Appendix G

Comments on LTADS Work Plan and Staff Response to Major Comments

Comments from Professor Thomas Cahill
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Comments for the UC Peer Review Panel and the California Air Resources Board on the Work Plan, Lake Tahoe Atmospheric Deposition Study (LTADS) Version of June 10, 2002

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Overview

O-1. I am appreciative of the enormous amount of funds being expended in so short a time primarily on the single question of atmospheric inputs to Lake Tahoe water quality. These funds dwarf all the efforts of the past 40 years, and will doubtless add enormous amounts of valuable data to the existing knowledge base.

O-2. It is hard to think of even a single study of deposition phenomena that was entirely successful, even those in which years of effort were expended. It is an inherently difficult area of atmospheric research. This study has the added problem of an extremely short time span. Yet the discussion of earlier deposition studies in this document is weak. I am referring particularly to the extensive studies at Lake Michigan and Chesapeake Bay. This literature should be incorporated in any final report.

O-3. The program attacks the problem with a large number of parallel studies, which with complex phenomenon and short time span, is probably wise. However, at times this seems to approach a "shotgun" pattern with resources spread very widely and thinly. At other times, new techniques and approaches proposed and relied upon, but they need to be backed up with parallel traditional approaches should the newer ones fail in the extreme meteorological conditions and low pollutant concentrations likely to be encountered in the Lake Tahoe basin. This is going to be a very difficult program to coordinate.

O-4. All investigators must appreciate the key role that their results will inevitably play in the legal challenges that will arise as TRPA attempts to use these data in its mitigation plans.

O-5. This program benefits by bringing new groups supported by unprecedented (for Lake Tahoe) funding levels into the basin, but because of lack of past experience, they may well encounter unexpected problems exacerbated by the extremely short time scale of the program. It is going to require close and active coordination.

O-6. This sprawling document is written in several different styles and thus is difficult to review. There is so much in it, some excellent and some weak, some of which is given with little supporting detail or cited literature, some with great detail. Strong promises are made that are both hard to evaluate and will be hard to keep. Some statements are confidently made without detailed citation that I believe may be at variance with prior published data and reports.

Major Technical Concerns:

1. The presence of the intense (up to 1OC) low level (<50 m) inversion present at Lake Tahoe in all seasons, but especially intense in Fall and Winter, has not been adequately factored into the study. This inversion provides a major barrier to vertical transport, both suppressing dispersion of pollutants emitted under it while preventing pollutants present above the inversion (from both transported and local sources) from reaching the lake.

I would recommend extensive efforts to continuously determine the presence and strength of this inversion both on the shore line and out in the lake throughout the entire duration of data collection.

2. The proposal mentions at several places the desirability of establishing the causal relationship between gasses and particles in the air and the rate of deposition to the water surface, but then shies away, sometimes for financial reasons, from performing causal experiments.

Such experiments should be done at at least two sites, the one in the north that is planned but also one at South Lake Tahoe, source of heavy lake inputs and site of much prior air quality work (ARB Sandy Way and TRPA SOLA), and during at least summer and winter seasons, to anchor the associative data from the dispersed sites.

3. Too many of the measurements have an averaging time far greater than the strong diurnal and even synoptic patterns at Lake Tahoe. Major assumptions will

have to be made regarding aerosol composition to match the highly time resolved mass data.

There should be a specific effort to evaluate day-night compositional changes, perhaps by using the TRPA aerosol data and earlier work.

4. Many of the methods proposed for this study have never been documented as effective in the very clean conditions that are present at Lake Tahoe at all sites during parts of the year and at some sites throughout the year.

The individual groups should be sure to have the prior literature to guide sampling and analytical protocols.

5. **The greatest resource for Lake Tahoe deposition studies is the long and stable deposition measurements of the Tahoe Research Group, which provide the basis of many TRPA regulations.**

More effort should be expended in trying to understand the nature of the TRG measurements (wet surfaces versus dry, ...) so as to tie the present results to the past decades of TRG data.

I have included examples of the data upon which these concerns are based in the Appendices, and mentioned them at specific places in the review. I am also including a major un-cited study, the USFS Watershed Assessment Study (2000) that summarizes much of the earlier data.

General Comments:

The very scale of this effort has a number of consequences that should be considered:

G-1. The level of effort will catapult ARB personnel into key roles in the inevitable law suits against TRPA when the conclusions of this study lead to attempts at mitigation. The obvious people to be subpoenaed in this effort are the leading ARB scientists.

Testimony at this level will require a considerable level of scientific credibility and knowledge of past and current efforts at Tahoe and similar programs elsewhere. Is the ARB willing to make this long term commitment years after the funds are expended?

There should be a designated Lake Tahoe ARB “expert” capable of providing the wide ranging testimony necessary to support TRPA litigation.

G-2. The coordination of the various parts of the program is critical to its success. The very short time period and the many sub-contractors will require a strong leadership -~ team that should be made aware of its responsibilities and given serious authority to make the program changes to meet this daunting time schedule.

G-2**a) I would like to see a much more explicit coordinating panel with serious capabilities to respond to unexpected circumstances, including reserve funds to seek alternative options if a scheduled program element falls behind.**

G-2b) I would also like to see much more detail on the critical time lines.

G-3. **This is not just a scientific study, it will be the bedrock for defense against the inevitable litigation. All investigators should keep a close eye on the nature of the problem being addressed and how their measurements resolve them. There should be data upon which to support the proposal plan choices and the effect (if any) on the overall conclusions.** At numerous places in the study, the writers lay out an analysis of what needs to be done based on the science, and then back off for financial reasons. This is unconvincing with a study so large, and opens the final conclusions to serious attack based upon the staffs own analysis. In this regard, the proposal, including earlier drafts, and the comments of the Peer Review Panels, are fully “discoverable” in any law suit. Thus, these criticisms will be raw material for attacks on the conclusions of the study.

G-4. Some parts of the study plan have the appearance of being lifted intact from earlier proposals and studies, so that in many instances one could replace “Lake Tahoe” by “Folsom Lake” and scarcely see a change in the plan. Integration with the special factors at Lake Tahoe (low level inversion, strong diurnal patterns, relatively to very cle conditions) and with the prior 40 years of work are poor, references are inadequate, and many prior assets that could be useful in the final analysis are ignored. **More effort must be made to integrate prior data into the present plan as an antidote to the short duration of the data collection effort and its consequent sensitivity to unusual meteorological conditions during the short data collection period.**

Much of this earlier work was in fact either:

1. performed by the ARB itself, (Lake Tahoe Study 1974, west slope Ozone Tracking circa *1985,* current air monitoring at South Lake Tahoe, 1977-present, and the 1999 Tahoe infrastructure enhancement), or
2. funded by the ARB (UCD/ARB Studies, 1977-1979, including weekly averaged PM10 and PM25 samples at numerous sites over 1□ years, aircraft flights, and meteorological data, the UCD Ozone / Valley Foothills circa 1995).
3. The rest was supported by other agencies such as the US EPA (Basin Carrying Capacity 1981), US Coast Guard (vital meteorological data on lake, 1966-1967), USGS (Laird’s snow survey circa early 1980s), US Forest Service (Sierra Nevada Ecosystem Project, 1997, Lake Tahoe Watershed Assessment 1999), TRPA (Visibility and Aerosol Monitoring, 1988-present, TRPA Scoping Document

2001), IMPROVE (aerosol monitoring, 2001-present), DRI (nitrogen studies (2000-2002), and the NSF and Lahontan in some uncited work of the Tahoe Research Group (1958-present).

As an example of under-utilized data, combining meteorological data, simple line source diffusion models, and CalTrans traffic data, with the ARI3 South Lake Tahoe Sandy Way site data, could give detailed emission rates from Highway 50 for the past 20 years. In another example, the TRPA visibility program has splendid data on in-basin and out-of-basin aerosol sources since 1989, published but not cited (Molinar et al, 1994) in the proposal. Another example is the snow survey of USGS (Laird et al) which is directly relevant to wet deposition.

This information could be enormously helpful in designing the study and must be included in any final report.

Specific Comments

S-1. Meteorology

The unusual meteorological conditions in the Lake Tahoe basin have not been adequately incorporated into the study design. In particular, the very intense (up to 10 C), stable (see bulk Richardson's numbers, UCD/ARB 1979), but low level inversion at 30 m ±15 m above lake level which occurs frequently throughout the year (see US Coast Guard meteorological data, Tahoe City, 1966-1967, for fraction of "calms") de-couples sources and measurements above this level from those below it. Details on this level are known to all in the basin, (even the squirrels climb to the tops of pines to warm up in the morning), supported by photographs (see USFS Watershed Report), and directly measured by aircraft in the 1979 UCD/ARB Final Report (with extensive appendices).

This inversion traps sources emitted under it close to the lake, and since it is usually accompanied be slow terrain driven down-slope winds, pushes these low level pollutants over the lake each night and morning. Conversely, all sources above this inversion, including those transported from the Sacramento Valley, can not move down through this level until it breaks up, generally in late morning. An excellent example is shown in the USFS Watershed Assessment Final Report, during which smoke from a prescribed fire at Spooner Summit slid down-slope and over the lake, but never reached the lake itself due to a clean layer < 100m thick that allowed one to see across the entire lake width under the smoke.

Measurements and models that do not take this unique meteorological phenomenon into account are doomed to fail. Re-evaluate the measurement and modeling effort to focus on pollutant sources and transport within this trapping level. The on-lake sampling, for example, is critical in this regard since it is the only way to see how far the pollutants move out over the lake, a vital parameter in the lake flux calculations.

