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Subject	Final Technical Review Sign off	Project Name	Christchurch City Council - Coastal Hazards Assessment
Attention	Maiki Andersen	Project No.	IS346200
From	Derek Todd		
Date	September 13, 2021		
Copies to	Jane Morgan		

I was appointed in May 2020 by Christchurch City Council as the External Technical Reviewer of the Christchurch Coastal Hazards Assessment undertaken by Tonkin + Taylor Ltd on behalf of the council. This has been a "rolling review" role, in which I have been party to all the technical discussions on methodology of the assessment and reviewed all memo's and versions of technical reports associated with the assessment since this time.

As a result, I can confirm that appropriate and relevant past literature and information on coastal processes influencing hazards on the Christchurch District coast have been consulted to inform the assessment. As technical issues have arisen, I have been involved in discussions to amend the methodology and I have also been involved in suggesting edits to drafts of technical reports and memos.

As a result of this "rolling technical review" process, it is my professional opinion that the methodology applied in this assessment and results reported in the technical report are fit for the primary purpose of the assessment, being to provide CCC with:

- Updated coastal hazard information to help inform coastal hazards adaptation planning for the Christchurch District.
- The methodology and results are presented in a format that is easily accessible, comprehensive and unambiguous.
- Provided the uncertainties and limitations are understood and appropriately managed, inform a range of other purposes such as coastal hazards provisions in the Christchurch District Plan, infrastructure planning decisions, consenting applications and Civil Defence Emergency Management.



Derek Todd, M.SC (Hons)

Jacobs Principal Coastal and Hazard Scientist

ID	Comment	Comment Date	Doc Version	Section No.	Reviewer	T+T Response	Status Apr-2021	Reviewer Discussion Notes on July 2021 Technical Report version	Status Jul-2021	Additional discussion 2-Aug-2021	Status 2-Aug-2021	Final Reviewer Check Comments 11-Aug-2021	Status 11-Aug-2021	TT Comments 6-Sep-2021	Status 6-Sep-2021	Reviewers Comment -10-Sept 2021	Status 10-Sep-2021
1	My technical questions/feedback are related to the way ARIs are determined for erosion distances along the coast. Reading the draft report I was triggered by the text in 3.1.2.2 about the data selection and extrapolation method used for the data. You will be well aware that the extrapolation has a significant effect on outcomes of the assessment and following adaptation process. My questions are: •Do you use all available profiles in a certain coastal cell to determine the erosion distance distribution?	10-May-21	01-Apr-21	3.1.2.2	RIL	All profiles are considered in the extreme value analysis	Propose closed				Closed		Closed		Closed		Closed
	• If so, how do you acknowledge the physical differences between the profiles when extrapolating the extreme values? Given that the range in erosion distance varies a lot between profiles.	10-May-21	01-Apr-21	3.1.2.2	RIL						Closed		Closed		Closed		Closed
	•Do you believe that the statistical extrapolation also represents the physical extrapolation? As an example, Figure 3.7 shows an acceleration in erosion distance for larger ARIs, where as in real life the acceleration distance might actually decrease as a result of the dune profile itself.	10-May-21	01-Apr-21	3.1.2.2	RIL						Closed		Closed		Closed		Closed
	•Do you have examples of the comparisons T&T made in 2017 (or even now) between the different selection methods (AM vs PoT) and distributions (GEV vs others)?	10-May-21	01-Apr-21	3.1.2.2	RIL	Included in technical memos from T+T (2017)	Propose closed				Closed		Closed		Closed		Closed
2	Figure 3.8 uses two different parameters to describe the location of the regression analysis. It could be helpful to refer to table 3.1 so that readers can look up which profile is at a certain chainage.	10-May-21	01-Apr-21	3.1.4.1	RIL	Have included reference to table 3.1 with chainage	Propose closed				Closed		Closed		Closed		Closed
3	I haven't found an explanation why data before 1970s is ignored (except for Brooklands) in determining regression plots?	10-May-21	01-Apr-21	3.1.4.1	RIL	Excluded for consistency in the regression rates along the length of coastline	Propose closed				Closed		Closed		Closed		Closed
4	At 3.1.4.3 I think it is good to mention that the NIWA report, on which this paragraph is based, concludes that a 9% increase could be the most likely case. I fully support sticking to the 0% change scenario for the assessment, but I think we should acknowledge that in terms of likelihood the positive sediment supply scenario is looking positive for the open coast.	10-May-21	01-Apr-21	3.1.4.3	RIL	Have updated text	Propose closed				Closed		Closed		Closed		Closed
5	Cliffs - Toe erosion: Following discussion at Progress meeting 17, are you still using the factor for future sea level rise impacts?	10-May-21	01-Apr-21	2.1.3	DT	Not for the regional cliffs but there are some banks where the factor for future SLR is used. Have re-worded the SLR section to make it clear how the factor is derived	Propose closed	Confirm that methodology adjusted to remove future SLR factor from regional cliff erosion assessment, but is still in Banks assessment (rightly so) without discussion on what are the factors or where from.	Open - future update	Have updated the equation so that it matches the discussion in Section 4.5.4.2	Propose closed	Updated equation is appropriate	Agree closed		Closed		Closed
6	If still using this, there is a need for an explanation of how this factor calculated. If not, then need to change equation to describe stable slope angle approach.	10-May-21	01-Apr-21	2.1.3	DT	Have added explanation under Bank SLR section	Propose closed	As above could not see the explanation under banks section	Open - future update	Have updated the equation so that it matches the discussion in Section 4.5.4.2	Propose closed	Updated equation is appropriate	Agree closed		Closed		Closed
