AMENDMENTS

TO THE

WATER QUALITY CONTROL PLAN
FOR THE SACRAMENTO RIVER AND
SAN JOAQUIN RIVER BASINS

FOR

PH AND TURBIDITY

Responses to Peer Review Comments
on
December 2004 Draft Staff Report
and Supporting Documents

March 2007

This document contains Central Valley Regional Water Quality Control Board (Regional Water Board) staff's responses to peer review comments on: *Amendments to the Water Quality Control Plan for the Sacramento River and San Joaquin River Basins for pH and Turbidity*, as presented in staff's Draft Staff Report (Staff Report) and appendices. The peer review comments were provided by: 1) Dr. Don C. Erman, Professor Emeritus, University of California, Davis (Attachment 1) and 2) Dr. Peter B. Moyle, Professor in the Department of Wildlife, Fish, and Conservation Biology, University of California, Davis (Attachment 2). Responses to Dr. Erman's comments are provided in Part I of this document, with Dr. Moyle's comments addressed in Part II of this document.

I. Responses to Dr. Erman's Peer Review Comments

Each individual comment has been identified and provided a number to assure that the comment has been addressed by staff (**Attachment 1**). Based on all individual, specific comments identified in Attachment 1, a number of "Master Comments" are identified that define the key topical areas of comment. Responses to the Master Comments are provided first below, followed by a response to each of the numbered specific comments. If the Master Response fully addresses an individual numbered comment, it is so indicated. Conversely, if Regional Water Board staff believe that the individual comment requires further address beyond that provided by the response to its associated Master Comment, additional response is provided under that specific numbered comment. This "cross-referencing" approach was used to prevent repeating responses in their entirety for multiple comments that essentially address the same issue. The Master Comments and "Master Responses" are divided into the same three categories that this peer reviewer divided his comments into, which are: 1) Scope of proposed amendment; 2) science supporting proposed turbidity amendment; and 3) science supporting proposed pH amendment.

I. Master Comments and Responses

A. Scope of the Proposed Amendments

Master Comment 1: The Staff Report is focused on municipal wastewater treatment plant (WWTP) discharges, with an emphasis on low-flow effluent dominated/dependent water bodies (EDWs), yet proposed amendments would apply throughout the basins (with specific exceptions). The scientific justification, to water bodies not effluent dominated, for this broader application of the change in objectives is not presented in any of the documentation. In addition, the staff report and accompanying information does not fully evaluate the impacts of allowing unlimited pH change within the range of 6.5 to 8.5 in larger water bodies.

Response: The Staff Report and its supporting documentation does utilize water bodies receiving municipal effluent discharges, and in particular EDWs, as the most prevalent example of where compliance problems associated with the current pH and 0-5 NTU turbidity objectives occur within the basins. Compliance issues associated with these objectives do not occur in water bodies uninfluenced or insignificantly influenced by controllable factors.

The need to address common and widespread compliance issues associated with the current pH and 0-5 NTU turbidity objectives were identified in the past several triennial reviews of the Basin Plan, which served as an impetus for staff to investigate the alternative means of resolving these compliance issues addressed in the Staff Report. While low flowing streams provide an excellent example of why the amendment is necessary, the scientific literature does not distinguish between low-flowing and high-flowing streams. The literature indicates that regardless of stream flow, beneficial uses are not affected by the proposed amendments to the pH and turbidity water quality objectives.

The Alternative to amend the basin wide water quality objectives for pH and turbidity without limiting the amendment by flow (i.e. Alternative 3) was selected as the preferred Alternative because it: (1) is consistent with State and Federal water quality laws, water quality criteria, and policies, including antidegradation policies; (2) protects beneficial uses; (3) is consistent with current science and will improve the scientific basis upon which the water quality objective is based; and (4) addresses regulatory issues. The need for appropriate pH and turbidity objectives that are neither over nor under protective of beneficial uses resulted in selection of Alternative 3.

The proposed pH amendment is technically consistent with U.S. EPA's past and present recommended pH criteria for freshwaters which do not include criteria based on flow. The current scientific information upon which the proposed amendment is based is the entire body of science compiled, presented, and interpreted by NTAC (1968) ("Green Book"), NAS (1972) ("Blue Book"), USEPA (1976) ("Red Book"), USEPA (1986) ("Gold Book"), and USEPA (2002) (Current EPA recommended water quality criteria). The body of science pertaining to the pH requirements of freshwater aquatic life presented in these documents supports the proposed pH objective not only for effluent dominated water bodies, but also for all freshwaters within the basins. Moreover, the proposed pH objective is essentially equally restrictive to the most restrictive recommended pH criteria published by NTAC (1968), and is more restrictive than the national pH criteria recommended for the protection of freshwater aquatic life (the beneficial use requiring the most restrictive pH criteria) since 1972, which is the same as that recommended by U.S. EPA today (USEPA 2002).

In the event that a waste discharge were to elevate one or more metal concentrations in the receiving waters to levels at or above the unadjusted CTR criteria and, concurrently, reduce pH to the low end of allowed pH range (e.g., 6.5), this could be effectively addressed in the NPDES permitting process, irrespective of the proposed amendments.

The same national criteria documents cited above were reviewed for their turbidity criteria recommendations. However, none of these documents recommend discrete NTU-based turbidity criteria for water bodies at times when background turbidity is 0-5 NTUs. Moreover, the proposed amendment only changes the current 0-5 NTU turbidity objective where background turbidity is less than 1.0 NTU. When background turbidity is ≥1.0 NTU, the amendment proposes no change to the current turbidity objective. Under conditions of background water body turbidity <1.0 NTU, visual esthetics quality becomes the beneficial use of primary interest because turbidity levels <1.0 NTU are below levels of concern for the protection of all other beneficial uses. The Staff Report has been revised to include key findings from recent research on low-level turbidity effects on clear water fishes (see Newcombe 2003) to address the reviewer's comment.

The staff report has been revised to more clearly state that the low-flow discussion provides the need for the amendment and the scientific literature provides the basis for establishing appropriately protective water quality objectives for pH and turbidity for all water bodies.

Master Comment 2: Reason for not selecting the averaging periods alternative is weak. In addition, the recommendation doesn't actually "reflect" federal criteria because the recommendation does not include a change to the maximum pH limitation. Also, the Staff Report and accompanying documents do not evaluate the effects of pH varying across the full range of 6.5 to 8.5 in all water bodies

Response: Averaging periods are allowed for determining compliance provided that beneficial uses will be fully protected. Therefore, while they can, in some cases, address regulatory issues they are not the best solution since they require the burden of development and tracking of averaging periods, by water body, while providing no water quality or beneficial use improvements. This point will be clarified in the Staff Report Section 1.3.3.1 and 9.7 to explain why this option is not viable and should not be selected.

The water quality objective range of 6.5 to 8.5 for pH is consistent with the current science and EPA criteria; although, it is not exactly the same as the EPA criteria. U.S. EPA has defined its recommended range for protection of freshwater aquatic life as 6.5 to 9.0 but allows states to adopt more stringent criteria. Most aquatic life do very well within a given *range* of pH, but do poorly outside that range. Each species is a little different in terms of its pH tolerance range, and this range can vary by life stage within a species as well. The current body of science shows that some common and important forms of aquatic life can begin to show low-level adverse effects from pH at or above about 9.0 (e.g., salmonids) (McKee and Wolf 1963, NAS 1972, Witschi and Ziebel 1979, and Modin, pers. comm., 1998). Hence, the decision to leave the Central Valley pH objective maximum at the existing 8.5 is consistent with the current science and the U.S. EPA criteria because it recognizes that local organisms such as salmonids are sensitive to pH at or above about 9.0. The Staff Report will be revised to clarify that the pH objective is consistent with current science and recommended federal criteria rather than reflects current federal criteria.

