# Lake Tahoe Total Maximum Daily Load
# Review

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## Overview

The Lake Tahoe TMDL study and its reports associated are evidence for the highly complicated and extensive efforts underway to protect and restore water clarity in a lake that is a national treasure. The technical efforts have involved hundreds of scientists, engineers, and other professionals in studies encompassing most of the present decade. The analysis leading to the recommended goal and strategy to achieve it relied on collection of new data, analysis of old and new data, and especially an extensive modeling component. Overall, my conclusion is that the work was performed carefully with considerable amount of oversight and review. State of the art techniques were employed in data collection and analysis and in the various modeling efforts. The reputations of the leading participants are sound, and many of the individuals, firms and institutions involved are well known internationally and highly respected in their fields. The study has involved considerable public input and stakeholder involvement, and much attention has been paid to developing a long-term strategy for the implementation plan that appropriately involves a sophisticated adaptive management strategy.

The watershed and in-lake modeling efforts used current modeling techniques and are impressive in their attention to detail. Although I describe some technical issues and concerns about the methods and results of these modeling efforts later in this review, I want to emphasize here that I recognize the huge amount of work that went into these components of the TMDL study and believe they constitute a "state-of-the science" effort.

This review first addresses some important technical issues and concerns I found in reading the TMDL document and associated technical report. Next, based on my reading of the documents and in reference to the technical issues mentioned above, I address the eight issues posed to reviewers in the June 4, 2009 revision of Attachment 2 to the memorandum from Douglas Smith, Chief of the TMDL/Basin Planning Unit to Gerald Bowes, State Water Resources Control Board (dated November 12, 2008). Finally, I list some smaller technical issues, wording problems and typographical/formatting issues I found in the TMDL documents. I want to emphasize that I did not view my responsibilities as a reviewer to focus on the latter problems, and the list is not intended to be a comprehensive enumeration of such errors in the report.

## Important Technical Issues

1. ***Is the goal really reasonable given climate change is occurring?*** Given the scenario painted on pages 12-7 and 8 of the TMDL, I wonder whether it is reasonable to have a clarity standard based on historical climatic conditions. Would it not be more realistic to accept that the described changes in climate—e.g., on the mix of snow/rain in precipitation, on increasing erosion from the greater proportion of precipitation falling as rainfall, and the other climate change impacts described in this section—would cause Lake Tahoe to have a different transparency even if there were no people living in the basin? I believe the TMDL should be written explicitly to account for this likelihood. Perhaps the initial target value does not need to be changed, but the documented climate changes in the region over the past 20-40 years (mentioned in the second paragraph on p. 12-8 of the TMDL) suggests that perhaps this should be considered. At the least the TMDL should acknowledge that the target should be a "climate-normalized" nondegradation standard.

2. ***Optical modeling in Lake Tahoe.*** Because the TMDL is based on a loss of water clarity (or transparency) in the lake, work related to predicting the effects of various lake conditions and concentrations of substances affecting Secchi depth are of critical importance to the credibility of the conclusions and goals stated in the TMDL document. The optical model thus is a critically important aspect of TMDL development for Lake Tahoe, and it needs to be described in much greater detail than it is in the TMDL document (hereafter referred to as "the TMDL"), where it is mentioned only in passing on page 8-2, or in the Technical Report (hereafter referred to as TMDL-TR), where it is described in one short sentence on page 3-14, paragraph 3. Readers (and reviewers) should not have to go to the original literature for such an important component of the study. The TMDL-TR gives a table of parameters used in the optical model in section 6, which helps a little to give an understanding of what is involved in the model, but this still is not sufficient to be able to evaluate the model.