7	I thought all cliffs were in the regional screening areas, therefore above discussion is not required.	10-May-21	01-Apr-21	2.1.3	DT	Have removed	Propose closed	Accepted	Closed		Closed		Closed		Closed		Closed
8	Compared to assumed stable slopes	10-May-21	01-Apr-21	2.1.3	DT	Have updated text	Propose closed	From figure 3.4 this slope is assumed to be 1:1, but this is not stated in text. Also need to include the methodology if the slope is less than this as per section 4.6.5	Open - future update	Have updated text	Propose closed	Updated text is appropriate	Agree closed		Closed		Closed
9	Significantly modified shorelines: In hindsight, I think that Sumner should also be included in this list	10-May-21	01-Apr-21	2.1.5	DT	Have updated Sumner to Class 1	Propose closed	Accepted	Closed		Closed		Closed		Closed		Closed
10	I think it would be worth including a bit of context around the magnitude and uncertainty in VLM estimates for chch, as per section 5.3.3 of the scoping report.	10-May-21	01-Apr-21	2.5	DT	Have added text around VLM in this section	Propose closed	Accepted	Closed		Closed		Closed		Closed		Closed
11	From the distances presented I assume that this is the mean and max for the erosional inter-survey changes only - which is what it should be. However, note that this is different from the data presented in Figure 3.6.	10-May-21	01-Apr-21	3.1.2.1	DT	Yes its slightly different data	Propose closed	accepted - text now clearer	Closed		Closed		Closed		Closed		Closed
12	This figure shows both erosion and accretion phases of dune toe movements, so should not be called just "storm cut".	10-May-21	01-Apr-21	3.1.2.1	DT	Have reworded	Propose closed	accepted - text now clearer	Closed		Closed		Closed		Closed		Closed
13	I have an issue with calling all of the data presented in the matrix "storm cut" as includes both erosion and accretion phases of dune toe position. Would be more accurate to call matrix inter-survey changes in dune toe position.	10-May-21	01-Apr-21	3.1.2.1 Fig 3.6	DT	Have reworded	Propose closed	accepted - text now clearer	Closed		Closed		Closed		Closed		Closed
14	While this is true, how does the analysis handle multiple erosion phases with the same year due to surveys within storms in series (e.g 1992)?	10-May-21	01-Apr-21	3.1.2.2	DT	It will still capture the maximum cut within the year and account for the cumulative impact of back to back storms	Propose closed	If this is case, the text needs to be rewritten to state that the AM method includes the cumulative effects of storms in series across multiple surveys within the series. See my new comments in report	Open - future update	As discussed and updated text in report	Propose closed	Text additional that the max inter-survey erosion in any one year is used, rather than cumulative across storms in series. So resulting erosion may be less than actually possible on an annual basis.	Agree closed		Closed		Closed
15	I don't follow this argument as small day to day fluctuations are not in the profile survey record.	10-May-21	01-Apr-21	3.1.2.2	DT	Have reworded - As there is limited profile data for the spits (i.e. one to two profiles), the AM method is not suitable as this would be skewed by the small fluctuations.	Propose closed	The limited number of profiles is not the issue, it is the higher likelihood of including the small fluctuations natural fluctuation in spit position in the erosion distribution, therefore skewing the mean value to a lower value than just storm response. See my new comments on report.	Open - future update	As discussed and updated text in report	Propose closed	While the intent of the additional text is right, I would not describe the skewing of results to "small storm cuts", it is more about being skewed by "small normal fluctuations in beach position that occur at the distant end of a spit.	Still some re-wording to address	Updated text in report -Section 4.1.2.2 "Due to the limited data points within these cells, the AM method is less appropriate as the resulting extreme value curve becomes skewed to the small, normal fluctuations that occur in beach position at the distal end of the spit and results in unrealistic storm cut values"	Propose closed	Wording now appropriate. Agree comment can be closed	Closed
16	Does this not create an inconsistency in approach? A sensitive assessment should be carried out to show the differences in the two approaches, and whether these are significant or not.	10-May-21	01-Apr-21	3.1.2.2	DT	AM method was agreed with previous peer review and however there is not enough data to apply AM on the spits. Overall values look sensible but a sensitivity assessment can be completed	Open - future update	There is the same amount of data for the spit profiles as the open coast, with the issue being the greater non-storm fluctuations as above and only two profiles to average the movements over. See my new comments in the report. Has the sensitivity assessment between the methods been undertaken to confirm the significant of the different approaches? I would still like to see this	Open - future update	Have reworded - As there is limited profile data for the spits (i.e. one to two profiles), the AM method is not suitable as this would be skewed by the small fluctuations. Sensitivity analysis has been completed which shows unrealistic ST for spit using the AM method (see table on right)	Propose closed	While agree with use of PoT approach at this site, See above comments re number of profiles. Adjacent table confirms PoT is more acceptable distribution than AM distribution.	Agree closed		Closed		Closed
17	I am unclear whether these values form the bounds and mode value of the distribution for the probability analysis or not. If they do, what is the rationale on choosing these ARI intervals? I can't find where this is covered in the scoping report.	10-May-21	01-Apr-21	3.2.2.2 Table 3.2	DT	The ARI intervals are just included for context - they are not used as parameter bounds. Could remove them from the table and just add a description in the text	Open - future update	Text still needs some work to make clearer what you have used for the parameter bounds. Is it +- the shape parameter? See my new comment in report	Open - future update	The extreme value distribution has been adopted (not triangular distribution with parameter bounds). Text updated.	Propose closed	Text update accepted	Agree closed		Closed		Closed
