Peer Review Suggestions – June 20-21, 2000

A peer review of the fall 2000 CALFED monitoring and analysis was conducted on June 20-21, 2000. Listed briefly below are some of the recommendations made by the peer review panel at the meeting that I thought were particularly important.

Please note that this list is not complete because the process is still incomplete.

I need to receive revisions to the official transcript from the peer reviewers and to respond to questions from the stakeholder community on the final transcript. The final transcript should be available soon for review by the stakeholder community. Revisions to the transcript are due July 24.

Current Recommendations

Sediment

- Make a direct measurement of sediment oxygen demand sometime during the study – this year or next. Chambers were considered to be the best approach, but core incubations could be substituted. This was a high priority item by all panel members.

- Obtain information on the sediment composition in the channel, particularly the proportion of organic versus inorganic composition and floc versus consolidated material. All members thought this was very important.

Modeling

- Conduct an error evaluation of the model results by Monte Carlo simulation and sensitivity analysis.

- Be sure to include the vertical sedimentation rates in the model. We agreed information was being gathered this year to address this need, but future work should consider application of at least a two dimensional model.

- This model is a link-node or Eulerian model and therefore has error associated with numerical mixing and diffusion calculations. This error should be fully evaluated. This error could be partially eliminated by a Lagrangian model, such as the one being developed by DWR.

Algal studies

- It will be difficult to separate oxygen demand from algal respiration and bacterial oxidation of living and dead organic material in the dark bottles. Size fractionation at 3 um may be useful. However, it would probably be best to assess the direct contribution of oxygen demand from each source (e.g., samples directly from a farm field or a point source).

- The best way to separate algal and bacterial respiration from nitrification is by carbon 14 dark bottle incubations. Nitrification inhibitors have too many side effects. Note: I will include this in the study.

- Separation of oxygen demand from nitrification of imported ammonia and breakdown of organic material in channel could be done using a geochemical box model. The required information appears to be available.

- The error in the flux calculation needs to be evaluated. Monte Carlo simulations may be a useful approach.

- Examination of a few zooplankton samples and benthic samples will help to assess the importance of herbivory for future work.

Nutrient load

- Develop background levels of nutrients and "bleed out" rates of the nutrients that control algal growth – what would the nutrient concentrations be if no fertilizers were applied. If the background nutrient concentrations are too high to regulate algal growth over a reasonably long time, then management needs to focus on other controls (e.g., light and temperature).

- Need to determine groundwater nutrient concentrations in order to determine background nutrient concentrations.

- Nutrient contributions should be made directly from each source (e.g., a field) because it is difficult to sort out sources in the river channel, particularly if there is a large groundwater contribution.

- Need to develop effectiveness of potential BMPs on a landscape scale. The available field and plot scale models are not sufficient to evaluate the effectiveness of alternatives.

Study design and implementation

- Be sure to conduct inter-laboratory comparisons of standard and field samples.

- Have periodic discussion of results.

PEER REVIEW OF FALL 2000 CALFED SAN JOAQUIN RIVER DISSOLVED OXYGEN STUDY

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07/08/2000

I. The main source of oxygen demand in the DWSC during August, September, and at least during higher flow conditions-later months, appears to be materials present at Vernalis. If this is true, a fundamental issue is the nature of these materials, their temporal variability, and their ultimate sources. In principle, these materials can be divided into phytoplankton-derived organic matter, other organic matter, and NH_x. We can get some sense of their relative magnitude at Vernalis by using the USBR (1968-1974) and DWR D1485 (1975-present) water quality data sets. The calculations presented here are based on all years for which data are available during 1968-1998. Comparisons of two numbers may therefore be distorted if the data do not come from the same set of years. In general, though, I do not believe that the qualitative conclusions are affected.

First, note that the oxygen demand due to NH_x is small (at Vernalis) compared to that of total $NBOD_U$ plus the $CBOD_U$ from just particulate organic matter (POM). We can estimate $NBOD_U$ = 4.6 x TKN and particulate $CBOD_U$ = 2.5 x VSS. Then the summary statistics for the BOD_U fraction comprised by NH_x at Vernalis are as follows:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	0.00	0.04	0.05	0.05	0.06	0.08
fall	0.02	0.07	0.08	0.08	0.09	0.12

The ratio is higher in the fall but still has a median of only 8 % and a maximum of 12 % for the entire record. It would be even less if the $CBOD_U$ of dissolved organic matter (DOM) were included (see below). So the BOD exerted by NH_x is probably a minor part of the DWSC BOD exerted by the load at Vernalis. The data at Mossdale present a similar picture, although there are occasional high values:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	0.02	0.05	0.08	0.09	0.11	0.33
fall	0.03	0.06	0.09	0.10	0.11	0.30

Of course, this picture changes downstream in certain seasons because of the Stockton RWCF, but we are concerned at this point only with the importance of the NH_x that appears by Vernalis. In terms of understanding the sources for these oxygen-demanding materials, NH_x is probably even less important:

[1]Not being present at the June 20-21, 2000, peer review panel, I did not have the opportunity to discuss individual work plans with their authors. Also, given the time constraint of two days for familiarizing myself with a large number of documents and preparing these comments, I had to limit the number of issues I could address.

at least some of it arises from the breakdown of the organic matter component, as opposed to runoff of ammonium fertilizer.

Second, note that phytoplankton and phytoplankton-derived detritus are important components of POM measured as VSS at Vernalis. The dry weight equivalent of the phytoplankton can be roughly estimated as 70 times the concentration of chlorophyll a (Chl). The dry weight equivalent of phytoplankton-derived detritus can similarly be estimated as 70 times the concentration of pheophytin a (Phe), although the conversion factor should probably be higher than that of Chl because of the loss of magnesium ion during degradation of chlorophyll. The ratio of this material to POM can then be summarized as follows:

	Min.	1stQu.	Median	Mean	3rd Qu.	Max.
summer	0.15	0.23	0.30	0.40	0.49	0.95
fall	0.09	0.15	0.18	0.24	0.26	1.28

In other words, phytoplankton-derived organic matter is a median of 30 % of POM in summer and 18 % in fall. In some years, it can constitute essentially all POM. Implicit in these calculations is an assumption that the measured plant pigments are produced by phytoplankton and not aquatic or terrestrial vascular plants or, for that matter, macroalgae such as *Chara* spp. In fact, this is not strictly true, but it is an adequate assumption for our purposes here.

