Peer Review Comment Summary and Responses
Dominguez Channel and Greater Los Angeles and Long Beach Harbor Waters Toxic Pollutants TMDLs

Table 1. Peer Reviewers

1. Arturo Keller, Professor, School of Environmental Science and Management, University of California, Santa Barbara
2. Patrick Brezonik, Professor Emeritus, Department of Civil Engineering, University of Minnesota

Table 2. Comments and Responses

No.	Author	Comment	Response
1	Arturo Keller, University of California, Santa Barbara		
1.1	pdf p. 1	Given the nature of this TMDL and its complexity, a large amount of information is needed to make a scientifically sound determination of the total maximum daily load for each of these pollutants, and the subsequent allocation to the various point and non-point sources. However, it is clear from the documents provided that the information available is rather limited, and in some cases insufficient to make a scientifically valid estimate. The large data gaps, to be discussed in more detail below, result in significant uncertainty in the determination of the TMDLs. Although this is sometimes acknowledged in the documents, the assessment of the actual uncertainty is inadequate. The proposed margin of safety is unlikely to be sufficiently protective, and may result in continued nonattainment of the beneficial uses.	Comments noted and responded to in detail, below. Staff agrees that the Dominguez Channel and Greater Harbor Waters TMDL is complex and that uncertainty in aspects of the TMDL calculations exists; however the TMDL must be established per a court order by March 2012. TMDLs are required to be based on the best *available* data, and staff has utilized all available information in the development of this TMDL. Further, in recognition of the uncertainties in this TMDL, the TMDL provides opportunities to collect additional data and conduct special studies during the 20-year implementation period, and then reconsider the TMDL to incorporate this additional information. Also, the TMDL incorporates both implicit and explicit margins of safety, consistent with previous TMDLs adopted by this board.
1.2	pdf p. 1	Another important issue is the assumption that these various toxic pollutants do not have any synergistic or antagonistic effects. The numerical targets have been determined based on the individual toxicity of each pollutant. However, it is quite likely that the organisms that will be exposed to these pollutants as a complex mixture will not be adequately protected by the individual numeric targets. The toxicological information simply does not exist to make an accurate determination of numeric targets that would take into account the	The TMDL does not attempt to develop site specific targets by assessing synergistic or antagonistic effects directly, but has not assumed that the chemicals in question have no synergistic or antagonistic effect. There are likely some synergistic toxic effects. The requirement to monitor and assess using the triad approach of the State Sediment Quality Objectives (SQO),

No.	Author	Comment	Response
		temporally-varying nature of the complex mixture of pollutants. Therefore, instead of assuming that there is no potential synergistic effect, an additional margin of safety for the numeric targets would result in a more protective TMDL and should thus be considered. While precedent in other TMDLs may have led others to assume that there are no synergistic effects, a risk assessment of this nature should be conservative and thus assume that there are likely some synergistic toxic effects, when an aquatic organism is exposed throughout its entire life to several metals and a cocktail of toxic organic pollutants.	which includes sediment toxicity tests and benthic community analyses, ensures that potential synergistic effects are identified. In addition, this TMDL provides opportunities for site-specific studies to be conducted in order to support the development of new, site-specific, targets that would take into account the temporally-varying nature of the complex mixture of pollutants in the sediments of the Dominguez Channel or Los Angeles River estuaries or in the sediments of the Greater Harbor Waters. Furthermore, while the numeric targets are for the individual chemicals, these targets are based on guidelines which were developed with effect-related field data. The Effects Range Low (ERL) guideline is the 10th percentile value indicative of the concentration below which adverse effects rarely occur. The toxicity predictive ability of ERLs has been tested in the field and when several ERLs are exceeded, the predictive ability is greater. Finally, the TMDL includes both implicit and, for some waterbodies, an explicit margin of safety that offsets the uncertainty in some of the calculations relied upon in the TMDL. These implicit and explicit margins of safety are consistent with the board's approach in other TMDLs addressing similar types of impairments.
1.3	pdf p. 2	**Appropriateness of the selected sediment, fish tissue and water numeric targets for pesticides, PCBs, PAHs and metals.** In assessing the impairment a number of water quality, sediment and fish tissue observations were considered (Table 2-8). However, in most cases little or no information is given in the Draft TMDL for each dataset reviewed with regards to the number of observations considered,	The development of this TMDL relied on a great deal of data; it would have been unwieldy to include entire datasets in the staff report. However, all of the data are part of the Administrative Record and have been available either on the board's website or upon request to stakeholders. These data include the Ports of Los Angeles and Long Beach

No.	Author	Comment	Response
		the number of exceedances, and a sense of the magnitude, frequency and duration of the exceedances. The best example is Table 2-16, but most other tables are lacking in this important information. The information on the magnitude, frequency and duration of the exceedances could be provided within the text, to put into context the magnitude of the impairment. It is important to know whether the objectives are always exceeded, frequently exceeded or only during very short periods; whether the short periods are frequent or only once in a decade; or whether the exceedance is 10% above the objective or 200%. For many of the datasets reviewed, little or no information is actually provided (e.g. 2.4.3.1, 2.4.3.3, 2.4.3.4, 2.4.3.5); they are essentially just mentioned but with no analysis. Given that the POLA & POLB 2006 sediment survey (2.4.3.5) is apparently of high quality, it would have been extremely useful to provide a detailed analysis. Same for the SCCWRP 2006 (2.4.3.6) study. This is a clear example of important information omitted from the Draft TMDL. It should at least be provided in an appendix, but a serious scientific report would have included a detailed analysis of this information within the main text. The level of credibility decreases when the information and its analysis are not provided. The summary provided in section 2.5 is inadequate, due to its lack of specificity.	2006 sediment survey data, which has been available to stakeholders on the board's website since 2007. Additionally, nearly all of the data has been reviewed during the CWA section 303(d) listing process and is also a part of the administrative record for the listing decision. Much of the sediment chemistry results included in the TMDL are provided in Appendix III. Water and fish tissue results are summarized in Staff Report, Section 2. Because these results are publicly available via other reports, staff did not repeat them in the TMDL Appendices. Section 2.5 is a short summary section and not intended to repeat all the information provided above.
1.4	pdf p. 2	Monitoring data for some individual PAHs is available (e.g. Table 2-12). However, criteria are based on either Total PAHs or benzo[a]pyrene (e.g. Table 2-3), which does not adequately reflect the toxicity or bioaccumulation of individual PAHs. The State of California explicitly considered in the 2006 303(d) listing for these waterbodies the individual PAHs as opposed to the general category of PAHs, yet this is not reflected in the assessment of NTs.	The data available for 303(d) listing and TMDL development included, in some cases, total PAHs and in some cases, multiple, individual PAHs. Some monitoring data for individual PAHs in water is available. Table 2-12 shows water data in Consolidated Slip. This data was collected with an in-situ, high volume pump to obtain high sample volumes as concentrations of PAHs in water are typically so low as to be difficult to measure. For water, PAH targets include only benzo(a)pyrene. CTR human health criteria were not established for total PAHs. Therefore the lowest CTR criteria for an individual PAH of

No.	Author	Comment	Response
			0.049 µg/L is applied for the sum of benzo(a) anthracene, benzo(a)pyrene, chrysene, phenanthrene, pyrene, and 2-methylnaphalene. Other PAH compounds in the CTR will be screened as part of TMDL monitoring. For fish tissue, bioaccumulation targets are set as fish tissue targets for total PAHs. Sediment targets include individual PAHs and High Molecular Weight PAHs, Low Molecular Weight PAHs, and total PAHs. The State of California did begin, in 2006, to make listing decisions based on individual PAHs instead of "total" PAHs. In fact, for the 2008-2010 303(d) list, chrysene and benzo (a)pyrene were added to the 303(d) list for Los Angeles and Long Beach Inner Harbor (sediment data). The 2008-2010 303(d) list includes listings for both total PAHs and individual PAHs in these waters.
1.5	pdf p. 3	It is difficult to understand how a regulatory agency in California would allow an NPDES discharger to report the concentration of toxic pollutants using analytical methods that do not have adequate detection limits to assess whether in fact the discharge meets the objectives (Section 2.4.3.2). What is the point of allowing NPDES dischargers to report the required information based on a method of analysis that is useless for the intended use of the information?	The data considered from the refineries discussed in Section 2.4.3.2, is from 1994 to 2004, which is over a period of time during which there were several different permits for the refineries. Permit requirements are updated when the permit is renewed, typically on a 5-year schedule. Sediment quality guidelines have not historically been part of the refineries' permits. This TMDL will set targets and allocations that will be applicable to the refineries' permits. In some cases, water quality objectives and thus TMDL numeric targets and allocations are established below levels that can be detected with readily available laboratory methods. Monitoring requirements in this and other TMDLs and those incorporated into permits to implement TMDLs require that dischargers incorporate new methods and/or detection limits in their MRP and QAPP as analytical methods and detection limits improve (i.e., development of lower detection limits).

No.	Author	Comment	Response
1.6	pdf p. 3	With respect to the Numeric Targets, as stated in the document (page 43), they should be “guided by the Basin Plan and the California Toxics Rule (CTR)”, but in order to use the most current scientific knowledge one should look into more recent studies. The authors have taken the rather conservative approach of using the CTR for most of the numerical targets, without considering more recent information. This is particularly concerning in those cases where the CTR provides no information. For example, in Table 3-1 there are several numerical targets which are indicated as n/a or ‘-’ (which is confusing to the reader, since n/a is not defined and it is not clear what the difference is between n/a and ‘-’). There is no mention of an effort to review other studies or sources of information that may be used to establish Numeric Targets for these pollutants.	While science continues to develop and the Regional Board will update and improve standards in the Basin Plan or be responsive to new guidance or rules from EPA, the CTR represents a very thorough, scientifically valid, toxicity-based, set of criteria. Staff has no evidence that other targets are necessary to protect beneficial uses given the documented impairments being addressed by this TMDL. Furthermore, a target is established for toxicity in water. If toxicity is observed, additional sampling and analysis may be required to determine the cause of the toxicity. If pollutants other than those regulated by the TMDL are identified as the cause, then the TMDL may be reconsidered to include additional numeric targets and allocations at that time. Regarding the CTR criteria, numerous variables were taken into account in the derivation of both the human health criteria and aquatic life criteria in the CTR. These variables included diverse toxicity studies (resulting in, for example, reference doses or cancer potency estimates or determination of chronic and acute toxicity) that, along with exposure factors and determination of risk level, and taking into account how other factors (such as pH, temperature or hardness) affect toxicity resulted in the criteria. The CTR, itself, was subject to peer review and public comment. The reviewer has not identified studies which show that the CTR criteria may be insufficient to protect beneficial uses. Table 3-1 has been modified to provide an explanation of ‘n/a’ and ‘-’ n/a indicates that no criteria were available in CTR and the “-“ indicates that no TMDL targets were established for that contaminant.

