BEFORE THE HEARING PANEL

IN THE MATTER of the Resource Management Act 1991

AND

IN THE MATTER of applications by Tararua District Council to Horizons Regional Council associated with **APP-1993001253.02** for resource consents associated with the operation of the Pahiatua Wastewater Treatment Plant, including earthworks, a discharge into Town Creek (initially), then to the Mangatainoka River, a discharge to air (principally odour), and discharges to land via seepage, Julia Street, Pahiatua

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REPORT TO THE COMMISSIONERS

DR BRENT COWIE (CHAIR), MR REGINALD PROFFIT AND MR PETER CALLANDER

SECTION 42A REPORT OF MICHAEL JOSEPH PATTERSON – SCIENTIST - FRESHWATER

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25 May 2017

A. INTRODUCTION

Qualification and Experience

1. My name is Michael Joseph Patterson and I am giving evidence in these proceedings on behalf of the Manawatu-Wanganui Regional Council (Horizons Regional Council - HRC). My qualifications are stated in my previous s42A evidence to the commissioners dated 21st April 2017.

2. This supplementary evidence has been prepared to expand on areas in my Evidence in Chief (EIC) and to provide a response to matters that have been raised through expert evidence and through submitters.

3. As per my previous evidence I confirm that I have read the Environment Court's Code of Conduct for expert witnesses contained in the Environment Court Practice Note (2014) and I agree to comply with it.

B. REPORT SCOPE

4. This report intends to cover the following:

4.1. Periphyton

4.1.1. Compliance

4.1.2. Exclusion of data

4.2. Phosphorus

4.3. QMCI

4.4. Wetland monitoring

4.5. Instream monitoring locations

4.6. Nutrient standards

4.7. QMCI and Significant Adverse Effects on Aquatic Organisms

4.8. Continuous Dissolved Oxygen

C. PERIPHYTON

5. When assessing compliance against the One Plan for chlorophyll a biomass, Dr. Ausseil in his section 41B report outlines his views that the use of a 95th percentile is not consistent with recent resource consents (i.e. Feilding WWTP) and that he does not agree with this assessment method. In my original evidence paragraph 42 I refer to the technical document supporting the water quality targets in the One Plan (Ausseil & Clark, 2007). I note that in this document it states in regard to chlorophyll a biomass that the "recommended approach is to assess compliance at the 95th percentile level, i.e. Up to 1 sample every two years may not be compliant". This recommendation was based on monthly monitoring i.e. one sample out of every 24 may not be compliant.

6. Given that this statement was unclear regarding the use of the 95th percentile or a percent compliance I undertook both assessments, the results of which are displayed side by side in Table 14 of my EIC. Both results in my assessment (95th percentile and 95% compliance) show the same result, namely that upstream of the Pahiatua WWTP complies with the target and downstream of the Pahiatua WWTP does not, and as such I proceeded to refer to one only. This does not alter the fact that the Mangatainoka River downstream of the Pahiatua WWTP discharge, in my assessment, does not meet the One Plan Schedule E target for Chlorophyll a.

7. In his section 41B report, paragraphs 6.13 through 6.18, Dr. Ausseil discusses how to deal with chlorophyll a results when a sample has not been taken due to high flows. He notes that this is a question that has not been addressed nationally which I agree with. He explains that direct comparison between sites is best done with paired sampling which I agree with. Dr. Ausseil then goes on to discuss different scenarios in which you either ignore high flow observations; or in which you include high flow samples by assuming periphyton biomass is 1 mg/m^2.

8. During my assessment I ignored high flows and believe this is the conservative approach to take. In paragraph 6.13b, Dr. Ausseil explains that periphyton biomass is known to be low when the rivers are in flood, and that a 3 times median flow is commonly used as a flow threshold in relation to periphyton biomass. He also notes that

