CAWTHRON

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Review of Horizons' Upper Manawatu Water Resource Assessment Draft Report

Prepared for

Horizons Regional Council

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Prepared for

Horizons Regional Council

by

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EXECUTIVE SUMMARY

This report reviews aspects of the draft report "Water Allocation Project Upper Manawatu Catchment, Water Resource Assessment – Allocation Limits and Minimum Flows, Technical Report to Support Policy Development" produced by Horizons Regional Council.

This review was commissioned by Horizons Regional Council, and forms part of an external peer review process. It is understood that NIWA were to review the hydrological aspects of the report.

Horizons sought comment from us on the parts of their report that fall within our area of professional expertise (*i.e.* freshwater ecology, freshwater fisheries and flow related instream habitat modelling). We were asked to comment on the methods used and the recommendations made, based on these methods. We were also asked to comment on the overall content of the report, and make some limited editorial comments, as required.

The majority of this review takes the form of notes, referenced to pages in Horizons' report. A number of minor editorial comments and additions were also made in the hard copy of the report provided for review.

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Report reviewed and approved for release by:

Rowan Strickland, Freshwater Group Manager

1. INTRODUCTION

This report reviews aspects of the draft report "Water Allocation Project Upper Manawatu Catchment, Water Resource Assessment – Allocation Limits and Minimum Flows, Technical Report to Support Policy Development" (Roygard *et al.* Draft report). The review was commissioned by Horizons Regional Council (Horizons), and forms part of an external peer review process.

Horizons sought comment from us on the parts of their report that fall within our area of professional expertise (*i.e.* freshwater ecology, freshwater fisheries and flow related instream habitat modelling). We were asked to comment on the methods used and the recommendations made, based on these methods. We were also asked to comment on the overall content of the report, and make some limited editorial comments, as required. It is understood that NIWA were to review the hydrological aspects of the report.

Given our areas of professional expertise, we were most able to make an informed contribution to the discussion of minimum flow setting and water allocation in chapters 4 and 5 of the report. However, we also reviewed several other sections that provided pertinent background information.

It is worth mentioning that we have generally only commented on points that we feel could be improved. However, in general the report seems to lay out a reasonably pragmatic framework for flow management in the upper Manawatu catchment. It is noteworthy that some consideration has been given to the likely effects of water allocation on the frequency and duration of low flows. Setting allocation limits that maintain medium scale flow variability, so that flow is not made to "flat-line" at the minimum level for prolonged periods, is likely to reduce the impacts on instream life, as well as reducing the frequency of restrictions on abstraction.

The rest of this review takes the form of notes, referenced to pages in Horizons' report (*i.e.* Roygard *et al.* Draft report). We have also made a number of minor editorial comments and additions in the hard copy of the report, provided for review.

2. GENERAL COMMENTS

The following comments do not relate directly to any particular section or page in the report.

1. We suggest that the sections reviewed by each reviewer be listed for clarification of what has been reviewed (Note: that the parts of the report that we reviewed are listed as section headings in the notes below).
2. Is it made clear somewhere whether the flow statistics referred to throughout the report are based on naturalized flows, or not? In the executive summary it is implied that flows have been naturalized based on the recent water use telemetry data. However, in the section on naturalizing the flow record (p64) it is stated that naturalized flow records have not been calculated due to a lack of suitable water use records, although this situation may change in the future. But in the discussion of the Tamaki at Stephensons minimum flow (p257), a naturalized MALF estimate is used.
3. It would be interesting to know approximately what the total essential domestic and stock water abstraction might amount to (*i.e.* how far below the minimum flow that flows would possibly be drawn to).
4. An estimate of the cumulative abstraction of permitted takes < 15 m3/s would be useful. If there are a lot of these they could take a reasonable volume and compound the

effects of essential domestic and stock water takes during periods of low flow. A rough estimate of the number of permitted takes might be able to be calculated based on stocking rates in the catchment (assuming that most of these small takes are for agricultural use).