No.	Author	Comment	Response
1.7	pdf p. 3	While it useful to list the CTR values for acute, chronic and organism only (human health), the document should be explicit as to which CTR value has been designated as the Numeric Target. One cannot use three different values for one pollutant in a given water matrix (freshwater or seawater). Therefore, Table 3-1 should be simplified, presenting only the specific Numeric Target for each pollutant in each type of water.	This TMDL covers multiple waterbodies, including fresh and salt waterbodies, and addresses multiple beneficial use impairments that have been documented over different exposure timeframes (i.e. acute and chronic). As a result, depending on the waterbody, the impairment being addressed, and the exposure timeframe of concern, different numeric targets for the same constituent may apply. Table 3-1 presents all relevant numeric targets for chemical constituents in water that are utilized in the TMDL. Permit limitations, as the TMDL is incorporated into permits, may include multiple limitations to address impacts from different exposure timeframes or, perhaps, the lowest of the criteria, as supported by the administrative record in the development of the permit.
1.8	pdf p. 3	The list of Numeric Targets for water (Table 3-1) is incomplete, given the scope of the TMDL. As indicated above and in Table 2-18, there are several other waterbody-pollutant combinations that require a TMDL to be developed, which are not included here, such as several individual PAHs (e.g. pyrene, chrysene, etc.), dieldrin, toxaphene, Cd, and Cr. Thus, the Numeric Targets are incomplete. There is no explanation for the omission, and in fact the text at the beginning of page 43 indicates that the intent was to consider all of these compounds. Also, in Table 3-1 staff explicitly designates a Numeric Target for just 4,4'-DDT. However, it is likely that the transformation products of DDT, namely DDD and DDE, are also present in the sediments and water column, and may be of concern. Either one considers each explicitly, or as the sum of DDT compounds, which is generally considered to be DDT + DDD + DDE.	For water, PAH targets include only benzo(a)pyrene. CTR human health criteria were not established for total PAHs. Therefore the lowest CTR criteria for an individual PAH of 0.049 µg/L is applied for the sum of benzo(a) anthracene, benzo(a)pyrene, chrysene, phenanthrene, pyrene, and 2-methylnaphalene. Other PAH compounds in the CTR shall be screened as part of TMDL monitoring. For Cd, Cr, dieldrin and toxaphene, no impairment for freshwaters has been established so it was not necessary to establish targets for those compounds in this TMDL. For the saltwater waterbodies, the data establishing these impairments was sediment data, consequently targets for these compounds have been established for the saltwater sediments but not waters.
1.9	pdf p. 3	Staff also used a "translator" to adjust the Numeric Targets for three metals, to account for water hardness. While the text provides an indication of the rationale for selecting the conversion factors, the	As described in Staff Report, USEPA Guidance offers three options to develop the metals conversion factors. Option 1 is preferred method and we selected the 90% value for each

No.	Author	Comment	Response
		calculations are not provided within the document or the appendices. It is important to present this calculation somewhere within the document or appendices, so that the method can be reviewed. There is also no explanation for the selection of only the acute values for this calculation. A likely explanation is the short residence time of the metals in the water column in freshwater bodies within this region, but this assumption should be made explicit.	metal so there is no calculation to present. Options 2 & 3 rely on correlation between TSS and metals; no significant correlations were observed in the analysis of site-specific data. Calculations are provided and explained in the Translator Guidance. The acute metals criteria were selected since elevated stream flows coincident with wet weather events last only one day within Dominguez Channel watershed.
1.10	pdf p. 4	For Water Toxicity, staff defines the use of the Toxicity Unit Chronic. While this is adequate, there is no mention of the methods that will be used to determine toxicity. Specific testing protocols/assays should be defined, so that it is not ambiguous and subject to interpretation. In the case of Sediment Toxicity, staff clearly defines the organisms to be used and the specific criteria for interpreting the test results (Table 3-4, p. 48). A similar approach should be used for water toxicity.	The responsible parties will use USEPA approved methods for water toxicity. Additional information is provided for sediment toxicity testing because, testing protocols for water toxicity have been in common use for some time; however, the sediment toxicity method and test species is relatively new and only briefly described in the State's SQO Policy – Part I.
1.11	pdf p. 4	The sediment concentration Numeric Targets are based on the sediment quality guidelines of Long and MacDonald (1995 and 2000). The use of the Effects Range Low and Threshold Effects Concentrations is scientifically valid, since as noted by staff, these are more applicable to the prevention of impairment, which is the objective of the TMDL. However, the application of these sediment numeric targets is inconsistent. For the toxic organics, Numeric Targets are set only for Marine Sediment. As will be established later in the Draft TMDL document, many or all of these toxic organics are still present in freshwater sediments which are transported through the various freshwater bodies to the harbor waters. Therefore, a Numeric Target should also be set for the freshwater sediment. In the absence of toxicity data (if indeed none is available), the default should be the marine sediment Numeric Target, such that the sediments delivered to the harbor do not enter at concentrations that will continue to impair these waters. For clarity, in Table 3-7 the labels "TECs" and "ERLs" should	Staff agrees use of the ERLs and TECs is scientifically valid. While upstream, freshwater, sediment targets have not been set, the Waste Load Allocations (WLA) for the upstream dischargers have been set to support the sediment targets in the estuaries and Greater Harbors waters. For clarity, in Table 3-7 and in the target table in the Basin Plan Amendment the labels "TECs" and "ERLs" have been removed.

No.	Author	Comment	Response
		be removed. These are now Numeric Targets, and there should be no confusion with other terminology.	
1.12	pdf p. 4	For the EFDC modeling effort (Appendix I), partitioning or distribution coefficients were determined for seawater in contact with the marine sediments, based on a comparison of observed concentrations in seawater and sediments. These provide a solid scientific basis for establishing the concentration of toxic pollutants in the seawater that will be in equilibrium with the concentration in marine sediments. This information should be used to determine whether in fact the marine sediment Numeric Targets are in concordance with the seawater Numeric Targets. If this is not the case, then achievement of one of the targets may not be feasible, since there will be continuous exchange between these two environmental compartments. The partitioning coefficients could also be used to develop seawater Numeric Targets for those PAHs and pesticides which were not listed in Table 3-1.	The EFDC model used locally determined distribution coefficients for organic compounds only. The partitioning study utilized sediment, porewater and overlying water results to estimate these distribution coefficients. -It would be best to enhance this study with additional samples and diverse analytical techniques to verify results before applying them to determine whether water/sediment targets agree with CTR.
1.13	pdf p. 4	The use of Fish Contamination Goals (FCGs) for fish tissue Numeric Targets (Table 3-8) is scientifically valid, since the FCGs have been based on scientific knowledge. However, Table 3-8 lists the associated sediment "targets", which in two cases are higher (less protective) than the Numeric Targets presented in Table 3-7 (e.g. chlordane and Total DDT), and in one instance is below (Total PCBs). This will lead to unnecessary confusion, since there shouldn't be two (or more) targets for a pollutant in a given environmental compartment, in this case sediments. The Draft TMDL document should be clear as to which one of the values IS the Numeric Target (either the one in Table 3-7 or Table 3-8). In addition, since there are Numeric Targets for dieldrin, and the PAHs in Table 3-7, the notation "n/a" is terribly confusing. How can it be that there are Numeric Targets in one table for these pollutants, and not in the next table?	Staff agrees that the use of FCGs for fish tissue targets is scientifically valid. The additional targets will be useful in the implementation of the TMDL. The responsible parties are required to achieve the lower of the two targets, *sediment* (ERL-based) or *fish tissue-associated sediment* targets unless the fish tissue target, itself, is met in fish. If the target is met in the fish themselves then the higher target is sufficient. The Staff report has been modified for clarity to address confusion regarding the notation "n/a".
1.14	pdf p. 5	The Numeric Targets for tissue residues are based on scientific knowledge.	Staff agrees.

No.	Author	Comment	Response
1.15	pdf p. 5	Overall, the document as currently written is confusing as to the specific Numeric Targets for water, sediments and tissues. Staff should separate the presentation of the underlying toxicity values (acute, chronic, human based on organism; sediment ERLs and associated sediment targets for fish tissue) from the final presentation of the Numeric Targets, which should be one value for a pollutant-matrix combination (i.e. pollutant-freshwater, pollutant-seawater, pollutant-freshwater sediments, pollutant seawater sediments, pollutant-fish tissue, pollutant-tissue residues). In addition, partitioning coefficients (e.g. sediment-water, fish tissue-water, fish tissue-sediments) should be considered to ensure that there is consistency in the various combinations of Numeric Targets.	As discussed in the responses above, the Staff Report and Basin Plan Amendment will be revised for clarity.
1.16	pdf p. 5	**Appropriateness of the selection of the numeric models to estimate load capacity and load reductions** Implementation and calibration of the LSPC models for the LAR, SGR, and DC watersheds was not provided. Therefore, this review can only provide an assessment of the scientific appropriateness of the implementation of the EFDC and LSPC model for near shore watersheds.	The LSPC models for the LAR and SGR are described in Tetra Tech, Inc., 2004 and 2005a, while the HSPF model for the DC watershed is documented in SCCWRP, unpublished results. These documents are cited in Appendix II. TMDLs have already been developed in the LAR and SGR watersheds; therefore, all associated documentation, including model reports discussing implementation and calibration, have undergone peer review.
1.17	pdf p. 5	Source assessment is a very important component of the linkage analysis. Section 4.1 essentially lists or mentions the point sources with NPDES permits. However, after more than 10 pages of generic descriptions, no specific information is provided on the results of monitoring by these important sources. Information is provided about some of the difficulties in monitoring. For example, one learns that the Los Angeles County stormwater monitoring has been of no use to date since they are using analytical methods with insufficient sensitivity to detect the pollutants of concern. Thus, even though taxpayer or ratepayer resources are being used to monitor these waters, the information cannot be used at all. The omission of NPDES monitoring results reduces the credibility of this document.	The data collected by the monitoring programs of the permitted dischargers is discussed in the Problem Statement and assessment sections of the Staff Report. The stormwater monitoring data has been of use for permit compliance and water quality assessment in waterbodies to which stormwater is discharged. The data collected by the permitted dischargers was collected over various periods of time during which there would have been multiple different permits. Permit requirements, including stormwater permit monitoring requirements, are updated when the permit is renewed, typically on a 5 year schedule. Sediment quality guidelines were established in

No.	Author	Comment	Response
			2009. This TMDL will set targets and allocations which will be applicable to the responsible parties' permits.
1.17	pdf p. 5	As indicated in the report, these watersheds include some highly industrialized sections. In particular, the area around the ports includes several heavy industry facilities. There does not appear to be any consideration of the difference in types of industry in the source assessment. An acre of light manufacturing (e.g. clothing) is considered the same as an acre of heavy manufacturing (e.g. refinery). This assumption is not supported by any evidence that suggests that there is no difference in stormwater quality surrounding these different types of facilities.	Different types of industrial facilities will have the potential to generate stormwater of different quality. A regional watershed modeling approach was used to simulate hydrology, sediment, and metals transport in the TMDL watersheds. The regional modeling approach assumes that loadings can be dynamically simulated based on hydrology and sediment transported from land uses in a watershed. Development of the approach resulted from application and testing of models for multiple small-scale land use sites and larger watersheds in the Los Angeles Region. The land use data used to represent the nearshore areas was the Southern California Association of Governments (SCAG) 2000 land use. Although the multiple categories in the land use coverage provide much detail regarding spatial representation of land practices in the watershed, such resolution is unnecessary for watershed modeling if many of the categories share hydrologic or pollutant loading characteristics. Therefore, many land use categories were grouped into similar classifications, resulting in a subset of eight categories for modeling: agriculture, commercial, high-density residential, industrial, low-density residential, mixed urban, open, and port activities. Selection of these land use categories was based on the availability of monitoring data and literature values that could be used to characterize individual land use contributions and critical metal-contributing practices associated with different land uses.

No.	Author	Comment	Response
			A regional watershed modeling approach and the land use information used in the development of this TMDL was sufficient to develop an appropriate TMDL.
1.18	pdf p. 6	The summary of results for the point sources (Table 4-2) is severely lacking in any detail that helps to determine the magnitude of the fluxes of the pollutants of concern. The last column reports on the "Potential for significant contribution", but there is no information in the entire document that supports this assessment. The lack of transparency is not scientifically adequate.	Table 4-2 describes overview information pertinent to various types of NPDES permits in Dominguez Channel watershed. "Potential for significant contribution" is based on professional judgment on type of discharges and associated potential pollutants may be carried by the discharges.
1.19	pdf p. 6	The assessment of direct atmospheric deposition is an interesting analysis, in that there is an attempt to link the emitters to the atmosphere to the watersheds where they operate, at least for three of the metals. However, airsheds and watersheds don't have the same boundaries. An emitter just outside a watershed may contribute significantly to the actual deposition in the watershed. While this may be captured in the more general deposition analysis, it may be better from a scientific perspective to determine what the potential radius of influence is for major emitters to the atmosphere that are in the vicinity of these watersheds. The estimated atmospheric deposition presented in Table 4-5 appears to be based on sound scientific knowledge and methods. The only issue is that it does not include PCBs and the pesticides that are also part of the TMDL, and is limited to three metals. Thus, the analysis is incomplete.	The understanding of the contribution of air emitters to water quality is developing in both science and regulation. The work that needs to continue in air deposition can include further analysis such as determination of the potential radius of influence of the emitters and the contributions of other pollutants.
1.20	pdf p. 6	The assessment of the loads in the freshwater bodies is based on model output from LSPC. Given the concerns with the model calibration discussed below, there is low confidence in these estimates. The estimates have a large uncertainty associated with them, which is not evaluated anywhere in the report. A table in Section 4.3.1 should provide the estimates, and a more thorough analysis of the loads (temporal variations, contribution from different regions, estimate of the uncertainty, evaluation of assumptions)	While model uncertainty is not explicitly calculated, sensitivity analyses were performed for the nearshore watersheds using the limited available data and are presented for the simulated pollutants. Estimates of watershed loads and details associated with these loads (temporal variations, contribution from different regions, estimates of uncertainty, and evaluation of assumptions) are presented in various sections of Appendices

