Response to Agency and Peer Review comments on **Lake Tahoe Seasonal and Long-Term Clarity Trend Analysis, Tahoe Science Advisory Council, Work Order #009.**

October 2020

Detailed review comments were compiled from agency representatives and the Tahoe Science Advisory Council (TSAC). This document summarizes the major comments to the document as well as comments and concerns raised in the word document organized by the major sections of the report. The document underwent substantial revision to improve the content and structure of the report. We added new sections and organized each hypothesis with consistent subheadings. Additional figures were added to the Appendix where questions by agency representatives were requesting more information. We appreciate the comments and suggestions provided by reviewers on the report which substantially improved the report. The following represents the major substance of the State and Federal agency representative review comments.

State Agency Representatives Major comments

As requested, attached and below are joint agency comments on the draft Lake Tahoe Seasonal and Long-Term Clarity Trend Analysis project report. Three high level comments on the report are:

Hypotheses 3 & 4 – Climate Change – The responses to hypotheses 3 and 4 paint a compelling picture that lake warming, stratification, and insertion depth are all impacting clarity. There is passing reference to these factors being climate change related, but it would be good to make that connection more explicit.

Response: There is no doubt that climate change has influenced lake temperatures (thermal stratification) as science has already been widely accepted and published on this. Climate change is also affecting snow accumulation, precipitation, streamflow runoff patterns and algal processes to name a few. It may not have been explicitly stated in this document that climate change is having an influence on thermal stratification because it is also having an effect on other processes as well and stating so for one process and not others may lead to a biased interpretation regarding the impacts of climate change. We added a paragraph to the introduction that makes these points, specifically, and provides a summary of relevant climate change work.

Hypothesis 2 – Winter Clarity – The hypothesis focuses on the change in trend in winter clarity. The trend analysis that precedes the hypothesis identifies the change in trend as having occurred prior to 2000, yet the analysis presented doesn't include data from that time period. The analysis appears to focus primarily on correlating annual clarity values with annual loading numbers. Given the long-term nature of clarity management, it would be beneficial to analyze the extent to which clarity trend changes are due to load reductions.

Response: Data was analyzed for winter periods separately for the duration of time over which these data were available. For example, Figure 3.3 shows the trends in winter lake particles from 2009 to 2019, a period that is within the timeframe during which winter lake clarity had leveled off (2000-2020). A large increase in suspended particles in the lake was observed following the precipitation and snowmelt that occurred in 2017. Winter periods were also evaluated for correlation between clarity and particles (fig 3.5) and clarity and Cyclotella (fig 3.6). Data of sufficient quality and duration on fine sediment particle (FSP) loading simply does not exist to evaluate load reductions and any correlated changes in winter clarity.

Hypotheses 3 & 5 – Physical vs. Ecological – The report seems conflicted on the dominant driver of clarity decline. Page 42 of the report appears to support a loading/climate change hypothesis: "Thus, changes in lake stratification expressed (e.g. more resistance to mixing) and higher loads of fine particles appear to be dominant processes affecting the reduced clarity in Lake Tahoe"; while the answer to hypothesis 5 appears to suggest the internal "loss of cladocerans, the "internal cleaners" of the lake, appears to be the dominant driver. Resolving the apparent discrepancy or providing greater context for how the two narratives fit together would be beneficial. There also appears to be a tonal shift in the response to hypothesis five that makes it sound more certain than the rest of the document.

Response: We have attempted to remove any impression of conflict on dominant drivers, and actually have tried very hard to dispel the notion that there is a dominant driver. Have also tried to remove differences in tone between sections.

The analyses that were conducted all indicate that the drivers of clarity change are "all of the above". During an extremely wet period (2017-2019) stream inflow was very important. At other times, the influence of changing stratification dynamics is very important as they can focus both small external loads into narrow bands close to the lake surface. It can also give Cyclotella a competitive advantage over larger algal species, thereby have a very large impact on clarity over the summer.

The fact that this report confirms earlier studies that showed that light scattering by fine particles (both organic and inorganic) are the causal mechanism of clarity loss, adds support to the influence the loss of Cladocerans (due to Mysis introduction) has had on clarity. The fact that the size range of light-impacting particles is precisely the size range that cladocerans can very efficiently remove, and that this occurred at the same time that long term clarity decline commenced is no coincidence. The fact that the cladocerans were effectively eliminated from the system (absent for years at a time and sporadic appearances at orders of magnitude lower abundance) means that it is not possible to assign a statistical significance to their role. Rather, as exemplified by Emerald Bay, when cladocerans returned in large numbers, particles disappeared, and clarity improved at a dramatic rate.

More detailed comments are included in the attached mark-up. We greatly appreciate the project team's consideration of our comments in development of the final report.

Response: Thank you for recognizing the value of the work we completed.

