

Prepared by CREST Consulting Team September 2008

Comment #	Section	Comment	Response
Jed Fuhrman- University of Southern California - Member of Technical Advisory Committee - Comment letter dated June 24, 2008			
JF-1	All	Overall, I believe the report describes a study that is generally comprehensive, well-designed, and well implemented. Although the data are inherently "noisy" and variable (as is common in this field of investigation), the main conclusions are derived from multiple lines of evidence. Therefore these conclusions are relatively robust and hold up despite several uncertainties that remain. Most of my comments below are aimed at specific technical details of the report, wording changes I suggest to make the report more easily understood (including for a non-technical audience), and issues that I think are ripe for follow-up or further analysis. They do not negate my general confidence in the overall report conclusions.	None.
JF-2	All	I think the report should include some clear statements that the lack of detection of particular pathogenic viruses in these samples by these particular techniques is not strong evidence that pathogenic viruses are very rare or often absent from the system. In other words, "absence of evidence is not evidence of absence." Many nonexperts would not necessarily come to this conclusion unless it is explicit. Viruses are hard to detect anyway even with specific assays (like these), but a given human subpopulation might not be infected with the viruses one is looking for at the time of the study (even if infected with and shedding dozens of other types of viruses). And these methods observed only small sample volumes so detection would not be easy. The sorts of results with "hits" here and there, often not correlating with bacterial contamination, is in fact common in environmental surveys like this. So the results are hard to interpret.	Good points. A critical discussion of virus measurements included in a new section entitled "Discussion of Human Virus Measurements" (Section 7.4)
JF-3	All	The report should discuss a little more clearly how the indicators used (e.g. human Bacteroidales) might relate to actual pathogens. This includes issues about regrowth and resuscitation of bacteria (pathogens as well as indicators), and also the point that viruses cannot reproduce in the water. For example, aerobic bacterial pathogens might generally follow E. coli, and anaerobic pathogens might more closely follow Bacteroidales. All this relates specifically to the question of what was happening in Reach 2 where the increase in indicators was unaccounted for.	A short discussion was added to Section 3.6.
JF-4	??	Resuspension of bacteria and viruses from sediments (as a reservoir or even growth location for some bacteria) can be considered, as discussed at the meeting by Stanley Grant, but it also should be considered that many sediment bacteria are in a slimy matrix and do not resuspend easily unless there is some scour from high flow or disturbance.	This is noted in the new Section 7.3.4.8 Potential Source #8: Regrowth in Sediments
JF-5	??	In the methods, it appears that quantitative PCR standards were DNA and not RNA, even if RNA was the initial target (e.g. enteroviruses). This is common procedure, but it should be recognized that it does not allow for correction of inhibition of the initial reverse transcription step of the amplification.	Noted in Section 3.6.2
JF-6	??	If I read the methods portion correctly, the PCR assays used 10 microliter subsamples from 200 microliters of DNA or RNA extract. These came from 200 ml (small volume) or 4 liter (large volume) samples. Because 1/20 of the total extract is used in each assay, this means that the PCR was actually testing the equivalent of 10 ml or 200 ml of the sampled water. These are very small samples, and when it is also considered that inhibition was examined with 10-fold dilutions (representing 1 ml or 200 ml each), it is no surprise that assays were only sporadically positive.	A critical analysis of virus measurements included in a new section entitled "Discussion of Human Virus Measurements" (Section 7.4)

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JF-7	??	It is disturbing that the large volume and small volume virus assays were not in agreement. My guess is that the substantial differences between protocols to capture viruses and extract the nucleic acids caused the difference due to variations in capture and extraction efficiency between different viruses. I prefer that explanation to random error.	The following note was added to Section 6.4.5: "The fact that the two methods differed is likely because (1) the protocols used to capture and extract viruses from the small- and large-volume samples differed, and (2) sample volume affects viral limits of detection both positively and negatively (Rajal et al., 2007a; Furhman et al., 2005). "
JF-8	??	I was surprised to see that the Bacteroidales measurements were sometimes so high that they were similar to amounts reported for pure fecal samples in the Kildare et al paper. Samples here were up to 3×10^6 gc/ml for universal and about 1×10^6 per ml for human specific Bacteroidales. Reported fecal samples were about 10^6 per ml for universal and 10^4 to 10^6 for human specific. Given that even pure sewage is mostly water rather than feces, I am puzzled by the amounts.	It is important to consider the composition of the solutions that were analyzed in Kildare et al. To create blind samples #1, #2, and #3 listed in Table 5, we diluted 0.3 to 30 mL of wastewater influent into 1-liter of DI water. There is a description of this in Section 2.10 of Kildare et al. Thus, the Kildare results do not really reflect "pure fecal samples". In fact, they're representative of wastewater influent, just as the BSI Study results. Thus we should expect the results to be similar.
Stanley Grant - University of California, Irvine - Member of Technical Advisory Committee - Comment letter dated June 29, 2008			
SG-1	NA	I very much enjoyed reading the report, and found the field studies well conceived and executed, and data analysis mostly solid.	None
SG-2	NA	The one possible exception is the mass balance calculations performed on several analytes (conductivity, E. coli, Enterococcus, and Bacteroides) over Reach 2 and 4 of the L.A. River. For these calculations, I believe that the analysis would be improved if: (1) errors were assigned to the loading rates measured in the storm drains and in the river, and (2) these errors were propagated through the mass balance model, for example using Monte Carlo simulations. The resulting uncertainty estimates will allow for assertions regarding whether mass balance for a particular analyte has been closed (or not) over a particular stretch of the river.	With Dr. Grant's guidance, we developed a Monte Carlo analysis to assess the potential effect of measurement variability and error. See the new Section 7.3.3.
SG-3	NA	At your request, I also calculated apparent doubling times for E. coli over Reach 2 of the LAR, using both BSI and Status and Trends field data. The analysis yields E. coli doubling times in this stretch of the river between 1 and 4 hours—considerably longer than the lower-limit of 20 minutes for E. coli growing in the laboratory, and in the range measured for E. coli regrowth in dry weather runoff elsewhere in southern California. These calculations are consistent with the idea that regrowth is responsible for the exponential increase in E. coli concentration over Reach 2 of the LAR.	Thank you, this language was added to the new Section 7.3.4.3.9 Potential Source #9: Regrowth or Resuscitation of E.coli in the Water Column
SG-4	NA	Overall, I am certain that the data described in the BSI report will provide a solid scientific foundation for the bacterial TMDL being developed for the LA River.	None
SG-5	Ex Summary	Page ES-1: You might consider stating upfront (in the introduction) what the study's goals are, at least in general terms. You state the approach adopted (a microbial toolkit), but I do not see a succinct (one sentence) statement of study goals.	Added goal statement.
SG-6	Ex Summary	Page ES-1: I suggest removing from the report superlatives, like those that appear in the last sentence of the Introduction. When I read sweeping statements like that, I wonder if they are true (e.g., have you reviewed all bacterial source tracking studies carried out in Europe?). In the end, I think statements like these detract from what is otherwise a well-conceived and well-written report.	Superlatives were toned down. But our colleagues are familiar with the MST research in Europe, and know of no other urban runoff studies that are as comprehensive as the BSI Study.

