CAWTHRON

14th June 2007

Kate McArthur
Horizons Regional Council
Private Bag 11025
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Dear Kate,

Re: Review of report on "Limiting nutrients for controlling undesirable periphyton growth"

This report is a good summary of the science relating to the control of undesirable periphyton growth by limiting nutrient supplies.

I agree with all the main points in the report and am particularly supportive of the concept that both N and P should be managed to restrict downstream effects on other ecosystems, even if one nutrient is considered to be limiting in a particular area. Consideration of potential downstream effects is an important part of integrated catchment management, which I believe is the correct approach to this issue.

I particularly liked the presentation of DIN:FRP ratios in Appendix 2, which clearly shows the temporal variation in likely nutrient limitation in various streams. This information clearly demonstrates the risks of focussing on only one 'limiting' nutrient.

The discussion of the merits of various approaches for inferring nutrient limitation was also very valuable.

I have only a few comments regarding the report, which are listed below.

1) Caution should be exercised before using the simple decision rule shown on page 4 to determine if nutrient management is required in a particular section of river. I presume that this decision rule was included in the report as an example of what could be done, rather than as a serious attempt to assist decision making relating to this issue. It is currently incomplete (some branches lead to dead ends), over simplified (for example, doesn't consider turbidity) and there is no justification for why particular thresholds were chosen (e.g. <20% cover for macrophytes). The commentary on the following pages provides a much more complete assessment of situations where nutrient management may not be needed, although as mentioned above I believe that nutrient management should be considered in almost all systems.

2) The authors state that nutrient criteria may not be required for natural soft-bottomed streams. However, they do not provide any details/mechanisms to explain this assessment. My understanding is that soft sediments are generally highly mobile and so provide a poor substrate for periphyton attachment – although periphyton mats do grow over soft sediments after long periods of low flows. Additionally, soft sediment streams often carry a relatively high load of suspended sediment, which reduces the amount of light reaching the stream bed (and hence periphyton production) and abrades periphyton from any solid substrates. The combination of these factors means that problematic periphyton growths are uncommon in soft-bottomed streams, even with high nutrient concentrations. It is important to note here that nutrient enrichment of soft-bottomed streams may have effects on the biota and instream values without stimulating periphyton growth, especially if the nutrients are in an organic form.

3) On page 6 the authors state that "grazer control of periphyton is most likely in streams that lack frequent bed-disturbing flows and have good instream habitat….." I'm not aware of any information to support this statement, but note that *Potamopyrgus* snails are probably one of the most effective grazers of periphyton (Biggs & Lowe 1994 NZJMFR 28:119-134), but do not require particularly good habitat, water quality or cool water temperatures.

4) On page 7, I am uncomfortable with the statement in the last bullet point that "turbidity can effectively manage periphyton growth". There is plenty of evidence suggesting that turbidity can control periphyton growth, but the way this sentence is worded suggests that turbidity could be used as a management tool. Given the range of negative effects of increasing suspended sediment loads I think it needs to be clear that this wouldn't be considered.

5) On page 8, the turbidities and depths at which periphyton can be seen is the reverse of what it should be. The correct relationships between these two variables is discussed in the first paragraph of page 6.

6) Biggs & Close (1989) is referred to in the report, but does not appear in the reference list. The citation for this paper is Biggs, B.J.F., Close, M.E. (1989) Periphyton biomass dynamics in gravel bed rivers: the relative effects of flows and nutrients. Freshwater Biology, 22, 209-231. Similarly, Bothwell (1989) does not appear in the reference list, but is referred to in the report. The correct citation for this paper is probably Bothwell, M.L. (1989) Phosphorus-limited growth dynamics of lotic periphytic diatom communities: Areal biomass and cellular growth rate responses. Canadian Journal of Fisheries and Aquatic Sciences, 46, 1293-1301.

7) On page 15 of the report the authors note the lack of studies that have demonstrated reductions in periphyton biomass or growth rates as a result of reductions in point source inputs of nutrients. Two recent long-term studies of algal growth rates and ecosystem respiration rates report effects of reductions in nutrient inputs as a result of improvements to waste water treatment facilities. These studies support the concept that reducing nutrient supplies will have positive effects on periphyton blooms. Relevant citations are: Uehlinger, U. 2006. Annual cycle and interannual variability of gross primary production and ecosystem respiration in a floodprone river during a 15-year period. Freshwater Biology 51: 938-950; Izigirre, O 2007. Temporal variability of whole stream metabolism at two contrasting Basque streams during 10 years. Chapter 7, PhD thesis, University of the Basque Country.

8) On page 18 it is stated that flood transported nutrient loads are less important than those at base flow. However, the 5th bullet point in the Executive Summary states that year-round control of both N and P is important. I consider that control of year-round nutrient loads is important when considering the downstream effects, especially in lakes. However, the need to invoke controls during high flows in systems discharging to well-mixed coastal areas is debateable and difficult to achieve. As the authors of the report point out on page 18, "the influence of flood flows on base-flow nutrient supply is not well understood."

9) On page 23, the authors make the comment that a rigorous demonstration of N limitation has not been achieved in marine water and refer to a paper published in 1988 (Heckey & Kilham 1988). This statement is somewhat misleading. My understanding is that there is a substantial amount of scientific literature on nutrient limitation in the marine environment and N is often considered to be limiting. Perhaps the authors are referring to open-ocean situations where trace elements like iron are considered to be limiting in some situations.

10) On page 23 the authors note that the effects of nutrient loads into the marine environment from rivers in the Horizons Region will be confined to estuaries and poorly mixed embayments. The high energy of the coastal waters around most of the Horizons region will ensure rapid mixing of freshwater with coastal water. I agree with this conclusion, but note that prevention of toxic algal blooms in the coastal environment should be a target, as well as prevention of macroalgal blooms.

Thanks for giving me the opportunity to review this report. I hope these comments are useful.

Yours faithfully,

Roger Young, PhD
Freshwater Ecologist