Dear Editor, referees and individuals who provided short comments

We are grateful to the four individuals who provided comments on our submission. We particularly value the opportunity provided by this forum to discuss the science related to this issue, which we believe has considerable importance to managing nationally iconic Lake Rotorua, as well as relevance to managing eutrophication effects in inland waters elsewhere.

Please see our response to each comment below.

Referee comment from V. Smith

This review states "I strongly concur with the four primary reasons identified by Abell et al. (2015)" and "I am in complete agreement with Abell et al. (2015) that the current eutrophication control strategy of managing both N and P inputs to Lake Rotorua is both desirable and completely warranted, and should without question be maintained." The reviewer cites additional research (Smith *et al.* 1987 and Paerl and Otten 2013) that supports our view that it is important to manage phosphorus inputs to the lake *in addition to* nitrogen inputs. We propose to refer to this research if we are invited to submit a revised manuscript.

In addition, the reviewer presents additional analysis of relationships between concentrations of chlorophyll *a* and nutrients (total nitrogen and total phosphorus) that supports our viewpoint. We propose to refer to this analysis if we are invited to submit a revised manuscript.

Short comment from U. Morgenstern

This comment disagrees with our viewpoint. Criticisms are presented of each of the four arguments that we make. We present our responses to each individual criticism below:

1. The short comment disagrees that Morgenstern et al. (2015) implied that "P does not have potential to limit primary productivity". We maintain that this is implied in the statement by Morgenstern et al. (2015) that the water of Lake Rotorua "has naturally high PO_4 concentrations, well above the threshold for primary algae production".

2. The short comment disagrees that Morgenstern et al. (2015) implied that "P control is redundant". We maintain that this is implied in the conclusion that "the only effective way to limit algae blooms and improve lake water quality in such environments is by limiting the nitrate load." It is also implied in the reiteration of this conclusion in the short comment, which states: "Therefore we consider our conclusion [that of Morgenstern et al. (2015)] correct - the only effective way to limit algae blooms and improve lake water quality over the long term, in an environment in which the P load entering the lake is naturally high and above the limit for primary productivity, is by limiting the nitrate load."

   Thus, if limiting nitrate loads is the only effective approach, then this implies that limiting phosphorus loads is unnecessary. We note that our interpretation of this conclusion matches that of the reviewers.

3. The short comment presents criticism of the use of "artificial in-lake P removal" in Lake Rotorua. We presume that this relates to the regional council's current policy of adding aluminium sulphate to two stream inflows to remove dissolved phosphorus from the water column. The short comment seemingly presents this criticism to counter our arguments. For example, the short comment states "artificial in-lake P removal in such environments with constantly very high natural P influxes has only a very short-term effect and is therefore not effective over the long term" (p. C5247). Most notably, the short comment presents a paragraph that opens with: "Considering the above facts, the arguments in Abell et al. (2015) to support in-lake P-removal strategies seem biased."

We find this criticism of artificial in-lake P removal in the short comment confusing and irrelevant to our arguments. We make only a single reference to such in-lake techniques in our discussion paper: this is in a footnote on p. 10383 where we incidentally note that the use of aluminium sulphate has the potential to reduce dissolved phosphorus loads to the lake, regardless of source. To clarify, the aim of our discussion paper is not to argue for greater adoption of 'artificial in-lake P removal'. Rather, we believe that long term and sustainable management of phosphorus loads to the lake through catchment-based actions such as limiting soil erosion on farmland is important to achieving lake restoration objectives. Thus, while we maintain that phosphorus control (in addition to nitrogen control) has an important role in effectively managing lake water quality, we do not present arguments to specifically "support in-lake P-removal strategies"

4. The short comment states "we agree that anthropogenic sources of P and N should be reduced wherever possible, for example through good farming practice". We are unclear how this statement is fully consistent with the argument made in Morgenstern et al. (2015) and reiterated in the third paragraph of the short comment that "the only effective way to limit algae blooms and improve lake water quality over the long term, in an environment in which the P load entering the lake is naturally high and above the limit for primary productivity, is by limiting the nitrate load." Also this statement seems partly inconsistent with the statement made later in the short comment that "P removal strategies may be required as temporary measures for lake water improvement".