Other Agency comments

This report represents a fantastic compilation of our latest understanding of the factors influencing Tahoe clarity. It contains an important body of analyses that will aid in the management of lake clarity. I've made a number of comments below, but wanted to register two primary critical thoughts up front:

The first is that the report contains many complex analyses, and these analyses are often difficult to follow in isolation, much less in context of the entire report. It doesn't help that the discussion and conclusion section contain more, new analyses, right when the reader is trying to come to grips with the meaning of the report in its entirety. It would help the usefulness of the report if the five hypotheses were synthesized in a manner that left the reader with key take home nuggets derived from the report. This could occur in the conclusion, but the story of how these hypotheses originated needs to be told upfront, and then neatly brought home in the conclusion.

The second thought is regarding hypothesis 5. There is much lacking in the analysis of this hypothesis. There really isn't much data or analysis that supports the conclusion of this hypothesis, and some of the analyses and literature directly refute the hypothesis. I recommend that this hypothesis be removed from

this document or changed to a section that discusses future questions or something. It's simply premature to reach many of the conclusions within that section of this report.

To conclude, while there are a couple of meaty items that need to be addressed, this document represents a critically important effort to collate all available sources of data and quantify the immediate causes of Tahoe clarity. The authors of this report have made significant advances in our understanding of this important issue, and I'm confident that this will contribute to improvements in how the partners manage the Jewel of the Sierra.

Response: We've done a lot of edits to the report to try to address these comments. Hypothesis 5 is still included; we've tried to make it clear which points are supported by data, and which will require more work to be considered verified. We have added additional text indicating that the findings of Hypothesis 5 are summarized from a manuscript that is in review. We were also asked by agencies to include this work despite the findings not being published yet.

All minor comments or suggestions that were provided through the review process were addressed in the report and were not included below for brevity. The following are major comments for each section of the clarity report.

Introduction

Comment: Language would be beneficial regarding clarity being the integral metric for physical and biological processes affecting the lake that are intertwined to varying degrees.

Response: Added statement about potential impacts of both biological and physical processes.

Comment: I never heard a clarity complaint in my 20 years with the Water Board. People think "clarity" is something quite different than what is measured by the Secchi disk.

Response: Reworded

Comment: Are measurements of underwater attenuation of PAR or other wavelengths of light available? Are other optical properties available?

Response: Yes, but this is the report introduction – not the place to list data sources.

Comment: As with the executive summary, it would help to say a few words on why this work is being done. In my mind, the reason is because after all these decades, we have not yet been able to come to grips with what is causing the decline in clarity. Thus, this work is necessary.

Response: The work was prompted by the Tahoe Agencies wanting a broader perspective on the causes of clarity change, particularly the apparent summer-winter divergence. UC Davis had presented the likely causes annually as part of their annual clarity release. As it turned out the conclusions of this broader group agreed with what has been said over the last 20 years. That is precisely the reassurance the Agencies wanted

Comment: How did these hypotheses come to be? Are they our best hypotheses for understanding clarity? Have other hypotheses been eliminated in the literature, and these remain? Are they simply our best hypotheses to test without collecting new data? I ask all of this because an obvious hypothesis to me and several others is old septic leakage. So, it begs the question of how we derived these hypotheses.

Response: The hypotheses were developed by agency representatives and presented to the report authors. In retrospect, a collaborative effort where agency representatives and TSAC members worked together to refine the list of hypotheses might have been preferable and might have resulted in a slightly different list of priorities for the investigation.

Comment: I realize that most are familiar with the acronyms used for data but suggest that these bullets tie-in the acronyms used in figure 1.2 and table 1.1 to these data sources – MLTP, LTP, LTMP, TRCD, and TERC; LTIMP already defined. Seems like there are other data sources listed on fig. 1.2/table 1.1

Response: Revised. Here we want to list programs, with details provided in the fig and table.

Comment: This discussion does not adequately describe the relationship between modeled load estimates and the RSWMP monitoring program. There was never any expectation or intent to monitor a representative amount of the urban watershed, so referencing the size of the monitoring fraction is misleading. The monitoring was developed to calibrate and validate modelling results. The discrepancy between a single year's monitoring and modelling is expected, as the modelling provides average annual estimates.

Response: Revised accordingly.

Comment: More specifics on Cyclotella datasets available would be helpful. Most of the analysis involving Cyclotella was conducted only using 5m depth data – was this the only depth where the only data existed or was this determined to be most representative.