3. CHAPTER 2 THE UPPER MANAWATU CATCHMENT (P21)

3.1 Section 2.7 Water quality indicators and relationships with flow (p129)

(p131) Rather than taking the average percentage of samples classified as satisfactory in terms of water quality and *E. coli* levels, the minimum of these two indicators would arguably better represent the amount of time that the contact recreation criteria were met. Presumably the water is deemed unfit for contact recreation if either water clarity or *E. coli* levels are unsatisfactory. Therefore, the criteria that is satisfactory the least often is the factor limiting contact recreation most of the time (*e.g.* the Manawatu at Hopelands site would only have been deemed satisfactory for contact recreation on 20 % of sampling dates, based on *E. coli* levels (assuming that these samples coincided with satisfactory water clarity levels)

(p132) Although it is true that some of Horizons' water quality data were collected during high flows (*i.e.* floods), it seems unlikely that this accounts for many of the water clarity guideline breaches. On the vast majority of sampling occasions flows were below 1.5 times the median flow (*i.e.* not really in high flood).

(p140) For nutrient levels, perhaps the maximum of the percentage of samples where either nitrate or DRP levels were satisfactory would provide a better indicator of overall compliance with the guidelines, than their average. If either of these nutrients were at satisfactory levels, then presumably it would limit plant growth regardless of the level of the other (since plants need both).

(p140) Seem to have adopted the lowland trigger level for nitrate, although much of the catchment is above 150 m, and would therefore fall in the upland category.

(p145) Same as above but for DRP.

(p153) Should read as: "… based on a relationship between the DO concentration (g/m^3) and water temperature to produce the percentage of DO saturation…"

(p166) It might be better to concentrate on temperature excedence during daylight hours. Trout are visual predators, feeding mainly during the day and evening. Even if temperatures fall below the thermal threshold, where feeding stops, for part of each 24 hour period, if this occurs only at night it may not be of much use, since they may not be able to feed effectively during this time due to low light.

3.2 Section 2.8 Macroinvertebrates (p168)

(p168) Stark (1985 & 1993) not in reference list.

(p169) Joy & Death (2003) not in reference list.

(p169) In paragraph four perhaps the second sentence could read: "A ratio of less than 1 indicates that fewer species than expected were found, suggesting that the site is likely to be impacted to some degree". Presumably an O/E ratio of less than 1 may be due to natural variability (rather than pollution).

(p169) In the second last paragraph it might be worthwhile saying how long these sites have been monitored for.

3.3 Section 2.9 Native fish (p173)

This section was read, but no comments were made.

3.4 Section 2.10 Upper Manawatu River sports fish information (p175)

This section was read, but no comments were made.

4. CHAPTER 3 VALUES IN THE UPPER MANAWATU CATCHMENT (P185)

All sections in this chapter were skim read, but no comments were made.

5. CHAPTER 4 WATER ALLOCATION METHODOLOGY (P 204)

All sections in this chapter were read (see comments below).

(p205) Shouldn't the definition for Group B read "… consented to abstract at flows above the minimum flow…", rather than the "environmental flow"?

(p205) The recommended minimum flows that we provided (Hay & Hayes 2005) were intended to fill the role of what is defined here as the "environmental flow". In the framework suggested perhaps the minimum flow could have been defined as 'the "environmental flow" plus an estimate of the reasonable need for domestic and stock water'. If all except these essential abstractions ceased at a minimum flow defined in this way, then the "environmental flow" should be maintained. In practice this may not make much difference, since the essential abstraction would presumably be quite a small volume.

(p208) We suggest an additional sentence in the last paragraph to make it clear that although the recommended minimum flows will provide a level of dilution of pollutants at low flows, this function was not explicitly considered when calculating the recommended minimum flows adopted in this report. This sentence could make reference to the rationale outlined two paragraphs earlier, "that providing additional flow to increase the dilution of out-of-stream contaminants does not avoid impacts…"

(p209) The quote from Hayes (2004) describing water temperature effects on trout has already been presented on page 164. Readers should be referred to this earlier page to avoid repetition.