3. ***Accuracy of predicted Secchi depth values and effects of stratification***. I consider the difference between measured and simulated in 2000 in Table 8-X (TMDL, p. 8-4) to be quite large, in spite of the fact that the table heading states the numbers are in good agreement. Overall, comparing the differences as percentages of the measured values is not very useful because the measured values (the denominator term) are high, leading to seemingly small percentage differences that actually are large (> 1 m, on average) in an absolute sense. A more appropriate analysis would indicate that the simulated values consistently overestimate SD, and the average overestimation is 1.4 m over the five years. Giving a standard deviation for the difference also would be useful. This difference is fairly large relative to the overall change in SD over the period of record and even larger relative to the hoped-for improvement in transparency over the next 20 years.

The effects of thermal stratification on lake transparency and timeframe of particle settling in relation to stratification are discussed in several places in the TMDL and TMDL-TR, but the statements are not always in agreement. For example, the last statement in the second paragraph on page 3-14 of the TMDL-TR seems to contradict the statement on the previous page about a decadal time frame for particle settling. It would seem to me that settling should be even more rapid in the quiescent waters below the thermocline than in the upper (mixed) layer. It is important that the discrepancy between these two statements on settling times be resolved. Similarly, the statement on page 3-20 (third line from bottom) seems to contradict earlier arguments about the slow settling of particles and about the negative impacts that deep waters have on transparency.

I also am concerned that the TMDL makes it sound like increased thermal stability and lake stratification can only make matters worse relative to lake transparency (page 12-9). I do not accept this. Increased stratification could decrease the residence time of fine particles in the top most stratified layer, particularly if the increased stability leads to a shallower thermocline. No evidence is provided that the bottom waters would become anoxic or even hypoxic in 20 years, and those are the critical conditions for increased P release from sediments. Although an infrequent (every 20 years) deep mixing event may cause a significant algal bloom, it most likely would be short-lived—a transient phenomenon.

4. ***Watershed modeling***. Overall, the TMDL and TMDL-TR have very detailed coverage of the extensive modeling that was done on export of nutrients and fine particles from the Lake Tahoe watershed, but I have several concerns and questions. First, I am aware that all municipal wastewater is exported from the drainage basin, but I wonder what happens to solid residuals (sludge) from water treatment plants. Also, many water treatment plants add phosphate to water to prevent corrosion problems and many plants also add ammonium as part of chlorination. If either of those practices occurs in water treatment within the Lake Tahoe drainage basin, they could contribute N and P loadings to the lake since not all the municipally treated water gets exported from the basin (e.g., some is used for lawn watering, etc.). Table

4-2 and associated text of the TMDL-TR at least should mention these potential sources and also should note that wastewater wasn't considered because it is exported from the basin.

Second, the EMC multiplying factor used to calibrate fine sediment loads (pages 4-62 and 63 of the TMDL-TR) seems rather arbitrary and empirical, and no explanation is provided for its basis (other than that it seemed to work). Some effort to explain the need for this empirical factor would seem to be appropriate. I note that the factor has a large range (> 6) and so it has a large effect on predicted loads. The same criticisms apply to the scaling factor based on quadrant.

Third, I always find graphs like Figures 4-27 to 4-29 of the TMDL-TR troublesome, especially when they are presented to illustrate "how well" the simulations fit to measured data. It is difficult to tell from the figures, especially in any quantitative sense, how good or poor the fit actually is, but it appears that the fit is not good in terms of simulating either the timing of events or the variability in the data. This is especially the case for 2000-2001 for all three modeled constituents. About the best one can say from these figures is that the simulated values are in the "same ballpark" as the measured values. Perhaps that is sufficient for the purposes of the TMDL study, but if that is the case, I doubt that the time and effort that went into developing such a comprehensive and detailed modeling approach can be justified. Simpler approaches that didn't try to model and portray short-term variability would have been sufficient. If the authors want to show how well (or poorly) the model simulates reality, they should present plots of simulated versus measured concentrations (scatter plots) and show the statistics ($r^2$ values) that quantify the degree to which the simulations explain the variance in the measured data. I suspect such plots would show poor fit of individual simulated values to measured values. I accept the arguments made in various places in the TMDL-TR that the goal was not to simulate individual measurements and that it is very difficult to achieve that, but some larger-scale statistics could and should be produced to show whether the simulations capture key features of the measured values at the time scale of a year (e.g., annual means and ranges, and annual variance).

