SAN JOAQUIN RIVER
DISSOLVED OXYGEN TMDL STUDIES

DRAFT PEER REVIEW REPORT
JULY 1, 2002

BACKGROUND AND OBJECTIVES

A peer review meeting was conducted on June 11- 12, 2002 to evaluate studies conducted in support of developing a Total Maximum Daily Load (TMDL) for dissolved oxygen (DO) in the San Joaquin River (SJR) Deep Water Ship Channel (DWSC). The overall goal of this peer review was to develop an unbiased description of the state of scientific knowledge, including uncertainties and assumptions, directly related to management of the dissolved oxygen sag in the ship channel. A second goal was to evaluate the response of the program to previous reviews (Is the state of knowledge improving?). The information gained from the peer review is intended to be used by the SJR DWSC TMDL DO Steering Committee, stakeholders and CALFED to evaluate the adequacy of the technical information base upon which the TMDL analysis and stakeholder allocations of loads and responsibilities will be developed.

This report summarizes the issues on which there was general agreement among the peer reviewers. In addition, individual comments from each peer reviewer are included as appendices to this summary report.

Peer Review Panel

The following members of the peer review panel were present at the meeting:

- Dr. Jim Cloern, United States Geological Survey
- Dr. Steve Chapra, Tufts University
- Dr. Bill Ritter, University of Delaware
- Dr. David Beasley, North Carolina State University

The following members of the peer review panel were unable to attend the meeting, but reviewed the studies and provided comments prior to the meeting:

- Dr. Alex Horne, University of California, Berkeley
- Dr. Alan Jassby, University of California, Davis

Documents Reviewed

The following documents were provided to the peer review panel for review:

1. Synthesis of Findings on the Causes and Factors Influencing Low DO in the San Joaquin River Deep Water Ship Channel Near Stockton, CA. Lee, G.F., and A. Jones-Lee, G. Fred Lee and Associates

2. San Joaquin River Dissolved Oxygen TMDL: Interim Performance Goal and Final Target Analysis Report. Gowdy, M. and C. Foe, Central Valley Regional Water Quality Control Board

3. Strawman Source and Linkage Analysis for Low Dissolved Oxygen in the Stockton Deep Water Ship Channel. Gowdy, M., C. Foe, and M. McCarthy, Central Valley Regional Water Quality Control Board

4. City of Stockton Year 2001 Field Sampling Program Data Summary Report for San Joaquin River Dissolved Oxygen TMDL. Brown, R.T., Jones and Stokes Associates

5. Oxygen Demand in the San Joaquin River Deep Water Ship Channel, Fall 2001. Lehman, P.W., California Department of Water Resources

6. Sediment Deposition Rates, Associated Oxygen Demands and Sediment Oxygen Demands in the Deep Water Ship Channel of the San Joaquin River, Stockton, California. Litton, G.M., University of the Pacific

7. Discriminating Between West-Side Sources of Nutrients and Organic Carbon Contributing to Algal Growth and Oxygen Demand in the San Joaquin River. Stringfellow, W.T., and N.W.T. Quinn, Berkeley National Laboratory

8. Statistical Model of Dissolved Oxygen Concentration in the San Joaquin River Stockton Deep Water Ship Channel at Rough and Ready Island, 1983-2001. Van Nieuwenhuyse, E.E., USBR

9. Improvements and Calibrations of Lower San Joaquin River DO Model. Chen, C.W., and W. Tsai, Systech Engineering, Inc.

10. Downstream Tidal Exchange in the Deep Water Ship Channel near Turner Cut. Brown, R.T., Jones and Stokes Associates

11. Development of Upstream Water Quality Model. Hutton, P., California Department of Water Resources

12. DSM2 Studies to Investigate the Use of Auxiliary Flow Pumps Across South Delta Flow Structures. Hutton, P. and P. Nader, California Department of Water Resources

13. San Joaquin River Diversion Data Assimilation, Drainage Estimation and Installation of Diversion Monitoring Stations. Quinn, N.W.T., and A. Tulloch, Tulloch Engineering

14. Evaluation of Aeration Technology for the Stockton Deep Water Ship Channel. Brown, R.T., Jones and Stokes Associates

15. Proposed Project Abstract.Hydroqual, Inc.

16. Results of Peer Review of 1999 Studies

OVERALL COMMENTS

The collective understanding of the sources and causes of DO depletion in the DWSC has grown substantially in the past year. Critically important new measurements have been made and diverse analytic approaches have been applied to transform data into knowledge. Much of this effort has followed guidance from previous peer reviews. The science being applied is appropriate and logical. Although the peer review identified some remaining data gaps, much has been done in the past three years to describe and understand the problem. Many of the remaining data needs are not holes, but conflicting or confounding types of data.

In addition to continuing work on understanding the sources and potential remedies for the DO problem, it is important to identify an appropriate DO target that would be protective of aquatic organisms in the SJR DWSC system. First it is necessary to determine the ecological groups and life stages that may be impacted by low DO concentrations (just migrating fish or also benthic/aquatic invertebrates?). The next step would be to determine protective DO thresholds, and how compliance should be defined spatially and temporally.

RECOMMENDATIONS TO CALFED

A comprehensive analysis of all current data has not yet been completed. The investigators need the opportunity to exploit historical and new data to:

- Refine conceptual models of sources and causes of the DO problem
- Identify high priority data gaps
- Design a road map for filling those data gaps

This can best be accomplished by extending contracts and funding expressly for this purpose. In addition, the hiring of a facilitator to improve teamwork and help all parties understand where the data needs are will assist the investigators to fully exploit the data.

Peer Review Questions

A series of questions was developed to focus the peer review. The questions are presented here along with a general response from the peer review panel, including issues where there was general agreement among the reviewers, as well as identification of issues on which there was some disagreement.

1. **Overall Understanding: Causes, Sources, Factors**
   Are there major uncertainties in the understanding of constituents and conditions that lead to violations of the Dissolved Oxygen (DO) objectives in the San

Joaquin River Deep Water Ship Channel (DWSC)? What are the most important uncertainties that limit understanding of causes, sources and important factors or processes?

For example:

a) Do the present studies differentiate the roles of flow, tidal exchange, basin morphometry and organic matter input (or its precursors) enough to distinguish controlling factors? If not, what additional studies are necessary to allow such differentiation?
b) Do the studies or data address what range of conditions/changes might need to happen in order to see a measurable change in water quality conditions? Do they address how to evaluate change these across years?
c) Are there major information gaps that can be filled with additional study, to improve understanding of the controlling factors?

[**Management Implications**: Managers will use this review to make decisions about whether lack of knowledge will hinder development of the initial phase of a technically valid, cost-effective management plan for eliminating these violations each summer/fall. They also need to decide if there is enough knowledge to proceed with a management plan (both initial phase and longer term) for controlling the low-DO conditions in the DWSC.]

Response:
There was general agreement among the reviewers that the data have established that there is a strong correlation between flow rates and dissolved oxygen levels. However, the roles of loadings of various types and sources of oxygen-demanding materials are not well understood. Dr. Chapra suggests that an analysis of Stockton discharge records be performed to construct a multi-year time series of flow and discharge concentrations of several key variables (see Appendix B for further details).

Preliminary identification of data needs includes:

- Continuous measurements of flow, DO, and representative measurements of phytoplankton, zooplankton, nutrients and other oxygen-affecting substances. These should be collected within the DWSC, upstream of the DWSC at Mossdale, and far upstream from one or more significant tributaries. These are critical for new modeling work as well as for quantifying the driving forces into the SJR and on to the DWSC.
- Information on critical levels of DO in water (and location) for various organisms of interest, both aquatic and benthic.
- Information on the importance of thermal stratification in the DWSC.
- Information on flow augmentation resulting from permanent tidal barriers in the Delta. These would factor into a major hydrodynamic change in the SJR/DWSC system. There is a need for a better hydrologic budget for better modeling of the upper SJR system.

- Data on certain high-priority watersheds within the upper watershed (to support development of control actions). This should include data on BOD loading from upstream wetlands.
- Data to resolve disagreement on the causes of DO depletion in the DWSC (upstream algae versus local ammonia inputs).
- Characterization of the dynamics between Mossdale and the DWSC, including the effects of zooplankton and especially macrobenthic grazing on algae levels.
- Information on species variation of the algal load along the SJR, which will demonstrate whether upstream algal inputs act as a seed population, or whether a new algal community develops. This distinction has a large impact on the eventual algal load into the DWSC.
  (Note that there was some disagreement among the reviewers regarding the importance of species composition and grazing by both zooplankton and benthic bivalves. While Dr. Cloern felt this was important information, Dr. Chapra does not believe that knowledge of algal species composition or grazing rates would have a significant impact on oxygen depletion. In addition, the Hydroqual model is not set up to simulate at the species level (it simulates two algal groups).
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General additional monitoring recommendations include the following:

- Extend monitoring upstream
- Install more probes to adequately define temporal and spatial variation in DO, conductivity, temperature, turbidity, and pH
- Continue "synoptic surveys" (Hayes cruises, etc.)

It is important to coordinate all data collection activities with modeling needs. If the monitoring and research proceed without input from the modelers, there would be a risk of obtaining information that could be incompatible with the model structure (i.e., its kinetic representation, as well as it's temporal and spatial resolution). This is particularly critical if the actual allocations in the TMDL will be generated by the HydroQual model. Considering the short time frame, the team cannot afford unnecessary research or data collection that (1) measures the wrong processes or variables, and (2) do not address the proper space and time scales.

An example related to data collection deals with the characterization of nutrients and organic carbon. In HydroQual's proposed model, there are 6 forms of carbon and nitrogen, 5 forms of phosphorus, and 2 forms of silica. The present suite of measurements is inadequate to discriminate among these forms. Regardless of whether more refined measurements are implemented or the model is simplified, the modelers and measurement people have to collaborate on the data collection effort.

Additional detailed responses to these questions are presented by Dr. Chapra and Dr. Jassby, in Appendices B and E.

2. **Please address the following specific issues, in addition to the above.**

a) Modeling .

Do the models help us understand the causes of the low DO? Will the existing models allow reliable forecasts of the implications of different management actions? For what purposes are each of the specific models likely to be most effective? [**Management Implications**: Managers will want to know how much to lean on specific modeling predictions for specific purposes.]

Response:

There is a legitimate difference of opinion among investigators on the role of ammonia in the DWSC DO depletion. The 1-D model or other suitable model can and should be used to obtain a version of the oxygen mass balance for the DWSC that accounts for all of the different information (primary productivity, respiration, sedimentation rates and SOD) and resolves the ammonia controversy or better exposes basis for differing opinions. Use of the 1-D model can accomplish this in a relatively short period of time.

Process studies of 1-D model rates should be conducted. These should include but are not limited to:

- Plant productivity
- Plant respiration
- Nitrification rate
- Grazing rates

The application of a statistical model to long term data is promising and should be pursued. There are problems with the existing statistical model that must be resolved to make it a valuable tool for analysis. (Dr. Jassby gives specific recommendations in Appendix E.)

b) Allocation of Oxygen Demand Load.

Is there enough information to determine where (what sub-watersheds) load reduction feasibility studies should be conducted or how much benefit might result from specific load reductions?
[**Management implications:** Managers want to know if it is feasible to begin some load reduction pilot studies; or must decide where to begin those.]

Response:

Ammonia concentrations in the DWSC are high. This deserves serious attention. Analysis of Stockton Regional Wastewater Control Facility (RWCF) effluent data needs to be performed to verify the occurrence and completion of nitrification. Recent data on ammonia concentrations in discharges indicate that ammonia concentrations are very high and adequate nitrification/denitrification is not occurring. There was consensus among the review panel that the Stockton RWCF treatment system needs upgrading to reduce ammonia concentrations. The 1-D Systech model can be used to make initial predictions for what impact upstream load reductions could have.

The evidence identifies Mud and Salt Sloughs as the primary subwatersheds for examining possible load reduction. However, the ultimate worth of any such reductions needs to be considered more thoroughly. There might be gains in water quality, but it is not clear at this point that they would be significant with respect to the ultimate goal.

c) DO Concentration Goal:
With what certainty can we estimate, now or with further study, potential changes in DO that are possible?
[**Management implications:** Managers want to know if it is possible to achieve the interim TMDL Phase I minimum DO concentration goal proposed by the CVRWQCB staff. Would unknowns limit the feasibility of implementing the goal (and if so, how), or are they more minor challenges that might be overcome as the goal is implemented? Is it possible to constrain the range of feasible DO concentrations within which policy makers might choose a goal, or develop methodologies to do so? Can you define what such a range might be?]

Response:
It is likely that the interim DO objective can be achieved, but a variety of control measures may be required rather than a single one. In addition, the feasibility of achieving this objective depends on how compliance is defined spatially and temporally.

d) Flow:
Are there sufficient data and analysis to determine how increases or decreases in flows from different sources affect DO conditions? If not, what studies and monitoring should be undertaken?

Response:
The relationship between flow and DO conditions has been described in general terms. Further statistical analysis of historical data, as well as refinement of the Systech model would be useful, as stated in Dr. Jassby's comments (Appendix E). Whereas it had value in the initial phases of the analysis, Dr. Chapra believes that the Streeter-Phelps model is too simplistic to be of further value.

e) Aeration:
Have sources, causes and factors been addressed in sufficient detail to proceed with aeration as an approach (at least in an adaptive management mode?) What environmental uncertainties need to be addressed in a proposal for aeration (approach or technology)? What are important monitoring and adaptive

management program considerations to be implemented during a pilot aeration program?