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SG-7	Ex Summary	Page ES-2: "a more than" should be "more than"	Fixed.
SG-8	Ex Summary	Page ES-2: again, not sure about "immense"	Prefer to leave this word - four teams of personnel on the river at once was indeed, immense.
SG-9	Ex Summary	Page ES-4: among the reasons that adenovirus might be more prevalent after storm events, is that rainfall could affect adenovirus survival (higher moisture content throughout the watershed?), and the time it takes water parcels to travel from one point in the watershed (e.g., a leak in a sewer pipe) to the LAR where samples were collected. Perhaps the latter is covered under item 2, but that wasn't clear to me.	Added this possibility to Section 6.4.5
SG-10	Ex Summary	Page ES-4: it is stated that the lack of correlation between land-use and storm drain water quality could be related to the sample design methodology (storm drains were not randomly selected). However, given that you did select a set of storm drains for GIS analysis, it is fair to ask if among those storm drains there was significant variability in land-use types. If the answer is "yes", then it would seem to me that the result you present is still significant; i.e., within the range of land-use types examined in these sub-drainages, there does not appear to be a relationship between stormdrain water quality and land-use. Perhaps the lack of correlation observed here reflects the fact that sub-drainages with heterogeneous land-uses tend to mask the positive/negative effects of any one particular land-use type.	Good point. Noted in the Exec Summary and Section 7.4.3.
SG-11	1	Page 1-1, second paragraph: "the approach to which was..." is awkward.	Fixed.
SG-12	1	Page 1-1, third paragraph. The goal statement would fit nicely into the first paragraph of the Executive Summary.	Done.
SG-13	1	Page 1-1, third paragraph. "unprecedented", "and may include the most extensive..."	Changed the latter, kept the former. (Since we're intimately familiar with the LA River, we know that that BSI Study is, in fact, unprecedented.)
SG-14	2	Table 2-1: Would be nice to have a column that indicates the period of time over which data are available (and how the data can be accessed).	Added a footnote that these data will be compiled and made available as a component of TMDL development.
SG-15	2	Fig. 2-4: I'm surprised that so many of the "wet weather points" plot at x-positions indicating moderate to low flow rates in the LAR at that site. Are these "wet weather points" measured during dry weather in the wet weather season? If so, that should probably be noted in the figure legend.	Wet weather includes the three days following a rainfall event, so wet weather samples can be collected during low flows. Added a note to clarify this point.
SG-16	2	Figs. 2-4 and 2-5: It is also interesting that there are quite a few load exceedences during high flow events at sites downstream of the WRP discharges, including Sepulveda and Figueroa. Any thoughts why this might be the case? Is it possible that the WRP perform less well during storm events, releasing some of these FIB to the LAR? It seems odd to me because the same dilution effect should apply in wet and dry weather periods (presumably the volumetric flow rate of the WRP effluent also increases during wet weather).	Our understanding is that the "influence" (dilution effect) of WRP discharges is much lower during high flow events (i.e. the proportion of in-stream flow that is from urban runoff is much higher), so wet weather samples from Sepulveda Blvd. and Figueroa St. exceed WQOs at much higher frequencies. Added a note to clarify this point.
SG-17	2	Page 2-14: Relative to VNC, you should probably cite a more recent (and relevant) paper by a Carl Bolster and his students: C.H. Bolster, J.M. Bromley, and S. H. Jones (2005) "Recovery of chlorine-exposed Escherichia coli in estuarine microcosms" Environ. Sci. Technol. V39(9):3083-3089.	This reference was added.

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SG-18	2	Page 2-14: It is also possible that FIB grow in the sediments, partition into the pore fluids, and then diffuse through a benthic boundary layer into the overlying water column. Perhaps this is already covered in the existing text, but I thought the discussion could be a little stronger.	This clarification was added.
SG-19	2	Page 2-15: Catch an extra comma at the end of a sentence in the second paragraph.	Fixed.
SG-20	2	Fig. 2-6: Not sure that the comparison of FIB on a MPN/100g and MPN/100 mL is meaningful. There are a couple of problems with normalizing across these two media types by weight (100mL=100g of water compared to 100 g of sediment). Sediment has a higher density than water, so 100g of sediment will generally occupy much less volume than 100 mL of water. Further, for most sediment, the interior of sediment grains is not accessible to bacterial growth, and hence even less of the sediment volume will be available for colonization. One way to put this on a more fair footing is to plot the sediment data on a per 100 mL of void space, which requires knowing the porosity of the sediment sample (easy to measure, but you may or may not have it).	Interesting point. We added the following note to Figure 2-6: "It may be more appropriate to compare sediment and water samples using volume of void space as opposed to weight, but the porosity of collected water samples was not available." Also, we removed the text in the section that stated sediment samples were more enriched than water samples.
SG-21	3	Page 3-5, second paragraph: "to supplement characterize" is awkward.	Fixed.
SG-22	3	Page 3-22: I wonder if, during the snapshot sampling events, it would have been better, in retrospect, to sample using a Lagrangian design; i.e., to sample the LAR sites based on the travel times from site to site. Of course, doing so might bias the results if sunlight is a factor, but diel studies would have addressed that point. Relative to the report, I guess it isn't terribly clear to me why you pseudo randomized the sampling of the storm drains, but adhered to a single schedule for the LAR sites. This sampling design raises questions about the mass balance approach you use later, because the storm drains flows have diel cycling (as perhaps do the FIB), and so the loading estimated from the LAR sites may be "out of phase" with loading at other LAR sites and in the storm drain discharges. I would have benefited from a clear statement of the strengths/weaknesses of the sampling approach adopted. The justification provided, that adhering to the same sampling schedule for the LAR sites provides "for more precise upstream/downstream bacteria loading rates" is not convincing to me.	Given the number of sites, length of the LA River reaches, and safety issues associated with nighttime sampling, a LaGrangian study was not feasible. More language was added to clarify our approach and the reasons for it. Note that the storm drain sites should NOT be "out of phase" because great efforts were made to vary sampling time over the course of the study.
SG-23	4	Page 4-1: Should indicate if you used Colilert-18 or Colilert-24.	Done, we used Colilert-18 and Enterolert-24.
SG-24	4	Page 4-5: "labeled at the 50 end" and "at the 30 end" should be "labeled at the 5' end" and "at the 3' end".	Fixed.
SG-25	5	Tables 5-1 and 5-2: Again, indicate whether you used Colilert-18 or Colilert-24.	Done, we used Colilert-18 and Enterolert-24.
SG-26	5	Page 5-4, last paragraph: should be "to avoid having a qualifier..."	Fixed.
SG-27	5	Page 5-4, last equation: where does the 3.24 factor come from?	I'm not sure. This RPD equation is commonly applied to bacteria studies.
SG-28	5	Figs. 5-1 through 5-4: it would be interesting to plot the lab duplicates and field duplicates on the same graph (with different symbols). That way the additional variability associated with the field duplicates can be evaluated (to my eye, it looks like most of the variability is lab-based, not field based).	Done.
SG-29	5	Page 5-13, first paragraph: "three of six and Snapshot..." should be "three of six Snapshot..."	Fixed.