18	Don't find this Footnote particularly clear on what the shape parameter is and how it influences the result	10-May-21	01-Apr-21	3.2.2.2 Table 3.2	DT	Have updated table note - "Shape parameter describes the shape of the distribution (e.g. a larger shape parameter results in a wider distribution)"	Propose closed	Better - but as above what are the bounds for the distributions?	Open - future update	As above the extreme value distribution is adopted	Propose closed	As above, accepted	Agree closed		Closed		Closed
19	My understanding of why factor 2 in equation is from "slope replacement theory of cohesiveness sand" (Clark & Small, 1982), in which cross-section area of deposited sand as talus (DT Recovery) = cross section area of eroded sand from dune top (ST Retreat), and since scarp face after stability adjustment is perpendicular to pre-storm face: h/(DTR*STR) = tan α, therefore STR = h/2tan α.	10-May-21	01-Apr-21	3.1.3	DT	Havent been able to find Clark & Small 1982 to reference	Propose closed	See figure beside	Open - future update	Thanks for the figure. Will leave as is to avoid any confusion with further equations and terms	Propose closed		Agree closed		Closed		Closed

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20	What appears to be missing is the testing of how strong the regression is (e.g. sensitivity of the R2 values), therefore how representative are the regression trends.	10-May-21	01-Apr-21	3.1.4.1	DT	Have added 95% CI to the plot	Propose closed	confirmed. Could add sentence about longshore trends in uncertainty based on the 95% CI results.	Open - future update	Added text 95% CI (uncertainty) largest near the spits and smallest near New Brighton)	Propose closed	Additional text is appropriate.	Agree closed		Closed		Closed
21	I would suspect that this erosion was as a result of the 1992 storm therefore not strictly correct to say erosion from 1974 to 1994. This could be checked from the profile record.	10-May-21	01-Apr-21	3.1.4.1	DT	Correct have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
22	Assume you are only referring to cells 10-13	10-May-21	01-Apr-21	3.1.4.1	DT	Have updated text -	Propose closed	confirmed - but should only apply to the earthquake effect not the potential dune planting/management effect, which should be whole coast.	Open - future update	Have removed reference to dune planting	Propose closed	Removal appropriate.	Agree closed		Closed		Closed
23	This reads like there has only been accretion since 2011, which is not correct. Would be better to say "hence the period of increased accretion following 2011".	10-May-21	01-Apr-21	3.1.4.1	DT	Have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
24	Are you referring to the increased accretion only in cells 10-13? As the following sentence infers that it is across all cells 6 to 13 since enhanced measures since 1990, for which no evidence is presented.	10-May-21	01-Apr-21	3.1.4.1	DT	Yes, have updated text -	Propose closed	As above in comment 22 - what evidence that dune planting/management only in cells 10-13? Should be the whole coast, and a lot of change in Cell 6. See my new comment in report	Open - future update	Have removed reference to dune planting	Propose closed	Removal appropriate.	Agree closed		Closed		Closed
25	Maybe better to say that "historic shorelines mapped from aerial photographs". This will reduce confusion with following statement of erosion 1940 to 1949.	10-May-21	01-Apr-21	3.1.4.1	DT	Have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
26	I don't follow this sentence. Is the upper/lower bounds a confidence interval of the mean (e.g. ± 95 percentile), or the upper/lower from the DSAS transects within the cell?	10-May-21	01-Apr-21	3.1.4.2	DT	Have updated text	Propose closed	While accept have added the sentence on why the 95th percentile is not used, still need to state the basis of the selection of the upper/lower bounds. See my new comment in report.	Open - future update	Have updated text - "The parameter bounds have been rationalised based on the variation in the mean regression rate within each cell. For example, the upper bound is based on the maximum mean regression trend within each cell and the lower bound is based on the minimum mean regression trend within each cell."	Propose closed	New text is appropriate	Agree closed		Closed		Closed
27	I am not sure why this statement is included as implies that this scenario is overly optimistic. The 11% reduction in supply could similarly be termed a pessimistic scenario. I suggest that the statement be re-moved.	10-May-21	01-Apr-21	3.1.4.3	DT	This will be replaced with commentary on more likely long term trends as per Item 4 above	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
28	I can't find Eqtn 6 from Hicks (20187b). For completeness this Eqtn should be included as a footnote so readers can follow the adjustments	10-May-21	01-Apr-21	3.1.4.3	DT	Have added equation as footnote	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
29	Need to check this value. Why is it less than current erosion if there is a reduction in sedi supply	10-May-21	01-Apr-21	Table 3.6	DT	Have updated value	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
30	Bathymetric contour data?	10-May-21	01-Apr-21	3.1.5	DT	Correct, have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
31	I have an issue with this comparison, as the storm profile drops at the seawall will be much greater than at the dunes due to energy reflection and lack of erosion input to the profile at the seawall. As a result the 2m drop shows a much larger erosion (40m) than obtained from the dune toe survey results at the dune site, hence is the erosion results will be over conservative.	10-May-21	01-Apr-21	3.2.2	DT	updated Sumner to Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