Finally, the regressions of Rabidoux (2005), described on p. 5-5 of the TMDL-TR, to predict particle fluxes as a linear function of stream flow involve a self-correlation. Particle flux (P) is a product of particle concentration, $C_P$, (in stream water) and stream flow, Q; i.e.:

$$P = C_P * Q \text{ (number/m}^3\text{)*(m}^3\text{/sec) = (number/sec)}$$

The regressions thus implicitly are $C_P$*Q versus Q, which is a correlation of a variable with a function of the same variable. Depending on the ranges of $C_P$ and Q this could lead to spurious self-correlations. The authors need to examine whether in fact this occurred in Rabidoux's analyses. There are straightforward statistical techniques for deciding whether this is a serious problem or not.

5. ***Atmospheric loading issues.*** I have two separate concerns about the work on atmospheric loadings. First, the issue of local versus regional sources for atmospheric particles and nutrients has very important implications in terms of implementing a control strategy, and the subject deserves more attention and description in the text than it is given. The text associated with Table 4-64 (p. 4-150 of the TMDL-TR) at least should provide a summary of the basis by which CARB concluded that most of the particulate matter, TN and TP in wet deposition is locally generated. This is a very important finding. I also note that the proportions of regional versus local contributions for fine particulate matter are reversed in winter-spring versus summer-fall, and that regional sources dominate in the latter seasons. This suggests that regional sources may be more important in affecting lake transparency during the critical summer period than implied by using the aggregated annual values of regional versus local contributions. The authors should address this issue.

Second, it is not entirely clear to me what the basis is for the expectation that watershed management will be sufficient to meet atmospheric load reductions, as is stated in the TMDL on page 11-13. The text notes that the majority of fine particles from the atmosphere are generated by urban roadways. As a minimum, the effectiveness of controls on particle loads from these roadways in decreasing atmospheric loadings will depend on the nature of the controls on stormwater from the urban roadways. If the controls primarily involve treatment of roadway runoff in detention/retention ponds, this will have no effect on the extent to which the roadways generate fine particles that are swept into the atmosphere during periods when it is not raining. Increased frequency of street sweeping could help decrease atmospheric loadings of fine particles derived from roadways, but it would have been useful to see a more thorough analysis of this.

6. ***Feasibility of adjusting the management plan in response to wildfires and climate change.*** Just because wildfires are sporadic does not to me seem adequate justification for excluding them from consideration in loading targets and management plans, as the TMDL states on page 12-11, first paragraph. It seems likely, given what the report describes concerning the consequences of climate warming, that wildfires will be more prevalent in the future than they have been in the past. At least the TMDL should acknowledge this and indicate that it will be considered as a part of the adaptive management program.

It will be very difficult to adjust the management plan to changing climate over the 20-year timeframe of the clarity challenge because of inherent noise in climate data. For example, five years of above average temperatures and below average precipitation could be followed by five years of below average temperatures and/or above average precipitation. The signal of increasing global $CO_2$ is apparent at near annual resolution from the long-term record in Hawaii, but the signal of climate change is not apparent anywhere near this level of resolution, especially for specific geographic areas. At best, I think the managers might be able to see a change in climate at the end of the 20-year challenge period and adjust their goals and management plans for the next 20 years accordingly. However, even this is not a certainty. The text should be modified to reflect the strong likelihood that we will not be able to see long-term climate changes within the timeframe of the initial implementation period (really the first 15 years of the challenge period).

