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9 November 2020

Re: Expert Review: Salinity Modelling of Mine Pit Water Discharge into Fortescue River

Dear Ryan,

Thank you for your request to provide RPS with an expert review of your salinity modelling of mine pit water discharge into the Fortescue River, as described in the document provided to Hydronumerics: *Fortescue River Discharge Modelling: Background Salinity Variability Study*, *MAW0762J.002* (the report).

I have examined revision 2 (Rev 2, dated 1/9/2020) and revision 3 (Rev 3, dated 5/11/2020) of the report. My comments and the responses from RPS have been recorded in the tracking spreadsheet attached.

The review considers the requirements of the scope that has been described in the report and I conclude that the modelling software and general approach taken by RPS is appropriate. I have reviewed the responses to my comments provided by RPS and I am satisfied that the revisions that have been made and responses that have been given adequately address my comments.

There are a number of important limitations to this review, which are:

- This review considers only the material presented in the report;
- Other project reports referred to in the report have not been reviewed as part of this review. It has been stated by RPS that these reports have been independently reviewed by others; and
- Consideration of ecological implications of the findings of the report, which focusses only on the hydrodynamic and salinity modelling, are out of the scope of this review.

Kind Regards,

Dr Peter Yeates

Services Director, Hydronumerics Pty Ltd

Review Response Tracking Sheet	
Project	Fortescue River Discharge Background Variability Study
Number	MAW0762J.002
Date	5-Nov-20
Reviewer	Dr Peter Yeates, Director-Services, Hydronumerics
Report	MAW0762J.002_Fortescue River Discharge Background Variability Study_Rev 3

#	HN Comment	RPS Response	Report Modified	HN Response	RPS Response
1	I have assumed that I don't need to assess the initial diffuser and hydrodynamic study and that these have been reviewed by others and the conclusion has been that they are fit-for-purpose.	Yes. The diffuser and hydrodynamic study were independently reviewed and the monitoring performance of the system to date has met regulatory requirements. The future regulatory requirement may change subject a future licensing application supported by the results of this study and a related ecological scope covered by a third party consultant. It is noted that the near-field diffuser performance may need to be re-assessed if regulatory requirements are modified.	N	Agree	
2	Can you add some clarity around which salts contribute most to the salinity?	The main ionic constituents of the discharge water are Chloride, Sodium, Calcium and Magnesium	Y (Section 1.1)	Agree	
3	Does this refer to previously modelled?	Yes, text updated to "The actual performance of the previously modelled existing diffuser system has been evaluated by CPM"	Y (Section 1.1)	Agree	
4	Can you elaborate on what is meant by general?	The word 'general' replaced with 'far-field', also the Scope (Section 1.2) was added.	Y (Section 1.1)	Agree. New scope section offers good clarification	
5	I have not reviewed these and assume that they demonstrate agreement	Its noted that the previous reports cited were not part of the formal scope of this review. The previous studies completed before the diffuser system was fully operational but routine regulatory monitoring by CPM has confirmed that the system is operating within the regulatory requirement it was designed for.		Agree	
6	Can you clarify salinity and/or TDS as the measure(s) of interest. There is use of both terms in the document that may need some explanation.	Added "The mass of total dissolved salts (TDS) in both the discharge waters and ambient waters is dominated by inorganic salts. Sodium, chloride, magnesium, calcium and potassium account for approximately 90 to 95% of the TDS mass. On this basis TDS concentration (mg/L) is considered to be equivalent to salinity (ppt) for the purpose of this study."	Y (Section 1.1)	Agree	
7	Is the 18m a precise measurement over a sample length or is this the distance at which the samples where taken? Can you provide more explanation about the data (or a figure). Given the measurements are periodic in a tidal system, how much confidence can be attached to this statement? Are there peaks past 18 m that lead to short-term elevated exposure?	The paragraph in question is basically alluding to the fact that the existing system is meeting current regulatory requirements based on the measurements required under current regulation. The regulatory requirements are based on percentile tolerances so it is possible for near-field limits to be exceeded on occasion but not more than 20% of the time. It is possible that the 18 m near-field region may be exceed on occasion but RPS believes it would take a relatively detailed field study to evaluate this. The reference to 18 m has been removed and replaced with a broader term 'near-field'. The specific 18 m length scale is really only relevant to existing regulatory requirement but as mentioned in comment 1, the regulatory requirements may be adjusted following review.	Y (Section 1.1)	Agree, this appears reasonable	
8	Can you clarify whether this relates to more concentrated and/or larger flows?	This is clarified later in scenario descriptions	N	Agree	
9	Will the discharge still follow the ebb-tide only rule?	No, as clarified later in scenario descriptions	N	Agree	
10	It may be good to offer the range here from spring to neap	Added "The tidal range is approximately 4 m during springs and in the range of 1-2 m during neap tides."	Y (Section 1.2)	Agree	
11	As above, can I suggest using salinity or TDS through the report and perhaps describe the TDS breakdown of the plume water and if/how it differs from the natural seawater.	The inconsistent use of TDS and ppt is because figure 1.2 was provided directly by the CPM. The additions made following comment 6 hopefully address this by making more explicit the assumption that ppt and TDS are used interchangeably for the purpose of this study		Agree	
12	I agree that groundwater and evap may contribute to the fluctuations in baseline salinity. It may be out of scope but is it possible to run some approximate calculations as to what these contributions may be and if they match the data. I realise that you may not have the information to do this, but if so it may give good context to the results against natural variation.	This is out of scope for this study but some additional response to this comment is provided in #23	N	Agree	

