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

Peer Review Charge Questions

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.

Please note: responses to questions are italicized

Charge Questions:

This is generally a well-prepared document. There has been considerable thought that has gone into the approaches given here, and generally the approaches that have been taken are good and defensible. A major issue that cannot be solved is the inherently variable nature of periphyton and the dynamic nature of multiple factors that control biomass. Some of the work is a bit difficult to follow, but these are complex analyses and models, and mostly the authors have done as well as could be expected in explaining the approaches.

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?

The report organization is fine, making them free-standing pieces would just require repetition of much information. It might be helpful to break the 3rd chapter into two, as it is pretty long and complex.

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

Generally the literature has been well represented. There are a few that would have helped, I have noted suggestions for additions and listed them at the bottom.

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

The authors are very careful to note the limitations of their data sets and how they analyzed them. Using multiple approaches is a great way to strengthen inference and the authors are to be commended for their comprehensive approach.

Executive Summary

Does the executive summary adequately capture the major findings of the report? Are the summary statements adequately supported in the body of the report?

The executive summary does adequately support the body of the report. The authors may wish to alter it a little based on my comments in the document.

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?

The background is a little thin in this chapter, but most of the required background is given in the introductions to the following chapters, so this is not really a very important point. As stated later, I would prefer a broader view of eutrophication based on potential shifts of heterotrophic state as well as autotrophic state with respect to biotic integrity and water quality. Some have referred to this as green versus brown food webs. This would set up much of the work that follows in a way that is closer to a more holistic view of eutrophication in streams. The first approaches were mainly based on the example of lake eutrophication management, planktonic algae and nutrients control being of main concern. Biotic integrity is an endpoint that can be altered through heterotrophic (allochthonous) food web pathways. The heterotrophic state may also tie to other water quality issues such as dissolved oxygen levels.

Chapter 2

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

The data in table 2.2 suggest that feedlots were not considered distinct from agriculture, that urbanization was considered equivalent to agriculture, and provides no real justification for the thresholds.

The authors may want to consider other methods for assessing reference. Smith et al (2003) have published reference distributions for the United States. Dodds and Oakes have published a method for backing out all human influence. Such an approach might be useful in the Central Valley were reference sites are few and far between.

It is important to set the upper 75% correctly and be certain that the values for reference indicate true reference. For example atmospheric nitrogen deposition can be considerable in California (e.g., Bytnerowicz and Fenn 1996). Smith et al. (2003) clearly indicate that this is an important and substantial correction to reference nutrients in many parts of the United States. Also, lumping all agriculture might be problematic. A proportional correction from the Smith et al (2003) article could be applied to the current numbers.

The expectation is that 25% of the sites will exceed 75%, so maybe the document should report the percentages over that in table 2.8. The executive summary stresses that 50% of all streams fall below the 75% reference level, which sounds ok phrased that way, but another way of looking at it is that there has been a doubling in the number of high nutrient streams.

I applaud the caveat that more detailed seasonal sampling could be important. Are the authors certain that an analysis of variance, or analysis of covariance that factors out intensity of human impacts cannot dissect out ecoregional differences? Data presented in chapter 4 sort of get to this, but not explicitly.

Chapter 3

Overall, I am very impressed by this chapter. I think that the authors should be encouraged to submit a streamlined version of this chapter in the peer-reviewed literature. In particular the application of multiple methods to determine thresholds on multiple indicators is really great, while it is not completely innovative, it really represents the state of the environmental science and pulls together a number of disparate approaches. I enjoyed this chapter.

It might be important in the introduction to state that increased nutrients can also lower biodiversity of indicator organisms and depress biological integrity. This is not necessarily related to algal biomass, as enriched detrital pools may have altered food value (work by Wang et al (2007) and by Evans-White et al, 2009), again the point of a more holistic view of stream eutrophication. It gets mentioned eventually, but more up front would be better.

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?

This is a fairly complex chapter and someone that had a basic training in science and statistics could probably get it, but it the chapter is not for the lay person. A section that defined key terms might be useful (say an inset box in the introduction that defines terms such as ALU, BMI, and WQO in addition to the table of acronyms at the beginning). I cannot see how one would write this chapter so someone completely unfamiliar with the methods would get it, but the authors have done as good of a job as could be expected.

Section 3.2.2 does not demonstrate an understanding that nutrient enrichment can alter heterotrophic state, and nutrient effects in streams are not always linked directly to increases in algal production. Thinking in terms of autotrophic and heterotrophic state can clarify this issue (e.g., Dodds 2007). The document eventually makes this point (page 80) but up to then the effort seems to almost entirely assume the nutrient-algae-invert link as cause and effect.