The body of scientific literature and the U.S. EPA recommended criteria do not distinguish between different water body types in terms of high-flow and low-flow. The

Staff Report discusses the situation in low-flow water bodies because that is where a regulatory issue has been identified and data is available. There is no similar data set for higher flowing water bodies and field sampling is not possible since varying pH of this type is not found in higher flowing water bodies. Because the literature and the U.S EPA recommended criteria do not suggest that different water body types need to be treated differently when regulating pH and low-level turbidity, staff recommendation is that the water quality objective in the basin plan should be consistent and not recommend different objectives for different stream flows or types.

B. Science in Support of Proposed Turbidity Amendment

Master Comment 3: The review of literature on sediments and turbidity is truncated in time and limited in extent.

Response: The additional article identified by the reviewer (i.e., Waters 1995) was reviewed. It should be noted that the discussions presented in Waters (1995) are about suspended sediment's effects on aquatic life, not low-level (i.e., <2.0 NTU) turbidity and its effects on aquatic life or other beneficial uses. The Waters article does not provide information directly relevant to the proposed amendment, nor does the information it does present change staff's findings with regards to low-level turbidity effects. While the Staff Report includes references to studies on suspended sediments, recent studies specifically on low-level turbidity are more relevant. Therefore, the Waters 1995 study will not be used but the Newcombe 2003 study will be used in the Staff Report. Edits will be made to the Staff Report, as necessary.

Additional literature, beyond that identified by the reviewer, also was reviewed to further demonstrate the adequacy of the proposed turbidity objective. Findings from articles obtained from this search that address turbidity effects on primary production are discussed under specific comment #12 (below). Findings from articles addressing the effects of turbidity on macroinvertebrates and fish are discussed below. These additional technical findings will be added to the Staff Report and supporting documentation.

Fish. Bash et al. (2001) prepared a review article on the effects of turbidity and suspended solids on salmonids. Of the scores of scientific articles/reports reviewed, none reported adverse effects on fish at turbidity levels addressed by the proposed amendment (i.e., background turbidity <1.0 NTU not to exceed 2 NTUs). Low-level turbidity effects on fish tend to be related to visually oriented fishes' ability to capture prey and/or avoid predation. Sufficiently high degree of effects on these key behaviors can lead to effects on growth and survival of certain species that, in turn, can lead to population or community level effects. Nevertheless, as shown by Bash et al. (2001), turbidities well above 2 NTUs are required to produce sufficient behavioral effects that would lead to reduced growth or population or community effects. Even subtle behavioral effects (e.g., reaction distance to prey, avoidance responses) were not reported at turbidity levels below 2 NTUs.

Sweka and Hartman (2001) evaluated reaction distance in brook trout under various turbidity levels. Data from this study show a statistically significant decline in reaction distance with increasing turbidity when all data are analyzed for turbidity levels between about 1 NTU and >40 NTUs. However, there was no statistically significant relationship between reaction distance and turbidity for turbidity levels <5 NTU. Servizi and Martens (1992) estimated that the threshold for avoidance behavior by juvenile coho salmon was 37 NTUs. Berg (1982, as cited in Bash et al. 2001) found that juvenile coho exposed to a short-term pulse of 60 NTU left the water column and congregated at the bottom of the test tank. When turbidity was reduced to 20 NTU, the fish returned to the water column. Similarly, Bisson and Bilby (1982) exposed juvenile coho salmon to elevated suspended sediment. Juveniles did not avoid moderate increases in turbidity when background turbidity levels were low. In this study, significant avoidance required a turbidity of 70 NTUs.

Most of the studies identified in the above literature did not evaluate turbidity increases as small as 2 NTUs. Waters having turbidities around 2 NTUs were typically part of the "control" or "clear water" test group, not an "elevated turbidity" treatment group. A good example of this is provided by Lloyd et al. (1987), who reported that arctic grayling were absent from reaches below mines, where average turbidities ranged from 75 to 727 NTUs, but that un-mined reaches, having average turbidities from 1.3 to 2.7 NTUs, had 0.5 to 8.7 grayling per haul. In tests conducted by Gradall and Swenson (1982) to examine behavioral effects associated with elevated turbidity, clear water control turbidities ranged from approximately 1.0 to 3.5 (mean = 2.4) NTUs for brook trout (*Salvelinus fontinalis*) and 2.0 to 3.0 (mean = 2.3) NTUs for creek chubs (*Semotilus atromaculatus*).

A key exception to the general trend in the scientific literature described above is provided by Newcombe (2003). This review article did evaluate effects down to the 2-3 NTU turbidity level. Newcombe (2003) evaluated the severity of effects (e.g., fish reactive distance, predatory prey dynamics, egg and larval growth rates, and habitat effects) for clear water fishes exposed to 2-10 NTU turbidity increases for exposure periods ranging from 1 hour to > 10 weeks. According to the model presented in Newcombe (2003), 3 NTUs would be protective of clear water fishes for long-term exposures.

Summary statistics for clear water Oregon streams during the low-flow period of the year (late May through early October) for the period 1992-2002 show variability in daily turbidity that includes and occasionally exceeds 2 NTUs. A number of water bodies (e.g., Siletz River, Umpqua River, Metolius River, Chetco River, and Wilson River) with median turbidities of 0.5 to 1.0 NTU show standard deviations of 0.4 to 1.8 NTU and maximum measured values during the low-flow period of 2.0 to 5.4 NTUs. A turbidity value of 2 NTU is within the range of low-level turbidity experienced by these water bodies during the summer low-flow (clearest water) period of the year (Rosetta 2005).

Macroinvertebrates. Quinn et al. (1992) evaluated the effects of long-term exposure to elevated turbidity on macroinvertebrate densities and taxonomic richness. Quinn et al.

(1992) determined that both densities and taxonomic richness decreased at downstream sites having higher turbidities compared to lower turbidity sites upstream. Invertebrate density decreased significantly at downstream sites that had median turbidity levels from 7 to 154 NTUs higher than upstream sites, which had median turbidities of 0.9 to 4 NTUs. Invertebrate taxonomic richness decreased significantly between all but two downstream sites having median turbidities 23-154 NTUs higher than upstream sites. The authors concluded that turbidity related effects on primary production and thus food supply for the invertebrates was the cause of the above-cited effects.

It should be noted that the lowest turbidity increase documented by this study to decrease macroinvertebrate density was 7 NTUs, and to decrease taxonomic richness was a 23 NTU increase. It is equally revealing that the upstream, low turbidity "reference sites" used in this study were characterized by median turbidities of 0.9 to 4 NTUs (i.e., a turbidity range that extends beyond that limited by the proposed objective). The nature of this study, and its findings, support the conclusion that the level of turbidity to be regulated by the proposed amendment (i.e., background turbidity <1.0 NTU, not to be increased beyond 2 NTUs) is below levels of concern for effects on macroinvertebrate communities.

Based on the collective scientific findings already cited in the Staff Report and additional literature cited above (and under the response provided for specific comment #12), the proposed turbidity amendment would be highly protective of beneficial uses in water bodies throughout the basins.

Master Comment 4: Much of the scientific literature cited and discussed is for total suspended solids and sediment rather than turbidity per se.

Response: Section 4.2.5 of the Staff Report explains why much of the scientific literature reviewed is expressed as total suspended solids (TSS) information rather than turbidity. In short, because suspended solids cause turbidity, the two parameters are often discussed together in the scientific literature and sometimes used rather synonymously. Nevertheless, they are distinctly different parameters, and the proposed objective addresses low-level turbidity.