Response:
There is a need to develop information on various aeration schemes/technologies, including performance of science-based demonstrations at pilot scale. Cost/benefit data are also needed. Detailed comments on aeration are presented in Dr. Horne's comments (Appendix D).

While there was a general consensus among the reviewers that aeration is one feasible alternative that should be further explored, especially as a short-term solution, there was also agreement that other potential solutions should be investigated. In particular, load reductions should be considered. In addition, it should be noted that the DWSC/SJR system is already heavily modified, and any further artificial modification must be performed with considerable caution. A discussion of some of the potential problems and philosophical issues associated with focusing on aeration as the primary control measure is presented in Dr. Cloerns comments (Appendix A).

Dr. Chapra (Appendix B) disagrees strongly with the notion that the fact that the "DWSC/SJR system is already heavily modified" should have any bearing on the assessment of aeration. However, he does agree that the installation of ***any*** engineered solution should be evaluated as to its efficacy, cost and environmental impact prior to being constructed. He suggests that an environmental impact assessment be included as an explicit part of the aeration pilot study. Dr. Chapra is concerned that a non-scientific controversy involving aeration could lead to delays in bringing the Channel's oxygen levels to compliance in a timely and cost-effective fashion.

Dr. Horne also disagrees with the statement that artificial oxygenation should be approached with caution. In his opinion, addition of pure oxygen should be implemented at once, to avoid further damage to the ecosystem. If and when other methods succeed in maintaining adequate DO conditions, the artificial system can be shut off. However, he believes that on cost-benefit terms, pure oxygen addition is likely to be the best long-term alternative.

Dr. Ritter agrees with Dr Chapra and Dr Horne that aeration is one approach that should be tested from a purely technical point and should not get tied up in the non-scientific controversy of further engineering or modifying the system. He believes that aeration may be the best short-term solution to improving the DO in the DWSC.

f) DWSC Geometry:
Are there challenges that have not been considered in predicting how changes in channel depth (deeper or shallower) would affect DO conditions in the DWSC? (Please also consider any strengths of the existing approach.)

Response:
The Systech model results show that the channel deepening has had a strong influence on DO conditions. There is some question as to how the geometry of the DWSC affects the settling and resuspension of sediments and oxygen demanding particulate matter. There is also a question as to the thermal stratification that occurs in the DWSC and what effect this has on the DO at various depths. In a broader sense, significant filling in or deepening the channel should have a profound impact on the structure and magnitude of turbulence. Models employing turbulence closure schemes are required to adequately assess such issues. The HydroQual and Davis/Stanford contracts should provide tools that could be valuable in this regard.

APPENDIX A

A Minority View on Structural Solutions to the Problem of Oxygen Depletions in the Stockton Deep Water Ship Channel

Dr. Jim Cloern

As a member of the Peer Review Panel that met in Sacramento on June 11-12, I struggled with the dual nature of our assignment. Our charge was to (1) provide technical evaluation on the progress made in scientific/engineering studies of the sources and causes of DO depletions in the DWSC, and (2) provide comments on the validity of aeration as a potential technical solution to this water-quality problem. The second assignment was problematic for me in two ways (beyond the fact that I have zero expertise in aeration technology), and I feel compelled to describe that conflict as a part of the public record of this technical evaluation. The views presented here are a mix of both professional judgments and personal opinions; they are presented as a minority view because they differ (in some cases substantially) from some views of my esteemed colleagues on this Peer Review Panel. These views do not represent, in any sense, the opinions of my employer or associates. They reflect the views of a research ecologist who has studied the San Francisco Bay and Delta ecosystem for 26 years.

The technical documents and oral presentations submitted to the Stockton DWSC Peer Review Panel included results from a series of studies that comprised new data collection and analyses and model-based computations. They also included two presentations describing potential structural solutions to the DO-depletion problem, one based on application of aeration technology within the DWSC and a second based on the construction of barriers at strategic locations within the interior Delta (both with and without additional manipulation of pumping across a barrier emplaced in Grant Line canal). Both concepts have merits and should be explored. However, my conflict in evaluating these concepts as a member of this particular Peer Review Panel has two bases. First, the studies submitted for review and supported with several million dollars of CALFED and other agency funds in the past year, were motivated to develop a scientific basis for establishing a TMDL (total maximum daily load) to solve this water-quality problem. The key letter here is "L", and inherent in the scientific and review processes is the central notion that, at least partial, solution to this problem will include actions to reduce Loadings of organic matter that accumulates in the DWSC, fuels microbial metabolism, and in the process consumes oxygen faster than it can be replaced by natural processes. Indeed, most of the new research funded by this program from 1999-2001 was designed specifically to identify those loads contributing to episodic DO depletion. My first conflict in evaluating proposed structural solutions is their (apparent) disconnect from the TMDL process and concept. Certainly part of my conflict must come from my own ignorance of the overall context of the full-stakeholder effort to solve this problem. In the absence of that overall context, those outside the process might conclude that efforts are now being conceived to circumvent the TMDL process by building new structures. It is therefore essential for us to consider and present a broad range of possible solutions to the DWSC-DO depletion problem; it appears the consensus view is that the ultimate solution will require multiple actions to attack this complex problem from multiple angles. However, it is also essential for us to describe those solutions in the context of the motivating TMDL process, and for us to conceive and present a balanced set of alternative solutions that includes serious consideration of investments to reduce source Loads. That balanced view of pragmatic solutions, including steps of source reduction, was not included in the written and oral reports submitted for peer review. I encourage stakeholders interested in this important water-quality problem to consider the importance of a balanced overall strategic solution to this problem, including the potential benefits of Load reduction as part of a solution integrating multiple action strategies. I agree with my Peer Review Panel colleagues that there is a scientific basis for exploring aeration technologies as a potential contributor to a multi-pronged solution to this problem. I hope that exploration of structural solutions is not pursued as a stand-alone allocation of resources that deflects our collective efforts away from the pursuit of solutions that include actions to reduce Loadings of oxygen-consuming constituents.

My second conflict comes from our rich history showing that structural manipulations of aquatic ecosystems almost always have unanticipated consequences. We did not anticipate introductions of lampreys to the Great Lakes when we built the Welland canal connecting them to the Atlantic. Only forty years ago we did not know that development in the watershed of Lake Tahoe would measurably alter its transparency and water quality. In our own Delta system, we did not know that dredging the San Joaquin River for deep-draft ships would create a physical system that traps (or retards the transport of) oxygen-consuming substances, leading to oxygen depletion. Neither did we anticipate the biological consequences of shipping, including introductions of exotic species that have transformed the ecosystem and altered processes supporting native biota from zooplankton to fish. We are just now beginning to understand how manipulations of hydraulics through, for example, emplacement of rock barriers, can alter Delta-wide transport patterns and water quality. These past experiences tell us that we are not yet smart enough to anticipate the complex suite of interacting ecosystem responses when we attempt to engineer a structural solution to a specific problem. This implies that there is large uncertainty in the outcomes of structural changes, not only in their effectiveness at solving the target problem, but also about complex interactive effects that propagate to change other ecosystem attributes such as bathymetry, flow patterns, water quality, ecosystem processes, or habitat value. If we consider new structural manipulation of the already highly-engineered Delta as solutions to specific problems such as DO depletion, I encourage concerned stakeholders to consider careful and thoughtful analyses of the uncertainty and range of ecosystem-wide impacts of structural change in addition to evaluations of economic costs and benefits. I also encourage proponents of structural solutions to consider design and implementation in an adaptive management framework that includes careful measurement of ecosystem-scale responses and adaptive integration of emerging new knowledge into evolving operations strategies geared to minimize unanticipated responses – i.e., to minimize the chances that further structural manipulations will create new ecosystem impairments.

APPENDIX B

San Joaquin DWSC DO TMDL

Dr. Steven C. Chapra
Tufts University

Introduction

This report constitutes my assessment of the present status of the CALFED San Joaquin DWSC DO problem. In particular, it addresses how the state of scientific and engineering understanding relates to system management and decision making.

The investigators have done a credible job of establishing the magnitudes of the major processes governing the evolution of low dissolved oxygen (DO) in the DWSC. Although more research should be implemented, I believe that present understanding forms a scientific basis for the initial phase of a technically valid, cost-effective management plan for eliminating DO violations each summer/fall.

This report is divided into two sections. In the first I summarize the science and its implications. The second offers my response to a series of questions posed by URS. These responses include action items that I believe should be implemented.

Science Summary

Based on the present state of science, (gleaned from my readings and the presentations at the Sacramento meeting on June 11-12, 2002), the following summarizes my understanding of the present situation:

The investigators provide persuasive data correlations and model simulations to support the thesis that oxygen levels in the DWSC are highly dependent on the magnitude of flow from the San Joaquin River. Thus, raising water flow would be expected to be an effective and predictable tool to ameliorate the oxygen problem.

Although sediment oxygen demand (SOD) and the Stockton WWTP discharge certainly have an impact on oxygen, the primary source of oxygen demand is the San Joaquin River.

The impact of SOD is not insignificant. However, the investigators (Litton 2002) have determined that it is not a dominating mechanism. This is partially due to the impact of tidal motion and scour on delivery of organic matter to the sediments.

For the Stockton discharge, the issue is ammonia. During mid-summer, the concentrations in the effluent are on the order o 0.5 to 3 mgN/L. In August, the effluent concentrations begin a linear increase that in 1999 and 2000 culminated in levels of 25 mg/L at the end of October. Mass balance assessments of the relative impact of the SJR and Stockton discharges by Lee and Jones-Lee (2002) and Chen and Tsai (2002) indicate that the river loading (as expressed as ultimate oxygen demand) is considerably larger than that from Stockton. That said, it should be stressed that the Stockton load is not insignificant. Its form (oxidizable ammonia) and proximity to the DWSC suggest that it should be included in an integrated program. Further, the issue of heightened levels in the fall suggest that it could become more prominent if future low-flow conditions occurred at that time of year.

The San Joaquin River source consists of several components. The river has ammonia levels of approximately 0.2 mgN/L that would directly exert an oxygen demand in the channel in the same fashion as the Stockton discharge.

In addition, the river carries live phytoplankton (chlorophyll *a*), dead phytoplankton (phaeophytin), and detritus (volatile suspended solids corrected for the plant related biomass). Because of their organic carbon and nitrogen content, a portion of the dead phytoplankton and detritus will eventually hydrolyze and oxidize in the DWSC. The fraction that would oxidize would increase under low flows when the residence time in the channel would be on the order of 14 to 28 days.

The phytoplankton impact is more subtle. First, sedimentation of phytoplankton cells is diminished by the effect of the tidally-induced resuspension. Thus, the impact of phytoplankton on oxygen reduces to the balance between oxygen-producing photosynthesis and oxygen-consuming respiration.[1] Depending on the relative magnitudes of growth and respiration, the plants can represent a net oxygen gain or loss.

Synoptic data (as shown in plots in Chen and Tsai 2002) indicate that the system has abundant nutrients and hence is not nutrient limited. Total inorganic nitrogen levels (ammonia and nitrate) are on the order of 1000 to 2000 μgN/L in the late summer. This is well in excess of nitrogen half-saturation constants which range from 10 to 25 μgN/L (Chapra 1997). Although I could not find plots of available phosphate, high total phosphorus concentrations (on the order of 100 to 200 μgP/L) suggest that phosphorus is also abundant. Typical half-saturation constants for inorganic phosphorus range from 1 to 5 μgP/L (Chapra 1997). Thus, solely based on nutrient levels, one would expect the system would have high productivity relative to respiration.

However, the phytoplankton are highly light limited due to the large quantity of suspended solids in the water column. Estimates were provided that photosynthesis was limited to the top 2 meters of the 10 meter deep channel. This is primarily due to the high inorganic solids entering the system from the San Joaquin River drainage basin. In addition, organic solids (phytoplankton, phaeophytin and detritus) also have an impact on water clarity. Within the channel, the problem is exacerbated by tidal scour, which tends to diminish the net sedimentation of particles within the DWSC. Because of all these

[1] Although it would be of less importance, their death would subsequently convert the plankton into nonliving organic matter subject to hydrolysis and oxidation.

factors, respiration dominates and the phytoplankton represents a net debit on the system's oxygen resources.

In summary, the magnitude of the DWSC oxygen problem is primarily dictated by flow, depth and turbidity. The depth increases the channel's residence time so that oxidation and respiration processes depress daily oxygen concentration to critical levels during low flow. The turbidity exacerbates the situation by making the phytoplankton a net oxygen sink.

The foregoing summary can provide some perspective on possible control options:

- Raising summer flows. This would be the simplest solution. However, other considerations such as water supply and irrigation needs might make it unfeasible.
- Stockton reduction. Reduction of the Stockton ammonia discharge would improve oxygen levels somewhat. However, it should be recognized that this alone would probably not be sufficient to solve the problem.
- SJR reduction. Reduction of the phytoplankton and the nonliving particulate organic matter and ammonia from the SJR would ameliorate the problem. However, this alternative may not be as straightforward as it would seem. This is because the DWSC is light limited and nutrient enriched, and hence it is unlikely that very high (and probably unrealistic) nutrient reductions would be needed to significantly improve the system.
- Artificial aeration. The foregoing suggests that engineering solutions such as artificial aeration might represent a viable, cost-effective, short-term solution. The preliminary assessments made by researchers (Brown) support this contention.