32	I wouldn't refer to these as 'nearshore', to me they are still past of the foreshore - covered and uncovered by tides.	10-May-21	01-Apr-21	3.2.2 Fig 3.18	DT	updated Sumner to Class 1 structure. Will also include glossary	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
33	Figure only shows one distribution, which from caption assume is for CC0112 in cell 29, so is the interpreted cut in absence of the revetment (bearing in mind over conservative approach as per above comment).	10-May-21	01-Apr-21	3.2.2 Fig 3.18	DT	Have updated section/figures with Sumner now as Class 1 structure	Propose closed	Figure removed- accepted	Closed		Closed		Closed		Closed		Closed
34	This appears to be inconsistent approach to cell 29, which also has revetment presence for all of profile record.	10-May-21	01-Apr-21	3.2.2 Fig 3.18	DT	updated Sumner to Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
35	It appears from this results that you have assumed cell 28 results for cell 27 due to presence of revetment as you note above. If this is the case, you need to state this.	10-May-21	01-Apr-21	3.2.2 Table 3.10	DT	Cell 27 is now a Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
36	Assume that these should be negative numbers	10-May-21	01-Apr-21	3.2.2 Table 3.10	DT	Correct, have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
37	As above, the results for this cell appear to be conservatively large due to the methodology	10-May-21	01-Apr-21	3.2.2 Table 3.10	DT	updated Sumner to Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
38	How were dune heights assumed for the seawall cells? Did these represent the variations in height of the revetments?	10-May-21	01-Apr-21	3.2.3	DT	Height of structure is used for the Class 1 structures	Propose closed	accepted but still need to make text clearer. See my new comment in report	Open - future update	Text updated - "Parameter bounds are defined based on the variation in dune/structure height within the coastal cell and potential range in stable angle of repose (Table 4.11 and Table 4.12). The stable angle of repose for Cell 28 is based on the angle of repose for dune sand, while the stable angle of repose within Cells 27 and 29 is based on an assumed angle of repose for fill material behind the structure."	Closed	New text is appropriate	Agree closed		Closed		Closed
39	This is likely to be also linked to the construction of the current Sumner Bay revetment, dated as being between the 1940's and 1950's	10-May-21	01-Apr-21	3.2.4	DT	Updated text	Propose closed	text removed - accepted	Closed		Closed		Closed		Closed		Closed
40	Nearshore or foreshore?	10-May-21	01-Apr-21	3.2.4	DT	updated Sumner to Class 1 structure. Will also include glossary	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
41	Accuracy of the relationship will be dependant on whether the slopes of the two profiles are similar. My feeling is that those at the Sumner seawall will be flatter than those at southshore.	10-May-21	01-Apr-21	3.2.4	DT	updated Sumner to Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
42	Are these actually required for the erosion assessment, as I would have thought that the Sumner seawall was a Class 1 structure, therefore as per the methodology the long-term erosion is set to the short-term on the grounds that the seawall will be replaced if fails.	10-May-21	01-Apr-21	3.2.4	DT	updated Sumner to Class 1 structure	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
43	Maybe worth including note that +ve values are accretion and -ve are erosion	10-May-21	01-Apr-21	3.2.4 Table 3.13 & all other regression rate tables in report	DT	Have updated all tables	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
44	As with chch open coast, how/why were these ARI's chosen?	10-May-21	01-Apr-21	3.3.2 Table 3.16	DT	Parameter bounds based on extreme value distribution - ARI are just included for context -	Propose closed	As with Table 4.1.2.2, still need to clarify what you have used for the parameter bounds. Is it - the shape parameter? See my new comment in report	Open - future update	The extreme value distribution has been adopted (not triangular distribution with parameter bounds). Text updated.	Propose closed	New text is appropriate	Agree closed		Closed		Closed
45	As per my comments on Scoping report, since method involves equilibrium profiles, the resulting erosion distances will be conservatively large due not being restricted by storm event duration	10-May-21	01-Apr-21	3.4.2	DT	Have added text to explain that it is conservative	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
46	how does this compare to the dune heights given in Table 3.24 for dune stability? There appears to be an inconsistency in approach.	10-May-21	01-Apr-21	3.4.2	DT	Dune heights are relative to the dune toe but for the Kribel and Dean method the dune height input is relative to NZVD16 as it needs to be related to the input WL	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
47	How these this compare to slopes obtained from LiDAR for these unconsolidated shorelines	10-May-21	01-Apr-21	3.4.2	DT	Have updated text "Based on the LiDAR a representative profile with an assumed berm elevation of 1.5m..."	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
48	Do these have the same ARI as the storm tides?	10-May-21	01-Apr-21	3.4.2	DT	Yes	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
49	Have not seen SWAN model outputs (Appendix ***)	10-May-21	01-Apr-21	3.4.2	DT	Appendix B added	Propose closed	accepted - but is appendix B	Closed		Closed		Closed		Closed		Closed

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50	Including post-quake	10-May-21	01-Apr-21	3.4.3.1	DT	Have updated text	accepted		Closed		Closed		Closed		Closed		Closed
51	Should be noted that long-term rates for AH2 to AH5 are likely to be influenced by shoreline protection works.	10-May-21	01-Apr-21	3.4.3.1	DT	Have added table note	accepted		Closed		Closed		Closed		Closed		Closed
