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November 14, 2005

Ms Lauren R. Clyde
Sanitary Engineering Associate
California Regional Water Quality Control Board
North Coast Region
5550 Skylane Boulevard, Suite A
Santa Rosa, CA 95403

Dear Ms Clyde:

It is my pleasure to respond to your invitation of October 12, 2005 to review the scientific basis of the Shasta River TMDL Action Plan and Staff Report. I understand that it is my responsibility under Health and Safety Code Section 57004 to provide my opinion about "whether the scientific portion of the proposed rule is based upon sound scientific knowledge, methods, and practices." The rule in this case is the Action Plan for the Shasta River Temperature and Dissolved Oxygen Total Maximum Daily Loads. I have read all of the material supplied to me. This letter briefly summarizes my findings for the specific questions posed. More detailed comments, suggestions, and edits are appended.

In my view, the methods used by the Board and its consultants to demonstrate the linkage between water quality conditions, stream habitat, and impacts to beneficial uses are based on sound scientific knowledge, methods, and practices. The excellent literature reviews clearly show the thermal and dissolved oxygen conditions that should prevail if the beneficial use of year-round salmonid fish habitat is to be sustained and improved. Appropriate field sampling of temperature, dissolved oxygen and factors affecting them has been carried out to document recent water quality in the Shasta River and major tributaries, although it could have been improved by further sampling of the inflows, including irrigation return water. Use of thermal infrared imagery has provided a perspective on thermal conditions along the river not obtainable otherwise. State-of-the-art modeling (with generally appropriate calibration and validation; but see comments about including all relevant factors) has been used to synthesize the data and provide a dynamic view of likely causes and effects and a way to test alternative influences and mitigating actions. The overall analytical structure of the project is scientifically sound. (Question 1)

Aquatic plant productivity is convincingly linked to DO and its extremely wide daily fluctuations. The effect is both direct and through nutrient-rich sediment that fosters macrophyte growth and sediment oxygen demand, largely caused by accumulated aquatic plant debris. There seems to be some confusion in the text about whether this is a sediment TMDL rather than a DO TMDL (see footer on draft amendment and first key point of Chapter 8). (Question 2.a)

The RMS models are generally appropriate for the task at hand and seem sufficiently calibrated and validated for the Shasta River conditions. I am acquainted with the TVA's RMS models (others have used them in our lab and I know the developers and their applications of the models). I also am generally familiar with some alternative models. I have some concern that factors like hyporheic flows in the upper reaches, which show cooling in the TIR data, may not have been adequately incorporated. Remaining factors like shade may have been overemphasized in the thermal model as a result. I was also surprised that reaeration was not effectively incorporated in defining remedial actions for DO (reaeration was the main factor manipulated in TVA's use of the models, resulting in construction of reaeration weirs). (Question 2.b)

The analyses convincingly demonstrate that carbonaceous and nitrogenous oxygen demand load coupled with nutrient-stimulated aquatic plant photosynthesis and respiration rates are dominant causes of the DO problems. Reducing these loads and rates will, in the long term, likely meet the DO objectives. I am not convinced that this long-term action (probably decades) will be sufficient for your objectives. Enhanced reaeration may be necessary to maintain adequate night-time DO, as in TVA's system, and rid the daytime of excess oxygen. These load reductions will not reduce river temperature, however. (Question 2.c)

The DO source and linkage analysis sufficiently establishes a link between channel substrate conditions (silty, organic-rich sediment that is thick behind flash dams) and the establishment and proliferation of aquatic plants. I was surprised, however, that this linkage was not capitalized upon more for corrective actions. I would suggest more active and periodic flushing with managed flows to scour fine sediments on which aquatic plants thrive. The flash dams seem particularly well linked to plant-enhancing substrates, and finding alternatives to these dams would seem advantageous (e.g., through pumping irrigation water from the gravel-bed aquifer instead of from shallow impoundments). Such actions seem needed in the implementation chapter. (Question 2.d)

The expression of DO load allocations under the compliance scenario as total daily oxygen demand seems to have been based on sound scientific knowledge, methods and practices with the exception of a realistic expectation for the timing of compliance. As noted above, simply selecting modeled inputs that would make the system comply is quite different from expecting these inputs to change in any reasonably short time frame. The goals of reducing nutrients and oxygen demanding loads from plant detritus and irrigation returns, as well as increasing shade from mature trees to reduce temperatures, are very long-term. A companion compliance action scenario for the shorter term may be needed if the goals are to be realized in our lifetimes. (Question 2.e)

The analyses demonstrate, and the photographs visualize, that the high degree of solar radiation transmittance is a major factor in causing warm stream temperatures in summer. I suspect, however, that this factor has been somewhat overused as a surrogate for other influences that are not well incorporated in measurements and the models. Because shade development is such a long-term remediation, additional focus on factors with nearer-term implications may be useful. Hyporheic flow seems to be operating in parts of the river to cool or ameliorate heating, yet it is not included in the model (there is some distributed inflow, but I did not see the corresponding distributed outflow, although I may have missed it). The heat input from irrigation returns was not well characterized, as the studies noted. I know from personal experience that when it is difficult to obtain information on many thermal factors, it is relatively easy to tinker with the shade component to calibrate and validate the temperature model. All-in-all, it seems unsatisfactory to do all the work that the studies represent only to conclude that the preferred remedial action is to plant trees that will take 50 years to have the desired effect. (Question 2.f)

