (integrated DEM at 2m resolution), and assumed morphological change over the study period was not significant, except through the construction of the Dawesville Cut. The estuary morphology over the study period may have been modified by: (1) changes to the net sedimentation pattern of particles; and (2) dredging activities related to marina and navigation channel developments.

The net sedimentation rates in the Peel-Harvey Estuary had been investigated in some early research studies. Gabrielson and Lukatelich (1985) estimated a net sedimentation rate of about 0.4-1.5 mm/year in the Peel Inlet and 2.9-6.7 mm/year in the Harvey Estuary; Hodgkin et al. (1980) estimated an overall rate of sediment deposition of 0.3 mm/year in the estuary. Assuming the rate is constant, the maximum total sediment deposition is about 75 mm in the Peel Inlet and 335 mm in the Harvey Estuary, over a period of 50 years, assuming not change in these net rates over time. However, these rates were estimated prior to the construction of the Dawesville Cut. After the year of 1994 when the Dawesville Cut was constructed, the sediment deposition in the system, especially in the Harvey lagoon, is expected to decrease due to higher tidal flushing and larger currents near to the channel. As we illustrated in the manuscript, a change in the tide elevation of ±0.15m, which could be theoretically equivalent to the change in the depth of the estuary, would have caused a small change to the hydrology compared to that introduced by the opening of the artificial channel, and the reduced river flows. The reduced flow rates over the course of the study period would also lead to a reduction in sediment loading. Therefore we argue the impact of sediment deposition on the morphology is small.

The development of canal estates and navigation channels would have further changed the local morphology, but is expected to only slightly modify the estuary hydrology at the regional scale. For example, The Yunderup navigation channel, located at the east side of the Peel Inlet, is one of the more significant dredging projects in the Peel-Harvey Estuary in the past decades. The Yunderup channel has a length of ~3km (mostly in the canal estate and shallow water areas) and a width of ~50m. The total area of this channel is ~0.015 km^2, which is negligible when compared to the area of the east Peel Inlet of 33.5 km^2. We therefore assume the changes brought about by the local dredging activities are negligible in analyzing the large-scale estuary hydrology when looking at the average properties over the regions, though we acknowledge some local-scale differences may result due to these changes.

We will integrate the above discussion into the manuscript to clarify the limitations of this study due to the assumption of static estuary morphology, whilst highlighting that we believe this does not change the nature of our conclusions.

6. The results for 2040 and 2060 based on projected climate seem to be too simple. And there is no any explanation.

Response: We acknowledge that our future climate projection is relatively simple to investigate the future hydrology in the Peel-Harvey Estuary, although our projections for weather and flow change were based on the average trend reported from more detailed studies using an ensemble of climate models (Silberstein et al., 2012; Smith and Power, 2014). The Peel-Harvey region has experienced a widely reported decline in rainfall over the last several decades (CSIRO & BoM 2007; IPCC 2007; CSIRO 2009; Hope & Ganter 2010). The trend in rainfall decline is expected to continue, based on the climate projections from general circulation models (GCMs) results (CSIRO 2009; Smith and Power, 2014). Given the nature of our research questions was to extrapolate the mean trend that we reported from the hind-cast simulations, we focus the future scenarios on the changes of hydrology under the projected average reduction in the flow from the ensemble models (Smith and Power, 2014), with an assumed mean rate of sea level rise (Kuhn et al., 2011), to highlight the general trend and allow for prioritization of adaptation strategies such as environmental water allocation policies. This approach is over-simplistic also in that it assumes no seasonal change in hydrologic trends, and there has been recent evidence that increasing summer floods are occurring and the winter peak flows are decreasing as a fraction of the annual total (McFarlane et al., 2020). We will enrich this aspect of the scenario description by integrating this response into the manuscript to further explain the approach of future projection, plus add to the discussion on the significance of this uncertainty and the requirement of future research on this topic.

7. In 4.1 section, the contents in paragraphs 1, 2 and 3 mostly repeated the results. Here, the compare between your results and other related research should be shown.

