IN THE MATTER OF	The Resource Management Act 1991
AND	
IN THE MATTER OF	Lake Rotorua Nutrient Management – **PROPOSED PLAN CHANGE 10** to the Bay of Plenty Regional Water and Land Plan

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STATEMENT OF REBUTTAL EVIDENCE OF DAVID PHILIP HAMILTON ON BEHALF OF THE BAY OF PLENTY REGIONAL COUNCIL

Rebuttal topic: Responses to the evidence of THOMAS STEPHENS FOR DAIRY NZ LTD.
6 March 2017

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1. INTRODUCTION

1.1 My full name is DAVID PHILIP HAMILTON. I hold the Bay of Plenty Regional Council Chair in Lake Restoration and am a Professor in Biological Sciences at the University of Waikato I confirm my qualifications and experience, and confirmation of compliance with the code of conduct for expert witnesses, as set out in my evidence in chief.

1.2 Below I have set out the main reasons why I disagree with Dr Stephen's evidence: it 'cherry-picks' data, and overlooks some of the usual rules of analysis and interpretation. I understand that weight accorded to evidence is a matter for the Panel to determine. I have explained the main reasons upon which I base my opinion and have pointed out many of the matters that I disagree with Dr Stephens about, and explained why. Where I have not commented does not mean I am in agreement.

1.3 In providing this rebuttal, I will reiterate parts of my summary and reinforce the evidence base for this rebuttal. It is important to appreciate that the information given in both my full and summary evidence condenses a large quantity of original research that I have conducted on Lake Rotorua over the past 15 years. This research includes empirical data, detailed analyses of the lake and catchment dynamics, and catchment and lake modelling. This research has been subject to multiple reviews, including:

(i) 'Blind' review by reviewers with expertise in the subject area, as part of a standard scientific review process for papers submitted for publication to scientific journals. The editorial process involves editorial evaluation of whether a paper is suitable for publication following consideration of the reviews (usually two), corrections by the authors, and one or more resubmissions of the paper until the paper is either rejected or deemed suitable for publication. A number of peer reviewed published papers are referenced in my evidence.

(ii) Evaluation of research (reports, data and analyses) by the Water Quality Technical Advisory Group for the Rotorua lakes. This group is selected by the Bay of Plenty Regional Council to provide independent scientific advice to the council. It comprises of external members from the University of Waikato, NIWA, GNS, DairyNZ, Scion, Te Arawa Lakes Trust and Rotorua Lakes Council. The Water Quality Technical Advisory Group provides a rigorous consensus based approach on the quality of the science and technical investigations required to better inform lake management.

iii) Internal review (within the Environmental Research Institute of the University of Waikato) of reports commissioned by Bay of Plenty Regional Council. This review includes scientific review independent from the authors and approval for release by a designated authority within the Environmental Research Institute.

1.4 The above processes have ensured a level of quality control and accuracy that is critical to informing the Proposed Plan Change 10. Such a review process (specifically 1.3(iii) above) was applied also when trends in water quality for Lake Rotorua were analysed during the period 2001-2012[1]. This analysis was undertaken at the request of Deputy Environment Commissioner David Kernohan in collaboration with the DairyNZ and on behalf of the Lake Rotorua Stakeholders Technical Advisory Group, as mentioned in the evidence of Dr Stephens. However, Dr Stephens appears to have used data selectively from this report (i.e., selected subsets of data within the period 2001-2012). As mentioned below, his evidence which relates to this report is his interpretation and is not the consensus of the authors, nor is his interpretation contained within the report. Most other evidence that Dr Stephens presents is based on a critique of the scientific papers and reports presented by me and which have been extensively peer reviewed and contain original research from field data and laboratory experiments, as well as modelling studies. I refer to the relevant sections of his evidence below, with the relevant paragraphs of his evidence in parentheses, in my rebuttal.

2. REBUTTAL TO STATEMENT OF EVIDENCE BY THOMAS STEPHENS FOR DAIRYNZ LIMITED[2]

2.1 (2.1) and (2.2) It is not clear what is meant by a “change to our understanding of algal-nutrient dynamics in Lake Rotorua”. An evidence-based understanding of algal-nutrient dynamics is given in the Smith et al. (2016) paper[3]. It provides a detailed analysis of nitrogen, phosphorus and chlorophyll a concentrations in Lake Rotorua, as well as a summary of multiple nutrient limitation bioassays that have been undertaken to contribute information on algal-nutrient dynamics in the lake.