52	This sentence needs to be put into context, as currently reads as in referring to the slopes in the three bays rather than the rest of the inlet.	10-May-21	01-Apr-21	3.5	DT	Have reworded	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
53	Again where has this assumed slope come from and how does it fit actual slopes from LIDAR?	10-May-21	01-Apr-21	3.5.1	DT	Have updated text - "Based on LIDAR, a range of different berm elevations with an upper slope of ..."	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
54	There should also be a comment that hard rock banks have been assessed as zero short term erosion and justification of why.	10-May-21	01-Apr-21	3.5.1	DT	Have added text - For the consolidated banks the short term component is not applicable as the banks behave differently to the unconsolidated beaches (see Section 2.1).	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
55	Assume that these are located on un-protected shorelines?	10-May-21	01-Apr-21	3.5.2	DT	Yes, have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
56	Need to state that these have been adopted based on the unprotected shorelines. This assumption does not appear to be stated.	10-May-21	01-Apr-21	3.5.2	DT	Have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
57	Need to explain the subscripts	10-May-21	01-Apr-21	3.5.4.2	DT	Have re-written equation to clarify and have included subscripts	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
58	Are these sentences and following diagram required given that there is no subsidence in the consideration of LT rates on the Peninsula?	10-May-21	01-Apr-21	3.5.4.2	DT	Probably not required - have removed	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
59	What in this makes them also suitable for Banks Peninsula?	10-May-21	01-Apr-21	3.5.4.2	DT	Have added text - This is in line with what was used by T+T (2019) for the embankments within Tauranga Harbour which are likely to have similar erosion susceptibility as the harbour banks within Lyttelton and Akaroa Harbours	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
60	It is unclear where this mode slope for banks is from as the text refers to 1:2 (26.6 deg) and 1:3 (18 degs) slopes.	10-May-21	01-Apr-21	3.5.6 Table 3.26	DT	Good spot - the slopes have been updated to range from 18 to 26.6 degrees for harbour banks	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
61	Same as comment above. Where is this slope from?	10-May-21	01-Apr-21	3.5.6 Table 3.26	DT	Good spot - the slopes have been updated to range from 18 to 26.6 degrees for harbour banks	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
62	It is confusing which of these values are closure slopes and which are SLR factors. Either use footnotes to identify, or split row into two	10-May-21	01-Apr-21	3.5.6 Table 3.26	DT	Have added a footnote - "1 Closure slope applicable for the Harbour beach morphology and SLR factor applicable for the bank morphology"	Propose closed	Is better, but still not totally clear where each of these have been applied. May be use two subscripts 1) closure slopes, 2) SLR factors, and include on the results as well at row heading.	Open - future update	Have updated as recommended	Propose closed	Subscripts appropriate and now much clearer	Agree closed		Closed		Closed
63	Assume that these have been assessed as having storm cuts between sheltered and exposed?	10-May-21	01-Apr-21	3.6.1	DT	Have added text - It is assumed that storm cut along the moderately exposed beaches is between the sheltered and exposed storm cut distances.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
64	Are you inferring that the southland current is responsible for the northward transport around the Peninsula?	10-May-21	01-Apr-21	3.6.3	DT	Have reworded	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
65	Need to state basis for this assumption - e.g pocket beach with similar orientation.	10-May-21	01-Apr-21	3.6.4.1	DT	Have updated text - "Subsequently, an assumed the closure slopes have been assumed the same as Taylors Mistake (0.02) which is a pocket beach with similar exposure to the Banks Peninsula beaches."	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
66	See previous comment about on why this is appropriate. Is it the same sediment type?	10-May-21	01-Apr-21	3.6.4.2	DT	Have added text - This is in line with what was used by T+T (2019) for the embankments within Tauranga Harbour which are likely to have similar erosion susceptibility as the harbour banks within Lyttelton and Akaroa Harbours	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
67	Would be useful to include the longshore chainages on figure to link to Table 3.30	10-May-21	01-Apr-21	3.7.1 Fig 3.50	DT	Updated figure	Propose closed	accepted - cell numbers added rather than longshore chainage	Closed		Closed		Closed		Closed		Closed
68	Is this elevation the RL of the berm height? If so it appears to be very low. Please confirm its source	10-May-21	01-Apr-21	3.7.5 Table 3.30	DT	No it is the elevation above the 6m RL contour - have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
69	Similarly this berm elevation is also extremely low. Please confirm its source.	10-May-21	01-Apr-21	3.7.5 Table 3.30	DT	No it is the elevation above the 6m RL contour - have updated text	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
70	Maybe also include reference to GHD(2021) here, as rely on it later for water levels in the estuary.	11-May-21	01-Apr-21	5.1	DT	Added	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
71	Check reference	11-May-21	01-Apr-21	5.3	DT	fixed	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
72	a bit vague on what is meant here. I think you are saying that the variability in run-up elevations that could occur would result in an unreasonably large number of inundation areas required to be mapped. However, despite this, are areas potentially subject to additional inundation from run-up identified in any way rather than a blanket non inclusion?	11-May-21	01-Apr-21	5.4	DT	Updated the sentence and referred to section 6 in which run-up attenuation for low crest levels is addressed	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