The degree of turbidity is not equal to the concentration or quantity of suspended solids. Rather, turbidity is an expression of only one effect of suspended solids upon the character of water, the ability of light to penetrate through the water column. TSS information is presented and discussed in the Staff Report for perspective, in part because articles in the scientific literature addressing low-level turbidity effects on aquatic organisms are not as abundant as articles addressing suspended solids. The concepts presented above also are presented in the Staff Report so that the reader can define the relevance of the available TSS and turbidity literature to the proposed turbidity objective. To address the reviewer's comment, more recent literature, such as Newcombe, 2003, which specifically study low-level turbidity, will be included in the Staff Report and supporting documentation.

Master Comment 5: Inferences drawn from the Newcombe and Jensen (1996) article indicate that there could be sublethal effects and was not discussed in the Staff Report or supporting documentation.

Response: Newcombe and Jensen's (1996) meta-analysis was developed using the results of many decades worth of studies that examined the effects of suspended sediment concentrations on aquatic life, all of which examined TSS values that would result in turbidity values exceeding the range of values under consideration for this turbidity amendment. In addition, the Newcombe and Jensen's (1996) model was extrapolated out to values greater and lesser than the empirical values used to develop their model; however, the authors acknowledged the lower reliability associated with the extrapolated TSS data. The reliability of their extrapolated data is further compromised by the poor correlation between turbidity and suspended sediment concentrations, as used in development of their model. Several authors (e.g., Ziegler 2002; Waters 1995; Bash et al. 2001; Henley et al. 2000) of studies that we examined (see the response to master comment #4) emphasized the difficulty (due to high variability in the TSS-turbidity relationship over time and across sites) in using turbidity as a surrogate for suspended sediment concentration, or visa versa. This is especially true at low suspended sediment concentrations, where the TSS and turbidity are most weakly correlated. Accordingly, the use of the Newcombe and Jensen (1996) model, particularly extrapolated low TSS aspects of the model, to estimate sub-lethal and/or long-term effects of turbidity in the 0-2 NTU range on aquatic life would be technically inappropriate.

C.P. Newcombe's subsequent work (Newcombe 2003) focused more on effects of low-level turbidity increases on clear water fishes. This work predicts essentially no effect on clear water fishes from a turbidity increase of 2 NTU. The proposed amendment would allow a maximum turbidity of 2 NTU under clear water conditions (i.e., when background turbidity is <1.0 NTU), which is less than a 2 NTU increase above background that was assessed by Newcombe (2003) and his conclusion that 3 NTUs is protective of clear water fishes for long-term exposures. As such, the body of scientific work compiled and evaluated by C.P. Newcombe indicates that the proposed turbidity amendment would be protective of clear water fishes. Appropriate edits will be made to the Staff Report and the supporting documentation to clarify staff's findings.

C. Science in Support of Proposed pH Amendment

No additional "Master Comments" were identified within this subsection of the peer reviewer's comments. Responses to comments made in this category are provided under Master Responses listed above and via additional specific responses to comments provided below.

II. Specific Responses to Numbered Comments (see Attachment 1)

Comment #1: The Staff Report does not provide an adequate explanation for expanding the applicability of the amendment to all water bodies rather than limiting the amendments to low flow streams.

Response: See response to Master Comment #1.

Comment #2: The Staff Report has chosen Alternative 3 as the preferred alternative for pH, in part because it "…reflects current Federal criteria." However, Alternative 3 will not have the same pH range as the Federal criteria.

Response: See response to Master Comment #2.

Comment #3: The Staff Report does not adequately justify not requiring averaging periods. In addition, it is unclear whether dischargers are subject to enforcement during the time period that they are gathering data to justify an averaging period.

Response: See response to Master Comment #2.

In addition, the reviewer posed the question: "…*have dischargers experienced enforcement actions for current and past violations?*" Yes, the compliance issues associated with the current pH and turbidity objectives have resulted in Cease & Desist Orders being issued to dischargers, and third party lawsuits for exceedances of the current pH and 0-5 NTU objectives. Due to these realities, resolving such regulatory compliance issues (by first improving the scientific basis for the objectives to assure protection of beneficial uses) is among the objectives for developing and processing these amendments.

Comment #4: The review of literature on sediments and turbidity provided in the first Technical Memorandum is truncated in time and limited in extent. References not cited that should be include work by T.F. Waters, 1995, Sediment in streams: Sources, biological effects and control (Amer. Fish. Soc., Mono. 7).

Response: See response to Master Comment #3.

In addition to reviewing the Waters Monograph cited by the reviewer, an additional literature search was performed to obtain and review additional scientific articles on the effects of low-level turbidity on aquatic life. Findings from articles obtained from this search that address turbidity effects on primary production are discussed under specific comment #12 (below). Findings from articles addressing the effects of turbidity on macroinvertebrates and fish are discussed under Master Response #3.

Comment #5: Inferences drawn from the Newcombe and Jensen (1996) article found that there is a lack of studies at the lower ranges and shorter durations of turbidity but goes ahead and discusses ill-effects, not just mortality as the staff report implies. The Staff Report needs to consider sublethal as well as lethal effects.

Response: See response to Master Comment #5.

Comment #6: Use of Newcombe and Jensen (1996) relationships to predict long-duration turbidity effects.

Response: See response to Master Comment #5.

As stated under Master Response #5, the Newcombe and Jensen's (1996) model was extrapolated out to values lower than the empirical values used to develop the model; which results is questionable utility in this range as acknowledged by the authors. The reliability of their extrapolated data is further compromised by the poor correlation between turbidity and suspended sediment concentrations, as used in development of their model, particularly at low suspended sediment concentrations, where TSS and turbidity are weakly correlated. Accordingly, the use of the Newcombe and Jensen (1996) model, to estimate sub-lethal and/or long-term effects of turbidity in the 0-2 NTU range on aquatic life is simply scientifically weak and inappropriate. This is, in part, why C.P. Newcombe turned his future research (Newcombe 2003) to look specifically at the effects of low levels of turbidity on clear water fishes – an area not adequately addressed by the Newcombe and Jensen (1996) article. Findings from Newcombe (2003) indicate that the increase in turbidity allowed under the proposed amendment would have no adverse effects on clear water fishes.

Comment #7: The turbidity Technical Memorandum has over-stated a conclusion about direct injury to fully developed fish by non-toxic suspended matter.

Response: See response to Master Comment #3, #4, and #5.

To assure no over-statement is made, the quote cited by the reviewer will be modified in the revised Technical Memorandum to read as follows:

> "Direct injury to fully developed fish by nontoxic suspended matter has been demonstrated in numerous studies, but only at concentrations that are much higher than the concentrations that would exist in water bodies characterized by turbidity levels of 2 NTUs or less~~common in both natural and polluted waters~~."

Staff disagree that citations to older literature (e.g., EIFAC 1965) are "at odds" with recent summaries of effects of suspended sediment on aquatic life. Part of the reason for citing and discussing the older literature is to lay out, in the Staff Report, the full breadth of scientific thought on the topic – not just that contained within the most recent article(s). This provides a true summary of the science from which better scientific inferences and decisions can be made. It is recognized that the science on any given topic advances over time, which is shown in the Staff Report and it's supporting documents, and will be better shown based on revisions made in response to this comment. Most older literature discussed suspended solids rather than turbidity, which is the subject of the proposed Basin Plan amendment. Recent summaries of low-level turbidity effects on aquatic life (e.g., Newcombe 2003, Rosetta 2005) show that the proposed turbidity amendment is highly protective of aquatic life.