Based on the foregoing analysis, I would make the following general recommendations:

- A pilot study of aeration should be implemented as soon as possible. This should be a field study to assess the effectiveness and cost of this technology. During the workshop it was suggested that lab studies also be considered. I believe that this would not be fruitful as aeration technology is well developed and has been applied effectively around the world. At this point, the field studies would more efficiently resolve the question of aeration's efficacy in the present case.

- The one-dimensional water quality developed by Systech should be used prominently as a tool in the initial phase assessing technically valid, cost-effective management plans. Although more advanced tools will be required in later phases (e.g., the proposed HydroQual and Davis/Stanford work), the Systech model is adequate to make trade-offs in the initial phases.

- Further research and data collection should be conducted. However, decisions regarding such efforts must be highly coordinated with HydroQual. Some suggestions for specific research and monitoring efforts are elaborated in the following section.

Questions

The following section consists of answers to a series of questions that were provided to me by the URS Corporation. These include suggestions for specific action items that should be implemented.

1. Overall Understanding: Causes, Sources, Factors

Are there major uncertainties in the understanding of constituents and conditions that lead to violations of the Dissolved Oxygen (DO) objectives in the San Joaquin River Deep Water Ship Channel (DWSC)? What are the most important uncertainties that limit understanding understanding of causes, sources and important factors or processes?

a. Do the present studies differentiate the roles of flow, tidal exchange, basin morphometry and organic matter input (or its precursors) enough to distinguish controlling factors? If not, what additional studies are necessary to allow such differentiation?

i. Flow, tidal exchange, and morphometry. The present studies demonstrate the direct effect of San Joaquin River flow on dissolved oxygen concentrations in the DWSC. They also indicate the extent of upstream intrusion due to the tidal exchange. These factors, along with system morphometry, have been integrated into a one-dimensional water quality model of the system. Such a model provides a vehicle for predicting the impact of system hydraulics and loadings on the oxygen levels in the DWSC.

Although these characterizations are not at a high level of resolution, they are adequate to address initial questions related to remediation of the DWSC oxygen problem.

ii. The impact of inputs. The two primary inputs of oxygen demanding loadings have been identified as (1) the inflow of organic matter from the San Joaquin River and (2) the ammonia loading from the City of Stockton.

Action Item: An analysis of Stockton discharge records should be performed to construct a multi-year time series of flow and discharge concentrations of several key variables including nitrogen species (not only ammonia, but also organic nitrogen), $CBOD_u$ and dissolved oxygen. One goal of this analysis would be to accurately characterize the seasonal trends of ammonia discharge from the pond. In particular, the analysis should establish the timing of the rise in ammonia discharge that occurs in the fall and the subsequent reductions that would occur in the spring.

b. Do the studies or data address what range of conditions/changes might need to happen in order to see a measurable change in water quality conditions? Do they address how to evaluate change across years?

The present analyses does a good job of explaining conditions during the growing season. Because of the small importance of SOD, it would be expected that the system should adjust quickly to loading or flow changes.

Winter conditions are not as well characterized. In particular, several instances of low dissolved oxygen have been observed during the non-growing season. Therefore, although the present state of knowledge supports analysis of the summer low-flow conditions, further study is necessary to characterize winter oxygen levels.

Action Item: Available time series data collected with data sondes should be systematically analyzed to ascertain the magnitude and frequency of low dissolved oxygen conditions during the winter. The first goal would be to evaluate whether winter low oxygen episodes are a significant recurring phenomenon. If so, an initial evaluation of possible causes should be performed. For example, the correlation of low oxygen with low flow should be analyzed.

Action Item: If the previous action item indicates that low winter DOs are significant, some winter surveys and model analyses should be conducted to quantify the cause/effect relationships underlying the phenomenon.

c. Are there major information gaps that can be filled with additional study, to improve understanding of the controlling factors?

Study and observation are needed in a number of areas:

Further rate experiments should be conducted to quantify the rate constants for nitrification, plant growth and respiration. In particular, there is a major discrepancy between model and bottle estimates of productivity (Chen and Tsai, Lehman). As Chen and Tsai point out, bottle rates can reflect artifacts due to the enclosure process. On the other hand, the order of magnitude discrepancy that presently exists seems too large. A simple test of Lehman's rate would be developed by using her rate in the Chen and Tsai model to assess the impact on the oxygen calibrations. Another approach would be to compare model predictions of diurnal oxygen swings with measurements on the river. HydroQual should be consulted to solicit there ideas for process studies to strengthen their model development.

Routine monitoring should be continued. This will provide HydroQual with additional data sets for their model calibration/corroboration process. Because they are developing a three-dimensional model of the channel, I expect that they will require more detailed spatial (vertical and lateral) and temporal (more diurnal automated sampling) sampling. Further, better characterization of the SJR upstream of the DWSC will be necessary (particularly to address questions related to the impact of upstream remedial measures on the delivery of oxygen demanding load to the DWSC.

2. Special Issues

   a. Modeling. Do the models help us understand the causes of the low DO? Will the existing models allow reliable forecasts of the implications of different management actions? For what purposes are each of the specific models likely to be most effective?
   [Management Implications: Managers will want to know how much to lean on specific modeling predictions for specific purposes.]

   I was surprised at how little the Systech model was referenced during our workshop. Although it could certainly be improved (by improved data and rate measurements), it is a technically sound tool for making initial assessments.

   b. Allocation of oxygen demand. Is there enough information to determine where (what sub-watersheds) load reduction feasibility studies should be conducted or how much benefit might result from specific load reductions?
   [Management implications: Managers want to know if it is feasible to begin some load reduction pilot studies; or must decide where to begin those.]

   I believe that there is sufficient information to make initial assessments of load reduction scenarios. Because the Systech model begins 20 kilometers upstream from the Stockton outfall, it can not only assess direct inputs such as Stockton, but could also provide good initial estimates of the impact of load reductions to the river. As mentioned in point 1, further study of the river would strengthen such applications.

   c. DO concentration goal. With what certainty can we estimate, now or with further study, potential changes in DO that are possible?
   [Management implications: Managers want to know if it is possible to achieve the interim TMDL Phase I minimum DO concentration goal proposed by the CVRWQCB staff. Would unknowns limit the feasibility of implementing the goal (and if so, how), or are they more minor challenges that might be overcome as the goal is implemented? Is it possible to constrain the range of feasible DO concentrations within which policy makers might choose a goal, or develop methodologies to do so? Can you define what such a range might be?]

   It depends on how the DO concentration goal is posed. For example, if the goal is couched as an average water-column concentration, the present model is adequate. However, if the goal is allowed to cover grab samples taken at various depths, then a 2-D approach would be necessary.

d. Flow. Are there sufficient data and analysis to determine how increases or decreases in flows from different sources affect DO conditions? If not, what studies and monitoring should be undertaken?

   Sufficient data and analysis are available for preliminary assessment. However, as stated above, a better characterization of the river would greatly strengthen the assessment of upstream sources.

e. Aeration. Have sources, causes and factors been addressed in sufficient detail to proceed with aeration as an approach (at least in an adaptive management mode?) What environmental uncertainties need to be addressed in a proposal for aeration (approach or technology)? What are important monitoring and adaptive management program considerations to be implemented during a pilot aeration program?

   Sufficient preliminary investigation has been conducted to support a pilot study of aeration. The technology has been implemented in numerous locations around the world and has proven useful in raising oxygen levels in estuaries, lakes and rivers.

   I disagree with the notion that the fact that the "DWSC/SJR system is already heavily modified" should have any bearing on the assessment of aeration. Such "beliefs" have no place in a scientific review. That said, I would agree that the installation of ***any*** engineered solution should be evaluated as to its efficacy, costs and environmental impact prior to being constructed. One possible way to reconcile this disagreement might be to include environmental assessment as an explicit part of the pilot study.

   It is a fact that the entire San Joaquin River Basin and Delta has already been profoundly "modified" by human activities such as population growth, agriculture, navigation channels, dredging, canals, water transfers, etc. At this juncture, it appears that outside of flow augmentation, the installation of relatively low-cost aeration could offer the only viable short term solution to attain acceptable oxygen levels in a light-limited, nutrient-rich system like the DWSC. I am concerned that a non-scientific controversy involving aeration could lead to delays in bringing the Channel's oxygen levels to compliance in a timely and cost-effective fashion.

f. DWSC Geometry. Are there challenges that have not been considered in predicting how changes in channel depth (deeper or shallower) would affect DO conditions in the DWSC? (Please also consider any strengths of the existing approach.)

   The existing Systech model could be run with a different depth to make an initial assessment of deepening the channel. However, the 3D models would be necessary to really appraise the impact of deepening on vertical gradients, horizontal mixing and scour at the sediment bed. In a broader sense, significant filling in or deepening the channel should have a profound impact on the structure and magnitude of turbulence. Models employing turbulence closure schemes are required to adequately assess such issues. The HydroQual and Davis/Stanford contracts should provide tools that could be valuable in this regard.

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APPENDIX C

Personal Comments on the Peer Review Process and Workshop for the SJR-DWSC Dissolved Oxygen TMDL

Dr. David B. Beasley
North Carolina State University

First, I'd like to express my appreciation to CALFED, URS, and all of the workshop participants and panelists for allowing me to participate in this process. The fact that many different points of view were presented in a generally collegial and informative atmosphere leads me to believe that the researchers, agency people, stakeholders, and general public will find a way to develop a solution for the problems we looked at and for the problems yet to be addressed. As the teacher of a course entitled "Land Resources Environmental Engineering," I can certainly state that there were some very good examples of how to do things and a few that probably fall into the how not to do things. Thanks for the course update!

As I stated a couple of times during the workshop, those of us who have worked primarily in the South, East, or Midwest are amazed at the complexity of the system we were asked to look at. The laws governing water rights and water use are VERY different from the ones we are used to. The fact that water seems to flow uphill in California (at least in some places) is also a bit different. And the most interesting and confounding thing is the sheer contention for the water resource.

Having been involved in the U.S. vs. Florida (South Florida Water Management District) Everglades court case, I can say that I've worked with one system that was just about as complex and had just about as many competing priorities and constraints on the water as the SJR-Sacramento Delta system has. The experience in Florida led me to say some of the things I said in the workshop, which I will repeat in these comments.

The panelists and others received Jim Cloern's comments at the end of last week. In those comments, Jim stated what he called a minority view on our work and particularly the role that aeration might/could have in the SJR-DWSC. I fully support his concerns. During the meeting, I voiced a concern that aeration was a quick fix that addressed the symptoms and not the problem. As the meeting progressed, I softened my stance on aeration and agreed that conducting research on the technologies and locations of aeration systems was a reasonable interim step to take.

However, I still believe that the Delta community MUST decide what the primary expectations for the SJR-DWSC are. It is already the most "unnatural" river system I'm aware of. Any of a number of seemingly disconnected management actions could be causing the DO problem we are looking to solve.

The sheer existence of the SJR-DWSC is one of those problems. I doubt it is going to go away, and, in fact, it may very well be enlarged (deepened). Another problem is the serious diversion of as much as half (or more) of the flow that normally enters the upper end of the SJR-DWSC for purposes of supplying water to points west and south of the Delta area. High loadings of ammonia from the wastewater treatment facilities of Stockton can, at times, have very pronounced impacts on DO reduction within the SJR-DWSC. The impact of drainage from agricultural and wetland areas upstream certainly seem to promote the growth of algae in the river, which ultimately becomes an oxygen demanding load in the slow moving, turbid SJR-DWSC.

Any workable, long-term solution to the DO problem should involve all of those causative elements. As additional TMDLs are required for some or all of the SJR system, those same entities will all have to be involved. The flow manipulations, agricultural uses, municipal/industrial extractions and inputs, and geometry modifications that are now an integral part of that river must ALL be held accountable for performance changes and must, therefore, be required to participate in finding solutions to the identified problems. This means that water law itself will likely have to be modified. It means that users in other areas who benefit from water coming from the SJR should help pay for remediation efforts that result at least in part from that use. It means that parties that maintain and use the DWSC and derive financial gain from its existence should also be required to help pay the costs associated with maintaining reasonable water quality in the river.

On the other hand, it is incumbent on some of the beneficiaries of higher quality river water to determine what level of quality is actually needed. It is not at all clear to me at this point that an adequate understanding exists of the impacts that lowered DO in the SJR-DWSC have on aquatic and benthic communities. I am not convinced that the current water quality objective for dissolved oxygen is reasonable, particularly given the very unnatural state of this river system. Although a fair amount was said about impacts of low DO on salmon migrations, I was not convinced that the migrations in the SJR system have been substantially or even marginally impacted. Is there compelling information to that effect? If we reduce algae production (and DO consumption) upstream, what will be the impact on the Bay? Could "fixing" the problem in the DWSC actually lead to a worsening of the situation somewhere else?

I think we pretty well stated the data needs/gaps. I also think the role of models (existing and "to be developed") is appropriate. I believe the collegiality issue is one that needs addressing within the research group. It appeared to me that the level of data and knowledge sharing could be improved and that some of the disagreements could be internally solved if communication lines were firmer. Although teamwork is, at times, a difficult thing to achieve, the impact on results can be profound.

In closing, I believe that the work conducted to date is scientifically valid and that many of the identified information gaps (from previous reviews) have been filled. Several remaining areas of information/data shortfall were identified and I concur with those findings. I believe the researchers, advisory committee, agencies, and stakeholders can all achieve more by improving their teamwork skills. The hiring of a facilitator, with that primary charge, is highly encouraged. Finally, I think the ultimate goal of this work should be to produce a sustainable solution to the DO problem in the SJR-DWSC. Aeration is an achievable interim goal, but it is not, by itself, a solution to the problem of low DO in the DWSC.

