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John H. Robertus, Executive Officer
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Mr. Robertus,

I have completed my peer review of "Bacteria-Impaired Waters TMDL Project I for Beaches and Creeks in the San Diego Region: Technical Draft" (**WQS: 79-0056.02:carias**). Please find my comments attached. If you have any further questions please do not hesitate to contact me.

Sincerely,

Kara L. Nelson
Assistant Professor
Civil and Environmental Engineering

Bacteria-Impaired Waters TMDL Project I for Beaches and Creeks in the San Diego Region: Technical Draft" (WQS: 79-0056.02:carias)

My overall assessment is that the approach used to determine interim TMDLs is technically sound, with the exception of the concerns raised below regarding the dry-weather model. I believe that implementation of the Interim TMDLs will result in a significant improvement in water quality, and is far preferable to postponing action until remaining sources of uncertainty can be addressed. However, there is an opportunity to learn more about the fundamental processes that contribute fecal indicator bacteria to the surface waters in the San Diego region through the monitoring that will be required to document compliance with Intermin (and Final) TMDLs. I strongly recommend that the Regional Board, in preparing the Implementation Plan, ensure that the monitoring data are collected in a manner that maximizes the amount of information that can be learned, including gaining more insight into the fundamental source, fate, and transport processes. My comments on the specific items I was asked to address are the following:

1. **Use of land use composition to quantify bacteria sources from all watersheds to affected beaches and creeks in the San Diego Region.**
   This is a reasonable approach.

2. **Use of wet weather model to simulate fate and transport of bacteria, and to calculate TMDLs, to affected beaches and creeks.**
   In general, the approach used for the wet weather model seems reasonable given the limited existing data. The method for calibrating and validating the model is presented well. Although the model results agreed fairly well with the observed concentration for the high flows (especially above 60% unit area flow, as reported in Appendix N Figures 12-25), at low flows the model often underestimated the concentrations. In the text on p. I-11 it is stated that these flows may be better modeled as dry flows. However, since the flow on these days was defined as a wet flow, it is not clear to me that these loadings are being appropriately incorporated into the TMDLs. It may be necessary to redefine the classification of wet flows. In addition, as the science describing the sources of fecal pollution and their transport mechanisms improves, the model will need to be improved and TMDLs reevaluated. For example, the resuspension and erosion of sediments in water channels during storm events may be an important source of indicator bacteria that is not accounted for in the current model.

   Specific comments on Appendix I:
   a. (p.I-4) Please provide a table of the percent (%) impervious for each land-use category.
   b. (p.I-6) I don't believe atmospheric deposition of fecal indicator bacteria is a potential source, unless you mean deposition from birds.
   c. (p.I-12) I would not characterize the model and observed data as "extremely" well. I would say "fairly" well.

   Additional comments on Appendix M:
   d. It is difficult to see the curves for the observed and modeled daily rainfall on the calibration and validation graphs because the peaks are so sharp and the lines so thin. Since this graph is the only one presented for the validation, I suggest changing it to monthly rainfall rather than daily rainfall (as was done for the calibration).

e. The legend for the validation curves is incorrect (states monthly instead of daily rainfall).

3. **Selection of a Los Angeles watershed as a "reference" for background loading of bacteria in the San Diego Region during wet weather.**
Given that sufficient data do not exist for a reference watershed in the San Diego region, it is reasonable to use a reference watershed in Los Angeles. However, the implementation plan should require that one or more appropriate reference watersheds are identified and characterized for the San Diego region, and that these data are used to determine the final TMDLs.

4. **Use of single-sample maximum objectives for wet weather numeric targets.**
The use of single-sample maximums for the wet weather targets is a reasonable approach.

5. **Reasonableness of assumptions (described in Appendix J) for wet weather modeling.**
The assumptions are reasonable, except please clarify that the first-order die-off rate is an "apparent" rate, not an actual rate.

6. **Use of wet weather modeling parameters to simulate build-up/wash-off of bacteria from a similar study in Los Angeles (LARWQCB, 2002).**
The use of data from L.A. is reasonable given that no local data exist. However, the starting values taken from the LARWQCB should be reported in Appendix I, or in a separate Appendix.

7. **Use of dry weather model to simulate fate and transport of bacteria, and to calculate TMDLs, to affected beaches and creeks.**
The assumption of plug-flow hydraulics to describe the creek flows, and the empirical approach used to model the bacterial concentrations appears to be an acceptable approach given the limited data that are available. However, I have some significant concerns about how the empirical relationships were developed. Appendix H is poorly written, and it is possible that most of my concerns could be addressed if the methods were explained more clearly and in more detail. My specific concerns are the following (many of these items are interrelated):
   a. Please number each of the equations.
   b. Please explain how the functional form (linear, exponential, etc.) and best fit (quantitative or qualitative?) for each of the equations in Appendix H was determined. In particular, how were the multiplication factors (constants) determined in the equations on p. H-5 and H-6? In the equation on p. H-6, why isn't A (total watershed area) multiplied by the rest of the equation? It seems to me that the fecal coliform concentration should increase or decrease proportionally (although not necessarily linearly) with the watershed area.
   c. How are infiltration and evaporation incorporated into the flow mass balance (equation at top of p.H-4)?
   d. (p.H-3) My understanding is that in the model for bacterial loading, the loading for the drainage area for each segment is added at the bottom of that segment (which is the top of the next segment). If this is the case, it is a conservative approach, because the decay of any bacteria that actually enter the watershed upstream of that point is not considered. This assumption should be discussed, and its contribution to the "Margin of Safety" should also be stated.
   e. I have some major concerns about how the empirical equations for the bacterial loadings and die-off rates were developed. It seems that first the Equation on p. H-6 was developed by