No.	Author	Comment	Response
		should be included within the main TMDL document. In fact, this level of analysis is not available anywhere within the documents provided.	I and II. TMDL models are based on publically available code. EFDC and LSPC model output information is available for additional analysis; thus stakeholders can continue to explore these topics. Section 4.3.1 was developed to provide a brief summary of the process used to calculate loads, while referencing particular sections and appendices for additional detail.
1.21	pdf p. 6	The assessment of the amount of pollutant present in the marine sediments is based on EFDC model output. Again, based on the major concerns with model calibration discussed below, there is low confidence in these estimates. Use of EFDC model output introduces considerable uncertainty in the calculation, and this uncertainty has not been evaluated or taken into consideration. The estimates presented in Table 4-6 are given with an apparent high level of precision, in some cases 7 significant digits. In reality, these estimates can only be given with 1 or 2 significant digits; the data should be presented in scientific notation and only to the level of precision justified by the uncertainty in the estimate. Otherwise one is misrepresenting the precision. It is also unclear as to whether the estimated loadings presented in Table 4-6 represent the mean value from 2002 to 2005, or the final value at the end of the simulated period (2005). In any case, this information does not reflect the current concentrations in 2010 or 2011. Given the significant bias in model output, observed data would provide a better estimate of the pollutants present in the marine sediments. Since the model provides output as of 2005 (simulation is from 2002-5), it is more dated than the 2006 and 2007 studies that collected observed data. As indicated before, these estimates do not cover all the metals and toxic organics which have been identified, so the analysis is incomplete.	While model uncertainty is not explicitly calculated, sensitivity analyses were performed for the receiving water simulations for both dry season (Appendix I.C) and long term load reductions (Appendix I.D). While model estimates do contain some level of uncertainty, staff find it is more appropriate to give values with as many as 7 digits as a means of showing our work/calculated answer. The estimated loadings in Table 4-6 represent the mean modeled value from 2002-2005. This will be clarified in the table title. The model simulations did not extend through 2010-2011. The LSPC watershed modeling was completed in 2006 – thus the modeling period went through 2005 and incorporated the available data to date at that time. The modeling period for the EFDC receiving water model was based on the watershed modeling period since this output was required as EFDC input. The 2006 observed data became available later in the modeling process. To complete the technical aspects of these TMDLs with finite resources, the final modeling period was not adjusted to include these data for calibration/validation (rather they were used to represent initial bed conditions to improve sample size and

No.	Author	Comment	Response
			monitoring station distribution). The data used were sufficient to develop an appropriate TMDL. If the model is updated for a later re-consideration of the TMDL, data from more recent years can be included.
1.22	pdf p. 6	Section 4.4 (Sources Summary) indicates that the major sources of metals are stormwater and urban runoff. Since no information was provided previously about the contribution from NPDES dischargers, this statement is not supported by the evidence. The statements also are restricted to the Dominguez Channel freshwater, but in fact there are contributions from other major watersheds (LAR and SGR), which are not discussed in any meaningful detail. The summary also indicates that there are a number of activities that contribute pollutants to the harbor, and in particular discusses the “re-suspension of contaminated sediments from propeller wash”. While this is a valid source, it was not discussed in the previous analysis, and there is no additional information provided here, so it is incorrect to bring up additional sources at this late stage with no justification. If the section intends to highlight those activities that were not considered, that should be made more explicit. One should then add that dredging is likely an important activity that was not considered in the assessment. It is also odd and confusing to be referred in this section to two tables in a later section of the report (Tables 6-9 and 6-11); that is poor scientific writing. Those tables present Waste Load Allocations, and do not thus pertain to the source assessment. Overall, the source assessment does not present sufficient information for a correct assessment of the sources, and relies too heavily on very uncertain modeling results, as discussed below.	The source assessment summary is a section to summarize the previous sections, thus it provides general information. See also response 1.18. Commenter’s statement regarding “re-suspension of contaminated sediments from propeller wash” is noted and the TMDL staff report has been modified.
1.23	pdf p. 7	The land use dataset used is somewhat dated (2000), but more importantly there is no distinction between different types of industrial activities. As indicated above, the emissions from heavy industry will be quite different than those for medium and light industry. If the same approach was used for the LAR, SGR and DC, that would introduce considerable uncertainty in all these models. There should also be a consideration of known hotspots that may be contributing more than the	The LSPC watershed modeling was completed in 2006 – thus the modeling period went through 2005 and incorporated the available GIS data at that time, including the 2000 land use data (2005 or 2008 data were not available). The grouping of the land use categories was based on available monitoring data and literature values that could be used to represent the land use category in the model. The

No.	Author	Comment	Response
		average load of a given pollutant.	potency washoff factor (POTFW) values were obtained from previous SCCWRP studies. Because the POTFW values are only available for certain land uses categories, all modeled land use groupings must be assigned one of the POTFW values. Model POTFW parameters were available for only a single industrial category; therefore, heavy industry could not be parameterized separately from other industrial uses. In addition, observed pollutant and flow data associated with heavy industry were not available for individual calibration of model parameters. Overall, this is consistent with the regional modeling approach (see **Comment 1.24**) that was applied to LAR, SGR, and DC. Similarly, data associated with known hotspots were not available during model development. If such data become available, and the modeling is updated for a reconsideration of the TMDL, the LSPC model can be updated to include these known sources.
1.24	pdf p. 7	It is important to mention that the information provided in the TMDL document is very incomplete with regards to the implementation and calibration of the LSPC model for the near shore watersheds, so the comments below refer to the information provided in Appendix II. The lack of transparency in the TMDL document with regards to the relatively poor calibration of the model is not acceptable scientific practice.	Previous wet weather watershed modeling and TMDL efforts have led to the development of a regional watershed modeling approach to simulate hydrology, sediment and metals transport in the Los Angeles Region. This approach was used to estimate loadings from the nearshore watersheds, as well as the Los Angeles River, San Gabriel River, and Dominguez Channel drainage areas. The modeling approach assumes that metals loading can be dynamically simulated based on hydrology and sediment transported from land uses in a watershed. The potency wash off factors (POTFW) used in the wet weather modeling analysis were originally developed by the Southern California Coastal Water Research Project (SCCWRP). For the nearshore watersheds, limited data were available to determine model parameters associated with the Port Activities land use – this category was unique to the nearshore watersheds and these activities are not found in the

No.	Author	Comment	Response
			Los Angeles River, San Gabriel River, and Dominguez Channel watersheds. Data available for this calibration/validation process were extremely limited for a few locations and were not robust enough to conduct calibration and validation at each site using data from different date ranges. Given the limited quantity of the data available for the Port Activities land use, further calibration and validation could not be performed without adjusting some parameter values previously calibrated in the LAR watershed outside of the recommended range. Overall, there were not enough data to justify refinement of the calibrated and validated parameter values associated with the regional modeling approach. Documentation of this calibration process for Port Activities is provided in Appendix II and documentation associated with the regional modeling approach for the other land uses is provided in other documents (referenced in Appendix II: Ackerman et al., 2005a; SCCWRP, 2004; Tetra Tech, Inc., 2004 and 2005a).
1.25	pdf p. 7	The original LSPC (and underlying HSPF) model is capable of handling in a continuous simulation both dry and wet weather conditions. Since there can be significant accumulation of pollutants on the landscape of these watersheds, and the antecedent soil moisture conditions play an important role in the hydrologic response, the current approach where the wet weather is simulated separately from the dry weather deviates from the original model assumptions. No evidence was provided that this approach (separating dry and wet weather) is scientifically better in terms of the representation of the system.	Separate wet and dry weather approaches to characterize pollutant loading is consistent with other TMDLs adopted in the Los Angeles Region (Santa Monica Bay bacteria TMDLs, metals TMDLs for LAR and SGR, etc.). In addition, parameters associated with the regional modeling approach (see **Comment 1.24**) were developed to represent wet weather conditions; therefore, a separate dry condition approach was necessary.
1.26	pdf p. 7	Simple visual comparison is insufficient for determining whether the simulated response adequately reflects the true system. However, in all cases (hydrology, sediment transport, and pollutant transport) the approach used for the LSPC modeling was based on this inadequate	Visual comparison is a common approach for evaluating model results as it provides an indication of whether the model is predicting the general magnitude and timing of flow as well as pollutant concentrations and loads. This type of

No.	Author	Comment	Response
		visual comparison. In all cases, it appears that only one storm event within the 10-year period of simulation was actually used for the calibration of hydrology, sediment transport, and pollutant transport. This is a very limited basis for calibration. In addition, the "calibration" was done only at one location, and then it was "validated" at two other locations. While the text in Appendix II seeks to lead the reader to believe that the calibration results are "well within acceptable modeling ranges", the reality is that most of the simulated results are poor representations of the observed values, for hydrology, sediments and pollutants. More significantly, the worst match is for the location with the highest flow and loads, that is the one that is most significant. Thus, the credibility of the results presented in the TMDL report in Tables 5-1 and 5-2 is low for the near shore watersheds. If the same poor fits were obtained with the LAR, SGR and DC watershed models using LSPC, then the linkage analysis for this section is not scientifically acceptable. However, the TMDL report does not provide sufficient information to make this determination.	comparison was used as well as some simple comparative statistics in tabular format, which is consistent with many other TMDLs in the region and nationally. As noted in **Comment 1.24**, Appendix II only presents model calibration and validation associated with the Port Activities land use as results for all other land uses have been presented in other documents (referenced in Appendix II: Ackerman et al., 2005a; SCCWRP, 2004; Tetra Tech, Inc., 2004 and 2005a). Calibration and validation of the other land uses was part of the regional modeling approach (see **Comment 1.24**). Given the limited quantity of the data available for the Port Activities land use, further calibration and validation could not be performed without adjusting some parameter values previously calibrated in the LAR watershed outside of the recommended range. While there are discrepancies between the modeled and observed values, overall, there were not enough data to justify refinement of the calibrated and validated parameter values associated with the regional modeling approach.
1.27	pdf p. 8	The linkage analysis for the freshwater loads also does not consider the entire list of pollutants. Thus, the analysis is incomplete. Since this information is the basis for the EFDC model, it introduces a significant amount of uncertainty in the harbor model, since the loads into the harbor are not adequately simulated. If a scientifically defensible approach had been used to estimate the uncertainty in the watershed loads, then at least one could make use of that information for the EFDC model.	The linkage analysis has been modified to include information about PAHs and bioaccumulatives. Both model reports (Appendices I and II) acknowledge that scientific information supporting modeling of metals is better than similar information for bioaccumulatives. In addition, see responses to **Comment 1.20**, **1.21**, **1.24**, **1.37**, and **1.95**.
1.28	pdf p. 8	The temporal simulation period considered is January 2002 to December 2005. The statement is made that "this period encompasses the greatest density of observational data for model calibration." However, the TMDL document indicates that the most extensive study of pollutant concentrations was the POLA/POLB study performed in 2006. Thus, the temporal simulation period is inappropriate. This important dataset	See responses to **Comment 1.21** and **1.23**.