APPENDIX D

REVIEW OF THE CALFED BAY-DELTA PROGRAM ON THE SAN JOAQUIN RIVER DEEP WATER SHIP CANAL

Dr. Alex Horne
University of California, Berkeley

SUMMARY

The 14 papers and the short peer reviews provide a good background for making decisions regarding the causes and solutions for the depressed dissolved oxygen (I)DO) levels in the Ship Canal. The authors of these reports and the CALFED organizers that coordinated the program should be congratulated. The work, taken as a whole, at last provides the framework for a logical set of short and long-term solutions to the DDO problems. The field, laboratory and modeling work can be compared and the most robust solutions selected. The sources and sinks of the DDO, the spatial and temporal measurements of DDO drivers have been adequately characterized, although of course some further measurements may be needed, especially for the diffuse upstream sources. In particular, the general agreement on the numerical values of the DDO drivers is most pleasing. My recommendations are made much more easily and with much more confidence due to the good work done by CALFED and the suite of scientists and engineers who participated in this study.

My conclusions, based on the 14 documents and my experience with systems similar to the Stockton Ship canal are the following:

1. The DDO is primarily due to the deepening of the ship canal for navigation and would not occur if the channel were its original 8- 10 feet deep.
2. The onset of DDO is initiated by reduced vertical mixing under lower flow conditions and presumably calm, warm weather.
3. The sources of DDO include ammonification of discharged wastewater and local algae decomposition.
4. Nutrients to support algae growth are super-abundant and are derived from both wastewater and agricultural runoff.
5. lnflowing BOD plays a relatively minor role in DDO
6. Rates of DDO in the water and sediments are typical of eutrophic waters of many kinds and do not show any abnormal influences.

My recommendations for the solution of the depressed dissolved oxygen (DDO) levels in the Ship Canal are the following:

<u>SHORT-TERM NEXT 1-5 YEARS, 2003 TO 2008).</u>

- **Channel oxygenation by submerged pressurized no-bubble oxygen additions.** Possible alternatives include various forms of aeration but these would be less efficient and less reliable in terms of meeting the DO for fish. A pilot project is not necessary since the full-scale system could be installed with current knowledge. Estimated capital cost $2 to 6 million, annual 0 & M costs $250,000 to $400,000 (costs could be substantially lowered by using sustainable energy, especially a combination of solar and wind power.
- **Nitrification of the ammonia from the City of Stockton Treatment Plant.** Ammonia should not be discharged at about - 2 mg/L at any time and nitrification is needed prior to dentrification in the proposed treatment wetlands. Estimated capital costs would range from $20-50 million depending on the design used and the degree of nitrification required. Annual O&M costs would depend on the methods used.
- **Installation of specifically designed nitrate and phosphorus removal wetlands for the City of Stockton Treatment plant.** Eutrophication in the ship canal and the adjacent river is a major cause of DDO and should be reduced since inexpensive methods with multiple use benefits such as wetlands have now been developed for large systems. In addition, long-term consideration of drinking water uses downstream and coastal ocean related human and large sea mammal deaths require nutrient removal (both N and P) from all wastewaters in the state. Capital costs would range from $3-30 million and annual O&M costs about $250,000.

<u>Minimum (2-10 YEARs, 2003 To 2012)</u>

Installation of specifically designed nitrate and phosphorus removal wetlands for the upstream agricultural wastewater, including those from wetlands used primarily to rear ducks for hunting. Eutrophication in the ship canal and the adjacent river is a major cause of DDO and should be reduced since inexpensive methods with multiple use benefits such as wetlands have now been developed for large systems. However, the details of the diffuse sources may need some further investigation to select the most important ;fnd most suitable for wetlands treatment. Estimation of capital costs for the agricultural treatment wetlands are hampered by lack of data but, based on other sites should be in the range of $ 10 to $ 100 million, primarily depending on the amount of phosphorus to be removed (nitrate will be easily denitrified to gas) and the cost of land at the wetland sites.

<u>LONG-TERM (20 YEARS HENCE, ~2020)</u>

- **Removal of the 303d listed status** of the ship canal in terms of dissolved oxygen.
- **De-commissioning of the oxygenation system** when the other sources of DDO have been eliminated

DETAILS: REVIEW OF THE SAN JOAQUIN RIVER DEEP WATER SHIP CANAL TMDL DISSOLVED OXYGEN TMDL

Overall

The field, laboratory and modeling studies presented in the report are a good basis for understanding the system and making recommendations to solve the problem The authors and all the people involved should be congratulated on a job well done for CALFED and the State and Regional Boards charged with the TMDL process. The information presented in the 14 papers that make up the report provide good data and complement each other. I find no internal contradictions between the different kinds of work and the conclusions reached. In addition, the data falls into the range generally to be expected of this kind of turbulent system. Thus I find the data to be robust in the sense that conclusions based on the work will be unlikely to be incorrect.

There was a question on the level of dissolved oxygen needed and some discussion of the needs of particular fish etc. These questions could promote research forever or we could just use the EPA-type standards. These are usually 5 mg/L for most fish (as low as 3 mg/L for some like carp and catfish and as high as 7 mg/L for some salmonids). Since the values are in absolute units they could be higher than the saturation value at the highest water temperatures (I do not know these but estimate that 32^{o}C would be required before the 7 mg/L, 100% saturation was exceeded).

An alternative method is to ensure that chemical, rather than biological, needs are met at a minimum. To prevent internal loading of nutrients, the most common cause of eutrophication in shallow waters, DO in the sediments must be maintained above zero (actually it's a redox of about 0 mV). Since both DO and redox measurements in the sediments are not normally carried out, my rule of thumb is to maintain DO in the bottom meter of water at >2 mg/L. The fish would not be very happy (zooplankton would rejoice at the lack of predation, however). So there is a mimimum chemical target to shoot for.

THE CAUSES OF DDO IN THE SHIP CANAL.

The overall picture of the causes of DDO in the canal were well reviewed by Lee and Jones-Lee (2002) in the first of the 14 papers. Many of their conclusions are repeated here.

Dredging to ship depths results in deeper water and DDO. The reports correctly report that a shallow, un-dredged canal 8-10 feet deep would probably not experience DDO, at least to below 5 mg/L (Lehman, Chen & Tsai, 2002), since wind and river flow mixing would provide all the DO needed via direct atmospheric additions. The conclusion is very important since it is not obvious to everyone that the increased supply of oxygen provided by deeper water is more than counterbalanced by the inability of wind action to move DO to the sediments and deepest water via wind mixing or noctural convection. Thus the DDO is partially due to the need for deep water for ship's passage.

Nitrification of ammonia and phytoplankton decay are the two main causes of DDO. Nitrification of ammonia from the Stockton wastewater effluent and the decay of phytoplankton accounted for 91% of the DDO in the channel Lehman, 2002). The role of other BOD sources such as drainage from upstream lands is small in the DDO area.

Ammonia from wastewater (?) is the major factor in DDO. Nitrification of ammonia in the treatment plant or in sand beds would remove a major source of DDO. Reduction of algae will require further work.

Role of advective flows of DO from outside the DDO region. The reports indicate that above certain net advective flows (- < 1,000 cfs) natural influx of DO from outside the region satisfied the DDO. However, satisfying DO by importing water which has a surplus DO of only about I mg/L is a very unsatisfactory and inefficient way to solve the DO problem for fish in the deep channel. Thus other methods are needed; either direct oxygen addition or reduction in DO demand from algal decay and nitrification of the added ammonia.

Role of nutrients. Currently, as might be anticipated from the mixed system and high absolute concentrations of nutrients, light limits the growth of planktonic algae and restricted active photosynthetic oxygen production to the upper couple of feet (Lehman, 2002). However, nutrient reduction. would eventually come into play as nutrients were reduced. The reduction of nutrients should thus be a goal of the project.

SOLUTIONS FOR THE DDO IN THE DEEP SHIP CHANNEL

The solutions presented are based primarily on the 14 papers and my experience with other similar sites. The solutions are presented in approximate order of importance with the most important listed first.

1. NITRIFICATION OF AMMONIA, ADDED VIA THE STOCKTON WASTEWATER DISCHARGE should be a main goal for the project. Nitrification in waste treatment plants is simple and can be relatively inexpensive. It would reduce the DDO by about 60%

2. REDUCTION IN CARBONACEOUS BOD BY PURE OXYGEN ADDITION. Carbonaceous BOD, mostly fresh phytoplankton, accounts for about 30% or perhaps more of the DDO in the deep ship canal. Carbonaceous BOD can be alleviated by direct oxygen addition using pure oxygen or possibly by aeration. Such methodology was well reviewed by Brown (2002) in one of the 14 papers, although his costs for oxygen may be too high due to the inflation in energy costs in 2001-2002.

The most obvious method for oxygenation of the channel and to reduce DDO would be to use pure oxygen. Liquid oxygen is an inexpensive commodity ($50-90/ton) and is less expensive and more efficient than compressed air on both an instantaneous and life-cycle basis. For example, since air is composed of only 21% oxygen, all equipment using compressed air is five times larger than that needed for pure oxygen. In addition, if the oxygen is pressurized, the oxygenation system can be as much as 25 times smaller than a comparable compressed air system,

Costs of oxygenation. The estimated DO demand is fairly similar in the papers presented (Gowdy et al., Litton, 2002; Van Nieuwenhuyse, 2002; Brown, 2002.). Using the value of about 500 tons/yr (million lbs/y), I estimate the annual cost of $250,000/yr for oxygen (@ $50/ton). Higher costs would occur if oxygen price was higher but I recommend that sustainable on-site oxygen production be used with solar and wind power and PSA molecular sieve methods being used. It should be noted that EBNIUD at Camanche Reservoir uses about 700 tons of oxygen per year with annual running costs for both oxygen and electricity for the 3 5 cfs water pump amounting to a total annual cost of $60,000 to $90,000 per year over the last decade. The oxygen system proposed here is relatively small. The TVA uses more pure oxygen in a week (100 tons/d) for just one of its reservoirs than is proposed for the entire year at the Stockton Ship Channel.
Capital costs depend on the design chosen and on on-site versus imported oxygen supplies. Based on similar sized systems the capital costs would be in the range of $2-6 million.

Choice of oxygen rather than compressed air. The prime reason for an oxygen rather than a compressed air system is the nature of the DDO problem in the Stockton Ship Canal as shown by several of the 14 papers. The channel's DDO problem is due to its depth (Lehman, Chen & Tsai, 2002), meaning that the key oxygen depletion must be in deep water. The majority of natural photosynthetic oxygenation and natural aeration comes from the surface and most of the oxygen depletion from the bottom. Thus what is needed is a horizontal zone of oxygen layered over the bottom rather than a vertical jet of air bubbles rising rapidly to the surface. A typical air or oxygen bubbly of 0.2 cm diameter rises at about 22 cm/sec, meaning that there is only contact between bubble and water for 45 seconds in a 30-foot water column. That is not enough time for much oxygen transfer and the system acts as a vertical mixing device that could easily be replaced with a propeller. The use of a pressurized, no-bubble system composed of pure oxygen is the obvious solution to the Stockton DDO problem since it would give a layer of high DO at the greatest density and would layer over the bottom. One version of the no-bubble system, a Speece Cone, has been a wonderful success in the 425,000'acre-foot Camanche Reservoir on the Mokelumne River (EBMUD) and totally overcame a serious DDO problem that involved not only low DO but also toxic hydrogen sulfide. The Camanche Speece Cone also reversed eutrophication and lowered chlorophyll a about 10 fold. Some results from the Camanche work are attached.

In the case of the Stockton Ship canal I recommend that a 100-foot deep well be used to pressurize the pure oxygen during addition rather than a submerged Speece Cone. The reason is that Camanche Reservoir was 90 feet deep so a cone on the bottom would provide 3 atmospheres of pressure. The ship canal is much less deep so an other system is needed. The well could be sited at the edge of the canal and water would be pumped from the deepest most oxygen depleted zone and passed down the well. At the bottom oxygen would be added giving a concentration of about 50 mg/L in the outlet manifold. The now oxygenated water would be passed back to the deepest parts of the channel where jets would reduce the open water oxygen concentration to well below that known to produce gas bubble disease in fish. The method proposed is very similar to the "side stream" system used over 40 years ago by the TVA and similar to the system I designed that is now installed in the Canning river-estuary in Perth Australia. The system described is almost identical to that proposed in April 2002 for solving the DDO problem in Upper Klamath Lake, Oregon.

3. NUTRIENT REDUCTION. The longer-term reduction of algae requires reduction in nutrients. Is this possible in the current situation? There are two main sources of nutrients; the Stockton wastewater treatment plant and the upstream agricultural discharges and drainage . The reports indicate that phosphorus may be limiting algae growth at times but this is probably due to gross nitrate pollution from the upstream agriculture. Water dominated by wastewater discharges is always limited by nitrogen since the ratio of biologically available NT is 3: 1, relative to the 10: 1 ration present in living matter. It can be concluded from the measured P-limitation that agricultural pollution is the major nutrient problem to be tackled, paradoxically by reducing nitrogen in the agricultural discharges. Combined with the need to reduce phosphate and ammonia in the waste discharge, the most obvious solution is the reduction of both nitrogen and phosphorus in both major discharges; agricultural and wastewater.

There are several methods of reduction of bioavailable nitrogen (ammonia and nitrate) and phosphorus (phosphate and much of the organic-P). These methods are reviewed below.

Conventional, non-sustainable methods of nutrient removal.