the One Plan uses the 20th Flow Exceedance Percentile (FEP). The problem is that neither a 3 times median flow nor a 20th FEP is appropriate for all sites. Instead factors such as substrate size and embeddedness specific to a site mean the flow required to cause physical abrasion through sand particles or result in bed movement, and so reset a system, varies. For example Kilroy et al (2016) used the periphyton and flow dataset held by Horizons from 2008 until mid 2015 to calculate which factor of median flow (i.e. 1.5 x median, 2 x median, 3 x median, etc.) explains the most variance in periphyton biomass at all appropriate sites (appropriateness includes factors such as reliable flow series located nearby, enough data points for analysis, etc.). From this they calculated that at Mangatainoka upstream of Pahiatua WWTP the most variance in periphyton biomass occurs at 9 times median flow (26% of variance explained), while downstream of Pahiatua WWTP it occurs at 15 times median flow (33% of variance explained). While this does not mean there would not be a reduction of periphyton biomass during a 3 times median flow, it does mean that it is not appropriate to assume a 3 times median flow is appropriate for all sites. Given the small degree of variance explained by flow, even at the much higher flows listed, it seems unlikely that changing all biomass measurements to 1 mg/m^2 above 3 times median is appropriate. Dr. Ausseil also outlines the risk of discarding the high river flow observations is that it introduces a bias in the data set, which I agree may be true, but I note that with the paired testing approach used by Dr. Ausseil, this same bias is applied to data from the upstream site. In this case the upstream site still complies, and the downstream site doesn't. Simply changing all non-measured data points to a $1mg/m^2$ biomass also adds a bias to the data, simply in the opposite direction.

9. Neither scenario proposed by Dr. Ausseil meets the One Plan Biomass Schedule E target in either 1 out of 24 samples (note that 1 out of 24 monthly samples relates to 95.8% compliance), or a 95% compliance. i.e. in para. 6.14 and 6.16 of Dr. Ausseil's section 41B report it is noted that downstream of the discharge exceeds the One Plan schedule E target on 5 occasions. If high flow observations are included as a $1mg/m^2$ chlorophyll a biomass, there are 94 paired observations which results in a 94.6% compliance (i.e. neither 95 or 95.8% compliance). If high flows are excluded there are 86 paired observations which results in a 94.2% compliance. For these reasons it is my opinion that the discharge has not met the Schedule E chlorophyll a biomass targets of the One Plan.

10. Note that I accept that the discrepancies are only small, however with a dataset of at least 86 paired data points (depending on whether you exclude data or not), I would contend there is enough data present that compliance can be assessed (bearing in mind 3 years of monthly data or 36 data points is often considered the minimum), and that ultimately compliance must be assessed at some point, even if only by a small margin.

D. PHOSPHORUS

11. In the evidence of Mr. Crawford (Table 6 and para. 10.4) there is a proposed effluent quality standard for DRP with a median of 0.5 g/m^3 and a 95^{th} percentile of $1g/m^3$. I note that this is somewhere in the vicinity of a 2 to 3 fold reduction in DRP being discharged from this plant.

12. Ultimately the concern with DRP from this discharge has been the stimulatory effect on growth of periphyton downstream of the WWTP. As stated above I believe that the discharge does currently breach the One Plan guidelines for Chlorophyll a biomass, even if only by a small amount, and I am of the opinion that this is as a result of the DRP component of the discharge. Given the proposed reduction of DRP from the WWTP it is my opinion that there will be a reduction in periphyton growth stimulation downstream of the WWTP such that the One Plan targets are no longer likely to be breached.

13. Further to the above, I consider summer and early autumn to be the period of most risk, due to low flows often occurring during this period. I note in Table 4 of Mr. Crawfords evidence he outlines flows and DRP loads that have occurred from the plant during 2016. From these an average daily concentration for each season can be calculated. For summer this equates to approx. 1mg/L and for autumn approx. 2.8 mg/L. In table 6 of Mr. Crawfords evidence he suggests proposed effluent quality standards of 0.5 and 1 mg/L for median and 95^{th} percentile values respectively. If this is achieved, I believe the DRP quality from the plant will be significantly improved during this sensitive period.

14. I am of the opinion however, that there is still likely to be an increase in periphyton growth to some degree, and that this may exacerbate dissolved oxygen conditions, as described in my EIC. For this reason I recommend that continuous dissolved oxygen monitoring is still undertaken.

