Comments on San Joaquin River Dissolved Oxygen TMDL Master Plan

Comments Prepared by
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Overall, I find that this proposed plan covers most of the bases that have to be covered to develop an appropriate TMDL to begin to address the low DO problem. There are a number of issues that need to be better defined to eliminate confusion on what will be accomplished through this effort. I find that the wording of the revised, proposed plan is loose and does not necessarily accurately reflect what will be done in the proposed studies. The wording needs to be tightened up where it precisely describes why the plan is being implemented, what the objectives of the plan are, and what will be done. It should also discuss the deficiencies in the plan compared to providing a comprehensive, overall program addressing water quality management issues of the lower San Joaquin River Deep Water Channel and Delta.

Page 1, in the first paragraph, states

> *"these improved water quality conditions are expected to benefit the San Joaquin River chinook salmon population by increasing the success of migrating adults to the Stanislaus, Tuolumne, and Merced river spawning areas. These water quality actions will be compatible with objectives to improve the Delta food-web (i.e., increased sources of primary production) from the San Joaquin River."*

This objective needs to be changed to: This action is being conducted to comply with the Central Valley Regional Water Quality Control Board's dissolved oxygen Basin Plan objectives for the San Joaquin River of 6 mg/L between September 1st and November 31st, and 5 mg/L between December 1st through August 31st. These objectives were adopted based on information that was purported to show that chinook salmon migration during September 1st through November 30th was inhibited by dissolved oxygen values less than 6 mg/L. The 5 mg/L objective is the CVRWQCB minimum dissolved oxygen allowed for all waters of the region. There are a number, including myself, who find that the technical base for the 6 mg/L DO being inhibitory to salmon migration is inadequate to justify this conclusion. The variety of other more plausible reasons for salmon migration inhibition, such as loss of chemical signal due to low flow conditions, other chemicals which block sensory perception of the homing water signal, etc. Changing the dissolved oxygen from 6 mg/L to 5 mg/L is not likely to be a significant deterrent to salmon migration. The homing tendency is far to strong for this change in DO to be a significant factor in their migration.

The work on salmon migration needs to be redone today, with a much more comprehensive data set developed and a more appropriate study plan to be certain that spending the funds to achieve a 6 mg/L DO during September 1st through November 30th will in fact change the homing tendencies of chinook salmon.

With respect to the statement that this action is compatible to improve the Delta food-web, this action is contrary to the Delta food-web since it will reduce the amount of nutrients that drive this food-web. This needs to be stated.

Basically, what can be said is that implementation of this TMDL will result in complying with somewhat arbitrarily established water quality objectives for the 6 mg/L and the 5 mg/L minimum DO values. What we should say in this plan is that this is what is being done. We are complying with CVRWQCB objectives and that is it. We cannot promise that it is going to do anything with respect to improving water quality-beneficial uses of the San Joaquin River and the Deep Water Channel.

The second item on the bottom of page 1, *"determination of realistic water quality objectives and other environmental targets that are to be achieved from the implementation of TMDL recommendations"*, is appropriate if it includes addressing impairments of beneficial uses of the San Joaquin River Deep Water Channel and the Delta where these beneficial uses are identified as removing any DO related inhibition of chinook salmon fall migration and any impairment of the designated beneficial uses of the San Joaquin River Deep Water Channel area due to dissolved oxygen excursions below 5 mg/L.

The third item in the listing of the major goals of the TMDL stakeholder process, which is the next to last paragraph on page 1, *"identification of sources and causes (i.e., ecological processes) contributing to the impaired water quality or other environmental impacts (i.e. habitat degradation or toxicity)"*, is not being addressed in our study which is focusing only on low DO and nutrients that contribute to algae which lead to oxygen demand, as well as oxygen demanding materials that through biological or chemical processes consume dissolved oxygen in the Deep Water Channel area. We are not addressing toxicity issues or a wide variety of other issues that can influence the ecological processes of the Deep Water Channel area. We are addressing some small part of these issues.

The same problem occurs with the last item on page 1, *"allocation of source load reductions (i.e. point-sources and non-point sources) and other management actions that will most effectively reduce the water quality impact and restore the currently impaired ecological processes."* We have not defined, and will not define, what the ecological processes would be if there were no degradation. We have not defined the extent of degradation and will not define this. We are only superficially examining a small part of this arena.

The next item on page 2, *"monitoring of individual source loads and resulting water quality conditions following the implementation of the TMDL recommendations to document and verify that improvements are actually achieved by the environmental management actions"*, will also not be achieved in these studies. There is no possibility of monitoring individual source loads from all of the sources of oxygen demanding materials. What can be done is to monitor dissolved oxygen concentrations within the Deep Water Channel, and this can be related to the goal of achieving CVRWQCB Basin Plan objectives.

Overall, the proposed study plan will not achieve many of the CVRWQCB "major goals for the TMDL stakeholder process". If we are going to list these goals for a successful TMDL in this plan, then we must accurately discuss the fact that the proposed plan will not achieve many of these goals. We also must clearly state what we will achieve through the implementation of this plan so there is no misunderstanding of what is expected to be achieved in the TMDL process.