Response: Thank you for the suggestion. We focused our discussion on how the interaction of the climate change affects with the artificial channel on the hydrology in these paragraphs. We first compared results from the current study because study cases of the interaction of the climate and artificial channel are relatively rare. We will extend the discussion to include more related research on the lagoon hydrology under climate stress (e.g. De Pascalis et al., 2011; Umgiesser et al., 2014; Newton et al., 2014) and anthropogenic interventions (e.g. Ferrarin and Umgiesser, 2005; Gong et al., 2008; Ghezzo et al., 2010; Garcia-Oliva et al., 2018). Specifically, as a response to this comment and also to the suggestion by the reviewer #2, we propose to carry further modelling work to study the mixing efficiency in the Peel-Harvey Estuary using the same numerical methods of water renewal time and flushing time as the studies of Umgiesser et al. (2014), who compared the impact of climate change on the hydrology of 10 Mediterranean lagoons. The advantage of the mixing efficiency method is that it is a useful parameter for comparing and classifying lagoons, and ideal to the Peel-Harvey Estuary for comparing the mixing character before and after the construction of the artificial channel, as well as under wet and dry conditions. The inter-comparison of mixing efficiency changes affected by the climate and artificial channel will therefore be added to the discussion, and placed in context of the results of Umgiesser et al. (2014) and other related lagoon studies (Jouon et al., 2006; Gong et al., 2008; Safak et al., 2016) who used similar water transport timescale to investigate the hydrology in estuarine lagoon environments. We will also expand the discussion to address other comments raised about model calibration and accuracy, by strengthening our discussion of model performance, and the limitations and uncertainty in these predictions that can form the basis of further work.

Minor points:

1. All important stations or locations mentioned in the manuscript should be occurred in Figure 1.

Response: Comment accepted. We will add the ungauged inflow locations and the weather station locations to the site map.

2. GL and mAHD should be changed to international units.

Response: Comment accepted. We will convert the GL to m^3, and check through the manuscript to use the SI units consistently. The unit of mAHD stands for elevation in meters with respect to the Australian Height Datum and we will explain this unit in the revision.

3. Impacts on the stratification were shown in the results and conclusions. Why were they not mentioned in abstract?

Response: Thank you for pointing out this issue. We will add the impacts on the stratification to the abstract as "The opening of the artificial channel is shown to increase the seawater fluxes and the salinity stratification, while the drying climate had reduced the salinity stratification in the main body of the estuary."

4. The citation of reference is disorder. If several references are cited together, they should be put in order according to publishing year.

Response: Thank you for pointing out this and apologize for the disorder of the reference citation. We will go through the manuscript and clean up the citation format.

5. In page 6, line 186, WA region means Western Australia? The indication of this abbreviation is not seen

Response: Yes the WA is an abbreviation for Western Australia. Sorry for missing the explanation of the abbreviation which will be added into the manuscript in the revision.

6. In Figure 1 “Peel Estuary” is indicated. However, in the text and other figures only “Peel Inlet” can be seen. Are they the same location? If yes, in Figure 1, text, and other figures they should be the same.

Response: Thank you for pointing out this. The Peel Inlet is also referred as Peel Estuary by local management agencies, but the formal name for this lagoon should be Peel Inlet. We apologize for the misuse of this name in the site map. We will unify the name to be Peel Inlet in the revision.

7. Page 10, lines 277-279, authors said “the tide elevations in the ocean showed similar characteristics in 1990 and 1998 in terms of the annual mean sea level (-0.071 mAHD and -0.027 mAHD in 1990 and 1998, respectively) and tidal range (both < 1 m)”. The plot (a) of Figure 4 shows the detailed sea levels in 1990 and 1998. Why did you only compare the annual mean sea level? It can be seen from plot (a) that the sea levels in 1998 also had a wider range variation, similar to the estuary surface elevation.