S-2. Emission source data

S-2 a) Reliance on existing source data (AP 42) for CMB for Lake Tahoe deposition studies would be a serious error. For example, the 1979 ARB studies showed that road sanding crustal materials become ground very fine during the Spring, with the ability to travel much farther out over the lake. The recent work of Jenkins et al (JGR, circa 1998) on the composition of smoke should be incorporated into any smoke work. The Mini-vol program is mentioned as meeting this need, but no details are given on size and composition and the strong seasonal effect and short time constraints make this difficult.

S-2 b) The basin specific measurements of automotive input must be broad enough to cover gasses and particulate matter from all types of vehicles, especially diesels. I have seen relatively little prior work on these particles from the UC Riverside programs. These must be included, as the TRPA aerosol program shows important diesel contributions at South Lake Tahoe.

S-3. Deposition studies

This is the most serious specific weakness of the plan. The entire goal is to establish airborne input into the lake. Yet little effort is made to establish the causal relationships essential for modeling efforts. Factors that must be considered include:

a) Deposition on an event scale. Presence of a forest fire, for example, could vastly enhance deposition over a few days.
b) Deposition on the diurnal time scale. There is no doubt that in magnitude and type, deposition has a strong day-night difference (by an order of magnitude).
c) Deposition on a synoptic scale, especially during the storms of winter, and
d) Deposition on an annual scale.

The measurements of species in the air potentially capable of deposition must be made in such a way as to match the time-varying deposition process itself. Certainly the causal models operate at high time resolution, and then must be averaged to match long term field data, much of which is in 2 week averages.

Thus, the widely dispersed but simple deposition measurements must be supplemented by at least one site by measurements designed to establish causal relationships, air quality to deposition rate. Ideally, that site should be at South

Lake Tahoe, since this area is the largest single source of air pollutants with potential for lake deposition.

At this site, the question of the gaseous versus particulate components in deposition could be measured, not just modeled. One could easily have one of the dry deposition systems use air from which particles have been removed, and a second with gasses removed. One could have systems that collect only at night, and another only during the day. Some could have water as a surface, others flat and dry (a factor of 3 according the Jassby 1994), others with dry rough surfaces. There should be several versions of the deposition system, including the national deposition network units. There should be detailed measurements of gasses and particles, by size and composition, at the same site and times as the deposition measurements are made.

Note by the way that removal of twigs, insects etc. from the buckets must be justified and supported by experiments. It is very possible that these items, sitting in the warm water of the TRG system for a week, most likely with strong microbial action, contributed to the DIN seen in the samples. Certainly early studies show that at time both the lake (and Hi-Vol filters) are yellow with pollen (see ARB 1994 for pollen mass measurements), and thus certainly an input to the lake. The hydrological cycle is dominated by snow. The data of the USGS (cited and plotted in Cahill et al, 1996) should be incorporated into the estimates (see page 19, paragraph 2, page 23). Some of the comments is this section appear to be at sharp disagreement with the USGS work.

S-4. Transport

There is much more known about transport from Sacramento into the Lake Tahoe basin than is included in this proposal. The very fine particles present in the air after transport from the Sacramento Valley have extremely low deposition velocities, reducing lake impacts. In addition, the role of transport into the basin that moves out into Nevada without deposition must be included in the study. This occurs because during a good part of most days. there is a persistent low level inversion that blocks lake deposition. A strength of the work is the west slope sampling site at Big Hill.

Some of the aircraft analyses cited are in clear disagreement with the IMPROVE data of TRPA at BLIS and SOLA (page 44, 45). For example, the work plan notes that the phosphorus greater or less than 3.5 microns were reported as roughly equal. But phosphorus levels at BLIS and SOLA were extremely low in the sub-2.5 μm mode, much lower than those reported by the aircraft. This could indicate a de-coupling between elevated levels and lake level concentrations.

S-5 a. Trends -temporal

The ozone trend data quoted (stable at 0.089) can be misleading. Earlier work by Popejoy and team showed a rise in ozone at South Lake Tahoe during the late 1980s. Has this ceased? Better is a comparison between Tahoe ozone data, derived almost entirely from upwind sources, and the strength of the known upwind source regions, many of which show declines. What is the impact of the rapidly rising traffic on Highway 50 in the Sacramento Valley foothills, directly along the summer wind transport vector?

What about the occasional episodes of stratospheric ozone subsidence reported in winter?

It would be useful for trend analysis to reproduce the comparative analysis of Goldman and Cahill (1976) in the Journal of California Tomorrow.

S5 b. Trends temporal

The spatial trends of gasses and TSP particles were established by the ARB in 1974, matched to an extensive CalTrans traffic survey in 1976, and particles by size and composition were measured by the UCD/ARB report 1979. These provide both information on concentrations around the lake that could aid in study design as well as provide a valuable comparison to present trends 25 years later. Note that since 1980 there has been a relatively modest change in traffic in number and pattern, but a major escalation in home building.

S-6. Modeling

The modeling effort must have robust predictions that are capable of verification, not just fit as a free parameter.

The prior modeling done for the USFS Watershed Assessment, the Lake Tahoe Airshed Model (LTAM) is a simple gridded Eulerian model that has verification for smoke concentrations and gaseous nitrogen species. It should be compared to any model that results in similar parameters.

S-7. Ozone and forest health.

This section would benefit greatly from the analysis of the CARB (1974) and the report by Palmer at al (SNEP, 1996) on ozone damage on the western slope of the Sierra Nevada. These data were used to get an ozone damage threshold in Cahill et al 1996.

Comments from Professor Keith Stolzenbach
(University of California, Los Angeles)

July 31, 2002

Leon Dolislager
California Air Resources Board
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Re: Review of Lake Tahoe Atmospheric Deposition Study

Dear Leon:

This letter is to transmit my review of the Lake Tahoe Atmospheric Deposition Study per the Interagency Agreement No. 980004-TO between the University of California and the Air Resource Board. Note that my comments reflect my professional areas of expertise. In that regard, I have not addressed issues of air quality monitoring methodologies or atmospheric chemistry.

My review is based upon the two documents sent to me in June 2002:

(1) "Work Plan for the Lake Tahoe Atmospheric Deposition Study (LTADS), Cal/EPA and the Air Resources Board, June 10, 2002

(2) "Lake Tahoe TMDL Research Team Binder" [no organization or date specified]

Of these two documents, the former is a reasonably integrated presentation of the air-side studies proposed; the latter is a collection of individual projects dealing with water-side issues.

General Comments

- The overall plan to combine emission source definition with modeling and measurements is a sound approach. We have used it successfully here in the Los Angeles area. I believe it is the only way to go given the inherent uncertainties and limitations in both modeling and measurements – the whole is greater than the sum of the parts.
- I think my greatest general concern, reflected some of the specific comments below, is that the measurement and modeling efforts are putting significant resources into systems that will adequately address average deposition rates on the scale of the lake itself, but may not resolve smaller scale spatial patterns associated with specific sources or local meteorological patterns and/or events to the extent anticipated in the proposal. The importance of variability at these smaller scales is not totally clear, but the air side proposal has expectations that may not be completely met with the proposed sampling and modeling.

Specific Comments

- <u>Chemical Mass Balance (CMB) Receptor Modeling:</u> This technique is commonly used to attribute fractions of measured concentrations of pollutants at receptors to known sources. It works well particularly when the sources are well defined and have emission characteristics that are well defined. This is commonly the case for anthropogenic point sources, but less so for non-point sources, such as the "area" sources identified as the major contributor to constituents of interest to this study and for natural sources such as wind blown dust. I am concerned that, although the proposed air quality measurements will resolve chemical species and particle size fractions at the receptors, the characteristics of the sources will not be sufficiently resolved to allow this technique to yield meaningful results.

  The general principle of mass balancing will be used in the context of the modeling and can be used to make appropriate adjustments. For example, in Los Angeles, the regional air quality model replicated the observed atmospheric concentrations for all but a few constituents of interest. On the basis of this result, and assuming that the agreement with the other constituents meant that the model had the overall air patterns and deposition rates right, we adjusted the source strength of these constituents to achieve a match.

  Model results, expressed in terms of the flow of material between different broadly defined "boxes", can be used to understand the overall fate and transport of constituents of interest. This kind of mass balancing may in the end be as useful as the CMB modeling.

- <u>Deposition Measurement and Calculation:</u> In document 1 there is lots of discussion about the difficulty of measuring deposition using surrogate surfaces. The study proposes to calculate deposition using several methods based on local surface conditions (wind speed, etc.). I think it is important to realize that there are significant uncertainties in both these approaches. Surrogate surfaces, including the wet-dry samplers proposed for this study, are not good analogs of real surfaces, particularly vegetation, and measured deposition rates are correspondingly in error. However, we have performed sensitivity analyses with theoretical formulations that convince me that this error can be greater than a factor of two but is usually less that an order of magnitude. My experience with other techniques for estimating deposition based on local meteorology (such as the Bowen ratio method) leads me to believe that the relative errors for these methods are also in this range. So I think it is not valid to discard the notion of surrogate surfaces completely. I raise this point because simple surrogate surfaces, widely deployed, can yield information about the spatial pattern of average deposition rates.

  One of the surrogate surfaces we have used in Los Angeles is the water surface itself. There are specialized, but not costly, instruments for collecting the surface microlayer of a water body. Sufficient volume can be obtained to do chemical analyses, and the sample can be characterized by the ratio (usually in the range

10-100) of the microlayer constituent concentration to the bulk water concentration. Although this ratio alone does not quantify the rate of deposition, it can be interpreted as a measure of the spatial distribution of deposition. This kind of measurement could be of particular significance to this study given the high proportion of the area that is water surface.

