Improving Tools to Link Nutrients to Adverse Effects on Stream Ecosystem Services in California

Peer Review Charge Questions

Background Information:

The California State Water Resources Control Board (SWRCB) is developing nutrient water quality objectives for the State's surface waters. Among the approaches that the SWRCB staff is considering is the cause-effect approach, known as the Nutrient Numeric Endpoint (NNE) framework (Tetra Tech 2006). As the California SWRCB prepares to propose nutrient objectives for wadeable streams, improved data from statewide stream probabilistic and targeted bioassessment surveys can strengthen the scientific basis for policy decisions by 1) using the empirical stress-response approach to characterize the relationship and quantifying thresholds at which eutrophication stressors (e.g., nutrient concentrations, algal abundance) begin to exert adverse effects on aquatic life use, 2) provide context for these thresholds by summarizing available data on reference and ambient concentrations of candidate nutrient and algal abundance indicators and 3) evaluate the existing BBST model performance in predicting algal abundance, given variation in the nutrient status and influence of other environmental factors (e.g., hydrology, light, etc.).

The objectives of the project were three-fold:

1. Estimate the natural background and ambient concentrations of nutrients and candidate indicators of primary producer abundance in California wadeable streams;

2. Explore relationships and identify thresholds of adverse effects of nutrient concentrations and primary producer abundance on aquatic life use indicators in California wadeable streams;

3. Evaluate the Benthic Biomass Spreadsheet Tool (BBST) for California wadeable streams using existing data sets, and recommend avenues for refinement.

This study found that the majority of the State's Wadeable Streams sampled are below the 75th percentile of minimally disturbed "reference sites." Statistically detectable thresholds were found for benthic chlorophyll *a*, AFDM, and nutrients; benthic chlorophyll *a* thresholds were below those described in TetraTech (2006). The validation exercise of the BBST indicates room for improvement; inclusion of landscape and site-scale factors provides an avenue for model refinement.

The objective of this peer review is to obtain expert feedback on the soundness of the methodology, as well as the soundness of the conclusions drawn from the data and analyses.

Charge Questions:

In your review of this document, please provide written responses to the best of your ability to the following questions. Additional comments and recommendations for improving this document and associated methodology are also welcome.

Is the report organization optimal for a document of this length? Would it be better if each of the report sections was a stand-alone piece?

Yes. The report is very well organized into a logical framework and gives a good platform for the final recommendations. I do not recommend separating it into stand alone pieces.

Is the cited literature sufficiently comprehensive? Are there any key references that have been omitted?

There are two areas, and associated literature, that don't appear to have been adequately canvassed for such an analysis:

1. The form of nutrients being measured and associated scale of the processes (time and space variability) that result in the end-point concentrations being measured in the streams. How do nutrient concentrations vary during the algal 'growing period' (eg. following scouring)? What is the relevance of 'point in time' samples? What is the best integrative measure of nutrient concentration (or flux) that the algae have access to? What does a total nutrient measure represent vs. the soluble fraction. These questions are fundamental to developing nutrient management objectives and controlling stream eutrophication at the watershed scale.
2. Appropriate ways of classifying the stream environments that are relevant to the scale of the individual sampling sites and better reflect local upstream conditions and the hydrological regime (eg, a hierarchical controlling factors approach). Sites could then be partitioned accordingly (at a less coarse scale than done in the present analysis) to help understand the variability in algal biomass and environmental drivers of this. Ecoregions are too broad.

There are many published studies on these two issues.

Are any limitations of the datasets and monitoring approaches employed to support the analyses in this report adequately addressed?

The datasets and BBST models have significant limitations. The authors generally recognize these and explain them well. However, there is little they can do to address these problems which I believe suggests that the results of this study have limited application. See further comments below.

Executive Summary

Does the executive summary adequately capture the major findings of the report?

Yes. A good summary.

Are the summary statements adequately supported in the body of the report?

Yes.

Chapter 1

Does Chapter 1 provide sufficient background information to put the rest of the report into context with respect to information needs for the state of California?

Adequate. However, while it is good to start with a wider international context I was expecting to see a good summary of water use/ecological issues caused by nutrient enrichment and associated algal growth in streams of California. I would have thought that demonstrating a clear State-wide problem, and then a commentary on where the most significant detrimental effects occur (and what these are), would be fundamental to justifying further work on this topic. Maybe this is well documented elsewhere and could be covered by another citation or two???

Chapter 2

Are the methods used to estimate reference and ambient values for stream eutrophication indicators pooling data across multiple monitoring programs scientifically valid?

It is difficult to answer this question as not enough detail is given in the description of the different programs and their methods. Major differences in data can occur amongst different operators using the same methodologies, let alone amongst programs using variations or quite different methods and habitats (eg, runs vs. riffles) for stream benthic algal sampling/monitoring. There needs to be confidence that the choice of the sampling reaches, then the choice of the sampling points, were consistent among the operators and programs (eg, how does the methodology deal with macro-algal patchiness?). What was the overarching QA system and statistics to ensure comparability of data and therefore that the datasets can be pooled? Unless inter-operator variability can be understood and factored out, then I suggest that there will be significant variance in the dataset that is not driven by environmental variation.