(p210-211) It should also be made clear that water temperature and DO were not explicitly considered when calculating the recommended minimum flows. The recommended minimum flows in our report (Hay & Hayes 2005; which appear to have been adopted, as is, in this report) were based solely on consideration of retention of physical habitat, defined in terms of water depth, velocity and substrate. Hayes (2003) statement that "despite the very real effects of high water temperature on fish… there is not much scope for mitigating high temperature in the context of flow management" (and the ensuing discussion of minimal temperature effects of flow reduction) can be used as a rationale for not explicitly considering temperature, but we think this decision needs to be explicitly stated. The fact that DO was "satisfactory" across most samples (even at flows below the MALF at Hopelands; Figure 43 p155) could be used to justify the decision not to explicitly consider DO in developing minimum flows.

(p213) Should make the distinction between the IFIM as a decision support framework, and instream habitat modelling as a component of the IFIM.

(p213) Hayes (2004) is cited as Hayes (2004a), but there is only one Hayes (2004) in the reference list (this also occurs in other places in the report).

(p215) The rivers don't show up on map 70.

(p216) RhyHabsim should be capitalized to RHYHABSIM and maybe defined (*i.e.* River Hydraulics and Habitat Simulation)

(p216) Paragraph 2 lists sections from Hayes (2004), then paragraph 3 begins with "These sections of the Hay and Hayes 2005 report provide supporting information….". we suggest this should be changed to read "Sections of the Hay and Hayes (2005) report also provide supporting information…".

(p216) We think it needs to be stated more explicitly what level the trout (native fish and invertebrate) population is to be maintained at under the management objective. It seems to be tacitly implied that the objective is to maintain these populations at, or near, the levels that would be expected to be sustained under the current flow regime (*i.e.* no detectable difference from present). However, this is not explicitly stated.

(p216) Hay and Hayes (2004) is not in the reference list. Neither is Death et al. 2005, although it is included in an appendix.

(p216) Paragraph 6 (3rd sentence) Should be changed to read "Managing flows for trout, as the most flow demanding and most highly valued fish in New Zealand rivers, ….. "

(p216) non-migratory galaxias has autocorrected to non-migratory galaxies.

(p216) Jowett 1990 and 1992 are not in the reference list. This could perhaps be circumvented by saying something like "see original reports for references cited in quotes". This would also cover other references cited in quotes throughout the report (*e.g.* Horizons (2004) in a quote of Hay and Hayes (2005) on page 219).

(p216) The last paragraph quotes from Hay & Hayes (2005) "When setting minimum flows for instream values the assumption is made that low flow is a limiting factor. This is supported by research that indicates that the MALF is ecologically relevant to trout carrying capacity (Jowett 1990; 1992), because the MALF determines the average annual living space for adult trout". Since this report (*i.e.* Hay & Hayes 2005) was produced we have revised the wording of this rationale, in light of the fact that the MALF is not an annual event (it has a return period of about 2.33 years), although it is indicative of the magnitude of annual low flow events experienced over time. This sentence would now read: "This is supported by research that indicates that the MALF is ecologically relevant to trout carrying capacity (Jowett 1990; 1992), because the MALF is indicative of the average annual minimum living space for adult trout."

(p217) "Hayes and Hayes" in first sentence of paragraph 4 should read "Hay and Hayes".

(p219) We suggest the last paragraph should make clear that the recommended minimum flows were derived based on consideration of physical habitat only (*e.g.* it could say "IFIM provides a suitable method to determine minimum flows that are consistent with the stated management objectives, and although not considered explicitly in the analysis on which these recommended minimum flows are based, should also provide for requirements based on legislative requirements, water quality…."

(p220) Suggested changes to wording of core allocation model description:
"The core allocation volume in this water resource assessment is calculated by defining the acceptable level of risk to the environment and to the resource user. This is done based on the concept that the management flow minus the minimum flow defines the core allocation *i.e.*

Core Allocation = Management Flow – Minimum Flow

The level of the management flow (and therefore the core allocation) can be set to control the risk of the minimum flow occurring, based on frequency and duration analyses of the hydrological record. When the core allocation is fully allocated to users and being fully used, the frequency of occurrence of the management flow becomes the frequency of occurrence of the minimum flow. This method then, quantifies the risk to the environment and to users of a minimum flow occurring."