73	Noted that water levels and waves still TBC	11-May-21	01-Apr-21	6.1.1	DT	Added now	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
74	I am not sure it is this an issue for CCC or not, but earlier levels were given in terms of NZVD2016, creating potential confusion for readers.	11-May-21	01-Apr-21	6.1.2.1	DT	updated in report	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
75	This is confirmed by sum of the residuals in Figure 6.1? Or have you taken into account the likely under/over prediction of the XBeach model based on the run-up comparisons?	11-May-21	01-Apr-21	6.1.2.1	DT	Yes, smallest sum of residuals (added in report)	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
76	Is this the same as the beach face or surf zone slope mentioned in other locations?	11-May-21	01-Apr-21	6.1.2.1	DT	Clarify in glossary	Propose closed	These terms not included in glossary, so same question - are foreshore/beach slope the same as beach face slope and surf face slope used in other places in report.	Open - future update	same technical meaning, different value depending on location. Updated text and added to glossary.	Propose closed	Noted terms are in glossary	Agree closed		Closed		Closed
77	I think you mean wave set -up, and therefore reference should be to surf zone slope	11-May-21	01-Apr-21	6.1.2.1	DT	updated	Propose closed	actually superseded	Closed		Closed		Closed		Closed		Closed
78	Just to confirm, we have all this discussion, but presentation of mapping of results?	11-May-21	01-Apr-21	6.1.3	DT	Mapping for run-up is not done, but levels, effects of erosion and inland attenuation distances are provided for information	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
79	For completeness and consistency, the XBeach model and USACE (2006) run-up formula should be included in the sensitivity as well? Otherwise, why are they good enough for consideration of set-up and not run-up?	11-May-21	01-Apr-21	6.1.3	DT	The Xbeach model is included (see figure 6.4 of previous version). Mase and Hedges&Mase are included in USACE (2006)	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
80	I assume that this is based on the sum of the residuals in figure 6.4	11-May-21	01-Apr-21	6.1.3	DT	Yes, added note	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
81	Is it different formula from T&T (2017) chch assessment?	11-May-21	01-Apr-21	6.1.3	DT	Not considered	Propose closed	Still needs resolved as only T&T (2017) in reference list is previous chch assessment, not Northland/	Open - future update	Yes, for Northland, but has now been updated so will be 2021. Text removed.	Propose closed	Resolved with removal of text	Agree closed		Closed		Closed
82	Why not just use XBeach run-ups, or does Mase (1989) provide a better result?	11-May-21	01-Apr-21	6.1.3.2	DT	In order to calculate wave run-up every time step. That's not possible with XBeach.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
83	I think this should be wave run-up, with reference being to beach face slope. A glossary would be useful to explain a number of these terms used in the text.	11-May-21	01-Apr-21	6.1.3.2	DT	updated	Propose closed	actually superseded	Closed		Closed		Closed		Closed		Closed
84	Should move definition of these terms to here.	11-May-21	01-Apr-21	6.1.5	DT	updated	Propose closed	Definitions were to be provided in glossary, but have not done so.	Open - future update	Added to glossary.	Propose closed	Noted terms are in glossary	Agree closed		Closed		Closed
85	I would think that surveyed slopes would vary considerably over this elevation range, so not sure how these have been averaged in Fig 6.10. However, during storm events (extreme water levels) the slopes across the foreshore would be more uniform, so have these been an attempt to determine these storm slopes?	11-May-21	01-Apr-21	6.1.5	DT	Clarified averaging profiles method in report. We have not attempted to come up with profile for during storm events as this may have an equal amount of limitations as using an averaged profile.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
86	I don't follow this sentence. The beach profile data is unlikely to extend much below the om or -1 m contour, so is a limitation of determining this surf zone slope, which may explain the variation shown in Fig 6.10	11-May-21	01-Apr-21	6.1.5	DT	updated and clarified in report	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
87	Reference to be checked	11-May-21	01-Apr-21	6.3	DT	updated	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed

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88	I would have thought that the bathymetry in the harbours is no may limited than at the outer Peninsula and Kaitorete barrier, where these slopes have been calculated (but with limitations outlined on comment below, so maybe they shouldn't be). There is also detailed bathymetry of the A-H estuary. So the use of Guza and Thompson appears for these areas appears to be an inconsistency (if we accept regional screening methodology for set-up)	11-May-21	01-Apr-21	6.3.2	DT	We haven't been able to derive slopes accurately from within the harbours due to limited bathymetry data (A-H does have more detailed data, but we've used the same approach for consistency). There is more detailed data along the open coast in the form of 0, -2m, -5m and -10m contours. Within the harbours there's typically only 1 or 2 contours which makes it difficult to accurately define slopes.	Propose closed	Don't accept this, as in section 7.2.1 there is mention of the -2m contour in Lyttelton and Akaroa harbours, and -1m contour in A-H estuary. Surely this contours could have been used for surf zone slope and would be not more limiting that the use of the beach profiles on the open coast. Beach slope could be obtained from LIDAR.	Open - future update	As discussed, T+T consider that this can not be done accurately enough to make it worthwhile.	Propose closed	Softening of wording to "challenging" is accepted	Agree closed		Closed		Closed