   Thus, as noted by Reviewer 2, this statement does seem to make a concession to our point of view but we are unclear of how it aligns with other comments that have been made, and the overall rejection of our arguments. Is the short comment advocating that lake management strategy: 1) focus on nitrate control; 2) focus on ensuring that all possible actions are taken to reduce both nitrogen and phosphorus from anthropogenic sources, or; 3) focus on nitrate control but also take some temporary measures to reduce phosphorus loads in the short term?

5. In our Discussion Paper, we argue that a focus only on nitrate control is undesirable as it has the potential to reduce the nitrogen to phosphorus ratio (N:P), which can create conditions that promote greater relative abundance of undesirable (nitrogen fixing) cyanobacteria in the lake. In relation to this (our fourth) argument, the short comment states "the fourth reason does not contradict our conclusion". The short comment states "the N:P ratio was naturally very low and any lowering of the current N:P ratio by removing N sources will shift the N:P ratio back towards the low natural ratio of the time before algal blooms began to occur". This point is presented in support of the strategy proposed by Morgenstern *et al.* (2015) to focus on nitrate control.

   We maintain that our argument does indeed contradict the proposed strategy to focus on nitrate control. We note that our fourth argument is strongly supported by Reviewer 1 (V. Smith), and Reviewer 2 describes this argument as "the crux of the invalidation of Morgenstern et al.'s (2015) single nutrient control solution". Considerable further detail is provided in the review by V. Smith to support this and, as noted above, we propose to extend this point by referring to references cited in the review by V. Smith. Further, we note that, while the N:P ratio was likely lower under reference (i.e. 'natural') conditions, lake water phosphorus concentrations would also have been much lower. Thus, a strategy of focusing on nitrate control is expected to result in a low N:P ratio *coupled with high phosphorus concentrations*. These are precisely the conditions that are associated with a high risk of harmful algal blooms, and high total phosphorus concentrations are often a better predictor of cyanobacteria dominance than low N:P ratio (Dokulil and Teubner

2000; Downing *et al.* 2001). Accordingly, we propose to include this extension of our argument in a revised manuscript.

In relation to this, the short comment acknowledges that "P removal strategies may be required as temporary measures for lake water improvement". We disagree that phosphorus removal strategies should only be temporary measures. Instead, we maintain that phosphorus control measures should implemented alongside nitrogen control measures to achieve long term and sustainable reductions in loads of both nutrients from the catchment. This is consistent with the current strategy for managing lake water quality, as we outline in our Discussion Paper.

Referee comment from Reviewer 2

The reviewer provides an assessment of our challenge to the conclusion of Morganstern et al. (2015), concluding that "I find Abell et al.'s (2015) comments on Morgenstern et al.'s (2015) sweeping conclusion that managing nitrogen inputs is the only feasible way to effectively reduce eutrophication in the lake, both relevant and appropriate."

In providing their assessment, the reviewer recommends ways that we could strengthen our arguments or provide more clarity for readers. We propose to adopt all of the reviewer's recommendations if we are invited to submit a revised manuscript. We provide further details below about this in relation to each specific recommendation.

Abell et al.'s (2015) comment could benefit by more explicitly drawing from the ample literature on nutrient co-limitation and dynamics, as Smith (2015) has done.

We propose to provide further discussion relating to the issue of nutrient co-limitation of phytoplankton biomass by referring to references cited in the review comment by V. Smith, in addition to further references that consider this topic from an applied perspective (e.g., Paerl 2009). We propose to add this material to our third argument, which concerns increased efficiency in achieving desired water quality outcomes that would arise from adopting a dual, not single, nutrient control strategy.

Abell et al. (2015) could benefit by presenting a nutrient budget for the Lake, demonstrating the nutrient loading for the lake, including in lake processes and overland flow, to complement the groundwater and stream nutrient concentrations presented in Morgenstern et al. (2015).