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SG-30	5	Page 5-13, first and second paragraphs: I am not a PCR expert, but I found it concerning that a relatively large number of Snapshot Event field blanks (3/6 and 2/6) and all WRP field blanks tested positive for Universal or Human Bacteroidales. I think these results should be more thoroughly discussed. Are these positives coming from within-lab contamination? If so, what steps were taken to track down within-lab contamination? What level of within-lab contamination would be tolerated before the QPCR data would be considered unreliable?	More discussion was added to Section 5.2.2. It appears the low-level contamination was from within the laboratories. The dataset in Appendix 2 includes qualifiers if the blank concentration was greater than 20% of that for the environmental sample. Because the positive environmental samples for human Bacteroidales often exhibited concentrations in the hundreds or thousands of gc/mL, compared to <10 gc/mL for blanks collected during Snapshot Events the contamination has little potential to induce error into the data analysis. However, the low-level contamination limits our ability to use presence/absence reliably. This "footnote" is now carried throughout the report.
SG-31	5	Page 5-13, third paragraph: sentence that begins "note that lack of a detection in both samples..." doesn't make sense to me. Is it supposed to read "note that the lack of a detection in one sample..."? The fact that in 8 cases, both field replicates were blank for Bacteroidales is not very reassuring (relative to the possibility of within-lab contamination), because as noted the fact that both replicates may have false negatives caused by inhibition effects.	The non-detect field replicates just provide another line of evidence that contamination was not prevalent. Note that it should be both samples (not just one) because we collected two samples and both were non-detect. Also, note that a multiple dilution approach was used to decrease the effect of inhibition. All of the above is now noted in Section 5.2.3
SG-32	5	Fig. 5-5: Isn't there shockingly little concentration difference between the field blanks and the field samples? To my eye, the medians all look like they are within an order of magnitude of each other.	Yes, the median values are similar for tribs vs. drains vs. blanks. But this shouldn't be shocking because the median is non-detect (or close to it). Because we substituted (censored) non-detect results with one-half of the detection limit (4.7 gc/mL), the median values look similar. We added language to remind the reader that this may make the blanks look more contaminated than they really are. Also, the reader is reminded that abundance is more important than presence/absence.
SG-33	6	Page 6-2: you might include the units of the conversion factor for k	None.
SG-34	6	Table 6-1: I would have benefited from a simple map that shows these Snapshot Event sites in relation to the Status and Trends sampling sites. For example, adding the Snapshot Event sites to Fig. 2-1 would be great, and then making that the first figure in Section 6. (I looked at the maps of sampling sites in Section 3, and they aren't very helpful to me—I had trouble locating the LAR Snapshot Event sites listed in Table 6-1, in part because these sites are either not labeled with the Table 6-1 site names (over view map), or the labels are hard to read (individual reach maps)).	Such a map would be useful, but time constraints did not allow us to generate it.
SG-35	6	Table 6-1: I'm also confused about the Figueroa Street sampling site. On the Status and Trends map this site is indicated as being part of Reach 3 (see Fig. 2-1), but in the Snapshot Event sampling it is indicated as being part of Reach 2 (Table 6-1).	A footnote was added to Section Table 6-1 to clarify this point.

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SG-36	6	Page 6-8, Third Paragraph: Could it be that something in the WRP effluent is killing FIB in the LAR before it mixes with WRP discharge? For example, there may be some residual chlorine, or less nutrients, that leads to not only a reduction in concentration (from dilution), but also a reduction in the total load because the effluent has a germicidal effect on bacteria already in the LAR. Anyway, it might be worth mentioning that the WRP effluent might be involved, because, in addition to the earthen bottom, the other unique feature of these two regions is that WRP effluent was added just upstream (if I have my geography right!).	This possibility was added to Section 6.1.2
SG-37	6	Page 6-9, second paragraph: same comment as above applies.	This possibility was added to Section 6.1.2 .1
SG-38	6	Figs. 6-3 and 6-4: really beautiful!	None
SG-39	6	Page 6-20, first paragraph: I think it is important to note here that interpretation of +/- detection of Bacteroidales is problematic in this study, because many field blanks tested positive for these organisms.	We agree. This "footnote" is now carried throughout the report.
SG-40	6	Page 6-20, second paragraph: same comment applies—can you really ascribe meaning to the number of samples testing positive for Bacteroidales when so many of the field blanks tested positive?	A footnote is now carried through the report reminding the reader that abundance is more relevant than presence/absence.
SG-41	6	Page 6-31, second paragraph: "Figure 6-X"	Correct to Figure 6-17
SG-42	6	Page 6-32, second paragraph: Any worry that the low-end concentrations of Bacteroidales measured in LAR might be contamination (i.e., how much higher are these numbers than concentrations measured in field blanks?)	A footnote is now carried through the report reminding the reader that abundance is more relevant than presence/absence.
SG-43	6	Figure 6-24: I would have benefited from some discussion of the diurnal pattern observed in this figure. Does the magnitude of the peak scale with the day of the week? It looks to me like there might be a recurrent 3 day pattern (although with such a limited time series it's hard to say). I note that the first big peak (on 11/10) was a Saturday, when you'd sort of expect people are out washing their cars, and watering their lawns. Also, I thought it was interesting that, on many days, there is a shoulder on the primary (morning) peak.	Agreed it would interesting to evaluate the effect of weekdays versus weekends. But as stated, the timescale of the Flow Meter Study is too short to perform this analysis robustly.
SG-44	6	Figure 6-26: This figure is a pretty impressive result. I guess this raises a statistical power question: how many samples (or "realizations") would one need to collect from a particular site in order to determine if it is really "worse" or "better" than any other site?	Statistical differences among sites are assessed in Section 7.2.3
SG-45	6	Page 6-60, second paragraph: "steady of the course" should be "steady over the course"	Fixed.
SG-46	6	Page 6-64: Again, given the high rate of false positives in the field blanks, does it make sense to ascribe meaning to the number of samples that tested positive for Bacteroidales? At a minimum, I think it would be a good idea to caveat such statements, with a reminder that many of the laboratory blanks were positive too.	A footnote is now carried through the report reminding the reader that abundance is more relevant than presence/absence.
SG-47	7	Page 7-2: If I recall correctly (it's been a while!), the null hypothesis for the t-test for correlation significance should be that the correlation is zero; i.e., $p = 0$. If $p < 0.05$, then with a 95% confidence you can reject the null hypothesis. Given the above, I find the notation "Prob>[p]" confusing in the context of the t-test. Please clarify.	Clarified. Also, replaced the term "Prob>[p]" with "p-value".