No.	Author	Comment	Response
		should be considered for calibration and modeled explicitly (i.e. at least to the end of 2006). As discussed below, this dataset should NOT be used for setting initial conditions. This is an important error (i.e. not simulating to the end of 2006 to use this important dataset correctly) which reduces the credibility of the modeling effort.	
1.29	pdf p. 8	The model considers the correct boundary conditions for freshwater and associated sediments, as well as exchange with the San Pedro Bay waters. However, it is unclear whether sediments can be transported in and out of the Harbor through the open boundary condition; omitting this exchange can introduce error and increases the uncertainty of the calculations.	Sediment (and associated pollutant loads) can be transported both in and out of the Harbor waters through the open ocean boundary (i.e., the system is not modeled as a box where all of the water and sediment must remain in the box – water and sediment can be exchanged in both directions with the open ocean). This fact has been clarified in the modeling report.
1.30	pdf p. 9	While the contaminants of interest include six metals and at least a dozen toxic organics (see Tables 2-18 and 3-7), the actual modeling considers only three metals (Cu, Pb and Zn) and three organics (DDT, Total PAH and Total PCBs). Thus, the modeling is incomplete in this regard. Given the significant differences in fate, transport and toxicity among these pollutants, it is not scientifically appropriate to use the subset of pollutants modeled as representative of the larger set of pollutants that need to be addressed in the TMDL.	Six pollutants were modeled, in part, because they are universally present in numerous waterbodies of these TMDLs. The other few metals (Cd, Cr, Hg) are present at significant levels in just 2 or 3 waterbodies. The other bioaccumulative compounds, such as chlordane, dieldrin and toxaphene, have similar transport mechanisms and exposure pathways as DDT and PCBs, so it is reasonable to model using only two bioaccumulative pollutants.
1.31	pdf p. 9	The partitioning of pollutants among seawater and marine sediment compartments is adequate for simulating the equilibrium distribution; however, it is not clear that under dynamic conditions the pollutants are truly at equilibrium. While this is a common and convenient assumption, it leads to some uncertainty in the calculations, which is not assessed or discussed in the document. The use of partitioning coefficients based on actual observed concentrations in seawater and sediments is a very good choice and reduces some of this uncertainty. However, the method for selecting values (visual best) is not scientifically appropriate. A statistical method should be used for this.	Use of equilibrium partition is supported by US EPA policy and is also supported by the lack of site specific information to use a non-equilibrium formulation. The site specific values show typical variability with no clear trend and thus average values were used. Use of statistical best fit for partition coefficients shown in Appendix I Figures 31, 34, 35, and 36 is not useful in the sense that the regression coefficients show no relationship. In some cases, the laboratory did not record complete data. For example the results presented in Appendix I Figure 33 represent a situation where TSS was not recorded and had to be assumed using an unrelated measurement. It is further noted that most of the field studies

No.	Author	Comment	Response
			were completed before the modeling study began, and thus the model utilized available data.
1.32	pdf p. 9	The EFDC model does not appear to take into consideration efflux of PAHs, PCBs and the other toxic organic compounds to the atmosphere. While these compounds have a low volatility, they can transfer from the marine environment to the atmosphere since they are very hydrophobic. A number of studies have quantified this efflux for different waterbodies around the world. Without an estimate by the modelers, this introduces another source of uncertainty into the EFDC modeling. A simpler model could have been used to perform a calculation to determine the relative magnitude of this flux, and decide whether it is significant enough to use a model that takes it into consideration. Along the same lines, there appears to be no consideration of the slow but continuous transformation via reaction of these toxic organics, which occurs mostly in the water column. Ignoring this transformation is a conservative assumption from a risk assessment perspective, but this is not explicitly stated in the report. Again, a more scientific approach would be to do an assessment of the magnitude of this process to establish how significant it is, and thus determine whether to include it or not in the model. The model as implemented does not appear to take into consideration these processes, which increases the uncertainty in model output.	Atmospheric exchange and biological mediated degradation were not accounted for (although the modeling software includes representations of these processes, so these processes could be included in future modeling efforts). In the case of DDT and PCB the high number of non-detect water column concentrations preclude accurate estimate of these losses. This was understood by both the regulatory agencies and the major stakeholders who were continually involved in the modeling study, while reviewing a number of earlier drafts of Appendix I.
1.33	pdf p. 9	The initial conditions for the pollutant concentrations in marine sediments were based on a substantial dataset. However, there is no explanation of the methodology used to consider data from different years. This lack of transparency in this important step reduces the credibility of the modeling effort. In addition, the modelers apparently used the dataset from 2006 to set the initial conditions in 2002. There is no discussion about how this was done. Scientifically this approach is not acceptable. The 2006 dataset should be used for calibration and validation, not to set the initial conditions for a simulation that starts in 2002.	Available data were used to characterize initial conditions of marine sediment. Multiple years of data were compiled at various locations to develop a set of initial conditions that was representative of the spatial variability of the waterbodies, while excluding data collected before dredging or capping activities. In addition, see response to **Comment 1.21** and **1.23** for more discussion on the model time period.
1.34	pdf p.	The input functions for the load of sediments and associated pollutants	See response to **Comment 1.21**, **Comment 1.23**, and

No.	Author	Comment	Response
	10	(dissolved and adsorbed) are based mostly on LSPC simulation output. Given the significant issues associated with the calibration of the near shore watersheds LSPC model, there is a significant level of uncertainty in this important model input. The values from LSPC are considered deterministic. No apparent effort was made to consider the uncertainty associated with these inputs and how this may affect EFDC model output. This lack of rigor in the evaluation of this important aspect seriously reduces the credibility of the EFDC model as implemented.	**Comment 1.24**, which is pertinent as this comment references LSPC model results that are largely based on the regional modeling approach.
1.35	pdf p. 10	Although there is a complete section in Appendix I that discusses "Model Performance Measures" in considerable detail, the document fails to present any quantitative assessment of the EFDC model performance with respect to scientifically acceptable measures of "goodness of fit". Although clearly the modelers produced a lot model output, all the comparisons between simulation ("predicted") and observations is visual. For the hydrologic calibration, there appears to be a noticeable difference in the tidal amplitude (e.g. Figures 5 and 6 in Appendix I), but without an objective measure it is difficult to determine whether this is an acceptable fit. The match between simulated and observed phase and amplitude of the tidal current velocities seems to be even lower (Figures 8-11 in Appendix I). Since the hydrologic calibration is key for model performance, this mismatch is likely to result in significant error in the simulation of sediment and pollutant transport. The authors of this appendix consider the match "reasonably good" (p. 12 in Appendix I), but that is strictly subjective and not based on a scientifically defensible performance measure.	The hydrodynamic calibration was judged to be quite good and was previously reviewed by a highly qualified consultant working for the stakeholders. The differences observed are generally consistent with other TMDLs (see also response to **Comment 1.26**). Graphs of water surface elevation and velocity are highly sensitive in appearance to small phase errors thus the harmonic analysis results in Appendix I Tables 3-7 (which are quantitative) and low frequency time series analysis are more definitive for water surface elevation. For velocity comparison one must consider that the comparison is between point measurements and velocities averaged over the horizontal model grid cells which range from 12,000 to 24,000 square meters in area at the locations where the current meters were deployed.
1.36	pdf p. 10	A significant amount of effort appears to have been placed in calibrating the salinity. It is not clear that this is very relevant to the issues considered in the TMDL. Again, no model performance measures are reported. In any case, the simulation of salinity appears from a visual perspective to be quite good at the bottom, which should not be very surprising since these waters are at a fairly constant salinity and are not diluted significantly by the incoming freshwater. However, there is considerable scatter in the data for the surface seawater salinity (Figure	Salinity is an important parameter to simulate as it is a measure of how the freshwater input is addressed in the receiving water model. While the model does not always accurately predict observed measurements, it generally captures the range of observations using the data and information available at the time of model development – thereby justifying its use in TMDL scenarios.

No.	Author	Comment	Response
		5-3 in TMDL report). This is further corroborated upon visual inspection of Figures A-1 to A-20 in Appendix I. As the authors indicate in p.23 of Appendix I, “point wise agreement is not always good”. Surprisingly, the TMDL report indicates that “the hydrodynamic model provides a good foundation for the simulation of sediment and contaminant transport”. Given the previous findings, the use of the word “good” seems unwarranted.	
1.37	pdf p. 10	The next step in the calibration is adjusting the sediment transport parameters. While there is a discussion in the TMDL document of the approach that should be taken to perform this calibration, there is no presentation of results. There is also no analysis of the results. Pages 78 and 79 of the main TMDL report fail to provide any serious discussion of the results for the sediment or the pollutant concentrations. This lack of transparency is not acceptable. If the results are not good, this should be made clear. The reader is referred to Appendix I for the bad news. In page 60 of Appendix I, the modelers note that “model predicted concentrations are reasonable, however a quantitative measure of agreement would be extremely low”. While this is an honest assessment, it indicates that the EFDC is not adequately predicting sediment transport. Only three graphs (Figures 40-41) are presented within this Appendix, and the simulated results show significant variability. A major issue is that the simulated results are for a temporal period (2002-5) that does not correspond to most of the observed data (2006 and 2007). The omission of the presentation of more results, and of quantitative “Model Performance Measures” is not scientifically acceptable. The credibility of the output of this implementation of the EFDC model with regards to sediment transport is thus very low.	Extremely limited sediment concentration data were available for sediment calibration; thereby, reducing the utility of more quantitative assessment methods. These results are presented in Figures 40 and 41 of Appendix I and are discussed in Section 8.1. As indicated in Appendix I, the sediment comparison plot (Figure 41) does show extensive scatter, but the model predicted levels are within the range of observations (the observed values have a slightly wider range than the predicted concentrations). In addition, a factor of 2 difference between predictions and observations is considered good and has been accepted in a number of major contaminated sediment modeling studies. Most of these studies have not been published due to the proprietary nature and/or ongoing litigation. Most often plots like those in Appendix I Figure 41 use log scales; however, these evaluations are presented using linear scales. The comparison in these Figures is actually between model-predicted dry season 2005 average concentrations and observations during dry season conditions in 2006 and 2007. Watershed model based flow and sediment loads were not available for actual simulation of 2006 and 2007 conditions for direct comparison; therefore, achieving the range of concentrations is determined sufficient (and statistical comparisons would not be applicable). Visual comparison is a common technique for comparison of modeled and observed values, especially in TMDLs, which

No.	Author	Comment	Response
			are required to be based on the best *available* data. Therefore, it is important to note that presentation of statistical evaluation of model uncertainty is not a requirement to justify a model's use for TMDL calculations. Additional discussion has been added to the TMDL report (pages 78-79) to describe the sediment simulation results. The simulated values used for TMDL or existing loading rate calculations were annual averages. Given that the model is in the range of observed values and averages are likely similar, the model is being appropriately used to determine loading estimates.
1.38	pdf p. 11	The final step in the calibration is the adjustment of parameters related to the various pollutants. Again, no results are presented in the main TMDL report, and there is no discussion of the results of the calibration. Although several figures are presented in Appendix I, no quantitative "Model Performance Measures" are presented. The authors remark that "the comparison show extensive scatter, but model predicted levels are within the range of observations". Clearly, the EFDC model as implemented does not adequately simulate the concentration of these pollutants. The comparison of the copper concentrations (Fig. 42) indicates that the model tends to over predict the concentrations in general. The over prediction is by a factor ranging from around 1.5x to 2x, based on visual inspection (since all we have is a graphic). The authors could have provided such analysis in their report, to be more quantitative in the comparison. The over prediction is more pronounced for lead (Fig. 43) and zinc (Fig. 44) concentrations, where the factors are 2x to at least 5x, if not more. This is a substantial difference, and is not truly "within the range of observations". The best correspondence appears to be for DDT concentrations (Fig. 45) although there are very few observations. For total PAHs, the over prediction is again around 3x to possibly 10x. The observations and simulation for PCBs indicate that these toxic organics are below detection levels (although the detection level considered to make this assessment is not reported). No temporal	See response to **Comment 1.37**. While this comment focuses on sediment, the response is similar for the contaminants as the modeling approach and available data for comparison were similar. In addition, water column observations for sediment and contaminant concentrations to support the modeling were very limited, which impacts the calibration. Observational data to calibrated sediment erosion was even more limited. The approach was to use best estimates of partitioning coefficients and erosion rates rather than manipulate these to achieve better prediction.

No.	Author	Comment	Response
		trends are presented for any of the toxic compounds modeled (metals or organics), so it is not possible to assess whether there is also a temporal bias (accumulating or depleting the reservoirs). The presentation of results is seriously lacking, with diminished scientific integrity. Overall, the calibration of the EFDC model is not adequate, since it has a clear bias towards over predicting concentrations of toxic pollutants in the harbor. While this may result in a more protective TMDL, a model should not have a bias.	
1.39	pdf p. 11	Overall the implementation of the EFDC model for the harbor waters had several important deficiencies, and the calibration of the various components needed to predict the concentrations produce inadequate results. The outcome is that the simulated concentrations of toxic pollutants in the harbor are biased and may not reflect the actual concentrations. Thus, the linkage analysis is seriously deficient. Section 5.3 (Summary of Linkage Analysis) makes no mention of the problems with calibrating the LSPC and EFDC models. Scientific integrity requires one to report and discuss the problems with the calibration, but this is not done. The summary introduces the presentation of pollutant load reduction scenarios; this should not be done in a summary, but rather in an earlier section. In any case, while Appendix III, Section 8 does present a “no upland loading scenario”, there is no mention in the appendix of the “reduction of contaminated sediments in receiving waters to attain desired sediment target concentrations” scenario. Thus the summary is misleading, or the results of the scenario were omitted. Since this information is used for the determination of the Waste Load Allocations (WLAs), the omission is significant.	While the model does not always accurately predict observed measurements, it generally captures the range of observations using the data and information available at the time of model development – thereby justifying its use in TMDL scenarios. Additional data collection within the harbor waters as well as in the watershed could be used to update the models if the TMDL is reopened for that purpose in the future. Model calibration discussion is presented in Appendices I and II. The presentation of the load reduction scenarios will be discussed in an earlier section. In addition, the TMDL scenarios will be clarified in Appendix III and the Linkage Analysis section to ensure the scenario descriptions are consistent.
1.40	pdf p. 12	**Appropriateness of estimate of load capacity and load reductions** *Toxicity TMDL in freshwater* There is no presentation of a load capacity for toxicity. The discussion is not very clear, but one can gather that the intent is to assume that the load capacity is 1 TUc, that is that each discharger must reduce the	Impairment due to toxicity has not been demonstrated in these other waters. While these watersheds may contribute to toxicity by contributing toxic compounds (and TMDLs are established, herein) no separate TMDL for toxicity has been established.