Response: We had data at 5 m and at 20 m, as well as at deeper depths. When Cyclotella was an issue, the SD was well below 20 m, so we used the 5 m data as a proxy. The literature shows that Cyclotella tend to be high in the water column. Seasonal *Cyclotella* along with clarity graphs were added to the Appendix A.2.1

Assessment of Trends in Lake Tahoe Water Clarity

Comment: This comment is made after reading this section – a very difficult section to get through because of much back and forth on time periods and significant/not significant trends. Difficult to parse out the take-away message, at least until the end. Suggest a more methodical approach, but would require a format change:

1. Divide the section into three parts – Annual, summer and fall/winter period

2. For each section, discuss clarity trends in order of the three time periods analyzed with a big focus on statistically significant trends

3. In each section, provide a few sentences briefly summarize thoughts on possible drivers for statistically significant trends or why we didn't see statistically significant trends

Response: section was revised accordingly.

Comment: Why use medians or why not use means and medians?

Response: Medians were used to remove the impact of outliers in monthly measurements.

:Comment: Why were these bins created and how were the categories determined?

Response: We could have evaluated shorter periods 10 or 5 year periods but we would have lost statistical significance with these shorter intervals.

Comment: What is the justification for the selection of the three time periods included in the analysis of hypothesis 1? Was there a consideration to isolate unique time periods of major change in land development, policy, or management techniques (egTMDL, stormwater treatment, land capabilities - TRPA policies**)?

Response: We didn't consider management actions when selecting time periods for analysis because we don't know how long those actions would take before any measurable effect on clarity would be noticeable. We needed a record sufficiently long to reveal statistically significant trends.

A separate analysis was done on 5- and 10-year periods and as expected, significance (p-values) were greater than 0.05. This value is commonly considered the threshold for identification of statistical significance.

Comment: How was the monthly analysis done? Did it include just the measurements taken in the month? I believe UCD interpolates between measurements to calculate the weighted average clarity over the year.

Response: The analysis was done by taking the median values for each month, then performing the trend analysis on the median monthly values.

Comment: The loss of winter clarity in the first 15 years is really large. Do we know if something happened with mixing?

Response Nothing appeared to have happened with mixing during that period. But it was an active period of urban development, and that is precisely the period when Mysis were introduced and cladocerans disappeared. Any materials washed in from urban areas during winter would not have a way of being removed. Unfortunately, there was not a routine monitoring program for Mysis and zooplankton at that time.

Hypothesis 1: Clarity is controlled predominantly by the distribution and (volumetric) density of fine particles in suspension.

Comment: Looks more like less abundance at greater depth for March, although shallow depth is diluted compared to before/after March.

Should it be mentioned here that the highest abundance occurs at less than 200 ft during summer stratification, to setup subsequent sections?

Response: More particles appear at depth (and less at the surface) in March because of the vertical mixing.

Comment: Somewhere in the background, need to clearly define abundance and counts so the reader knows you mean something different, or if there is no difference between the two, suggest using one term only throughout.

Response: They have the same meaning; we've attempted to clarify this.

Comment: Do you mean particles don't occur above this size, or that analyses have not been done for above this size? Also, aren't diatoms above this size, as stated in previous sentence?

Response: There are very few particles above 5 microns, percentage-wise, and as they get larger they tend to settle out of the water column rapidly. Also particles above this size range have very low scattering efficiencies, meaning that they do not impact clarity significantly.

Comment: Why not just show which measurements are Cyclotella and which are FSP? Why is it important to distinguish Cyclotella counts > 1 million cells.

Response: Per fig 3.7, Cyclotella numbers are an order of magnitude less than total. If plotted alone they won't be visible. This was a way to look at effect of total number of particles and also see if larger number of Cyclotella cells makes a difference.

Comment: Does the lake depth, season, or particle size need to be specified? Seems there are some nuances here that could confuse folks, such as figure 3.8 showing a significant correlation in clarity (inverse Secchi) and total particle count. This is why it's important to summarize the most significant trends and correlations in words or bullets.

Response: Added the word annual to clarify.

Hypothesis 2: The change in trend of winter clarity is a response to decreasing fine suspended sediment concentrations resulting from load reductions.

Comment: Given the hypothesis and the length of the LTMP data record I would have expected the analysis to consider longer term load from streams and when the change in winter clarity trend occurred. This approach would seem to be further supported by Fig 3, and the suggestion that stream and urban loads are highly correlated.

Response: We don't have data suitable for computing continuous loading from streams. Observations of fine particles in streams are infrequent and often miss peaks in the inflow hydrograph.

Comment: This is good info but the intent of this hypothesis was to determine if load reductions are occurring, and determining if there is a correlation with clarity.

Response: Understood. But we have no data on continuous loading of fine particles from streams. So we got what we could out of sporadic particle counts.

Comment: Disagree - the intent of the hypothesis was to determine if load reductions are causing reductions in fine sediments that is resulting in winter clarity improvement. Double mass plots over the period of record could help lend insight if loading rates are staying the same or changing through time.

