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December 2, 2007

BY ELECTRONIC MAIL
Dominic Gregorio
Ocean Unit Chief
Division of Water Quality
State Water Resources Control Board
1001 I Street
Sacramento, CA 95814

Re: Peer review of Sediment Quality Objectives for Enclosed Bays and Estuaries of California

Dear Mr. Gregorio:

I am writing in response to your request of November 9, 2007 to provide a peer review of the Sediment Quality Objectives and supporting documents that you transmitted to me. On the attached pages, I provide my comments on the Draft Staff Plan and Appendix A (the implementation language). Although I did not follow the exact format of the scientific issues that you listed in attachment A, I attempted to address all of the scientific issues that I felt qualified to assess.

If you have additional questions or would like to discuss my review, please do not hesitate to contact me.

Sincerely,

David L. Sedlak
Professor

cc: Professor David Jenkins, UC Berkeley
Chris Beegan, SWRQCB

General Comments:

The proposed Sediment Quality Objectives (SQOs) for Enclosed Bays and Estuaries of California relies upon multiple lines of evidence to assess sediment contamination and its potential to cause adverse impacts on benthic organisms. The goal of the SQOs is to provide tools that can be used to identify areas where regulatory or remedial actions are needed to alleviate risks associated with sediment-associated contaminants. As explained in the supporting documents, this approach was brought about by lawsuits and legislated changes in water quality regulations. The supporting documents describe a number of studies that were conducted to implement a SQO triad, in which three lines of evidence (i.e., chemical analyses, toxicity tests and benthic community analysis) are used to establish when sediment contamination is unacceptable. In some cases, dischargers will be required to support additional monitoring and to conduct studies to identify the causes of toxicity. While the research team has done a good job integrating different SQO approaches proposed by other researchers and regulatory agencies, I am uncomfortable with this application of SQOs and believe that it is a very inefficient way of achieving the goals of the RWQCB. It appears that the RWQCB has not fully considered the implications of adopting these regulations and that the science supporting the proposed approach is not well enough understood to support the stated objectives. While I recognize that regulatory decisions often must be made with an imperfect scientific understanding, I believe that the end result of adopting the proposed SQOs will be counterproductive, ultimately redirecting valuable resources to collect data that will not move the RWQCB closer to its objectives of protecting the aquatic environment by addressing pollutant sources that can be controlled in a cost-effective manner.

In my review I have attempted to identify areas where I believe that the approach is problematic.

Specific Comments:

1. I am troubled by the use of California Logistic Regression Models (CA LRMs) to set threshold levels for sediment-associated contaminants using the same type of toxicity data that are used later in the sediment toxicity tests. As I understand the approach, a large dataset of paired sediment toxicity tests and chemical analyses was used to calculate the CA LRMs that are then used to determine if the chemicals in the sediments are present in unacceptable concentrations (as determined by the observation of toxicity in benthic toxicity tests). These same kinds of toxicity tests are used later in the SQO process to establish if the sediments are toxic. From a scientific standpoint, it appears that these two legs of the SQO triad are not independent.

   One problem with this approach is that it is assumed that the chemicals measured in the sediments are responsible for the observed toxicity or that the concentrations of chemicals measured in the sediments will be correlated with those chemicals responsible for toxicity. These assumptions are never examined explicitly in the SQO development process. If the source of contamination is similar across the entire region (e.g., urban runoff and atmospheric deposition) then the approach is probably valid. However, if there are unique point sources of contamination in the training

data set or if the effects change as the relative mixtures of contaminants change, the CA LRM will not be scientifically valid.

2. On the basis of the scientific data presented in the supporting documents it appears that the contaminants hypothesized to be responsible for sediment toxicity are metals and PAHs. Aside from PCBs and some legacy pesticides, I could not find any evidence in the supporting documents that any other contaminants were suspected of causing benthic toxicity. From a scientific standpoint, I cannot understand why these contaminants cannot be addressed directly. The authors of the documents state that bioavailability and other complicating factors make it impossible to assess these issues with measurements of contaminant concentrations. I am not convinced that the complicating factors associated with benthic toxicity are more difficult to capture than those inherent in the soil, water and air pollution regulations that are the basis of almost every other environmental regulation. In response to the concerns associated with bioavailability and other factors, the authors of the SQO document have developed a complicated approach for identifying contaminated sediments that ultimately will require that the chemical or chemicals responsible for toxicity be identified prior to defining corrective actions. I believe that a less unwieldy approach could have been developed if the same amount of effort was directed at resolving the bioavailability issues. For example, there is a strong scientific foundation for explaining metal-related sediment toxicity by using the ratio of simultaneously extracted metals (SEM) to acid volatile sulfides (AVS). The authors of the document dismiss this approach because SEM and AVS data were unavailable. If the goal of the study is to develop an approach for protecting sediments, it seems reasonable to advocate for the collection of relatively simple data that provide a mechanistic understanding of the factors affecting toxicity. Likewise, previous research has shown that the bioavailability of PAHs is affected by sediment organic matter content and the collection of data that can be used to predict bioavailability could yield an understanding that would support later regulatiry actions.