No.	Author	Comment	Response
		concentrations in their discharges to less than or equal to the chronic concentration of each pollutant. The interim allocation is <2 TUc, which apparently is currently being achieved, although the data presented in the TMDL report is insufficient to make this assessment. The final allocation is <1 TUc, which would be protective of freshwater organisms within the Dominguez Channel. Presumably similar determinations were made for the SGR and LAR. It is unclear why this section does not make it more explicit that this TMDL, WLA and LA will be applicable to all watersheds draining into the harbor waters, including LAR, SGR, DC and the near shore watershed, even if those actions have been or are being taken as part of separate TMDLs. Since no modeling was needed to arrive at this TMDL and the corresponding allocations, this TMDL is not affected by the issues discussed in the previous sections.	Water quality monitoring is required for the contributions of the Los Angeles River and the San Gabriel River. If future data shows direct toxicity impairment or contributions of toxic compounds such that the downstream targets will not be met, then, at that time, TMDLs for upstream sources including targets and allocations can be developed.
1.41	pdf p. 12	*Toxicity TMDL in freshwater* According to staff, an implicit margin of safety (MOS) is included in these TMDLs. There is no significant discussion of how this implicit MOS is determined. Although the NOEC were used, it would be useful to evaluate the methods used by the CTR to estimate the chronic criteria, to see whether an MOS is truly implicit in the determination of these criteria. In addition, as mentioned earlier, the assumption that the freshwater organisms can be exposed to a mixture of pollutants all at the chronic toxicity NOEC may not be warranted, and thus to be protective an explicit margin of safety should be included.	For toxicity, a NOEC was used to define the toxicity unit of TUc. An implicit margin of safety exists in the final allocations of toxicity because the chronic toxicity unit was used which will be protective of both acute and chronic exposures. Concerning the CTR, see response to **Comment 1.6**.
1.42	pdf p. 12	*Wet weather metals TMDL in DC* The approach taken by staff is to consider the daily storm volume and the numeric target to calculate the maximum daily load acceptable in DC. The numeric target considered for the calculation is the acute criterion for each metal. However, as stated by staff earlier, “the Basin Plan narrative toxicity does not allow acute or chronic toxicity in any receiving waters”. Therefore, to meet the narrative toxicity and the Toxicity TMDL, the numeric target must be the chronic criterion, not the acute one. Otherwise, a discharge at the acute level would	Application of acute criteria for metals during wet weather conditions is appropriate given the duration of elevated flows with Dominguez Channel freshwater portion are for one day, therefore more consistent with an acute exposure timeframe.

No.	Author	Comment	Response
		immediately violate the chronic criterion. Table 6-2 should consider the chronic numeric targets, not the acute criteria.	
1.43	pdf p. 12	In addition, the daily storm volumes were estimated using LSPC. Given the issues with the calibration of this model, there is likely a significant amount of error in the estimate of daily volumes. Thus, the estimated allowable load has significant uncertainty. A 10% explicit MOS is insufficient for capturing the uncertainty in the LSPC estimates. Table 6-3 should be revised considering a higher MOS. Given that there is also considerable uncertainty in the estimate of the existing load, the percent reduction should be considered a rough estimate, rather than a very precise value. Certainly it is not known to three significant digits, as currently indicated in Table 6--3.	In addition to the explicit margin of safety for the Dominguez Channel freshwater allocations, the targets and allocations for the Dominguez Channel Estuary and Greater Harbor waters include an implicit margin of safety (MOS). The implicit MOS is based on the selection of multiple numeric targets, including targets for water, fish tissue and sediment, among other conservative modeling assumptions. Staff finds that together these margins of safety are reasonable and adequately offset the uncertain estimates, including estimates of daily volume in Dominguez Channel during wet weather conditions. However, an additional explicit margin of safety will be considered and may be applied if any chemical-specific sediment quality target is revised or updated contingent on future sediment quality studies. That is, there may be uncertainty associated with revised sediment quality values, which may warrant including an additional explicit margin of safety. See response to **Comment 1.21** regarding significant digits.
1.44	pdf p. 13	An additional concern is that since no exceedances have been observed during dry weather, then the decision by staff is that no TMDL is needed under these conditions. The rationale makes sense for freshwater organisms within DC, although it is possible that these waters can exceed the toxicity thresholds as the water volume decreases during dry weather. More importantly, the most severe problem is in the estuary and harbor waters. The cumulative load during dry and wet weather has an impact on the amount of metals present in the harbor. Thus, since the DC freshwater organisms are already protected by the Toxicity TMDL, the focus of the reductions should be the protection of the marine organisms, and the load capacity should reflect the maximum capacity	Several permits in each watershed, including the Municipal Separate Storm Sewer System (MS4) permit, require receiving water monitoring including monitoring for toxicity. Data up to this point have not demonstrated an impairment of those waters due to toxicity. These watersheds may contribute to toxicity by contributing toxic compounds (and TMDLs are established, herein) or if future data show direct toxicity impairment or contributions of toxic compounds such that the downstream targets will not be met, then, at that time, toxicity TMDLs including targets and allocations can be developed.

No.	Author	Comment	Response
		of the receiving TMDL zones in the estuary and harbor. If the maximum capacity of the receiving waters is greater than allowed by the Toxicity TMDL, then the default should be the Toxicity TMDL for the freshwater loads.	
1.45	pdf p. 13	The approach used for the WLA and LA calculations is scientifically sound, except that a 10% MOS is extremely small given the uncertainty in the load capacity estimates. To be clear, the explanation in Sections 6.2.2.2 and 6.2.2.3 should indicate that the allocation is done by area, as presented in Appendix III and Table 6-4. Good scientific writing practice is to refer to the section in an appendix or other supporting document where more details are presented, so that the reader can easily follow the calculations.	Freshwater loading estimates were compiled using 10 years of modeling flows. Staff acknowledge that uncertainty exists and have added an explicit 10% MOS to account for some uncertainty (i.e., variability in the flows).See also response to **Comment 1.43**.
1.46	pdf p. 13	There is a significant difference between the "Allowable Loads" in Table 6-3 and the TMDL in Table 6-4. For example, for Cu the allowable load in Table 6-3 is only 234 kg/yr or 640 g/d. The TMDL in Table 6-4 is for 1,416.6 g/d of Cu (clearly the TMDL cannot be calculated to 5 digits of precision!). Even if one considers only the wet days, there is no explanation of how the calculation goes from the Allowable Annual Loads in Table 6-3 to the TMDLs in Table 6-4, and Appendix III does not provide any information. Since this is a crucial calculation for the TMDL, it should be more transparent.	Table 6-3 compares the annual existing load, based on modeling of the average annual loading capacity for each metal, during wet weather to the allowable load using the numeric targets. However, Table 6-4 shows wet-weather TMDLs and allocations for copper, lead and zinc (g/d). Allocation values presented were based on daily storm volume associated with stream flow rate.
1.47	pdf p. 13	The interim metal allocations are presented in Table 6-5. In the preceding text, staff indicates that these are calculated "based on the 95th percentile of total metals concentration from January 2006 to January 2010." Where was this information presented in the entire report (TMDL document and appendices)? In addition, these values are substantially above the interim toxicity allocations. A reconciliation of these interim allocations (toxicity vs. individual metals) is needed, to ensure they can be met.	The information is available in the administrative record for the TMDL and in Los Angeles County Stormwater Monitoring Annual Reports, at http://ladpw.org/wmd/NPDES/report_directory.cfm. The information was available to stakeholders. Toxicity may result from diverse pollutant types and many sources, and synergistic effects are considered in addition to metals. See response to **Comment 1.2**.

No.	Author	Comment	Response
1.48	pdf p. 13	*Wet weather metals TMDL in Torrance Lateral* The approach taken by staff is different than for DC. In this case, the staff has not taken into consideration the LSPC model results. This may be a good decision. In this case, water and sediment "allocations" are based on concentrations. The approach is scientifically sound, with the exception that these are based on acute concentrations, so it again does not follow the Basin Plan: "the Basin Plan narrative toxicity does not allow acute or chronic toxicity in any receiving waters". Thus, the chronic toxicity values must be used to be protective. Rather than assume an implicit MOS, it would be scientifically more defensible to assume that an explicit MOS is needed if more than one of the metals is present at concentrations near the chronic criterion.	See response to **Comment 1.9** and **1.42**.
1.49	pdf p. 14	The Waste Load Allocations for the ExxonMobil refinery are based on a stormwater flow rate of 3.7 MGD for only 7 days/yr. While this flow rate may be reasonable, no data was presented to support the calculation. The Numeric Targets used are not indicated; if the acute targets were considered, this would not meet the Basin Plan.	The information on flow rate and duration is contained within ExxonMobil's August 26, 2010 comment letter. The targets are the same as for the other sources. See response to **Comment 1.42**.
1.50	pdf p. 14	Interim sediment allocations for metals are based on observed concentrations. Staff considered the 95th percentile values of the observed values for this interim allocation. There is no specific justification for the use of the 95th percentile, as opposed to a lower level; it is likely set at a level that will not be easily exceeded. It would be better to have a justification for this choice, other than it being consistent with NPDES permitting, since this is not an NPDES permit. More importantly, the underlying data for this choice is not presented anywhere in the document, and there is no explanation of how data from different years was combined to produce a single value. It is possible that the 95th percentile values reflect samples from 1998, while the current condition may be much better, or it could be the inverse. In either case, the scientific basis is not transparent so that one can clearly understand the selection of the values in Table 6-8. For the PAHs, instead of using a value for Total PAHs, the interim and final allocation should be based on individual PAHs, as presented in Table 3-7. There is	A 95th percentile value is a typical value for an interim target unless there is information to support a different value. Since no such information is available at this time, or information sufficient to develop step-wise interim targets, then the only alternative would be to have no interim targets. In a TMDL which is expected to take as long as 20 years to achieve, setting no interim limits is ill advised. Reconsiderations of the TMDL after 5 years or at another time, as appropriate, may give the opportunity to develop different interim limits based on more recent data or stepwise interim limits developed to achieve interim milestones in the progression to the final target. See response to **Comment 1.47**. Interim allocations are based on existing data of recent conditions. Sufficient individual PAH data or other pesticide

No.	Author	Comment	Response
		no mention of interim allocations for pesticides other than DDT, which indicates that this is not a complete set of allocations.	data were not available to calculate interim allocations.
1.51	pdf p. 14	It should be noted that in some cases, using the 95th percentile value means that the Numeric Target is exceeded by almost two orders of magnitude, particularly in the LA Harbor Consolidated Slip which apparently is heavily polluted. Thus, higher priority must be given to these areas in terms of reducing their concentrations to the Numeric Targets.	Staff agrees.
1.52	pdf p. 14	The TMDL, WLA and LA are presented in Table 6-10. The description of the methods in Section 6.4.3.1 (page 90) is quite vague, and thus hard to evaluate whether these critical calculations are scientifically sound. The short description of the approach in Appendix III (Section 1) is also rather limited. This lack of transparency is not appropriate for building credibility.	Information has been added to Staff Report to describe how the allocations were determined.
1.53	pdf p. 14	It should be mentioned somewhere in this section that the “Current Load” in Table 6-10 is calculated based on the sediment concentrations in the table in Appendix III that lists “Sediment Concentration Information per model zone (top 5 cm)”, which was generated using EFDC. The current loads are presented in Table 4-6, but again the connection is not made clear in the document. Again, it is not clear if these predicted concentrations are at the end of the simulation (2005) or the average from 2002-2005. In any case, the current situation by 2010 may be quite different, so the observed values would have provided better estimates of the current load. Given the uncertainty associated with EFDC output, discussed above in the Linkage Analysis question, these sediment concentrations may not reflect the actual values. Note the significant difference between the values in Table 6-8 and the values in Appendix III. The depth of sediment considered for the Current Load is not clear – just the top 5 cm? There is also no mention of whether the load in the water column was considered or not.	Clarifications have been made to the Staff Report.