regression analysis. Then, using the same data set, die-off rates were incorporated and their values adjusted until the "best fit" was achieved between the modeled and observed (geometric mean) values at each sampling station. Thus, the die-off rates are just accounting for the inability of the regression equation to describe the observed data. If this is the case, the die-off rates are just fitting parameters but there is no reason to believe that what is being modeled is actually die-off. Furthermore, I do not understand how the die-off rates for total coliform bacteria and enterococci were determined independently from the multiplication ratios (on p.H-7), nor how the regression equations were evaluated for best fit. For example, in Figure H-11 the results are presented for the calibrated enterococci model, but the observed concentrations are significantly lower than the modeled concentrations. Thus, it does not seem that the model was calibrated correctly. In addition, it is not clear to me what parameter would be adjusted to achieve a better fit – increase the die-off rate, or decrease the multiplication factor?

f. Other limitations to the empirical approach are evidenced by the fact that equations relating total coliform bacteria and enterococcus concentrations to land use could not be developed. I expect that the use of multipliers to determine the concentrations of these indicators as a function of fecal coliform concentrations is a major source of error in the model, because different sources of fecal waste may have different ratios; furthermore, the rates of removal and inactivation in the environment may differ for the different bacteria. The variation in the fecal coliform:enterococci ratio is expected to be particularly large, since it is known to range from a ratio of less than one in human waste to greater than 40 in some animals wastes. Thus, although there was fairly good agreement for the creek segments used to validate the model, I expect these assumptions to introduce significant amount of error for other creek segments (those that were not used for model calibration.)

g. Some of my concerns with the empirical approach used to develop the equation on p.H-6 may be addressed if the explanation was better. Section H.4 needs significant improvement:
   i. In addition to the number of sampling stations for each Creek, please also report the number of samples for each station.
   ii. Clearly large data sets are better than small data sets, but was the number of samples at each station taken into account for the regression analysis? Was the data from some stations not used?
   iii. How is it known that 40 data points is enough to adequately represent the range of conditions at one sampling station?
   iv. Please explain exactly how the regression analysis was performed. How did the regression analysis of the data at each station result in the final equation?

8. **Use of data from Aliso, San Juan, Rose, and Tecolote Creeks to characterize dry weather source loading in the entire San Diego Region.**
It is difficult to assess whether these three creeks are representative of the rest of the watersheds in terms of runoff and bacterial densities. I suggest including a paragraph with a short description of these three watersheds and a discussion of how they compare to others. In the Implementation Plan, a strategy should be outlined for incorporating data from additional watersheds into the development of final TMDLs.

9. **Use of geometric mean objectives for dry weather numeric targets.**
The use of the geometric mean seems to be an appropriate water quality objective if the

assumption that dry weather concentrations are fairly constant is correct. However, if future monitoring efforts identify high episodic concentrations, this approach may need to be reevaluated because health impacts are likely to result from exposure to the high episodic concentrations, which may not be adequately represented (and therefore regulated) by geometric means.

10. Reasonableness of assumptions (described in Appendix J) for dry weather modeling.
Most of the assumptions are reasonable, except:

a. Please clarify that the first-order die-off rate is an "apparent" rate, not an actual rate. Also, I agree that given the lack of data on the occurrence of bacterial regrowth in the Southern California region, it is not possible include regrowth in the model for dry weather flows. However, regrowth has been demonstrated in tidally-influenced river sediments in Florida (e.g. Desmarais, T. R., Solo-Gabriele, H. M., and Palmer, C. J. 2002. "Influence of soil on fecal indicator organisms in a tidally influenced subtropical environment." *Applied and Environmental Microbiology*, 68(3), 1165-1172.) Thus, regrowth should be recognized as a potential source of error, and should regrowth be documented in the region in the future, it may need to be incorporated into the modeling framework.

b. There is a typographical error in the "regrowth" assumption – it says "wet" instead of "dry".

11. Location of *critical points* for TMDL calculation.
The location of the critical points is appropriate.

12. Use of conservative assumptions to comprise an implicit Margin of Safety.
The use of conservative assumptions rather than an explicit Margin of Safety is appropriate. Also see comment 7d above.

13. Several of the references to Appendices, Tables and Figures were incorrect, as documented below. (The entire document should be checked).

- (p.7) Reference to Appendix B is incorrect (should be Appendix F?)
- (p.H-2) Reference to Sections I.2.2. and I.2.3. incorrect?
- (p.H-13) Should be Figures H13 through H-15 (not I)
- (p.I-10) Should be Tables I-3 through I-5 (not D-3 through D-5)
- (p.I-11) Should be Tables I-3 through I-5 (not D-3 through D-5)
- (p.J-1) Should be Appendices I, M and N (not I, K and L)