Response: As noted above, the available data do not allow meaningful quantification of loading of fine sediments from streams.

Comment: Of course this is always desirable, but do we have a sense of how much uncertainty (+/100%, +/1000% ?) we have due to stated lack of temporal resolution, relative to what we are trying to quantify?

Response: The uncertainty depends on when sampling occurs, how often, and how well this sampling represents the actual inflow hydrograph. Two days per month is very low resolution – consider how poorly the mean of two instantaneous observations would represent a monthly mean of a variable such as outdoor air temperature. With enough assumptions we could estimate the error bounds but we did not do this.

Comment: What's our point about these streams being the best predictors? Also, what about the streams that are not currently monitored?

Response: We had to limit ourselves to consideration of streams for which data are available. There is an implicit assumption that there's no unmonitored stream whose input would overwhelm the loading patterns evident at the monitored streams.

Comment: Is 'should be expected' the correct phrase, or 'could be expected?' I can't imagine that a 400 – 900% error should be expected, which implies, at some level, that this error is acceptable

Response: Changed, as suggested.

Comment: As stated previously, another way to look at this question is why is winter clarity stabilizing or improving? Are there other factors that could be looked at besides fine particle loads. Are there specific recommendations that can be looked at implementing to evaluate if load reductions are working as intended. As stated previously, this project is to form the basis for the clarity modeling project. Although the data are inconclusive for this project assessment, what data would be useful for input into a model that can lend more insight into if this hypothesis is true or false?

Response: We were not asked to find the reason for winter clarity changes. We were asked to investigate whether it was a response to reductions in fine sediment loading. We also were not asked to develop recommendations for action (and some on the work team are not allowed by their employer to provide that type of recommendation). But we will note things that could be done to improve the ability to answer the science questions.

Comment: This hypothesis looked at much more than winter clarity.

Response: We're simply restating the hypothesis that was provided to us here. The focus of the work described above was sediment loading from both streams and urban areas. And since we really couldn't do what was asked, we stretched the boundaries a bit to look for useful or at least interesting results.

Comment: Double mass plots could be looked at to determine if loads from streams are decreasing over time. Unfortunately, the urban monitoring record is too short to enable this analysis.

Response: Domalgalski et al., 2020 have shown that trends in SSC have declined in LTIMP streams over the period of record. Presumably similar trends exist in the fine fraction, but data to confirm this are not available.

Comment: Again, if possible, it would help to give some indication of what would be necessary. I'm not saying you need to do a power analysis, but what other streams should be monitored to definitively answer the question?

Response: we did not undertake an effort to identify which of the unmonitored streams should be monitored. Simply adding more streams would not be enough. It would also be necessary to increase the temporal resolution of measurements.

Hypothesis 3: Changing hydrodynamic conditions within the lake are increasing thermal stability and resistance to mixing.

Comment: How does this compare to wavelet analysis?

Response: Wavelet analysis permits exploration of the frequency response over space (with depth). This was not the purpose of this analysis, so we utilized simpler Fourier analysis

Comment: This is a rather simplistic analysis of mixing. A single maximum value in summer hardly characterizes the temporal and spatial mixing processes or extent of mixing.

Response: The approach we took had to be scaled to the time and resources available, as well as to the precise question being asked. The hypothesis we were asked to address was "Changing hydrodynamic

conditions within the lake are increasing thermal stability and resistance to mixing." Although it is a far more interesting and complex question, the temporal and spatial mixing processes were not the focus. The annual values do indicate the trends of stability and resistance to mixing.

Comment: This is pretty definitive – is this the over-arching conclusion? Blends two hypothesis (particles and stratification)

Response: It blends more than two hypotheses. For example, particles are changing due to changed hydrology driven by climate change. Particles are increasing due to the removal of the principal grazers (Daphnia). Stratification is altering where in the water column particles are being introduced (i.e. the extent to which they are above or below the Secchi depth) as well as favoring small algal cells that don't settle out as fast as larger algal cells. Deep mixing extent appears to also be changing both in its depth, frequency and timing. What we are trying to convey is that all these factors are interlinked and with year-to-year variability different factors have a different relative impact each year.

Hypothesis 4: The trend in summer clarity is a result of earlier, prolonged, and more intense stratification.

Comment: This isn't really a change caused by earlier stratification.

Response: The depth at which the stream (and urban) flows insert into the lake is an interaction between stream temperature and the state of ambient stratification in the lake. If the latter is changing then the insertion depth will also change.

Comment: Is this depth sufficient to assess the trajectory of the inflow?

Response: It is not ideal, but it has the advantage of having the high temporal resolution that is needed. We believe that it represents the dynamics of diurnal stratification changes within the epilimnion and that is what is most important here. Once the inflow plunges below the Secchi depth, it is not impacting clarity.