Response: Thank you for pointing out this. We agree that the tide condition in the year of 1990 and 1998 need to be further declared. We further analyzed the exceeding percentage distribution of the tide elevations (Figure 2) and the tide constituents (Table 1) in these two years. The tide elevations in these two years both ranged between -0.6 mAHD to 0.8 mAHD and present a similar exceeding distribution pattern, though the elevation is the year 1998 is slightly higher than 1990. The tide constituent analysis also indicated that the principal constituents (K1, O1) have similar potential energy and amplitudes contributing to the tide. We will integrate this analysis into the manuscript and further explain the use of these two years for comparison.

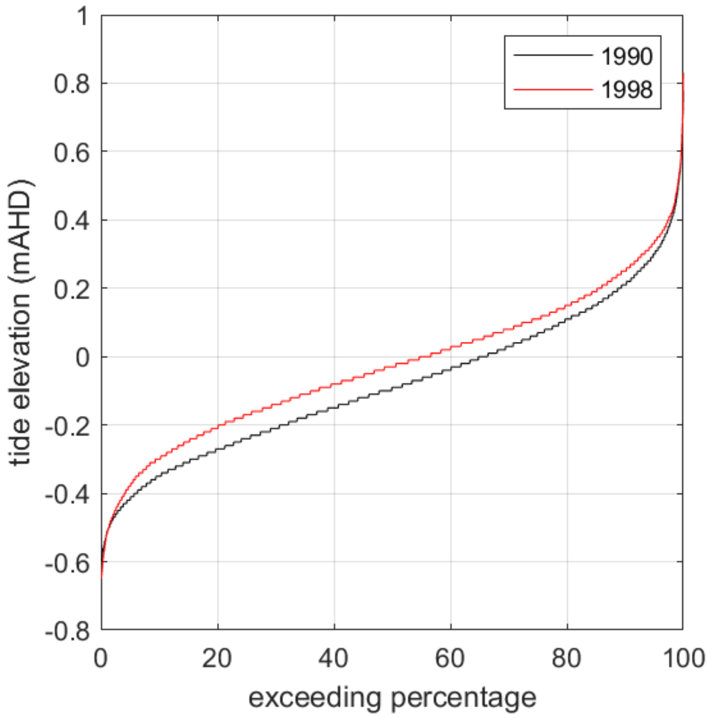

Figure 2. Exceeding plot of tide elevations in the year 1990 and 1998.

Table 1. Comparison of principal tidal constituents in year 1990 and 1998.

Constituents	**Potential Energy (%)**		**Amplitude (m)**	
	1990	**1998**	**1990**	**1998**
K1	57.19	56.79	0.159	0.156
O1	30.28	30.05	0.115	0.114
P1	5.45	5.66	0.0490	0.0494
M2	3.92	4.31	0.0415	0.0431
S2	3.16	3.20	0.0373	0.0371

8. Caption of Figure 4 is not proper. It looks like three figures.

Response: Comment accepted. We will add the tide exceedance percentage analysis plot (as response in above comment) into this figure and reword the caption of Figure 4.

9. Table 3: (1) In caption of the table, "Summary" should be deleted. (2) About the performance of salinity, it can be seen that errors after 1998 are clearly larger. Why?

Response: (1) Comment accepted. We will delete the "Summary" from the caption. (2) We wonder the introduction of the artificial channel after 1994 may have increased the complexity of salinity in the 6 monitoring sites within the estuary, therefore introduce more bias in the model output when compared to observation. For example, mechanical sand bypassing has been undertaken in the Dawesville Cut each year to maintain the channel since construction. This operation may have affected the water exchange, however, cannot be resolved by the hydrological model. We will add this point into the result section to discuss the possible reasons for the larger errors after 1998.

10. Caption of Figure 10, "The darker symbols indicate the years with accidental summer rainfall events and caused the catchment inflows higher than 15 GL". In this sentence "and caused" seems syntax error.

Response: Yes it is a type error and thank you for pointing out this. We will reword the caption to be: "The darker symbols indicate the years with accidental summer rainfall events, during which the total catchment inflows in summer season are higher than 15×10^6 m^3."

Reference:

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