Comment #8: Inferences from the Newcombe and Jensen (1996) article indicates that there may be sublethal and lethal effects at the turbidity levels expected from wastewater treatment plan discharges.

Response: See response to Master Comment #3, #4, and #5, and response to specific comment #12.

Comment #9: A more up-to-date review of the literature that may build on the lethal and sublethal effects inferred from Newcombe and Jensen (1996) and other studies is warranted.

Response: See response to Master Comment #3 and #5, and response to specific comment #12.

Comment #10: The methodology for use of a Secchi disk is appearance and disappearance of the disk. There is no methodology that discusses the use of what was described in the Staff Report and supporting documentation, which was the appearance of the disk at a certain depth by observing fogginess or graying. For the applications described in the Staff Report and supporting documentation, it would be more appropriate to do horizontal observations.

Response: The purpose of the visual aesthetic characterization performed using a secchi disk was to document the visual aesthetic quality of water having turbidity of 0.83 NTU and 1.7 NTUs (i.e., turbidity in the 1-2 NTU range) and to indicate that the difference is indistinguishable with the naked eye. Staff agrees that the secchi disk was not used to evaluate light attenuation, per se. The supporting documentation will be revised to clearly state its intent and to clarify that the secchi disk is being used for convenience, to evaluate people's ability to detect low-level changes in turbidity, rather than for its typical use. Use of a secchi disk in a horizontal fashion would be appropriate if the intent was to address how low-level turbidity changes affect organisms' ability to see through the water column (under water), which was not the intent of this work. The intent of this work was to evaluate whether people looking into a water body such as a creek or stream would detect significant degradation in visual aesthetics of the water at various turbidity levels below 2 NTUs. For this purpose, the approach utilized worked well.

Comment #11: Use of the vertical secchi disk was inappropriate for these circumstances. A more appropriate methodology would be use of the horizontal secchi disk.

Response: See response to Comment #10.

Comment #12: The effects of organic carbon and suspended sediments on light transmission that, in turn, may affect aquatic plants and the productivity of other organisms have not been addressed.

Response: In streams with a background turbidity <1.0 NTU, an increase to 2 NTU will not adversely affect primary production because light will continue to penetrate to the channel bottom.

Lloyd et al. (1987) studied the effects of turbidity on light penetration and primary productivity in Alaska streams and lakes. These researchers evaluated the relationship between the 1% light depth (i.e., depth to which 1% of available subsurface light penetrates) and turbidity by measurements made in 14 lakes. The data evaluated showed that the 1% light depth varied little among four lakes having turbidities below 2 NTUs, but showed a notable decrease between turbidities >2 to 10 NTUs. Based on their study findings, these authors concluded that a high level and moderate level of protection to aquatic ecology, based on effects on primary production, would be provided for streams and lakes, respectively, by limiting turbidity increases to 5 NTUs above natural conditions. Relative to lakes, streams show lesser effects on primary production due to increases in turbidity because of their shallower depths (where light often reaches the channel bottom) and a lesser reliance of the invertebrate community on phytoplankton production.

Findings and conclusions from Lloyd et al. (1987) suggest that the proposed amendment, which limits turbidity increases to 2 NTU for waters having background turbidity <1.0 NTU provides a high level of protection. The additional scientific literature information obtained from Lloyd et al. (1987), discussed above, will be added to the Staff Report and supporting documents.

Comment #13: The Staff Report does not provide a good explanation for expanding the amendment beyond low-flow streams. In addition, there is no evidence provided that only minor changes in pH will occur with the amendment to the water quality pH objective.

Response: See response to Master Comment #1and Master Comment #2.

As discussed in Master Comment #1 and Master Comment #2, neither major nor minor changes in pH cause adverse impacts as long as the pH remains in an acceptable range (6.5 to 8.5).

Comment #14: Comment suggests that, under the proposed pH objective, a stream with a predominant pH of 8.5 could be changed to a predominant pH of 6.5, which could change the water body's aquatic communities.

Response: Criteria documents do not indicate any detrimental effects in the pH range from 6.5 to 8.5. Although aquatic organisms do vary in their respective pH preference by species, and even by life stage, the body of scientific literature indicates that virtually all aquatic life can exist suitably at pH levels between 6.5 and 8.5 (see NTAC (1968) ("Green Book"), NAS (1972) ("Blue Book"), USEPA (1976) ("Red Book"), USEPA (1986) ("Gold Book"), and USEPA (2002) (Current EPA recommended water quality criteria)). The exceptions to this generalization would be unique organisms that require highly alkaline or acidic conditions for survival. These, however, are the exceptions, not the norm, and are handled with site-specific objectives in the Basin Plan, such as for Goose Lake. See also pH comment by second peer reviewer, Dr. Peter Moyle, below.

Comment #15: Natural pH fluctuations are generally associated with specific conditions, typically high nutrient levels, abundant aquatic plants, and low alkalinity. It is overextrapolation to conclude that if under some set of conditions pH can fluctuate, then it is acceptable if the same range of pH change occurs elsewhere. An example of possible concern is that continuous discharges of hydrogen ions could consume alkalinity and eventually lead to the condition downstream where added daily fluctuations in pH from photosynthesis and respiration would lead to water quality concerns.

Response: pH fluctuations, whether naturally occurring or anthropogenic, are basically caused by changes in the free hydrogen ion concentration in the water. As the review of scientific literature indicates (see Master Comments #1 and #2), changes in pH while remaining within an acceptable range do not adversely impact beneficial uses so whether or not the source of the pH fluctuations is natural or not, the pH change itself is not expected to impact beneficial uses. The source of the pH changes most likely has its own effect on water quality. Factors affecting water quality are regulated in a manner such that they meet adopted water quality criteria/objectives not just at the point of discharge into receiving waters, but also at all locations downstream. Regulated discharges must comply with all water quality objectives and not just the pH objectives. If a discharge met the pH objective at the point of discharge, but could be shown to cause or significantly contribute to an exceedance of the objective at some downstream location, then permit limitations for that discharger would be adjusted to assure compliance with the objective at all downstream locations. Direct, water body specific measures taken to assure that the adopted objective is met occurs through permits issued that control downstream water quality effects of a given factor (e.g., a NPDES permit for a particular WWTP discharge).

Comment #16: Comment addresses the potential fate of metals adsorbed to sediments accumulated downstream of a wastewater treatment plant, and states that arguments made in Section 5.2 of the Technical Memorandum (RBI, et al 2004) are illogical.

Response: The referenced technical memorandum goes beyond the scope of this project and was inadvertently left in the staff report and the supporting documents. References to this document will be removed.

Comment #17: This comment makes several points regarding the "Technical Memorandum on pH and metals toxicity, RBI, May 2004"

Response: The reference to this technical memorandum was inadvertently left in the staff report and the technical memorandum was inadvertently included in the peer review materials. The Biotic Ligand Model goes beyond the scope of this project. Evaluating the effects of individual metal constituents and the additive effects of metals are beyond the scope of this project. Current state regulations already account for pH effects on metals toxicity.

Comment #18: Comment suggests collection of field data could further clarify conclusions made in the trace metals Technical Memorandum based on use of the Biotic Ligand Model.

Response: Staff agrees that field data to confirm model assumptions and results are always useful. The BLM, throughout its development, has been extensively field calibrated and verified. In this case, inclusion of this Technical Memorandum was inadvertent, and goes beyond the scope of this project. Current state regulations already account for pH effects on metals toxicity.

Comment #19: Comment states that focus for pH amendment is on effluent dominated water bodies and that there is insufficient documentation and rationale for extension of the amendment to all water bodies within the Region.