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SG-48	7	Page 7-3: I found the observation that FIB concentrations were inversely correlated with flow to be interesting. For measurements at a single location during dry and wet periods, we see exactly the opposite: FIB concentrations increase with increasing flow. But there is a difference between measuring FIB and flow at a single site over a long time encompassing both dry and wet weather periods (like we did), and measuring FIB and flow at a few times during dry weather at many sites (like you did). It leads me to wonder if there is an inverse correlation between the size of a subdrainage (and hence the dry weather flow rate) and FIB concentrations. Was this tested in the GIS analysis presented later?	Yes, the effect of drainage size on flow rate was evaluated. It was one of the strongest correlations among the measured and GIS parameters. See Section 7.4.3
SG-49	7	Fig. 7-1: I really like this!	None
SG-50	7	Page 7-8, first paragraph: this first sentence (beginning "loading rate is a function ...") is awkward.	Fixed.
SG-51	7	Page 7-8, first paragraph: If flow rate and concentration are not significantly correlated (i.e., $p > 0.05$), then it's reasonable to treat them as stochastic variables, and thus generate a cdf of loading rates from the cdfs of concentration and flow using a Monte Carlo simulation. If the cdf of loading rates thus generated is similar to the cdf of loading rates measured (i.e., by multiplying coincident measurements of concentration and flow rates), then you've got something useful—the ability to generate a probability distribution of loading rates from independently measured concentration and flow data. This approach would account for the enormous variability you document in both flow and concentration.	With Dr. Grant's guidance, we developed a Monte Carlo analysis to assess the potential effect of measurement variability and error. As recommended (and demonstrated in Section 7.1), flow rate and concentration were considered independent variables. See the new Section 7.3.3.
SG-52	7	Table 7-5: I agree with your decision to perform correlations with the log-transformed FIB concentrations, because these data are typically lognormally demonstrated (as you clearly demonstrate in Fig. 7-1). However, I'm less clear why you chose to perform statistical analyses on logtransforms of some of the other variables, particularly in cases when the analyte in question does not vary over multiple orders of magnitude. For example, it looks like TSS varies over $10^{1.5}=32$, and DO varies over $10^{0.6}=4$. But truth be told, now that I think about it, log-transformation should not affect the correlation result if you are using Spearman rho (because it works by correlating the rankings, not the values). I guess that raises the question of why you log-transformed prior to computing Spearman rhos!	The log-transforms were performed because this transform resulted in the "most normal" distribution, which was ideal for plotting purposes. As you said, it has no effect on the Spearman correlations.
SG-53	7	Page 7-14: I found the heading "Storm Drain Behavior" a little strange. Storm drains have behavior? Could you use a more descriptive heading? Perhaps "Storm Drains: Temporal and Spatial Variability". You use the term "storm drain behavior" throughout this section, and if possible you should replace it with something along the lines of what I suggested above.	Fixed. We changed "Storm Drain Behavior" to "Storm Drain Observations".
SG-54	7	Tables 7-19 and 7-20 are interesting. Well done.	None
SG-55	7	Page 7-52 and Table 7-21. I guess the idea behind your mass balance analysis is that the electrical conductivity is proportional to the salt concentration in the sample. That assumes that these conductivity measurements are temperature corrected, and that the distribution of different salts contributing to the conductivity don't change appreciably from one site to another. Please see comment below regarding propagation of errors through the mass balance—I think that is the only quasi-serious problem I can discern in the (considerable!) analysis presented in the report.	The conductivity measurements were temperature corrected. As noted above, a Monte Carlo analysis is presented in the new Section 7.3.3
SG-56	7	Table 7-23: I didn't notice this elsewhere, but you are carrying far too many significant digits. Within the resolution of the tests you are using, 173,907 should be 170,000, 5012 should be 5000, etc.	In progress.