Another point regarding deposition prediction is that, at least on land surfaces, there is always the possibility that deposited material will be resuspended. Our current study in Los Angeles is addressing this process in terms of trying to understand the extent to which the deposition “footprint” from a source is determined by a single deposition event for each particle, or whether a substantial portion of the particles are resuspended, perhaps more than once, leading to a wider footprint.

- Air Quality Modeling: This project is devoting considerable resources to a regional and local air quality model predictions. I note that about $500K out of the $1870K total budget is devoted to wind measurements alone for the purpose of model validation (although some of the $500K goes for air quality measurements also). The models to be used appear to be state of the art for regional air quality with a resolution of 4km. The proposal mentions increasing this resolution to 1 km to resolve local air patterns. I am wondering several things: first, have the physical parameterizations in the models been exercised at this scale? There are special physics involved in the way air flows around hills, for example. At even smaller scales (buildings) the prediction of local patterns is still state of the art. Second, do we know that a 1 km resolution will be sufficient to resolve local wind patterns? I could imagine a spectral gap in wind variability such that there was more important variation at a scale of 100 m than at 1 km. Third, I assume that the models capture general seasonal patterns, but to what extent do they include important meteorological events that may be important for deposition episodes (or resuspension episodes)? For example, in Los Angeles we modeled the Santa Ana wind pattern as a distinct event relative to the seasonal pattern. This is an issue of time resolution that parallels the questions about spatial resolution above.

- Air Quality Measurements: The proposed study discusses the possibility of relating observed air quality variations to known or hypothesized sources. On the other hand the air quality monitoring network, while spatially intense relative to usual air quality monitoring designs, may be too sparse to resolve source specific air quality patterns, which may be quite small (100-1000m) in scale. Deposition related to episodic meteorological events (locally high winds, etc) may also not be resolved spatially. The monitoring network is certainly capable of resolving variations on the scale of the lake, and these may be adequate for the goals of the project.

The proposed study is investing considerable resources in the resolution of diurnal variations in air quality (the BAM monitors). It is not clear to me that this investment is warranted. The lake water quality certainly does not respond to this time scale of variation. Techniques for predicting deposition are marginally capable predicting diurnal variations simply because the relationship between the deposition rate and the meteorological parameters that are changing at this scale are not sufficiently well known. And finally, deposition related to localized episodic events are not resolved spatially by the monitoring network (see above), so even if an air quality monitor detects a small time scale event, the spatial extent of it may not be determined.

One of the most critical aspects of this study, mentioned briefly at one point in the planning document, is whether the particles of interest should be the larger ones, which would carry the largest mass of material and thus constitute a major portion of the loading to the lake, or the smaller fraction, which would be most important in affecting water clarity. This is a key question that affects the focus of sampling and modeling. In Los Angeles, we have been working with the hypothesis that it is the larger particles. This has led us to be interested in deposition footprints with a smaller scale than would be associated with finer particles that disperse regionally. For larger particles, surrogate surfaces become a more accurate way to measure the deposition rate directly.

- Water side studies: I did not see in either document any mention of the connection between the dynamics of the plankton population in the lake and the residence time of suspended particulates. It is well known that biogenic packaging of smaller particles, either by ingestion or scavenging by sinking aggregates of cells, is the dominant mechanism for removal of suspended particle mass from the upper layers of a natural water body. Given the dual interests of this study in particulates and plankton, some consideration of this linkage is appropriate.

The water side planning documents discussed the issue of bioavailable phosphorous in detail and much of the sampling is focused on this aspect of the loading. However, I did not see much if any mention of bioavailabilty as an aspect of the air quality sampling for phosphorous.

The water side documents say fairly conclusively that the lake productivity is now nitrogen limited, although it was phosphorous limited in the past. On page 17 of the air side document it states the opposite in terms of priorities. On the other hand both studies seem to give relatively equal weight to phosphorous and nitrogen.

The water side documents describe a study focused on near shore turbidity. This implies that the turbidity in the lake is not homogenous horizontally and that conceptual modeling focused on vertical processes may not be entirely adequate. In addition, the study of near shore turbidity patterns should be linked with the air side study of likely sources.

There are several places in the water side documents where it is not clear to what extent the studies are coordinating with each other and with the air side study:

- ❖ The study of fine particles in streams should be coordinated with the measurement of particle size in the air.

- The autosampler component of the stormwater monitoring study should be coordinated with the study of fine particles in streams.
- The atmospheric reconstruction study should coordinate with the air quality modeling study in terms of defining appropriate meteorological scenarios. It is not clear wether the historical study will attempt to resolve individual storm events or will focus on average conditions.
- The periphyton component of the water side study seems to be planning some atmospheric deposition studies that are independent of the air side study, although some coordination is mentioned.
- It is not clear if the groundwater study will actually collect any groundwater quality data. If so, this should be coordinated with other water quality components.

I hope that this review is useful in your future planning and execution of the project. Please contact me with any questions you may have.

Very truly yours,

Keith D. Stolzenbach
Professor of Civil and
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Comments from Professor Gail Tonnesen
(University of California, Riverside)

Comments on the proposed Lake Tahoe Atmospheric Deposition Study

Submitted by Gail Tonnesen, University of California Riverside
July 31, 2002

Summary of Proposed Study

These first few pages are my attempt to summarize key point in the study, primarily for my own use.

Problem Statement:

The study is motivated by observations of rapidly declining visibility in Lake Tahoe, as much as a 1 foot per year decrease.

Hypotheses are that:

1) Direct atmospheric deposition of particulate matter (PM) may be the primary factor reducing water clarity.

2) Increasing inputs of biologically accessible forms of phosphorus (P) and nitrogen (N) cause algal growth in the Lake which is a major factor in the decline of water clarity.

3) Initial estimates indicate that about half of the total N loading and one-quarter of the P loading to the Lake enters via atmospheric deposition (fertilizer runoff and underground seepage of waste may account for the remainder.)

Study Goals are to provide:

1) improved estimates of the annual and seasonal loading of phosphorus, nitrogen, and particulate matter from atmospheric deposition to Lake Tahoe and its watershed,

2) improved attribution of the in-basin and out-of-basin sources (categories) of these materials,

3) assessment of the effects of control programs already in place and potential future regulations to further reduce the loading,

4) assessment of the effect of ozone concentrations on forest health, and

5) the confidence levels associated with these estimates and assessments.

Specific goals are listed in Section 1.2 and include the following:

1. Obtain documented data sets of specified precision, accuracy, and validity that support modeling and data analysis efforts.

2. Review existing data sets and publications to document background concentrations of ozone, N, P, and PM and to determine how these have changed over the past few decades.

3. Identify and describe transport pathways and estimate the fluxes of ozone, N, P, and PM transported at ground level and aloft under meteorological conditions associated with seasonal variations.

4. Quantify the uncertainty of emissions rates, chemical compositions, locations, and timing of N, P, and PM that are estimated by emission models.

5. Quantify the uncertainty of meteorological models in simulating transport and local mixing of precursors and products within and between air basins.

6. Quantify the uncertainty of air quality models in simulating atmospheric transformation and deposition.

7. Provide the meteorological and air quality measurements needed to estimate, with stated uncertainty intervals, the contributions from background, regional mixing and transport, and local emitters to N, P, and PM deposition in the LTAB.

8. Provide the meteorological and air quality measurements needed to estimate the effects of different emission reduction strategies within and outside the Lake Tahoe Air Basin, and identify those that cause the greatest reduction in deposition for the least cost.

Key problems to address in study design:

Wet deposition measurements are easy to make but deposition to the Lake is highly variable by location, time of day, and season. How best to design a wet deposition monitoring network that correctly measures the total mass of deposition.

Dry deposition measurements are very difficult to make, hence, redundant approaches must be employed with the hope that different approaches will converge to a similar result, thus improving the confidence in the estimated dry deposition. Variability in dry deposition by location, time of day, and season is also a key issue that must be addressed.

Summary of ambient monitoring

Major monitoring components of the study include:

Particulates:
TWS collocated with Beta Attenuation Monitors
optical particle sizing counter

Gas species:
LIF for NO2, HNO3 and organic nitrates at transport site
Continuous nitrate analyzer at the transport site
NOx or NOy analyzers at most sites.
TWS will include denuders for measuring NH3 and HNO3.

Deposition:
3-way deposition method comparison
  wet/dry deposition sampler with water - standard TRG method.
  wet/dry deposition sampler without - standard acid deposition method.
  water surface deposition sampler aerometricly designed to minimize disturbance.