Comment: Why is the analysis so limited? The work is intended to assess long-term changes, no?

Response: The data we need for this analysis is only available since 2015. What we are showing is the process of how lake stratification and stream temperatures interact. Both of these are changing over time (as shown in previous sections).

Comment: One of the summary findings indicates that higher frequency of occurrence of insertion overflows coincide with reductions in clarity. It's difficult to interpret this from this table. Could the data be plotted to illustrate this a little better? It is interesting that the BN's in this table are all much higher than the threshold of 5.

Response: That latter observation is also evident in Fig 6.4. The conclusions have been slightly modified to say that the insertion depth is a factor. The important thing that is also made clearer now, is that changing lake stratification and changing stream conditions have the potential to alter where particles are introduced.

Comment: Seems hard to disentangle the impact of greater particle loads from 2017 on (as present above) with the BN analysis presented here.

Response: You are absolutely correct. First and foremost, 2017 is an exceptional year and probably should not be used as a guide to most years. The point of this exercise was to show that the changing conditions in the streams (vis a vis timing of spring runoff, more rain and less snow, etc.) and changing lake stratification conditions (timing and strength of stratification) do impact where the stream particles are introduced and what their impact will be on clarity. While there is not a long enough record to show how this has actually changed, it does indicate that understanding (and measuring) this going forward is important.

Comment: Surface Lake data exists for Tahoe over a longer period of record, although not sure the resolution. Can the temperature from a decade or two ago be used to determine the relative extent of overflow or plunging as compared to more recently (2015-2019)? Can something be said about the documented change in surface temperature over the last several decades and how that could change insertion depths and stratification timing and affect clarity through time? Generally, the tie to climate change as a driver could be strengthened.

Response: Yes, lake surface temperatures do exist for a longer time, but stream temperatures do not. It is not the absolute value of each that matters, but the **density difference** between the lake surface and the stream water. This is a far more complex problem than it may appear to be. During spring (highest runoff period) stream temperatures can fluctuate more than 6 deg C (12 deg F) during a day, and be close to 0 deg C for a large part of the day. At temperatures between 0 and 4 deg C, the stream water is always lighter than the lake surface water.

Part of the reason for this analysis was to indicate how important and how sensitive this process is, and how important climate change is as an INFLUENCER. Climate per se may not be a driver, but it has a very large influence on all the processes that do drive clarity change.

Hypothesis 5: Ecological (food web) interactions are causing changes in the trends of seasonal or annual clarity.

:Comment: Is Lake Tahoe a case study in this paper, or Cyclotella occurrences in other lakes? Could strengthen the case if Lake Tahoe, or consider generalizing the sentence if not '….reported a general trend for alpine lakes that among diatoms…..' for example, depending on area studied

Response: That paper dealt specifically to Tahoe, although the experiences with Mysis in other lakes were described.

Comment: I don't understand. If changes in Cyclotella abundance is due to climate change and thermal stratification, then why are we talking about Mysis? This needs to be reconciled.

Response: This is the introduction. This is resolved in the main body. Winder and Hunter attributed increased abundance in the period 1982-2006. They first appeared in significant numbers following Mysis introduction as stated above. So two things are happening – Cyclotella start to appear in large numbers in the 1970s and on account of climate change they become more dominant. Without their appearance in the 1970s the climate change connection would be moot

Comment: But we conclude above that Cyclotella is only responsible for 29% of the variation in clarity deviation. There's a whole lot more happening than Mysis eating cladocerans, which may or may not have encouraged Cyclotella numbers. I hope we're able to show selective foraging by cladocerans below.

Response: We are saying here that CLARITY improved, nothing about Cyclotella. You are correct, and as described below, Mysis also consume all fine particles. So the change in clarity is likely due to greater

removal of them due to cladocerans. Nothing hydrologically or meteorologically different was happening in EB compared to Tahoe over this 3-year period. The only difference was the disappearance of Mysis and the reappearance of cladocerans and a doubling of the clarity in EB.

Comment: Seems rather strong assertions.

Response: Yes, it is strong. But the capacity for particle removal by cladocerans is far larger than the numbers present in Tahoe. Not saying the only factor, not saying the largest factor, but definitely a large part as opposed to a tweak around the margins

Comment: Should show plots of the data. What were cladocerans populations like during that time period?

Response: There are very few data, and in the interests of reducing the length of this report we chose to simply state what the range of data were over this six-year time period from 40 years ago. Cladocerans were near-zero in nearly all samples.

Comment: Some statistical analysis associated with these observations is needed. Why isn't a correlation between Mysis and the other three species run for this? It should be.

Response: It is more of a presence –absence assessment. It is not so important whether it is Bosmina or Daphnia. Rather, the presence of cladocerans coincided with increase in clarity

Comment: Should acknowledge the relationship is not statistically significant.