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SG-57	7	Error Propagation in Mass Balance Calculations: One problem with the mass balance approach that you are using is that you don't track errors (as becomes evident when you report loadings to 6 significant digits!). So, a difference between measured and predicted loading rates of "100%" could be significant or insignificant, depending on the propagation of error through the mass balance. This may seem like a trivial issue, but it's not. If you want to use the mass balance approach as a source tracking tool, it is imperative that errors be properly accounted for. I realize that each of the loading rates calculated for each event are based on single measurements of concentration and flow at storm drains and LAR sites, however, there are still approaches that could be used to assign errors for loading rates. For example, one approach is to pool all of the event data (excluding the event that included the SSO which is clearly anomalous), assume log-normal distributions for the loadings (which seems reasonable based on your other data), and then propagate errors through the mass balance equations using a Monte Carlo simulation. By the way, I don't d	With Dr. Grant's guidance, we developed a Monte Carlo analysis to assess the potential effect of measurement variability and error. Flow and concentration were considered independent variables. See the new Section 7.3.3.
SG-58	7	Table 7-24: Please check all tables and numbers in the report to make sure that the number of significant digits reported are defensible.	In progress.
SG-59	7	Page 7-56: You conclude that the E. coli increase along Reach 2 is probably non-human origin because the Bacteroidales concentration remains constant along this stretch, but wouldn't you expect some decay in the Bacteroidales signal with distance downstream? Couldn't the fact that it doesn't decay be construed as being caused by additional input along this reach?	An analysis of the effect of decay on Bacteroidales measurements was conducted, as presented in the new Section 7.3.4.1 (see Tables 7-29 and 7-30).
SG-60	7	Page 7-57: When you report that field workers were nauseated by the smell, was that a sulfidic smell?	Yes, it was sulfidic. Noted.
SG-61	7	Tables 7-28, 7-29, and 7-30: I didn't see a correlation test between FIB concentrations and total subdrainage area (it appeared to be broken out by area of a given type of land use). Were those correlations carried out? As mentioned earlier in my notes, I think that might be an interesting number (or of no interest at all—hard to say!).	Yes, the correlation among concentrations and land use were performed. All were weak. The correlation among flow rate and drainage size was large.
SG-62	8	In general, I think the approach used to identify "problematic" storm drains—of classifying them based on the three criteria, different, consistent, and rare—makes sense. It is a creative way of dealing with the non-unique answers that come from the different indicator measurements.	None.
SG-63	8	Figure 8-1 and 8-2: A great summary of the results.	None.
SG-64	8	Table 8-3: It is striking that the loading rates are so much higher than the allowable loading in Reach 2, but about equal to the allowable loading in Reach 4. This difference must reflect different concentrations of E. coli in the storm drain runoff from the two sites, because the ratio of the measured to allowable loading is just equal to the ratio of the measured concentration to the single-sample standard. Could it be that whatever is causing the concentration of E coli to increase in the LAR along Reach 2 (e.g., regrowth) is also causing E. coli to increase in the stormdrain runoff along that reach? From a broad geological or biological perspective, is there anything fundamentally different about Reach 2 and 4 that might be worth keeping in mind?	We attribute this difference to the fact that more runoff was discharged along Reach 2. Because concentration and flow rate are not correlated, more runoff produces more loading.
SG-65	8	Page 8-9, second paragraph: Would the E. coli loadings from storm drains along Reach 2 be sufficient, just by themselves, to cause WQO exceedences in the river (assuming that there were no other sources of E. coli in the LAR)?	Yes WQO exceedences might occur if storm drain and tributary loading was zero. Noted.
SG-66	9	First paragraph: hyperbole necessary? This section could easily start with the second paragraph beginning "A summary of the findings..."	We prefer to leave the first paragraph, but toned it down some. The agencies that supported this study deserve to have its significance stated.

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Comment #	Section	Comment	Response
SG-67	9	Otherwise, this was a really nice summary of the report.	None.
SG-68	10	Seminal?	Changed to "important"
Sunny Jiang - University of California, Irvine - Member of Technical Advisory Committee - Comment letter dated July 1, 2008			
SJ-1	NA	This report presents the investigation of bacteria contamination in Los Angeles River (LAR) using multiple research approaches. The study is well designed to capture all possible sources of bacterial input from storm drains and tributaries along three reaches of LAR where bacterial contamination exceeds state REC standards. Flow rate and runoff volume was carefully measured and calculated to capture the total loading of bacteria to the river and to identify the storm drain that has the most impact to the water quality of LAR. Mass balance approach was used to determine the unaccounted source of bacterial contamination in Reach 2 and Reach 4. Advanced microbial source tracking methods were applied to identify human waste contamination in the storm drains and the tributaries of LAR.	None
SJ-2	NA	The report is well written and data are well synthesized and presented in the easy to understand graphs and tables. The weight - of-evidence approach incorporates multiple considerations to provide answers to the study questions. My comments below hope to provide insight to additional data analysis and our understanding of current methodological limitation to the study.	None
SJ-3	??	Bacterial mass - balance and growth: Application of mass - balance calculation in this study suggested that growth of fecal indicator bacteria within Reach 2 of LAR may be an important contributor to the fecal bacteria that could not be accounted for from all known sources. A growth calculation can be made by assuming that all bacteria at the end of Reach 2 is generated by growth. If the generation time produced by this exercise is unreasonable (too high for the known bacterial doubling rate), a decay constant can be incorporated from literature. This may give us additional information as if growth in the water can be a reasonable source of fecal indicator bacteria in Reach 2.	An analysis of doubling time is assessed (with Dr. Grant's input) in the new Section 7.3.4.3.9 Potential Source #9: Regrowth or Resuscitation of E.coli in the Water Column
Microbial Source Tracking Methodology:			
SJ-4	??	Two microbial source tracking methods were used in this study: bacteroidales and human viruses. Both assays are based on the detection of genomes of the target organisms. There is sufficient discussion in the methods section on the limitation of genome based methods, a few sentences should be added to the result section to help interpreting results from genome based methods.	Footnotes were added to Section 6 to ensure the reader is aware of the differences in genome- vs. culture-based methods.
SJ-5	??	Bacteroidales were detected in very high concentration in terms of genome numbers. An explanation of how the genome numbers can be converted to cell numbers will be useful for audience to understand the outcome. Although decay of the organisms is not the focus of this study, it is important to know how fast the genome decay in the environment or decay under condition that is similar to the LAR environment. The decay of the target is balanced by the input. Discounting decay through the course of river flow will significantly under estimate the input to the river.	An analysis of the effect of decay on Bacteroidales measurements was conducted, as presented in the new Section 7.3.4.1 (see Tables 7-29 and 7-30).