Third, note that despite the importance of phytoplankton-derived materials, usually most of the POM is not obviously derived from phytoplankton. It is possible that some of this material ultimately derived from phytoplankton and lost its plant pigment signature during degradation, but this is only speculation. The nature and origin of this material is largely uncertain.

Fourth, and finally, note that DOM is another source of BOD load not included in VSS. There are no long-term-data that enable us to make direct estimates of the DOM:POM ratio, but we can gain some insight by examining VSS, TKN - NH_x data. Total organic nitrogen (TON) can be estimated as TKN-NH_x. If we assume that VSS is 40 % carbon, then the particulate organic carbon (POC) to TON ratio can be summarized as:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	2.15	3.10	3.53	3.49	4.00	4.40
fall	1.22	1.77	2.13	2.12	2.48	3.11

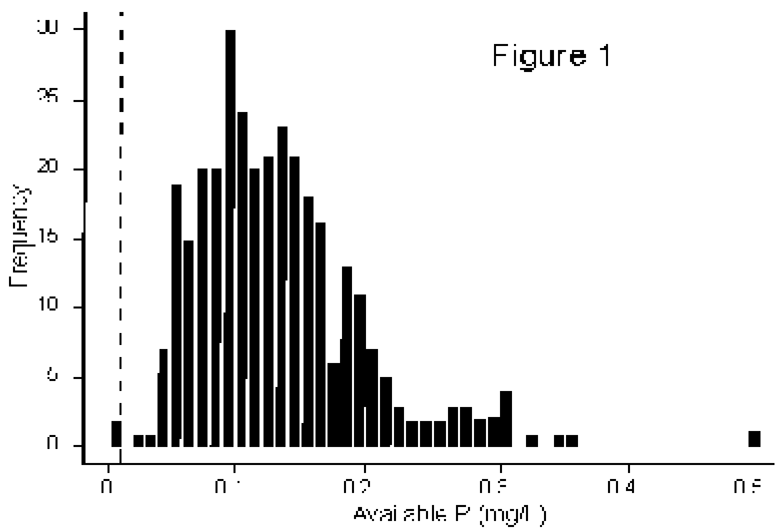
Now, characteristic ratios of river total organic carbon (TOC) to TON in our current CALFED project are approximately 12, as opposed to median values of 2.13 to 3.53 in the above table.[2] This implies that a large percentage of the TOC is associated with DOM. For example, if we combined the median POC:TON value of 3.53 for summers with the TOC:TON ratio of 12, the implication is that 71 % of the TOC is dissolved. This value is similar to the values we are measuring in our current project.

The refractoriness of this DOM is, of course, a large factor in determining its BOD contribution to the DWSC. For residence times of one to several weeks, the availability of river DOM is typically a bout 25 %.

[2]A D. Jassby and J.E. Cloern, in press, Organic matter sources and rehabilitation of the Sacramento-San Joaquin Delta (California, USA), Aquatic Conservation: Marine and Freshwater.

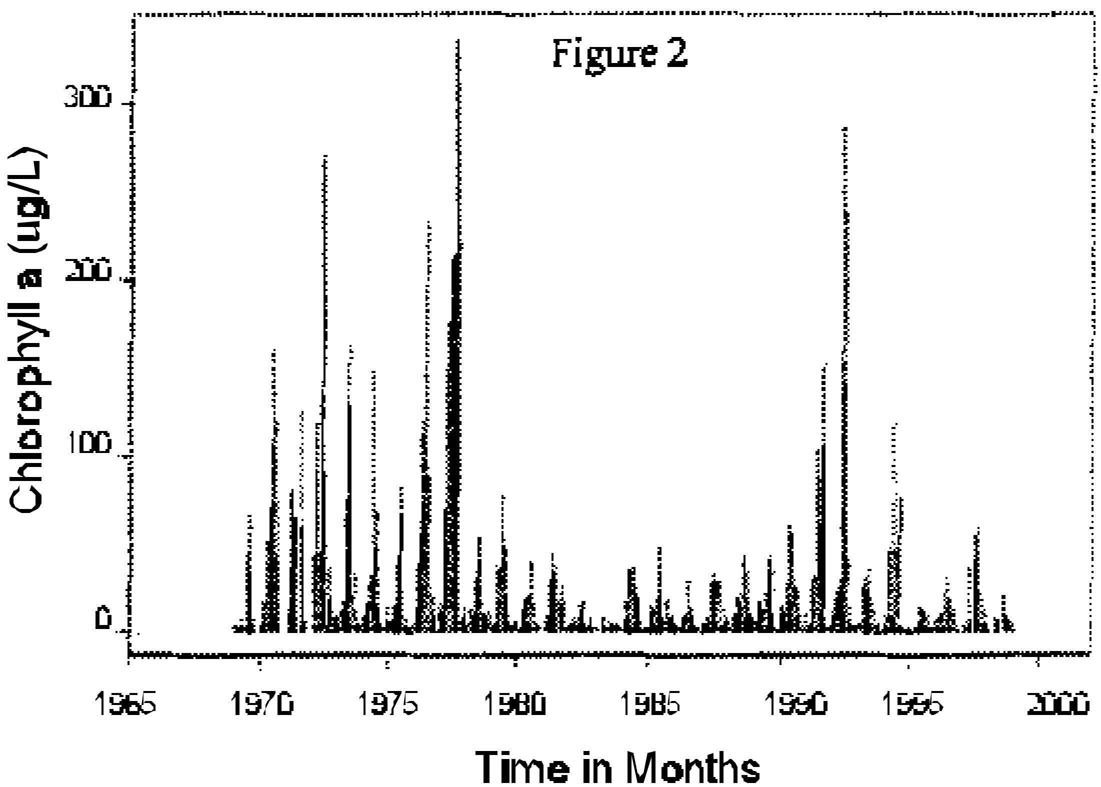
Therefore, despite the fact that most of the DOM may pass through the DWSC without contributing to the BOD, an amount similar to the POM could be metabolized.