Response: You may be missing the point. The presence of Mysis themselves does not reduce clarity – for one thing they exist deeper than the Secchi depth. It is the fact that they remove the cladocerans. They are highly correlated and significant.

Comment: This tells us that Mysis concentration has nothing to do with clarity. I don't understand why this hypothesis isn't rejected.

Response: This is a good example of where statistics can be misleading. The Mysis are generally present below the thermocline, which in Emerald Bay is usually below the Secchi depth reading, i.e. they are spatially separate. They do not directly affect clarity. The connection between the two comes via the Mysis predation of cladocerans. The data in Figure 1, along with a huge body of literature from Lake Tahoe and other lakes has established that.

Also, as the topic of this hypothesis makes clear, this is about a food web, which take time to change. It is not as instantaneous a response, as for example, a relationship between stream flow and turbidity would likely be. When the measurements commenced, Mysis were absent. It is not known how long they had been absent. Over time – several years – cladocerans numbers increased, and as Mysis returned, cladocerans numbers tailed off. The rate of decline of cladocerans is similar to the reported rate of decline that occurred when Mysis were introduced in the 1960s. The point is, there is a long term process occurring, with variable time lags, and a short observation window. The lack of statistical significance merely reinforces that.

Comment: Correlations are the backbone of testing this hypothesis. While cladocerans may have been near zero, Mysis number weren't zero. What is the correlation between Mysis and the other cladocerans? What about Mysis and clarity? These are important analyses, without which we just tell a story.

Response: There are no other cladocerans. If the cladocerans Daphnia and Bosmina are zero most years, and incredibly near-zero other years I don't know what a correlation would show. It would likely say that

clarity and cladocerans numbers are not correlated. But we do know that before Mysis were introduced cladocerans were the dominant zooplankton, and clarity was high.

Comment: These charts are too small, making it very difficult to read. Why do concentrations of cladocerans cycle so much?

Response: We will make them bigger. The Cladocerans cycle because of their annual reproductive cycle. They appear in spring and summer in highest concentrations. Bear in mind that the populations have just reappeared with the absence of Mysis, and so there may be inherent instability at this point in time

Comment: What's the estimated density we need to start seeing an impact on clarity

Response: The earlier literature from Tahoe showed that when Mysis numbers dropped below 27 per sq. m. the cladocerans could coexist. The actual numbers of cladocerans you would need is something that I am not sure of, but as evident in Fig. 7.1, at values above 3000/cu.m clarity was rapidly improving. Later in the report we provide a little more guidance on this.

Comment: I thought this data was suspect by a factor of two. Why, then, does this analysis use continuous data? I would expect something different, like a logistic regression analysis or something using presence/absence methodology. It's not intellectually honest to say that the data isn't useful for certain analyses, only to use it for those analyses.

Response: See comment above. No "analysis" was done, and no intellectual dishonesty performed or intended. The data are simply plotted, and after the 1970s there was virtually no Fragilaria observed in the system. For the earlier period, prior to the introduction of Mysis, there were values over 200 cells/ml. The uncertainty means that number may actually only be 100. Doesn't really change the information in the figures. I possibly should have added that there were some zeros in that early period, but that is due to the seasonality of phytoplankton populations.

Comment: Because of the overall drop, aren't there fewer small cells today than there were in 1985?

Response: The figure that was here originally had an error in the 1985 data – the cell counts were off by about a factor of two. The corrected figure still shows that 1985 had large cell counts (approx. 3,000 in August) which is higher than the other later years shown, but not necessarily out of the ordinary. If you look at Fig 3.7, you will see that in 2009, 2010 and 2016 the small Cyclotella cell counts were in excess of 4000 cells/ml (higher than in 1985). The point is that the algal blooms are highly variable year to year and can quickly outnumber the inorganic particles while the bloom is taking place.

Comment: Unless I misunderstood the disclaimer above, the 1969 data need to be removed from this analysis because we admit they are erroneous.

Response: Likely my poor wording is to blame. The uncertainty (not erroneous) relates to whether the phycologist doing the counts actually counted only live diatoms, or whether she counted live and dead. Here dead refers to the empty silica frustule that is left behind and that will eventually sink out or dissolve. Sometimes they counted both. Sometimes just live. Our records indicate that typically there are approximately equal numbers of live and dead diatoms. Hence the factor of two uncertainty. These figures are referring to ALL phytoplankton, not just diatoms. If you look at the right hand (orange) axis, even factoring in a factor of two uncertainty (halving or doubling the values) for Fig 7.7a, the percentage of small phytoplankton (i.e. those that can physically impact clarity) is very different than what is observed for the years afterwards. Likewise, we are talking of 2-3 orders of magnitude differences between pre-Mysis and post-Mysis. The factor of two uncertainty is not changing the conclusions.