7. ***Consistency in methods for long-term data.*** The report uses some of the valuable long-term data collected on Lake Tahoe, but it does not indicate whether consistent methods were used to obtain the results over the entire period of record. For example, in discussing trends in primary production, the report indicates a significant increase over time since Goldman's original measurements in the 1959 (TMDL, page 3-4, line 2 from bottom; Figure 3-5). I wonder whether the same measurement methods were used throughout this time period. Are the earlier results really comparable with the later ones? The text should comment on this. Similarly, the TMDL-TR (page 4-18, first paragraph) compares fertilizer use in the basin in 1972 with current or recent rates. One wonders whether the 1972 data were underestimates. If so, perhaps fertilization rates have not increased so markedly in the basin. Some attention to this possibility seems in order.

8. ***Monitoring issues.*** Future monitoring activities on Lake Tahoe are described in the TMDL in the second paragraph on page 13-8. I recommend that the monitoring program add pH, specific conductance, and DOC/TOC as routine measurements and annual measurements of major ions (including alkalinity), iron and manganese. None of these is expensive to measure, and they will add greatly to the usefulness of the long-term database. Specific conductance and pH are very basic limnological parameters measured in nearly all chemical studies. DOC is related to transparency, at least indirectly.

Given the huge budget problems facing the state of California, one wonders how certain the authors of the document are (or can be) that the LTIMP tributary monitoring described on page 13-9 of the TMDL will continue to provide data that can be used to assess the effects of load reduction measures. I think this issue needs to be addressed explicitly in the report.

9. ***Need for more specificity and examples in citing shifts and trends.*** In several places the reports the report describes shifts that apparently have occurred in certain characteristics in the lake but the text is vague on the magnitude of the shift. Inclusion of some numbers would be useful to put the comments into perspective. An example related to thermal stratification is on page 3-8, line 3 of the TMDL. Similarly on line 9 of the same page, the text is vague about the shift in the deep chlorophyll maximum. Some vertical profiles illustrating the change would be useful (or referencing where they may be found in an accompanying document would help).

## Review Issues Requested by California Regional Water Control Board—Lahontan Region

The request to review the Lake Tahoe TMDL and associated documents requested responses regarding eight issues of primary concern. In each case the reviewer was requested to determine whether the scientific portion of the proposed Basin Plan Amendment (related to the stated issue) is based upon sound scientific knowledge, methods, and practices. The eight issues are listed in bold below followed by my analysis and conclusions.

**1. Determination of fine sediment particles (< 16 μm) as the primary cause of clarity impairment based on interpretation of scientific studies, available data, and the Lake Clarity Model.**

The reports provide sufficient evidence based on field studies and analysis of historical data that fine particles (< 16 μm in diameter) are the primary cause of clarity impairment in Lake Tahoe. Actually, the reports provide evidence that clarity is affected primarily by particles ≤ 5 μm in diameter. The reports also demonstrate that the clarity reduction is caused by fine (mostly inorganic) particles exported from the watershed and also deposited directly onto the lake surface by atmospheric wet and dry deposition, as well as by in-lake generated particles produced by phytoplankton growth. To some extent, the study relies on the seminal findings of Jassby et al. 1999 to make the case for the importance of inorganic particles of watershed and atmospheric origin, but I think sufficient data are presented in the TMDL documents to make the case. By use of the Lake Clarity Model, the researchers were able to make predictions of what would happen to lake clarity under a range of scenarios of nutrient and fine particle loadings to the lake. The work related to this issue is based on sound science and widely accepted scientific methods.

**2. Identification of the six sources of pollution affecting lake clarity of which urban upland areas was found to be the primary source of fine sediment particles causing Lake Tahoe's clarity loss.**

Based upon my review of the TMDL and TMDL-TR, I conclude that the study adequately and appropriately identified the six main sources of pollution affecting Lake Tahoe water clarity and was correct in assessing urban upland areas as the most important of these sources. The work described in the reports was based on sound and currently accepted scientific methods, as described elsewhere in this review. I agree that the reliability of the estimates was checked, where possible, by using several independent methods of analysis or calculation. Of course, there is a stronger database and much longer historical record available to assess the contributions of nutrients than fine sediment particles, but my assessment is that the study was adequate to address this specific issue.

