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TO: Mr. John H. Robertus, Executive Officer, San Diego RWQCB
FR: Patricia A. Holden, UCSB
DATE: March 27, 2005

Re: Review of "Bacteria Impaired Waters Total Maximum Daily Load Project 1 for Beaches and Creeks in the San Diego Region"

Following are my responses to the specific questions outlined in your February 7, 2005 letter requesting this review. Thank you for the opportunity to be of assistance.

Scientific Issues for which Scientific Peer Review is Requested:

1. Use of land use composition to quantify bacteria sources from all watersheds to affected beaches and creeks in the San Diego region.

In concept, this seems fine. However, as per the regression model on page H-6, not all land uses correlated with indicator bacteria discharge during dry weather. There were 13 land use categories overall, and eight are listed on page H-6. Perhaps comments are being requested for only the wet weather calculations (this review point only).

As for the wet weather usage, how current are land use data from 2000 (page I-4)? Has development in the region been so rapid as to make these land use data obsolete in some areas?

2. Use of wet weather model to simulate fate and transport of bacteria, and to calculate TMDLs, to affected beaches and creeks.

Few details about the model are provided, but the methods appear to be well-referenced. The model simulations (e.g. Figure N-3) of concentration appear to fit the real data well (where there are data). However, for some of the figures (e.g. N-1, N-2) it is not possible to tell how well the simulations worked because of the density of the simulated data.

3. Selection of a Los Angeles watershed as a "reference" for background loading of bacteria in the San Diego Region during wet weather.

In the absence of a sufficiently characterized "reference" (i.e. relatively undeveloped) watershed in the Sand Diego region, designating a nearby, well-characterized, similarly undeveloped watershed in the Los Angeles region as a "reference" watershed seems fine. However, the use of the "reference" watershed as a concept or decision tool is not clear. The document refers to a 22% exceedance frequency in the Arroyo Sequit Watershed (in Los Angeles) and this compares similarly to two undeveloped watersheds in San Diego (Tables 4-1 and 4-2, San Mateo Creek and San Onofre State Beach). However, on page 15 (Section 4.1) of the document it is stated there is no "reference watershed implementation policy" which seems to imply that the use of a "reference watershed" concept is not allowed. This is confusing and it is suggested that it be clarified by either moving this reference watershed discussion to a later point in the document (i.e. implementation) or more clearly stating how it is used at this point in the TMDL process.

The "reference" watershed concept inherently assumes that all indicator bacteria are created equal. That is, indicator organisms from an urbanized area are just as problematic as those from an undeveloped watershed. This may not be the case. If false positive results on indicator organism assays frequently occur at the outlets of undeveloped watersheds, this would imply that natural lands discharge bacteria but few pathogens. Transferring an allowable exceedance from an undeveloped watershed to a developed one may inadvertently "allow" the discharge of more pathogens from developed watersheds because it is more likely that microbes discharged from developed watersheds will include pathogens.

4. Use of single-sample maximum objectives for wet weather numeric targets.

The use of single sample maximum objectives for wet weather seems fine. However, given that rainfall events subject the watersheds to more variability in flow and load, the use of a geometric mean for wet weather seems more practical. This is discussed again for the dry weather assumptions.

5. Reasonableness of assumptions (described in Appendix J) for wet weather modeling.

In Section 8.1.1, it is stated that the "92nd percentile" was used as the critical condition for wet weather years. Other than SCCWRP used a 90th percentile previously, what is the scientific justification for this? Was 1993 an El Nino year? Is there an accepted process, similar to flood frequency estimations used in treatment facility designs, for selecting a storm frequency for this process?

6. Use of wet weather modeling parameters to simulate build-up/wash-off of bacteria from a similar study in Los Angeles (LARWQCB, 2002).

There is insufficient information in the report for this to be evaluated. The idea of simulating build up and wash off is logical and sound. But the modeling parameters are not detailed sufficiently for comment. The Santa Monica Bay TMDL used the same approach, but the report provided does not contain detailed information on the modeling.

7. Use of dry weather model to simulate fate and transport of bacteria, and to calculate TMDLs, to affected beaches and creeks.

The model on page H-3 is a simple first order decay model. The derivation of a correct and appropriate model based on mass balance principles, within the context of the assumption of a plug flow reactor, should be provided. Even if each reach is modeled as a complete mix reactor, the resultant equation will not be what is given on page H-3. It should also be stated that bacteria are assumed to be discrete particles that don't settle unless "die off" refers to the combined processes of settling of particle-associated bacteria and death.

The dry weather flow rate of 15 cfs is stated as an assumption (page H-4) but the justification is not provided.

The significances (p values) for regressions (beginning on page H-4) are important. If they are greater than 0.05 (assuming 95% confidence intervals for these estimates) then the use of the correlations should be further justified.

8. Use of data from Aliso, San Juan, Rose, and Tecolote Creeks to characterize dry weather source loading in the entire San Diego Region.