Page 2, under Summary of Major TMDL Stakeholder Process Tasks, under Task 1, as the last line *"(i.e., e-mail list and server, web-page)"*, I have found on several occasions that the server that was selected for the SJR has severe limitations compared to normal server capacities, such as not being able to accept Adobe documents. Adobe is a standard format for transmission of documents used by many governmental agencies and others. A server that cannot accept transmission of Adobe documents through the internet is severely limited in it's ability to serve it's customers. There are readily available servers which, for $21/month, have this capacity.

Page 2, under Task 2 in Data Compilation, states that one of the stakeholder processes will be compiling of fisheries data. I have seen no evidence, or heard any discussions, of any attempt to compile fisheries data. Further, we are only going to address a small part of the water quality data that is available. Task 2 needs to be reworded to reflect what is actually going to be done so that we do not mislead others into believing that we are going to provide an integration of "all available" historical data on the San Joaquin River and South Delta region.

The last sentence on Page 2 and the first sentence on Page 3 states *"The water quality model should be directly linked to the database to allow stakeholders to test and independently evaluate potential changes in water quality conditions. Prepare a "rough-cut" approximation of current loads into the river based on best available water quality and streamflow information."* This statement is too broad. With respect to the Chen model, it is not a water quality model, it is a DO model. There are many aspects of water quality that are not being addressed. Also, with respect to current loads, this process addresses the current loads of materials that either directly or indirectly are believed to be impacting the dissolved oxygen concentrations in the Deep Water Channel. There are many "water quality" loads that are not being addressed in this TMDL plan.

Page 3, Task 3, Develop Models and Hypothesis, lists several suggested areas for special study. The wording on several of these special studies is loose, for example in (a) *"and other organics"*, we are not going to investigate other organics per se, we are going to attempt to characterize these in terms of BOD (i.e., organics that exert an oxygen demand).

The same applies to item (b) - the word organics is used far too broadly. We are not going to investigate many of the organics that may be important in affecting fish migration, such as chemical sensors disruptive agents, pesticides, other toxics, etc.

With respect to item (c), I would change *"seasonal fluctuations in sediment oxygen demand"* to fluctuations in sediment oxygen demand, not tie it to a season since this is far too long a period.

Basically, we need to look at this as a function of various events, such as ship traffic, at no greater frequency than monthly during the period May through November.

Item (g) in this list discusses *"fish abundance and fish migration pattern"*. I have seen no evidence that this is going to be addressed in these studies. It is a critical component of the studies that is now missing.

Page 3, Task 4, Select Analytical Tools, states *"calibrate (with possible modifications and enhancements) the available water quality model(s) to simulate the observed water quality processes and DO concentrations in south Delta channels."* Again, this is too broadly worded; there is no question about the need to modify and enhance the existing Chen DO model for this region. Also, in the same task is the mention of the water quality model and water quality conditions. These terms are used too loosely. We are talking only about DO issues, and we should be sure that is what we focus on, not the generic water quality. Water quality is properly related to beneficial uses. We are not discussing or addressing beneficial use issues, we are discussing achieving a DO water quality objective which may, in some yet undefined way, be related to true water quality issues of concern to the public. We should not mislead the public (stakeholders) and others into believing we are addressing water quality issues. We are addressing DO issues that may be related to water quality. It should be recognized that we could cause the public (stakeholders) in the San Joaquin River watershed, and the people of California to the extent that they help fund this, to spend large amounts of funds controlling oxygen demanding materials and have little or no impact on the overall beneficial uses of the San Joaquin River Deep Water Channel and the Delta. In fact, we may be impairing the overall fisheries resources of the Delta by these proposed actions.

It is extremely important that we start to educate regulatory agencies, consultants, and the public that the chemical concentration approach, which is the one being followed in these studies, is not necessarily valid to address true water quality issues. What we need is a study that focuses on chemical impacts to beneficial uses, not chemical concentrations.

Page 3, Task 5, Evaluate Basin Plan Objective, is an extremely important area that needs attention. I have seen no evidence, however, that this is being done. We need to expand the discussion here to more than just temperature. There are a wide variety of factors that can influence fish homing migration tendencies. The statement is made that the Technical Committee will consider conducting biological monitoring to collect data on fish abundance and passage - funds should be sought to do this work.

This is a topic that I worked on while teaching at the University of Wisconsin at Madison in the 1960s, where my graduate students and I specifically focused on chemical tracers that motivate fish to home to a particular location. Further, I was involved with Dr. Art Hasler, University of Wisconsin at Madison Department of Zoology, who is an international expert on fish homing, in studies on coho salmon migration in Lake Michigan where he was able to imprint fish with synthetic chemicals which then caused the fish to home to streams which contain these synthetic chemicals. This work established that chemicals are the major factor in homing. The studies in Task 5 should be part of what is considered in our Technical

Subcommittee activities, and not left to someone else who would likely be able to use the assistance of the expertise within our subcommittee to design, implement, and interpret the results of future studies on why, if it still occurs, that coho salmon do not migrate through the Deep Water Channel during the fall months.

In page 4, Task 6, and other places, mention is made that this plan will address south Delta Channel low DO issues. There are many south Delta channels that will not be considered in this process. We need to be much more specific about what is being considered in this project.

Also in Task 6, mention is made of *"BOD and ammonia"*. We need also to consider organic nitrogen as a source of oxygen demanding materials in all of our studies. This can occur directly through mineralization of organic nitrogen to ammonia which exerts an oxygen demand, or which converts to nitrate, then stimulates algae which exerts an oxygen demand. Both of these types of processes need to be considered.