Comment: The % small (right y) seems to be stable since about 1985, but clarity has continued to decline since then….

Response: This is just showing for certain years the shift in percent small algae. Part of the reason clarity has declines is on account of ALL small particles (biotic and abiotic). Cladocera have the ability to remove ALL fine particles of a size that impacts clarity. Also, phytoplankton blooms are very episodic and occur some years and are low in other years. Similarly, in dry years the concentration of terrigenous particles are low.

Comment: Big jump in concentration appears to occur between 1969 given the aforementioned issues in identification are we sure this jump is real?

Response: It is real, and that is the whole point about the introduction of Mysis. Tahoe used to be dominated by large phytoplankton that did not impact clarity. When Mysis came and removed the cladocerans and large algae, the small algae were able to grow without grazing controls.

Comment: For each of these years, was there a correlation between concentration and clarity? Was there a big effect of the very large spike in 1985 on summer clarity? This is an order of magnitude higher than currently seeing more recently in 2002 and 2018.

Response: These are good questions. We haven't computed the correlations as we would be comparing the integrated Secchi depth value with the discrete Cyclotella count at 5 m. It would also not take the inorganic particles into account.

Comment: This is all speculative (referring to mysis grazing).

Response: The data on Mysis diet for Tahoe does not exist from the period in question. However, I thought that providing literature citations to what Mysis have been observed to eat in a range of other lakes would serve to indicate that the hypothesis is plausible. Clearly if the data from every other lake showed that Mysis did not eat algae, that would make it less likely that they would be different at Tahoe.

Comment: What explains the extremely high populations in 1985 but then goes down an order of magnitude through the last two decades? Wouldn't you expect that concentrations would have gone up since that time, or at least stayed steady? What is the impact of say 1000's of cells/ml have on clarity versus 100s of cells/ml, in terms of secchi lost from 0 cells/ml?

Response: Phytoplankton blooms are very episodic and should be viewed as a continuum over years. One month can have extremely high values and very little the rest of the year. It is not possible to say why 1985 was so high. But the data have been checked. I do not know the impact of specific cell numbers. Also difficult to assess as the number of cells throughout the water column down to the Secchi depth will have an impact. We measure at discrete depths (e.g. 5 m) and use that as an indicator.

Comment: The correlation of Mysis, cladocerans, and Cyclotella seem pretty solid, certainly for Emerald Bay, and likely for Lake Tahoe. Hard to delineate which process is truly affecting lake clarity in the summer though, given the previous presentations on fine sediment loads and stratification. Suggest phrasing this more as another influence contributing to declining summer lake clarity, as in the initial question posed. Revised wording to show a possible example.

Response: That is a good suggestion and we will modify accordingly.

Comment: This is all interesting storytelling, but it doesn't belong in this paper. We need to test hypotheses and learn using the scientific method.

Response: The scientific method includes awareness of what has come before and applying that knowledge. All the data that we would like to have for Lake Tahoe does not exist. There is no reason to believe that the grazing rates for Daphnia that have been determined by other researchers and used for decades in the literature should be inadmissible.

Comment: Again, why is this here? This should contain a discussion of the results, not a discussion on what might or might not be.

Response: Respectfully disagree. We know that fine inorganic particles scatter light and cause a decline in clarity – most of this report is about that. Recognizing and showing evidence that cladocerans ingest these fine inorganic particles is a critical point. It means that they are one of the few (possibly only) means of rapidly removing such particles. The only other mechanism I am aware of is aggregation, and on account of Tahoe's low ionic strength that is a slow process.

C**omment**: Based on what result?

Response: The result that when the food web in Emerald Bay was radically altered, the annual clarity was greatly changed. The record of improved clarity for that event (2+years) is too short to say anything about the seasonal clarity impact, but given that Daphnia would be largely present in summer when clarity has been diminishing the most, suggests that seasonal improvement could also be expected.

Comment: Is this the only option? What about inoculating the lake with Daphnia and Bosmina directly? Management direction seems out of place with the rest of the document.

Response: The science is pretty clear that at high levels of Mysis (that currently exist) Daphnia and Bosmina are grazed heavily. It appears that a level of less than 27 Mysis/sq. m is what is required to allow co-existence. That is a pretty explicit target for management, and an option to consider along with the current strategy of removing fine particles from runoff entering the lake.

C**omment**: There is much to learn before we can jump to this conclusion.

Response: There is definitely more to be learned. But Emerald Bay provided a 5-year natural experiment from which this hypothesis was developed. That is a pretty good start that should be built upon. Given that monitoring has continued in Emerald bay, and that Mysis have remained high, cladocerans low and clarity correspondingly low, there is now more data than there was before. I agree with you that we need to learn more. It would seem that the literature is replete with information on Mysis, cladocerans etc., their life cycles, eating habits, grow rates etc. The knowledge that is missing, in my opinion, is whether we can deliberately remove the Mysis and get the clarity to return. Emerald bay provides a good pilot site, as it could potentially have Mysis reduced sufficiently in 3 months.