**3. Determination that the Lake Tahoe Watershed Model was an appropriate model to estimate upland pollutant source loads.**

The Lake Tahoe Watershed Model is based on several existing components that have been accepted and used by others and were adapted and further developed for application to the drainage basin of Lake Tahoe. As indicated elsewhere in this review, the reports describe in considerable detail the work done to develop and use this model. Although I have a few specific concerns about the way the model was used (e.g., see item 4 of the previous section), I do not have any concern that the model was inappropriate or represents a less than "state-of-the-art" approach to modeling pollutant export from watersheds. The university and firm that conducted much of the watershed modeling work are well respected institutions, and based on evidence provided in the text, I conclude that the model development was carefully done.

**4. Determination that estimates of groundwater nutrient loading rates are reasonable and accurate.**

I preface my conclusions on this issue with two initial remarks. First, I do not consider myself to be an expert on ground-water modeling. Second, the TMDL and TMDL-TR documents rely heavily on the U.S. Army Corps of Engineers study (USACE 2003) and mostly summarize what is reported in that document. The TMDL documents do not provide the level of detail on ground-water loading estimates provided on watershed modeling. Consequently, I was not able to perform a thorough, independent review and analysis of the technical details on ground-water nutrient loadings. Nonetheless, the descriptions provided in the reports indicate that the USACE work was competently and carefully performed, with attention to issues of heterogeneity in the ground-water aquifers of the basin. The concentrations of nutrients reported for the aquifers and the nutrient loading rates appear to be reasonable. It also was reasonable for the study to assume that ground water is not a source of fine particles to Lake Tahoe.

**5. Pollutant loading rates from atmospheric deposition directly to the lake surface were quantified and in-basin sources were found to be the dominant source of both nitrogen and fine particulate matter. Direct deposition of dust accounts for approximately 15% of the average annual fine sediment particle load.**

The studies undertaken to quantify nutrient (N and P) and fine particle loadings to Lake Tahoe from atmospheric deposition directly to the lake's surface were extensive, and they appear to have been competently done. Both historical and new data were used to make the assessment. In my opinion, the conclusions related to rates of N and P deposition and the fraction of annual fine particle load contributed by direct deposition of dust are based on sound scientific knowledge, methods, and practices.

I am unable to make the same statement about the conclusion that in-basin sources were found to be the dominant source of nitrogen and fine particles. As noted in item 5 of the previous section, I found the report deficient in its description of how CARB reached this conclusion. This is not to say that the wrong conclusion was reached or that the work was scientifically unsound or based on unsound methods. I simply am unable to evaluate these issues on this topic because the report lacks sufficient detail. Additional documentation should be added to the TMDL-TR to describe how this was done. In addition, the high variability in local versus regional contributions across the seasons suggests that merely looking at the annual loadings may not be adequate. The data in Table 4-64 of the TMDL-TR indicate that most of the atmospheric loadings in summer are from regional rather than local sources, and this could impact water clarity negatively during this period, which is critical from lake-user perspective.

**6. Pollutant Reduction Opportunity (PRO) analysis identifies fine sediment particle and nutrient reduction options that can be quantified. The PRO findings offer basin-wide pollutant load reduction estimates and costs for a range of implementation alternatives for reduction loads from urban uplands, forest uplands, stream channel erosion, and atmospheric deposition sources.**

Much of the work done on this issue was not highly technical (at least not of the nature of the analyses and modeling efforts that led to the loading estimates, targets, and allocations), and a somewhat different basis is appropriate to address its adequacy. The PRO analysis and related IWMS involved a wide range of experts from many stakeholder groups and extensive amounts of review of preliminary findings. I am not an expert on the processes whereby pollutant reduction options have been analyzed in other TMDL studies, but I found the approach used in this study to be thorough, objective, and open. The results presented in the PRO appear reasonable to me, although I also am not an expert on many of the load reduction technologies. The costs associated with the implementation efforts needed to achieve the clarity challenge are truly daunting in this day of (many) billion dollar state deficits and trillion dollar national deficits.