13	Despite how shallow the systems is do the salt gradients lead to any stratification? It may be worth noting this one way or the another in this introduction. In addition, the flushing of the system in the early months of the year is not reflected in the salinity data which is 37+ ppt for all samples. Can you explain whether this is because of sampling frequency? My comment stems from asking the question whether the discharge potentially removes or decreases natural events that lead to intermittent freshening of the estuary and whether this is ecologically important.	RPS doesn't have sufficient measurement data available to conclude if stratification occurs on occasion. The profile data that we have been provided doesn't show any evidence of stratification but there's not enough data to support a definitive statement. There is no evidence of freshwater flushing in the baseline data because the sampling resolution of that data is monthly and the seasonal flushing isn't reliable from year to year. There are years when the Fortescue River has no flow and occasional years when cyclones cause very large but relatively short-lived flows such as 2020. The years 2018 and 2019 were particularly dry with almost no flow. In Fig 1.3 the monthly mean flows were plotted together with the monthly average flows to highlight how cyclone events cause the average flows to be well above the median flows. The flows associated with a cyclone event are orders of magnitude higher than the discharge so we don't expect the discharge system will interfere with this kind of intermittent flushing.	N	Agree. If I understanding you correctly is it worth considering making a statement similar to your comment here regarding the expected lack of impact?	Yes, added a closing sentence to Section 1.3 to state expected lack of impact
14	Refer to comment above but at a conceptual level this appears to be an appropriate modelling approach. However, as discussed the curvilinear approach has not been used so you may wish to remove your reference to this as it may be misleading.	reference to curvilinear was removed	Y (Section 2.1)	Agree	
15	Do you have data to comment on what the wave field is like to justify this? Is it addressed in the other reports?	RPS understands based on communication with CPM that the waters of the estuary are well protected from waves. The offshore wave energy is low in this region except during cyclones. Therefore we think its conservative to assume there will be additional mixing facilitated by wave energy in the estuary.	N	Do you mean 'no additional mixing' for the conservative approach. If so, seems reasonable.	Yes, we added "The exclusion of small waves effects is conservative with respect to mixing."
16	See comment above about impact of diffuser as times when the estuary may otherwise (and naturally) be fresher?	We interpret this comment as asking whether the ecosystem might be reliant or dependant on the salinity level in the estuary falling to some minimum level on at least an annual basis. This particular question is beyond our scope but as mentioned in response to #13, we think the normal state of this estuary is ocean salinity or higher except for short periods after irregular cyclone events. In a median year (i.e. no major cyclone event) it is noted that that the estuary may be expected to have a minimum salinity in February when median baseflow is highest (Fig 1.3), all other things equal. We agree that the impact of the diffuser system would be to increase the minimum salinity level in February. We think the magnitude of the increase would be similar to the general level of increase found for P20, P50 and P80 cases, i.e. in the range of 2-6 ppt. We have added an additional paragraph to the conclusion section so the potential ecological significance is flagged.	Y (Section 6)	I agree with the statement made.	
17	I found this section a little confusing. I assume that this report is focused only on the grid in Fig. 2.3. Consider working out from this as a starting point to explain the grid nesting.	Added some detail to the first sentence to bring the inner model grid to attention earlier in the section	Y (Section 2.1.1)	Agree, just check typo on first sentence	
18	A couple of notes here: A figure of the channel slice showing the grid may help show this. Will the sigma grid make it hard to resolve a halocline? Does this matter?	We think the use of 20 vertical layers is sufficient to resolve the halocline with a sigma grid. The model indicates the potential for some stratification during neap tides but vertical mixing otherwise, which seems reasonable given the large volumes of water exchanged during regular and spring tides. Stratification would likely be important to the biology if it were persistent through the duration of the discharge, however, CPM have advised that their monitoring data to date doesn't show evidence of stratification with the existing system in operation.	N	Agree, is it worth making this statement in the report?	Yes, we have added a new paragraph to the conclusion Section 6 to make comment on stratification.
19	Is this the same at Delft3D-FLOW?	Yes, text has been corrected	Y	Agree	
20	Was the sensitivity to the choice of Dxy tested?	No. The choice of Dxy was based on RPS experience and consistent with guidance from Delft documentation.	N	Agree. Consider stating this assumption based on DELFT guidance.	Yes, we have added a sentence in 2.1.2.1 "The background horizontal eddy viscosity parameters used for the model were selected based on Deltares (2013) "
21	Does this represent a particular substrate?	Not particularly, this is a general default value and is around the middle of the normal range suggested by Delft documentation. There would have been potential to calibrate this parameter but the validation of the model was considered acceptable using this value.		Agree, but perhaps also consider stating the origins of the assumption	Yes, we have added to the last sentence of Section 2.1.2.1
22	Ryan, I have assumed here, as per previous comments that these board set-up descriptions and assumptions have been written up and review in previous reports.	Yes, the broad model characteristics were unchanged from the previous set-up that was independently reviewed.	N	Agree	