No.	Author	Comment	Response
1.54	pdf p. 15	For the TMDL calculation, the Numeric Target (ERL) was presumably multiplied by the mass of sediments up to the same depth. That is a scientifically sound approach, assuming that the mass of pollutant (dissolved and associated with suspended sediments) in the water column is very small relative to the mass in the sediments.	Staff agrees the TMDL calculation used a scientifically sound approach.
1.55	pdf p. 15	The air deposition estimates are explained in Appendix III Section 6. Those follow scientifically sound methods. It is important that the TMDL document make reference to the section in the appendices where such calculations are provided, so that the reader can easily follow them. One important issue with the air deposition estimates is that there is no estimate of the uncertainty or variability in these values. Since these calculations are based on a few data points in a relatively short timeframe, some allowance for uncertainty should be taken into account in an explicit MOS.	Staff agrees the air deposition estimates follow a scientifically sound method. Uncertainty is relevant to air deposition estimates and is best addressed via collecting additional air deposition monitoring data in future optional studies.
1.56	pdf p. 15	There is no explanation of how the Load Allocation for "Bed Sediments" was done. Are these based on the total sediment deposition rates presented in Appendix III, multiplied by the pollutant concentration calculated by EFDC? Or the pollutant concentration calculated by the corresponding LSPC models? Given this lack of information, the scientific validity of these estimates cannot be determined. In any case, the total sediment deposition rates in Appendix III have considerable uncertainty and may be in error, based on the relatively poor calibration results; they are certainly not known to 5, 6 or 7 significant digits as presented in the table in the appendix. There is also considerable uncertainty in either of the models with respect to pollutant concentrations, so again the estimated LA for these bed sediments has considerable uncertainty.	See response to **Comment 1.52**.
1.57	pdf p. 15	Waste Load Allocations are apparently determined based on the freshwater input estimated for each permittee and waterbody based on their area (a well known value) and the LSPC flow rates (a value with potentially significant uncertainty and bias as indicated by the calibration results of the near shore watersheds model). There is no	See response to **Comment 1.52**.

No.	Author	Comment	Response
		mention of the pollutant concentrations used to estimate the WLAs. Given this lack of information, the scientific validity of these estimates cannot be determined.	
1.58	pdf p. 15	Although the text mentions that “refineries which have provided discharge flow data along with monitoring results receive mass-based allocations”, Table 6-10 does NOT list any refinery explicitly. In fact, only the TIWRP is identified explicitly as a point source, other than the MS4 permittees (LA County, City of Long Beach and CalTrans). Throughout the TMDL document, information about these point sources (i.e. refineries and other major sources) is at best obscure. It is possible that these are indeed minor sources, but the lack of transparency is a major issue.	Refineries are identified and described in Section 4.1.2 of the Staff Report (pg. 59).
1.59	pdf p. 16	The use of concentration-based limits, applied as daily average limits, for minor or temporary sources (e.g. construction), is a scientifically sound approach. The problem is that the values in Table 6-9 don’t correspond to the Numeric Targets in Table 3-1 for Cu, Pb, and Zn, and that the value for benzo[a]pyrene is being used for Total PAHs, when the impairment is by individual PAHs, not the total. This lumping of PAHs is not as protective, since PAHs have distinctly different toxicities and bioavailabilities.	Staff agrees the use of concentration based limits is a sound approach. Table 3-1 is expressed in dissolved values and Table 6-9 is expressed in total recoverable metals. For PAHs, see response to **Comment 1.4.**
1.60	pdf p. 16	Staff mentions that “an implicit margin of safety exists in the final allocations.” Since the method for calculating the TMDL and allocations is not transparent, this statement cannot be evaluated. However, given the uncertainties, it is unlikely that an unquantified “implicit” MOS is protective. The assumption that the LA in bed sediments and air deposition is calculated with significant certainty does not seem warranted, given the issues with modeling. Even if the information is not based on modeling (i.e. observed sediment concentrations in a given volume), there is some uncertainty in the determination of the pollutant concentrations in these sediments, which should be reflected in an explicit MOS.	See response to **Comment 1.43.**

No.	Author	Comment	Response
1.61	pdf p. 16	The other three metals that had not been considered in any of the modeling or previous calculations are finally considered in Table 6-11. If there is no effort to reduce their loading from the watershed, then a much longer time may be needed to achieve the Numeric Targets. It is unclear why these values do correspond to the Numeric Targets in Table 3-7, but those in Table 6-9 do not.	These three other metals (cadmium, chromium, mercury) impair only two or three waterbodies and staff finds the watershed contribution is small relative to pollutant loads in existing bed sediments. For this reason, these metals are given concentration-based allocations equivalent to the sediment chemistry numeric targets. Table 6-9 presents water column concentration-based allocations, not sediment chemistry concentration-based allocations.
1.62	pdf p. 16	The proposal by staff to achieve the Direct Effects TMDL either by meeting the final sediment allocations or by demonstrating the desired qualitative condition via multiple lines of evidence is a scientifically sound approach, IF the final sediment allocations are truly protective of the aquatic organisms. As mentioned before, the lack of transparency in the calculations reduces their credibility, and the implicit MOS may not be protective enough.	Staff agrees. For the implicit MOS, see response to **Comments 1.43**.
1.63	pdf p. 16	The term bioaccumulatives is used incorrectly in this TMDL document, since PAHs and some metals are also bioaccumulated and thus should be considered here. It would be best to either use the term toxic organics (and move the PAHs to this section) or just organochlorines.	Staff disagrees; there is little scientific evidence of metals and PAHs bioaccumulation in aquatic organisms. Some biomagnification may occur in certain organisms; while certain fish can metabolize PAHs, both of which are different from bioaccumulation across trophic levels.
1.64	pdf p. 16	As mentioned in the response to the first question, the use of numeric targets for different pollutant-media combinations requires a consideration of the partitioning coefficients, otherwise a numeric target could contradict another one. Thus, staff considered the ERLs in some cases and the BSAFs in other cases. The most protective value was used, which is scientifically sound. It would be best if this problem was resolved at the moment the numeric targets are set, so that it is clear what the target is.	Staff agrees with using the more protective of the ERLs or the BSAF. In addition, see **Comment 1.13.**
1.65	pdf p. 16	Although there is a better description of the method used to determine the TMDL in this section (Equation 3), and the method is scientifically sound, the approach for allocating the loads to LA and WLA is not clear. The lack of transparency does not permit the evaluation of the method	Staff agrees that the method used to determine the TMDLs is scientifically sound. However, staff also finds that the approach of an implicit MOS is sufficiently protective. The implicit MOS is based on the selection of multiple numeric

No.	Author	Comment	Response
		used to determine mass-based WLAs. The approach used for minor and temporal sources is scientifically sound. The implicit approach for determining the MOS is not scientifically sound; an explicit calculation of the uncertainty should be done to determine the MOS. A 10% MOS is unlikely to be protective. The selection of multiple numeric targets is not by itself a determination of an implicit MOS. The most conservative target must be used, but there are uncertainties in the calculation of the loads, so an additional MOS is needed. The concentration based WLAs for chlordane, dieldrin and toxaphene require a better assessment of the sources to be useful for the TMDL.	targets, including targets for water, fish tissue and sediment, along with other conservative modeling assumptions. See response to **Comment 1.43** and **1.60**. For the explicit MOS, see response to **Comments 1.45**.
1.66	pdf p. 17	One issue with concentration-based load allocations is that it could lead to a total load greater than the TMDL under some circumstances. Therefore, monitoring of the actual loads will be needed to ensure that the TMDL is actually being met.	Staff agrees on the importance of monitoring.
1.67	pdf p. 17	The critical condition would be a large wet weather event that produces extensive contaminated sediment transport through the channels as well as contaminated sediment redistribution in the estuary and harbor waters. Thus, although the report indicates that the "critical condition is not identified based upon flow or seasonality", there is clearly a seasonal nature to the critical condition, i.e. high precipitation events during the rainy season. The concern is that areas that achieve attainment of the beneficial uses may again become impaired due to such events. As such, the current analysis does not contemplate what to do in this case. A solution would be to implement a monitoring program after such events, to reassess the situation and determine whether the TMDLs and allocations are adequate.	The monitoring required by this TMDL includes water sampling during two wet weather events each year. Sediment sampling typically occurs in the dry weather season when it is safer for samplers and when sediment transport has minimized for the wet season. The data to be collected should be sufficient to determine if rain events are recontaminating sediments. This TMDL has a scheduled reconsideration at year 6 so that adjustments can be made if appropriate.
1.68	pdf p. 17	**Sufficiency of proposed monitoring program to assess effectiveness of the TMDL and attainment of water quality standards** The proposed monitoring program is generally scientifically sound. The samples should be analyzed for all the pollutants listed in Table 2-18. The current text is unclear as to the metals to be considered. The text	 For analytical methods issue, see response to **Comment 1.17.** In addition, the monitoring plans developed by the

No.	Author	Comment	Response
		also does not indicate that any future samples MUST be analyzed using analytical techniques with detection limits low enough to indicate whether the Numeric Targets are being met. While this seems an obvious requirement for any QAPP, it is still distressing to have read that so many samples have been and are still analyzed with unsatisfactory analytical instruments. The proposed frequency is appropriate, except that as noted in the Critical Conditions section above, after an extreme wet season a round of sediment sampling should be conducted to assess the situation and make adjustments to the TMDL and allocations as needed. Since eliminating toxicity is the primary goal of this TMDL, toxicity testing should be required of all stations in Table 7-1, and should include both water and sediment toxicity. Hopefully, the reduction of the pollutants targeted by this TMDL will eventually eliminate toxicity, but such a monitoring program would ensure that toxicity does not continue due to new pollutants not targeted here.	responsible parties will require approval by the Executive Officer including approval of pollutants to be analyzed, frequency, and analytical methods. The monitoring requirements in the TMDL also require toxicity testing in water or sediment, depending upon the waterbody.
1.69	pdf p. 18	**Evaluation of the implementation plan and allocations** The narrative for the implementation plan is generally scientifically sound. The proposed phase approach, where some more immediate actions are taken along with a more detailed monitoring program, makes sense. Given the large uncertainties in the source terms and modeling results, in addition to these steps, a full revision of the TMDL and allocation calculations should be done before beginning Phase II.	Staff agrees that the implementation plan is sound. Revisions to the TMDL including revision of the sediment TMDLs will be considered after 5 years of implementation. Revisions to allocations may be considered at that time, if appropriate.
1.70	pdf p. 18	It is surprising (in a bad way) that the Superfund sites present in this area, which are likely major contributors, are only mentioned at this late stage in the document. These potentially major sources should have been considered in the Linkage Analysis and the TMDL. How can such hot spots not be taken into account?	DDT is the only pollutant associated with Superfund sites within the scope of the TMDL. We have mentioned Superfund monitoring results within Section 2. We have added Montrose site information to Section 4. Staff believe the sources of DDT compounds to ambient waters are from stormwater runoff and/or diffusive fluxes from the contaminated sediments therein. In addition, the TMDL did recognize the potential contribution of these Superfund sites, in the following way: A. Potential pollutant run-off from these sites is