I am not an expert in doing solar transmittance measurements and projections, but the information presented seems logical and may represent sound scientific practice. I gathered from comments about access that some landowners may not be cooperative when it comes to planting trees or fencing riparian areas from cattle. As noted above, development of shade is a long-term proposition, and the thermal TMDL won't be met for quite some time if this is the main remedial action. (Question 2.g)

In my detailed comments, I have highlighted some fairly major issues for you to consider. These include (in order of my notes on the chapters): fish passage issues at Dwinnel Dam (and perhaps at flash dams) in the demise or reduction in salmon populations, whether the salmonids would normally occupy the mainstem Shasta River in summer based on life-cycle strategy and behavioral preferences, claiming thermal exceedences when life functions are not occurring (e.g., incubation in summer), lack of consideration of percent saturation in discussions of DO (high values can lead to gas bubble disease), seasonal nutrient releases from Lake Shastina, possible methane releases from Lake Shastina in summer (adding to downstream oxygen demand), hyporheic (subsurface) flow affecting temperatures, temperature influences of Lake Shastina discharges (current and potential management opportunities), the need for more scouring flows to remove fine sediments and lower SOD, better quantitative characterization of irrigation return flows and use of them for TMDL actions, more study of and possible reduction in number of flash dams, doubts about the N-15 evidence, possible exaggerated influence of shade when bottom effects (e.g., hyorheic flows) are turned off in the model, flow effects seem to be modeled strangely, not considering reaeration in setting the actions.

I noted that in several places the TMDLs were referred to as "sediment and temperature" rather than DO and temperature. The text and appendix material makes a good case that nutrient-rich sediment is a major factor for both habitat for macrophytes and nutrients to make them grow (and cause DO problems), but the focus should still be on a DO TMDL along with temperature.

In summary, I found the analytical approach sound and quite thorough, and the analyses to be of generally high quality. I had questions and suggestions that you may want to consider. I was somewhat disappointed with the bottom line for temperature for it included mostly action to increase shade while just assuming that warm inputs can be eliminated by edict, which seems impractical. Relying on shade will be a very long-term remediation, one that the salmonid populations may not live to see. For DO, I agree with focus on nutrient-rich sediments (both input and accumulation) and their stimulatory effects on macrophytes, but suggest that there may be other useful control measures such as managed flushing flows and finding alternatives to the flash dams for irrigation water supply. I surely concur with the need for monitoring and periodic revisiting of the issues by the Board.

Thank you for the opportunity to review your extensive work.

Sincerely,

Charles C. Coutant, Ph.D.
Aquatic Ecologist

Cc: Matt St. John

Via e-mail and hard copy
Attachment: Detailed Peer Review Comments

Detailed Peer Review Comments on Shasta River TMDL
C. C. Coutant

Note that major issues are highlighted in **bold**.

CHAPTER 1: INTRODUCTION

Page 2, next to last paragraph: Might note which EPA Region is responsible.

Page 3. 1.3.1: The coordination among subbasins is good. Is it the intent to make the respective TMDLs somewhat similar?

Page 4, 1.3.4: The Technical Advisory Group was a good idea for early information and buy-in.

Page 4, 1.4.1: According to my map, the Siskiyou Range is more northwest of the Shasta basin. What is the red outline on the inset in Figure 1.1 (This needs to be identified, as it could be confused with the Shasta basin, which is the green. I found later that this must be the outline of the Klamath River basin in California, but it should be identified.). I found that going to my road atlas gave me a better picture of the topographic setting of the Shasta basin than does the inset, for it included the mountain ranges and colored topographic information. You might consider using such a map. The ranges and mountains could be labeled (see note for 1.4.4).

Page 4, 1.4.2: Gazelle and Edgewood communities are not located on Figure 1.1. As a general editorial rule, any place mentioned in the text ought to be identified on a map.

Page 6, 1.4.4 and 1.4.5: Mt. Shasta and Mt. Eddy are noted many times in the text but not labeled on figures. It would be good to do so.

Page 9, 3d paragraph: Many creeks named in text are not labeled on the figure.

Page 9, 4th paragraph: The MWCD canal seems to be shown on Figure 1.4 but is not labeled. Other canals seem to be shown, too, but not labeled. It would help for comparisons of river and canal flows if they were given in the same units.

Page 18, Table 1.2: Good survey, but no river mile labels are shown on maps to go along with the river miles in this table. Although one can get a general scale from the bottom paragraph of page 16, a separate map with river mile designations would help.

Page 20, 3d paragraph: Big Springs Lake is in text but not labeled on figures.

Page 20, 4th paragraph: Note use of cfs here whereas the earlier figures were acre-feet. Common units (or easy conversions) would help. Change who to whom in last line.

Page 23, 4th paragraph: Have to note that flood irrigation is a wonderful heating mechanism for return flows. Hope that this is covered in rest of document. (I was subsequently disappointed that it wasn't really covered well, and that the return flows were usually not measured for temperature or other important variables.)

Page 23, Table 1.4: What does "acres per 1000" mean under Irrigated Crop Area? Do you mean thousands of acres?

Page 25, top paragraph: Spring Chinook salmon likely migrated upstream past the present Dwinnell Dam in cool, spring conditions to cool summer refuges in the mountains (getting to the cold mountain streams to oversummer is what allows a spring run). Considering the season of spring Chinook migration (river still cool) one suspects that **blockage by the dam was more important than temperatures from the reservoir for the demise of spring Chinook. If so, then fish passage should be an issue, too.**

Blockage is mentioned on page 29. The blockage issue detracts from the justification for a thermal TMDL for the mainstem.