Regarding the statement on page 4, Task 6, *"all future management decisions should be supported by real-time water quality monitoring"*, I do not agree that we need real-time monitoring to verify that the DO objective is met. Real-time monitoring often generates massive amounts of data which represent a severe compromise of what is needed and what can be readily obtained by appropriately conducted monitoring. I am concerned about the emphasis on attempting to continuously monitor chlorophyll through fluorescence measurements. As I have pointed out several times, I am highly familiar with the unreliability of fluorescence measurements as a measure of planktonic algal chlorophyll. I had a student do his thesis on this topic. A far more reliable approach than can be achieved through conventional chlorophyll monitoring, is grab sampling of chlorophyll as a function of depth. It may be cheaper in the long term, and will certainly yield more reliable data.

An aspect of the chlorophyll measurements which I feel has been inadequately addressed thus far is the potential for a just above bed transport of a fluff zone of dead algae/chlorophyll in the San Joaquin River. This may account for the disappearance of some of the nutrients and algae between Vernalis and Mossdale. The past studies have, in general, been superficial with respect to properly sampling the San Joaquin River in the Deep Water Channel with depth. When I first reviewed the past studies last May, I immediately found these studies had failed to consider the importance of sampling versus depth. This needs to be done. The value of this approach is being demonstrated by the follow up to my suggestions on vertical profiles of oxygen, etc., in the Deep Water Channel area. We need to do the same thing in the San Joaquin River to be sure that we understand how materials are being transported in the River. Any work which involves automatic or continuous monitoring must be accompanied by substantial, in depth monitoring at the site at periodic intervals to verify that the continuous monitoring system properly measures the concentration of constituents at the location and time of sampling.

Page 4, Task 7 - Prepare TMDL Report, should include a discussion of the uncertainties associated with the conclusions and recommendations set forth in this report.

Page 5 under Task 1- Facilitate Stakeholder Process for TMDL Investigations and Evaluations, the first paragraph mentions *"supplemental funding for active participation by environmental organizations and key regulatory and resource agency staff."* I strongly support this approach, but wish to emphasize that agricultural interests will also likely need funding. While municipal stakeholders have a potential source of funding through their tax-base and fees to cover their activities, agricultural interests, environmental groups, and regulatory agencies do not have this same funding mechanism. In order for the TMDL process to be a true stakeholder developed process, it is essential that adequate funding of all stakeholders be available.

Page 5, Task 1, paragraph 2, states that the objective *" is to create and implement a watershed-wide, adaptive management strategic plan to achieve dissolved oxygen objectives for the protection of beneficial uses in the lower San Joaquin River..."*. I would delete *"the protection of the beneficial uses in the lower San Joaquin River"*, this is too broad a statement. The focus of this effort is to achieve a dissolved oxygen objective. A markedly different, much more comprehensive study needs to be conducted to address the protection of beneficial uses of the lower San Joaquin River.

Page 5, third paragraph, defines the watershed as *"land that drains into the lower San Joaquin River and South Delta area"*. Again, the definition of South Delta is not restricted to the Deep Water Channel. We need to be certain we understand what the area of consideration is for our project. Also, as of yet I have not ruled out atmospheric sources as part of the study area.

Page 5 under Task 1 focuses on "scientific research". It has become clear that there is a lack of understanding of the role of engineering in this process. This is an engineering problem which is to based on scientifically developed information. There is considerable engineering methodology needed to properly develop a TMDL for the low DO problem within the Deep Water Channel of the San Joaquin River. Engineering approaches to issues of this type are often quite different than scientific approaches. Engineers, by training and objective, focus on problem solution using the best possible science. Most of what is being called scientific in these studies is basically engineering. It needs to be recognized as such.

Page 6, first paragraph states that *"the dissolved oxygen concentration at all other places in the San Joaquin River should be at least 5.0 mg/L."* This issue needs to be better defined since, if it is taken in a worst case based interpretation, it means that there shall be no exceedance of the 5 mg/L DO water quality objective at any location, including a millimeter above the sediment-water interface, at any time more than once every three years. Is this what is meant by this statement? If so, it needs to be clearly defined as such.

As part of this definition, the situation that exists early in the morning in backwater channel areas, where there is active photosynthesis during the day but significant diel dissolved oxygen changes from late afternoon to early morning, needs to be considered. Are we attempting to control, through this TMDL process, dissolved oxygen at 4 or 6 in the morning? I have prepared a separate discussion of these issues which I plan to circulate among the Technical Committee for review to begin to formulate a clear definition of these issues, so that the TMDL goal is explicitly defined. If a chemical concentration, worst case based approach is used as a TMDL goal, no DO exceedances below the 5 mg/L standard during the December 1st through August 31st, and 6 mg/L DO objective between September 1st and November 30th is the goal at any place in the water column and at any time during the day, then this is a significantly different goal than one of protecting the beneficial uses of the San Joaquin River Deep Water Channel, and the associated upstream and downstream waterbodies, from the impacts of dissolved oxygen depletion including salmon migration through the area.