What are the implications of these points for the research program? First, let us consider inorganic nutrients, especially N, P and Si, which commonly limit phytoplankton biomass. It would appear that, because phytoplankton-derived material is such an important component at Vernalis, potentially limiting nutrients are also important. I do not believe this is the case. In fact, it is difficult to understand the emphasis on nutrient loading in the overall program. In the 30 years of water quality data available for Vernalis, nutrient limitation of phytoplankton is rarely, if ever, present. For example, we can make a rough estimate of available P from the concentrations of SRP and TP. It is usually assumed that all of the SRP is readily available, as well as a certain fraction of the particulate P, estimated as TP-SRP. Although this fraction obviously depends on the nature of the particles, a value of 20 % is often used for estimation purposes. Some of this TP is locked up in phytoplankton, but the phytoplankton portion can be estimated from Chl by assuming a typical C:Chl ratio of 35 and a typical C:P ratio of 40. The frequency distribution of the resulting available P is shown in Figure 1.



The vertical dashed line represents a P level that typically limits phytoplankton growth rates (0.010 mg/L; actually, 0.001-0.010 is typical). The qualitative conclusion is obvious: available P is almost always far above levels that could be considered limiting. In fact, the very few times it occurred was at extraordinarily high phytoplankton concentrations such as during extreme dry ENSO events. Only at those times can one see TP and DIN (=NO_3 + NO_2 + NH_X) values that could be limiting biomass (incidentally, Si at Vernalis also could be limiting at those times). This is not a result of recent enrichment of the watershed but is characteristic of the entire data set since the beginning: note, for example, the large Chl levels present at Vernalis in the 1970s shown in the time series of Figure 2.

Moreover, nutrients are usually in such excess, by one or even two orders of magnitude, that it is not practical to think of bringing them down to limiting levels without a revolutionary change in the economy of the watershed. It may indeed be useful in the context of certain issues to characterize sources of phytoplankton nutrient loading from the San Joaquin watershed, but the DWSC DO deficit is not one of them. NH_x is a special case because of the NBOD exerted independent of utilization as a phytoplankton



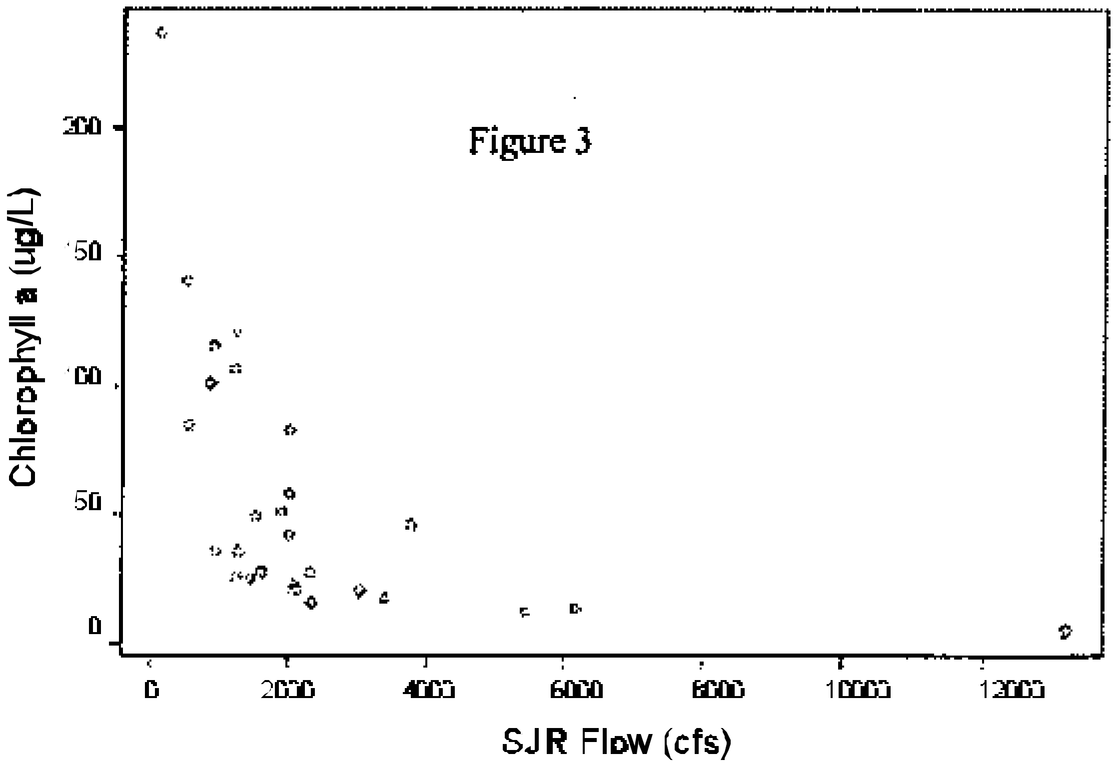
nutrient. Nonetheless, as pointed out above, this NBOD probably originates in part as organic matter and, in any case, is probably a minor component of the DWSC BOD load. As such, it does not deserve much emphasis compared to more pressing issues.

Much more important than the nutrient loads are the loads of organic matter, including phytoplankton, phytoplankton-derived detritus, other POM, and DOM. If the materials at Vernalis are the primary cause of DO depletion in the DWSC, it seems critical to know the amount of these materials, their temporal variability, and their BOD equivalences. The fall program will determine phytoplankton, DOM, and POM contributions at Vernalis. It is unlikely to dissect how BOD is apportioned among the various materials, however, as only eight BOD measurements are planned at this location. The investigators might consider a greater effort at determining the relative BOD of the constituents they measure, especially DOM. Bill Sobezak at the USGS in Menlo Park probably has much useful advice to offer in this regard.

A separate issue is the source of these materials. It is easy to identify the phytoplankton contribution-or at least its lower limit-through measurements of plant pigments. The planned UV_{254} measurements will probably also be of value, at least qualitatively, in indexing a soil source. Identification of the remaining sources, however, which form the major share, is more difficult, particularly if they have been resident in the river for some time. Biomarkers may be of use and were mentioned in the original CALFED proposal but do not appear to be a part of the fall 2000 plans. They are no panacea, however, and it may be impossible to relate biomarker concentration quantitatively to the BOD load that they represent. It is unlikely that stable isotope ratios for carbon alone can suffice because of variability and ratio overlap in source materials, as well as additional processing that takes place by the riverine food web. Liz Canuel at VIMS and Jim Cloern at the USGS in Menlo Park have experience with biomarkers and stable isotopes in the Delta, and it would be useful to contact them in this regard. Organic carbon and VSS measurements at the sites bracketing important reaches and tributaries of the SJR will provide useful information on the location of organic matter sources, although it is uncertain how they will identify the nature of the sources. In summary, a strategy for identifying sources has not been described convincingly.