**7. Lake Clarity Model was the most appropriate for predicting the lake response to changes in pollutant loads.**

Insofar as the Lake Clarity Model (LCM) was developed specifically for Lake Tahoe, which is a highly unusual lake with respect to water clarity, I agree that this is the most appropriate model for predicting responses of the lake to changes in pollutant loads. The LCM is based on a hydrodynamic sub-model that has been tested internationally and is widely accepted as appropriate. This sub-model produced reasonable simulations of thermal stratification and related patterns in the lake. The LCM takes a comprehensive approach to simulating the behavior (and formation) of light scattering and light absorbing particles in Lake Tahoe. The component dealing with phytoplankton growth is explained thoroughly in the report and appears to use appropriate mathematical formulations.

In some respects, however, the core of the LCM is the optical model that was developed by Swift and coworkers. Unfortunately, as indicated in item 2 of the previous section, the reports do not provide sufficient information for a technical review of this critically important component.

**8. Allocation of allowable fine sediment particle and nutrient loads is based on the relative magnitude of each pollutant source's contribution and the estimated ability to reduce fine sediment particle and nutrient loads.**

Although limitations in the field data cause a fair amount of uncertainty to remain in the estimates of particle contributions from specific sources, the study did a creditable job of estimating these contributions for each pollutant source. This was a very difficult task, and the researchers recognized the limitations in the data and compensated as best they could by using (where feasible) independent methods of analysis and calculation to reach their conclusions. Overall, I conclude that the work on this issue was based on state-of-the-art techniques and involved extensive review and oversight. Based on my review of the reports, I conclude that allocations of allowable loads were done objectively based on the relative magnitude of source contributions with proper attention to technological and economic constraints in the ability to reduce loads from various sources. Nonetheless, some issues should be addressed, as noted in items 1, 5, and 6, and the last paragraph of item 4 in the previous section.

## Smaller Technical Concerns and Editorial Issues

(Note: "fb" in the column for "line" denotes "from bottom" of the page; ¶ denotes paragraph number)

| Page | ¶/Line | Comment |
|---|---|---|
| ES-2 | 4fb | It would be clearer if the values were given as percentages of the required reduction (e.g., 24.5*100/32 = 76.5% of the reduction should come from urban uplands.) |
| 2-1 | | The map (Figure 2-1) is not very helpful. It is unclear where the line between CA and NV is. It is not clear that the unnamed area on the NW end of the lake is a part of |