Page 27, Figure 1.13: Fall Chinook do not normally rear over the summer elsewhere. They have a typical strategy of migrating out of the rivers in their first spring, as underyearlings. Many of us salmon biologists consider this life-history strategy to be an adaptation to avoiding normally warm summer water temperatures (see my 1999 report for EPA Region 10 on Perspectives on Temperature in Pacific Northwest Fresh Waters). Is there evidence to back up this figure showing year-around rearing of fall Chinook in the Shasta? The basinwide figure may be misleading with respect to the mainstem TMDL. None of these species would be expected to be found in the mainstem in summer. They typically move out of the mainstem to the ocean (fall Chinook) or up into cooler tributaries, like the steelhead seasonal movements you show in Figure 1,12. **This could be a big factor in justifying your temperature TMDL, which is focused on the low-flow summer months.**

Page 27, bottom paragraph: “brown bull” should be brown bullhead; “blue gill” and “mosquito fish” should each be one word (bluegill, mosquitofish). Also in Figure 1.14.

Page 29, Table 1.5: Note that the species name for Tui chub needs to be italicized.

Page 29, first paragraph under table: Rainbow should not be capitalized, nor should Largemouth and Brown in the next paragraph.

Page 30, line 2: underway is usually one word.

Page 31, 3d line from bottom: restore should be restored.

Pages 30-31. These summaries are excellent and show that the local folks are concerned and active in environmental restoration. Kudos to them! But why do the study reports in the appendices note that the study teams often could not obtain access? Physically no way or landowner objections?

CHAPTER 2: PROBLEM STATEMENT

Page 1, 2nd paragraph: gestation should be incubation.

Page 2, Table 2.1: Why is Lake Shastina not used for sport fishing? Surely, there is spawning, reproduction, and/or early development of fish and other aquatic life in Lake Shastina. Do you refer only to salmonids? If so, this should be stated.

Page 4, 2nd paragraph: **This paragraph sets up the regulation of irrigation return water as part of the TMDL for both temperature and DO. It seems inconsistent with later decisions not to include them as point sources.** The monitoring and modeling studies noted the deficiency in getting data from them.

Page 5, top paragraph: I know this is intended to be general, but it is a bit too general. Temperature is not always a stress (although too warm or too cold temperatures can be). There are always temperatures, so we can’t do without them. The intent seems to be to comment on too-warm temperatures for salmonids.

Page 5, line 7-8: This sentence doesn’t sound right. Better to say: A MWAT can be selected that allows for optimum growth rate of salmonids during peak temperatures in summer (Armour 1991). However, this may not be desirable, because, by definition, it means that water is so cool the rest of the year that growth rates are sub-optimal.

Page 5, line 8-9: It may be common, but the instantaneous maximum temperature is never a good measure of acute effects, unless it is extraordinarily high. A temperature of 90F (32C) is clearly acutely lethal to salmonids (death would occur very quickly), but an instantaneous temperature of 77F (25C) is not acutely lethal unless there are days of exposure. There are many good references to the time-temperature relationships of salmonids (e.g., Brett's publications) that are cited in the thermal literature review given in eh appendices.

Page 5, second paragraph as a whole: This paragraph is ok if it is simply describing the various measures available, without value judgments. All have recognized drawbacks.

Page 5, bottom paragraph (extending to top of page 6): I hope they measure presence/absence of the salmonids, too. **This seems to be a key question—whether the juvenile salmonids would actually be in the mainstem in summer**. See note above.

Page 6, Table 2.3: Despite the references, I think 16C for core juvenile rearing is low unless one is intending to maintain optimum conditions all the time (probably unreasonable). The optimum growth temperature for Chinook is above this according to research by Brett. It is still a useful goal.

Page 6, Table 2.3 footnote 2: defines should be defined.

Page 6, Table 2.4: This table probably should be footnoted to say that the lethal threshold is for a long-time exposure (usually taken as a week). It's not a sharp cutoff. These temperatures can be experienced briefly and fish survive and do well. That's why instantaneous temperature is a poor measure of acute lethality. Nonetheless, these are good benchmarks for lethal conditions.

Page 7, Table 2.5: This table needs a better legend to indicate it is about *maximum* temperatures in three measures, peak temperature ("temp."), weekly average ("WAT"), and maximum weekly ("WMT") (assuming I'm correct). The last three columns should be identified as the summary for the 1994-2003 period of record. Any idea how representative the monitoring stations really are of the total streamflow?

Page 9, bullets: (see notes from Figure 2.3, below). I was surprised to not find any mention or data on daily temperature fluctuations here. Daily maximums mean a whole lot different things if the daily range is small or large. Same for DO later on. This is especially strange since so much is made later on about fluctuations.

Page 10, Figure 2.2: This is a good figure, but note that the bottoms of the river mile numbers are cut off and the tributary names are difficult to read or incomplete.

Page 11, Figure 2.3: This figure is misleading, if not technically incorrect. Although it is dramatic, it **is unrealistic to show exceedences for spawning, incubation, and emergence of salmonids in summer months when these life stages do not occur**, especially at the station from which the data are taken (lower mainstem). Thus, the red bars are inappropriate at least from the middle of June (the likely end of any trout emergence) through the end of August (when some salmon might begin to spawn). My timing may be a little off for the Shasta fish, but the principle remains. Note same problem with 5th and 7th bullets on page 9. It's true the threshold is exceeded, but the functions are not occurring then. For the last bullet, I have a similar concern, for what evidence is there that juveniles are rearing there in summer, and adults migrating and holding then? In many salmon streams, the mid-June through August period is pretty devoid of salmonids. Your use of these data would be stronger if presence were clearly supported.