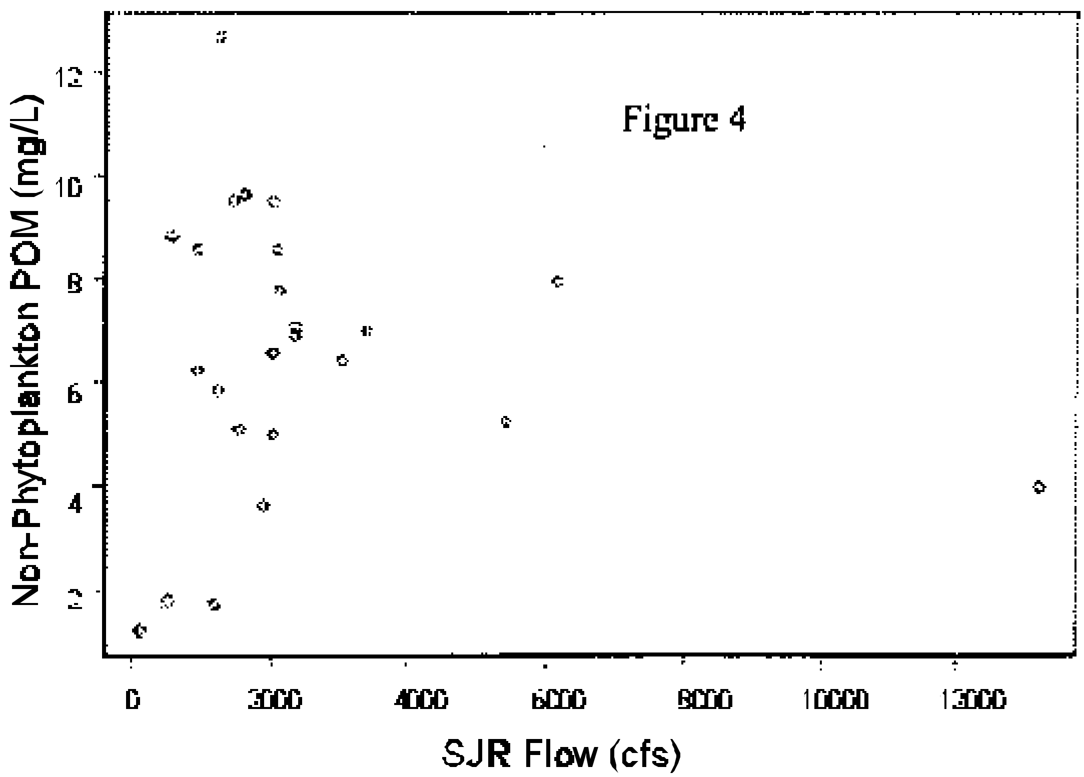
A reconsideration of the source identification part of the TMDL process is warranted. The issue of phytoplankton sources especially involves certain subtleties that do not seem to be appreciated. The tendency in this program is to consider nutrient source as a surrogate for phytoplankton source. Available nutrients are certainly a precondition of phytoplankton biomass, but they are only one condition. The effect of flow, for example, on Chl at Vernalis has been known since the 1970s. Figure 3 depicts the relationship between Chl and SJR flow for the summer season. The flow (residence time) effect is strong, although other sources of variability-perhaps light availability, grazing rate, and/or temperature-are also present. Available nutrients may set an upper limit on phytoplankton biomass but, in fact, phytoplankton levels are usually far below this limit and therefore determined in practice by other factors. One could argue, of course, that this does not absolve nutrient-emitters of their responsibility because phytoplankton would have been limited by nutrients had anthropogenic sources never appeared. Maximum phytoplankton levels would have been lower, no doubt. It is not clear, however, how often nutrient limitation would actually have taken place and if the phytoplankton BOD load would have been innocuous when phytoplankton were below nutrient-limited levels.

Grazing by zooplankton and especially by benthic macro-invertebrates is known to be important elsewhere in the Bay-Delta. In fact, the whole trophic structure of Suisun Bay and the extreme western Delta has been altered by *Potamocorbula* grazing since 1987. One of the greatest deficiency in Delta monitoring programs is the relative absence of long-term benthic macroinvertebrate data, aside from a few sites such as the Old River station D28A. *Corbicula* is often present at high concentrations in the Delta and it could well be a major drain on phytoplankton populations at places and times. Some of the zooplankton and possibly benthic filter-feeders are sensitive to certain pesticides and, just to make a point, one could entertain that pesticide inhibition of grazing is more important than nutrient stimulation of biomass in determining phytoplankton levels. In principle, then, one could argue that the emphasis should be on pesticide rather than nutrient sources.



A similar point can be made about light availability. The Delta is turbid and phytoplankton productivity is highly light-limited compared to other estuaries. If mineral suspensoids were not present in such high quantity, phytoplankton productivity and perhaps biomass could be much higher than at present. This leads to the unsettling conclusion that erosion from the SJR watershed could actually be suppressing phytoplankton biomass and the BOD load at the DWSC. Does this mean that we should encourage erosion in the SJR watershed? Of course not. The point, though, is that the responsibility for phytoplankton BOD cannot be laid at anyone's doorstep with confidence. Perhaps, then, the resources currently being expended to identify nutrient sources-at least within the context of the DWSC TMDL process-could be better used trying to understand what really affects phytoplankton biomass at Vernalis. Of course, phytoplankton-associated material at Vernalis is probably an important component of the food supply to the base of the food web in the southern Delta.[2] So even if we understood the mechanisms behind phytoplankton variability in detail, we would not be justified in managing it on the basis of the DWSC issue alone.

In principle, the sources for the remaining organic material can be ascribed to certain land uses and land users. But can this really be accomplished in practice? These sources are probably diffuse and it is unlikely that any "smoking guns" can be identified. For example, plotting the non-phytoplankton POM at Vernalis, estimated by VSS minus the phytoplankton dry weight equivalent of plant pigments, against SJR flow for the summer quarter, we see little effect of flow (Figure 4).