No.	Author	Comment	Response
			addressed under MS4 permit. B. EPA Superfund program has already completed several actions to both remove DDT contaminated soils as well as apply a cap to stabilize site soils and thereby minimize any potential pollutant run-off.
1.71	pdf p. 18	The timeline for the implementation (Table 7-2) is reasonable, although the deadlines for Tasks 12 and 13 have considerable uncertainty. Key will be to (1) have a much better monitoring program; and (2) have much better models that can help to make a better assessment.	Comment noted. See also response to **Comment 1.68**.
1.72	pdf p. 19	**Minor comments for Draft TMDL document:** Page 21: what is meant by "Some areas changes also occurred."?	For the purposes of 303(d) listing, the definitions of some waterbodies within the Harbor waters were refined for simplicity and clarity. For example "Los Angeles Harbor/Southwest Slip" became part of Los Angeles/Long Beach Harbor Inner Harbor."
1.73	pdf p. 19	Page 32: The statement is made that "From 1994 to 2004, sampling frequency has decreased and now only occurs only in years when there is a discharge, such as 2005." The first part of this statement refers to a particular period, yet the second part refers to a year outside this period. Did the sampling frequency return to normal after 2004? Apparently not. We are in 2010, so it would be useful to know what is happening today, not 6 or more years ago.	From 2004 until present, sampling only occurs when there is a discharge. One year in which there was a discharge, for example, was the high rain year of 2005.
1.74	pdf p. 19	Page 50: The document states that "the chlordane, dieldrin, toxaphene, DDT and PCBs sediment targets presented in section 3.1.2 may need to be revised". Section 3.1.2 refers to water numeric targets, not sediment.	The Staff Report has been corrected to refer to section 3.2.1.
1.75	pdf p. 19	Page 55: Lots of information is provided, for example the requirements of Storm Water Management Plans, but this is not relevant to the TMDL. The document should not be padded with such information.	The MS4 permits, including the Storm Water Management Plans, will be important in the implementation of the TMDL.
1.76	pdf p.	Page 56: A map of all these permittees would be quite useful. A table	The jurisdictions draining to the nearshore watersheds maps

No.	Author	Comment	Response
	19	indicating the monitoring data collected by each of the permittees is necessary, as well as an appendix with the actual data. Table 4-1 is too general. When did they start monitoring? What parameters? Are the results indicating that these are important sources? For example, at the end of the page it is mentioned that the City of Long Beach received a permit since 1999, but no monitoring results are reported.	are in Appendix III and a map of the jurisdictions has been added to the Staff Report. .
1.77	pdf p. 19	Page 64: The correct units are µg/m2-day and ng/m2-day, not µg/m2/day or ng/m2/day. Also, the acronyms of the water bodies should be provided in a footnote.	The Table has been updated.
1.78	pdf p. 19	Page 65: The heading of Section 4.3.2 is incorrect. This is not an analysis of the existing “sediment”, but rather of the pollutants within the sediment.	The word “pollutant” has been added.
1.79	pdf p. 19	Page 82: The equation for TUc was already introduced in page 45. It is not good practice to be repetitive within a report. However, in this case there is an example provided, in which the authors state that “if the NOEC is estimated to 25% using hypothesis testing”. What does 25% refer to? Percent of what? Presumably 25% of the NOEC, but this is unclear. The definition actually should be revised. It should be the sample concentration divided by the NOEC. Thus, a sample concentration which is twice the NOEC would have a TUc of 2.	The 25% that the example refers to would be a 25% dilution of the water being tested.
1.80	pdf p. 19	Page 94: The paragraph in this page does NOT correspond to the Margin of Safety discussion. A separate heading is needed.	A separate heading has been added for these sediment conc-based allocations..
1.81	**pdf p. 19**	Page 114: The information that separate TMDLs are being implemented for LAR and SGR should be mentioned earlier in the document, and in an earlier section numerical information should be provided to be able to determine how the joint actions of the various TMDLs will eventually result in achievement of the beneficial uses.	The suggested information has been added to the Staff Report.
1.82	pdf p.	**Comments on Appendix II (LSPC Watershed Model)**	

No.	Author	Comment	Response
	20	This appendix intends to present the methodology utilized to setup the LSPC watershed model, for calibrating and validating the model, and its subsequent use for developing the loads associated with the various sources in the Los Angeles and Long Beach Harbors. While the appendix does provide many important elements of the model setup, there are some important gaps in the information provided. More significantly, the calibration of the LSPC model for the near-shore watersheds is not scientifically supportable. The analysis relies on previous implementations of the LSPC model of the LAR and SGR for the load calculations; insufficient information is provided in this report to determine whether those calibrations were adequate, but if the same approach was undertaken, the scientific validity would be questionable. Although the authors attempt to "validate" the model, the results of the validation are not adequate, particularly for TSS. Since the transport of the metals and toxic organic compounds studied here depends considerably on the flow and TSS calibration, those results are questionable as well. Quite frankly, the answer to the question of whether this study is scientifically adequate is no.	As this comment refers to the application of the regional modeling approach, please see **Comment 1.16** and **Comment 1.24**.
1.83	pdf p. 20	App. II, page 1. reference to a modeling approach for metals is given, but the citation is SCCWRP "unpublished results". Since this reference is not available, it is not useful at all. Need to provide date and number of any report cited in the document.	Modeling approach refers to LSPC methods developed for TSS and metals in other Southern California watersheds. The Dominguez Channel information has not been specifically published by SCCWRP but is a part of the administrative record for the TMDL.
1.84	pdf p. 20	App. II, page 2. Authors indicate that they use two different approaches for wet and dry weather loads. They justify this indicating that other TMDLs in the LA region have been done that way (without indicating which ones, so the statement is not backed by a proper citation). It is unusual that they have chosen to use an approach that appears not to be able to handle a continuous simulation of dry and wet weather. One could incorporate source functions for the dry weather flows into LSPC to account for them, so it is unclear why the authors have chosen this more complicated and less scientifically defensible approach. Antecedent conditions can be important for the simulation of hydrology,	See response to **Comment 1.25**. Citations will be added to section 2 to identify the TMDLs in the LA Region that have used separate wet and dry approaches. LSPC is used to perform a continuous simulation so antecedent conditions (including dry condition) are included in the watershed modeling. However, for existing conditions calculations, only the wet weather flow and associated concentrations were extracted from LSPC. The interim dry conditions were replaced with dry weather loading calculations as described in Appendix II.

No.	Author	Comment	Response
		TSS and pollutant transport, and the current approach seems to a priori discount their influence.	Source functions could have been included in LSPC to represent dry-weather flows, but this was determined unnecessary since those flows would still need to be estimated, which would still require a separate approach. The estimated dry-weather flows, using the separate approach, could have also been represented in LSPC, but this step was determined to be unnecessary since the estimated flows could simply be represented as direct inputs to EFDC.
1.85	pdf p. 20	App. II, page 3. The authors correctly point out that although non-point sources are distributed throughout these watersheds, there are likely some hot spots. For example, although PCBs can come from several land uses, there are likely some electrical transformer locations which are hot spots. However, this observation is then completely disregarded. There is no effort to identify hot spots since "their presence and impact to receiving waters are difficult to identify/characterize." Since these may be the most significant sources for specific pollutants, a proper study would have made the effort to identify them and consider them in the model. Management actions will have to be specific for these hot spots, so ignoring them (or averaging them into the land use coefficients) is not useful.	The best available data and information were incorporated into the TMDL (the LSPC watershed modeling was completed in 2006; therefore, data available through 2005 were included). If data and information on potential hot spots or other point sources were available, they would have been included in the LSPC model. If such information is collected or identified in the future, revisions can be made to the LSPC model in the future. Required sediment monitoring and the Sediment Management Plans will identify hotspots and remove hotspots as appropriate during the implementation of the TMDL.
1.86	pdf p. 21	App. II, page 4. It is mentioned that the LSPC model "has been successfully applied and calibrated" for the LAR and SGR. There is a need to objectively define "successfully". What was the goodness of fit measure used to determine success? Was there any other statistical approach used to evaluate this? Since this is also a major problem with the current analysis, one is left to wonder how "successful" the development of the LAR and SGR models was.	Visual comparison is a common technique for comparison of modeled and observed values, especially in TMDLs, which are required to be based on the best *available* data. Therefore, it is important to note that presentation of statistical evaluation of model uncertainty is not a requirement to justify a model's use for TMDL calculations. However, the data available for calibration and validation of the LAR and SGR watersheds were significantly larger than those for the nearshore watersheds. These modeling reports provide graphical and statistical comparisons for flow calibration and validation (see response to **Comment 1.99**

No.	Author	Comment	Response
			regarding the goodness of fit used to determine success). While the water quality comparisons were largely graphical, these efforts were also validations of the regional modeling approach (see SCCWRP, 2004 for more details on the approach). In addition, sensitivity analyses of the metals and sediment parameters were performed during the SGR modeling. These efforts both went through independent peer review during their TMDL development process.
1.87	pdf p. 21	App. II, page 4. The criterion used to discriminate between wet and dry weather was the 50th percentile observed flow. Was there observed flow for every day at all monitoring locations, to be able to make this determination? Or was this based on flow at a particular location? Was it consistent across all the watersheds, or were wet days specific to a watershed (LAR, SGR, etc.)? How are the antecedent conditions handled for this discontinuous approach?	As discussed in their respective TMDLs, the LAR and SGR flow cutoffs were based on daily observed flow at stations near each river mouth (the SGR flow was based on stations at the mouths of the SGR plus Coyote Creek, a major tributary that discharges near the mouth of the SGR) . There is a typographical error in this section and the flow cutoff should read 90th percentile to maintain consistency with the LAR and SGR TMDLs. This update has been made to the text in Section 3. These were complete flow records with daily flow for multiple years. Wet days were calculated specific to each watershed. Antecedent conditions were considered as LSPC was used to perform a continuous simulation (including dry conditions). However, for existing conditions calculations, only the wet weather flow and associated concentrations were extracted from LSPC.
1.88	pdf p. 21	App. II, page 5. Define CALWTR and provide a reference.	Definition and reference for CALWATR has been added to section 3.1.1.
1.89	pdf p. 21	App. II, page 5-6. The drainage of Machado Lake was not considered in the analysis, even though the authors indicate that it may be connected to the Harbors during extremely large and rare meteorological events. While these events may be rare, they are large, and could represent a significant fraction of the cumulative load, since they tend to wash the landscape more intensely. The exclusion does not seem justified without additional analysis to show how rare they are (some measure of	Technical analyses were performed to identify Machado Lake as a sink in the system during most conditions and a discussion of these analyses will be added to Section 3.1.1. It is anticipated that monitoring of L to confirm this assumption. If such information on overflows and sediment loading from Machado Lake are performed or identified in the future and suggest that Machado Lake should be

No.	Author	Comment	Response
		frequency) and whether they can be a relevant fraction of the cumulative load to the Harbors.	included, revisions can be made to the LSPC model if the TMDL is reopened for that purpose in the future. In addition, a TMDL for Machado Lake Toxics has been adopted by the Regional Board (and City of Los Angeles Proposition O funds are dedicated for necessary remediation), so this potential source will become diminishing in the future.
1.90	pdf p. 21	App. II, page 6. The gaps in the rainfall data were patched. While this was done only for less than 5 percent of the records, there is no information on how significant these patches were. Since rainfall is sparse in Southern California, 5% may be a significant number of rain days. There needs to be a table indicating all of the meteorological stations, the number of records per station, the number (or frequency) of missing records, and an indication of the station used to fill in the gaps. This is particularly important since the authors considered hourly precipitation, and the approach indicate in the last paragraph of page 6 tends to reduce the validity of using "hourly data", if it is going to be spread out throughout the day.	For the nearshore areas of the Los Angeles Harbor model, Station CA5085 was the only station used in the model. Any missing periods at this station were *patched* with several nearby stations. This process identifies days with missing records and matches these days with nearby stations to see if they have data. If they do, the normal-ratio method is used to estimate the rainfall (factoring average rainfall amounts) from identified nearby stations. Looking back at the data, missing day were usually in the summer months, when there is little rain in the area. The last paragraph on page 6 refers to data that accumulated within the hourly data set. While the majority of the data are hourly, there are a few instances that have rainfall data reported over a longer period. These "accumulated" periods were disaggregated based on the process described in the last paragraph on page 6. Since this process was performed for a limited number of periods, it does not reduce the validity of hourly data. That paragraph should not be interpreted as all the data is accumulated and disaggregated. The Staff Report text has been updated to ensure this is clear.
1.91	pdf p. 21	App. II, page 7. While it was adequate to discretize the land uses as indicated, the parameter values associated with each land use were assumed deterministic, with a single value for a given land use. For the sensitivity analysis, it would be important to allow the most important parameters to vary to have a better idea of the real sensitivity, and then to be able to determine the uncertainty in the load estimates. The current	Prediction of the metals is based on sediment predictions; therefore, the sediment parameters were used for sensitivity analyses. Additional investigation on both sediment and metals (washoff potency factor or POTFW) parameters were performed during the SGR metals TMDLs (Tetra Tech, 2005a). Overall, the land use-specific POTFW parameter