One of the issues that needs to be considered in any TMDL development is the appropriateness of the TMDL goal, with particular reference to the application of the worst case based, no DO less than 5 mg/L at any time, at any location. The Deep Water Channel area is, to me, an area that could clearly be justified for a potential change in the beneficial use listing during certain times of the year. It is clear from my many years of experience that it will not be possible, even after spending many tens of millions of dollars for nutrient control, to control the DO depletion immediately adjacent to the bottom of the Deep Water Channel. There will always be sufficient algal growth in this system, even with extreme measures in controlling nutrient input, so that there will be DO depletion in the Deep Water Channel near the sediments each summer and fall. While this DO depletion may not be significantly adverse to the salmon migration, as well as fish production, within the Deep Water Channel or elsewhere in the San Joaquin River, it could be adverse to certain benthic invertebrates. This situation is much the same as in every eutrophic lake in the world where the sediments just below the surface are anoxic, contain hydrogen sulfide and ammonia, and are highly toxic to certain forms of aquatic life due to the low DO, sulfide, and/or ammonia.

Since the CVRWQCB faces an impossible situation of trying to meet a TMDL goal that would require there be no dissolved oxygen depletion below 5 mg/L at the sediment-water interface, it is part of the work plan for the TMDL goal to consider modifying that requirement so that an impossible goal situation is eliminated and reasonable goals can be established. Ultimately, the TMDL for nutrient control will have to be a compromise between various technologies for control of nutrients and other oxygen demanding materials relative to their cause, as well as the potential for providing oxygen to the Deep Water Channel. Part of the process of balancing cost and goals and what can be accomplished should be investigated from a technical and engineering perspective as part of the TMDL work plan.

My wife Dr. Jones-Lee and I, and my graduate students, have done extensive work on dredged sediment management in many waterbodies around the country. Based on having investigated many waterbodies associated with dredging operations, as well as many lakes and several estuary systems, it is my assessment that the Deep Water Channel in the San Joaquin River will always have a DO depletion problem even if all farming, all municipalities, and all inputs from anthropogenic sources were stopped. The development of that Channel creates a situation where DO depletions near the bottom are characteristic of a system that will occur no matter what nutrient control program is implemented. This does not mean that nutrient control should not be implemented, it just means there is need to establish reasonable goals to be certain that expenditure of funds results in a significant beneficial use of the area. Rather than waiting until the end of the TMDL process to begin to address this issue as part of the TMDL plan, this issue needs to be addressed now through a special committee of experts who can work with appropriate stakeholders to begin to formulate how best to address this issue.

Page 6 under Task 2, as discussed above the accumulation of all fisheries data for the San Joaquin River and South Delta, and its integration with water quality data, is not something that, as far as I have heard thus far, is going to be accomplished in this project. That information, while highly desirable, is apparently beyond the scope of funding available.

Page 6, Task 2, first paragraph mentions the continuous DO monitors operated on the San Joaquin River. If I understand this situation correctly, these monitors only sample surface waters. It is difficult for me to understand why anyone would establish a DO monitor that only samples surface waters. It has been known for at least 40 years that in situations such as this there will be vertical changes in DO. In order to describe DO in the water column at a particular location it is necessary to monitor at several depths. Continuous DO surface monitoring can be used in a meaningful way if, and only if, periodic vertical profiles are taken at the location where the surface monitoring is taking place. Without vertical profiles the DWR surface water monitoring is of limited value in addressing real issues of concern to water quality management, and may, as it has done, lead to incorrect conclusions concerning the DO situation in the Deep Water Channel where there was a lack of understanding of DO depletion near the bottom by those who had been working on this issue for a number of years at the beginning of summer 1999.

The first line of page 7 mentions fluorescence monitoring for chlorophyll will be added to the DWR stations for continuous DO monitoring. As I have indicated, any continuous monitoring of chlorophyll using fluorescence must be accompanied by a comprehensive program to relate what is measured through fluorescence to actual chlorophyll that is related to algal biomass. In a situation such as the San Joaquin River, the fluorescence measurements can easily lead to highly unreliable assessments of the planktonic algal biomass present in the sample.

Page 7, third paragraph states *"the volatile suspended solids (VSS) measurements were eliminated recently, but should be continued because this is the only measurement of organic material"*. As I discussed at a recent Technical Committee meeting, VSS can be highly unreliable in estimating organics, especially if there is any significant amount of magnesium carbonate present in the evaporated samples. Magnesium carbonate and some other inorganic constituents are volatile under the temperatures at which combustion of suspended solids take place in the VSS test. Studies need to be done to evaluate what VSS measurements on the San Joaquin River mean relative to algal concentrations as made by properly conducting chlorophyll measurements. Another factor to consider is that diatoms will show a different relationship between VSS and total solids/chlorophyll because of the silica content of the organisms. A much greater percentage of other types of algae will be measured as volatile suspended solids than for diatoms.

Page 7, third paragraph states that *"BOD samples should be added to the sampling plan for at least a year or two to obtain correlations with chlorophyll and VSS"*. I agree, although BOD should be measured routinely throughout the TMDL development and implementation process. There should be no termination of BOD measurements for at least five to ten years, if ever. It is the only parameter that potentially measures the oxygen demand in the water column.

Page 7, fourth paragraph mentions there is a possibility that the City of Stockton would reinstate it's frequent measurements of various parameters if the data were determined to be useful. Data of this type is useful to this project. We need to have current data on these parameters to compare to the 1991 extreme drought period. The City of Stockton should conduct these types of studies for several years.