Point sources, in contrast, tend to decrease with flow. Ultimately it may be necessary to examine land use and associated organic matter yield coefficients on a watershed-wide bases, combined with a model that describes the degradation of this organic matter on its way to the DWSC. This, however, is a major undertaking that is impractical within the current time scale for TMDL determination. This should not be taken as an argument against controlling nonpoint sources of mineral suspensoids, organic matter, and nutrients (especially nitrate), but rather the practicality and utility of doing so in the context of the DWSC DO deficit alone.

II. The material at Vernalis undergoes further changes as it flows downstream. On average over the past 30 years, VSS has decreased 11 % in summer and 14 % in fall during the passage from Vernalis to Mossdale:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	0.00	0.61	0.75	0.89	1	4
fall	0.11	0.65	0.83	0.86	1	2

The year-to-year variability is huge, however. In some years, at least according to this data set, VSS has gone up by a factor of 4, whereas in others VSS has virtually disappeared by Mossdale. These extremes may represent errors in measurement or recording but it is clear from the distribution of data that interannual differences are large. This should make us cautious about any generalizations that come from the half-year program under review. Chl, in contrast to VSS, usually increases on the way to Mossdale, on average by 20-25 %:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	0.4	0.87	1.06	1.25	1.30	4.12
fall	0.1	0.95	1.10	1.21	1.42	4.00

Again, interannual variability is large. The same can be said of Phe:

	Min.	1st Qu.	Median	Mean	3rd Qu.	Max.
summer	0.28	0.84	1.07	1.25	1.43	4.37
fall	0.21	0.88	1.13	1.21	1.36	3.54

If the phytoplankton biomass or POM at Vernalis is the major oxygen-demanding load to the DWSC, then the changes illustrated here just on the way to Mossdale are highly significant and, in some years, represent huge increases or decreases of the load due to in-channel processes. Presumably, more such changes take place on the way to Channel Point, apart from inflows from French Camp Slough and the Stockton RWCF. Lehman and Ralston show in their 2000 draft technical report that Chl dropped by a factor of 2-3 between Mossdale and Channel Point August 26 and September 23, 1999. They correctly question "the use of algal biomass from Vernalis to estimate upstream load to the DWC".

These longitudinal changes imply that a careful budget must be drawn up for each of (1) phytoplankton-derived materials; (2) other POM; and (3) DOM in the reach between Vernalis and Channel Point. The two endpoints are being monitored in Peggy Lehman's program, including plant pigments, TOC, and DOC. These measurements will provide the net material changes in the reach. Accompanying BOD measurements will provide the net BOD changes in the reach. Apparently, the same set of measurements will also be made at Mossdale. Moreover, Lehman's continuous monitoring program will provide additional relevant measurements of DO and fluorescence at Mossdale and Channel Point. This program should provide the needed materials and BOD budget for this reach. It is not clear, however, that it can address the mechanisms behind the observe changes. This is unfortunate because, as we have seen, the differences from year to year are enormous. So one has to question the value of measuring in detail for a single year, unless the program is designed to uncover why the changes occur. In order to do this, additional information is necessary. What are the changes in oxygen-demanding substances immediately before and after Old River, French Camp Slough, and Stockton RWCF?

What grazing pressure is being exerted by the filter-feeding zooplankton and benthic macroinvertebrates all along this stretch of the river? Can water clarity and Chl be accurately interpolated between the intensely monitored stations so that phytoplankton productivity can be interpolated between Vernalis and Channel Point? Grazing, for example, could turn out to have a major impact on phytoplankton levels in the channel. If this is so, it would be an extremely important finding and have important ramifications for modeling, given the large year-to-year changes that typically occur in grazer populations. I understand that it is early in the project, that the investigators have their hands full, and that these issues may be addressed at a later time; I do not intend this as a criticism, but rather as a reminder of what will ultimately be useful.

III. Assuming that we are able to characterize fully the load of oxygen-demanding substances past Channel Point to the DWSC, there still remains the issue of external BOD load to the channel directly from local sources, as well as the net effect of autochthonous primary productivity within the DWSC itself. The important positive effect of autochthonous production may have been demonstrated by an incident in August 1999. At that time, the DO Model significantly overestimated DO levels in the DWSC. According to Lee and Lee-Jones' "issues document," the DWSC was highly turbid at this time due to an influx of *inorganic* turbidity. If the particulate materials were indeed inorganic, then this incident constituted a "natural" experiment in which local primary productivity was inhibited through further light limitation and DO levels "crashed" as a result. (Chen and Tsai state that the overprediction at this time was probably due to lack of inclusion of detrital organic matter in the model. If the overprediction truly coincided with the influx of inorganic materials, a depression of phytoplankton productivity is more likely. In any case, the revised model should still definitely include a detrital component, given that most of the organic matter cannot be identified with living phytoplankton.).

An accurate assessment of autochthonous productivity therefore appears to be essential. Unfortunately, the light /dark bottle oxygen technique is not well suited to isolating net phytoplankton productivity (NPP). The light bottle measures net *commmunity* productivity, as it includes all BOD sources, not just phytoplankton. Moreover, adding the dark and light bottle measurement does not give a good estimate of gross primary productivity (GPP) because the true phytoplankton respiration is underestimated in dark bottles.