2.2 (2.2) Dr Stephens statement that “…the lake response to aluminium sulphate (“alum”) dosing has achieved the Regional Water and Land Plan Objective 11 (TLI ≤4.2) under markedly different nutrient loading mechanisms than expected”, requires clarification as to what these loading mechanisms are. I interpret that Dr Stephens means that the TLI

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[1] Abell et al. (2012)
[2] Numbers in brackets denote the paragraphs in Dr Stephen’s evidence that I am rebutting.
[3] Smith et al. (2016)

target was achieved despite external loads exceeding the 'sustainable' values, but he has failed to mention that the overriding reason for this is due to alum dosing.

2.3 (2.3) Dr Stephens states that "I do not comment on the risks nor appropriateness of continued alum dosing". This is incorrect; see footnote 43 of his evidence.

2.4 (3.1) It is unclear why Dr Stephens has truncated his analysis to 2003 when the report that he co-authored[4] analysed data from 2001. Incidentally, 2003 was the year with the highest TLI value. Choosing this year as a starting point biases the analyses. In this paragraph Dr Stephens mentions that "lake health has oscillated about a TLI of 4.2 (±0.2), required by the Bay of Plenty Regional Water and Land Plan, Objective 11" [since 2012]. Dr Stephens has misinterpreted the TLI objective in the Water and Land Plan. It is a limit, not a target. Dosing of both streams commenced in 2010 and since that time the TLI has ranged from 4.1 to 4.4; it has only ever been *less than* 4.2 in one year (2014[5]). Because of the log-based nature of the TLI scale, a 0.2 overshoot of the TLI represents a significant failure to meet Objective 11. This error is also relevant to paragraph 4.1 of his evidence. It is also of note that the data have again been 'cherry picked' by Dr Stephens; his analysis starts in 2012, which is the first year when the TLI was less than the target, yet alum dosing of both inflows commenced in 2010.

2.5 (3.2) Dr Stephens states that "Improving TLI at Lake Rotorua was disputed prior to 2012" and references the joint report[6]. It is not clear what was disputed as the report was the first to provide a >10 year data set for evaluation of a contemporary TLI trend. In this paragraph he also states that "the lake has become P-limited, largely due to alum dosing" and refers to the report by Hamilton et al. (2015)[7]. This statement is inaccurate. First, it should refer to phytoplankton (not the lake) in terms of nutrient limitation; a mistake made consistently throughout his rebuttal. Second, the Executive Summary of the Hamilton et al. (2015) report states: "Even with alum dosing, Abell et al.'s (2014) study and field observations suggest that in Lake Rotorua there are locations where, and periods when, either nutrient or both limit phytoplankton growth". The statement in the Hamilton et al. (2015) report is consistent with the occurrence routinely of co- or dual-nutrient limitation.

2.6 (3.4) Dr Stephens suggests that there is "…evidence that P-limitation is effective at attaining a TLI ≤4.2" but he has failed to mention in this paragraph that there is no direct evidence of P-limitation and that P reductions have been attained by alum dosing of two

[4] Abell et al. (2012)
[5] Scholes and Hamill (2016)
[6] Abell et al. (2012)
[7] Hamilton et al. (2015)

streams. His statement that there should be "research into P-management strategies" reinforces the need for Plan Change 10 to include a limit on nitrogen loads to Lake Rotorua, with concurrent phosphorus reductions, as indicated in my evidence. Dr Stephens' statement in this paragraph appears to have also ignored two highly relevant reports about methods to mitigate phosphorus,[8,9] of which McDowell (2010) specifically addresses P mitigation in the Rotorua catchment.

2.7 (4.1) It is not clear what Dr Stephens is referring to when he states that "I demonstrate this was not predicted by in-lake modelling" because the modelling that he refers to[10] was undertaken only up to 2009. The purpose of that study was to predict TLI under effects of land use change (external loading) and climate change, and in-lake effects of alum were neither considered, nor simulated. This was because until 2009 alum was only dosed to a single inflow and at lower rates than for contemporary dosing.