(p222 para 2) We don't see how water abstraction could act to increase the magnitude of flow statistics. Surely less water left in the stream → lower MALF, median *etc.* and shorter return period for low flows of a given magnitude. Is this paragraph intended to be talking

about the effect on the flow statistics of naturalizing the flow record to take account of consented abstraction?

(p223 para 1) "the more frequently rainfall response occurred", should this be "rapidly"?

(p223 para 7) Makes reference to "Sections 2.4.5 and 0". Not sure what section 0 refers to?

Only skim read pages 224-226

(p226) Table 29 has an error message in it.

(p227) Refers to section 4.4.1. However, this is still section 4.4.1. Might be better to say "earlier in this section".

(p234) In the definition of a Permitted activity, point "b." the word "fish" appears to have been omitted after "juvenile"

(p236) It is ambiguous which method is being referred to in paragraph 2, of the two methods discussed in the previous two paragraphs. We assume (based on the context) that it is the second method, but this should be clarified.

(p236) Repeated phase "during low flow" in sentence 2 of paragraph 2.

(p236) The last sentence in the last paragraph says "… the median take has been on average…". Does this mean that the value given is the average of a series of median values for a given time period (*e.g.* monthly medians)? And if so, what is the time period? Or maybe having median in the sentence is a typo?

(p237) There is quite a lot of repetition of the Ministry of Health guidelines for minimum water supply being 250 litres/head/day, as well as other methodological assumptions discussed on the previous page. Also the source of these guidelines does not appear in the reference list (*N.B.* they are first mentioned on p235).

(p239) Aqualinc (2004) is not in the reference list.

(p239) PDD and ADD are not defined. We assume these are peak daily demand and average daily demand per stock unit.

(p243) A map showing the management zones (including sub-zones) and their respective recorder sites would be helpful. On first reading through the descriptions of the management zones it sounds as though sections of some tributaries are not included in any zone (*e.g.* Tamaki downstream of the water supply weir). These sections seem to be picked up later as subzones. But it would be clearer if they were all laid out in a figure.

5.1 Section 4.9 Defining flow statistics for managing the sub zones (p 244-279)

We read only subsections relating to minimum flow recommendations in this section.

(p252) In paragraph 2, where the original recommended minimum flows are outlined, it should be stipulated that these were based on retention of yearling to small adult brown trout habitat, since this is the major point of difference with the minimum flow adopted on the next page (which is based on retention of large adult brown trout habitat).

(p253) If we understand Method 2 correctly, calculating the average proportion of flow at Okarae Road that the flow at Weber Road constitutes, amounts to an alternative way of fitting a line to the data (rather than the least squares method used in Method 1). The resulting line has a slope of 1.19 and a y-intercept of zero. This method is still likely to have a low R^2 value (if not lower than Method 1), you just don't get to see it (except in the range of possible slopes based on each individual data point, of 1.11 - 1.27).

(p254) Paragraph one should read "…Cawthron were asked to complete the IFIM analysis using large adult brown trout habitat suitability." The addition of the adjective "large" would also be necessary in the following paragraphs.

(p257) The first sentence in the second last paragraph should read "…calculated for brown trout yearlings / small adults feeding habitat." Either a "/" or a "-" or the word "to" could be used, what ever is consistent with mention of these criteria in the rest of the report.

(p257) We can't understand the last sentence on the page. We think it is saying that abstractions downstream of the Stephensons recorder will have to cease abstraction when the flow measured at Stephensons falls below a flow, equal to the stated minimum flow at Stephensons (0.36 m^3/s) + the total volume allocated downstream to the confluence with the Manawatu, from the abstraction site in question. This would ensure that the minimum flow set for Stephensons would also be maintained downstream. Is this correct?

(p268) We suggest that the first sentence of the second to last paragraph should be worded "Based on the IFIM habitat analysis a minimum flow was recommended to retain a proportion of the habitat available at the MALF." The important thing is that it is a proportion of the habitat at the MALF that is to be retained. The current wording could be interpreted as meaning that all the habitat available at the MALF would be retained.

(p279) Same comment as p268 above.

6. CHAPTER 5 WATER ALLOCATION OPTIONS FOR THE UPPER MANAWATU CATCHMENT (P 204)

We read all sections in this chapter.