Response: See pH component of response to Master Comment #1.

Comment #20: Comment states that part of the justification for the pH amendment is that distance and duration of discharge-related changes are short. As such, staff may wish to specify the duration of exposure.

Response: The remarks on distance and duration relate to the expected impacts on low flowing streams. These remarks have been revised. The scientific literature does not specify that pH changes within the acceptable range, regardless of distance or duration, cause adverse impacts. Therefore, staff does not expect beneficial uses to be affected by the proposed amendments to the pH and turbidity water quality objectives.

II. RESPONSES TO DR. MOYLE'S COMMENTS

[See following page]

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ATTACHMENT 1

Peer Review Comments by Dr. Don C. Erman
Professor Emeritus, University of California, Davis

43200 E. Oakside Place
Davis, CA 95616
March 7, 2005

Richard McHenry
Senior Engineer
Central Valley Regional Water Quality Control Board
11020 Sun Center Drive #200
Rancho Cordova, CA 95670-6114

Dear Mr. McHenry:

I have completed my review of the proposed Central Valley Regional Board Basin Plan Amendments for pH and turbidity. Attached please find my review (Word 98), including two figures embedded in the text. Let me know if you have any difficulty opening the attachment or figures.

I have sent a copy of my review to Dr. Jenkins.

Sincerely,

Don C. Erman
Professor Emeritus

A Review of

"Amendments to the water quality control plan for the Sacramento River and San Joaquin River Basins

for

pH and turbidity"

and

associated supporting documents

by

Don C. Erman
Professor Emeritus
University of California, Davis

March 7, 2005

My comments are divided into three sections: those that pertain to the scope of the proposed revisions of the Basin Plan, those that apply to the science supporting the changes in turbidity objectives, and those that apply to pH objectives.

Scope of Revisions to the Basin Plan

The supporting documentation, review of literature, rationales, logical deductions and examples are tied closely to the specific situation of effluent dominated water bodies. However, the proposed amendments apparently will be applicable throughout the basins (with specific exceptions) and will apply to all water bodies regardless of the condition of "effluent dominated." The scientific justification, to water bodies not effluent dominated, for this broader application of the change in objectives is not presented in any of the documentation.

The proposed amendments are also tied to the need to address regulatory issues associated with "low flow streams." Thus, effluent dominated water bodies and low flow streams are connected as reasons for the change in objectives. Yet, again, the proposed amendments are apparently for all water bodies (with specific exceptions) in the basins. The Draft Staff Report Functional Equivalent Document has expanded the apparent need for the project (p. 9-4) to all municipal and industrial discharges into low flow streams. In fact, any project that may require a NPDES permit would be affected by the proposed
1 amendments or, I presume, any other activity regulated by these objectives. There is no discussion, review of conditions or rationale for this expansion in the scope of the proposed amendments. For example, in the supporting document on influences of pH on metals toxicity, the authors have made extensive use of the Biotic Ligand Model (BLM) to estimate potential toxicity of waste water treatment plants (WWTP). In order to use the model, one must supply data on several water quality constituents, including the crucial variable of dissolved organic carbon inherent in WWTP discharges. The resulting model outputs are entirely dependent on the parameters associated with the selected WWTPs. No discussion is provided for a wider application of the BLM to all projects which may require NPDES permits. The Environmental Impacts Review section of the Staff Report (section 9) also places all discussion of impacts in the context of wastewater treatment plants.

Furthermore, "low flow streams" are not defined in any document. The streams of the basin with a few exceptions of springflow dominated systems all demonstrate a seasonal flow regime characteristic of Mediterranean climate or snowpack release. Relatively unmodified flow regimes would all have major reductions in flow in summer. By extending the proposed amended objectives to all streams and leaving "low flow streams" undefined, the Staff Report opens a door to future regulatory conflict and confusion.

Other Conclusions in the Staff Report

The Staff Report has chosen Alternative 3 (modify current objectives) as a preferred alternative. In part of the logic for this choice, the Staff has argued (p. 9-26) that Alternative 3 has the advantage, among other reasons, because it reflects "current Federal criteria." However, as noted on the same page, Alternative 3 will not have the same range of pH (i.e., 6.5 to 8.5) as the Federal criteria (i.e., 6.5 to 9.0). Thus, the Staff appears to conclude that a change of 0.5
2 pH units is not consistent with Federal criteria but a lower 0.5 pH units in the upper range is consistent. I think the State has many situations in which it selects criteria that may be different from Federal criteria. And, one must assume, the US EPA has approved of such differences between California and federal regulations, just as it has approved of different criteria proposed by other states. The staff may wish to reexamine the justifications for alternatives.

A main argument for not selecting Alternative 1 (and Option 4, Use of Averaging Periods, viz. p. 9-26) for retaining existing basin objectives is that some (percentage undefined) wastewater treatment plants do not have sufficient data to develop suitable averaging periods. First, it seems a weak argument for a state regulatory agency to accept the notion that an applicant for a permit or the agency has no obligation to collect necessary data to enable reasonable regulations to be made. Site specific objectives or suitable averaging periods have the advantage to both dischargers and enforcers that the regulations are specific to the situation and not general. The observation is made that with suitable averaging periods, a change in basin-wide objectives would not be necessary (p. 9-27). Many of the so-called scientifically based reasons for altering current pH or turbidity objectives rely on measurement variables and estimates of possible effects. Surely, the necessary information on stream conditions for averaging in these cases can only help the Board establish reasonable and protective regulations. To chose the preferred alternative (Alternative 3), a change in the objectives basin-wide for all situations because there are incomplete data for one class of discharger, has implications far beyond the discharger (or class of
3 dischargers) seeking relief.

The Staff Report has also argued that to gather necessary data to establish suitable averaging periods exposes dischargers to enforcement for violating existing limitations in their permits (p. 9-28 One is left to ask, have dischargers experienced enforcement actions for current and past violations? In not, then one must presume that either the Regional Board is selectively applying enforcement decisions, or that existing regulatory processes can deal with the situation while the discharger and Regional Board make good faith efforts to resolve the situation. The rationale here is weak and staff should reconsider the justifications for rejection and selection of alternatives.

Scientific Basis for Change in Turbidity Objectives

I will discuss separately the two supporting documents; one a review of scientific literature of effects of turbidity and the other a report of tests conducted to evaluate visual aesthetic characterization of low turbidity waters.

4 The review of literature on sediments and turbidity provided in the first Technical Memorandum is truncated in time and limited in extent. This general subject area has been of great interest in aquatic ecology and fisheries, and the literature is vast. I can understand how daunting a task it may be to succinctly summarize the literature. Nevertheless, the review has failed to consider much recent work including key reviews. Lacking in the report, for example, is the comprehensive work by T. F. Waters, 1995, Sediment in streams: Sources, biological effects and control. (Amer. Fish. Soc., Mono. 7). A graph of the papers cited for justification of the change in basin objectives shows the limited coverage of recent research (Fig. 1).