2.8 (4.3) Dr Stephens has presented a very narrow evaluation of nutrient ratios. Phytoplankton do not grow on ratios and they are strongly dependent on the availability (viz. concentration and supply) of nutrients. Both concentrations and ratios play a role in limiting phytoplankton growth, and these may vary amongst lakes depending on phytoplankton assemblages and their nutritional and physiological status. A range of thresholds in mass TN:TP ratios have been defined to represent likelihood of N-limitation and P-limitation, respectively and these are presented by Abell et al. (2010)[11], e.g., <13:1 and >36:1, <9:1 and >23:1. Annual means for TN and TP presented in Abell et al. (2012), of which Dr Stephens is a co-author, yield an annual mean TN:TP mass ratio of 17:1 for the alum dosing period (2007 to 2012); within the range of literature values for co-limitation presented in Abell et al. (2010) and within the range of P-limitation in the lower ratios actually used by Abell et al. (2010) though it is important to note that ratios varied from 12:1 to 28:1 through this period. Dr Stephens notes the need to "manage acute risks of failing to restrict a limiting nutrient". This statement appears to contradict much of his other evidence that argues against restrictions of N. And as Dr Stephens notes "Non-limiting nutrient reductions are increasingly also recommended for N or P even where a single nutrient strongly limits algal yield." This statement is consistent with the need to manage both N and P in the Lake Rotorua catchment as set out in PC 10.

2.9 (4.4) The lead sentence by Dr Stephens: "Trend analysis for Lake Rotorua demonstrates a consistent improvement in TLI since 2003, with most rapid improvement

[8] McDowell (2010)
[9] McDowell et al. (2013)
[10] Hamilton et al. (2012)
[11] Abell et al. (2010)

between 2010 and 2012" should be disregarded. Firstly, Dr Stephens has used a starting period of 2003 instead of 2001 (see paragraph 2.3 above) and secondly, an independent evaluation of TLI for the period 2010 to 2012 was not undertaken. In this paragraph Dr Stephens states, with reference to trend analysis for the period 2010 – 2014 in a report by Hamill and Scholes (2016)[12] that "statistically significant improving trends were still noted in Chl-a, TP, TN and TLI (in decreasing order of magnitude from 12 to 1%/yr; $p<0.05$)", but again he is incorrect. Of these variables only TN is reported to show a statistically significant trend across the two monitoring stations in Lake Rotorua (averaging 3.9% p.a. for the three years; a rate of change that I would hesitate to call a trend). Dr Stephens has misinterpreted these trends as levels of statistical significance across the two stations exceed 0.05 for Chl-a, TP and TLI.

2.10 (4.5) Dr Stephens again uses 2003 as the starting date for 'trend' analysis (see his paragraphs 5.2 and 5.4 also). In this paragraph the reference to "strengthen P-limitation" in Hamilton et al. (2015)[13] is incorrect; I made no such statement. In this paragraph Dr Stephens has again made reference to a trend over three years (2003-2006). Once again he has 'cherry picked' a particular set of data for which (i) 2003 had the highest TLI value and (ii) there is no trend as the TLI does not go down consistently from 2003 to 2006 (values in 2004 are lower than in 2005 and 2006).

2.11 (4.6) Dr Stephens indicates that "Recent improving trends in TP, Chl-a, SD and TLI were not predicted by in-lake and catchment modelling" and, following on from paragraph 4.5, presumably refers to the period between 2010 and 2012. However the Hamilton et al. (2012)[14] report simulated only the period from 2002 to 2009 (i.e., before alum dosing rates were increased substantially and there was a consistent year-on-year decrease in TLI). It is also not clear what is meant in this paragraph by "Whilst understandable given the effects of alum in Lake Rotorua were inadvertent".

2.12 (5.1, 5.2 and 5.3) The statements that "Lake Rotorua has become strongly P-limited (5.1)" and "recent improvement in TLI strongly indicates algal growth in Lake Rotorua is now P-limited (5.2)" and "Ongoing P-limitation of Lake Rotorua (5.3) are incorrect assertions by Dr Stephens and are not based on multiple sources of evidence for nutrient limitation that I present in my evidence. A change in TLI should not be used as an indicator of limitation by a specific nutrient. In paragraph 5.1, it is also wrong to refer to a revised knowledge of P-limitation since Burger et al. (2007), as these authors were the first to find that there were

[12] Scholes and Hamill (2016)
[13] Hamilton et al. (2015)
[14] Hamilton et al. (2012)

periods and locations where P-limitation occurred in Lake Rotorua (although co-limitation by N and P was most common)[15]. The 'earlier consensus' mentioned in paragraph 5.2 should be disregarded as the TLI had not been formulated in the 1960s.