Phosphorus in wastewater effluents is composed almost entirely of bioavailable soluble phosphate that can be removed by precipitation with trivalent metal salts such as alum. However, the process is costly in space and imposes a large non-sustainable fee in terms of disposal of the alum sludge. Nitrogen can be reduced, if removed completely, in waste treatment plants by nitrification at the end of the treatment process followed by denitrification. Nitrification reduced TIN from about 25 mgAL (as ammonia) to about 15 mg/L as nitrate. Values as low as 5 mg/L can be reached using denitrification but the cost is quite high.

Sustainable nutrient removal using specifically designed constructed treatment wetlands.

Wetlands can remove nutrients as well as other pollutants such as heavy metals, pathogens, pesticides and many other organic compounds. However, natural wetlands remove almost no pollutants since the hydraulics favor rapid passage through channels not slow flow across the leaf litter and biofilm. In contrast, properly designed treatment wetlands provide the proper hydraulics and can successfully remove nitrate and phosphate.

Nitrogen removal. It is much easier to remove nitrate than phosphate using wetlands. For example, the 90 mgd nitrate removal wetland at Prado, Riverside County (Orange County Water District) reduces nitrate from 10 to I mg/L in 7 days in the 500 acre Prado Wetland. This wetland, as in others that I designed, also function as superb wildlife habitats (see Ecological Engineering, 2000 Vol 14 pp 1-80).

Capital costs of the nitrate removal wetlands vary primarily with the concentration of the inflowing nitrate and the desired target for removal. I normally use a target of I mg/L N03-N as the outflow concentration. For a 10 mgd plants nitrification in the treatment plant costs between $20-50 million and denitrification in the wetland would cost about $1.2 million (150 acres@ $8,000 per acre). 0 &M costs for a 150 acrewetland would be about $150,000 per year.

Phosphorus removal. Phosphorus is much less easily removed by wetlands but the 1,000-acre South Florida Water Management District's STA 4 shows that sustainable P removal is quite possible. There has been much current research on P-removal in wetlands based on the need to provide a sustainable process similar to that now used for nitrate. Capital costs for a phosphorus removal wetland would be about 5 times that of the nitrate removal wetland since P-removal has no gas phase.

It is recommended that nitrate and phosphorus removal wetlands be constructed as soon as possible for the agricultural drain water and the Stockton wastewater effluent. It should be noted that ammonia should never be added to wetlands. The toxicity results in mosquito problems and the anoxic nature of wetlands provides almost no treatment for ammonia. Ammonia must first be nitrified before being denitrified in wetlands.

LONG-TERM NEED FOR NUTRIENT REMOVAL IN THE SACRAMENTOSAN JOAQUIN VALLEY.

It may seem overkill to remove nutrients from the waters of the central valley. All the small and large cities as well as agriculture contribute their waste to the rivers that flow from the Delta. Nutrient removal using wetlands, however, is the long-term and sustainable solution to the Ship Canal DDO problem.

Reservoir water quality south of the Delta. More importantly, nutrient removal is needed for all the reservoirs in the State Project below the delta. Serious water quality problems in Los Vaqueros Reservoir~ San Luis Reserovir, and the reservoirs south of the valley (Perris, Castaic) and poses potential problems for Diamond Reservoir. Nutrient removal is needed to reduce algal growth in these reservoirs (taste and odor problems, blue-green algal toxicity to humans, increased DOC and disinfection byproducts).

Reduction of toxicity to humans and marine mammals, Poisoning of marine mammals (dolphins, whales) as well as other smaller creatures is due to various algal poisons (domoic acid, diatoms: saxitoxin, dinoflagellates) is a great threat to all who consume marine products, including humans. The algae that produce these toxins require larger amounts of nutrients than most marine species. Thus nutrients from inland increase their abundance and the potential of toxicity. It should be part of the CALFED plan to generally reduce eutrophication in the coastal ocean by reducing nutrient in the Delta region where possible. The new development of high quality nutrient removal wetlands that are fully compatible with wildlife habitat makes such actions possible.

APPENDIX E

Comments on the San Joaquin River DWSC DO TMDL

Dr. Alan Jassby
University of California, Davis

Comments specific to individual reports:

1. Synthesis…G. Lee & A. Jones-Lee

This report is not a synthesis. Much of it is repetition of the individual reports at an unnecessary level of detail and without any attempt at concise summary and integration.

Note that the tidal excursion is 2.8 mi at Channel Point (p.9), and the RWCF outfall is about 1 mi further upstream. This suggests that water quality measurements made to estimate river loading using net flow should be taken at 4 mi upstream from Channel Point, in order not to be influenced by DWSC or RWCF processes. Mossdale, which is about 14 mi from Channel Point, may allow too much processing (including primary consumption) during downstream transport (especially during dry years when the DO deficit is greatest: e.g., 6-d transit time at 250 cfs), while Channel Point will be influenced by both the DWSC and RWCF outfall. This calls into question load estimates using either Channel Point or Mossdale discrete water quality measurements. Reverse flow periods present an even greater challenge.

The fit between pigments and BOD must be interpreted cautiously (p. 27 and appendix E). First, the slope at Mossdale is quite different from the slope in the DWSC. The BOD_5 yield per unit pigment in the DWSC is about 50% higher than at Mossdale. Although this could be due to a faster degradation of pigment relative to bulk phytoplankton organic matter, it could also easily be due to other materials correlated with phytoplankton abundance (as many variables, including phytoplankton, are related to flow). The unexplained variability, approximately half of the total, could also be due to materials other than phytoplankton, rather than simply a result of variable pigment content. Perhaps most disconcerting is the 1999 evidence. Should this be dismissed as a mere error or anomaly, or is it telling us that phytoplankton is by no means the whole story? I believe more caution in interpretation is in order here.

There is other evidence pointing to river phytoplankton as an important source, however. A quick comparison can be made of the dry weight equivalent of algal pigment with VSS. Assuming a C:Chl (uncorrected for pheophytin) ratio of 30, and a C:DW ratio of 0.4, leads to the following monthly medians (1983-2001) for the phytoplankton component of VSS:

Month	1	2	3	4	5	6	7	8	9	10	11	12
Vernalis	0.15	0.13	0.19	0.27	0.19	0.35	0.32	0.27	0.29	0.28	0.18	0.17
Mossdale	0.19	0.16	0.3	0.39	0.32	0.65	0.48	0.37	0.44	0.27	0.16	0.17

Rough &Ready	0.14	0.15	0.29	0.26	0.23	0.21	0.21	0.22	0.22	0.36	0.24	0.23

One general trend is the increasing contribution of phytoplankton between Vernalis and Mossdale during the summer season (see section 3). Also, given the uncertainty, the fraction attributable to phytoplankton is consistent with the notion that phytoplankton is the main source of organic matter downstream. Moreover, given the higher lability of phytoplankton relative to bulk organic matter in most rivers, the relative contribution to BOD should be even higher than to VSS. The lower value in the DWSC may reflect decomposition, the fact that data come from the upper part of the water column (p. 67), or settling of phytoplankton after entering the DWSC (section 6). It should also be noted that uncertainty works in the other direction as well and all of these ratios could be overestimates. The point, though, is that the data are at least consistent with phytoplankton being a major component of the river BOD load.

One outstanding issue is the change in river BOD load between Mossdale and the DWSC. Here (p. 45), the similarity between Mossdale and Channel Point BOD is taken to mean that any losses are compensated for by the additions of the Stockton RWCF outfall. This in itself implies that the Mossdale load is an overestimate of river contributions. Dr. Lehman's 2001 data (section 5, Fig. III-11) show a consistent decrease in chlorophyll between Mossdale and Channel Point, as well as many other changes in organic matter markers. But more important is that the Channel Point concentrations are probably heavily influenced by mixing with DWSC waters and may have no bearing at all on the magnitude of river-borne loads. In fact, this is acknowledged in the next paragraph on the DO deficit at Channel Point. The possible change in river-borne load between Mossdale and the DWSC remains an important unknown. The river load may be significantly smaller than assumed here and it may be different in composition (algal vs. nonalgal).

The calculation of 'Oxygen Demand Exerted in DWSC' (Tables 3, 4 and 5) seem incorrect to me. It assumes that BOD inputs are accompanied by DO-saturated water. In fact, the net water movement into the DWSC may be supersaturated due to algal photosynthesis (often the case at Mossdale), or even undersaturated. In other words, a term is missing from the equation (it is recognized as 'oxygen deficit' for two of the sources in Fig. 15, but is not actually estimated). If the water is supersaturated, then this implies that the exerted oxygen demand is even greater. For the purposes of estimating aeration needs, this may not be relevant, but at least the name of the calculated quantity should be changed.

The Turner Cut calculated residual oxygen demand is also of questionable value (p. 48). It assumes a travel time that does not take into account the 2-3x retardation found by Dr. Litton for particles (section 6). But surprisingly, the *measured* residual demand is larger, on average. Attributing it to photosynthetic production contradicts the Systech model (section 9).

Despite these issues, the agreement between loads and sinks (Table 7) is heartening and indeed "remarkable."

The use of the Vollenweider-OECD approach (p. 81) does not make sense in a system that is light-limited and has an excess of phosphorus. Any correspondence with actual conditions is coincidental. Also, the 5-17 µg/L Chl a at Turner Cut does not necessarily represent production in the DWSC.

2. Interim DO Goal…M. Gowdy & C. Foe

Does 'daily minimum' refer to a particular water column depth, to the water column minimum wherever it may occur, or to some statistic of the water column distribution of DO values (e.g., mean, a certain percentile, etc.)?

The schedule for the interim performance goal is not clear. Has a date been set for assessing attainment of the interim performance goal?

3. Draft Strawman…C. Foe et al.

As in other sections, the comparison tests here (eg, surface-bottom differences on p. 6) make the erroneous assumption that the samples are independent. I do not believe that this affects the conclusions here, but it is something that people should be aware of when analyzing data: sometimes it does matter.

Given the tidal nature of this system, shouldn't the Streeter-Phelps model include a dispersive term? Perhaps this is part of the reason for the discrepancy between predicted and observed, at least for the overprediction of the inflection point's location. Nevertheless, the significance of flow as demonstrated by the Streeter-Phelps model seems generally sound. The same goes for the significance of dredging as demonstrated by the discontinuity in chlorophyll.

The arguments on significance of upstream sources do not seem to be as well founded. The Streeter-Phelps model (despite the intimation on p. 17) does not distinguish among the sources of BOD, especially RWCF versus upstream loads. The organic matter composition of these upstream loads is also an important issue. The regression relating BOD_{10} to pigment concentrations is certainly useful (p. 18), but it does not rule out organic matter from other sources that could easily be correlated with pigments (because these and many variables are correlated with flow). Recent work confirms that detritus dominates riverine and estuarine organic matter supply and supports the majority of ecosystem metabolism (community respiration) in the Delta[2]. In short, I cannot understand the contention that "the previous section demonstrated that phytoplankton was the primary oxygen requiring substance exported from the upper basin" (p. 19).

The inverse correlation between Mossdale chlorophyll and daily minimum DO at Rough and Ready Island is also presented as evidence for the importance of upstream chlorophyll loading. The Streeter-Phelps model implies that minimum DO at any point should be related to initial BOD entering the channel and so this could be construed as evidence that Mossdale chlorophyll eventually constitutes most of this initial BOD. However, south Delta chlorophyll is strongly (inversely) correlated with flow, and the

[2] W. Sobczak et al. *Proc. Natl. Acad. Sci. USA* (2002): in press.

model also implies a DO-flow relationship at any fixed point in the channel. In fact, on an interannual basis—which removes the obscuring effects of shared seasonal patterns—the correlation between summer Vernalis flow and minimum DO is stronger than the correlation between summer Vernalis chlorophyll and minimum DO (r = 0.73 versus r = -0.59 during 1983-2001). So the chlorophyll-DO relation could be simply a surrogate for a flow-DO relation, or both could be important. This is an issue of great importance, because it determines whether the proper control strategy should be flow-oriented, algal control-oriented (see below), or both. I believe a proper analysis of the historical data might resolve this.

Figure 9 is also interesting in this context. It shows a very weak decline in Channel Point BOD as a function of flow. In fact, if the 3 highest flows are removed (out of 25 total points), even the weak relationship disappears. Yet chlorophyll upstream at Mossdale is closely (inversely) related to flow. How then can the BOD entering the DWSC be due to upstream chlorophyll? Actually, I believe this is more the result of Channel Point being a poor place to use as the initial BOD level. As pointed out above, it must reflect DWSC processing to some extent because of tidal dispersion.

The use of Channel Point BOD as L_0 in the Streeter-Phelps model may therefore be a serious flaw in this analysis, and is certainly a source of error that needs some investigation.

Mud Slough is identified as the one major source of chlorophyll loading that may be subject to PO_4 control. I suggest that, although this may be true, PO_4 control of Mud Slough phytoplankton may not be warranted. Although a major source, it is still only 20-24% (2000-2001) of the "potential" Maze load. Consider the following summer averages for minimum monthly DO, Vernalis chlorophyll and Vernalis flow (there may be better variables, but I used what was made readily available to me):

	MinDO	C10chlu	Qvern
1983	7.12	10.7	13200
1984	4.50	42.5	2330
1985	4.89	26.0	2370
1986	5.63	32.0	3420
1987	5.29	41.7	1620
1988	4.79	43.2	1460
1989	5.30	52.6	1270
1990	5.58	49.4	973
1991	4.62	115.0	568
1992	3.30	175.0	522
1993	3.94	32.8	2090
1994	NA	NA	NA
1995	5.27	20.7	6180
1996	3.38	37.1	2140
1997	3.93	72.5	1900
1998	6.37	16.5	8000

1999	4.47	35.6	2030
2000	5.03	47.8	2130
2001	3.67	89.8	1370

In 1999, for example, the chlorophyll was about 25% less than in 2000, while the flows were within 5%. Yet the 1999 DO minimum was even *less* than in 2000. This discrepancy may reflect the overwhelming significance of other factors and other oxygen-consuming materials. Cutting the chlorophyll by 25% as a remedial measure may not be supported by the historical data, an issue that should be delved into more thoroughly before embarking on a PO_4 control program.