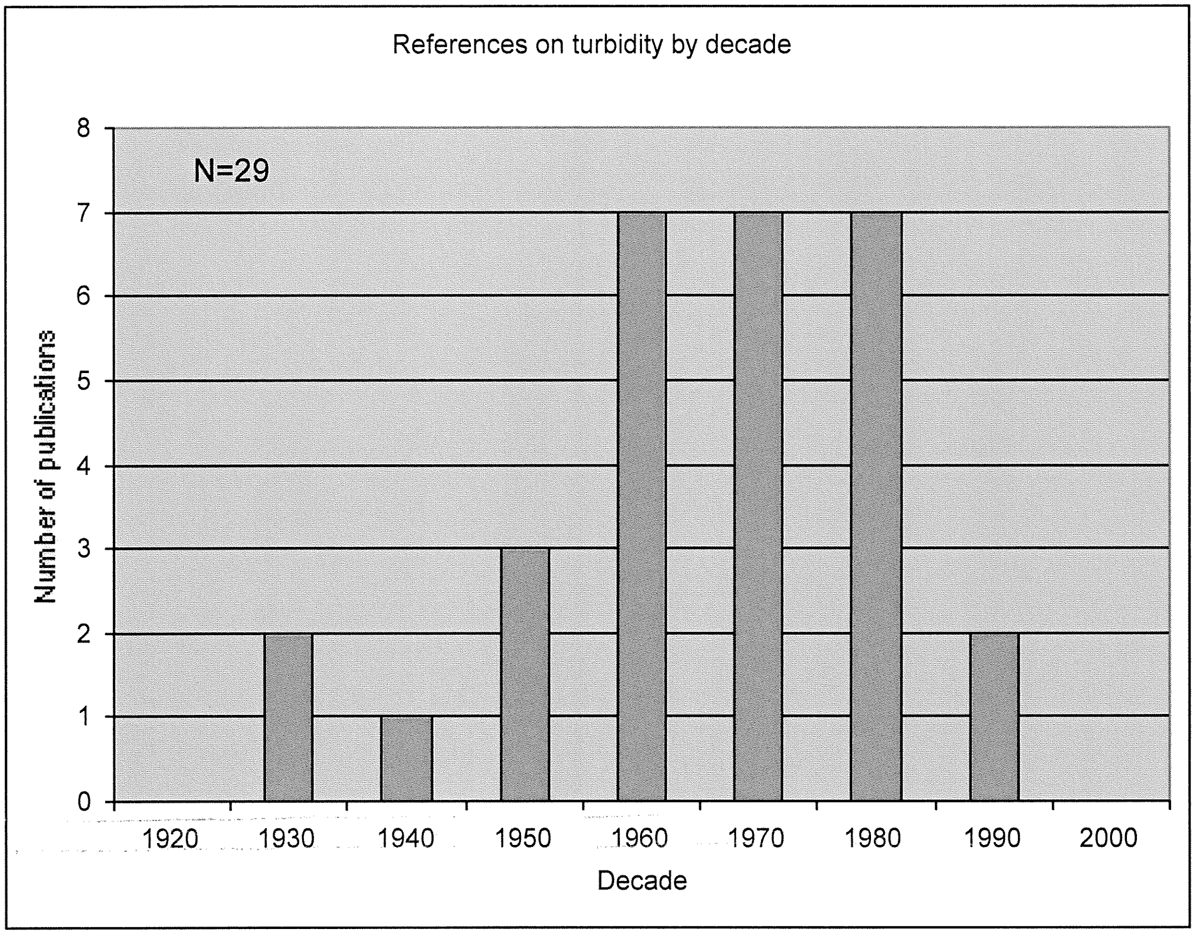

Figure 1. Frequency distribution by decade of research publications cited in the Technical Memorandum on turbidity, May 2004.

One consquence of such a skewed use of the literature is to suggest selectivity in the references cited as well as a lack of familiarity with the subject. As presently summarized, the science presented in the Technical Memorandum leaves many questions and is inadequate to form complete judgement on a change in basin objectives.

As troubling as the coverage of literature is the use of some of the articles reviewed in the Memorandum. In particular, the most recent publication cited in the Memorandum (Newcombe and Jensen 1996) is an important meta-analysis of 80 articles on the effects of suspended sediment on fish. The Memorandum has cited this work for a statement that the authors found suspended sediment of 20-

45 mg/L for 4 months would be required for fish mortality. The Memorandum further seems to emphasize that effects short of acutely lethal are inconsequential. First, such a review would have directed readers to more recent literature, including important related research papers by Newcombe (2003, Impact assessment model for clear water fishes exposed to excessively cludy water. J. Amer. Wat. Res. Asso. 39:529-544.) on the subject at hand. Second, the Memorandum has made little use of the important analysis by Newcombe and Jensen (1996) except to infer no significant effects count except mortality, and that there were no important responses in the range of turbidity and TSS under consideration.

The analysis by Newcombe and Jensen (1996) summarized the various studies in terms of "average severity of ill effects" as a score. The scores and associated fish responses are informative in themselves for the decision on a change in turbidity. The created a 14 class score in which they identified Nil Effects (score = 0), Behavioral Effects (1-3), Sublethal Effects (4-8), Lethal and Paralethal Effects (9-14). They organized the studies in terms of dose-response from "264 data triplets consisting of (I) suspended sediment concentration, (ii) duration of exposure, and (iii) severity of ill effect for fishes" (Newcombe and Jensen 1996).

5 (cont.) In brief, the article found that the data from the literature is missing in studies for low concentration and short duration exposure to suspended sediment. Nevertheless, the authors found good association between the classes of ill effects and dose-response for wide regions of concentration and duration—sufficient to establish an exponential equation for predicting an ill-effect score for concentration-exposures not empirically available. These predictions allowed estimates for "missing" data within the range of empirical data (interpolation) as well as outside the range (extrapolation). The authors recognized the more limited reliability of extrapolation. (Note: the problem of estimating effects at the low end of exposure ranges is common to many suspected toxic effects, and historically was a major impediment to accepting low dose-response thresholds for radioactive elements, pesticides and other substances.) However, in the case of a proposed change in the Regional Board's basin plan, I suggest it may be a better approach to make use of the Newcombe and Jensen (1996) paper to estimate potential effects than to take a (very limited) qualitative review of selected literature to inform decision-makers.

6 I will show below one outcome of the relationships established from the review by Newcombe and Jensen (1996). The plot is from their calculated effects for junvenile and adult salmonids based on the literature. I have only plotted the results for the lowest three levels of suspended sediment (1, 3, and 7 mg/L) and

exposure periods up to 4 months (2880 hours). Their full report should be consulted for other combinations of concentration, exposure and fish groups.

My plot (Fig. 2) of their data shows the exponential nature of the dose-response relationship. The sharp decline in ill-effects score with time is the more "conservative" model for estimating effects at the low end of dose-response curves. But the nature of the curve was developed from the data in other parts of the range. It also illustrates that even the lowest concentration (1 mg/L, TSS) and exposure (1 hour) results in an ill effect category 1 of "alarm reaction" behavioral response. After 4 months of exposure, the relationship predicts sublethal effects of 1 mg/L, TSS including reduced feeding and moderate physiological stress in salmonids (score 6, 2880 hours).

6 (cont).