E. WETLAND MONITORING

15. The inclusion of the wetland as part of the treatment system is likely to introduce new concerns that need to be addressed. Primarily amongst those are the need for ongoing maintenance. Mr. MacGibbon in paragraph 39 of his section 41B report states that after plants have reached their full size and maturity their nitrogen and phosphorus removal declines due to reduced vigour, and they can shed more leaves resulting in greater levels of nutrients being lost. For this reason it is important that a wetland maintenance plan is included as a consent condition.

16. Mr. MacGibbon also states that a well-performing, well maintained wetland with a 2 to 4 day water residence time can be expected to extract 50% or more of the nitrate included in the effluent. He also points out (para. 13) that in some cases wetlands can be effective at reducing living faecal bacteria. Counter to this is the risk of an increase in nitrogen and phosphorus loss if the wetland is not maintained, and the evidence of Mr. Crawford (para. 11.21 – 11.23 section 41B report) which suggests that non-human *E. coli* sources can enter the wastewater stream within the wetland and that the monitoring point for UV disinfection should be upstream of the wetland.

17. Given these different views, it is my opinion that ultimately discharge monitoring should be carried out at the last weir at the bottom end of the wetland prior to reaching the Mangatainoka River. The applicant may additionally propose to monitor upstream of the wetland (as suggested by Mr. Crawford), and this can be done as well, but compliance in my opinion should be assessed on what is leaving the treatment system as a whole, and if the wetland is considered part of the treatment system then it should be included in the monitoring results.

18. I am also aware of some of the potential risk associated with the wetland, i.e. increased faecal contamination from waterfowl, and increased Nitrogen/Phosphorus release if the plant is not well maintained. Given this is the case I would suggest for the sake of surety for the Regional Council, compliance would be after the wetland to ensure that any potential risk is accounted for.

F. INSTREAM MONITORING LOCATIONS

19. The discharge location of the wetland is proposed to be across land to the Mangatainoka River. The upstream monitoring point for biological monitoring in

particular is important to consider. There is a single riffle section upstream of the discharge point that is suitable for monitoring of invertebrates in particular. It is important that the use of this is maintained due to the Pukemiku Stream and a weir associated with an infiltration gallery a sort distance upstream. For this reason the ultimate discharge point needs to be as far downstream as possible, within the bounds of the land that is available. If the discharge point is at the most downstream end of the available land, I am of the opinion that it will be possible to maintain an appropriate upstream monitoring point.

20. There has been some discussion on the effect of groundwater and where this may discharge in relation to the monitoring point. Particularly if it were to travel parallel to the River and so bypass the downstream monitoring point. I would suggest that an appropriately placed downgradient monitoring bore would allow us to detect such an occurrence. I would also suggest that lining the wetland right to the edge of the Mangatainoka River would ensure certainty around our monitoring points (as suggested by Mr. Teo-Sherrell in his submission), though the viability of maintaining a liner in a flood would need to be discussed by an appropriately qualified person.

G. NUTRIENT CONDITIONS

21. Ammonia – In the section 41B report of Mr. Crawford, a median (table 6) and mean (para. 10.3) ammonia concentration of 10mg/l is proposed. Table 6 proposes a 95th percentile of 15 mg/l. I note that during 2010 – 2017 effluent monitoring showed a median of 1.8 mg/l and 95th percentile of 16.64 mg/l is achieved; and from January 2016 until January 2017 a median of 0.34 mg/l, and 95th percentile of 2.64 mg/l is achieved (note: mg/L = g/m^3) from the plant. It therefore seems the suggested median and 95th percentile of Mr. Crawford is a significant step backwards and is less than they are already achieving.

22. scBOD5 – Mr. Crawford states that there is currently no effluent scBOD5 data available from the plant. I am uncertain if Mr. Crawford hasn't been given access to a complete dataset or if there is some miscommunication, however scBOD5 has been measured at this plant for multiple years (at least since 2008). I have reconfirmed in the Horizons computer system and with the laboratory that scBOD5 is being analyzed. Given the results that the plant is already achieving are better than the proposed scBOD5 values for the plant, I would suggest that these will need to be revised. I have suggested a

median of 1g/m^3 and 95th percentile of 3 g/m^3 both of which should be achievable based on the 2016 results.