Page 8, first paragraph, mentions that the City of Stockton routinely measures certain parameters which include total phosphorus. Soluble orthophosphate should also be measured, as well as chlorophyll. Also, there is need for the City of Stockton to do a study on the characteristics of it's effluent that lead to increased ammonia concentrations being discharged from it's treatment works beginning in August each year. There is need to understand why this occurs and what its significance is to contributing to localized DO depletion. It may not be possible to judge this significance based on a diluted effluent. There may be localized impacts that need to be considered as part of the TMDL process.

Page 8, at the bottom, mentions three basic sources of BOD materials, one of which is the *"Stockton RWCF effluent load (i.e., BOD and ammonia)"*. Added to these sources should be organic nitrogen. Organic nitrogen discharged by Stockton could, at certain times of the year, contribute to the sediment oxygen demand through the mineralization and conversion to ammonia which then either depletes oxygen directly, or through the conversion to nitrate under aerobic conditions, stimulates the growth of algae. A sub-set on the Stockton sources that is not mentioned is the late spring and summer wet weather and dry weather discharges from urban areas within Stockton, and possibly other communities, to the San Joaquin River system which either add oxygen demanding materials directly, such as ammonia, or add nutrients, such as nitrate, which in turn stimulates the growth of algae that leads to oxygen depletion. The urban stormwater impacts on the oxygen resources of the Deep Water Channel need to be included in the special purpose studies.

Page 8, last item, *"sediment sources of biochemical oxygen demands (i.e., SOD and ammonia)"* should be changed from biochemical oxygen demand to sediment associated oxygen demand. A substantial part of the oxygen demand associated with sediments is not biochemically controlled. It has been well understood for nearly 40 years that an appreciable part of sediment oxygen demand is due to chemical (abiotic) reactions between ferrous iron and sulfides which react directly with oxygen that either migrates into the sediments, or, when the sediments are stirred in the water column, reacts with oxygen in the water column. These reactions are extremely fast in the neutral to alkaline pH range compared to the biochemical oxygen demand of conversion of organics, including dead algae, through respiratory processes to CO_2 and water. Failure to recognize the abiotic sediment oxygen demand reactions is a recurring problem that continues to crop up in the SJR writings about sediment oxygen demand issues. I have commented on it number of times. There is need for all of the SJR TMDL participants to read the sediment oxygen demand conference proceedings booklet that was held at the University of Georgia several years ago.

Page 9, first paragraph, line 7, which states *"three basic sources"*, change that to four basic sources which includes the urban stormwater and dry weather flow contribution.

Page 9, third paragraph, I agree that there is need to do a special study on Stockton's oxidation pond effluent, especially relative to the ammonia situation. I am also concerned about the summer addition of nutrients from local nearby sources to the San Joaquin River Deep Water Channel area which stimulates local growth of algae. This needs to be addressed.

Page 10, second paragraph, mentions the continuous monitoring of chlorophyll fluorescence at Mossdale will be conducted. As I have discussed, there is need to be certain that monitoring yields representative results, not only surface water monitoring but also monitoring near the bottom. Situations such as a slow moving river can have appreciable near bed load transport of dead or dying algae, which can be much higher than at other locations in the water column.

A study needs to be done to better understand what happens between Vernalis and Mossdale with respect to nutrients and algae concentrations. Are the differences that have been observed due to the inadequate sampling at either or both of these locations? More detailed sampling at both locations needs to be done every couple of weeks to determine what the real load is, and how the measurements that are being made on a routine basis correspond to what the real algae and oxygen demand load is at these locations.

Page 10, fourth paragraph, states in the middle of the paragraph, that the River is fully mixed. One of the issues that I am seeing through my attending various Technical Committee meetings is an apparent lack of understanding of how little temperature change in the temperature range of 20-30°C can lead to stratification. The change in density of water with temperature in the 20-30°C range is very steep, leading to small changes in temperature causing significant stratification barriers to mixing. It is important that consideration of density differences versus depth be considered in assessing whether the system is fully mixed. From what I have seen of this system it may not always be well mixed, especially under low flow conditions. This needs to be better understood.

Another aspect of this situation is the effect of small boat and, in the Deep Water Channel, large ship traffic on mixing. A special purpose study should be conducted to see if the intensive small boat traffic in the San Joaquin River and large boat traffic in the Deep Water Channel are significant factors that, in times of intense use, lead to destruction of stratification and the mixing of low DO waters from near the bottom into the water column.

Page 10, fourth paragraph, discusses the relationship between turbidity and algae. Several years ago I had a large project in Spain concerned with assessing nutrient loads/eutrophication response of

Spain's 800 reservoirs. As part of this study, and the international OECD studies that my graduate studies conducted during the 1970s and 1980s, we established a relationship between planktonic algal chlorophyll and Secchi depth in waterbodies. This relationship can be used to estimate the impact of inorganic turbidity and color on algal growth in waterbodies. We found that in order for a water body to have decreased algal production compared to what it should, based on its phosphorus load, the Secchi depth during the summer had to be less than about two-tenths of a meter. There is need to examine the Secchi depth planktonic algal chlorophyll data for the surface waters of the San Joaquin River and Deep Water Channel area to see whether the inorganic turbidity arising from upstream erosion is contributing to decreased rates of algal growth. This is of potential significance since the control of the erosion upstream could result in increased water clarity which would stimulate additional algal growth in the critical areas just upstream of the Deep Water Channel.