23	Without the surface fluxes how does the model increase upstream salinity? Or is this not important for this assessment and if so why?	For this scope the upstream salinity gradient is only introduced as an initial condition and there was no ongoing source of upstream salinity in the model apart from the discharge itself. The upstream source of salinity is of significance but the mechanism of background salinity is uncertain. The source of background salinity is likely a combination of groundwater and evaporation from salt pans that are perhaps only connected to the estuary at high tide. The three different initial conditions tested for each scenario in this study were designed to broadly characterise to the background variability based on past measurements because explicit representation of the salinity sources in the model would have required model inputs that were beyond the scope of this study and uncertain as to whether they could be obtained within practical time and resource constraints.		Agree in principle, you may want to consider elaborating/reiterating here on how the initial conditions have been used to impose the gradients and why.	Yes, the 2nd paragraph of Scope (Section 1.2) has been expanded on this point
24	Is it of concern that mixing zone length < grid scale may result in a overly dilute initial discharge of the diffuser water into the model? You may wish to discuss further by 16 m is sufficient.	For this scope the near-field diffuser characteristics were not revisited from previous work. The diffuser was designed to achieve at least a 27x dilution of the discharge and the D3D model grid scale was set to match the designed dilution. The scope of this current work was to focus on the capacity of the estuary to accept a larger volume of discharge. Once that capacity has been assessed and revised regulatory targets are decided the original near-field work will need to be re-evaluated. We have added a paragraph to the section to clarify.	Y (Section 2.1.2.5)	Agree	
25	Was this done to match model output frequency or remove spikes?	Added "The 1-hour filter smoothed high frequency fluctuations that were assumed to be associated with sub-grid scale flow features and high frequency fluctuations that were not represented in the model boundary forcing."	Y (Section 2.2)	Agree	
26	A couple of observations to consider: Does the over-prediction of spring tide velocity amount to any concern with regards to discharge dilution? Does the filtering of the field data smooth the peaks too much? Does the ADCP accurately pick up the surface bin(s) where velocities are highest, or is it clipped? I'm not suggesting these are issues, as I agree the results are generally in good agreement with the data - merely notes to consider. I also realise this may be written up elsewhere.	The slight over-prediction of velocity apparent in Fig 2.5 is more clearly quantified by the Q-Q plot in Fig 2.7, which we think confirms the model predictions are fit for purpose of this study. We think that because the water level changes are quite well predicted some of the differences in measured and modelled velocity are likely explained by errors in modelled bathymetry and/or smoothing of bathymetric features to the model grid scale. We agree the ADCP can miss the very near to surface current within an error margin of the height of the bin but we think the dominance of tidal forcing in the estuary implies that depth averaged currents give a reasonable representation of the water column.	N	Agree	
27	As noted above is there reason to consider the case of potent effect on the estuary during flushed conditions. We can discuss this.	RPS response to this comment is provided in #13	N	Agree, see above.	
28	Is this a baroclinic response? Are the plan-view figures showing surface or average?	We checked the plan view and confirmed that the low salinity near the shoreline is an artefact due to the stranding of some water in grid cells when tide is low, so anomalies of cell wetting/drying algorithm rather than a baroclinic response. The plan view figures are mid-depth, now added to caption of Fig 4.1.	Y (Section 4)	Agree	
29	Would it be worth also adding context around flooding events and storm surges?	We think this response is covered in responses to #13 and #16		Agree, see above	
30	I don't know what/where the important ecological receptors are but it may be worth justifying the choice to use mid-depth.	The main focus of the output is on the depth averaged results at the three monitoring stations for consistency with past measurements and current regulations. The plan-view figures are designed to provide additional context so the mid-depth range was chosen as representative, avoiding the extremes up surface and bottom layers that are extremely thin when water level is low.	N	Agree, consider making this point in the text.	Yes, modified the first paragraph of Section 5.2 to introduce these concepts
31	See comment above. Was the bottom considered?	The scope for this work was to consider the potential for general build up of salinity in the estuary as indicated by the depth averaged results at the three monitoring stations. As mentioned in #18, the model predict some stratification during neap tides but this isn't persistent so depth average gives reasonable representation of the system. We have added an additional paragraph to the conclusion section to flag that the effect of both the depth averaging and the time averaging is to smooth peaks and troughs in the salinity data.	Y (Section 6)	I agree with this statement.	
32	I agree that filtering helps with the interpretation, however, it may be worth commenting on the use of 24-hr filter and how that it keyed to the natural tidal cycles.	This comment is addressed by the response to #32		I assume you mean #31.	Yes