Additional types of threshold other than the example diagrams can exist that indicate the areas where systems move out of a manageable state (Dodds et al. 2010). An important one in this case is the *level of nutrients above which chlorophyll no longer increases as a function of nutrients, and this represents a level of nutrients above which nutrient control is pointless unless the nutrient can be brought way below those levels. This threshold plays into the models of Chapter 4, but does not pop up till there.*

It is good that this document describes 1) multiple response variables to eutrophication and 2) multiple methods to determine thresholds. Both of these approaches, while perhaps adding some increased uncertainty over specific numbers, avoid reliance on a single approach when each approach can give different results because they are applied to naturally variable environmental data. They will give the regulated community ammunition to criticize, but they represent the scientific uncertainty in a broader sense. Individual methods applied separately can give relatively narrow confidence bands for threshold points, but the approach taken here is more honest.

Table 3.4 gives a really nice summary as does the following text.

The use of the arcsine transformation is falling out of favor for some (Warton and Hui 2010).

Table 3.7 and throughout the document PO4 should be soluble reactive P (or DRP) since the assays don't exactly give phosphate.

1st Paragraph at 3.3.4. This is a very excellent point that is often misunderstood by managers.

Table 3.11 could include values from Dodds et al. 2006.

Are the methods used to estimate response thresholds scientifically valid? Have statistical assumptions been adequately tested?

Yes, the application of multiple methods makes this a very strong analysis

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

Yes it does.

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? Can you suggest any improvements?

Some of the logical flow could be improved, as there were places (noted above) where the authors eventually got to an important point, but if they set things up better in the beginning, then they results and their discussion would follow better. However, the specific syntheses of analyses is good.

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?

There were some confusing bits in there, as noted below. I wonder if there is some clearer way to explain this? Maybe a flow chart or something instead of the screen shots? It really took me a while to figure this out and there were some parts I am still not clear on.

In general I think that ways to smooth very variable data (e.g. seasonal means) may remove much of the variance and lack of predictability in the models. Even the nice total nutrient vs chlorophyll plots for lakes are based on summer mean epilimnetic vales. Another approach would be a probability of exceeding some set value.

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

Note the Dodds et al. 2006 paper presents corrected values for the 2002 paper.

I am curious why the Dodds model screen shots have canopy and light components as those are not presented in the original equations? How was the Biggs model included when it is based on inorganic nutrients but the Dodds model based on total nutrients? OK I see the later explanations, maybe the screen shots would be better after the detailed description?

How do nodes in figure 4.13 correspond to USEPA proposed nutrient ecoregions, by guess is that there is something here to start delineating ecoregions even though earlier it was stated there are not enough data to do so.

Could the heavy influence of ammonium in the models be related to the ion indicating anoxic conditions? It seems likely to be an indirect indicator, even if it is a nutrient source. However, ammonium is the most available inorganic nutrient.

Page 147. "Inclusion of explanatory variables that are integrative over time and space" is a key point as well as the idea that periphyton biomass is intrinsically variable. This is exactly why the seasonal means used by Dodds et al. yielded better correlations with nutrients than the spottier USG measurements. A central point in all this is that there is lots of natural variance, and control of nutrients lowers the probability that algal biomass will exceed a certain amount.

Have the authors considered time-lagged models for P? This is what Lohman and Priscu (1992). found for the Clark Fork in Montana where luxury consumption of P in spring alleviated P limitation in the summer.

Page 149. How do you know that filamentous algae are responsible for higher biomass accrual? They are more obvious to the eye, but often the chlorophyll is the same for them and for diatoms.

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

Yes, the conclusions are accurately supported. They authors of this document are careful to point out the limitations of the modeling approaches employed here, and there application to management.

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

Yes, it should help. This is actually sort of related to another approach that has been taken for some statistical models using ANCOVA to separate out categories such as nutrient ecoregions, and improve models within regions or allow combinations of some regions to increase the amount of data. One of the key refinements and issues that has come up all along in these suggestions for nutrient criteria *is how long of an average do you need, and how should you gauge what is acceptable deviation from the target nutrient amounts. Unfortunately, the natural variance of streams may preclude models with a high degree of predictive ability.*

Bytnerowicz, A. and M. E. Fenn. 1996. Nitrogen deposition in California forests: a review. Environmental Pollution **92**:127-146.

Dodds, W. K. 2007. Trophic state, eutrophication and nutrient criteria in streams. Trends in Ecology & Evolution **22**:669-676.

Dodds, W. K. and R. M. Oakes. 2004. A technique for establishing reference nutrient concentrations across watersheds affected by humans. Limnology and Oceanography Methods **2**:333-341.

Dodds, W. K., V. H. Smith and K. Lohman. 2006. Nitrogen and phosphorus relationships to benthic algal biomass in temperate streams (Vol 59, pg 865, 2002). Canadian Journal of Fisheries and Aquatic Sciences **63**:1190-1191.

Lohman, K. and J. C. Priscu. 1992. Physiological indicators of nutrient deficiency in Cladophora (Chlorophyta) in the Clark Fork of the Columbia River, Montana. J. Phycol. 28:443-448.

Smith, R. A., R. B. Alexander, and G. E. Schwarz. 2003. Natural background concentrations of nutrients in streams and rivers of the conterminous United States. Environmental Science and Technology **37**:2039-3047.

Warton, D. I. and F. K. C. Hui. 2010. The arcsine is asinine: the analysis of proportions in ecology. Ecology **92**:3-10.