Page 11, first paragraph, discusses the settling of diatoms because they are poorly adapted to the Deep Water Channel area, where they settle to the bottom. The issue is not adaptation to the Deep Water Channel, it is that algal and bacterial respiration exceeds photosynthesis and there is a net oxygen demand due to respiratory activities and eventual death and decomposition.

Page 11, first paragraph mentions that measurements of settling and decomposition rates in algae will be undertaken. As has been discussed at various Technical Committee meetings, the measurement of settling algae is very difficult to do and often highly unreliable.

Page 11, second paragraph states that *"light/dark bottle measurements will be used to determine the light conditions necessary to maintain algal growth and to characterize the effects of algae on DO in the downstream deep water ship channel"*. We do not need to do light and dark bottle measurements to make those estimates. Secchi depth is adequate to characterize the photic zone. There is so much work throughout the world on this topic that it is relatively easy to estimate. Some confirmation may be desired to show that the classical relationships that have been found by many investigators in many parts of the world are applicable to the San Joaquin River system as well.

Page 11, second paragraph again focuses sediment oxygen demand on organics. While organic material such as dead algae and possible particulate wastewater components have originally contributed to and continue to maintain sediment oxygen demand, they may not be the primary basis for exercising an oxygen demand during the summer months. The accumulated reduced ferrous iron and sulfide are almost certainly a primary factor in controlling sediment oxygen demand. This has been found in investigation after investigation throughout the world. As I discussed last June, studies need to be done to establish the relative significance of biologically mediated oxygen demand in sediments versus chemical-abiotic sediment oxygen demand.

Page 11, second paragraph mentions settling chambers to determine the deposition of algae and other organic materials in the river sediment. Great caution must be exercised in following this approach. How will the issue of large ship traffic, which stirs the sediments from the bottom to the surface, be interpreted? There is no doubt the passage of a ship will stir up large amounts of materials from the sediments which then could appear as fresh deposited algae in any sediment traps.

Page 11, third paragraph, discusses the potential contributions of urban drainage as a source of oxygen demand and nutrients. One of the components of the studies that should be conducted is to evaluate the source of the nutrients that are found in the stormwater runoff, especially during low flow conditions. Of particular concern is the potential for high groundwater within the City to be contaminated by nitrate from lawn fertilizers, which in turn then is a source of nitrate within the dry weather flows. If high nitrate is found in the dry weather flows, then studies upstream of the source of this nitrate should be conducted. Further, if the dry weather flows are high in ammonia then the source of the ammonia (i.e., possibly decomposing organic nitrogen that has accumulated in the storm sewer system) should be investigated. Is this due to leaves and other vegetative materials entering the storm sewer system?

Page 11, last paragraph, in mid-paragraph mentions first sediment chemistry and then COD. First, the term chemistry is wrong. Chemistry refers to the chemical reactions involved. Sediment chemical characteristics is the proper terminology. Second, as I have discussed in detail previously, COD is not a measurement that gives an interpretable assessment of anything. It should not be measured. As I have discussed at previous meetings, what needs to be measured is the rate of oxygen exertion by sediments taken from various depths, as well as suspended into the water column near the sediment-water interface. Through the kinetics of oxygen uptake, it is possible to determine whether the oxygen depletion is due to abiotic-chemical reactions between ferrous iron, sulfide, and oxygen, or is due to biochemical processes which have much lower rate constants for oxygen depletion versus time.

Page 11, last paragraph mentions the study of temperature effects. One of the things that may be found is that the temperature effects on sediment DO depletion rates may be very small due to the fact that the reaction between ferrous iron and oxygen have a low activation energy, and thereby limited temperature coefficient, while BOD typically has a doubling per 10°C change in rate.

Bottom of page 11, top of page 12, mentions the proposed approaches for assessing sediment oxygen demand. As I mentioned previously, great caution should be exercised about proceeding with sediment oxygen demand measurements as were proposed in the CALFED proposal. Certainly, the original proposal approach of simply measuring a bunch of sediment oxygen demands at various locations with the stirred chambers is highly questionable and should only be done after proper evaluation has been made in which there is an understanding of how to interpret data of this type. Such measurements do not provide realistic assessments of the true sediment oxygen demand of sediments, and can easily yield erroneous conclusions on the role of oxygen demanding materials of sediments in depleting oxygen in the overlying waters.

Page 12, second paragraph mentions the USGS has a turbidity meter at the Stockton UVM station. Does this measure turbidity at various depths? If not, then it may be giving an unreliable assessment of suspended particulates in the water column. Again, as with all these continuous measurements, there is need to do detailed vertical profiles at various times over several years to be certain that the measurements made represent interpretable results.

Page 12, second paragraph uses the term "effective SOD". I do not know what "effective SOD" is. This should be defined, or deleted and just use SOD.

With respect to SOD, as discussed previously, it is very important to incorporate the concept of bedded SOD (the SOD associated with sediments that have not been stirred into the water column), and suspended SOD (the SOD associated with water currents, ship traffic, organism activity, etc.).

Page 12, third paragraph states *"because the most likely sources of SOD materials are upstream river loads and the RWCF effluent, ..."*. I do not know that the most likely source of SOD is upstream river loads. There can be a very significant SOD associated with the algae that developed within the Deep Water Channel. The issue of algal development within the Deep Water Channel area versus upstream sources needs to be investigated in this study.