33	For discussion: I agree with the analysis as presented but is this purely a hypothetical/numerical exercise as there would likely be a some balance struck between flushing and natural salinity increases where salinity in the estuary reaches a steady state (on average) somewhere above 37 psu and is only lower if freshwater is coming in? Would like discuss if I am thinking about this the right way as/and it may been to be reiterated here.	Yes, agree with your interpretation. The lack of an ongoing natural salinity source term is significant, however, by comparison to the baseline simulations without discharge the 'above background' contribution of the diffuser was able to be isolated from the background and is summarised in conclusions.	N	I agree with what you have indicated, however as with #23 above, consider making it explicitly clear that this process has been considered and built into the initial conditions.	Yes, see #23
34	I suggest showing other times in the tidal cycle to illustrate this effect	For this scope the focus is on the far-field build-up of salinity and the near-field component is to be addressed iteratively once the overall capacity of the estuary ecosystem to tolerate an increased level of salinity has been established by the wider team. At present its not clear what the ultimate near-field dilution targets/distances will be under a revised regulatory regime	N	Agree	
35	Does the 80th initial conditions + no salinity increase from evap or groundwater mean that the potential worst case is missed when you then add the discharge? For the tables are entries for the peak early needed to highlight a conservative measure of potential impacts? How does the choice of values 50% and 80% and end of cycle relate to the ecology/exposure?	We agree the 80th initial condition isn't the worst case and that the worst case would be the maximum (100th) baseline plus the discharge. We have re-worded the final paragraph of conclusions to make this point clearer. The choices of 50% and 80% were made by the client and other consultants, we understand the choice relates to the typical way regulations are applied in WA.	Y (Section 6)	Agree, check typos in sentences. Is it above 10 ppt at times?	Yes, see #39
36	Have you considered the effects further upstream of FR3. At FR3 the discharge leads to +~4psu for this case at the end of the simulation. Does adding this, plus potentially higher evap concentrations further upstream need to be considered?	The output locations were specified by CPM - this is now added to a 'scope' statement in the background. We think the salinity gradient between FR2 and FR3 is small enough to suggest the direct impact of the discharge doesn't accumulate upstream, however, we agree that the natural background salinity is probably a bit higher on average at the upstream limit of the estuary than at FR3 but there is no baseline data to confirm. We think the large volume exchange in the estuary during regular and spring tides would constrain the development of a large and long term gradient.	Y (Section 1.2)	Agree	
37	It may be dealt with elsewhere in the approval documents but how important are the short-term peaks? First in terms of elevated psu exposure in each peak to but also in terms of repeated peaks. The data suggests high peaks (50-55 psu) have been observed but how often are these? The model suggests peaks this large for days on end during spring tide.	This has been touched on in response to #31 and goes to the ecological implications that are being considered by other consultants. The re-worded conclusion section (2nd last para) makes this point explicit so it can be brough to their attention.	Y (Section 6)	Agree	
38	This needs some clarification as I assume you are referring to baseline case, not comparing to scenarios.	The wording was slightly adjusted to clarify the intended point about the relative role of spring tides vs neap tides.	Y (Section 6)	Agree, please consider making the first sentence of "The model results.." clearer about what comparison is being made when indicating decrease and increase	Yes, now reworded as "The filtered time-series model results for all discharge scenarios indicate that during spring tides the total salinity in the estuary system trends downward and during neap tides the total salinity trends towards a relative increase in salinity concentration"

39	Flagging again here comments above about peaks being 10+ psu higher but short lived and whether this is important.	Additional wording added to the last paragraph of conclusions	Y (Section 6)	AS above, check typos in sentences. Is it above (not around) 10 ppt at times?	Agree that the wording should be tightened. We think 10ppt is appropriate to characterise the entire 3-4 days, but agree that there is a shorter period within when the peak is higher, so sentence is modified to "During neap tides the time averaged contribution of the discharge can temporarily be around 10 ppt for a period of 3 to 4 days and in the middle of this period salinity may briefly peak at around 12 ppt in the worst case (80 ppt discharge case at FR2 station). "