(p280 para3) We suggest that when referring to the results of the IFIM habitat analyses, that they be referred to as the "IFIM habitat analyses" (or "analysis"), rather than the IFIM survey. The term "survey" should be retained to describe the field measurement component, and could profitably be changed from "the IFIM survey" to "the IFIM habitat survey". This comment applies throughout the report.

(p281) In paragraph 1 two tables are referred to (Table 33 and Table 35, which is in Appendix 8 [although it currently says that it is Appendix 4]). It would make it a lot easier for the reader if page numbers where given when a table, figure or map, referred to in the text, is located elsewhere in the report. This is common throughout the report and because the report is so large, it becomes quite time consuming to track down the appropriate figure, *etc*.

(p281) In the justification for applying the Hopelands habitat modelling results to derive a minimum flow at the Tiraumea site, it would be worth mentioning that the channel slope is similar between these points (if indeed it is), as well as the similarity of channel shape already mentioned. If it is not, then this should be mentioned anyway, so that the reader is informed regarding the strength of similarity between these reaches.

(p284-286) References made to Appendix 4 should actually be to Appendix 8.

(p288) The first sentence in the last paragraph is a little confusing. This could be improved by removing repetition of the fact that it provides a core allocation which is less than 30 % of MALF (*i.e.* the first sentence could read: "Option 3 allows allocation of 4.214 – 4.109 = 0.105 m^3/s (less than if the 30% MALF allocation limit method was used)".

(p289) We don't see how considering the Group C consents as part of the core allocation will allow the discrepancy between the maximum daily and instantaneous rates of abstraction to be resolved. It seems to us, from the discussion around this point, that it is the inclusion of the Group C consents in the core allocation that cause the instantaneous rate of abstraction to be over-allocated. Is this resolution due to the Group C consents having to cease abstraction at a level greater than the standard minimum flow applied to Group B consents? We think that this could do with being explained more clearly. [The existence of Group C consents (defined on p205 as being consented to abstract within the supplementary allocation, and being able to abstract above a specified flow), if they were

considered outside of the core allocation, would seem to require that the supplementary allocation be defined].

(p289) Also, in relation to the comment above, we can't see how considering Group C consents as a part of core allocation would allow permitted takes, that exceed the rate of take allowed, to be legitimized into the consented volumes.

(p290) Part of the rationale for cumulative allocation at any point in the catchment not exceeding 30 % of the MALF (p206), was Jowett & Hayes' (2004) suggestion of this level of allocation as a trigger level for more indepth consideration of instream habitat and possible downstream effects. Although this 30 % of the MALF level may be exceeded in some parts of the catchment, instream habitat analyses have been undertaken in a number of places throughout the catchment. Depending on the area in the catchment, the results from these analyses may indicate that allocation of > 30 % of the MALF would not be expected to have serious adverse effects (this would have to be considered on a case by case basis).

(p295) Applying the minimum flow based on the IFIM habitat analysis for the Manawatu at Weber Road to the section downstream of Weber Road to the Tamaki Confluence is likely to result in a larger reduction in habitat availability than the 90 % habitat retention level applied would imply. There are at least two named tributaries contributing flow to this section on the NZMS 260 series map. The rationale put forward for not extrapolating a MALF estimate based on the proportional increase in catchment area from Weber Road (*i.e.* that the channel form is likely to be significantly different) is equally applicable as justification for not simply applying the minimum flow calculated for Weber Road to this section, if not more so. One possible alternative might be to manage this section based on the Manawatu at Hopelands, taking into account the flow contribution of tributaries downstream of this section. Alternatively, the method proposed and then discounted (*i.e.* the same catchment extrapolation method applied between the Manawatu at Hopelands and at Tiraumea) might be reasonable (this would also be more consistent, since this method is used on the next page to derive a management flow, which defines the maximum core allocation). This method would produce a MALF of approximately 2.3 m^3/s, 70 % of which is approximately 1.61 m^3/s (based on no more that 30 % of the MALF being allocated without indepth consideration of instream habitat and downstream effects). This could be interpreted in two ways; 1) the minimum flow suggested for this reach (1.6 m^3/s, based on the Manawatu at Weber Road recorder) is very similar to the flow that would be required to meet "the 30 % of the MALF rule" (proposed earlier in the report; p 232), providing some justification for applying the former. 2) the minimum flow suggested for this reach is less than 30 % of the MALF, and therefore breaches the "the 30 % of the MALF rule" (possibly justifying managing this reach based on the Hopelands site, since this would explicitly take downstream effects into account). The interpretation depends on how strictly the "the 30 % of the MALF rule" is to be applied.