Page 12, tributary findings: Again, I'd be careful making too much about high temperatures in summer when salmonids are often not there doing their thing.

Page 12, and Figure 2.6 (Lake Shastina): It would be useful for downstream conditions to know at what depth water is discharged from Lake Shastina (this is mentioned in the appendices, but it would be good to have it here, too). It is a deep discharge with low DO.

Page 17, Figure 2.7: The legend needs to say what the text does, that this is a composite of all mainstem DO measurements.

Page 16: **I was surprised that no mention was made of percent saturation in the DO section.** A DO value of 19 mg/l is 135% saturation even at a cold 2C, a saturation level that is bad for salmonids because of gas bubble disease (the EPA criterion for total dissolved gas is 110%). Saturation at 20C is only 9.2 mg/L; at 20C, the EPA criterion of 110% saturation occurs at 10.2 mg/L. Although DO is only one part of total dissolved gas, there have been fish kills elsewhere from superoxygenation of waters by photosynthesis. If one takes from Figure 2.1 that water temperature is above 20C in the mainstem Shasta River from mid May to end of September (roughly) then from Figure 2.8 roughly 40% of the time the DO may be at lethally HIGH levels for salmonids. **In my opinion, this fact absolutely must be considered in the dissolved oxygen TMDL and is a reason for minimizing the daily fluctuations.** As with temperature, the daily cycling is important (see anecdote on page 28) and should be presented (percent saturation will vary with both DO content and temperature). The ill effects of high percentages of saturation are a further justification for doing a TMDL.

Page 18, Figure 2.8: This figure needs a better legend to describe the axes and what is being shown. The figure is informative, but takes long to figure out.

Pages 23-24, tables 2.8 -2.11 If the point was to show that P and N are high essentially everywhere below Lake Shastina, the data support that point well.

Page 25-26, Lake Shastina P: Is there any information on the seasonal cycling of P in the reservoir, particularly the sequestration in the sediments? Typically, plankton in a eutrophic reservoir will scavenge P and deposit it in the sediments during non-summer months. With stratification, the anoxia of the hypolimnion releases P into the water column again. Reservoirs that do not stratify and do not go hypoxic in summer become traps for P, which limits their biological productivity and the productivity of downstream waters (e.g., Lake Koocanusa in Montana and British Columbia and the Kootenai River). On the other hand, **deep discharges from a stratified reservoir in summer can release a lot of P into the downstream river**. If the P can be permanently sequestered in the bottom of Lake Shastina, then the P levels downstream might be lowered. Could be worth investigating.

Page 26, last full paragtraph, 4th line: spelling of border.

Page 27, 4th bullet: measure should be measured (monitored?)

Page 27, paragraph 2.6: The cautions noted above should probably be acknowledged here. It is inappropriate to say that USEPA temperature thresholds are exceeded at times when the salmonid life history events for those thresholds are not occurring.

Dissolved oxygen concentrations are also regularly ABOVE the EPA-defined lethal levels for salmonids (gas supersaturation).

Page 28, 3d paragraph: I'd be wary of making too much of this anecdote, especially since it involves a side channel fed by a spring with a (presumably) natural flow with a DO of 0.05 mg/L. Surface aeration and photosynthesis presumably raise DO in the side channel as water moves downstream. This tells the reader little in support of DO problems in the mainstem river. I don't dispute that temperature and DO are a problem, just that this is not very good evidence for it.

Page 29, 3d paragraph: I see here that the discharge is from the bottom of Lake Shastina. This virtually insures a high P load to downstream from the reservoir in summer. This P load may be more manageable than some of the other sources noted in the TMDL action plan.

Also, hypolimnetic discharges often have high loads of **dissolved methane**. Methane contributes to the oxygen demand of released waters, and may be more important as a source of oxygen demand than bottom sediments. Several reservoirs where this is a problem are installing aerating weirs downstream of the dam outlet to get the dissolved methane to transfer to the air and to help oxygenate the water. I have not seen methane mentioned in the TMDL document.

CHAPTER 3. TEMPERATURE SOURCE AND LINKAGE ANALYSIS

Page 1, first bullet: Solar exposure is the main source of heating, to be sure, but the base temperature from groundwater, springs, hyporheic flow, etc. sets the starting point.

Page 1, second bullet: This "balance" may not be true if the water is cold at start and the air temperature is quite warm, as often is the case in the region. This goes beyond the correlation noted on page 2, last full paragraph.

Page 4, Figure 3.1: This figure is nearly identical, but better, than Figure 2.2.

Page 5, next to last paragraph: Riparian vegetation is not likely to actually cool the stream water, but to prevent it from heating. There may not be any noticeable groundwater accretion, but I suspect there is a lot of **hyporheic flow**. If there is sub-channel flow, the emerging water would be cool and would provide the cooling seen in the TIR imagery. RM 24.2 is actually a bit of a warming reach.

Page 6, Figure 3.2: The arrow on the right-hand image is pointing to the warm open field, which is confusing. Better to align the arrow with the stream.

Page 7, First paragraph after Figure 3.4: I'm surprised that the shading from mature trees did not make any more than 1C difference in average daily temperature. Is the last sentence correct? If so, it weakens the case (given later) for controlling temperature with shade.