Meteorological data:
three mini-sodars distributed around the Lake
one radar wind profiler with radio acoustic sounding system

Monitoring sites will include:
Fixed sites:
  South Lake Tahoe,
  Lake Forest (NW shore)
  Thunderbird Lodge (NE shore).
  on a buoy near the center of the Lake

Special short-term sampling studies will include:

1) mid-lake sampling on buoys on Lake Tahoe,
2) a near-shore study of the falloff in concentrations with distance from particle source areas,
3) measurements via an instrumented aircraft of air quality and meteorological conditions above the Lake during spring, summer, and winter to characterize vertical and horizontal variations,
4) measurements via an instrumented boat during the winter to characterize horizontal variations on the Lake,
5) continuous meteorological and particle sampling (BAM) and some limited collection of materials with a TWS (and compositional analysis) during the summer at the top of Homewood to relate observations of transport at the Sierra slope site to what arrives at the

western edge of the Tahoe Basin, and
6) a limited number of snow samples collected in the early spring and analyzed in the laboratory to estimate the total deposition of materials during the winter. \

General Comments

Overview:
The LTADS is comprehensive and well thought out. It is likely that some technical aspects of the modeling in particular will change by the time the modeling phase of the work begins. However, the plan provides a thorough review of aspects of the modeling and model evaluation that will be useful as the modeling component is revised in the future. The more immediate tasks relate to ambient monitoring and emissions inventory development. Additional emphasis should be placed on more measurements of nitrogen (N) species. While the study plan does mention that spectroscopic techniques would be valuable if sufficient funding is available, I recommend increasing the emphasis on these additional measurements. I believe that a comparison of the LIF and TDLAS (or TDL) techniques is being planned, and the measurement program should be reevaluated when that data becomes available. I did not see a description of the chemiluminescence NOx instruments, i.e., is it planned to use high sensitivity NOx analyzers in the study? I expect that high sensitivity NOx instruments would be needed for the rural, low NOx conditions in the LTB. Regarding NOy analyzers, it would be useful to include a discussion of instrument bias and variability as reported by Dennis Fitz in an ARB funded project that compared several NOy analyzers. Dennis found a positive interference from ammonia that would be of concern.

The aircraft instruments do not include a CO analyzer, it would be useful to include CO measurements on the aircraft if this is possible.

Surface Runoff and Atmospheric Deposition to the Watershed:
More discussion may be needed for the issue of N and P deposition to surrounding land areas (the watershed) and subsequent runoff to the Lake. Estimates of runoff are given in Table 1. The EIP stream monitoring is mentioned briefly on page 44. Page 1 states 'This study will also provide information on the atmospheric deposition to the Lake Tahoe watershed where runoff from rain storms and snow melt contribute to the P & N loading of the Lake.' However, the study plan is focused on monitoring on the Lake or on the periphery of the Lake. If deposition to the watershed followed by runoff is important, will it be adequately characterized? While snowmelt is addressed it is not clear to me if dry deposition to the watershed followed by runoff is investigated. For example, can N and P accumulate in the watershed during the dry season and be incorporated into runoff during rain? If previous snowmelt studies showed little contribution to N and P, should new snowmelt studies be a high priority? Distinguishing whether the stream N and P are produced from fertilizers and waste versus atmospheric deposition is a high priority and it may not be adequately addressed in the plan.

Diurnal Time-series of N:
The study will rely primarily on the Two Week Sampler (TWS). This is the best approach to use because of the infeasibility of hourly sampling. However, the TWS data will not provide a rigorous test of air quality models to represent the diurnal variability in deposition rates. Hence, it is possible that models can correctly represent the two week average data but not represent the diurnal variability. If this were to occur, it would raise concerns regarding the robustness of the model and its ability to predict the response of deposition to emissions reduction in futre control scenrios. Thus, it will be useful to have some limited hourly monitoring data to validate this aspect of the air quality model. The use of the collocated continuous Beta Attenuation Monitors should address this issue for the PM species. It would also be useful to obtain hourly resolved measurements of NO, NO_2, HNO_3 and other N species during a special study period of several weeks during each season. (Note: I see that this is addressed later in the chapter - you might want to mention this on page 13 where the use of TWS as the cornerstone of the sampling is first discussed. Also, the twelve-hour on/off operation of the TWS will help get better time resolution for NH_3 and HNO_3 if it can be made to work, so this should be a high priority. IF this does not work, is there a backup plan for getting diurnal NH_3 and HNO_3?)

Sensitivity of N Instruments:
NOx and NOy instruments will be located at most sites. Will these instruments be sensitive enough to detect low concentrations? It may be necessary to use high sensitivity instruments, but high sensitivity NOx instruments are more expensive than routine NOx instruments. The LIF measurements will be valuable for low concentrations, but the LIF is available only at one site. A TDL instrument can also provide sensitive, high temporal resolution measurements of NO2 and HNO3. It would also be useful to compare the LIF and TDL - I believe that such a comparison is currently planned at the UCR smog chamber. The result of this comparison should be reviewed by the LTADS planning team when it becomes available.

New Dry Deposition Schemes:
One key advantage of air quality modeling is that deposition velocities can be calculated for specific surfaces and vegetation types. New versions of EPA's Models3/CMAQ are likely to include much more sophisticated deposition schemes than are currently available in CMAQ or other models. These schemes will require high resolution land use data including by vegetation type. It would be useful to discuss data needs for this in the plan and whether sufficient land use data will be available for the 1 km grid. It would also be useful if the plan included a section describing the schemes available in the model to represent dry deposition. Specifically, is there a need for additional model algorithm development (i.e., more advanced schemes for representing dry deposition.)

CCOS and CRPAQS Modeling Studies:
The planned CCOS and CRPAQS modeling studies will be carried out for just a few scenarios. It would be useful to perform long term modeling to estimate transport to the Lake Tahoe area for a wider variety of conditions. Annual modeling is becoming more and more feasible (e.g., WRAP annual modeling of regional haze). It would be useful to consider the feasibility and usefulness of long-term or annual modeling in CA for determining transport to Lake Tahoe.

Fire Emissions:
As discussed in the plan, fire emissions of N and P may be important sources affecting Lake Tahoe. The plan should discuss the types of fires (wildfires, prescribed burning, and agricultural burning) and the available inventory data for each. Because wild fires are stochastic processes, they may not be well represented in short term modeling studies being performed for CCOS and CRPAQS, and this also suggests a need for long term modeling.

Specific Comments on Intro/Chapter 1:

Page 3, Table 3:
Page 3 states that there is improvement in O_3 but Table 3 shows zero change in the 8-hr average O_3 from 1980 to 2000. I would guess that the progress in O_3 reduction refers to exceedences of the 1-hour 120 ppb standard (?) but this is not shown in Table 3.

Page 18, Section 1.3.3:
'**Nitrogen compounds** because the algal growth in the Lake could become nitrogen-dependent in the future if effective controls are not instituted to reduce phosphorus loading.' [Is the word 'not' missing in the previous sentence – if P controls are implemented the lake should remain P-limited and not become N-limited]

Page 18, Section 1.3.3:
Suggested to insert at the end of the paragraph 'Additional N species of concern are NO_2, PAN and organic nitrates.' [It is possible in this rural area that the largest fraction of gas phase N may be in the form of PAN and other organic nitrates. This needs to be investigated.]

Table 5 is a good summary of data and approaches for calculating dry deposition.

Page 22, Section 1.4.1:
'The data indicate a downward trend in N deposition in both urban and rural areas. Assuming these data are also representative of the trend in the Tahoe basin, the CADMP data are consistent with the hypothesis that the water clarity of Lake Tahoe is more P dependent than N dependent because the water clarity has continued to decline despite reduction of the N deposition..

I suggest deleting the last sentence - is it possible that there is a cumulative effect of N deposition such that it high N deposition rates are continuing to contribute to lake visibility problems even though the trend in N deposition is downward? If the problem is P, and if N reductions have no benefit, then it seems there would be questions about the usefulness of making very expensive speciated N measurements. N measurements could still be needed for O_3 control but perhaps not for lake visibility UNLESS the lake is expected to become N limited. This issue needs much more consideration.

Page 30, Section 1.6:

I agree entirely with the proposed integrated approach and ARB staff has done a good job of outlining its methods and benefits.

Page 30:
'The measurement program will serve both to refine model inputs (especially meteorology) and to calibrate and evaluate the model's performance (air quality and deposition).'
I suggest deleting any references to 'calibrating the model' because of concerns about 'tuning the model' to fit the observations. A calibrated or tuned model may not be useful for predicting future emissions scenarios.

Page 36:
'continuous nitrate (NO_3)'
What instrument is being used to measure the NO_3? The plan should specify whether this is gas phase or aerosol NO_3 (I assume that it is aerosol, but it is best to specify this.)

Page 43, Section 1.6.3.4:
Excerpt from text: 'Though we cannot collect hourly HNO_3, some day vs. night concentration data would be valuable. For diagnostic MPE, if it were a question between sampling one-day (24-hour average) once every six days vs. an integrated two-week sample, the 1-in-6 day sample would have more value for diagnostic purposes. However, if the two-week sample happened to initially match well with modeled concentrations, that would give some confidence in the model results. In the more likely case that the model did not initially replicate the concentration of HNO_3 from two-week sampler, the two-week samples would not be very useful for diagnostic purposes. For support of MPE, a sampling strategy should be devised that provides some observations of the difference between concentrations during the day and at night. '

It could be fortuitous 'if the two-week sample happened to initially match well', so I do not think this would provide much confidence in the model. Getting day/night speciated N data would be very useful even if for only limited time periods. Can the LIF provide hourly HNO_3? The possibility/cost of TDL measurements should also be considered.

Page 51, Section 1.6.3.9.2:

'The models would be enhanced to provide 1-km grid resolution in the Tahoe basin and fine-tuned to fit with observational data collected during...'

This language seems to imply that the model will be tuned to fit the data. See comments above for page 30 regarding tuning or calibrating models.

Comments from Professor Anthony Wexler
(University of California, Davis)

Title: Review of Lake Tahoe Atmospheric Deposition Study

Reviewer: Anthony S. Wexler, Professor of Mechanical and Aeronautical Engineering, Civil and Environmental Engineering, and Land, Air, and Water Resources, University of California, Davis

Date: June 21, 2002

Task Order: 98-004

Introduction

The staff of the California Air Resources Board has developed an ambitious and comprehensive plan expending very limited financial resources to assess atmospheric contributions to clarity degradation in Lake Tahoe. The plan included measurement of key meteorological fields, concentrations of relevant gaseous and particulate pollutants, and important emissions sources, and subsequent modeling of the results leveraged with CCOS and CRPAQS modeling efforts to more fully understand the dynamics of the deposition problem and potential remediation strategies.