89	From this I assume that there is a single output location in each of the harbours. So, this assumes that set-up, and therefore wave climate is the same in all bays of each harbour. I can't accept this assumption.	11-May-21	01-Apr-21	6.3.3	DT	We have included wave height ranges in the report, with adopted set-up for each harbour. As discussed during the meeting, for the purpose of the map viewer it is clearer to have a single output location. We have included this rationale in the report.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
90	Reference to be checked	11-May-21	01-Apr-21	6.4	DT	updated	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
91	'at' or 'in'? I think you mean in so that you can assess potential inundation on the lake shores rather than the open coast.	11-May-21	01-Apr-21	6.4.1.1	DT	updated	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
92	Following on from assumption that water level records of interest are those inside the lakes, I assume that the extreme levels being assessed are those for when the lake mouths are open.	11-May-21	01-Apr-21	6.4.1.1	DT	A timeseries was used to assess extreme levels. The timeseries implicitly include times when lake was open and closed. The lake is typically closed.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
93	To what depths as assume that these contours limited to 10 m depth – so this is inconsistent slope calculation to those on the detailed open coast. Also should include sensitivity of set up results under USACE and Guza & Thompson formula to confirm that there are not major differences in results.	11-May-21	01-Apr-21	6.4.2	DT	This has been derived based on the 0m, -2m and -5m depth contours and LIDAR data. USACE has been used consistent with open coast.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
94	where in the lake – assume location of greatest wave fetch/storm tide influence	11-May-21	01-Apr-21	6.4.3	DT	Single output for lake	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
95	again where in lake?	11-May-21	01-Apr-21		DT	Single output for lake	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
96	The NIWA 2015 report did not analyse Summer sea level data, only Lyttelton.	16-Jun-21	01-Apr-21	5.1	JC	Clarified in report	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
97	This is the first time the bathtub approach is mentioned and is under a heading of mapping which doesn't seem to fit the context	2-Jun-21	28-May-21	5.4	MA	Updated heading to 'mapping methodology'	Propose closed	accepted - but the introduction to bathtub approach may be better placed in S6.1 - conceptual approach	Closed		Closed		Closed		Closed		Closed
98	Possible Alternative Wording: Sensitivity analysis was undertaken between the hydrodynamic modelling results and the bathtub modelling results to test the suitability of the bathtub approach.	11-May-21	01-Apr-21	5.4	DT	updated wording	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
99	Need to include that this suitability was limited to one in spatial extent to immediate coastal hinterland, and not the full extent of catchments subject to tidal water level variations as explained in more detail below – suggest moving that paragraph to here	11-May-21	01-Apr-21	5.4	DT	Moved paragraph as per suggestion	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
100	Also could benefit from further explanation why it is suitable.	2-Jun-21	28-May-21	5.4	MA	Refer to Appendix B	Propose closed	reference to web viewer appears to have been removed from text	Open - future update	Added into scope section at start of report	Propose closed	Accepted		Closed		Closed	
101	form on web viewer with	11-May-21	01-Apr-21	5.4	DT	updated	Propose closed	Line on map included but not inland modelling - unless is in appendix C - which I assume is the technical memo?	Open - future update	TVHydrodynamic modelling memo included as Appx C	Propose closed	Accepted		Closed		Closed	
102	Should we be showing a map that shows modelling landward of the inland extent boundary and include the line on the map.	2-Jun-21	28-May-21	5.4	MA	Updated on maps	Propose closed	GHD (2021) touch on independence between surge and tide, so have included that. In general wave height and tide are independent (independent sources), so these are not assumptions.	Closed		Closed		Closed		Closed		Closed
103	How well do these assumptions align with recent joint probability work? i.e. is this consistent with findings meaning that using the storm tide is sufficiently representative? Keen for the rationale to be clearly stated	2-Jun-21	28-May-21	5.4	MA		Propose closed	Explained that there are no further data points available.	Closed		Closed		Closed		Closed		Closed
104	It is probably my ignorance but this rationale doesn't really explain to me why only two have been used and how they are representative?	2-Jun-21	28-May-21	6.1.2.1	MA		Propose closed	Added explanation why scenarios considered	Propose closed		Propose closed		Closed		Closed		Closed
105	What is the rationale for using these two scenarios?	2-Jun-21	28-May-21	6.1.4	MA		Propose closed	Updated text to discuss response of profile with sea level rise	Propose closed		Propose closed		Closed		Closed		Closed
106	Check the description in this para with what is in the table 6.1. I think the debris line levels have been mixed up in the table.	16-Jun-21	01-Apr-21	6.1.2	JC	Updated	Propose closed	This has not been resolved. See my new comments in report	Open - future update	Switched dates in table	Propose closed	Dates corrected in Table		Closed		Closed	
107	What were the thresholds used and why?	16-Jun-21	01-Apr-21	6.1.2.2	JC	This has not been specified in report as this is different for each area. The thresholds have been selected such that only extreme storms are included, with the EVA giving a reasonable fit through the data without the CI's becoming too wide.	Propose closed	Include this explanation as a footnote in report	Open - future update	Have included explanation in report	Propose closed	Text added		Closed		Closed	
108	I think these need to be defined/differentiated/explained	16-Jun-21	01-Apr-21	6.1.5	JC	Include in glossary	Propose closed	Not in glossary	Open - future update	Included in glossary	Propose closed	Noted terms are in glossary		Closed		Closed	