In general, the work in this section supports the importance of flow in regulating DWSC DO. The case for a significant role for upstream chlorophyll, on the other hand, is not strongly substantiated by these arguments alone.

4. Data Summary…Jones & Stokes

No comments. Essentially provides supporting data for other investigators.

5. Oxygen Demand…P. Lehman

This project has contributed data that have been and will continue to be critical for understanding the DWSC hypoxia. There are several issues in the report, however, that deserve closer scrutiny.

A minor point is the assertion that algal growth in the photic zone of the DWSC is similar in magnitude to the daily load from upstream near Channel Point. The implication is that photic zone growth in the DWSC can be treated the same way as allochthonous loads in comparing sources of organic matter for decomposition. But note that any net photic zone growth is accompanied by photosynthetic production of oxygen. Some of this oxygen—perhaps most of it—will be used to support community respiration processes, offsetting algal biomass production within the DWSC. A more appropriate quantity for comparison may be *water column* net primary production (NPP), depending on vertical mixing and other factors. In general, the balance between water column photosynthesis and respiration in a completely mixed water column is determined by the ratio of photic zone to water column depth[3]. The value of this ratio is about 1:6, according to the photic zone depth reported by Dr. Lehman. In fact, this is a ratio at which water column NPP should be close to zero, if not negative (section 9). If the phytoplankton is not well mixed, though, perhaps because of their buoyancy, there can be positive NPP. It is, of course, legitimate to include both photosynthetic oxygen production and algal biomass respiration in the channel as separate components of a DO budget, but photic zone NPP by itself is not a very useful concept here. This point is appreciated in other sections (section 1 and 14).

A second point is the conclusion that upstream loading is unimportant, based partly on the lack of correlation between non-ammonia TKN load and either ammonium or NBOD in the DWSC. This analysis ignores the ammonia load from upstream of the RWCF. It is

[3] J. Cloern. *Continental Shelf Research* 7 (1987): 1367-1381.

only about 10% of the TKN load at Mossdale, but it may make a substantial addition to the load from the RWCF, especially if it increases during passage from Mossdale downstream. Is it possible to estimate this load and consider its importance? Mention is made of the low relative NBOD load from the upper San Joaquin watershed (10-20% of BOD), but the author's own data show the absolute amount and the ratio increasing downstream. At Mossdale, most of the BOD_{10} is NBOD. Perhaps this trend continues even further downstream. It is therefore important to define the relative contribution of ammonium from upstream. On a related point, why is the non-ammonia TKN load not plotted against NBOD (note that the Fig. III-10 caption is incorrect)? There is actually not a total lack of correlation between these two quantities, although the important mid-August peak is missing from the load time series.

A further issue involves deductions made from some of the correlations. A minor point is that the p-values are probably incorrect because the samples are not actually independent, although I do not think that is a significant problem. The main point is that possible spuriousness of the correlations is not being considered. For example, the assertion is made that algal production rate accounted for 30% of the total variance in DWSC BOD. In the Delta, one important control on algal NPP is suspended particulate matter and its effect on light availability. TSS tends to be associated with flow, and therefore NPP, flow and many other variables are related. It is not possible to make reliable conclusions about variance contributions without considering all the possible mechanisms and their covariance.

In summary, the correlation between RWCF ammonium load and NBOD in the DWSC is very telling. It is unlikely to be spurious because of the unique pattern of the RWCF discharge. It seems to me to be strong evidence that the RWCF discharge plays an important role in DWSC BOD. However, the NBOD is only 50-70% of the BOD. Moreover, as pointed out above, a substantial portion of the NBOD may be coming from upstream. Further, the remaining CBOD cannot be simply attributed to DWSC NPP, because of problems with the way DWSC NPP was assessed and because of possible spuriousness or interferences with the correlations. In other words, a large role for upstream loading cannot be dismissed based on the research presented in this report.

6. Deposition Rates…G. Litton

This research has provided two important conclusions: first, the SOD is small compared to other sources of oxygen demand; second, particles that make it out of the DWSC have a much longer residence time (2-3x) than suggested by water residence times because of sedimentation and resuspension. The latter process has been incorporated into the current model (section 9). It would be useful to know how sensitive the model is to this process, in order to decide whether the further studies recommended here are warranted.

7. West-Side Sources…W. Stringfellow & N. Quinn

This research has produced valuable results in the context of the TMDL process by identifying large sources of BOD to the San Joaquin River, namely Mud and Salt sloughs (see Table 4, especially).

Given the combined contribution of over 40% of the Crows Landing BOD, it would be advisable to define more precisely the actual substances constituting Mud and Salt Slough BOD and their sources. Note that the algal component of Salt Slough particulate organic matter may be only a few percent, based on typical dry weight: pigment ratios (75) and the pigment and VSS data of Table 2, and so the exact nature of the Salt Slough BOD is unknown.

The contribution of algal-derived material to Mud Slough is higher, and it may be that these algae can be managed by limiting PO_4 availability. However, as described above in the comments on Section 3, there is reason to believe that this may have little effect on DWSC hypoxia.

8. Statistical Model…E. Van Nieuwenhuyse

Dr. Van Nieuwenhuyse investigated the extensive historical water quality databases available from the IEP and Stockton in order to understand the causal factors for DWSC hypoxia and to compare management alternatives. This contribution is important to examine because it offers a different view and somewhat different conclusion from the other studies. With that in mind, Dr. Van Nieuwenhuyse kindly provided me with the monthly time series he has assembled for data summary and analysis. The assembly of these data in a usable form is itself a challenging task, and one that will no doubt prove to be of continuing value. Given the time constraints of this review process, I was able to examine the data only cursorily.

Dr. Van Nieuwenhuyse investigated the comparative importance of various variables for explaining minimum DO variability in the context of a statistical model. The variables were chosen on the basis of physical considerations and included water temperature, Stockton RWCF ammonium loading, Vernalis chlorophyll and flow, export flow, and presence/absence of "Head of Old R." barriers. Data were binned monthly. Certain variables were log-transformed and cross-product terms were used as well, resulting in eight predictor variables. The model fit the data well, all parameters appeared to be significantly different from zero, and residuals satisfied certain basic criteria. The model was then used to estimate minimum DO under six alternatives for reducing the DWSC DO deficit. Perhaps the most important conclusion was that control of ammonium loading might improve conditions for migrating fall run Chinook salmon more than any other potential management action.

There are several issues that should be raised in connection with the model and its implications, varying in importance:

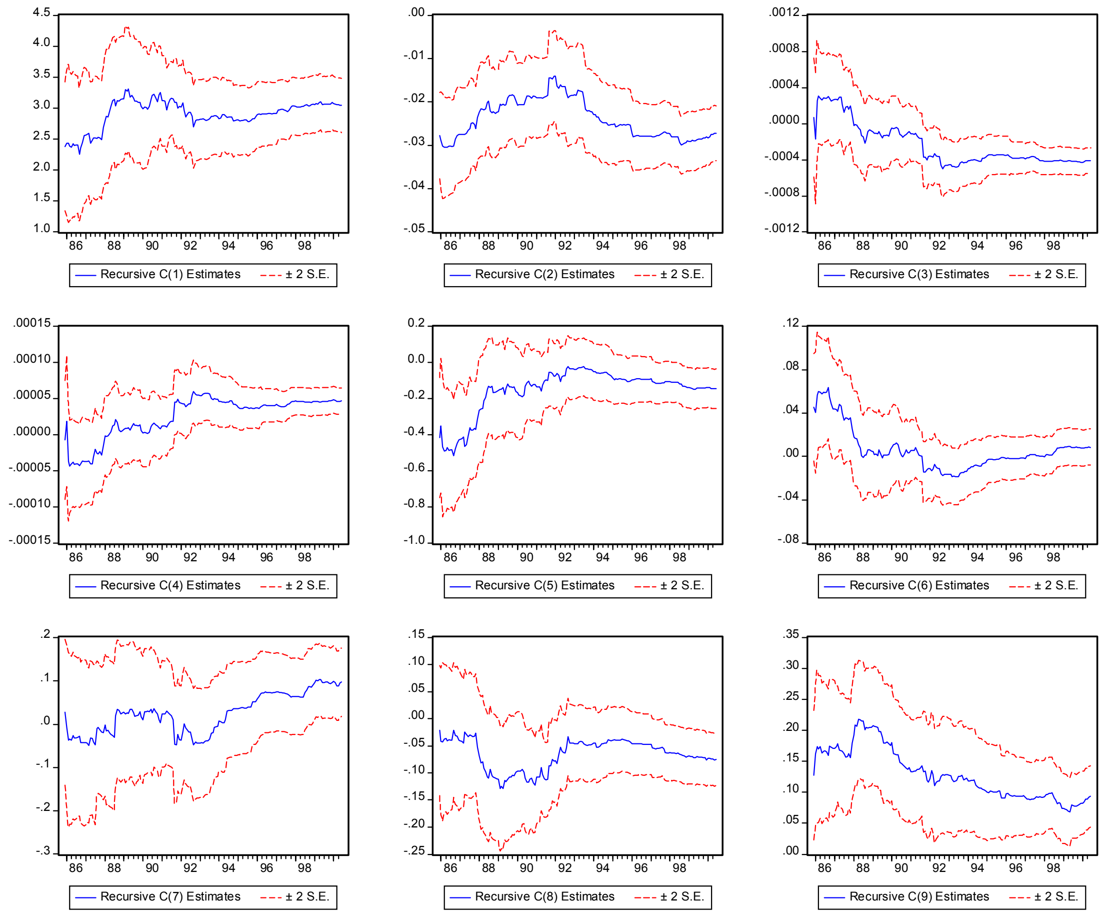
1. **Model specification**. The impact of changes in a given predictor variable is highly dependent on the way the model has been specified, i.e., the predictor variables chosen and the way in which they are related to the response variable. Therefore, it is important to be able to justify the specification on physical as well as statistical grounds. I made a minor change in the existing model by dropping the log(Vernalis chlorophyll) x log(Vernalis flow) term, which was the least significant. The resulting model is actually a

better prediction equation according to one widely used criterion (Mallows $C_p = 8.0$ vs 9.0 for the original model):

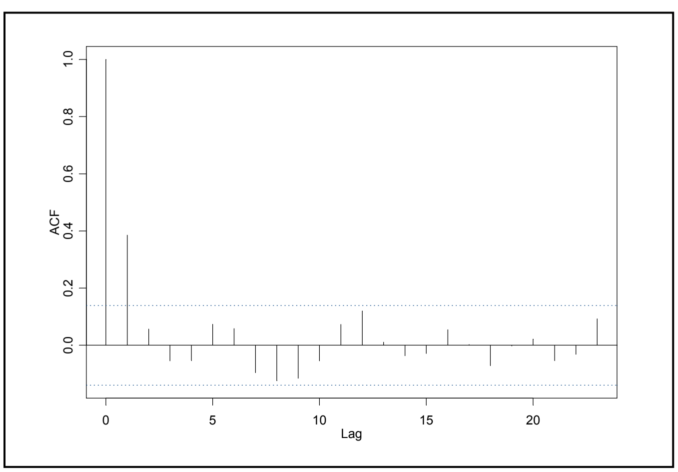
```
                           Value Std. Error  t value Pr(>|t|)
           (Intercept)  14.4336   1.1547    12.4994   0.0000
              MeanTemp  -0.1539   0.0169    -9.1094   0.0000
                 AmmLd  -0.0027   0.0003    -8.9364   0.0000
I(AmmLd * log(Qvern))    0.0003   0.0000     8.0259   0.0000
         log(C10chlu)  -0.4970   0.1004    -4.9493   0.0000
                  HORB   0.6282   0.2063     3.0451   0.0026
             log(Qexp)  -0.5579   0.1279    -4.3623   0.0000
           log(P8chlu)   0.5832   0.1152     5.0651   0.0000

Residual standard error: 0.8967 on 198 degrees of freedom
Multiple R-Squared: 0.7281
F-statistic: 75.74 on 7 and 198 degrees of freedom, the p-value is 0
```

Indeed, the fit was virtually unchanged with one less term (although this model also suffers from all the faults mentioned below). Some of the coefficients changed markedly, however. For example, the coefficient of log(DWSC chlorophyll) changed from 0.44 to 0.58. The effects of a change in DWSC chlorophyll are 32% higher just from a seemingly small and beneficial modification to the model. Unless one can actually justify the model structure, conclusions about management strategies must therefore be considered tenuous.

2. **Instability of coefficient estimates**. We can trace the evolution of each of the nine coefficient estimates as the series length increases. Most of the coefficients show significant variation as more data are added, including dramatic jumps where there are real breaks in structure. Interestingly, the ammonium load coefficient C(3) shows the most stability. The lack of stability in most of these coefficients indicates that we cannot have much confidence in the existing parameter estimates. In turn, this implies that we cannot have much confidence in the use of the existing model for examining scenarios.