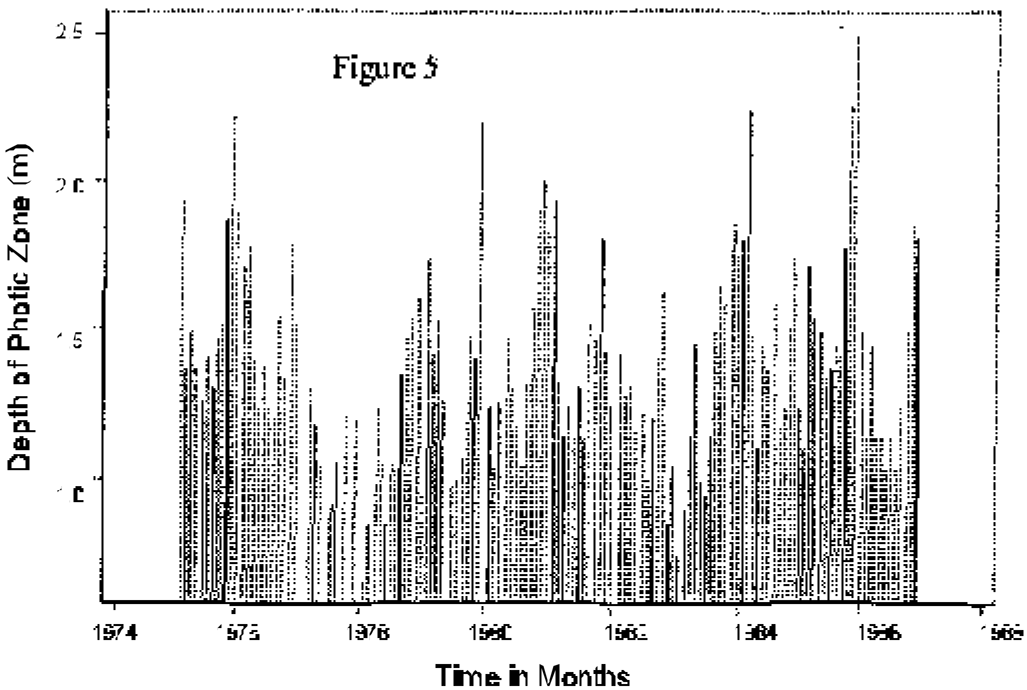
I would recommend at least comparing another approach, one that will not entail much additional work. Jody Edmunds and Brian Cole of the USGS in Menlo Park conducted over 50 experiments at various Delta locations in 1997, measuring GPP with short-term ^{14}C uptake. These experiments were then used to calibrate a simple theoretically based model of light-limited photosynthesis in a well-mixed water column. The model has been used in many water bodies since the early 1980s. It describes estuarine data extremely well and requires only incident light, light attenuation coefficient, and Chl:

$$GPP = \frac{4.6 \Psi I_0 B}{k}$$

where I_0 is the surface flux of photosynthetically active radiation (PAR) (E m^{-2} d^{-1}); *B* is phytoplankton biomass (mg Chl *a* m^{-3}); *k* is the attenuation coefficient (m^{-1}); and Ψ (mg C [mg Chl a]$^{-1}$ [E m^{-2}]$^{-1}$) is constant. The latter is the only calibration parameter; it represents the photosynthetic efficiency of a water column containing phytoplankton and other light-attenuating substances. The estimated value of Ψ was 0.60±0.02 mg C [mg Chl a]$^{-1}$ [E m^{-2}] (p=0.000, $R^2=0.942$)[2]. The model can also be modified for water columns that are not well mixed. Estimate of NPP from GPP is more uncertain, although a correction derived from Chl concentration and GPP is probably adequate.[2] The same model can be used to estimate GPP and NPP in the reach between Vernalis and Channel Point, if Chl and some measurement of light attenuation were to be made along the reach.

This simple approach could also be incorporated into the Stockton SJR DO Model. The DO model is not described in detail in the documents given to me. I did notice, however, that four parameters are needed just to describe the phytoplankton growth portion. Given the lack of nutrient limitation and the light-limited conditions, most of these parameters are unnecessary and the simpler approach is justified. The more parameters required in a model, the more the uncertainty that surrounds each parameter value. Unless these parameters are truly required for a more accurate specification of the underlying mechanisms, the simplest approach usually gives more accurate predictions.

It was not obvious from the documents I was given that the DO Model took into account variable light attenuation. Light attenuation varies dramatically in the vicinity of the DWSC. For example, consider the record of photic zone depth in Figure 5 (the depth which receives 1 % of surface PAR). These data are from station P8, Buckley Cove.



During 1974-1986, they ranged from 0.66 to 2.5, a factor of 4. In a light-limited environment, GPP is directly proportional to photic depth, which implies that photosynthesis could be undergoing dramatic variation without the consequences being recorded in the DO Model. If DWSC photosynthesis is an important source of DO, then this must be taken into account. Otherwise, the calibration procedure is simply incorporating this source of variability in some other process parameter, which means that the predictive ability would be impaired. Light attenuation is explicitly included in the simpler approach described above.

An additional problem with the phytoplankton portion of the model is the absence of a grazing loss term. The DWSC is deep enough that benthic macroinvertebrate grazing is probably not important, but at times zooplankton grazing might be. The use of a grazing term needs to be investigated more closely. Its importance (or lack thereof) could be deduced from zooplankton data and bioenergetic considerations. At the moment, any such effects are simply incorporated into other processes such as sinking, and their parameter values are accordingly distorted. Because of the ability of grazers to exert large losses that are not necessarily correlated with any of the included processes, the investigators should convince themselves that grazing can be excluded from consideration.

A related problem is the formulation for SOD. The SOD is considered to arise from the accumulation of organic matter over the years and is set to a constant. In fact, SOD appears to be much more dynamic and responsive to the flux of organic matter settling from the overlying water as well as to DO in the sediment vicinity. Generally, SOD increases nonlinearly (less than linear) with downward flux of organic matter and with DO. Have the investigators explored incorporating SOD as a state variable rather than simply a model input?

On University of California, Davis Letterhead

June 26, 2000

Dr. P. W. Lehman
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Sacramento, CA 95816-7017

Dear Peggy:

It was a pleasure to meet with you and the other study participants and peer reviewers to discuss the CALFED San Joaquin Dissolved Oxygen Study. As requested I have reviewed the reports and work plans you have provided, and have prepared the enclosed summaries of my reflections on the study. I hope they will be useful in your planning for the next phases of the study.

As you will note, I have enclosed some general comments on the project, and also the evaluation I previously provided on Dr. Litton's studies of sediment oxygen demands. I've corrected a few typos in the latter and added a brief conclusion.

I would still like to review Carl's model in greater depth, but will need a copy of its documentation. I am concerned that the model may need some significant improvements in order to serve as a useful tool in evaluation of future strategies for DO management.

Please let me know If I can be of further assistance.

Sincerely,

Gerald T. Orlob
Professor Emeritus
Civil and Environmental Engineering

SAN JOAQUIN RIVER TMDL STUDY PEER REVIEW

MEETING OF JUNE 20-21, 2000

G. T. Orlob, PhD, PE
Peer Review Panelist

Scope and Intent of Review

The following comments are based on presentations made by project participants in the San Joaquin River TMDL Study, supported by draft reports on study activities to date. They are intended to be constructive, suggestive of topics in need of clarification in draft reports or that might need to be addressed in future field and laboratory investigations. The reviewer accepts full responsibility for his comments, including errors or misinterpretations.