A further issue that is correctly identified in the Discussion (section 2.4) is the limitation of using one-off sampling. Major temporal variability in algal abundance in streams is well documented in the literature, as well as asynchronous seasonality amongst stream types. Biomass can vary by orders of magnitude over periods of several months. A minimum of monthly sampling for at least a year is required to get a reasonable estimate of maximum and mean biomass for any given site, and thus a true indication of the biological response to watershed scale nutrient stressors. Nutrient leaching and transport processes, as an output of landscape processes, varies over weekly to seasonal to annual time scales so it is important to characterize the biological response over

similar periods to understand the true nature of the instream effects. The algal – BMI relationships can occur over different time scales as well, which point sampling will not correctly typify (ie, 'duration' of any given high algal biomass event is very important in determining BMI effects).

As a result of the two major points above, I question the ability of these datasets and analyses to provide authoritative guidance on wadeable stream algae and nutrient criteria, and fulfil the Objectives of the study. Given the huge potential impact on economic uses of the streams, and of the needs of the instream communities if insufficient protection is given, I suggest that a great deal of caution should be used in applying results from analysis of these pooled datasets. A study that is specifically designed to meet the objectives of developing criteria is really required here.

Chapter 3

Have the different methods for evaluating response thresholds of primary producer biomass and nutrient effects been described adequately so that someone previously unfamiliar with these methods can understand the approach and the method strengths and weaknesses and interpret the results?

Yes. The associated citations give more detail, which is appropriate.

Are the methods used to estimate response thresholds scientifically valid?

Probably. I don't have a working knowledge of all the methods so I can't comment on this with confidence. However, I do note that while many of these methods appear 'analytically elegant', the most useful and meaningful analyses were the simple bivariate 'dose – response' plots (eg, Figures 3.14 – 3.17). Use of integrator indices is questionable in the current context due to the number of assumptions and the need to show clear cause – effect stressor relationships to water managers and the public. This type of work needs to focus on such approaches and then become more sophisticated analytically at a later stage, but only if this is required to clarify relationships and process understandings. We should always try to keep it simple when developing recommendations for regulators.

I have a query about the use of TN and TP. How much of these measures are just re-suspended benthic algal cells?? I suspect that there is a significant degree of auto-correlation inherent in these ALU vs total nutrient results.

Have statistical assumptions been adequately tested?

They appear to have been, or the tests are robust to non-conformities (eg, to requirement for normal distributions)

Are the conclusions of this chapter adequately supported by the analyses and results?

Yes. The conclusions are appropriately very cautious.

Does this chapter do a good job of synthesizing the results of multiple analyses contributing to a weight-of-evidence approach that could be used to support numeric nutrient endpoint development?

Yes. However, I have significant concerns about the dataset, as described earlier. I suggest that the relationships (eg, thresholds) for algal biomass and ALU condition metrics are likely to be more robust than the nutrient – algal biomass relationships due to the stronger influence of temporal factors on both nutrients and algal biomass.

Can you suggest any improvements?

The dataset should probably have been partitioned to remove obvious non-relevant sites (eg, those from urban areas where local issues occur which are specific to those sites such as sediments, toxic pollutants and concrete lining of channels).

Chapter 4

Is the evolution of the NNE benthic biomass spreadsheet tool adequately explained to allow the reader to understand its use and potential strengths and weaknesses?

Yes.

Are the methods used to evaluate the performance of the NNE benthic biomass spreadsheet tool scientifically valid? Have statistical assumptions been adequately tested?

There are many ways that this analysis could be done. The approach does appear to be valid.

Are the conclusions of this chapter adequately supported by the analyses and results?

Yes.

Does the analysis of residuals for model predictions presented in this chapter help to guide future improvements in these models?

This could be strengthened by, for example, a better portioning of the dataset to allow for the different environmental domains the sites came from.

One issue that has not been canvassed very well is the strength and validity of the original BBST models used for this analysis. For example, the datasets used for the Dodds models encompass point values (incl. seasonal maxima) through to mean monthly values from year-long sampling campaigns. The sampling protocols are all completely different in his datasets and so are the analytical methods (eg, different extraction methods can result in 2 – 3 fold differences in chlorophyll concentrations on the same samples!).

Given the issues with the Californian data (as appropriately recognized by the authors) and the problems with the BBST models, I suggest that it would not be prudent to attempt to develop nutrient criteria or objectives from the current study. A purpose-designed sampling program that includes a strong temporal component and a full range of water quality and other environmental parameters (particularly flow), is required to develop the understandings and models necessary to achieve the desired management objectives. I suggest that this should be a high priority for the State.

Please provide your written comments to me no later than **four weeks from contract initiation.** Comments may be sent by regular mail to the address below, or by email to houk.virginia@epa.gov.

If you have any questions concerning the draft report or the charge, please contact me at 919.541.2815 or houk.virginia@epa.gov. We sincerely thank you for your input to our peer review process.

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