Page 9, 3.2.3: Figure 3.6 is not clear about supporting the point made. The figure needs labels. Where is the irrigation return? Which way is the river flowing? The thermal pattern looks about the same all along the right-hand image.

Page 9, last full paragraph: I don't see how an increase in flow would increase the daily minimum temperature unless there is a lot of groundwater (or hyporheic flow) that is swamped by the increased (and warmer?) surface flow. The paragraph ends with the statement that flow management is important, but the preceding sentence suggests lower flow is better. The modeling seems to show that higher flow reduces daily fluctuations caused by both solar heating and cool subsurface water.

Page 11, 3.2.5: **Hyporheic flow seems like a major omission.** Hyporheic flow is not groundwater accretion but subsurface flow in the gravel channel. This larger streamflow buffers against solar heating and actually cools the surface flow as the deep water from earlier months returns to the surface. The TIR imagery (Figure 3.1) suggests several zones of hyporheic flow where there is gradual cooling (e.g., miles 31-34, 35-37). This points out a potential problem with using a model. If the model doesn't include something like hyporheic flow, it will not show up as an influence in the model runs. Stream models all have shading, so prominently that tinkering with the shade factor is used as a calibration tool for stream temperature models. But unless the model was developed for gravel-bed rivers (unlikely that the TVA one was) it will not even acknowledge hyporheic flow as a mechanism.

I think the existing model can be gerry-rigged to handle hyporheic flow. The model includes distributed lateral inflow (which is inflow not attributed to a specific tributary and which is distributed across a reach) that can be used to handle groundwater inputs. (A single spring input would just be handled just like a trib.) This distributed flow is assigned its own water quality values (temp, DO, CBOD, and NBOD). For hyporheic flow, if you have some idea of the rate of flux in and out of the gravel, you could treat the flux into the gravel as withdrawal from the stream (water of ambient quality) and replace it downstream with distributed inflow representing the flux out of gravel (with water quality of the hyporheic flow). Being able to model the hyporheic flow as a separate entity would be better, but this method should maintain the overall water balance and produce the water quality interactions between the stream and the hyporheic flow of interest. You seem to be using this distributed lateral inflow logic for the irrigation return flows.

Page 14, middle of the second paragraph: **I suggest you need more information on the cool deepwater discharges from Lake Shastina.** Such discharges are, as stated, usually cool and can have a large beneficial cooling effect on the temperature of the downstream river if managed well. Many reservoir tailwaters support trout fisheries throughout the country (even in the South) precisely because of these cool hypolimnetic discharges. This chapter skims over this topic all too lightly, in my view, especially since there are temperature management opportunities there.

CHAPTER 4: DISSOLVED OXYGEN SOURCE AND LINKAGE ANALYSIS

Page 1, Figure 4.1: Unless it is meant to be included in CBOD, I do not see oxidation of methane from hypolimnetic releases from Lake Shastina.

Page 2, bottom paragraph, first line: end parenthesis is missing.

Page 3, Figure 4.2: The legend needs improvement. Isn't this figure the daily measured dissolved oxygen range compared to the respective calculated saturation values (based on temperature and barometric pressure)? This and Figure 4.3 on the next page are fine figures that clearly make the point.

Page 5, first full paragraph: Don't you think comparing the river BOD to untreated sewage is an unfair comparison that does not serve your point well? Wouldn't it be better to compare with another river, perhaps an organically polluted one? **What**

about methane oxidation in Lake Shastina tailwaters in summer? I'd like to see this explored, especially since it is important below other eutrophic reservoirs with hypolimnetic discharges (accounting for a large proportion of the oxygen demand).

Page 6, end of second paragraph of 4.2.3: Incomplete sentence.

Page 8, end of first full paragraph: Just a side note that juvenile fall Chinook salmon feed on the chironomids in (or emerging from) the macrophytes.

Page 9, Phytoplankton: What about phytoplankton released from Lake Shastina into the Shasta River? Lake phytoplankton are often just organic detritus once placed in a riverine habitat. That organic release is probably important in the reach below the dam, as it often is below eutrophic reservoirs. At least it ought to be ruled out as a major source of BOD.

Page 10, first full paragraph: This is weak, especially so since so much of the TMDL rests on the macrophytes. If possible, I'd beef this up.

Page 11, top paragraph: This paragraph is right on target. **Scouring flows are needed more often in the Shasta River.** This may need to be an explicit management tool as well as reliance on a once-every-10-years natural occurrence of flood flows. Short bursts of high flow over a few days may be sufficient, and not interrupt most irrigation storage. Managed flood flows may be more likely to have the desired effect than other management measures, such as nutrient reduction. Certainly warrants more consideration. It is the fine, macrophyte-enhancing sediments noted in 4.4.3 that would be washed out.

Pages 11-12, section 4.4.4 Light: These comparative relationships are quite true, but riparian shading takes many years to accomplish from scratch. While the riparian zone is building, other measures will likely be needed, such as more scour of plant material and fine sediments.

Pages 13-16, the Algae model: As far as I can tell, this is a good model and it has produced useful results. While the nutrients are stimulatory, the sentence on page 15 just below the table is important. However, I would not say "in the absence of other water quality improvements" but rather …in the absence of other water quality, water quantity, and habitat improvements…not enable dissolved oxygen standards for the river to be met.