Again (as above), the significance (p value) of the derived correlation should be provided. Otherwise, it is hard to know that the equation is valid for predictions (page H-6). It is interesting, and somewhat curious, that the correlation is to so many factors (land uses and watershed size). How this analysis was performed would be important to convey in the document.

If the p value is high for the equation on page H-6, this would suggest that monitoring of the other watersheds should occur. Even if the p value is high, however, the lack of data would suggest that little knowledge exists regarding the need for TMDL extrapolation to the other watersheds, and that data should be collected to refine the process.

9. Use of geometric mean objectives for dry weather numeric targets.

The use of a geometric mean for dry weather numeric targets should be discussed in light of monitoring activities at beaches and how convenient this will be for making posting and closure decisions. A single sample-basis target is potentially more useful (for decision making) regarding beach closures. Also, dry weather conditions are likely to be less variable as compared to wet weather conditions.

10. Reasonableness of assumptions (described in Appendix J) for dry weather modeling.

The assumptions appear to be sound. As above, the plug flow modeling probably needs to be shown more completely and double checked. The multivariate regression analyses should be double checked for significance (p values) and significances reported.

11. Location of critical points for TMDL calculation.

The locations of critical points (mouths and bottom of creeks and watersheds) are reasonable for protecting beach water quality. The impact of the watershed at this point is fully integrated from up to downstream. However, where small estuaries or lagoons separate the creek mouth from the coastal ocean, they should also be considered in this process. Lagoons and estuaries can accumulate and discharge fecal coliform-laden sediments during low and high flow conditions, respectively.

12. Use of conservative assumptions to comprise an implicit Margin of Safety.

In this reviewer's mind, a "margin of safety" is an explicit add-on to a limit. It is really difficult to tell what are the "conservative assumptions". For example, in wet weather modeling, it might not be conservative to make the creek mouth the critical point if there is a lagoon or estuary. On the other hand, most of these discharges do not have lagoons or estuaries downstream of the creek mouth. In any event, the Assumptions in Appendix J don't explicitly describe the "implicit' conservative assumptions, and the only real text devoted to the margin of safety issue appears to be in Section 8.1.6 rather than in the modeling appendices (I and H). It would be worth while to add some text to the document that more explicitly outlines where the "implicit" margin of safety is built in to each model.

Overarching Questions:

1. Are there any other issues with the scientific basis of the proposed rule?

The mixed use of REC-1 and SHELL criteria for water quality targets at the same location may introduce some difficulty to water quality managers. The SHELL criteria are more stringent, so the mixed use of these results in a Total Coliform criteria that is lower than Fecal Coliform. Practically, this is difficult to achieve since Fecal Coliform are, in concept and practically, a subset of Total Coliform. How will TC levels ever be lower than FC levels at the same location? See Table 8-6 for the summary. It appears that this is only a problem at Beaches.

Section 10 on Implementation is nonexistent. The impression from the placeholder paragraph is that dischargers may amend the TMDLs and that the timescale for implementation is unknown. If more data are to be collected for more study of the watersheds, and the resulting impact is delayed or uncertain implementation, this would delay protection of the coastal water quality in the San Diego Region. Implementation measures are the translation of the science into effective water quality management. The degree to which the science can be implemented adds to its validity in the TMDL process. Therefore, an additional comment on this document is that the presentation of Implementation strategies and monitoring plans should be part of the TMDL document. One aspect of implementation will be flow measurement. As stated in Appendix H, few flow measures are available, yet to comply with the TMDLs these will have to be made.

2. Is the scientific portion of the proposed rule based upon sound scientific knowledge, methods and practice?

In Appendix E-1, a small editorial recommendation is to remove the word "species" from the first line of page E-1. This is because "total coliform" and "fecal coliform" are empirically-defined groups of bacteria and are not "species" per se. While many taxonomic groups make up the total and fecal coliform, these indicator organism classifications are not derived from any accepted taxonomy.

Overall, it is great to see the development of and use of simulation tools for modeling bacterial discharge under two seasonal regimes as the basis for TMDL development. However, as with all TMDLs, there is a need to demonstrate a relationship between indicator bacteria and threat to swimmers and fishers. Increasingly, DNA-based metrics of human-waste associated *Bacteriodes* or *Enterococcus* are used to make a more robust link between the presence of bacteria in coastal waters and the presence of human waste. Better yet, these methods are increasingly becoming quantitative with the availability of real-time or quantitative polymerase chain reaction (QPCR). At the time of this review, there is a reasonable amount of evidence in the peer-reviewed scientific literature that DNA-based markers of human waste can be used to more definitively understand the presence of human waste. At the very least, new TMDL programs, as part of the monitoring portion of implementation, should strive to gather a better understanding of the real presence of human waste using DNA-based evidence from sampling and analysis in conjunction of standard indicator organism assays.