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| | | Placer County. The middle county in NV is labeled Ormsby, but the text (p. 11-7) refers to it as Carson City Rural. |
| 2-2 | 11fb | There should be no spaces between the dash connecting a range of numbers and no apostrophe for pluralizing numbers (should read: 1900s-1950s). This is a consistent problem in the text and should be corrected in the final report. |
| 2-3 | 3-4 | The text does not agree with what the map shows. Much of the west shore is developed; only the SW end appears undeveloped. Similarly, much of the east shore appears to be developed except for a few stretches on the northern third of the east shore. |
| 2-4 | Fig. 3-2 | Box indicates the line of best fit is a linear fit but the line clearly is curved. The best fit equation should be provided in the box. |
| | 6fb | This is an understatement. The figure shows that ~70% of the scattering is due to particles < 5 µm in diameter. |
| 3-4 | 5 | I doubt that we can know this increase with the accuracy implied by the text (725%). |
| | 10 | Use of double slashes is incorrect and a mathematically ambiguous way to display areal rates. The report should use either $g\ C/m^2 \cdot yr$ or $g\ C\ m^{-2}\ yr^{-1}$. |
| 7-7 | 6 | "Data" is a plural word; text should read "water quality data were collected.…" This error occurs in a number of places in the TMDL and accompanying technical document and should be corrected in the final versions. |
| | 1,2fb | "provide" and "estimate" should be written in the past tense. |
| 7-8 | 13fb | One wonders how inorganic versus organic particles were determined. |
| 8-5 | Figure | The slope of the "Projected trend" does not appear to fit the data in the graph. |
| 9-5 | 18 | Some text appears to be missing. |
| | 22 | Ditto |
| 9-9 | 6fb | It would be clearer to say "providing 75% of the needed reduction in fine particles…" |
| 10-4 | 15fb | Should be Tables 10.2 through 10.4 |
| | 8fb | Should be Tables 10.5 through 10.7 |
| | 6fb | Should be Tables 10.2 through 10.4 |
| 11-7 | 16fb | County is identified as Ormsby on Figure 2-1. |
| | 14 | Appears to be some missing text at end of line. |
| 11-10 | | Most of the example load reductions are vague and not very helpful. |
| 12-8 | ¶ 2 | What is this evidence? Merely citing a couple of references is not adequate here. The text should indicate the magnitude of the changes. |
| | Last ¶ | It would be useful to have some measure of variability for the deep mixing phenomenon. (4 $\pm$X years). I suspect the record is long enough to provide a reasonable estimate of the variability in the frequency of deep mixing. |
| 13-4 | ¶ 1 | This paragraph strikes me as indicating that a huge and unseemly amount of bureaucracy is associated with the management of Lake Tahoe. |
| | ¶ 2 | It would be useful to say something about the way stormwater samples will be collected. Presumably (hopefully) they will represent event-integrated samples rather than grab samples. Note that "un-ionized" (line 4) should be hyphenated to avoid confusion with the word unionized. |
| 13-7 | Last ¶ | The text should say how far from shore the index station is. The map in Figure 13-1 shows the station as very close to the shore. Text elsewhere indicates the station is 2 km from shore. The figure may need to be corrected, and it would be useful to label each TERC station on the map. |
| 14-2 | 1 | It is not clear exactly what the $10 million figure refers to. |
| 14-3 | ¶ 1 | It would be helpful if the text would provide some measure of the uncertainty remaining in the key models and the magnitude by which the uncertainty was decreased as a result of developing the site-specific models. |
| 14-4 | 8fb | I think the authors mean "First, *conservative* assumptions were made.…" It would help if this paragraph would indicate that examples of the conservative nature of the |

assumptions in the two areas are described in subsequent paragraphs (although there is not a lot of information provided) or are described in detail in the technical report).