From my reading of the study plan, CARB and related stakeholders are interested in quantifying the degree of clarity degradation that is due to atmospheric deposition. The clarity deposition is due primarily to algal growth, which is caused by an increased level of nutrients, particularly nitrate and phosphate. Currently, the algal population is limited by phosphorus since the nitrate loading is so large, but it is worth investigating both loadings. Phosphorus exists exclusively in the particle phase and is primarily crustal in nature, so appears primarily in coarse particle sizes. Nitrate is found in the gas phase primarily as nitric acid and in the particle phase primarily as ammonium nitrate. Air enters the basin primarily through a limited number of passes on the western edge of the basin and exits through a couple of passes to the east. Airflow into and out of the basin is relatively small compared to its volume since the steep mountains on all sides limit flow, so it is probably a reasonable assumption that the air is relatively well mixed in the basin.

We can crudely model the basin as a well-mixed container, with pollutant inputs locally and via wind over the mountains to the west. These pollutants either deposit on the lake or surrounding watershed, or are advected out of the basin. By characterizing the amount of pollutant advected into the basin, the amount generated within the basin, the amount deposited to the surface and the amount advected out of the basin, closure can be obtained, since all these figures must add up.

Technical Critique

1. Closure

Background: The understanding that I have expressed in the previous paragraph identifies the first shortcoming of the proposed plan. Reasonable and substantial efforts will be undertaken in the study plan to quantify advection into the basin, emissions within the basin, and deposition in the basin, but I did not notice an effort to quantify advection out of the basin. If the primary routes for advection out of the basin can be identified and monitored for wind speed, direction, and pollutant concentration, a complete closure of pollutant fluxes can be obtained. This is a valuable self-consistency check and will also help quantify uncertainties in each of these key quantities since the advection in and emission must add to the deposition and advection out.

Recommendation: Identify advection routes out of the basin and monitor wind speed, direction, and pollutant concentrations there.

2. Phosphorous Emissions

Background: Since the limiting factor appears to be phosphorous on algal growth and clarity degradation, emphasis must be placed on these compounds considering the limited resources to be expended for this project. There is extensive discussion throughout various portions of the study plan outlining and discussing emissions quantification and associated contracts for quantification of vehicle emissions, stationary sources and others, but very little space and effort appears to be given to quantification of phosphorous emissions. I only found a brief description of the phosphorous emissions effort on page 59 and nothing related to the methods or who is targeted to perform this work. This is very important since the implication is that phosphorous is primarily coarse, so it will have a substantial settling velocity and therefore long-range transport is likely to play a minor role in its deposition to the lake. Thus local sources probably dominate the deposition and these must be quantified.

Recommendation: Expand the effort to identify and quantify phosphorous emissions sources.

3. Phosphorous Transport

Background: Again, since phosphorous appears to be the limiting compound to algal growth, its transport must also be understood. Specifically, I have hypothesized above, as is implied in the study plan, that phosphorous is locally emitted and deposited. To confirm this, an effort must be mounted to quantify its advection into and out of the basin.

Recommendation: Measure phosphorous concentrations at the primary transport sites.

4. Nitrate Time Resolution

Background: Transport of pollutants to the basin may be continuous or momentary, but it is likely that only occasional meteorological conditions prefer substantial transport to the basin, so momentary transport is more likely and we will use this as a working hypothesis. Most of the monitoring equipment, such as the TWS, will not be able to resolve momentary bursts of pollutant transport. Instead, these bursts will be integrated into the two-week period and the temporal nature lost. The BAMs will resolve momentary concentration spikes, but only for overall PM mass, which may not necessarily be indicative of the pollutants of concern here. Dr. Goldstein's group will be deploying one of their nitrate monitors as part of this program, which will provide valuable highly time resolved nitrate concentration in one or possibly two locations. I am not sufficiently knowledgeable about the state of this new instrument to be able to judge whether it is ready for a long-term field campaign such as is proposed here, but the RP semi-continuous nitrate monitor has been used at many supersites with success.

Recommendation: Deploy highly time-resolved nitrate monitors at all the primary transport sites, either using the UCB design, the RP semi-continuous design, or a suitable competitor.

5. Phosphorous Time Resolution

Background: The same argument, that transport may be momentary instead of continuous, applies to phosphorous as it did for nitrate. But phosphorous is more likely to be locally emitted and its transport may not be very far if it primarily resides in coarse particles that have a substantial deposition rate.

Recommendation: Investigate time-resolved phosphorous monitors and deploy them at the primary sampling sites if a suitable design is available or can be built.

6. Deposition Rates

Background: The study plan presents a number of complementary measurements that will be made to determine the deposition velocities for the relevant compounds. The wet and dry bucket measurements are direct forms, with their stated uncertainties and problems. The complementary techniques include eddy covariance and relaxed eddy accumulation along with similarity techniques and the Bowen ratio to infer deposition of compounds that cannot be measured directly. These eddy covariance and accumulation techniques are difficult to employ unless the scalar can be readily measured, which is the case for temperature and relative humidity. The problem arises when similarity is employed to relate these fluxes to those for particulate matter. There are three dominant mechanisms for deposition – turbulent transport to the near surface, diffusion or settling through the laminar sublayer, and accommodation on the surface medium. Gases and heat readily diffuse through the laminar boundary layer, so their transport is primarily limited by turbulent transport (this is not the case if the gas is not soluble at the surface as is the case in some climates where deposition to vegetation dominates and is controlled by stoma status). Particles are transported by turbulence nearly the same as the gases and heat, but they are very poorly transported through the laminar boundary layer. Particles smaller than about 0.1 micron (not very relevant to this study) can diffuse through the laminar layer, particles larger than a few micron can settle through the laminar layer, but particles in between do not have an efficient mechanism for penetrating the laminar layer so their deposition rate is often orders of magnitude lower than that for gases. Similarity techniques assume a Prandtl number near one for the constituent measured compared to the constituent flux being inferred, and this is off by orders of magnitude for many particle sizes of relevance to this study.

Recommendation: Particle flux as a function of particle size could probably be measured directly by coupling a relaxed eddy accumulation system to an APS or OPC.

7. Influence of Highways 50 and 80

Background: Lake Tahoe is surrounded by a very high and complex terrain that can block and channel flows that transport pollutants into the basin. The study plan includes extensive measurements to quantify these flows and therefore the transport. Some of the discussions in the study plan center on avoiding measurement of pollutants near these highways to avoid local effects. Considering that the air will enter the basin at the lowest points, as will the vehicles, this may bias the transport results against one of the likely external sources. Certainly, there are many more vehicles in the central valley and Sacramento, than on the highways leading to and from Tahoe, but the air from the central valley sources will have been substantially diluted before it reaches the passes and the cars and trucks driving to the passes are making a steep climb that is a large expender of fuel right in what may be the primary path for air flowing into the basin, and in steep valleys where dilution may be limited. The combination may mean that the primary sources "outside" of the basin may be the traffic on the highways leading to and from the basin. NOx from these highways will not have had time to convert to nitric acid before entering the basin, but they may contribute to the nitric acid formed there.

Recommendation: Carefully consider the flow patterns into the basin from the west and the likely paths for pollutants, and consider locating sampling stations and chemical analyses to distinguish local highway emissions and transport from longer-range transport from the valley. Avoiding these corridors may not be the best way to proceed.

8. Wind Fields

Background: As a follow on to the previous comment, it would be worthwhile to study the flow patterns into the basin numerically to try to obtain at least a qualitative understanding of these complex flow patterns and their dependence on regional meteorological conditions. This work may have already been done, but I did not notice it in the study plan.

Recommendation: If this investigation has not been performed already, there are MM5 experts in the ARB and at a few of the UC campuses who can attempt to model transport in this complex terrain. In addition, WRF (Weather Research and Forecasting Model – http://www.wrf-model.org) may be ready soon to tackle such a task and researchers at Los Alamos National Laboratory have done extensive complex terrain modeling on Mexico City, another mountain basin with air pollution problems, and may be able to apply their techniques to Tahoe (under DOE funding).

9. Chemical Transport Modeling

Background: As stated in the study plan, mathematical models of wind fields and photochemical reactions play an essential role in both understanding the dominant mechanisms that influence nitrate and phosphorous deposition to Tahoe and allowing emissions control strategies to be investigated. Unfortunately, the current budget does not include funding to support this modeling effort.

Recommendation: Additional funds be allocated to the modeling effort and that the effort begin as soon as possible so that issues learned from the modeling can possibly help direct the measurement effort.

Stylistic Critique

The study plan is well organized and accessible to a technically oriented audience, but it contains a number of typographical errors and other presentation shortcomings that would make the plan more accessible to the audience of researchers and technicians who may need it in the future.

Page i: change illucidate to elucidate

Page iii: “the less environmentally friendly white man” is a completely gratuitous and racist comment that is also wrong, according to recent anthropological studies. It is the population density that matters, not the race of the population.

Page 7 in paragraph just before 1.1.4: “based on more”

Page 7 in paragraph just after 1.1.4: add close parenthesis

Page 8 and 9: that there is sufficient fetch for direct deposition measurements via eddy correlation or eddy accumulation is only established by personal communication with one investigator. Since this study depends on this assumption, more should be done to establish it. With air spilling over the mountains into the basin, I am surprised that there is sufficient fetch anywhere in the basin, but this is not my area of expertise.