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Comment #	Section	Comment	Response
SJ-6	??	For human virus detection, two types of human viruses are targeted: adenovirus and enterovirus. There are several reports on adenovirus detection in the wastewater and other aquatic environments in Europe. These literatures should be included in the report to compare with the current methods and results. The concentration of human viruses detected in LAR - BSI study from human sewage is a lot lower than the reports from Europe. Human enterovirus was not detected in human sewage. Considering the positive detection of enteroviruses in other studies, the report should note the difference between this and other studies. Only very few positive viral samples were found, while human contamination is shown to be prevalent (based on HumBac) in the LAR. This is contradictory to the results reported in other studies (Europe, Korea, South Africa). A discussion of the difference in methods used in this study with other reports will be helpful to understand the outcome. I think the sample volume used, viral concentration methods, and perhaps different primer set used for targeting different regions of the viruses are important	A critical discussion of virus measurements included in a new section entitled "Discussion of Human Virus Measurements" (Section 7.4)
		This study showed that viruses were not detected by the method applied here, which did not imply that viruses were not there. It means that viral source tracking is a lot harder to use than other methods, such as HumBac method. The conclusion should be that it is impractical to use viruses as human waste marker because the technology for concentrating and recovery of viruses are more challenging than others.	A critical discussion of virus measurements included in a new section entitled "Discussion of Human Virus Measurements" (Section 7.4)
SJ-7	2	Figure 2 - 4 and 2 - 5: The x - axis is difficult to understand for people who are not familiar with flow data. Perhaps it will be helpful to explain that the x - axis is the expression of 1 - CDF.	Added the note: The x-axis expresses the cumulative distribution frequency (CDF).
SJ-8	2	Figure 2 - 6: This is an interesting graph plotted using historical data. However, I don't know the reason to tinker with the scale of x - axis. This may produce miss - leading impression of the curve (ie. from convex to concave). Since this graph is to show the difference between water and sediment bacterial density, I don't see the need to modify the scale on X - axis.	These plots are from a previous report. The x-axis is scaled based on standard deviations (the z-scores). We added a note to indicate this.
SJ-9	3	Page 3 - 26: Recommend expanding this section to include references for human adenovirus study from Europe (where this type of virus was first suggested as an index of human waste contamination). The methods section should compare the method used this study with other existing methods for viral concentration and primer targets.	In Section 3.6.2, we reference the published assays that UCD modified or adopted. Further details re: the UCD assays are provided in Section 4.4.5. In Section 7.4 the results of our application of the UCD assays are compared to the Europe studies.
SJ-10	4	Page 4 - 5 line 4 - 5 and 7: There are typo for 5' . It is not 50. It should be 3' not 30.	Fixed.
SJ-11	4	Page 4 - 7: Same problem in viral assay section as well. 5' not 50.	Fixed.
SJ-12	6	Page 6 - 7 Figure 6 - 2: Add label for each event to the figure legend.	Added a figure note to explain the line labeling.
SJ-13	6	Page 6 - 22: The genome copy numbers for Bacteroidales are very high. Is there an understanding between genome copy and CFU?	Footnotes were added to Section 6 re: the typical rule of thumb that 5 gc/mL = 1 cell/mL.
SJ-14	6	Page 6 - 36: Section on human viruses in WRP. The interpretation of the results should be carefully considered. The conclusion that WRP effluent has small impact on LAR may not be accurate. There is a decent amount of literature on adenoviruses in sewage and environment. It seems the lack of detection of adenovirus and enterovirus in the sewage samples may be the limitation of the methodology applied here and constraints of handling large volume of samples.	We added many footnotes and Section 7.4 to remind the reader to be careful in his/her interpretation. It should be noted that adenoviruses were readily detected in all sewage samples, which suggests the applied assay is reliable for source tracking in the LA River. In contrast, the enteroviruses assay were not positive in sewage, which makes in unreliable for source tracking in this watershed (see the new Section 7.4)
SJ-15	7	Page 7 - 8: Line 2, Should it read "concentration can drive loading rates" (rather than "flow rate").	Fixed.

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Comment #	Section	Comment	Response
SJ-16	8	Page 8 - 9: Line 5: typo - "he" should be "the".	Fixed.
SJ-17	9	Page 9 - 5: "Enterovirus was never detected during this study, even in sewage, which questions the utility of the employed enterovirus markers for source tracking." I agree with the statement that enterovirus markers for source tracking are perhaps impractical. However, the statement should be reworded to highlight technology challenges of detecting viruses. The occurrence of Enteroviruses have been investigated in sewage all over the world and shown to be an important component.	Reworded. A critical analysis of virus measurements included in a new section entitled "Discussion of Human Virus Measurements" (Section 7.4)
SJ-18	12	Page 12 - 1 The following reference is repeated twice Boehm, A.B, J.A. Fuhrman, R.D. Morše, and S. B. Grant. (2003). A tiered approach for identification of a human fecal pollution source at a recreational beach: case study at Avalon Bay, Catalina Island, California, USA. Env. Sci. & Tech. 37(4): 673 - 680	Fixed.
Susan Paulsen - Comments sent via e-mail August 4, 2008			
SP-1	Ex Summary	Suggest adding an executive summary and really distilling the essence of the report.	An executive summary was posted on the CREST website.
SP-2	6	Chapter 6 seems to be missing from my report - probably my error, but I haven't looked at this one...]	Section 6 was posted on the CREST website.
SP-3	??	Suggest adding some literature supporting the assumption that indicator bacteria from human sources pose more of a health risk than indicator bacteria from non-human sources...	Added references to Section 3.2 and 3.6.
SP-4	7	P. 7-6: fix spelling of heath to health	Fixed.
SP-5	7	I like the summary tables starting on p. 7-16. Thanks also for noting the SSO and diversion maintenance events.	None.
SP-6	7	In the bubble plots, at least in my printed out copy, it is very hard to read the storm drain names (too small when plots are presented on a quarter-page).	The pages are 11x17, which allows for printing with larger text.
SP-7	7	P. 7-50: suggest that we shouldn't make up data (i.e., shouldn't use 90th percentile values in the absence of data) - but rather, should just note that IF those had 90% values, they'd be small in comparison to the rest, esp for Reach 2 - OR use subsequent data to assess whether later measured values were at or near 90% levels (though this is obviously imperfect too)	Estimation of unmeasured inputs is an important consideration to the mass balance. If we ignore the unmeasured inputs, we certainly underestimate storm drain loadings. By using the 90th percentile, we provide confidence that unmeasured inputs are not responsible for "open" mass balances.
SP-8	7	In mass balance sections, suggest strongly adding back in the pie charts or some other graphical representation of just how much of the mass loading is from "unknown" sources. To me, this is the most important part of the study, esp. as we look to evaluating standards (note that P-C Section 13241(b) and (c) will require evaluation of "...the quality of the water available" [i.e., background conditions] and "water quality conditions that could reasonably be achieved through coordinated control of all factors..."]	The pie charts were added to Section 7.3.2. Also, the newly-added Monte Carlo analysis (Section 7.3.3) graphically displays the probabilities associated with various loading rates and percent contributions.
SP-9	7	Regarding contaminated groundwater, most groundwater has very low bacteria counts due to the "filtering" ability of soils. Only if rising groundwater picked up bacteria from bedded sediments and carried them into the stream would this be a likely source, unless there were some known soil contamination (e.g., from a sewage spill). Also, suggest mentioning any EC measurements you have for groundwater. Suggest revising this to low potential.	Indeed, recently-collected samples of groundwater exfiltrating through sub-drains along Reach 2 were non-detect for indicator bacteria. This is noted in the new Section 7.3.4.3.6 Potential Source #6: Groundwater Exfiltration
SP-10	7	Re: regrowth or resuscitation: do pathogens associated with indicator bacteria also regrow, or are they effectively destroyed or rendered un-infectious by UV treatment? Seems important to note the implications for human health, if known.	Viruses cannot regrow in the environment. Noted in Section 7.3.4.3.9