This portion of the review generally follows the sequence of discussions in the peer review meetings of June 20-21, 2000 supplemented by draft progress reports. A draft summary report "Sediment Oxygen Demand, Sediment Deposition Rates and Biochemical Oxygen Demand Kinetics" that was reviewed in preparation for the meetings is attached. Comments are as follows:

Scope and Intent of the Project

It appears that the "project" has two major goals and several minor goals. The first major goal is to characterize quantitatively the fundamental physical, chemical and biological properties of the hydrologic/hydrodynamic system in order to ascertain the system's response to specific compliance measures, e.g., reductions in TMDLs. The second major goal is to determine how to use this knowledge of system behavior in order to determine the net reduction in TMDLs that would have to be achieved by future adaptive management alternatives to assure compliance with dissolved oxygen standards for the San Joaquin River in the reach encompassing the Stockton Deep Water Ship Canal and turning basin. Additionally, a number of specific research goals would have to be reached in support of quantitative description of the system, e.g., kinetic coefficients required for modeling.

Quantifying System Behavior

The San Joaquin River in the vicinity of Stockton, specifically the reach extending from Mossdale to Prisoner's Point, including the Port of Stockton deep water turning basin, is influenced hydrodynamically by both tides and runoff from the basin upstream of Vernalis. It is an <u>unsteady</u> system, experiencing large fluctuations in velocity and water level over the tidal cycle and significant variations in net flow, both in the upstream and downstream directions. At times of low net inflow from the basin upstream the net flow passing Stockton may actually be landward. These fluctuations in hydrodynamic properties of the river strongly influence mixing processes that in turn determine the distribution of oxygen demanding substances.

Also, while the system experiences large variations in hydraulic properties along the principal axis of flow, there are significant variations in water quality both longitudinally and vertically that influence its hydrodynamic behavior. Although lateral variations in water properties are probably not of great significance in estimation of DO, both longitudinal and vertical distributions of hydrodynamic and water quality properties are prime considerations. In this reviewer's opinion the system should be characterized as vertically stratified, i.e., as <u>two-dimensional</u>. Preliminary evidence from the field supports this argument. Significant vertical variations are evident in dissolved oxygen, suspended sediment, dissolved and particulate organics, SOD, light intensity, salinity, and fluid density. More detailed spatial and temporal quantification is needed.

Boundary Conditions

Aside from the hydrodynamics of the system, transport and distribution of oxygen demanding substances are strongly influenced by the magnitudes and locations of sources and sinks of water entering and leaving the main channel of the river. Unfortunately, many of these cannot be directly monitored, such as those associated with irrigated agriculture along the river. The significance of these should be assessed in future sensitivity analysis using appropriate modeling of hydrologic fluxes and mass balances. A specific concern expressed by another peer reviewer is the role of seepage through levees along the river channels, both in water balance and nutrient loading of the system. DWR can provide useful information on locations of diversions and drainage returns and some estimates of amounts.

It should be appreciated that field observations to date have been under abnormal hydrologic conditions, i.e., high runoff from the San Joaquin basin upstream, several times greater than recorded in recent drought years at the Vernalis gage. Thus, water quality observations at these times are likely <u>not</u> representative of the most stressful conditions likely to be experienced.

Modeling the System

It is important to acknowledge a *priori* that no model is an exact replica of the system and that any model's principal role, once it has been calibrated against field observations, is one of discriminating between alternatives. The process of applying the model in the assessment of TMDL. management alternatives for the San Joaquin River requires the establishment of a credible data base from field observations. In this case careful description of vertical as well as longitudinal variations in the flow field and affected water quality parameters is required.

The modeling process should include <u>calibration</u> against the most credible data set, <u>sensitivity testing</u> of important model coefficients and assumptions, e.g., numerical analysis assumptions, and <u>validation</u> against an independent set of field data. Caution should be exercised to assure that the data sets correspond to conditions most likely to represent reasonable tests of feasible TMDL management options.

The model should be updated to provide the best possible characterization of vertical distributions of parameters and variables that may affect dissolved oxygen balances. Specific attention should be given to vertical variability in DO, SS, turbidity, light intensity, and phytoplankton, and other constituents that are not uniformly distributed in the water column. The trade-off between empirical representations of reaeration fluxes and sediment oxygen demand should be addressed by sensitivity testing or by direct measurement of benthic oxygen demand. In the absence of direct measurements of oxygen fluxes at the water surface and the bottom, calibration of the model may be largely a matter of adjusting uncertain coefficients. Field studies should include some direct measurements of SOD.

The model presently being used is a successor of the Link-Node models developed originally in the 1960s by WRE (Water Resources Engineers) in connection with San Francisco Bay-Delta Water Quality Management studies. It has since been adapted by Systech for study of the Stockton Ship Canal, incorporating some new routines intended primarily to overcome a basic deficiency in numerical solution of the advection-dispersion equation in the water quality module. Numerical mixing that was inherent in the original model is now surpressed by adding a negative correction term to the equation. This numerical adjustment, although convenient, is not consistent with the hydrodynamics of the system which requires that dispersive mechanisms be related to local turbulence and advective transport be cast in a Lagrangian framework, i.e., in a moving coordinate system. A more hydrodynamically correct model that has these attributes is DWRDSM-Qual, developed by H. Rajbhandari for DWR. It has been adapted to the Delta system, but is not yet fully calibrated.

Conclusion

The San Joaquin River System in the vicinity of the Stockton Ship Canal and turning basin is fundamentally unsteady, driven hydrodynamically by tidal action imposed at the downstream boundary and hydrologic fluxes introduced at the upstream boundary and along the main channel. Water quality characteristics are also unsteady, variable on a diurnal and tidal cycle basis, but also in accord with seasonal hydrology. There is a need, already being partially addressed in this project, to define more closely the physical, chemical and biological properties of the system as they vary both spatially and temporally. This effort should be intensified in the interest of providing a sound basis for modeling.

From the modeling viewpoint, the system is best characterized as two-dimensional and unsteady. Important gradients in hydrodynamics and water quality occur along the principal axis of flow and in the vertical. The model needs to be further calibrated, validated and tested for sensitivities of coefficients and other assumed properties of the system, particularly as concerns the vertical distributions of water column characteristics that are treated in the model. The dispersive properties of the system, as they are approximated numerically in the model, should be critically assessed.