H. QMCI

23. In my original evidence I spoke to the possible inclusion of a 15% change in QMCI rather than a 20% change in QMCI as an indication of enrichment between upstream and downstream of the Pahiatua WWTP. I acknowledge Dr. Ausseil not supporting this approach and he has outlined several reasons why this would not, in his view, be acceptable.

24. The state of MCI upstream of the discharge is already at least partially degraded. In my EIC I outline the conditions in the Mangatainoka River. MCI upstream of the discharge ranges between 110 (2015) and 94.1 (2012). This results in either a “Good” or “Fair” category as outlined in Table 1 of my original evidence.

25. I have attempted to establish what a 20% change in QMCI would result in for some of the other metrics that are regularly calculated, and how this would place in the range of sites monitored in the Manawatu-Wanganui region. In order to do this I have calculated a 20% reduction to QMCI at the upstream site, then incrementally removed the highest tolerance invertebrates from the upstream data set until this 20% lower QMCI value is reached. MCI, %EPT taxa and %EPT abundance can then be calculated on this new data set. This is displayed in Table 1. I acknowledge this does not allow for the lower tolerance species that often become more common in enriched systems (for example a proliferation of choronomids). This will affect MCI in particular and my calculated MCI will underestimate what would actually be expected (as I may not account for the additional scores of some genus, for example some of the chironomids, if they do not currently exist upstream). This does not affect the calculation of the EPT metrics in particular however, as I would not expect additional EPT species to arrive in an increasingly enriched system. I also acknowledge the perils of comparing sites based on QMCI that are in different rivers, however think it is appropriate to at least indicatively place this 20% reduction in the context of all sites monitored in the region.

Table 1: Calculated metrics resulting from a 20% change in QMCI based on the Upstream Pahiatua WWTP discharge site

Date	2016	2015
Upstream QMCI	4.17	3.87
20% reduction	3.32	3.09
Upstream MCI	98	110
20% reduction	71.25	64
Upstream %EPT richness	43.5%	61.1%
20% reduction	23.5%	20.0%
Upstream %EPT abundance	29.1%	51.9%
20% reduction	26.9%	5.5%
Upstream ranking (QMCI)	66/91	72/81
20% reduction ranking (QMCI)	89/91	79/81

26. In Table 1 I included both 2016 as a wet year, and 2015 as a dry year. This is to give an indication of the range of effects. In both cases I note that the QMCI value will end up in the 'Poor' quality class for this metric as outlined in table 1 of my EIC. Note that in 2016 this is a change in class from 'Fair' to 'Poor'. Note also the large reduction in the %EPT abundance, particularly in 2015. This is particularly relevant in a river with Schedule B value as a Regionally Significant Trout Fishery. EPT species (i.e. Stoneflies, mayflies and caddis flies) tend to be larger bodied, more energetically efficient prey items for trout, and so a reduction in EPT abundance impacts upon trout diet.

27. From the above, I conclude that a 20% change in QMCI from the upstream discharge site could greatly impact metrics such as %EPT abundance (or in fact even total EPT abundance given my calculation has not added additional invertebrates in). It would also mean the downstream discharge site would have to rank as one of the worst in our region before it was acknowledged there was an effect. Given the values associated with this reach of river, particularly the regionally significant trout fishery, I suggest it would be appropriate to apply a more stringent allowable decrease in QMCI.

I. QMCI AND SIGNIFICANT ADVERSE EFFECTS ON AQUATIC LIFE (RMA 107, 1, G)

28. I note that in the past QMCI has been used as the sole measure of life supporting capacity and effects on aquatic life. Dr. Ausseil makes reference to this in his section 41B report (paragraph 9.2 a). While I agree that this is appropriate for measures relating

to nutrient enrichment of a waterway, I do not believe it accurately covers dissolved oxygen conditions. During his presentation to the hearing panel Dr. Ausseil spoke of the lack of data that was behind the 80% threshold for dissolved oxygen that was included in the One Plan. I note that one of the documents used to shape the One Plan, by Hay, Hayes and Young (2006), outlines dissolved oxygen conditions that affect trout fishery values. This includes reference to suitable dissolved oxygen levels for eggs. They outline that in their opinion, the >80% saturation guideline provides protection for trout, including for eggs and fry.