109	Needs to be clearer that these are lake levels not sea levels near the lakes	16-Jun-21	01-Apr-21	6.4.1.1	JC	updated	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
110	The mouth so these lakes are closed most of the time and are barely influenced by the tide. Extreme water levels are mainly affected by catchment inflows and consented opening trigger levels. Is it therefore appropriate to call the EVA a "storm-tide" analysis when the processes and components are very different to coastal extreme water levels?	16-Jun-21	01-Apr-21	6.4.1.1	JC	updated	Propose closed	This has not been updated. Agree with JC comment, and you should not be adding SLR to these lake levels as is the case for open coast sites to get future extreme lake levels. See my comment on report	Open - future update	Removed SLR from the reported and mapped levels for the lakes	Propose closed	Resolved with removal of text around this point		Closed		Closed	
111	This was determined to be applicable on the open coast CHCH beaches due to surveyed debris lines for calibration. You probably don't have that luxury here, but can you give some justification for using USACE (2006) for the rest of the coast rather than any other formula? Or is it just for consistency?	16-Jun-21	01-Apr-21	6.4.2	JC	Used for consistency	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
112	I don't know much about the instrumentation on the lakes but if they are just water level recorders rather than tide gauges will they just record total water level, which includes wave setup? If so will wave setup be included in the EVA for the lake data? And therefore does it need to be added additionally?	16-Jun-21	01-Apr-21	6.4.2	JC	We have adopted 0.1m to account for any wave effects that may not have been picked up by the tide gauge. Tide gauges are typically situated in sheltered locations such that it does not measure effects of breaking waves.	Propose closed	accepted	Closed		Closed		Closed		Closed		Closed
113	I assume that this is the standard scientific definition of MHWs, which is commonly exceeded by 12% of the high tides, however for Canterbury due to moon shadow effect is exceeded 37% of time. For this reason it is common for Canterbury to refer to MHWs (Pragmatic) which is level exceeded 12% of the time, or MHWs (ECan) being M2+ N2 tidal constituents (exceeded approx. 10% of time)	23-Aug-21	Aug-21	2.4.1	DT		Propose closed					Text added is appropriate but need definition of MHWPS added to glossary (is it pragmatic or perigee?)	Open - future update	Perigean, added to glossary.	Propose closed	Insertion in glossary appropriate. Agree comment now closed	Closed
114	Should there be short comment on IPCC (2021) assessment?	23-Aug-21	Aug-21	2.4.3	DT		Propose closed					Text added		Text added	Propose closed	Texted added appropriate. Agree comment now closed	Closed
115	Is it each year or each inter-survey period? I suspect that it is (and should be the second, except where there are multiple erosion in the same year due to multiple storms (e.g 1992), as per my previous comment, in which case the cumulative erosion over the erosion phase should be used in the distribution.	22-Jul-21	28-May-21	4.1.2.2	DT	text added to clarify	Propose closed					Text additional that the max inter-survey erosion in any one year is used, rather than cumulative across storms in series. So resulting erosion may be less than actually possible in a storm-in-series occurrence	Open - future update	Text updated	Propose closed	Updated text accepted. Agree that comment now closed	Closed

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116		23-Aug-21	Aug-21	5	DT							Sorry for not picking up on this before, but I can not find reference to where MFE(2017) refers to 5% probability as being "highly unlikely" From Appendix F of MFE(2017) Table F-3: Quantitative terminology for likelihood: 10% is "very unlikely" and 1% is exceptionally unlikely, but no terminology is given for 5%. Can you please confirm the rationale for using 5% instead of 10% as a creditable and accepted indicator of "highly unlikely", other wise for consistency with mFE (2017), I would suggest changing reporting to 10% "very unlikely"	Open - future update	Text updated to explain P5% is middle of the 0-10% range	Propose closed	I can accept the amended text setting out the basis for using a 5% "very unlikely" position, as the middle of the range. However, I note that it is still an inconsistent approach compared to 66% being the limit of the "likely range", but agree that comment can now be closed	Closed
117		23-Aug-21	Aug-21	5.1	DT							What is low and high? From results you should be able to give examples of the SLR under each -	Open - future update	Example SLR values added to text	Propose closed	Added text appropriate. Agree that comment now closed	Closed
118	Would like to see a comment on how reasonable you consider the assumption that extreme water levels (storm tides) will vary by the same magnitude as the variation in MHWS (especially given the difficulties in defining MHWS in Canterbury). This assumption also implies that the influence of storm surge on storm tide is the same in both locations, which also may not a great assumption given they are exposed to different storm weather systems.	23-Jul-21		7.2.1	DT	text added to clarify				Added text generally appropriate , but question whether is it reasonable to assume that storm surge will be similar due to exposed to different weather systems. Is it better to assume that Storm surge in Akaroa Harbour may be up to 0.1 m higher than in Lyttleton Harbour	Open - future update		Changing levels for Akaroa at this late stage would require significant rework to reprocess the GIS data for the printed and online maps. Given that we don't have a definitive more correct value, the other various uncertainties that make up the final level, and the 0.2-0.5m precision of the final depth mapping, making a small 0.1m change does not seem warranted.	Proposed closed	Points around uncertainty and precision are accepted, and agree that no change is justified. Agree that comment now closed	Closed	