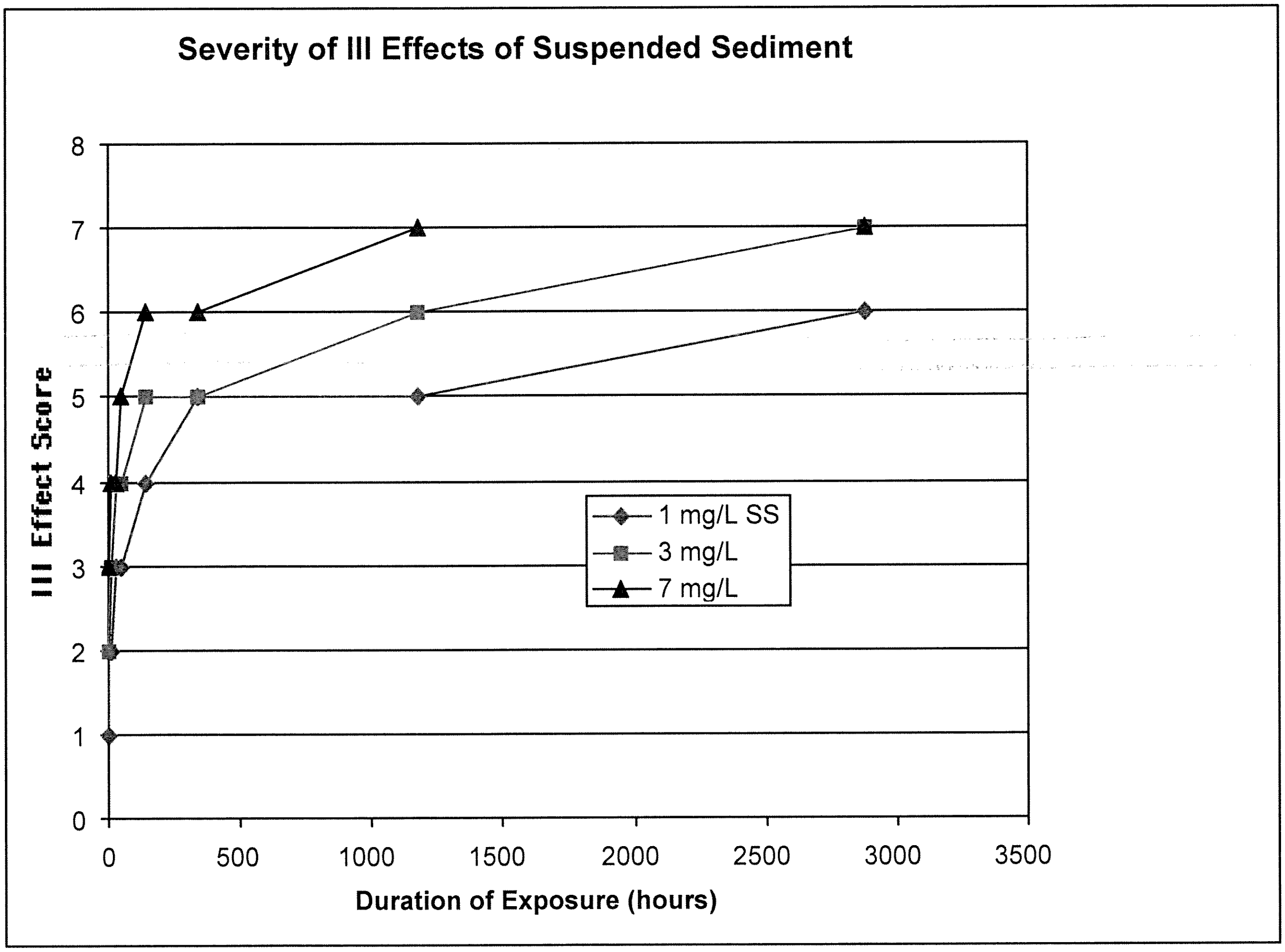

Figure 2. Predicted severity of ill effects from suspended sediment at various exposures on juvenile and adult salmonids. Selected data are used from the analysis of literature by Newcombe and Jensen (1996).

6 (cont.)

If the proposed change in regulations results in potentially longer times of exposure than 4 months (2880 hours) then higher sub-lethal effects scores are predicted. For example, at 3 mg/L suspended sediment and 4 months of exposure, the model predicts "moderate habitat degradation and impaired homing" (score 7). And at 7 mg/L for 30 months, a Lethal and Paralethal Effect (score 9, reduced growth rate; delayed hatching; reduced fish density) is predicted. 7 Examination of the full paper (Newcombe and Jensen 1996) also illustrates that the Technical Memorandum (May 2004) has over-stated a conclusion: "Direct injury to fully developed fish by nontoxic suspended matter has been demonstrated in numerous studies only at concentrations that are uncommon in both natural and polluted waters." Citations to other, older literature (e.g., EIFAC 1965) by the Technical Memorandum is likewise at odds with recent summaries of effects of suspended sediment.

8 Because the units of concentration in the paper by Newcombe and Jensen (1996) are as suspended sediment (mg/L) and the likely values for turbidity may be twice as small in nephelometric units (NTUs), the Regional Board staff has a more complete basis for evaluating the consequences of changing the current objectives for turbidity, at least with respect to fishes. Based on this recent summary and model of effects, it seems clear that the change could result in something more than "nil effects" and possibly lethal to paralethal effects near the boundaries of concentration and exposure expected by some wastewater treatment plants. The complete article also includes confidence intervals for predicted scores; and for these and other details, the Board staff should consult Newcombe and Jensen (1996). 9 A more up-to-date review of the literature that may build on Newcombe and Jensen and other studies is also warrented. The Board staff should also be explicit as to what constitutes "harm". An implication from the supporting documents is that effects short of acutely lethal are of little concern in regulating discharges.

10

<u>Visual aesthetic characterization of low turbidity waters</u>

The Technical Memorandum on this topic contributes little to the understanding of the topic. The data are unreplicated and incompletely explained or discussed. The secchi disk has been used for some time to evaluate water transparency and the limitations and restrictions are well known. I know of no studies that have used "graying effect" as part of a secchi disk methodology.

The primary use of a secchi disk is to obtain total depth to reach visible light extinction (i.e., secchi depth transparency). The value for distance or

10 (cont) "quality" is a single unit of depth (technically, the average of appearance and disappearance). There are recent studies that have evaluated the use of secchi disk (all white and black-white quadrants) and other conventional methods for horizontal visibility distance in flowing water (with shallower vertical depths than most lakes). The results (total secchi distance) are consistent and repeatable and among methods and are highly correlated. A more appropriate use of a secchi disk for evaluation of low turbidity waters would be horizontal measurements at varying TSS. However, even in this case, much limnological research has found that the secchi disk may be unreliable at distinguishing the effects of light attenuation at the lower light levels near 1-5 % transmission.

11 The primary question of aesthetics would be if a reduction in secchi disk depth (distance) caused by low levels of turbidity are detectable by a casual observer. For people swimming in a stream, the question may be whether they notice that visibility is impaired when they look around underwater. Standard scientific practice might be to quantify such judgements by observing the horizontal distance (for streams) at which the observer could no longer detect a secchi disk.

12 <u>Additional comments on light transmission</u>

In the case of wastewater treatment plant effluents, the documentation repeats the finding that dissolved organic carbon (DOC) is a persistent and important component in the effluent. There is a substantial literature on the role of DOC on light transmission as well as the effect of suspended sediment. The effect of DOC in general is not only to substantially reduce the total transmission of light but also to selectively absorb certain wavelengths (whereas suspended sediment is typically non-selective). Therefore, when the Board staff evaluates the effects of increased suspended sediment in receiving waters, it should also consider the fact that the receiving water will have potentially much altered light conditions from the discharge of dissolved organic compounds compared to upstream. The combined effects on aquatic plant composition and productivity and other organisms and functions have not been considered in the proposed changes in the basin plan amendments.

13 Scientific basis for change in pH objectives

The Staff Report and related documentation support the conclusions that pH changes of 0.5 units from municipal effluent discharges into low-flow, effluent dominated streams will likely have no significant toxicity for biological resources. However, once again I am in doubt as to the basis for extending the change in basin plan objectives for all conditions and locations beyond the circumstances of the discharge class under consideration. The Staff Report and supporting documents have couched their analysis closely in terms of the conditions and experience from wastewater treatment plants and low-flow streams. For example, the summary conclusions (p. 9-18, Draft Basin Plan Amendments) imply that the new pH objectives "would typically result in minor 13 (cont.) changes to pH conditions in some reaches of some water bodies (primarily low flow streams), relative to existing conditions." (My emphasis.) Evidence for this conclusion comes from earlier analysis of the "rebound" in pH conditions downstream from municipal treatment plants and other factors. The supporting review of literature and Staff Report also have developed arguments for the new pH objectives as if it is understood that "change" implies it is rapid and short-term. (e.g., see p. 4-11). Professor Cooper in his response to the question on pH change was cautious in his conclusions and was responding to a question on "potential effects of rapid pH changes on benthic macroinvertebrates within the context of what can occur across the mixing zone associated with a municipal effluent discharge."(p. 9, Technical Memorandum, pH requirements of freshwater aquatic life.)