3. The application of the SQOs to NPDES permits (page 21 of Appendix A) illustrates the circular nature of the approach that is being proposed. If the RWQCB determines that SQOs are being caused by a NPDES discharge the situation can only be remediated after the pollutants responsible for the degradation are identified and their loading is quantified. It appears as if the large amount of data collected during the SQO will only begin a process of identifying the pollutants that need to be quantified through a TIE process. In essence, every time the SQOs identify sediment toxicity a research project will have to be conducted to identify the cause. Before the RWQCB adopts this policy, it would make sense to estimate the amount of effort required to achieve the stated goals. On the basis of the monitoring data included in the supporting information, I would guess that many locations in Southern and Northern California will be identified as toxic by the SQOs. Does the RWQCB have the resources needed to complete these analyses? If the causes of toxicity turn out to be the mixture of contaminants originating in urban non-point sources (e.g., metals and PAHs) is there a practical way that these can be controlled? How many years of research will be needed to reach a scientifically defensible conclusion? Why not

initiate monitoring of the compounds that are likely to be responsible and develop a scientific framework for evaluating the factors controlling their bioavailability (e.g., SEM/AVS for metals)?

4. Page 10: The authors state: "Consequently, the benthic community present at a site may be altered by a variety of factors in addition to adverse effects from contaminants." Although benthic ecology is outside of my area of expertise, it appears that this is the weakest part of the sediment triad. It is unclear to me that the authors of the report have established that benthic community structure has a meaningful relationship with chemical contamination because so many of the habitat variables that could alter benthic community structure co-vary with the contaminant sources (e.g., it seems like the physical conditions near where the Los Angeles River discharges will not be ideal for sensitive benthic communities, even if all of the contaminants are removed). I believe that more data are needed on the role of physical disturbance on benthic community structure before the problem of co-variance can be assessed.

5. Page 70: The basis for the low toxicity threshold is unclear. The moderate and high thresholds are related to statistical tests of the data while the low threshold appears to be based upon best professional judgment. The basis for defining the low threshold needs to be better documented and related to an actual adverse effect, in the same manner that is presented in the supporting information for the moderate and high thresholds. This kind of logic appears later in the document in the definition of effects categories (p. 85) as, "…a small response that might not be statistically significant from background conditions." From a scientific standpoint, the statistical test criteria can be relaxed if there is a reason to be concerned about drawing an incorrect conclusion (e.g., test for a 10% chance that the sample is different from background instead of a 5% chance) but taking an action on the basis of a difference that is not statistically significant does not seem to be scientifically valid.

6. In Figure 5.1, it appears that a test result in which the response is not significantly different from the control can be classified as exhibiting low toxicity. What is the scientific basis for concluding that a sample that is not different form control is impacted? A sample in which the response was large but not different form control would appear to indicate a problem with the test and not necessarily an indication of toxic sediments. I would expect this kind of finding to trigger a new analysis of the sample rather than a decision that the sediment is toxic.

7. In the Draft Staff Report the Figures are not referenced with the same convention as they are referred to in the text (e.g., Figure 1 in the text and Figure 2.1 in the captions).

8. Page 8: "Additional nonpoint contaminant sources include atmospheric deposition and groundwater. Most of the contaminant mass from all of these sources is associated with particles…" I would be surprised if groundwater that was not contaminated by a specific point source was a significant source of sediment

contamination. If it were, the contaminants would not be associated with particles in the groundwater.

9. Page 8: "Upon introduction, most contaminants not already associated with particulate matter (in the source) will associate with suspended particles in the water column." This statement is incorrect. With the exception of partitioning of organic contaminants from the gas phase into water, the most significant sources of sediment-associated contaminants will contain a higher concentration of particles than what they are likely to encounter in the water column. If anything, there is more of a potential for particle-associated contaminants to desorb back into the aqueous phase when they are discharged to surface waters.

10. Page 9: "…co-occurrence of binding constituents, such as sulfides…" The term "binding constituents" will not be familiar to environmental chemists or geochemists. I suggest that the authors rewrite this sentence to emphasize that the forms of metals in sediments will be controlled by sulfide at low redox potential and organic matter, metal oxides or clay minerals at higher redox potentials.

11. Page 28: "Sediment samples were collected only…"

12. Page 34, second paragraph under 4.3: "humanhealth" should read "human health".

13. Page 39, third paragroah under 4.4.2: "…guidelines used recently currently…" This sentence doesn't make sense.

14. Page 53: Near the bottom of the page, it is stated that, "The bioavailability of both hydrophobic and inorganic pollutants is strongly influenced by salinity." While there are some small effects on partitioning directly associated with salinity, the impact on hydrophobic partitioning and the formation of sulfide minerals from salinity alone is modest at best. This statement needs to be revised or better documented.

15. Page 93: The title to section 5.7 is unclear. Should it read, "Application of Proposed SQOs Within Specific Programs"?

16. Appendix A: Page 9: What is the scientific basis for using a different mesh screen in San Francisco Bay?

17. Appendix A, Tables 6 and 7: The compounds included in the list of high and low molecular weight PAHs should be included in a footnote.

18. Appendix A, page 21: Under section B how is the "discharge gradient" defined? Also, under B.2. shouldn't the wording include reference to the degree through which the NPDES discharge is contributing as in, "…discharge is causing or significantly contributing to the SQO exceedance…"