Page 13: There are continuing rumors that UCOP will remove support of the UC Davis aircraft and decommission it. Please check that the airplane will be available when this study is to occur.

Page 15, paragraph 2: I could not figure out what this paragraph was supposed to mean.

Page 16, paragraph 3: I am concerned that much of the atmospheric deposition is to leaves, which are then washed into the lake by precipitation. Although the surface area of the lake is larger than the rest of the watershed, the surface area in the rest of the watershed due to vegetation may by many times greater than that of the lake and therefore have a much higher deposition flux.

Page 21, last paragraph: Considering modern day technology, is it reasonable to use a chart recorder which then has to be manually entered (with that associated error and cost) into a computer for analysis? Can a digital system be purchased or fabricated?

Page 22, third paragraph: Why is phosphate and phosphorous not included in the analysis?

Page 22, last paragraph: reference for Yi left out of reference section.

Page 24, paragraph before 1.4.2: How can the phosphorous deposition to the lake be measured with REA?

Page 27, second to last paragraph, change complimentary to complementary

Pages 34 and 83: The maps are almost completely illegible. They should be reproduced once and full-page size with clear lines and fonts. Place the map earlier in the report since it helps investigators to understand what is happening where. The map on page 37 is much more clear.

Page 41, paragraph after 1.6.3.5.3: How will PM flux be measured? How will flux to tree and other plant surfaces be assessed or estimated?

Page 42, section 1.6.3.6.2: Why is not effort placed on improving the phosphorous and phosphate emissions inventory?

Page 110-123: Why are there no data quality objectives for PM?

Comments from Professor Akula Venkatram
(University of California, Riverside)

REVIEW

WORK PLAN FOR THE LAKE TAHOE ATMOSPHERIC DEPOSITION STUDY (LTDAS)

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INTRODUCTION

The objectives of the Lake Tahoe Atmospheric Deposition Study are to:

1. Quantify atmospheric deposition rates of phosphorus, nitrogen, and particulate matter into Lake Tahoe.
2. Determine the relative contributions to this deposition of emissions in the Lake Tahoe Basin versus those associated with upwind urban centers, such as San Francisco and Sacramento.
3. Characterize ozone levels in the forests surrounding Lake Tahoe, and estimate the source-receptor relationships that govern these levels.

The ARB has formulated an ambitious plan that combines measurement programs, data analysis, and mechanistic modeling to achieve these objectives. I have some general observations on the work plan:

1. The work plan is impressive is in its scope and provides enough detail to follow the arguments for choosing a particular approach among several alternatives.
2. Because several authors have written the manuscript, it appears to have been difficult to splice the contributions from different authors into a form that can be read easily. The plan needs to be edited to avoid repetition of points, and improve readability.
3. Section 1.7 on the study timeline can be clarified through a diagram or table that shows an ordered sequence of activities and deliverables that connect these activities in a coherent fashion.

Some suggestions that might improve the plans for achieving the objectives are given in the following sections.

QUANTIFICATION OF ATMOSPHERIC DEPOSITION

Table 1 presents initial estimates of atmospheric deposition to Lake Tahoe. It is important to know how these estimates were made, and how the ARB plan will improve upon them. The results from Murphy and Knopp (2000, the reference is incomplete) need much more discussion than that in the work plan.

The ARB plans to use several methods to estimate dry deposition of the relevant species. These include multiplying a calculated dry deposition velocity by the measured surface concentration, and a profile technique that uses the eddy diffusivity of heat or momentum to estimate the downward transport of the chemical of concern. I suspect that the Bowen ratio (The reference is missing) technique is also indirect because it must assume (Is this justified?) that the eddy diffusivity for water vapor is equal to that of the chemical species of concern. These indirect methods can be unreliable, especially for particle deposition.

Direct measurement of the downward mass flux using the eddy covariance method appears to be unfunded at this stage. ARB proposes to use the water surface sampler to make dry deposition measurements. There is not enough detail in the plan (the Yi et al., 1997 is missing) to understand how this approach overcomes the surrogate surface problem. The budget on page 160 does not indicate that this task will be funded.

Because of the obvious importance of dry deposition, I would like to see the use of at least one direct method to measure dry deposition. The results from this method should be compared with the more routinely used indirect methods to estimate the errors associated with the indirect methods. This should be done right at the beginning of the program before investing resources into indirect dry deposition measurements. There is no guarantee that the range of values from the indirect methods will include that from a direct method.

The ARB should consider measuring dry deposition by measuring concentrations near the surface of the lake at several distances from the shore along the direction of the wind. Then, the effective dry deposition velocity can be calculated from the integral equation (Horst, 1983; Doran and Horst, 1985):

$$C(x) = C(0) - v_d \int_0^x C(x')D(x - x')dx' , \qquad (1)$$

where the dispersion function, $D(x - x')$, can be estimated experimentally by releasing a tracer over the surface of the lake.

The two-week-sampler will serve as the backbone of the sampling network. ARB proposes to use a beta attenuation monitor to infer hourly concentrations of PM2.5, PM10, and TSP by prorating the TWS measurements. While this approach is reasonable, it needs to be validated by making direct hourly measurements of the concentrations of the relevant chemical species.

DETERMINE THE RELATIVE CONTRIBUTIONS TO DEPOSITION OF IN-BASIN VERSUS OUTSIDE-BASIN EMISSIONS

ARB proposes to use several approaches to estimate the contribution of in-basin emissions to atmospheric deposition on Lake Tahoe. It is difficult to follow any of these approaches because the description of measurements is not related to the associated data analysis. I would like to see a clear description of the method or model that will be used to estimate the relative contributions of the two regions, before the measurement program is presented. Without the context provided by the data analysis methods/models, it is impossible to evaluate the usefulness of the sampling program in meeting the objectives. While Table 13 (Page 73) is informative, it needs to be expanded to show how the measurements will be used to make quantitative estimates of deposition and source-receptor relationships.

Figure 6 on page 56 suggests that PM emissions from paved and unpaved roads contribute to well over 50% of the phosphorus emissions in the Lake Tahoe basin. My expertise in this area suggests that there is a very high degree of uncertainty in estimating PM emissions from roads. Considering the importance of phosphorus and PM to atmospheric deposition, the ARB might want to spend resources to reduce this uncertainty by performing some field experiments. Estimating ammonia emissions is also an uncertain exercise. It would be useful to conduct a limited number of field experiments to evaluate these estimates.

The ARB might consider making carefully designed measurements to delineate in-basin emissions. For example, we can estimate the in-basin contribution by examining concentrations only when the wind is blowing from the east. We can formulate simple models to see whether these concentrations are consistent with basin emissions. Such an approach is preferable to source-oriented methods that rely on uncertain emissions. In the complicated situation of the Lake Tahoe basin, I would be wary about using flow field models in combination with emissions.

Table 27 indicates that $223K will be spent on purchasing 4 mini-sodars. I would like to see more detail on how the data from the mini-sodars will be used. Because $133K will be spent on aircraft measurements, I would make sure that the information they provide can be used quantitatively. Aircraft measurements should have more justification than that expressed in Purpose of Table 13, which states: “Upwind sources may be decoupled from basin surface concentrations due to vertical temperature structure.” My experience with using aircraft measurements to evaluate comprehensive models indicates that the benefits to cost ratio is extremely sensitive to plans on using the data. Aircraft measurements have the potential of using up resources that could be better spent on collecting surface data of more immediate use. Table 27 indicates that about $570K will be spent on meteorological measurements. Because this represents 30% of the available resources, it might be useful to plan a phased program in which say 50% of the meteorological measurements budget is used in the first year. This should be preceded by meteorological modeling (with MM5) to identify special features that need to be evaluated with measurements. After evaluating the model at the end of the first year, ARB can proceed with the second half of the program. This approach will reduce the risk of using resources to collect data that might not be useful, and not collecting data that is required to understand the flow in the LTB.

A phased approach might also be useful in the air quality program. For example, ARB might want to purchase only a small number of BAMS during the first year. They can evaluate their usefulness during the first year before purchasing the rest of the instruments; ARB proposes to buy 14 BAMS for $213K.

ARB proposes to use photochemical models to estimate the transport of oxidants and nitrogen species into the Lake Tahoe basin. While photochemical models have reasonable skill in estimating ozone concentrations, they generally perform poorly for nitrogen species. It is not clear how ARB proposes to address this problem. If the primary reason for running the photochemical model is to estimate the concentrations of oxidants that convert in-basin NO_x

emissions to HNO_3, why not use measured levels of oxidants and thus avoid the errors associated with model estimates?

SUMMARY COMMENTS

The work plan is impressive in its scope and in the details of the technical approach. It is clear that ARB staff have given a great deal of thought to formulating the work plan. The readability of the plan can be improved by a) reorganization, b) removing repetitious sections, and c) moving paragraphs that describe operational details to appendices.