Pages 16-18, Return Flows (quality **and quantity**): The section is probably a fair assessment of what we know, but it is a shame that these flows are not better characterized. The number of samples seems inadequate in both time and space. The return flows contribute actual fine sediment that fosters habitat for macrophytes and exerts SOD, they contribute suspended solids that are settled by macrophytes (more SOD), and they are nutrient rich (more macrophytes and algae). It would seem essential to determine the quantity as well as the quality, that is, what percentage of the Shasta River flow is made up of irrigation return water. Are there no mandatory settling basins before water is returned? Other river basins have them. Requiring such settling basins might be more effective than some other control measures for oxygen-reducing substances/features. **This seems like a major factor that needs more attention.**

Page 18, City of Yreka: Although the second paragraph of this section notes that the City of Yreka contributes to the nutrient load and nitrogenous oxygen demand in Yreka Creek, this is given no quantitative perspective. Is this loading a significant proportion of that to the Shasta River below Yreka Creek? This ought to be relatively straightforward to determine based on relative flows and concentrations. Certainly the City would want these comparisons made solidly before it embarks on any nutrient control measures.

Page 19, bottom paragraph: **This discussion of Lake Shastina discharges is exceedingly weak. Low dissolved oxygen in the tailwaters below eutrophic reservoirs is a nationwide problem that is well recognized and subject to extensive and expensive corrective measures.** TVA reservoirs are examples. There was even a famous court case in which EPA would have had to declare reservoir discharges as point sources of pollution because of the low DO and high nutrient loads (EPA has not had to do so as a regulatory matter, but the facts about DO and nutrients remain). With this background, it is simply not sufficient to say "…differences in dissolved oxygen concentrations above and below Lake Shastina <u>may also be due to</u> the fact that the outlflow …is discharged near the bottom of the reservoir… [underlining mine]. This should have been one of the first features for study in a tailwater river with low DO in summer.

In addition to the immediate low DO in the discharge, **methane is often a major dissolved constituent of hypolimnetic water and an important source of oxygen depletion in the tailwater and on downstream** (I noted this earlier). Unless there is good aeration in the tailwater (natural or induced), methane will remain in solution as the river passes downstream and its oxidation can be a large part of the oxygen demand. I don't know if this is the case for the Shasta R., but it ought to be explored. An aerating weir may be needed below the dam, as at several TVA dams.

[In re-reading these comments after having read the appendices, it seems that the Lake Shastina discharges are not a large part of the river flow. Perhaps my emphasis is less important than it might be if Lake Shastina had a strong outlet to the river. Nevertheless, I'll keep the comments here in hopes that the treatment of the discharges and the management opportunities they may offer are strengthened.]

Page 20, Figure 4.4: This figure has several problems. First, the legend should say dissolved, not dissolve. The four lines cannot be distinguished from the key (the key shows only one broken line). The symbols are too small and are difficult to resolve. The line that goes to lowest DO has several thicknesses along its length. Needs work.

Page 20, springs: The spring issue is interesting. You say in line 4 of the first paragraph that nutrient measurements were made. I seem to have missed where these concentrations and their volumes relative to the river are given. The nutrient load may be more important than the DO levels.

Page 22, second paragraph, line 6: …influenced by… In line 10 do you really mean to say that SOD levels were the lowest? I would have expected high macrophyte density and *high* SOD levels to go together.

It would appear that **further investigation of the small impoundments would be needed and desirable to quantify the effects of the aggregate of such dams on the Shasta R.** The appendix gave more information, but it still is just a sampling. Alternative ways to get irrigation water are available (such as pumping from onshore wells drawing from the hyporheic flow) that do not entail damming up sediment and creating wonderful macrophyte habitats. Reducing macrophyte habitats by reducing such dams could be more effective than long-term nutrient controls. Removing the dams would also help promote scour, discussed earlier.

Page 23, flow: It seems to me that effective reaeration is more likely reduced by increased flow. At higher flows, there is less surface area per volume of cross section. Also, the water is moving faster, so an initial low DO is carried farther downstream before being influenced by reaeration. The same thing happens with temperature—stream temperatures equilibrate fairly rapidly at low flows but excess heat (or cold) is carried farther downstream at higher flows. I guess I'll learn more in Chapter 7.

Page 23, last 3 lines: **The statement that N-15 wouldn't have come from salmon in July and August doesn't comport with the large literature on salmon-transported, marine-derived stable isotopes.** Salmon carcasses are scavenged by aquatic insects, mammals, fungi, bacteria, all of which rapidly transfer the marine-derived nutrients to the surrounding ecosystem. The salmon nutrients don't wash out within the season. They wind up in lots of places, including macrophytes and periphyton that make suspended organic matter. I would certainly not use this isotope information as evidence of anthropogenic nitrogen enrichment. There are no doubt anthropogenic sources of N, but the isotope information would not be critical evidence in this regard.

CHAPTER 5: METHODS

Page 4, Temperature component: I had a few questions about TVA's River Modeling System (RMS), so I contacted Ming Chen Shiao of TVA for more information. The RMS has been used mainly in dam tailwater systems. It was developed mainly for quick tailwater water quality assessment and built with components that have most impact on the temperature and DO in dam tailwaters. He characterized it as somewhat crude in conceptual design but useful for TVA's assessments of the cold temperatures and low DOs below TVA dams. The RQUAL component includes logic for bed heat exchange, mainly to keep water temperature from dropping too much in early morning hours. This heat exchange is simple conduction that does not take into account hyporheic flows (flows in and out of the sediment).