We propose to present this information to support our second argument, which concerns the relative magnitude of phosphorus loads to the lake from natural and anthropogenic sources. To support this, we propose to refer to the results of recent research that we have been involved in to estimate these loads. This research combined monitoring data with estimates of aquifer mean residence times (Morgenstern et al. 2015) and baseline (reference) phosphorus concentrations for streams draining New Zealand catchments with volcanic acid geology (McDowell et al. 2011) to estimate that approximately ~30% of the dissolved reactive phosphorus load to the lake and ~50% of the total phosphorus load to Lake Rotorua is from anthropogenic sources such as farmland (Hamilton *et al.* 2015). This work helps to further support our contention that the magnitude of the anthropogenic phosphorus load to the lake is not too small to prohibit worthwhile control efforts.

Abell et al. (2015) could be strengthened through incorporating a recognition of the spatial interplay between nutrient critical source areas and groundwater flow

We agree that some discussion of local variability in nutrient pathways in the lake catchment will be a useful addition to our Comment; however, we propose to make only concise additions to address this comment as we emphasise that our Comment focussed on a strategy to manage water quality at the catchment scale. We propose to provide further material on this issue in relation to our third argument.

In doing so, we propose to refer to the results of Morgenstern et al. 2015, which we believe provides authoritative and highly valuable information on this issue.

Abell et al.'s (2015) does not address the question of whether in-lake control of phosphorus loading will contribute to a long term solution rather than a temporary fix. To strengthen this rationale, Abell et al. (2015) should address this question and cite relevant examples.

As we discuss above, our Comment focused on the optimum scientific strategy for achieving water quality objectives (i.e., which nutrients should be controlled), and we did not provide recommendations for *how* nutrients should be managed, which entails socioeconomic and political considerations. However, we recognise that some discussion of the sustainability of in-lake nutrient management would be relevant. Hence, we propose to briefly discuss this in the introductory paragraphs with reference to other studies that have examined the sustainability of in-lake nutrient control strategies for Lake Rotorua (e.g., Abell et al. 2011) and more broadly (e.g., Mackay et al. 2014).

<u>Short comment from T. Baisden</u>

This comment states that "I support all conclusions reached by Abell et al., but offer that their case would be stronger and simpler if they provide clearer background on the substantial legislative, policy and management undertaken thus far". The comment lists eight suggested improvements.

We had deliberately avoided providing a detailed background on the substantial legislative, policy and management issues related to Lake Rotorua to avoid diluting the focus of our submission. However, the comment provides a strong rationale for the recommendation to add further details on these issues. Accordingly, we propose to provide such background material in the introductory paragraphs of our Comment if we are invited to submit a revised version. As suggested, this will include greater reference to the Action Plan (BoPRC 2009) and a brief overview of the Trophic Level Index (TLI) metric (Burns et al. 1999). Of the eight suggested improvements, we propose to address the first seven. We propose to only partly address the eighth suggested improvement, which is:

Last, and perhaps most challenging is placing the opportunities for reduced N and P inputs on a timeline of progress for Lake Rotorua's TLI. The implied question, address[ed] *in the Comment's point (3) is whether P management provides an opportunity for more immediate gains than N management. This is a difficult topic for scientists because it may involve calculations that are political and economic. If it is possible to address this using scenarios, that might be desirable. In doing so, I would caution that the use of MRTs implied on P10381 L5 may be incorrect, or lead to misconceptions about the time-response of N delivery. Instead the distribution of transit times (as shown in Morgenstern et al.'s Figure 10) should be referred to, and contains a large (and possibly uncertain) component with a faster response that* [than] *the "MRT".*

We propose to adopt the recommendation to make reference to the distribution of transit times shown in Morgenstern et al. (2015) by discussing it in relation to our third argument, which relates to groundwater lag times. We do not propose to adopt the first part of the recommendation, which is to place "*the opportunities for reduced N and P inputs on a timeline of progress for Lake Rotorua's TLI*". As the comment indicates, such a timeline is subject to social and political considerations, which we cannot claim to represent. We therefore believe that a timeline of actions is better presented within the context of lake Action Plan development, which is an established process that reflects the views of multiple stakeholders. The suggestion to address this using scenarios is a valuable one; however, we consider the development and examination (modelling) of such scenarios to be far beyond the intended scope of our comment. Nonetheless, we agree that such a study would be highly valuable and note that such work is an ongoing focus of research that we are involved in with colleagues.

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