I would like to stress the importance of paying attention to the following points:

1. The results of the program will be compromised if direct measurements of dry deposition are not made.
2. The large uncertainty in estimates of PM emissions from paved and unpaved roads needs to be reduced by conducting a few well-designed field experiments. I have similar concerns about ammonia emissions. The funding for emission inventory improvement could be increased by making reductions in budgets for Tasks 4 and 5 (Page 162).
3. All data analysis requires the use of explicit or implicit models. Even if the data analysis is simple, it is better to be explicit about the underlying model by writing down the governing equations. This can avoid the risk of drawing unjustified conclusions from the results of the data analysis. I suggest supplementing Table 13 with the model equations that will use the measurements as inputs.
4. ARB proposes to use a comprehensive model to estimate nitrogen and ozone transport into the Lake Tahoe basin. Because comprehensive models are sensitive to emissions and boundary conditions, and can produce unreliable estimates of nitrogen species concentrations, it might be useful to use a hierarchy of models ranging from simple mass balances to mechanistic models. The simple models will be necessarily semi-empirical in that they will be anchored to observations. I am concerned that relying too much on mechanistic comprehensive models might reduce the value of observations, which are best analyzed using simple models.
5. Most of the LTADS budget has been allocated to meteorological measurements ($348K) and air quality measurements ($844K). To reduce the risk of investing these resources in collecting data that might not be as useful as first intended, ARB might want to consider a phased approach in which a fraction of the budget is spent to collect data for analysis during the first year. The results of the analysis can determine whether the existing program is expanded/modified or the resources are spent on other activities. Although I am not an expert on these matters, I am concerned about spending $175K on measuring N species with laser-induced fluorescence. This expenditure seems to be out of the ordinary especially when there is no money for direct measurement of dry deposition.
6. I notice that ARB staff will carry out all the modeling and data analysis tasks. While there is no doubt that ARB has the necessary expertise to accomplish these tasks, the credibility of the results of the analysis, the final product of this project, might be enhanced with participation from other organizations.

REFERENCES

Doran, J.C., and T.W. Horst, 1985: An evaluation of Gaussian plume-depletion models with dual-tracer field experiments. Atmospheric Environment, 19, 939-951.

Horst, T.W., 1983: A correction to the Gaussian source-depletion model. In Precipitation Scavenging, Dry Deposition, and Resuspension. Prupacher et al., Editors.

Summary of Comments on LTADS Work Plan and Staff Responses

I. Synopsis

A) Re: General Approach

1) TWS appropriate as backbone of program?
Staff Response: *A TWS network is the only cost-effective way to have continuous coverage and speciation, including NH_3 and HNO_3. The TWS has worked well in other studies.*

2) Excessive resources invested in meteorology?
Staff Response: *Perhaps but meteorological data are important for characterizing depths and direction of surface air flows. In addition, the data are necessary to support data analyses and modeling.*

3) Rather than touch upon several factors, why not focus on 1 or 2 major issues?
Staff Response: *The TMDL schedule and funding windows force us to pursue the best possible answers now.*

4) Instrument sensitivities appropriate for clean environment?
Staff Response: *Yes, based on specifications from the instrument manufacturers and the analytical capabilities claimed by the laboratory. However, staff is pursuing the possibility of masking the TWS filters to ensure consistent detections.*

5) Modeling approach the most appropriate?
Staff Response: *The current state of the emission inventory for Tahoe Basin and uncertainties about the ability of prognostic models to perform well in complex terrain certainly do not support a robust modeling effort at this time. Staff hopes to take advantage of overlapping modeling efforts to confirm "ballpark" estimates and to pursue simpler modeling efforts than airshed modeling. A goal of some of the LTADS contracting is to collect additional meteorological and emissions data in the Basin to support Tahoe-specific modeling in the future.*

6) Do emission sources have sufficiently different fingerprints for distinguishing relative source contributions by CMB?
Staff Response: *Probably not but staff is interested in having as many independent assessments as possible to confirm the deposition estimates.*

B) Re: Study Design/Details

1) must address persistent presence of shallow inversion layer
Staff Response*:* *Standard surface met measurements will be collected at the RWP/RASS and mini-sodar sites. The RASS temperature*

measurement might "miss" the nature of very shallow inversions but the mini-sodars will detect the thin layers because they would likely have different air flow patterns from the air above them.

2) deposition comparison should include SLT where emissions greatest
Staff Response: *Staff plans on initiating surrogate surface measurements with a wet/dry bucket sampler at the Sandy Way site. Staff will also locate TWSs on both sides of Highway 50 (SOLA & Sandy Way) to investigate local influences and differences in an urbanized area.*

3) need to validate LIF measurements with standard methods
Staff Response: *Staff plans on collocating HNO_3 (from TWS), NO_3^- (from TWS and a continuous analyzer), and NOy equipment at Big Hill to compare with the measurements from the LIF instrument.*

4) replace airshed modeling with more & simpler models (e.g., box)
Staff Response: *Staff will likely include as the development of databases for Tahoe-specific emissions inventory and meteorology may not happen rapidly enough to support airshed modeling in time for the TMDL.*

5) include a downwind transport assessment site
Staff Response: *Ideally, staff would like to do so. However, determining and gaining access to a "representative" transport site downwind of Tahoe would be difficult and the equipment and staffing resources necessary to support such a monitoring program are insufficient at this time.*

6) include more emphasis on P emissions
Staff Response: *Staff has initiated two contracts to better characterize emission sources, including P, within the Tahoe Basin.*

7) separate the impact of Hwy 50 emissions from transport assessment at Echo Summit
Staff Response: *Echo Summit may be influenced somewhat by emissions along Highway 50 but a greater concern is on-site emissions associated with the California Conservation Corps activities. To reduce the influence of idling emissions, etc., staff plans to raise the inlet level for the monitoring probe to 10 meters.*

II. Specific comments by reviewer

A) Dr. Tom Cahill

1) 1-year study too short a time frame to adequately address issues
Staff Response: *May be true but TMDL deadline, air quality priorities, and resources do not permit more time. Nevertheless, a 1-year study will help to refine previous estimates and provide new insights.*

2) must include earlier studies and other dep work in report
Staff Response: *Staff will reference previous work as pertinent but a comprehensive summary of earlier studies is not part of the purpose or scope of the LTADS report given the TMDL timeline and other responsibilities for ARB staff.*

3) plan needs more specificity and documentation
Staff Response: *The work plan is primarily a guidance document of what staff desire to accomplish during the study. Specifics can, and likely will, change as constraints develop and problems are encountered during the implementation of the plan.*

4) persistent low level inversion over Lake needs to be addressed
Staff Response: *Staff agrees that inversions play a critical role in determining the amount of atmospheric deposition. The frequency and persistence of inversions over the lake is an important consideration. Unfortunately, vertical temperature profiles over the Lake are difficult to make as aircraft cannot get to low levels over the Lake, remote sensing techniques require substantial power, and radiosondes are labor intensive. Staff hopes to address this question by analysis of collocated water and air temperatures.*

5) dep sampler comparison should include both N & S sides of Lake
Staff Response: *Staff is primarily interested in dry dep to water surfaces and in the historical changeover from the standard NADP method to the TRG water-based method. To quantify the methodological differences in measuring dry deposition to a water surface, staff has access to a loaned water surface sampler designed to simulate deposition to a water surface with minimal disturbance to air flow as occurs with bucket methods. Because staff has access to only 1 water surface sampler and 1 TRG water-based sampler, the comparison study can only be conducted in one location. Staffing and logistics dictate that the only viable potential options are to conduct the comparison in S. Lake Tahoe or Sacramento, with Sacramento being the most feasible option.*

6) TWS time scale is much greater than the diurnal and synoptic scale of processes affecting deposition – consider day/night sampling
Staff Response: *Staff is planning to include/day/night TWS sampling at the SOLA site but is also including hourly BAM (PM) measurements at the TWS sites.*

7) Will the sampling program work in the cold & clean environment of Tahoe?
Staff Response: *Probably, but conditions there are definitely harsher than typical during the winter and the clean air quality will approach the detection limits of some instruments at times.*

8) Maintain consistency w/ TRG dry dep record for trend purposes
Staff Response: *Staff plans to conduct a comparison of dry dep measurement methods, including the traditional NADP method, the water-based TRG method, the TRG snow tube, and, if available, a loaned water surface sampler from Clarkson University.*

9) Long-term commitment needed by ARB, not only for field studies addressing critical issues but also to meet informational timelines and respond to litigation
Staff Response: *The level of long-term commitment is determined by ARB's upper management and will depend on a variety of factors that establish priorities; realistically, air quality in the Tahoe Basin is excellent and a significant amount of resources cannot be dedicated to it at the expense of more problematic areas.*

B) Professor Tony Wexler

1) use box model w/ downwind "exit" site to bound deposition estimates
Staff Response: *Although this is a good idea, characterizing an exit site is much more difficult than characterizing an upwind site; also not knowing conditions aloft well (e.g., transport, mixing depth, concentrations) would be somewhat speculative. Staff will consider concept in terms of resources and feasibility of characterizing components reasonably well.*

2) need more emphasis on characterizing P emissions
Staff Response: *The primary P sources are anticipated to be roaddust and smoke. Staff plans to collect samples of these sources to reflect a more Tahoe-specific P inventory.*

3) measure P at transport sites too; any way of getting time-resolved P?
Staff Response: *Phosphorus will be an analyte of all the TWS and MVS PM measurements. Seasonal results are the finest temporal scale planned although measurements will be of various shorter time-scales (24-30 hrs for samples on buoys, 1-2 weeks for MVS samples, and 2-weeks for TWS samples).*

4) concerned about TWS missing short term variations (BAMs don't characterize species)
Staff Response: *This is a valid concern and a critical assumption. However, emission sources and activity patterns are generally known and have consistent patterns. A day/night TWS sampler is planned for the primary source-impacted site (SOLA) to shed insights into temporal variations in PM species, ammonia, and nitric acid.*

5) use continuous NO_3 analyzer at primary sites

Staff Response: *Staff plans to use at the upwind site, if possible, and especially desires to collocate with the LIF instrument.*