If bed heat exchange is turned off for this application (as said on the bottom of page 3) and the riparian shade logic is retained, **it seems as though the riparian shade aspect (or other aspects of the air-water interface heat budget) would be exaggerated to counteract cooling from the inactivated bed effects.** Won't this artificially inflate the expected benefits of managing shade?

Page 5, Oxygen component: With respect to the oxygen component of RQUAL, Ming said that methane oxidation would be considered part of the empirical CBOD. Methane oxidation could be included as an added explicit step, if desired (3.56 mg/L of oxygen is required to oxidize 1 mg/L of methane). The problem with lumping everything in with CBOD is that it gives no clues as to what might be corrected to improve DO. RQUAL makes all CBOD a fifth (?) order decay, which is lumping a lot of different Cs into one package. Ming suggests another model, CEQUAL-W2, would be better for differentiating Cs and getting at the source of the problems.

Three edits in this section: (1) first indented paragraph: "are represented" is written twice; (2) third "where" item: should be dissolved; (3) on next page, fifth line:

shouldn't "firth" be fifth? Also, note in this section liter is abbreviated with a small letter l, whereas earlier in the text liter is a capital L, as in mg/l vs. mg/L. A detail, perhaps.

Page 6, paragraph 5.5, line 3: address should be addresses.

Page 7, 5.6.1, Flow: This may be naïve, but with so much of the case for DO problems hinging on the diurnal curve being so exaggerated (supersaturation to below standards and back) isn't it somewhat important to have the "sub-daily deviations" correct in the model? Aren't the intra-reach operations (diversions and return flows) features that are potentially controllable under the TMDL? Controllable doesn't necessarily mean elimination, just management to help the temperature/DO problems. To me, it doesn't make good sense to excuse away or otherwise eliminate the very features that can be managed for water quality improvement.

CHAPTER 6: TEMPERATURE TMDL

Page 2, second paragraph: Was any consideration given to landowner preferences regarding riparian shading? Locally, we have river reaches with homeowner developments and there is strong movement to making lawns right to the river's edge. The potential for increasing shade there is low unless it is tall, mature trees (these can be encouraged with some success).

Page 2, table 6.2: This table would be more interesting and informative if it had both current and potential values for the reaches side by side.

Page 3, Big Springs Creek: This looks like a good strategy. One would expect the heating rate of the cold spring water to be higher than for the warmer Shasta River water, from air contact alone.

Page 4, first paragraph of 6.3.2.3: I don't understand how the irrigation return flows can be assumed to be at thermal equilibrium. In my experience, surface return flows are very warm in summer because of solar heating in the fields and ditches (and settling basins when they occur). Even given this detail, **I'm surprised that one of the more controllable features for temperature can be smoothed over with such broad model assumptions.** Infiltration galleries are available for return flows, which allow the return flow to percolate through the gravel bed of the river floodplain and get cooled geothermally as the water returns to the surface (or hyporheic) flow.

Page 5, Critical conditions: In terms of temperatures, it is a good high-temp/low flow year, but the issue still remains whether the salmonids you wish to protect would be there at that time. Fall Chinook juveniles have usually moved out by then; adults have yet to arrive, I suspect. Resident rainbow, yearling steelhead, and yearling or older coho would likely have gone into cooler tribs anyway. I think you will have a much more defensible case for criticality of you make it on both physical and biological bases.

Page 7, Figure 6.1: The colors on the model output locations are hard to see against the heavy green background. Also, some colors are difficult to differentiate (e.g., 2 & 3, 10 & 11).

Page 17, second paragraph: **I don't see how river flow can be changed on a reach-by-reach basis.** This stepwise analysis eliminates the influences of upstream flows and essentially treats the reach as a pond, no? Temperature is greatly affected by transit

time in relation to local heat balance dynamics (available time to come to equilibrium). Unless I can find out why this incremental approach is valid, I have to look at the results with a great deal of skepticism.
I understand better from the appendix, but my concerns remain.

Pages 18-19, Tables 6.5 to 6.8: I found these tables very difficult to read. The color helped.

Page 20, top paragraph: Is it realistic to think that the outflow of a group of springs can be increased? Or is this being done by reducing withdrawals?

Page 23, bottom paragraph, first line: insert to after attributed.

Page 24, last paragraph: That was a lot of work to simply say, “make more shade.” And it won’t happen quickly, for it takes many years to get a tree to shade a river. I’m sure it is a result the landowners will like, but will the salmon? I just have the feeling that a more explicit model (or better use of the capabilities this model has) might have pinpointed other avenues for temperature reduction.

CHAPTER 7: DISSOLVED OXYGEN TMDL

Page 2, general: I still wonder how much methane oxidation from the Lake Shastina outfall influences the downstream DO. TVA has installed aeration weirs downstream of several dams that are very effective in both reducing (volatilizing) methane and adding oxygen. I will include a clipping from the local newspaper that came as I was reviewing this section. Aeration weirs below dams with deep discharges are so effective they might be considered for the Lake Shastina tailwater. Such weirs create rapid improvement as opposed to long-term effects of shading and nutrient removal. Aerating weirs might also be considered for larger irrigation returns (it pays to think of implementation as the analyses are being done).

Page 3, Table 7.1: Either the legend or table should show the units, as do tables 7.2 and 7.3.

Pages 5-13, figures: These figures look good and certainly show the improvement you want. But I have to wonder how realistic and timely it is to reduce the rates of oxygen depletion/production stated on page 2 with just controlling the factors you have identified.