REVIEW OF DRAFT SUMMARY REPORT

SEDIMENT OXYGEN DEMAND, SEDIMENT DEPOSITION RATES
BIOCHEMICAL OXYGEN DEMAND KINETICS

Prepared by

G. T. Orlob, Ph.D, PE
Peer Review Panelist

Scope and Intent of Review

The following comments are based on a draft report which is subject to future revision. They are intended to be constructive, suggestive of topics in need of clarification, either in the context of the report or in the course of future research and investigation. The reviewer takes full responsibility for his comments.

The review addresses issues as they are presented in the draft report, beginning with <u>Executive Summary</u> as page 1 and concluding with <u>References</u> as page 38.

Page 1

1. "Combined these measurements provide..." or "In combination these measurements provide...."
2. A reference should be cited for the so-called DWSC, if there is one
3. What is intended by "lateral variability of DO? Does this comment suggest that the DWSC is in reality a three-dimensional water body and that estimates of SOD require more detailed characterization of the hydrodynamics of this system?
4. Also, due to tidal action and water project operation the system is unsteady, i.e.,steady state assumptions may be inappropriate. "Assuming steady-state conditions where the SOD is equal to the rate <u>at which</u> oxygen utilizing matter is <u>settled</u> multiplied by its associated oxygen demand.'

Page 2

1. Sediment resuspension is a dynamic process which may impose an immediate oxygen demand.
2. What is meant by "over-trapping"?
3. The abiotic demand will be exerted immediately following resuspension whereas the demand associated with <u>microbial decomposition of organic matter</u> may require a much longer period.
4. Actually, these demands are exerted continuously from the initiation of resuspension.

Page 5

1. Model calibration is a critical process that requires quantitative evaluation (formulation) of mixing and sediment suspension processes.
2. What are the hydrodynamic conditions that must be favorable to estimate the SOD from profiles? Does this imply that only steady state conditions are appropriate for this estimation? If so, we may be missing the most important dynamic parts of the suspension and resuspension processes.

Page 6

1. The legend identifying monitoring locations is incomplete.

2. Stations upstream of Stockton in the San .Joaquin river are conspicuous by their absence. Data at such locations will be essential for establishing boundary conditions for modeling.

Page 7

1. Is the reason that no water samples were taken in November a consequence of the time required to do this while recording DO profiles? Why not employ a continuously recording DO probe?
2. What is the "upper limit of possible SOD values"?
3. Domination of near-sediment DO levels by tidal flows reinforces the notion that the system is highly dynamic and three-dimensional.

Page 8

1. Figure 2 could be made more general by contrasting DO profiles (perhaps actual observations) over a more explicit period of time than the beginning and end of the slack tide event. The slack period is too poorly defined to serve as basis for estimation of SOD.
2. By accident Figure 4 which is referenced in the text is not a <u>depiction of DO profiles</u> but rather a display of <u>pH observations</u>. The figure does, however, provide some interesting information, e.g., lower pH values near the bottom (possibly related to anoxia deeper in the sediments) and higher pH values near the surface in a few profiles (possibly related to photosynthesis).

Page 9

1. Figure 3. It would be useful also to have profiles of temperature, DO saturation and salinity for this period of observation.

Page 10

1. Figure 4. Again, this figure shows pH profiles rather than DO profiles as intended.

Page 11

1. This is an interesting figure that suggests a period of significant vertical mixing evidenced in the nearly uniform DO values top to bottom and tidally related oscillations (especially in the lower 15 feet or so) during the HL tidal period.

Page 12

1. Reference is made to high DO concentrations in the San Joaquin River, but no data are given. Conditions in the San Joaquin may well dominate those of the DWSC, especially during the later months of the year when flows from upstream sources are much increased, over conditions earlier in the year when DO conditions might be most critical. The recorded flow at Vernalis on November 6, 1999 was about 2473 cfs, significantly above the minimum of about 500 cfs that may be experienced during some critical drought periods. It is important to recognize also that diversions for agriculture along the reach between Vernalis and the DWSC may actually cause reversal of flow directions in the vicinity of the DWSC.
2. Where are the "relatively high DO concentrations measured in the San Joaquin River..."
3. "hydrodynamics and spatial variability of DO concentrations appeared to strongly mask SOD effects on water column profiles." It is acknowledged that "reliable estimates of SOD were not possible with the profile presented.."
4. The configuration of sediment traps should be described with sketches. How were these traps oriented in the water column and with respect to the flow?

Page 13

1. The procedure for estimation of SOD conforms to standard practice.

Page 14

1. Units for trapping rates should be stated explicitly in Table 2 and elsewhere grams/square meter/hour?
2. Depth of water column should be stated.

Page 15

1. 84,490? What is this number?

Page 16

1. Data Indicate that between Channel Pt and Light 45 ?????? is significant....

Page 17

1. "The data suggest that the microbial population was not acclimated to degrade the volatile organic fraction of the sediment.

Page 18

1. Agreed that the approach to estimate SOD is limited. The approach fails to account for both spatial and temporal variations in the hydrodynamic regime that in turn determine the fate of suspended, deposited and resuspended sediments.

Page 19

1. Although not stated, it is assumed that all reaction rate coefficients were referenced at a standard temperature, e.g., 20 degrees C. This should be indicated where these rate coefficients are cited.

Page 20

1. Were samples analyzed for DO actually filtered to remove SS before analysis? Were samples containing sediment agitated during incubation?

Page 21

1. What is meant by "shallow"? What are the dimensions of a typical core?

Page 24

1. Tidal flows in the San Joaquin do tend to increase on the average toward the ocean but this does not mean that velocities also increase proportionately, since the channel geometries also change. In general, maximum velocities of the order of 2-3 fps are reached in most channels. Occasionally local velocities up to 4 fps can be observed. These are sufficient to result in periodic scouring with intermediate low velocity periods when deposition can occur. In recent years sediment deposition in the Southern Delta has greatly modified channel geometries, velocities and deposition rates. It can be expected that water quality and ecological factors will also be modified.

Pages 25-29

1. Overall, laboratory procedures and interpretations conform to standard practice. Agreed that NBOD results are still questionable. This may be troublesome in modeling, particularly where oxidation of ammonia is a likely process impacting DO balance. Ammonia may not be a factor in well-oxidized effluents from waste water treatment facilities. However, it could result from decomposition of benthic organics. I [it] could account for depression of DO in the water column

near the sediment-water interface.

Pages 30-37

1. Laboratory results plotted and tabulated. Satisfactory

References

1. Add specific references to model documentation and application.