Page 12, third paragraph states that *"natural isotope analysis (i.e., carbon, nitrogen) to identify the dominant source of sediment organic material"* could be explored in this study. Great caution has to be used in all of these attempts to use tracers to investigate sources of constituents. As an individual who has done considerable work on natural and anthropogenic tracers or constituents, I know, from my own experience and the literature, that it is virtually impossible to use tracers for any other purpose than to say that a particular source, such as an upstream dairy, contributes the tracer compound to a particular point where measurements are made. We already know that information. If the dairy discharges into the San Joaquin River, or to land where stormwater runoff from the area receiving the discharge enters the San Joaquin River, then you would expect that materials derived from the dairy will be present in the San Joaquin River, including the Deep Water Channel. If these materials are either particulate or incorporated into algae, then they will show up in the sediments. This does not mean, however, that the finding of a constituent in settling organics has any meaning to the source of constituents from the dairy, etc., being a significant factor in sediment oxygen demand. A major study will have to be done with each tracer to see how the tracer behaves with respect to transport, transformations, etc., relative to the constituents of concern, namely those constituents that either directly or indirectly through algae contribute to sediment oxygen demand.

These kinds of studies could require massive expenditures and prove to be largely unreliable. Tracers may have very limited application to addressing real issues associated with understanding the water column and sediment oxygen demand in the San Joaquin River Deep Water Channel area. Any proposal to use tracers within this system must be accompanied by a detailed discussion that can be reviewed, prior to their use, on how the data will be used to interpret the relationship between the tracer measurements at a particular location in time, and the constituents that exercise an oxygen demand within the water column and sediments.

Page 12, under Task 4, second line, mentions water quality models. As discussed elsewhere, these are not water quality models, they are DO concentration models. The coupling between DO and water quality beneficial use impairments is not considered nor addressed in the model.

Page 12, Task 4, near the end of the first paragraph, states *"the models can be tested by comparing measurements with simulated historical conditions"*. Basically, this is curve fitting. While this is the conventional approach that is used for modeling at this time, it is not necessarily reliable and has often been found to be unreliable in predicting how altering nutrient loads to a waterbody will affect algal production-biomass and sediment oxygen demand. There have been a number of studies in the Great Lakes region on the relative merits of various types of modeling, where it has been concluded by several expert panels that numeric models of the Chen DO model type, while useful tools for helping to understand processes through defining areas that need attention, may have limited reliability in predicting the impact of altering nutrient loads on water quality characteristics. The basic problem is that these models are all gross oversimplifications of the real system. While they may be tuned through adjustment of coefficients to an average between wet years, dry years, etc., they do not have the ability to be extrapolated to significant changes in nutrient loads. Therefore, while they are the best tools available for this type of situation, it should be understood that they may not be very reliable even if the data can be curve fitted to several years.

Page 13, first paragraph, discusses *"the model(s) be calibrated and confirmed by independent peer review"*. Independent peer review is not going to solve the inherent limitations of the predictive capabilities of such models under significantly altered nutrient conditions. All the peer review can do is to point out potential problem areas, it cannot correct the problems.

Page 13, under Task 5 - Verify the water quality targets for the TMDL, states that *"the DO objective of 6 mg/L during the fall period is protective of migrating chinook salmon"*. As discussed elsewhere, that statement is largely without a technical foundation. The studies that have been done do not support that statement. They provide inference that over 30 years ago, based on tagging just a few fish over a limited period of time, it appears that the fish migration upstream through the Deep Water Channel area is inhibited by some factor which could be dissolved oxygen, temperature, a variety of other pollutants, a lack of flow, and while not discussed, the loss of chemical signal from home waters. It is certainly inappropriate to extrapolate from the mid 1960s to the 2000 conditions and say that those studies have any relationship to the conditions that exist there now. If the 6 mg/L DO TMDL objective persists, then there is an urgent need for substantial efforts to establish that it has some technical validity for today's conditions.

Page 13, second paragraph under Task 5, mentions that DFG is planning to do some fish tagging studies in the fall of 1999. I understand that these studies are not going to be done now. This should be verified. Even if there is a delayed migration it should not be interpreted, without considerable additional studies, that this delayed migration is in fact due to a depletion in dissolved oxygen in the Deep Water Channel area. There are many reasons why fish may not migrate upstream. Before the stakeholders in the San Joaquin River watershed are trapped into spending tens to maybe one hundred or more million dollars in nutrient control to try to meet a 6 mg/L DO level within the Deep Water Channel area, there is ample justification to properly investigate whether a delayed fish migration does in fact occur, and the cause of this delayed migration be reliably established, with particular reference to whether DO less than 6 mg/L is a cause.

Page 13, last paragraph, and top of page 14, discusses hydroacoustic monitoring of fish migration. I strongly support such approaches because of the fact that fish migration issues play such a dominant role in establishing one of the TMDL goals.

Page 14, end of the first paragraph, states that *"timing of the chinook migration in years with relatively high flows (i.e., good water quality conditions) can be used as a template for evaluating the effects of poor water quality (high temperature, low DO) on chinook migration"*. Even if there is a difference found between high flow years and low flow years, it still does not establish that dissolved oxygen is the constituent responsible for the lack of migration under low flow conditions. Associated with low flow are a variety of other factors that can influence fish migration which are likely much more important than changes in the DO from 6 mg/L to 5 mg/L.