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| **Page** | **¶/Line** | **Comment** |
|---|---|---|
| 3-1 | ¶ 2 | There is no "typical value" of watershed/lake ratio. I will grant that the watershed/lake ratio for Lake Tahoe is small, but the value of the ratio ranges widely, and it is misleading to imply that there is such a thing as a typical watershed that has a watershed/lake ratio of 10. |
| 3-4 | Fig. 3-2 | This is a better map than Fig. 2-1 in the TMDL report. The authors should consider replacing Figure 2-1 with this or a similar figure. |
| 3-11 | Fig. 3-9 | Authors should give the $r^2$ and equation for the line of best fit. One wonders what a linear fit would look like. The data are sufficiently scattered that it is dubious whether a curvilinear fit is really appropriate. |
| 3-13 | ¶ 1 | One wonders at what depths the sediment traps were deployed and whether the settling velocities are representative of the entire water column. Given the fact that N- and P-containing particles are undergoing continual degradation on their downward journey, the point made in the last sentence (about mineralization and recycling) is especially pertinent. |
| 3-15 | 1 | Figure 3-13 does *not* show that lake clarity increased. One can infer that it likely increased from the trends in mass sedimentation rates, biogenic silica fluxes, and inferred primary production, but the figure itself does not have any transparency parameters on it. The authors need to be careful in how they phrase the text on such an important and sensitive issue. |
| | ¶ 2 | The decline in transparency has *not* been caused primarily by the gradual accumulation of pollutants over time, but is caused by *continuing* inputs of the specific pollutants. Again, this is a matter of being precise in the use of language. As written, this paragraph implies that pollutants accumulate in the lake for long periods of time. I don't want to get into arguments about the meaning of "long," but as the text in paragraph 1 on this page indicates, reductions in loadings of sediment and nutrients likely leads to increased transparency in relatively short periods of time. |
| 3-16 | 3 | Saying that algae "require" N:P in a ratio of 7:1 is at best simplistic. This should be restated after consultation with a limnologist who understands the nuances of nutrient ratios. |
| | 9 | The text should replace total Kjeldahl nitrogen (TKN) with total organic nitrogen (TON). I doubt that laboratories analyzing Lake Tahoe samples actually use the Kjeldahl method anymore; most limnologists and environmental laboratories converted to a more sensitive alkaline persulfate oxidation method 10-20 years ago, which gives accurate results for total N (from which TON is calculated by subtracting separately measured values for nitrate-N and ammonium-N). |
| | ¶ 4 | "Bioavailability" depends on the method used to determine it. The text should give some indication of how bioavailable P was determined. |
| | 4fb | The range 16-56% is so large that it is not very meaningful to say that the value of 40% found by Hackley et al. agrees with the results of Dillon and Reid. |
| 4-1 | ¶ 3 | It would be more appropriate and accurate to state that Reuter et al. developed the first nutrient budgets for Lake Tahoe. Nutrients (N and P) are not pollutants per se, although there is widespread agreement that excess nutrient inputs are a type of pollution. Even pristine Lake Tahoe requires some nutrient input to survive as an ecosystem. In addition, I think it would be more accurate to use the term fine particles rather than fine grained sediment because not all the particles are (or have been) sediment; atmospheric particles certainly fall in this category. I think the terminology used in this paragraph is a |

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| | | little careless. Also, if the budgets were developed in 1998 and revised in 2000, why were they not published until 2003? Given that Jassby et al. noted the concern about fine particles as a pollution source for the lake in 1999, the argument that the budgets focused on nutrients because they were thought to be the principal cause of clarity loss are a little strained. |
| 4-4 | 3fb | Actually, it is 72%, which is closer to three-fourths. |
| 4-7 | 1 | It would be helpful if the report would show results demonstrating that ground water in fact is "nutrient-rich," as this line states. Alternatively, it would be fine if the text would refer the reader to any table or figure elsewhere in the report where such documentation is given. |
| 4-11 | | "principals" should be "principles." |
| 4-12 | ¶ 2fb | Missing word "have" in line 2? |
| 4-13 | ¶ 2 | The word "ambient" is misused here and in Table 4-4. Why not say what you mean—undisturbed? Also, it is not clear what the difference is between vegetated and forested undeveloped and undisturbed areas (last line of paragraph). |
| 4-90 | 5 | I think the authors mean "latter" not "later." Nonetheless (line 8) is one word, not three. |
| 4-109 | | One wonders why the streambed samples that were analyzed for TP were not analyzed for TN at the same time. The same digestion procedure can be used for both N and P, and the amount of additional labor would have been minor. |
| 4-121 | ¶ 1 | The reasoning in this paragraph to ignore organic particles is questionable. Certainly the authors would agree that phytoplankton and detritus produced from phytoplankton and other microbial activity in the water does have an important effect on water clarity even though the particles are nearly entirely organic. I cannot see any reason why organic particles from the atmosphere would not affect lake transparency. |
| 5-13 | | The standard deviations for most sites exceed the mean values for both particle sizes, in some cases substantially so. This indicates that the data are highly skewed. The text should acknowledge this and describe what was done to overcome this problem. |
| 5-14 | ¶ 2 | Use of four-place precision (318.3) for the multiplication factor is a rather extreme example of going overboard in creating a false sense of precision in the analysis. There is no way that the authors can imply that the factor is known to that level of precision and accuracy. Rounding to one place (300) would describe better the accuracy with which they can estimate the factor. |