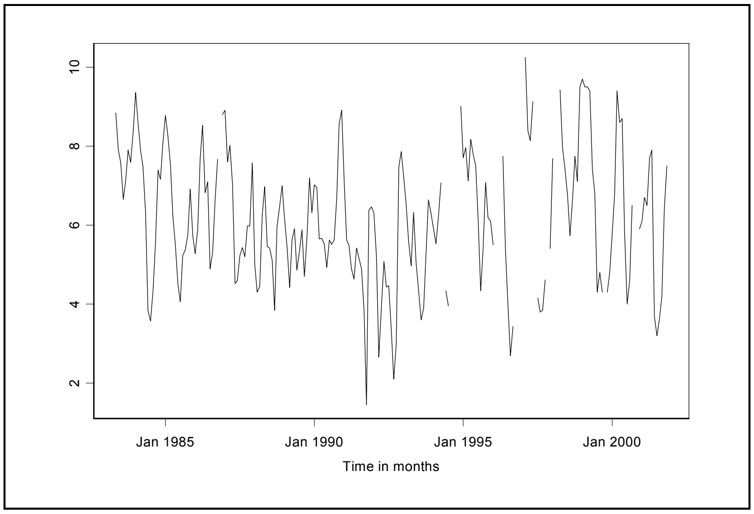


3. **Significance of predictors.** One of the assumptions of conventional linear regression models is that the residuals are independent. This assumption is not true for the current model, which shows strong serial correlation in the residuals:



As a result, the standard errors and confidence limits for the current model are also too optimistic. This is a common problem with applying conventional techniques to time series, and existing time series methods can easily address the problem.

4. **Model fit.** The good fit of the model to the data is somewhat of an artifact. These are time series in which the seasonal variability is stronger, often much stronger, than the interannual variability. For example, consider the minimum DO series:



Any series with a strong seasonal cycle of the right phase will be correlated with this series, even though their interannual variability may be totally uncorrelated. For example, a sine curve with the same frequency and phase has a significant correlation with the above series of 0.3 ($p<.001$). By the same token, a factor that is truly causative may not be detected at all if it lacks sufficient seasonality or if it is out of phase with minimum DO. Ammonium load, for example, is highly seasonal but is about 3 months out of phase and has an insignificant correlation with minimum DO of 0.07 ($p=0.3$). Conventional linear models can be completely dominated by these seasonality-dependent effects. One upshot of the current approach is that it gives us far too much confidence in the utility of the independent variables as true predictors and also may occlude the real causes. As mentioned by Dr. Van Nieuwenhuyse in his summary, these problems can be diagnosed and solved, at least in principle, by using time series analysis and modeling techniques. Other techniques are available that may be even more suitable. It is questionable whether the effects of the many, correlated causal variables can be disentangled through data analysis, but it is certainly worth the effort.

One of the major disadvantages of most statistical approaches such as multivariate linear regression is that they can identify reasons for variability, but not necessarily reasons for long-term mean conditions. For example, consider the situation in which the supply of material X is actually the biggest source of oxygen demand but has not varied much over the years. Then X will be completely ignored by a model selection procedure that proceeds on statistical grounds only. Yet it may be that a small change in X is all that is necessary to solve the problem. This is why model specification on physical grounds is so important, which may require novel methods to incorporate historically less variable but potentially important processes from the viewpoint of future management.

Although the existing model is inadequate for reliable scenario building, Dr. Van Nieuwenhuyse has made a valuable start on a rather ambitious undertaking by compiling the data set, summarizing the historical data, and perhaps isolating some of the critical variables on statistical grounds. It is important to build on these efforts, rather than abandoning them. The historical data set in the Delta is excellent and was compiled with enormous expense and effort. Every effort should be made to examine these data in the context of the DWSC DO issues. A more thorough exploratory analysis should precede any model identification and specification. There is much precedent in the Delta for discovering important and sometimes surprising features from the historical data that are later corroborated through experimental and field work. In the current context, there should be continuing data analysis directed toward identifying the variability characteristics of hypoxia and likely predictor variables of this variability.

Then one must confront the issue of using this understanding to build "what-if" forecasts. The multivariate linear regression model used here is inappropriate, as we have seen. Conventional ARIMAX time series analysis may address most of the problems, but it is not the only approach. Others include decomposing the monthly series into a small number of annual series through PCA time series analysis, structural equation modeling, state space modeling, and yet additional ones. This choice requires a significant analysis effort. But the effort used to assemble the historical data sets, as well as the cost of this entire TMDL program, suggests that additional data analysis and statistical modeling is a very cost-effective addition to the tools being used.

9. Lower SJR DO Model…C. Chen & W. Tsai

One of the most convincing demonstrations of a model's capability is an ex post forecast, ie, a forecast for existing data that were not used to calibrate the model. This was done for 2001 and the model appears to forecast temperature and DO well, capturing the seasonality and the magnitude (for DO by less than 1 mg/L). How good were the forecasts for the remaining state variables that are not shown?

The sensitivity to boundary conditions, especially river flow and load, is extremely interesting. If this sensitivity is real, ie, if the actual DWSC DO responds so sensitively to boundary conditions, it has important implications. For one thing, diversions upstream and downstream of Vernalis (apart from Old River diversions), which have a big impact on flow (section 13), offer a correspondingly large opportunity to affect hypoxia. The same can be said of Mud and Salt Slough inputs to the San Joaquin River. Because of the implications for planning and assigning responsibility, it will be important to test hypotheses about sensitivity through other models and data analysis.

A similar comment should be made with respect to the management scenarios. The implications of these scenario runs are very clear, and that is certainly a benefit, but the reliability is less clear. The ex post forecast for 2001 is partial evidence for reliability, but there is no indication of forecast uncertainty propagated from coefficient uncertainty and mis-specification. It would be useful, for example, to know how these forecasts responded to detrital decomposition rates, which involve two of the more sensitive coefficients. As in the case of boundary conditions, complementary approaches need to be encouraged as well.

10. Tidal Exchange…R. Brown

No comments.

11. Upstream Model…P. Hutton

No comments.

12. DSM2 Studies…P. Hutton & P. Nader

No comments.

13. Diversion Data…N. Quinn & A. Tulloch

No comments, except to note that this report makes clear the need to include agricultural diversions both up- and downstream of Vernalis into any simulation model. One possible issue: It is my understanding that releases from the New Melones to manage water quality are sometimes diverted in excess of water rights before the water can reach the San Joaquin River. From an analysis of historical data, it also appears to me that the operation of the New Melones has important effects on downstream algal biomass load. Can these excess diversions affect the water balance significantly, or are they so small that the “official” reported releases from the reservoir could be used for analysis and management purposes?

14. Aeration…R. Brown

No comments.

15. DO Depletion Modeling…Hydroqual

There are four major management areas recognized in the conceptual model—nonpoint sources, point sources, HOR tidal gate, and DWSC aeration. There are other processes affecting both flow and organic matter concentrations in the river. In particular, discharges from the east side reservoirs should be having a major effect on flow and concentration (section 3). In addition, diversions both above and below Vernalis probably exert an effect (section 13). Will the model not include these as possible management areas?

The water quality model diagram does not include zooplankton grazing, although this is mentioned briefly in the text. Perhaps more important, neither the diagram nor text refer to filter-feeding by macrobenthic bivalves. These are known to be important, if not overwhelming, sinks for phytoplankton in parts of the Delta. Moreover, major primary consumers such as *Corbicula* are known to be highly variable from year to year at existing monitoring sites n the Delta. Is this source of variability simply being ignored? The average depth of the San Joaquin River between Channel Point and Mossdale is about 3 m. This is a depth at which macrobenthic consumers could begin to play a major role. It would not be difficult to simulate macrobenthic grazing effects through a forcing function, using the existing understanding of their feeding behavior in the Delta, and it seems to me to be potentially important to investigate the influences of primary consumers.

The RCA model includes 25 state variables. Seven of these appear to be completely extraneous to the problem. These are the five phosphorus variables and the two silica variables. As the participants recognize, phytoplankton limitation by either P or Si is extremely rare in the south Delta, perhaps occurring only once during the massive blooms of the 1976-77 ENSO event. Phytoplankton limitation by nitrogen is equally unlikely, but N is important to track because of the oxygen demand exerted by ammonium. Would not the remaining parameter estimates be rendered much more reliable by eliminating 7/25, ie, more than ¼ of the state variables? (Yes.) It is true that P may be of some interest because of the potential for limiting algal growth in the San Luis Drain (section 7), but this load appears to be an input to the HydroQual model and is not explicitly modeled.

Mention is made in Task 4 (SJR WQ modeling) about evaluating the linkage between nutrients from upstream sources and river algae growth. Actually, the phytoplankton may very well be growing at nutrient-saturated rates with its growth rates regulated by light and temperature. Far more important, I believe, at least for understanding the development of phytoplankton in the river, is the size of the "inoculum" coming in from Mud and Salt Sloughs. The phytoplankton apparently has a reproduction time of about 2 d in the SJR (section 3). The travel time from Mossdale to Channel Point at 500 cfs is only about 3 d. This suggests that more variability will arise from inoculum size than from growth rates. Will there be any attempt to include this variable input of phytoplankton from tributaries?

Questions for peer review panel:

1. Overall understanding

a) Controlling factors

Controlling factors can be divided into two categories: those that can practically be managed, and those that cannot. Of the former, RWCF loads, upstream river loads, and river flow are hypothesized to be key factors:

- A significant role for RWCF loads is evidenced by the mass balance (section 1) and by the correlation between NBOD and RWCF ammonium loading (section 5). The estimated relative contributions of the RWCF loads are subject to uncertainty originating mostly from estimates of river loads.
- A significant role for river loads is also suggested by the mass balance, as well as correlations between the seasonal variability of hypoxia and algal pigment concentrations at Mossdale (section 3). The specific role of upstream phytoplankton is further evidenced by the ratios of algal pigment to VSS, and by correlations between BOD and pigments (sections 1 and elsewhere). The estimated river loads are subject to uncertainty arising from alterations of both the total load and the relative roles of its components during transition from Mossdale into the DWSC. Moreover,

some of the correlation evidence could be at least partially spurious, in that the tests did not consider other correlated causal pathways.
- A significant role for river flow is evidenced by both the Streeter-Phelps (section 3) and Systech (section 9) model.

See (c) for additional studies recommended. Other important controlling factors, which cannot be managed practically or independently, include DWSC depth and temperature.

b) Controlling measures

Practical controlling measures considered by the study include decreased RWCF loading, decreased river loading, increased flow and aeration. The evidence suggests that any one of these controlling measures could have a significant impact. With existing data (and with variable uncertainty, depending on the controlling measure), one can estimate in principle the quantitative impact of decreased RWCF loading, flow regulation and aeration. The optimal way to combine these measures is not clear, however. It is possible that a subset of these measures will be sufficient. The resolution of this issue is contingent on both the development of a model and a way of determining true costs and benefits that everyone accepts as reliable. Certain management measures are inherently simpler and more reliable than others (although possibly more costly). For example, both enhanced DWSC aeration and enhanced RCWF treatment can be budgeted, implemented and managed in a relatively straightforward way. Flow management, on the other hand, has to satisfy criteria beyond hypoxia amelioration. Furthermore, the success of nonpoint source control is by no means assured at this point, as no single source (even Mud Slough) can clearly be shown to dominate river loads.

c) Information gaps

- There is still much uncertainty on the fate of river loads downstream of Mossdale but upstream of the DWSC. It would still be helpful, as suggested in the last review panel, to establish stations between Mossdale and Channel Point that evaluated changes in both the total BOD load and the relative role of different constituents (algal-derived materials, ammonium, other refractory and labile detrital organic matter). This would also help to address Dr. Lehman's contention that river loads are much more refractory than expected.
- A better estimate of river loads into the DWSC is necessary. At the very least, a station several miles upstream of the RWCF outfall would be more appropriate than Channel Point or Mossdale when using discrete measurements. For chlorophyll, continuous flow and fluorescence monitoring should enable load estimates at any point.
- A related issue is the role of primary consumers. It is still important, as noted in the last review panel, to find out what role primary consumers are playing in the DWSC as well as between Mossdale and Channel Point. Because primary producers are so variable (especially *Corbicula fluminea,* a major macrobenthic filter-feeder in the Delta), this information is essential to calibrating a reliable simulation model, as well as to understanding BOD changes downstream of Mossdale.
- There is a very large body of historical evidence (DWR, DFG, and USBR datasets) that can be brought to bear on some of the questions here and that remains unexploited. Historical data analysis and time series or other statistical models offer a

cost-effective addition to this project that could produce results in a timely manner with respect to the TMDL timelines. Moreover, they offer a long-term, data based perspective to the results generated by other types of analyses and models. Agreement between such different approaches gives us a much higher degree of confidence in the conclusions. Disagreement subjects all approaches to a more rigorous examination.

2. Specific issues

a) Models

The Van Nieuwenhuyse model requires extensive modification before it can be used to either understand or forecast DO variability.

b) Specific load reduction benefits

The evidence identifies Mud and Salt sloughs as the primary subwatersheds for examining possible load reduction. However, the ultimate worth of any such reductions needs to be considered more thoroughly. There might be gains in water quality, but it is not clear at this point that they would be significant with respect to the ultimate goal.

c) DO concentration goal

It seems clear that the interim TMDL Phase I minimum DO concentrations can be achieved by at least a combination of measures, if not by a single one.

d) Flow effects

The relationship between flow and DO conditions has been described in general terms, but it is not clear how reliable the specific estimates are. Further statistical analysis of historical data would enable a concise description of relationships based on many years of data, and these relationships might have forecasting value. The Systech model requires more rigorous testing of reliability using ex post forecasts and should incorporate propagation of uncertainty. The Streeter-Phelps model suffers from several problems, including the use of Channel Point measurements to estimate initial BOD loads.

e) Aeration

No comment.

f) Geometry

No comment.