2.13 (5.4) It is not clear what is meant by "empirical testing into the effects of P, N or P and N removal from Lake Rotorua. Clearly it is not possible to experimentally test removal of different combinations of nutrients at a whole lake scale.

2.14 (5.5 and 5.6) In this paragraph Dr Stephens has again incorrectly paraphrased statements made by me and my co-authors. He states that we concluded that "a drive for N-limitation would be ineffective and likely to degrade water quality by promoting cyanobacterial blooms" and "…refuting the recommendation for N-limitation". The title of the paper which Dr Stephens as commented on[16] is "Phosphorus and nitrogen loading restraints are essential for successful eutrophication control of Lake Rotorua, New Zealand". In this paper we make it clear that nitrogen loading controls are important to maintain or reduce current levels of phytoplankton growth-limiting nitrogen in Lake Rotorua. We also state that "Nutrient management policies that advocate reductions only in N loading are likely to lead to sharp and highly predictable declines in the water quality by stimulating the development of CyanoHABs dominated by heterocystous N2-fixing genera such as *Dolichospermum* (basionym *Anabaena*) and *Aphanizomenon*". This is consistent with the proposed Plan Change 10 which seeks to reduce phosphorus loads concurrently with nitrogen loads. The statement by Dr Stephens that "P-limitation has led to reduced dominance of phytoplankton by heterocystous (N-fixing) cyanobacteria in Lake Rotorua since 2003" is flawed from two perspectives. First, there was no proof of P-limitation. Second, dominance by heterocystous cyanobacteria was comparable in 1999, 2000 and 2001 (100%) compared with 2003 (97%), while heterocystous cyanobacteria were also strongly dominant in 2005 and 2007 (97%).

2.15 (5.6b) Dr Stephens states that "Simple linear regressions of TN or TP on Chl-a do not account for the interactive effects of nutrients". Our paper does not present these regressions as being intended for any other purpose than for statistical correlation purposes. Further, the statement that "…despite only TP having driven Chl-a" with reference to Fig. 5 in Smith et al. (2016) is grossly inaccurate on two counts. First, these relationships for TN and TP on Chl-a are statistically inferences, and do not necessarily identify 'drivers'. Second, to interpret that a 1% difference in the Chl-a variation explained by the regression relationships (42% for N and 43% for P)17 means that only TP is relevant, is not a scientific interpretation

[15] Burger et al. (2007)
[16] Smith et al. (2016)
[17] Smith et al. (2016)

of the results presented by my co-authors and I in this paper; there is no “error” (5.6b) in our results.

2.16 (5.7) Dr Stephens points to “P-limitation achieving a TLI of 4.2…contrary to the Section 42 report (paragraph 34) which says “altering the focus from nitrogen to phosphorus alone would not result in the TLI being achieved” ”. Dr Stephens has not considered the challenge required to achieve P limitation in the absence of alum dosing. In my summary of evidence I indicate how this might only be achieved by reducing the anthropogenic load to zero – something that is clearly preposterous.

2.17 (5.8) Dr Stephens cites Hamilton et al. (2012) when indicating “Hence, alum dosing has resulted in combined external and internal TP loads of 34.5 tonnes/yr, approximately that of the sustainable P-load and suggesting the latter is appropriate to sustain a TLI of ≤4.2”. But this report did not evaluate the effect of alum on external and internal TP loads and I am not aware of ever having provided any such estimate.

2.18 (5.10) With reference to Dr Stephens’ comments about the report by Hamilton et al. (2015), we do not run scenarios of P limitation; we run scenarios of different N and P loading to ascertain effects on the TLI. In this paragraph, with reference to model results presented by my co-authors and I, Dr Stephens states that “If external P-loads were reduced to 23.4-30.3 tonnes TP/year, then even if N-loads rose from today’s 642 tonnes TN/year to a projected 730 tonnes TN/year at steady-state (representing contemporary loading without groundwater lag), the TLI would be between 3.5 and 4”. Dr Stephens has not considered what might be possible in terms of reducing catchment phosphorus loads in the absence of alum dosing. His P loads stated above would require the anthropogenic P load to be negligible or zero (see Tempero et al. 2016). The 730 tonnes TN/year referred to by Dr Stephens actually refers to a contemporary land use and future steady state stream concentration *accounting for* groundwater lags: (refer to my evidence in chief at paragraph 15(i)).