29. During the development of the One Plan Dr. Russell Death collated known environmental limits for invertebrates and fish in New Zealand (Death, 2006). In this he lists invertebrate species found in the Manawatu-Wanganui Region, and notes that very few of these taxa have been examined for environmental tolerances. He provides no environmental tolerances for DO on invertebrates, presumably because none could be found. He does note that for native fish the main studies of the effect of DO suggests that the USEPA (1986) levels for Salmonid waters would be suitable for preserving New Zealand Native fish. This is summarised below.

Table 2: Water column dissolved oxygen concentrations (mg / l) recommended by the USEPA (1986) to confer 5 levels of protection for waters containing salmonids. Modified from table 5 Dean & Richardson (1999).

Degree of impairment	Early life stages	Other life stages
None	11.0	8.0
Slight	9.0	6.0
Moderate	8.0	5.0
Severe	7.0	4.0
Acute limit	6.0	3.0

30. From this we can draw the conclusion that the effects of dissolved oxygen variation are best linked to fish, and that the One Plan targets for DO were not set up with invertebrates in mind. Therefore when discussing the effects on aquatic life, where low DO is a potential concern, I would suggest that QMCI should not be the sole metric considered.

31. During the first day of the Pahiatua hearing there was discussion regarding the ability of fish species to avoid areas of low dissolved oxygen. This does not take into account how this may reduce fish feeding time, increase stress levels, or how these low DO

conditions may impact other life stages of fish such as eggs and/or fry that are unable to avoid these low DO areas. For example there are several bully species known to be present in the Mangatainoka catchment that spawn during January and February when DO fluctuations are likely to be high. In some areas this may limit available spawning habitat or potentially result in death of eggs.

For the above reasons, I am of the opinion that QMCI alone can not be used to measure either Life Supporting Capacity values, or effects on aquatic life. I am of the opinion that continuous dissolved oxygen also needs to be considered. To this end a condition will need to be formulated that addresses this concern.

J. CONTINUOUS DISSOLVED OXYGEN

32. As stated above in paragraph 14 I am of the opinion that there is still a possible stimulatory effect on periphyton due to the discharge (largely due to the reduced, however, still present quantity of DRP being discharged) that could result in an increase in DO fluctuations. It is my opinion that this needs to be monitored. Dr. Ausseil covered this in his presentation at the hearing during which he suggested spot measurements in the early morning as a more cost effective and sensible method of initially monitoring DO minima, as opposed to continuous monitoring. He gave justifications such as the requirement to have structures in a waterway for continuous monitoring and cost.

33. I have carried out short 2 to 3 week continuous monitoring programmes in the past, and do not see this as being as difficult as Dr. Ausseil may have implied. In practice either a metal Waratah fencing post, or other easily installed structure can be installed in a waterway from which the probe can be suspended. The cost of the probes has decreased substantially in the past few years, and I would not be surprised if there are not consultants or other businesses that these can be hired from. By comparison, spot samples means staff will be required to daily calibrate a hand held probe (which will still need to be purchased or hired if it isn't currently owned), travel to the site and sample, and this will have to be done so that the sample is taken somewhere between 4 and 5 am to ensure the instream minima is represented with some degree of accuracy. Spot measurements during these times would also create Health and Safety issues with sampling having to occur during the dark. In addition a minimum of at least two staff would be required (more likely four) as people would need to be present at the upstream and downstream sites at the same time to have simultaneously captured data.

34. The other major advantage of continuous data, and corresponding disadvantage of spot measurements, is certainty that the minima have been captured (within the bounds of the continuous monitoring probes sample frequency). It is difficult to be certain that spot measurements will actually be at the time when minima are reached, because there is no way to compare this to what happened before or after. I will also note that Dr. Ausseil suggested that analysis could be undertaken by looking at the amount of time that DO falls below certain thresholds (i.e. 80% saturation, 50% saturation) and this could be compared between upstream and downstream sites in order to try and quantify the effect of the discharge. This would not be possible with spot measurements carried out once daily. Instead the only possible comparison would be minima between sites.

35. For the above reasons, I am of the opinion that continuous DO monitoring is preferable, and recommend that this is undertaken.

DATED this 25th day of May 2017

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Michael Joseph Patterson

REFERENCES

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