Page 14, first paragraph, again the term water quality is used too loosely to describe DO conditions. The authors of documents in this process should stop using the term water quality as a generic term, when they really mean DO. As discussed elsewhere in these comments, water quality has a much broader context than just DO in the Deep Water Channel.

Page 14, third paragraph, mentions a Corps of Engineers aeration device. At a recent Technical Committee meeting the statement was made that the Port/Corps was operating the aeration system at this time. Following that meeting, I visited the area and found that the aerators have not been unpacked for this year. They were not operating and, in fact, were still covered with a canvas cover. There is need for someone to contact those who control the operation of this system to find out the actual practices with respect to how the decision is made when to operate, what operation is actually done, etc.

Page 14, third paragraph, discusses studies to verify that the generator is functioning as designed. Before any field studies are undertaken a critical evaluation of what is expected from this type of device, based on studies conducted elsewhere, should be conducted. Further, some preliminary calculations should be made on the potential cost of installing and operating aeration devices of the type that has been installed, as well as other types of devices that could be installed, to contribute 1, 2, 3, and 4 mg/L to the dissolved oxygen deficit in the Deep Water Channel during times when the DO is less than 5 and 6 mg/L. Is aeration of this system even potentially economically feasible with the best of the aerators that are available? If the results of those calculations show promise, then possibly proceed with field studies of the type discussed. Some pre-engineering calculations should be done before any field testing is done of the existing aeration device.

One of the issues that is not discussed as a potential remedy for the low DO situation is a diversion of Sacramento River water flow to the San Joaquin River to flush out the Deep Water Channel during low flow conditions. There can be little doubt that ultimately the southern California interests will be able to develop some type of "peripheral canal" which will divert Sacramento River water around the Delta for export south. As part of this process, consideration needs to be given to whether the size of the diversion channel could not be increased, down to the San Joaquin River just above Stockton, to transport Sacramento River water to the San Joaquin River to increase flow of the San Joaquin River during low flow conditions.

Page 14, last paragraph, states *"there are substantial urban drainage and agricultural drainage loads of nutrients and organics..."*. This discussion takes place in connection with assessing "non-point sources" of constituents. It should be understood that stormwater for regulatory purposes, at this time from any community that has a population over 100,000, is classified as a point source and that likely beginning this fall smaller communities which will be regulated under US EPA's Phase II urban stormwater management program, will be point sources since they will have NPDES permits. The only non-point urban sources will be those small communities that are excluded from Phase II. The Phase II rules are not yet published in final form, and therefore the communities that would be involved are not yet known.

Page 14, near the bottom of the last paragraph, states *"both urban and agricultural best management practices (BMP) for controlling nutrients and organic runoff will be investigated and evaluated"*. Is this to be done under this TMDL process? If so, then a detailed write up of this issue should be included.

This write up, and elsewhere in our discussions, the term BMP has been used, as it traditionally is, to imply some kind of a process that will control constituents in non-point source runoff. The term BMP, however, is frequently incorrectly used. The BMPs that have been developed for urban stormwater runoff, and for that matter agricultural runoff, have been developed without regard to what they accomplish in the way of improvement in the beneficial use of receiving waters receiving the BMP treated water. Several years ago Dr. Anne Jones-Lee and I wrote a paper entitled "Stormwater Managers Should Beware of Snake-Oil BMPs for Water Quality Management" (1998) - available from our website, www.gfredlee.com. Typically, BMPs for treating urban stormwater runoff, and for that matter agricultural runoff, are selected from a compilation in a BMP manual. They include such things as retention basins, grassy swales, infiltration systems, etc., and non-structural approaches such as good housekeeping, etc. They can also include advanced wastewater treatment technology such as reverse osmosis, activated carbon treatment, etc.

Unfortunately, the urban stormwater field has been using the term BMP incorrectly, where those working with cities and some city public works departments assume that if they use a "BMP" out of a BMP manual, and install this to satisfy regulatory requirements of having "treated" the stormwater runoff with a BMP, that they are protecting water quality. However, when the efficacy of a BMP is evaluated from a water quality management perspective, this approach is obviously technically invalid. The purpose of the BMP (best management practice) is to control the water quality use impairment in the receiving waters for which the BMP is installed. While typically BMPs are evaluated by an across the unit removal, the only way to truly evaluate the efficacy of a BMP is to assess its impact on the beneficial uses of the receiving waters which are treated by the BMP.

An example of the inappropriate approach that conventionally occurs is the removal of heavy metals from urban stormwater drainage. Urban street drainage contains high concentrations of several heavy metals, such as copper, lead, frequently zinc, and occasionally cadmium. Most of these heavy metals are in particulate form. Running urban stormwater through a detention basin will remove some of the particulate heavy metals and give, depending on the hydraulic loading of the detention basin and its design, a significant percent removal of heavy metals can be achieved. However, when this removal is examined on the impact to the receiving waters, it is found that the particulate heavy metals in urban stormwater drainage are in non-toxic, non-available forms and do not affect the beneficial uses of the receiving waters. The toxic, available forms of heavy metals pass through the detention basin. These are the forms of the metals that need to be removed. The efficacy of the BMP evaluation, however, ignores this issue. It is important that any BMP evaluation associated with this TMDL development be based on actually addressing the DO in the San Joaquin River Deep Water Channel.