Other Variables that Influence Winter and Summer Lake Clarity

Comment: Though caveats are noted wrt to these statistical analyses, it does seem odd to do a 'shotgun' set of analyses when we have mechanistic understanding to guide our thinking.

Response: This approach was taken because of the focused questions raised by the agencies were leaving out other variables from consideration.

Comment: Would be good to list the abbrevs in an appendix and reference the appendix in the figure title…I didn't check to see if they are indeed listed in an appendix

Response: Created two tables defining variables for winter and summer in Appendix 4.

Comment: Were tests for cross correlation among variables done?

Response: Yes, many of the variables are correlated. The variables that are significant aside from max and peak BF are not.

Comment: How about lake and air temp? were these included in the analysis? A list of parameters analyzed would be beneficial.

Response: Lake temperature was included in the correlation analysis. Air temperature was not.

Comment: Has winter mixing changed over time to help explain the change in the winter clarity trend from declining to level or slightly increasing?

Response: I evaluated trends in maximum depth of mixing and it was not responsible for a decrease in the slope of clarity during winter. In fact, Fall trends mimic Winter trends in that they are leveling off. One might presume that mixing is not responsible for fall trends.

Comment: The analysis appears to focus primarily on explaining clarity in a single year as a function of other variables in that year. As I understand clarity it is a multiyear complex issue, and the management program. Thus I think the more important question to be addressed here is what are the drivers that are changing that are causing clarity to decline.

Response: To the extent that clarity declines are affected for months after large events, it is true. Clarity can be impacted from one year to another. The most influential drivers for clarity trends may need to be resolved with a more formal multi-regression model. This analysis is simply reporting that questions evaluated in this report were focused on particles without identifying other variables that are important. The correlation among these variables with clarity are indicative that they are too are also changing.

Comment: Were tests for cross correlation among variables done?

Response: Yes, but the results presented in the report focus on analysis of correlation between pairs of time series with zero lag.

Comment: Is this normal in long term climate record? Be good to present the classification of runoff for the full data record here.

Response: We only evaluated the declines associated with the last 20 years. 20 years ago, clarity during summer also leveled off. What appears to be important is understanding the hydrological conditions that caused a reduction in particles during winter.

Comment: How about doing analysis similar to 8.3 but over last five years – due to wet years this would suggest that clarity would be less than average

Response: The wet years of 2017 and 2019 were substantial water years and would have an effect on the 5-year trend. Not sure why a 5 year is important given the trend slope with n=5 would not be significant.

Limitations

Comment: Why are there no concerns expressed for hypothesis 5? I really think it's premature to have included hypothesis 5 in this report. There really isn't enough data to support the conclusion that is reached.

Response: we have modified the wording of our report in several places in ways that should alleviate this concern. It is acknowledged that the available dataset does not allow for validation of some of the explanations provided in the discussion of hypothesis 5. We have attempted to provide plausible explanations that are likely to warrant further investigations and are in many cases at least consistent with available observations.

Comment: To what extent is re-suspension of particles near shore contributing to FSP number offshore? Further characterization of the optical properties of FSB would seem worthwhile since both the theory and measurement capabilities have improved.

Both good comments, but we did not have data available to investigate the first question, and the second goes well beyond the scope of our effort – it would need to be a separate research project.

Comment: One would expect that urban flows are warmer due to warming from impervious surfaces. Was temperature looked at? This could affect insertion depths of runoff. Was timing of urban runoff looked at? Was relative volume compared to stream runoff looked at? Watershed model results considered?

Response: We did not look at temperature of urban runoff. These are good questions but would require a larger effort than was supported by this project, something that could be looked at in subsequent studies. Pollutant Load Reduction Model (PLRM) results were considered and included in an appendix.

Comment: Are all the data used in the analysis, existing, publicly available data or are there unpublished datasets being used? If the latter, should state that its unpublished when that dataset is mentioned in the report

Response: We're saying here that data are being provided in spreadsheet form. The remaining issue is where to post it. We'll resolve that while the report is in review. UC Davis maintains a publicly available data repository and we are investigating the feasibility of posting the data there.

Discussion and Conclusions

Comment: The work was intended to determine the reasons and causes for the divergence in the winter improving clarity trend and steady summer decline. Please keep in mind that the hypothesis were agencies best attempt at framing questions that would get at this overall charge.

Response: We did not interpret our charge that way, at least initially. We worked hard to address the specific questions we were provided.

Comment: The report could use more robust conclusions.

Response: Revised accordingly.