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Comment #	Section	Comment	Response
SP-11	8	p. 8-7: question 1a is "Are storm drains and tribs responsible for the significant bacteria loads...and causing exceedances...?" Answering "yes" implies that storm drains and tribs are the primary cause of exceedances, and, esp in R2, they're probably not. The way the question is worded I think needs a more nuanced answer, e.g., "while concentrations in storm drains and tribs exceed WQO, they are not sufficient (esp R2) to account for the observed concentrations/loads." The language on 8-9 is better than that at the bottom of 8-7 (see below).	Our response to Question 1a attempts to address the fact that both storm drain and uncharacterized loadings are too high to allow for WQO attainment. Thus, the answer has two components, which seems to address this comment.
SP-12	8	p. 8-8: suggest including the mass loadings leaving the reach in the table, so that the reader can do the math without flipping around.	Done.
SP-13	8	p. 8-9, top: disagree with "probably yes" for R4. I think we just don't know. It could be a happy accident of regrowth and decay rates that the loading figures nearly balance here. I think we don't understand either growth (whether from regrowth, sediments, birds, whatever) or decay/loss well enough to make this conclusion	The newly-added Monte Carlo analysis (Section 7.3.3) allows us to calculate probabilities to be associated with "yes" and "no". This language has been added to Section 8.2 .
SP-14	8	p. 8-9, middle: "yes, but..." is a better answer (for R2)	None.
SP-15	8	p. 8-10 to 11: you haven't answered question 2a directly. Suggest a concluding sentence to sum up the argument(s) you're making.	Short answers, similar to those listed in Table 8-11, were added to the text.
SP-16	8	Actually, the last point goes to all the study design questions: suggest adding a concluding sentence or two or three after the discussion of each section, to lay out for a less sophisticated reader (or one with little time) exactly what you can and can't conclude from this study. Perhaps call out the bottom line answer in italics or bold.	Short answers, similar to those listed in Table 8-11, were added to the text.
SP-17	8	You've called out some important points in the conclusions - e.g., variability in loading rates, what would happen even if loadings were cut, effect of the diversion. Thanks.	None.
SP-18	9	9-3: suggest calling out up top of the paragraph on adenovirus that those from WRP effluent are unlikely to be infectious. Were viruses in WRP effluent not measured, but only in influent? If so, please state here. [This contradicts 3-10, which says that effluent was monitored...please include mention of these results...granted my review was cursory, but I couldn't find effluent measurements.]	A footnote re: viability was added. The WRP effluent measurements are presented in Section 6.
SP-19	9	9-4: 100,0000 is an odd number - do you mean a million?	Fixed.
SP-20	10	10-1: here's a mention of R6, but it didn't appear in the conclusions. Could you make a statement about what you did/did not learn from R6 samples in the conclusions section? [sorry if I just missed it]	Reach 6 results are summarized in Section 9 under "LA River water quality" and "genomic vs. viability methodologies"
SP-21	NA	Can we make any statements about what happens between R4 and R2? Can we look at mass loading rates and concentrations and determine if there's dilution, or decay/loss, in R3? I'm curious about why concentrations drop so precipitously and think we should state what we do/don't know. Same for R6 to R4...	Possibilities for the decreased loading between R4 and R2 are noted in Section 6.1.2.1 and 6.1.2.2
SP-22	NA	Is there mention of the idea of an equilibrium? Perhaps conditions are different in the different reaches, such that the "carrying capacity" of the stream is different from one reach to the other...can this concept be discussed?	The idea of an equilibrium concentration is interesting. But we know of no scientific data re: stream equilibria of indicator bacteria, and would rather not discuss a phenomenon for which there is no scientific evidence.

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Comment #	Section	Comment	Response
SP-23	NA	Overall: the implications of this information for human health isn't clear. Would it be possible to add to the conclusions or recommendations some statement regarding what (if we know) is the most important consideration for protection of human health? If we don't know, please state. A negative statement is just as important as a clear one.	In general, we recommend addressing sources that are high in both traditional and human-specific indicators (Figure 3-1). The weight of evidence approach highlights for the stakeholders the sites that meet this criteria.
Penny Weiland - City of Los Angeles Watershed Protection Division - Comments sent via e-mail August 6, 2008			
PW-1	Summary and Chapter 9	The sentence discussing the loading rate increase along Reach 2 being "much greater" than the loading rates measured from all storm drains and tribs should define be more descriptive and define "much greater" to better represent the data. I think the difference (unknown source) was up to 10 times more than that measure in the storm drains.	Done.
PW-2	Summary and Chapter 9	In addition to the above comment, I think it would be beneficial to explain in the Exec Summary and Chap 9 that with these data results, diversion of all the storm drains in R2 would not bring the reach into compliance.	Done.
PW-3	7	Page 41, Sec 7.2.3.2, 5th bullet - Shouldn't the second R2-A be R2-N?	Yes, fixed.
PW-4	7	Page 58, Sec 7.4.1 - This section states that some of the subwatersheds could not be delineated and thus were not included even though they may have been considered problematic in terms of microbial water quality. I think we need to revisit the exclusion of these sites and discuss the details at a follow-up meeting. At a minimum, I would like to see the list of sites not included and the corresponding data. Maybe there is some work City staff can do to resolve this issue.	The sites that were not delineated are all of those that are not listed Table 7-33 and Table 7-34. We are available to discuss this with the City.
PW-5	7	Page 62, Sec 7.4.3, 1st paragraph - Please show the predominant land uses for Reach 2 table, such as industrial, since several of the sites have very low residential and open space percentages.	Done.
PW-6	8	Table 8-1 - As discussed in our last TWG meeting, please show the percentile calculations without the SSO in this table?	Done. Also, we added an Appendix 5 that presents the Weight of Evidence results without the samples from R4-T during the SSO or R2-N (which is normally subject to low flow diversion).
PW-7	8	Section 8.2 Answer to Question 1 - I have strong reservations about the way this question is being addressed. As discussed in our last meeting, please clarify these tables and include the downstream data in tables 8-2 and 8-3. Please ensure that the discussion of the unknown source is shown in these tables and discussed in combination to properly answer the question. I am not convinced that breaking this question into a perspective 1 and perspective 2 is the best approach.	The downstream loading along each reach was added. We feel it is critical to consider Question 1a from the two perspectives. Specifically, it is important to convey that along Reach 2 both storm drains and uncharacterized sources are important. The Monte Carlo analysis was used to provide additional insight into this study design question.
PW-8	8	On Table 8-3 please verify the asterisk is correct for event 3.	Confirmed. Note that the reported values are loading rates, not concentrations.
PW-9	8	Section 8.3 and Table 8-10 - In answering question 2(a) please include a percent area to give the reader a better sense of the degree of impact.	We have not delineated the entire sections of the LA River watershed that drain to Reach 2 and Reach 4, and therefore we cannot provide the "% area" of the sampled storm drains.
PW-10	8	Table 8-9 - similar to comment on chap 7, the use of residential and open space land use may not be appropriate for R2.	Done.