Page 15, second full paragraph through the end of the page: These are results-oriented statements that are true only to the extent that the model has included all the relevant and important factors, especially the ones that might be manipulated to achieve the desired water quality. The results show the change required, but not how to get there (that is, what specifically needs to be done). This criticism is strongly brought home when you get to the last paragraph, where you explicitly ***remove reaeration*** from the equation. **Why you would remove the very factor that has most aided DO improvement for rivers elsewhere baffles me.** Altering flows is not the only way to affect reaeration. You use the TVA model but then don’t use it for what the TVA found it to be most beneficial—improving DO. True, TVA was faced with the major task of boosting an initial low DO in the dam discharges, but I suspect you are, too, with Lake Shastina releases (at least to some degree). But the weirs reduce the oxygen load from methane as well as adding oxygen.

Page 17, Table 7.4: Shifting units are confusing. What are the unlabeled lower right boxes (per mile?)?

CHAPTER 8: IMPLEMENTATION

Page 1, first bullet, second line: **Why does this say "sediment and temperature related water quality objectives" when I thought it was temperature and DO?** A cut and paste error? Admittedly, much of the implementation for DO will be through controlling input of nutrient-rich sediment and the buildup of sediment in the channel that fosters aquatic macrophytes. But the TMDL is for DO, no? Note the same sediment problem in the footer for the Draft Amendment Language. I sense some confusion of objectives.

General Comment: I did not go into this chapter in detail. You know the social system there and what would have to be done (and the authorities for doing it). Some of my comments on the science suggest other areas for implementation that may be as effective or possibly more effective. I like the general approach of working with other agencies and groups, including landowners. If done cooperatively, you can generate a lot of enthusiasm for making corrective actions and even get the local landowners to take much of the initiative; if done dogmatically and authoritatively, you could have much resistance.

CHAPTER 9: MONITORING

Page 1, Key points: good.

Page 1, last paragraph, first line: are should be is

Page 2, top: Such photographic documentation monitoring is not effective for temperature and DO, but may be effective for vegetative cover (shading), macrophyte abundance or elimination of irrigation return flows, for example.

Page 2, second line of 6.1.3: states should be state

Page 2, end of 6.1.3: Don't forget temperature. Something like this might be added: Temperature monitoring would require measurements at hourly or sub-hoourly intervals at selected instream locations.

Page 3, top paragraph: Temperature is so easy to monitor, why hold back? The use of the term "discharger" seems inconsistent with the previous determination that there were no point source discharges on the Shasta River. I agree that irrigation return flows are discharges and should be monitored (at least the large ones).

Page 3, first bullet: Isn't DO itself to be measured at all? Seems incredible that it wouldn't be.

Page 3, first full paragraph: I agree with the plan to have the RWB design a monitoring plan specific for giving feedback on this TMDL. I suspect that the actions planned will not yield results in the near term, but take a long time. Alternative near-term actions will probably be needed.

Page 3, first full paragraph, next to last line: data should be date

Page 3, bottom paragraph: Why not use the USGS monitoring system?

General: Some of the monitoring words seem pretty generic and lifted from other situations (e.g., using "discharger" when this TMDL background says there aren't any).

CHAPTER 10: REASSESSMENT

This all seems pretty logical and thorough. There may be items from my other comments that might apply.

DRAFT AMENDMENT LANGUAGE

Throughout: The footer says "Sediment and Temperature" TMDLs. Isn't this for temperature and DO? Note earlier comment on Chapter 8.

Page 1, 3d paragraph: What permit actions might be taken? It could be useful to indicate some, probably not here but somewhere else (Chapter 8 on implementation?).

Page 1, first paragraph of Problem Statement: **If the 110% supersaturation criterion for gas bubble disease is invoked then the daily high levels of DO are above what ought to be basin objectives. This would seem to offer an additional justificaton for a DO TMDL.**

Page 2, III.B: As a novice for the Shasta, I'm surprised that there are no point sources (irrigation return flows seem like point sources to me).

Page 3, Table 1: Since the text refers to totals, it would be helpful to total the columns. How and where these numbers are calculated might be indicated.

Page 5, IV.A: I'm surprised air temperature is not listed.

Page 6, top paragraph: I'm sorry to see you use the words "natural receiving water temperatures" for these are notoriously hard to define. It is better, as the temperature TMDL does, to set a temperature goal appropriate for the beneficial uses to be protected.

Page 7, two paragraphs below the figure: How realistic is it to mandate that irrigation return flows not contribute to heating of the river? If there are return flows in summer, there will be heating. This seems to me to be the ostrich with its head in the sand.

Page 7, 3d paragraph below figure: Falling back totally on shade for the load allocation has problems, as noted above. In the model, shade is easily tinkered with to account for the real influences on water temperature such as subsurface flows and small tributaries (return flows). Shade can take decades to implement, thus essentially giving up on any improvement in the near future. I'm not sure the science as reflected in the work done for this TMDL would support going to just shade.

Page 8, second paragraph: I really question whether the sensitive life stages are actually in the mainstem Shasta River all year. The statement says "basin" but the TMDL is applied to the mainstem. This is a point of vulnerability for someone wishing to challenge the TMDL.

Pages 8-12, Table 2: I would support essentially all of these actions, but have noted above others that might be considered.

Page 13, Monitoring: I would try to establish a RWB or RWB contractor monitoring system as well as relying on the discharger.

Page 14, top paragraph: This paragraph refers just to sediment, as was done before. Isn't the TMDL for temperature and DO? Was this a copy & paste mistake or is sediment the only thing really considered important?