2.20 (6.2) It is not that P reduction programmes do not work, it is that they cannot remove as much phosphorus as alum because more than 50% of the catchment load is from natural sources.

2.21 (6.3) It is unclear what is meant when Dr Stephens states that “Only general information on the effectiveness of dedicated P-management strategies has been produced for Council (in 2010)”, with reference to the report by McDowell (2010) which is entitled The efficacy of strategies to mitigate the loss of phosphorus from pastoral land use in the catchment of Lake Rotorua[18]. Prof. McDowell is New Zealand's leading researcher on losses of phosphorus from pastoral catchments and he was tasked by the Bay of Plenty Regional Council to focus on phosphorus mitigation strategies specific to the Rotorua.

2.22 (6.4) Dr Stephens has again made grossly generalized and incorrect assertions derived from a paper[19] by my co-authors and I. He states that "For instance, since 2003 cyanobacterial biomass has declined nearly a thousand-fold and shifted to non-toxic species under stronger P-limitation". Cyanobacteria biomass was higher in 2004 than 2003, and it was also higher in 2009 than any of the preceding four years. In this paragraph Dr Stephens has also asserted that "P-limiting and co-limiting P-loads are by principle the same", but this is wrong. Similar levels of growth limitation are likely to occur under higher phosphorus loads when there is co-limitation than when there is only phosphorus limitation.

2.23 (6.5) Dr Stephens discusses how "inputs of atmospheric-N by heterocystous cyanobacteria would [then] negate reductions in anthropogenic-N", but this is conjecture and it is highly unlikely that heterocystous cyanobacteria would replace anywhere near the 320 t y^{-1} of nitrogen removed in the incoming load.

2.24 (6.6) Dr Stephens states that "It remains uncertain however, if mitigation of anthropogenic P-loads can generate equivalent or greater reductions than alum has in achieving a TLI ≤ 4.2". This is a reason why a nitrogen reduction strategy is also required, concurrently with reducing phosphorus loads.

2.25 (7) The summary provided by Dr Stephens has the same flaws identified above which underpin his evidence:

(i) Adaptive management in relation to phosphorus is fundamental to a Bay of Plenty Regional Council's nutrient management strategy for Lake Rotorua (counter to Dr Stephen's paragraph 7.1).

(ii) P-limitation (as opposed to co-limitation) has not been demonstrated by me and should not be interpreted as such by Dr Stephens (in his paragraphs 7.2a and b).

(iii) The Bay of Plenty Regional Council does not propose "A focus on N reduction alone" (Dr Stephens' paragraph 7.2c).

[18] McDowell (2010).
[19] Smith et al. (2016)

2.26 (7.3b) Dr Stephens proposes that "Prioritising P-management would carry with it lesser cost or risk to the community in achieving a TLI ≤4.2 than the focus on N-management in PC 10". He appears to ignore that a major risk will be in not meeting the TLI 4.2 target at all as a result of focus on P mitigation and not carrying out mitigation of both nitrogen and phosphorus.

2.27 (7.5) I agree with Dr Stephens on the value of regular reviews of trends in TLI (as currently carried out annually), recalibration of predictive models (but perhaps more accurately focused towards model development and updates as new data arise) and assessment of effects of P-reductions (but logically also including N based on the goals of the proposal Plan Change 10).

2.28 (7.6) The assertion by Dr Stephens of "the sustainable P load needing to be met irrespective of reductions in N loads, to result in co- or P-limitation at TLI ≤4.2" is wrong because, by definition, co-limitation implies a dependence on the N load.

2.29 (7.7) Dr Stephens has provided a confusing final statement because (i) he has interpreted that a phosphorus reduction strategy is not part of the proposed Plan Change 10 and (ii) he has confused nutrient ratios and nutrient limitation when considering that a reduction in nutrients (incorrectly referring only to N reduction) would degrade water quality.

3. REFERENCES

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