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Comment #	Section	Comment	Response
PW-11	NA	As discussed in our TWG meeting, please add pie charts or bar graphs to better communicate the loadings per reach.	The pie charts were added to Section 7.3.2. Also, the newly-added Monte Carlo analysis (Section 7.3.3) graphically displays the probabilities associated with various loading rates and percent contributions.
Robert Wu - Caltrans - Comments sent via e-mail August 8, 2008			
RW-1	Section	Comment	
RW-2	2	Please add a discussion of illicit discharges to the LA River and reference any studies or estimates of the presence, number, and potential loading from these sources.	From our perspective, illicit discharges fall into two categories: those that discharge via outfalls on the side of the channel and those that don't. Those that discharge via outfalls are captured with snapshot monitoring . Those that occur directly to the river would be "in-channel". We added illicit discharge as a potential in-channel source, along with a note to Section 2.2.1 re in-channel inputs: "In-channel inputs characterized as illicit discharges would be inputs directly to the LA River, as opposed to through an outfall connected to the channel. For example, an RV could discharge waste from a bridge. No data regarding illicit discharges are known to be available."
RW-3	3-4 to 3-5	In these sections, enterovirus and adenovirus are referred to as source tracking assays. May be more accurate to refer to these activities as direct pathogen monitoring.	For simplicity we prefer to refer to all of the PCR assays as "source tracking". In addition, ultimately, the goal of the pathogen testing was to indicate human fecal impacts, which is source tracking.
RW-4	3	Table 3-7 Please keep the table heading attached to the rest of the table.	Fixed.
RW-5	3.8.1	In Clean Sampling Techniques, please discuss the sample collection containers and the steps taken to ensure these are properly sterilized.	All sample containers were disposable, never reused. Noted.
RW-6	3.8.6	Please discuss why field blanks were not collected for the WRP monitoring	Travel blanks were collected to simplify the efforts of WRP personnel. Noted.
RW-7	4	Please add a general discussion of how the IDEXX system works to calculate the concentrations of fecal pathogen indicators.	Details were added to Section 4.1.
RW-8	4	Please discuss what results in the IDEXX laboratory analysis were considered positive and which negative. What was the standard used to make the determination? What quality control measures were inacted to ensure consistency in reading the results?	Details were added to Section 4.1.
RW-9	4	After what period of time were the IDEXX trays removed from the incubator and the results read?	Details were added to Section 4.1.
RW-10	4 and 5	Please reorganize and rename Sections 4 and 5 to clarify the differences in the information being presented. A suggestion (as presented in Chapter 1 of the report) is to concentrate solely on the laboratory methods and materials in Section 4 and to discuss quality assurance/quality control and the results in Section 5.	The organization reflects that which is presented in Chapter 1. Section 4 discussed lab methods. Section 5 discussed QA/QC results.
RW-11	5.2.2	Please discuss the potential causes of contamination of the Bacteroidales Field Blanks. Please discuss both potential sources in the field and also during laboratory analysis. How can these be eliminated for future monitoring?	Done.

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Comment #	Section	Comment	Response
RW-12	8-2	On page 8-2, under the section, Dry Weather Urban Runoff Water Quality, the report states that when <i>Bacteroidales</i> concentrations were high, concentrations of <i>E. coli</i> were also high, but that the inverse was not true. Please discuss the significance of these results.	Done. The reader is referred to Section 7.1.2.1, where this issue is discussed in detail.
RW-13	8	Tables 8-4 and 8-7 Are these sites listed based upon their ranking according to the weight of evidence approach?	No, they are simply listed from upstream to downstream. Stakeholders will need to rank the different sites based on their perspectives. A footnote was added to clarify.
RW-14	8 and 9	The mass-balance calculations discussed in section 8 and the in-stream sources discussed in Section 9 refer to unaccounted for sources. The in-stream sources should also include illicit discharges, wildlife, and additional SSOs.	Illicit discharges were added as a potential source to Section 9.
RW-15	9 and 10	Section 9 states that there may be several reasons why a relationship between storm drain outfall microbial water quality and land use characteristics could not be correlated. The recommendations for future efforts may wish to include conducting a more detailed evaluation of the relationship between storm drain outfall microbial water quality and land use.	The effect of land uses is an important consideration, but our approach is to limit the follow-up studies those considered the highest priority.
RW-16	9-2	The last sentence of the first paragraph states that "Dead cells and free DNA fragments likely decay very quickly in the environment." Please include a reference for this statement.	Instead, we just removed the sentence.
RW-17	10	Why is the evaluation of wet weather conditions included as a recommendation for future efforts if the uses are suspended during high-flow conditions? As discussed earlier in this report, wet-weather monitoring is highly variable and can require an intensive program to develop an understanding of the sources and processes involved. Please describe what information is recommended to be collected.	More language added. Note that uses are not suspended during rainfall events less than 0.5-inches, which is most of the storms that intersect the watershed.
RW-18	10	Please add the identification and evaluation of illicit discharges to the LA River as an additional recommendation for future efforts. Illicit discharges could potentially account for a significant load of pathogens to the river.	Illicit discharges were added to the recommendation re: identifying sources of unaccounted-for loading
RW-19	10	Under the section, Identify Sources of Unaccounted-for <i>E.coli</i> Loading in Reach 2, an evaluation of the survival and die-off of <i>Bacteroidales</i> could also be a useful tool.	The laboratory at UC-Davis is currently conducting studies of <i>Bacteroidales</i> survival, so this was not included as a recommendation.