(p295) The first sentence of the last paragraph says "…the level of the <u>maximum daily rate</u> of abstraction close to that of the <u>maximum daily rate</u>". One of these should be the maximum <u>instantaneous</u> rate. Also, the other sentence in this paragraph doesn't seem to make sense.

(p298) Deriving naturalized flow statistics would not necessarily impact on the minimum flows recommended. If the management objective is to maintain the trout (and other aquatic organism) populations with no detectable difference from the <u>present</u> condition, then minimum flow calculations based on non-naturalized flow statistics would suffice. However, if the <u>natural</u> condition of these indicators is to be maintained, naturalized flow statistics would be needed. Naturalized flow statistics are probably necessary to derive management flows that define the core allocation (as discussed in the last paragraph on

p298), because the core allocation is presumably intended to include existing takes (so it is important to know how much of the time the natural flow regime will be able to provide that much water for abstraction). The proposed way of deriving the core allocation limit in this management zone (*i.e.* as 30 % of the estimated naturalized MALF) does not provide any idea of the likely effect on the frequency or duration of minimum flows and water restrictions. However, the effect is likely to be minor, since only a relatively small proportion of the MALF is to be allocated.

(p301) Given that the flow statistics for the Kumeti at Te Rehunga site should be fairly robust (being based on the flow recorder at that site), and that the instream values (trout spawning and rearing, and the dwarf galaxias population) are arguably of relatively lower value than the large adult brown trout fishery in the mainstem Manawatu, perhaps the 70 % habitat retention level could have been adopted in this reach (this type of latitude, given sufficient confidence in the flow statistics, is suggest in Hay & Hayes 2004). This would provide allocation of a bit more water.

(p303) As well as the two allocation limit scenarios presented, perhaps one other might be useful. An allocation limit based on the upstream sites that are currently not fully allocated being fully allocated, and those sites that are currently over-allocated remaining so, would give an indication of the situation if the proposed allocation limits were instituted, without revoking existing consents.

(p305) The first paragraph is confusing. The first sentence starts "Allocation upstream of Manawatu at Hopelands totals…". Then the second sentence starts "Allocation upstream of this zone totals…". But each sentence gives different total values. We assume that these sentences must be referring to different zones that are successively upstream of one another, but we can't decipher where these zones might be. Again a map might be helpful to facilitate this explanation.

(p306) In the paragraph "When fully allocated upstream of the Water Supply Weir site the maximum allocation limit for the Tamaki at Stephensons site is (0.460 – 0.260) * 0.3 = 0.060 m^3/s…". It would be clearer to the reader what was going on if this formula was expressed as "…0.460 * 0.3 – 0.078 = 0.060 m^3/s". It is only an issue the order of operations, but expressing it this way makes it clearer what is being done (*i.e.* to find the maximum allocation limit in the downstream reach and then subtract the amount already allocated upstream).

(p308) Paragraph 4 refers to Table 56. However, Table 56 shows management flow options for the Manawatu at Weber Road reach. We are not sure of the relevance of this table to the discussion.

(p308) The last paragraph discusses a Group C consent to abstract 0.042 l/s. This seems like a very small take. Should it be 0.042 m^3/s?

(p313) Table 60 is an excellent summary.

7. CHAPTER 6 KNOWLEDGE GAPS AND FUTURE DIRECTIONS (P ???)

These two pages are not numbered, and one of them has been inserted upside down.

It is a pity that the monitoring programme discussed in the last "knowledge gap" was not initiated prior to setting these new flow management regimes. A few years of "before data" would have been helpful as a baseline for assessing any effects of the new regime