Addendum added after Review Panel meeting

The above comments were made before the review panel meeting. As a corresponding member of the panel, I did not attend the meeting in person but have since had an opportunity to review notes of the meeting and of other panel members. None of the additional information, I believe, contradicts my comments. I do, however, feel that it would be worthwhile to emphasize a few of the points that bear on future research and mitigation strategies:

1. The role of river loading from upstream in controlling DWSC DO levels is uncertain. The research to date has made a strong case for the role of channel dredging, RWCF wastewater discharge and river flow. But some of the best guesses that have been brought to bear on the importance of river loading are contradictory. Resolving the relative importance of river loading should be a research priority, whether it involves additional field measurement or analysis of existing data.

2. Given the uncertainty regarding river loading, the most important known load is from RWCF wastewater. Even if river loading proves to be relatively important, wastewater contributions will remain significant. Improving wastewater effluent quality is therefore at this point the most likely way to reduce TMDLs to the system, and at any time an effective way to reduce TMDLs to the system.

3. The TMDL studies are being funded at least partly in the name of ecosystem restoration. To most people that would imply reducing the anthropogenic inputs that have developed over the past century. Creating additional facilities to control and route flow in artificial ways through the system, when there are alternatives in the form of load reduction, seems to me a step away from what is understood publicly to be restoration. Moreover, as mentioned above, flow management already "has to satisfy criteria beyond hypoxia amelioration". Additional facilities would therefore add to an already large and sometimes contradictory collection of criteria for managing flows. Finally, there is the extremely important point emphasized by Dr. Cloern that hidden dangers often accompany flow manipulation.

APPENDIX F

Review Comments On The CALFED San Joaquin River Deep Water Ship Channel Dissolved Oxygen TMDL

Dr. William Ritter
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Overall Understanding of Causes Sources and Factors for the Dissolved Oxygen Depletion:

Based upon a review of the various papers that were given to the peer review panel to review and the presentations on June 11 and 12 it appears the dissolved oxygen (DO) violations may be caused by the following:

1. Deepening of the shipping channel.
2. Ammonia discharges from the Stockton wastewater treatment plant.
3. Transport of oxygen consuming organic matter into the Deep Water Shipping Channel (DWSC).from the San Joaquin River.
4. Production of oxygen consuming organic matter within the DWSC.

I believe the Chen-Systech model clearly shows that if the DWSC had not been constructed and the San Joaquin River below Stockton had remained as an 8 to 10 ft deep channel, the DO depletion below 5 mg/L below the Port of Stockton would not exist, especially at flows above 500 to 1000 cfs.

The relative causes of DO depletion in the DWSC are not clearly understood. There is disagreement among several of the scientists as whether the algae from upstream transported into the DWSC or the ammonia discharged from the Stockton wastewater treatment plant or the ammonia production in the DWSC are major causes of DO depletion. Lee and Jones-Lee (2002) from their black-box model of oxygen demand sources and sinks concluded that the oxygen demanding material transported in the DWSC from the San Joaquin River above Mossdale was the greatest oxygen demand load. Lehman (2002) conclude that nitrogenous BOD (NBOD) caused most of the oxygen demand in the DWSC and comprised 50 % to 70 % of the load. NBOD was highly correlated with ammonia concentrations which were very high in the DWSC and ranged from 0.5 to 1.0 mg/L. Ammonia loads from the Stockton wastewater treatment plant directly coincided with both ammonia concentrations and NBOD in the center of the DWSC study reach and at adjacent stations. Foe et al (2002) in the Strawman analysis concluded that the oxygen demand load from the upper basin is the major source of oxygen demanding material in the summer. They compared the chlorophyll concentration at Mossdale and the daily minimum DO concentration at Rough and Ready Island in 2000 and 2001.

In a detailed study by Litton (2002) it is well documented that the oxygen demand from the bottom sediments in the DWSC is low compared to other sources. It is well documented based upon the studies by Lehman (2002) that the penetration of light in the DWSC limits the algae growth to the upper few feet in the water column.

Water quality measurements made at Mossdale have been used to estimate the river loads to the DWSC at Channel Point. The total distance between Mossdale and Channel Point is 14 miles. The tidal excursion is estimated to be 2.8 miles above Channel Point which suggests that water quality measurements could be made downstream from Mossdale for estimating the river loads without interference from the tidal excursion or the Stockton wastewater treatment plant. In the mass balance approach Lee and Jones-Lee (2002) tried to examine the changes in BOD_5 between Mossdale and Channel Point, but concluded there was no change in BOD_5 between Mossdale and Channel Point. They concluded the additional BOD_5 from the Stockton wastewater treatment plant compensated for any BOD exertion below Mossdale. They also indicated there may be some other inputs of oxygen demanding material from some of the sloughs between Mossdale and Channel Point. Lehman (2002) found algae species carbon varied between Mossdale and Channel Point. She also found chlorophyll a concentrations decreased from Mossdale to Channel Point each month from June to October. In the year 2000 Lehman found the organic matter load decreased by a factor of 2 between Mossdale and Channel Point.

Based upon what is known and what the uncertainties are in what is causing the oxygen depletion it is recommended

a. Further research be conducted on more accurately delineating the major sources of oxygen demanding material that are causing the oxygen depletion in the DWSC.
b. A more detailed analysis of historical data from the DWCS, San Joaquin River and Stockton wastewater treatment plant discharges.

Modeling:

The Chen and Taai (2002) Systech model has been a valuable tool in helping understand the causes of the low DO in the the DWSC. The model shows that the low DO is partly caused by dredging the river from 8-10 ft to 35-40 ft for the Stockton Deep Water Ship Channel, which increases the hydraulic residence time. The model also showed increasing the flow above 1000 cfs in the DWSC decreased the DO deficit. The verification of the model from the 2001data showed a reasonable fit between the predicted and measured DO and temperature.. At the present time the Chen and Tsai model is the best tool CALFED has available to evaluate the DO depletion in the DWSC and to evaluate various management alternatives. The model is capable of estimating the different sources and sinks of DO in Figure 15 of the Lee and Jones-Lee (2002) report that have an unknown beside them.

CALFED has contracted with HydroQual and Monismith et al to develop two-dimensional and three-dimensional models for the DWSC and the San Joaquin River system. It is important to go ahead with the development of the more sophisticated models. This should give us a better understanding of the dynamics of the system and be able to evaluate management alternatives more accurately. In order for the more complex models to be of any use, it is very important to collect more data. There is a need for continuous measurement of flow and DO and for regular measurements of phytoplankton, zoaplankton, nutrients and other oxygen demanding materials. Measurements need to be taken in the DWSC, at Mossdale, between Mossdale and Channel Point and upstream in the major tributaries. This data will be critical to calibrate and verify the new models as well as quantifying the factors causing the DO deficit in the DWSC more closely.

It is also important for the modelers and other scientists collecting the data to work closely together. The modelers need to tell the scientists what they data needs are as to parameters to measure and frequency of collection. This needs to be done before anymore data is collected.

Allocation of Oxygen Demand Load:

There is not enough information available to determine where load reduction feasibility studies should be conducted or how much benefit might result from specific reductions. There is disagreement among the scientists as to the major causes of DO depletion in the DWSC. There is a need to reduce the uncertainty in the causes and sources of DO depletion before load reduction studies are conducted. To reduce the uncertainty, the principal investigators need to collect more data and do a more thorough analysis and synthesis of historical data. The flow and water quality data of the Stockton wastewater treatment plant should be examined more closely to determine ammonia discharges and temperature relationships and how these relate to the DO in the DWSC. Continuous measurement of flow and DO and representative measurements of nutrients and other oxygen demanding substances should be collected within the DWSC, upstream of the DWSC between Channel Point and Mossdale and far upstream in some of the significant tributaries. With this new data the new 2-D and 3-D models under development can be calibrated and verified to accurately predict what effect the different oxygen demanding load reductions may have on the DO in the DWSC. More support to model different management options with the 1-D model to account for all sources and sinks to obtain an oxygen mass balances for the DWSC may provide CALFED with enough information to initiate some load reduction feasibility studies.

There is enough data available to determine which of the tributaries are the major sources of oxygen demanding material that is transported into the San Joaquin River channel. What is not known with certainty is what are the causes of the oxygen demanding material in the subwatersheds of the tributaries.

DO Concentration Goal:

I believe there is enough data available and it is well documented that if the flow is above 2000 cfs DO concentrations below 5.0 mg/L in the DWSC will not be commonly observed. The DSM2 modeling studies with Grant Line Canal, Middle River and Old Man River barriers in place from April to November and auxiliary pumping to maintain a flow of 2500 cfs below the head of Old River showed significant improvements in the DO levels in the DWSC, but occasionally the DO fell below the fall target of 6.0 mg/L The model clearly indicates that as flow increases, the DO levels in the DWSC will improve. During the calibration phase, the predicted and measured DO levels at Rough and Ready Island (RRI) were generally within 1 mg/L of one another. The model was not able to accurately predict the diurnal DO changes at RRI.

There is some uncertainty in the amount of dissolved oxygen needed to maintain DO levels above 5.0 mg/L. Foe suggests the range is 2000 to 10000 lb/day assuming a 100%

efficiency. This conclusion is supported by several data sources. With the development of more complex models it should be possible to predict the potential changes in DO fairly accurately.

Flow:

There are sufficient data and analysis to determine how increases in flow affect the DO deficit. Both analysis of historic data and the DSM2 modeling results indicate that as flow increases above 2000 cfs, the DO deficit violations below the 5.0 mg/L DO goal become less frequent. In the range of flow rates between 500 and 2000 cfs there is a great deal of uncertainty in how BOD loads, temperature and flow rate affect the DO levels in the DWSC. There is a need to collect more data in this flow range with continuous DO, temperature and flow rate at various points within the system and to obtain accurate measurements of BOD loads to determine the relationship between flow rate, DO and BOD loads to the DWSC with more certainty.

Flow is one variable that can be controlled to some extent in the DWSC. Since there is evidence that the DO deficit levels in the DWSC decrease with an increase in flow and a shorter hydraulic residence time, this is one management option that CALFED needs to explore in developing the TMDL for the DWSC.

Aeration:

It appears aeration is a technology that could be one of the management options used to improve the DO in the DWSC during certain times of the year. There are a lot of uncertainties in what type of aeration to use and how much improvement aeration will provide in the DO in the DWSC. From the Brown et al (2002) report there are a number of aeration technologies available that could be applied. Aeration has been used for many years to treat wastewater and improve DO levels in lakes and reservoirs. Before a large investment is made in aeration, it is important to develop information on various aeration technologies and schemes such as compressed air versus pure oxygen and whether continuous aeration is needed or periodic aeration would be as effective.

CALFED should go ahead with a pilot scale aeration demonstration. It is recommended an RFP be developed for the aeration demonstration and the proposals be evaluated by a peer group of scientists and engineers. There is also a need to develop detailed cost/benefit data for different aeration schemes. The new 3-D models being developed could be used to help decide where to place the aerators and what benefits different aerator placement schemes would provide.

DWSC Geometry:

It is fairly clear how the DWSC increases the hydraulic residence time and affects the DO conditions in the DWSC. There is some question how the geometry of the DWSC affects the settling and resuspension of sediments and oxygen demanding particulate matter. There also is a question to the thermal stratification that occurs in the DWSC and what effect this has on the DO levels at various depths.

Lee and Jones-Lee Synthesis Report:

Overall the authors did a good job of summarizing the reports and synthesizing the important conclusions from reports. The reports taken a s a whole give a good understanding of what is happening in a very complex system. CALFED should be congratulated on coordinating such a fine effort. The synthesis report probably could have been somewhat shorter because a lot of the detail in the report is a summary of the findings of the individual investigators. The authors have listed the following key issues that need to be resolved:

a. A better understanding of the oxygen demand development and changes in the oxygen demand that occur in the San Joaquin River during transport into the DWSC.
b. The influence the City of Stockton wastewater discharge pattern has on DO in the DWSC.
c. The cause of DO "crashes " in the DWSC where the DO may go to 2 mg/L in a number of locations for short periods of time.
d. The oxygen demand dynamics between Mossdale and Channel point.
e. DO depletion within the South and Central Delta.

The last issue was not discussed much during the peer review, but the other issues raised by the synthesis report probably are issues that most of the peer panel and most of the scientists agree on. One issue that the synthesis report did not mention as an issue is that there is disagreement among the scientists as to what are the major causes of DO depletion in the DWSC. The authors of the synthesis report used a relatively simple black-box model approach to arrive at their estimation of the causes, which does not agree with some of the other findings.

Phased TMDL Approach:

Because of the short timetable required to develop a TMDL for the DWSC DO violations, a phased approach to developing the TMDL is the right thing to do. Although there are a number of uncertainties, the data collected over the past three years provides a sound basis for developing a Phase I TMDL. Over the next few years more data can be collected and more complex models will be developed that will improve our understanding of the system. Also the phased approach will allow for pilot scale testing of some different management alternatives without implementing them on a large scale. Some management options like some agricultural BMPs may not prove to be effective in reducing the DO deficit. By a series of demonstration projects and better understanding of the dynamics of the system, the most cost effective management options can be adopted.