6) use particle sizing counter under different dep settings in conjunction w/ eddy covariance and accumulation techniques
Staff Response: *The details of the OPC studies have not been defined but will likely address specific questions, including variable meteorological/depositional settings. At this point, eddy covariance technique not planned.*

7) what is the impact of emissions along Hwy 50 and I-80? Any bias on transport assessments at Echo Summit?
Staff Response: *Obviously, these major highways are also significant sources of materials. Staff is planning to raise the inlet probe at the Echo Summit site to reduce the impact of local sources on the monitors located there. Local roads will have much more influence on the air quality measurements within the Tahoe Basin than the I-80 emissions would.*

8) Need better characterization of wind fields (modeling?) to evaluate transport potential
Staff Response: *In a complex terrain setting such as Tahoe, it will be difficult to have the density and representation desired in a monitoring network. Staff believes the plan contains the minimum number of sites necessary to capture gross airflow aloft features.*

9) In-house modeling efforts adequate? More funding needed to exercise control options?
Staff Response: *CARB modelers have expressed their interest and desire to perform Tahoe modeling; as you are likely aware, in-kind resources are always subject to change due to competing deadlines and priorities. The control options will probably be based on Best Available Practice concepts and not detailed control strategy modeling.*

C) Professor Akula Venkatram

1) concerned about time-scale of TWS – supplement measurement program w/ hourly measurements of species
Staff Response: *Staff shares this concern but do not have the resources for hourly, or even daily, speciation and will collect hourly PM mass measurements to impute diurnal variations in the species.*

2) uncertainties associated w/ various dry dep estimation methods – need comparison w/ at least 1 direct measurement method; need direct measurements of dry dep

Staff Response: *Staff considered eddy covariance but is increasingly skeptical of the likelihood of being able to conduct it with current resources.*

3) relative contribution estimates for transport assessment uncertain – how will mini-sodars and aircraft directly assess? Because met and BAM measurements a large portion of study costs, can the study be phased (and coupled with modeling) to reduce costs or ensure proper network design?
Staff Response: *Remote sensing is necessary for understanding the dynamics of the meteorological processes aloft. The TMDL timeline prevents a phased study approach. Overlapping modeling efforts will be reviewed for Tahoe results; ideally, staff would like Tahoe-specific modeling application but this is unlikely to occur in the timeframe of the TMDL.*

4) address the uncertainties in road dust emissions
Staff Response: *A contractor will investigate road dust emissions.*

5) articulate all assumptions and equations used
Staff Response: *Staff will elaborate in reports.*

6) simple models might be more useful than complex models
Staff Response: *Staff agrees that this statement may likely be true and simple modeling may even be necessary within time constraints. However, if the data are not collected for more complex modeling, such modeling can never be done.*

7) amount of $s for met, LIF, etc. large when don't know the accuracy and benefits
Staff Response: *These measurements are important for better understanding the meteorology and chemistry and will also be useful for other analyses. The contractor proposals indicate that the measurements will be sufficiently accurate and useful for the study purposes.*

8) concern about reliance on in-house modeling
Staff Response: *Staff shares the same concern; staff is supportive but the effort ultimately depends on competing projects and management priorities when the modeling would need to be done.*

D) Professor Gail Tonnesen

1) compare LIF w/ standard methods
Staff Response: *Staff plans to have a continuous nitrate analyzer and nitric acid from the TWS to evaluate the LIF performance.*

2) include CO analyzer on plane
Staff Response: *Space on the airplane is very limited and CO analyzers available for LTADS are not sensitive at the levels likely to be encountered aloft.*

3) include more detail regarding deposition to the watershed
Staff Response: *The watershed is not the focus of CARB's efforts as it will be a component of the watershed modeling effort being contracted by the Lahontan Regional Water Quality Control Board.*

4) use of TWS appropriate but not ideal for short-term dep factors; BAMs will address PM but need hourly NO, NO_2, HNO_3, etc.
Staff Response: *Staff agrees that the TWS is not ideal for short-term variations but it is ideal for collecting continuous air quality data in a cost-efficient manner. LTADS will have hourly NO measurements and crude* NO_2 *measurements at several LTADS sites. Collocated NOx and NOy measurements at SLT – Sandy Way will provide some insight into hourly* HNO_3 *variations. Regarding the effects on deposition estimates, NO and* NO_2 *deposition rates are small compared to all the other N species; the nitric acid deposition will be estimated via TWS measurements.*

5) is the sensitivity of the N measurements sufficient for the objectives?
Staff Response: *During exceptionally clean periods of air quality, some N species might not be detectable but the measurement sensitivities should generally be sufficient to meet our objectives.*

6) Consider using additional models (e.g., EPA's Models3/CMAQ) to estimate deposition
Staff Response: *The study planners will leave the choice of deposition model(s) to the modeling experts to choose based on the data availability, model performance, product deadlines, etc.*

7) Consider annual modeling to address transport
Staff Response: *Staff hopes to have results from an overlapping annual PM modeling effort but is skeptical of good performance in the Tahoe Basin due to the crude (large) modeling cells for emissions and meteorology and the Basin. Furthermore, being near the edge of the modeling domain, the Basin was not given much consideration in evaluating model performance.*

8) Provide more detail about fire emissions and how to model the annual impact
Staff Response: *At this point, staff is skeptical that the current source emission profiles for the various types of fires; the number, size, and nature of fires; nor the meteorological conditions during fires are adequately characterized for reasonably determining the annual impact of*

fires. Staff plans on collecting these types of information however to advance the capability of modeling fire emissions and their impacts.

9) If water clarity is P-limited, why invest so heavily in N measurements?
Staff Response: *Staff is placing emphasis on N measurements because TRG states that the Lake used to be N-limited and staff is concerned that if P emissions are significantly reduced, then the Lake could again become N-limited. By looking at N, P, and PM during one integrated study, staff is hoping to collect the information necessary to model and guide air quality decisions for the foreseeable future. Furthermore, detailed information on N species is needed for evaluating model performance.*

E) Professor Keith Stolzenbach

1) concerned about adequately characterizing the short temporal scales of emissions & deposition
Staff Response: *Staff agrees that this is a major concern and has planned on hourly measurements of PM mass, ozone, NOx, NOy, and meteorology to help clarify the processes at work on shorter time scales than the TWS provides. Although the primary need of the water clarity model is annual deposition, staff has pursued shorter time scales in some measurements to elucidate the relative roles of various atmospheric processes in determining annual impacts.*

2) will CMB work well in the Tahoe context? Does ARB have good source differenciations? Box models may be useful
Staff Response: *Although CMB has inherent limitations, the low concentrations at Tahoe and the use of non-Tahoe-specific source profiles will introduce additional uncertainties that will limit the usefulness of CMB in the Tahoe context. Staff is investing resources to improve the source profiles for Tahoe. Box models might be useful analytical tools assuming the emissions and meteorology can be characterized reasonably well.*

3) significant uncertainty exists in the dep estimation & measurement methods – consider a micro-layer water sampling approach
Staff Response: *Staff agrees that the proposed approach has significant uncertainties for quantifying deposition to Lake Tahoe. Staff has had some discussions about collocating some air and water sampling measurements at the Lake surface but it is unclear at this point how that effort will evolve.*

4) resuspension of PM on land a concern
Staff Response: *Staff concurs; the TWS and MVS networks will "see" what is in the air several feet above the ground/water but this may be too high to characterize the resuspension and saltation processes. Staff*

hopes to address this better with "dust" experiments involving optical particle counters which will be closer to ground/water level.

5) field study invests large $s for wind data (presumably for model validation); have model parameterizations been exercised at 1-km resolution before? 1-km resolution still wouldn't capture the physics of local effects such as trees & buildings; how will models be applied to get seasonal variations? Ideally, need to characterize all scales of spatial and temporal events; AQ network might not be dense enough to properly characterize small scale processes; anticipated size of particles should effect monitoring network design
Staff Response: *Staff agrees that fine-scale features (trees, buildings, roads, topography, etc.) could play a significant role influencing deposition rates. The collocation of BAMs with TWSs, the siting of TWSs on both sides of Highway 50 in SLT, and the dust experiments with optical particle counters are all intended to provide additional insights into the role of fine-scale (temporal and spatial) processes.*

6) determine the bio-availability of the P particles
Staff Response: *Staff plans on sending some samples to the University of Nevada, Reno for analysis of bio-available phosphorus.*

7) coordinate air & water studies (especially particle size and storm events)
Staff Response: *LTADS is primarily a continuous sampling operation with limited "episodic" activities. Analysis of data associated with special periods or additional studies should be possible.*

F) Professor Alan Gertler

1) concerned that none of the current MV emissions models (e.g. EMFAC & MOBILE) remotely come close to properly characterizing MV emissions at altitude and grade (slope); consider using remote sensing results from Denver; there is large uncertainty in the MV source emissions which is also the largest source of N
Staff Response: *Staff agrees that standard conditions and factors are not ideal for understanding processes at Tahoe. Emissions inventory and modeling staff are likely to take advantage of all reasonable resources for improved characterization of emissions in the Tahoe Basin. Staff concurs that the local conditions introduce additional uncertainty in the motor vehicle emission estimates and that motor vehicles are the largest source of N emissions in the Basin.*

2) use adaptive modeling – redirect efforts as learn more about chemistry and emissions; consider using box models (cheaper, easier to run and can address more scenarios (e.g., uncertainties, boundary stressors) than airshed models)

Staff Response: *Staff will pursue efforts to enhance the theoretical foundations for modeling. It is likely that simpler models will be used before a comprehensive model could be run.*