But, unless I misunderstand how the new objectives for pH would apply, the new objectives mean that any discharger or activity could change a stream with a predominant pH of 8.5, say, to a stream with a predominant pH of 6.5 regardless of downstream extent or duration. There is no analysis of whether such changes would have no effect on aquatic communities. There are observational data from the literature that aquatic communities under one 14 regime of pH are different from communities under another regime. There is much species data that indicate preferences for pH. Thus, one would predict that if the new objectives were carried to their allowable extreme, then very different communities of plants and animals could be created in an affected system.

The review of literature and conclusions therefrom implies that substantial daily, natural pH fluctuations are to be expected. Such observations of large daily pH changes have been observed but they are also associated with a number of specific conditions. Chief among these commonly are high nutrient levels, abundant aquatic plants (rooted and unicellular), and low alkalinity. It is 15 overextrapolation to conclude that if under some set of conditions pH can fluctuate, then it is acceptable if the same range of pH change occurs elsewhere. Under another set of conditions, one could argue that continuous discharge of hydrogen ions that "consumed" alkalinity could eventually lead to the condition far enough downstream where added daily fluctuations in pH from photosynthesis and respiration would lead to water quality concerns.

The Technical Memorandum on pH and metals toxicity seems to have mis-stated the implications of upstream to downstream transport of sediment

16

that may carry metals. First, the report discusses the potential for metals desorbing from sediments under certain pH conditions. Then it argues that sediments with adsorbed metals carried past the treatment plant would not be available. The logic for this conclusion is that transport occurs during the season
16(cont) of runoff while pH concerns occur during low-flow conditions at another season. Therefore, the potential problem is separated by time. It is well known that sediments are transported during higher flow and that they also are deposited and stored, including in areas below a treatment plant, all along a channel. It is plain that if metal desorption is an issue then storage of sediment below the treatment plant remains a consideration. The arguments against this position on p. 14 (section 5.2) of the report are illogical.

The Biotic Ligand Model (BLM) is a powerful and useful newer tool for incorporating site specific water quality characteristics and predicting likely toxic effects from trace metals (affected by pH). The coverage of literature and more up-to-date articles help support the conclusions reached. (I note, however, that Selenium, surely an important element for the state and region, was not discussed in the Technical Memorandum.) The Board staff may also wish to consult some recent articles related to the BLM that have examined the model. Although trace metals of potential toxicity may occur in multiples, the current BLM only estimates single parameters when there may be interactions among them. Some work has addressed this issue. Other papers have suggested weaknesses and corrections in the BLM for invertebrates (e.g., Niyogi, S. and CM Wood. 2004. Biotic ligand model, a flexible tool for developing site-specific water quality guidelines for metals. Env. Sci. Tech. 38:6177-6192. Playle, RC. 22004.
17 Using multiple metal-gill binding models and the toxic unit concept to help reconcile multiple-metal toxicity results. Aquatic Tox. 67:359-370. De Schamphelaere, KAC and CR Janssen. 2002. A biotic ligand model predicting acute copper toxicity for Daphnia magna: The effects of calcium, magnesium, sodium, potassium, and pH. Env. Sci. Tech. 36:48-54.) . Again, however, the supporting documentation and conclusions were placed in a specific context: the questions to be answered applied to "...specifically effluent dominated water bodies, where municipal effluent discharges typically reduce receiving water pH which may make trace metals more bioavailable." (p. 1, Technical Memorandum on pH and metals toxicity, RBI, May 2004). The basis for applying the conclusions to the entire basin and other conditions is inadequate.

<u>Conclusions and comments on pH</u>

There are several instances where questions (application of the BLM, distances downstream of potential concern and possible downstream metal
18 desorption) could be clarified by modest data collection. Whenever a model prediction or assumption can be confirmed with site-specific observations, the

18 (cont.) Regional Board should carefully consider obtaining the data. Such results can only reassure dischargers and the public that beneficial uses are being protected.

19 The overall context for evaluating changes to the existing Basin Plan objectives for pH is closely nested within the conditions of effluent dominated systems on low-flow streams (however they are defined). There is insufficient documentation or rationale for extension of the proposed changes to all water bodies and activities.

20 In the context of the discharge class, it appears that the Board staff may also wish to consider the importance of specifying the duration of exposure and possibly the downstream extent of changes. At least part of the basis for a change in objectives has been that distance and duration are quite short.

ATTACHMENT 2

Peer Review Comments by Dr. Peter B. Moyle
Professor in the Department of Wildlife, Fish, and Conservation Biology, University of California, Davis

UNIVERSITY OF CALIFORNIA, DAVIS

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ONE SHIELDS AVENUE
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December 27, 2005

Richard McHenry
Senior Engineer
California Regional Water Quality Control Board
Central Valley Region
11020 Sun Center Driver #200
Rancho Cordova CA 95670-6114

Re: review of pH and turbidity WQC plan

Dear Mr McHenry:

Attached is my review of the proposed amendments to the Water Quality Control Plan for the Sacramento and San Joaquin River basin for pH and turbidity. I also reviewed all the background documents sent to me in support of the amendments.

I have no conflict of interest in that I was not involved in developing the amendments nor do I have any economic interest in the outcome of the proposed board action.

Sincerely,

Peter B. Moyle
Professor

Review of amendments to the water quality control plan for the Sacramento and San Joaquin river basins for pH and turbidity.

Peter B. Moyle
27 December 2005

pH

The amendment basically proposes to maintain pH standards of the range of 6.5 to 8.5 while eliminating the requirement for keeping changes caused by discharges no more than 0.5 pH unit. This is a very reasonable amendment, which recognizes that pH values can naturally change substantially over the course of a 24 hr period and that most aquatic organisms live without stress at pH values ranging from 5.5 to 8.5 (often 9.0) pH units and can move within that range with very little physiological stress.

It was actually surprising to me that such a standard for unit change was in place. As the review by Robertson-Bryan indicates, the effects of pH on aquatic organisms have been well established for decades, with a large spate of studies in the 1960s and 1970s in relation to acid rain. While very high and very low pH can be detrimental to most fishes and other aquatic organisms, sudden change in pH within the 'normal' range is not. Even the 6.5 to 8.5 pH standard is quite conservative and therefore protective of aquatic organisms.

Turbidity

The amendment here basically proposes to allow very clear water (<1 NTU) receiving a discharge to have turbidity increased by 2 NTU rather than just 1 NTU. This seems reasonable to me because a 2 NTU change is likely to be barely within the detection errors of most instruments (including operator error), will have no effect by itself on fish or other aquatic organisms, be visually undetectable to most people, and be within the range of natural variation of turbidity in clear waters. As the Robertson-Bryan review (and other sources) indicates, turbidity by itself is mainly a problem at very high levels when it can serious impair visual cues of fish and invertebrates, depress primary production, and have other impacts. Usually the substances causing increased turbidity are more likely to be a problem than turbidity itself (e.g., dissolved toxins at low turbidities, sediment at high turbidities).