No.	Author	Comment	Response
		approach for the sensitivity analysis is overly simplistic.	values for trace metals were modified slightly from the regionally calibrated values. More robust sensitivity analyses for the nearshore model could be performed if the TMDL is reopened in the future. In addition, this effort would greatly benefit from additional data collection to refine the Port Activities parameters.
1.92	pdf p. 21	App. II, page 11. Most point sources do not have a constant outflow, and their concentrations are also quite variable. In particular, there is no reason to believe they control the metals or toxic organics in their discharge, so these values are likely to vary considerably from day to day. Table 2 does not indicate which dischargers have limited data. The authors indicate that the average flows are in the model database, but that information should also be provided in this appendix. If the majority of the NPDES dischargers are being treated as constant flow and loads, then this is likely to be an incorrect representation of the point source loads.	The point sources in Table 2 may not have constant discharge, but are considered relatively consistent sources of flow and concentrations throughout the year; therefore, they have less of a relative impact during the wet weather conditions that are represented by the LSPC model (i.e., their flow makes up a smaller percentage of the flow during wet weather than during dry weather). The best available data and information were incorporated into the models and as indicated in Appendix II, the values in the modeling database can be easily modified if more complete data become available and the TMDL is reopened. Additional information has been added to Section 3.1.6 to describe the use of point source data.
1.93	pdf p. 22	App. II, page 14. The authors indicate that "after comparing the results, key hydrologic parameters were adjusted". Using what goodness of fit measure? Nash-Sutcliffe? It appears from the later sections that it was all done visually, which is an unscientific approach. Even if the goodness of fit is not good, it is important to know how bad it is, not just whether it "looks" good or bad.	See response to **Comment 1.26**. In addition, it is important to note that presentation of statistical evaluation of model results is not a requirement to justify the model's use for TMDL calculations.
1.94	pdf p. 22	App. II, page 15. The statement is made that "During low flow conditions, the model is unable to predict dry urban runoff". If the authors had considered adequate source functions for the various landuses, this could be modeled using LSPC. Their ad-hoc approach is not as defensible.	The remainder of the section the reviewer refers to mentions that a separate dry weather approach was used because the model was not developed to represent the dry weather loading. In addition, please see response to **Comment 1.25** and **Comment 1.84**.
1.95	pdf p.	App. II, page 15, Fig. 4. The model clearly over-predicts flow at all	Model calibration and validation requires a balance and in

No.	Author	Comment	Response
	22	times during this event, perhaps by over 25-30%. The authors should also look at the cumulative flow. They would then see that the simulated pulse is much bigger than the measured pulse. This would have significant implications for TSS and toxics transport, and also affects the simulated concentrations, if more water is available for diluting the load. The authors indicate that "this small discrepancy in flow is well within acceptable modeling ranges." Based on what? This statement is very misleading. In reality, this error is significant and most modelers would continue calibrating to reduce the bias (over-prediction). Since the authors are only using a visual comparison, they feel they have done an acceptable job, but in reality this is a poor fit.	the case of the nearshore watersheds, very limited data were available to achieve this balance. The Forest subwatershed was used as a calibration location as it consisted solely of the Port Activities land use, which was the only land use requiring parameterization. The results presented in Figure 4 for the Forest Subwatershed do over-predict the flow, but the overall volume is likely not too significant since the flows are so low. These Port Activities parameter values were then incorporated with the regionally calibrated land use parameters during the validation simulations for the Pier A subwatershed (the Maritime Museum subwatershed did not have any Port Activities land use; therefore, it was fully parameterized using regionally calibrated values). Given that the Port Activities parameter values are the only ones that could be adjusted (since these storm data were too limited to justify re-calibration of the regionally calibrated parameters for the other land uses), during the validation process, it was determined that the calibrated Port Activities values achieved the best fit when balancing the results at both the calibration and validation subwatersheds. In addition, overall loads were also considered during the calibration and validation process, since these are ultimately the inputs to the receiving water model. The simulated metals loads were generally in the range of observed loads and the differences observed are consistent with other TMDLs in the region. If additional storm data (particularly multiple storms at a single location) become available, more substantial calibration and validation could be performed during a reconsideration of the TMDL in the future. The discrepancies between modeled and observed values for the individual storms are not unusual when evaluating individual pollutographs and hydrographs for TMDL studies, especially given the limited amount of observed data and the use of an hourly modeling frequency compared to sub-hourly observed

No.	Author	Comment	Response
			data. Additional description on the evaluation of model fit has been added throughout Section 3.2.
1.96	pdf p. 22	App. II, page 15. It appears that only one storm event was used for the calibration, out of the 3 years of simulation. There is no basis to think that this one storm event is representative of typical events. If no additional storm event data was available, this should be stated clearly. One obvious solution would have been to collect a few more events. In addition, it is surprising to see that the Forest Subwatershed which has the lowest flow is used for "calibration". It is the least representative of the three locations. Thus, the baseline for the calibration was poorly chosen.	Very limited data were available for calibration and validation and it was not possible to collect more data during the study period. The TMDL was developed using the best data available. Clarification has been added to Section 3.2 to make this clear. While the Forest subwatershed does have the lowest flow, it was selected as a calibration location since it was the only subwatershed consisting of solely the Port Activities land use. This was the only land use category that required determination of modeling parameters, as all other land uses were parameterized as part of the regional modeling approach (**Comment 1.24**). Therefore, the other subwatersheds were used for validation as they were parameterized using the Port Activities values (determined for the Forest subwatershed) as well as values for other land uses previously calibrated during the regional model development. Clarification has also been added to Section 3.2 to explain these selections.
1.97	pdf p. 22	App. II, page 16. Most modelers would use data from the same location at a different time to do a proper validation. The authors have chosen to use two different sites for their validation. However, the underlying parameter values are different, given the different land uses, so this approach has much lower scientific validity.	Available data were extremely limited to calibrate and validate the Port Activities land use. Data consisted of one storm at three separate locations. If multiple storms at each location were available, they would have been used for calibration and validation; however, since only one storm was available at each station, the locations (rather than time periods) had to be divided for calibration and validation.
1.98	pdf p. 22	App. II, page 16, Fig. 5. The match is poor even by visual standards. The authors indicate that "the initial peak was low; however the second peak was fairly close." Again, only a visual comparison. The authors fail to state that they miss the size of the first peak by around 75%, and that their overall pulse is much broader so that they are simulating a much larger total flow than was observed, by a significant factor. Thus, stating	See response to **Comment 1.95**.

No.	Author	Comment	Response
		that it was "fairly close" is rather inaccurate. An analysis of the cumulative flow would have shown that flow at this location is also seriously over predicted. This site has about 8 to 10 times more flow than the Forest subwatershed, so the over predicting is quite significant.	
1.99	pdf p. 22	App. II, page 16, Fig. 6. At least the authors acknowledge that "the validation results did not match the measured flow". In this case, the model seriously under predicts flow, both the peaks and the cumulative flow. This is the most important subwatershed in terms of flow, and it is the worst in terms of model output. Clearly the choice of subwatershed for calibration was a poor one. The authors also mention that they did not adjust the LAR watershed parameters "outside of recommended ranges." Who recommended the range of parameter values? Is there a basis for these ranges? Are the studies or literature values to refer to?	See response to **Comment 1.95** and **Comment 1.96**. The Los Angeles River hydrology calibration and validation is presented in Tetra Tech, 2004 and associated appendices and references to these documents have been provided in Section 3.2.1.2. Model calibration and validation were performed using both quantitative and qualitative techniques; these results include graphical, tabular, and statistical presentation of the observed and modeled flow. The "recommended criteria" in the LAR report for quantification of model error in predicting hydrology were obtained from a U.S. Geological Survey report (Lumb et al., 1994). These were reported to provide a reference for evaluation of model error and were used as a guide for model calibration. For LAR hydrology, several analyses were reported for multiple watersheds that included graphical and tabular comparison of measured and observed flows and volumes. Additional statistical quantitative analysis can be performed for hydrologic results, but such an analysis would provide no indication of the conditions (e.g., high flows or baseflows) or time periods (e.g., seasonal storms) that impact model results, and include specific modeling parameters for characterization. The analysis of hydrologic model error based on volumetric comparisons provided sufficient evaluation of model error for purposes of the LAR study. Lumb, A.M., McCammon, R.B., and Kittle, J.L., Jr. 1994. Users Manual for an Expert System (HSPEXP) for Calibration of the Hydrologic Simulation Program--Fortran: U.S. Geological Survey Water-Resources Investigations Report 94-4168.
1.100	pdf p. 23	App. II, page 17. The authors mention a "robust calibration and validation process" for Ballona, LAR and SGR. What is the basis for	See response to **Comment 1.99**, which discusses the calibration and validation process for LAR (similar

No.	Author	Comment	Response
		saying it is "robust"? Is there a more objective quantification of the quality of the fit? If the current implementation of the LSPC is an example, then one has to wonder what the authors consider as "robust". Clearly, the parameter values were not just transferable, but the authors go ahead and assume this is OK, even after a poor outcome in the calibration and validation process.	techniques were performed for Ballona and SGR). Citations have been added to Section 3.2.2 to refer the reader to the original studies. In addition, the limited amount of new data for calibration and validation did not justify the re-calibration of the LAR, SGR, and Ballona parameter values, which were based on much larger datasets.
1.101	pdf p. 23	App. II, page 17. The paragraph that starts with "Similar to …" should be the first paragraph in section 3.2.2.1.	The suggested change has been made to the Staff Report.
1.102	pdf p. 23	App. II, page 18, Table 4. This is a very good table. A similar table should have been presented in the hydrologic calibration section, with all the hydrologic parameters, showing the adjusted values and the ones from the previous (LAR, SGR) models.	A hydrology parameter table has been added to Section 3.2.1. Other than the addition of the Port Activities land use, these values are identical to the LAR study.
1.103	pdf p. 23	App. II, page 19, Fig. 7. The model clearly over predicts the TSS pulse even in this small subwatershed. The authors indicate that "these discrepancies are well within acceptable modeling ranges." This indicates that either the authors (1) have no significant previous modeling experience; or (2) have no significant scientific integrity. Either way, it is not good. The match is poor, and if this is the best they can obtain, then the resulting load calculations, which rely to a great extent on TSS concentrations are going to be incorrect. If they consider the difference in cumulative TSS load in this pulse between the simulation and the observed values, they will realize that they are simulating a pulse that is probably an order of magnitude greater. That is clearly not "within acceptable modeling ranges." This in addition to the notion that a model can be calibrated based on a single event at one location.	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent.
1.104	pdf p. 23	App. II, page 20, Fig. 8. The authors claim that it is similar to the Forest subwatershed, but given that the highest observed concentration is 200 mg/L vs. 800 mg/L in the simulation, the error is much larger. In addition, the over prediction of total sediment flux is much greater.	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent.
1.105	pdf p.	App. II, page 21, Fig. 9. The simulation does not even resemble the	See response to **Comment 1.95** and **Comment 1.96**. These

No.	Author	Comment	Response
		observed data at all. The model under predicts the sediment load significantly. This of course has to do with the poor hydrologic match.	responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent.
1.106	pdf p. 23	App. II, page 21, Fig. A-2 to A-15. Without a scientifically valid measure of goodness of fit, it can't be stated whether the model predicts the TSS well or not, but in general it appears that the model over predicts them substantially.	See response to **Comment 1.95**. Visual comparison is a common technique for comparison of modeled and observed values, especially in TMDLs, which are required to be based on the best *available* data. Therefore, it is important to note that presentation of statistical evaluation of model uncertainty is not a requirement to justify a model's use for TMDL calculations.
1.107	pdf p. 23	App. II, page 21. Amazingly, the authors have the audacity to state: "Overall, the model appears to reproduce the magnitude of the observed data well." This model has clearly been poorly implemented. Another option is that this model is not applicable to these conditions. But to fool oneself into thinking that the output of the model is valid is incorrect.	See response to **Comment 1.24**, **Comment 1.95**, and **Comment 1.106**.
1.108	pdf p. 23	App. II, page 21. What is the significance of Jan 1995 to July 2005? Why not extend the simulation period to cover the time frame where very good observed data is available for the harbors, in 2006? What is the temporal resolution of the LAR and SGR models for this longer period? Still hourly?	See response to **Comment 1.21** regarding the model period. The temporal resolution for the LAR and SGR models is hourly for January 1995 - July 2005, which is identical to the nearshore model.
1.109	pdf p. 23	App. II, page 22. What was the source of observed data for the concentrations of toxic organics used? There should be a table summarizing the datasets (source, period of record, number of records per toxic, detection limits, etc.) How representative is this data of the entire watershed?	Details on the data sources and locations and provided in the pollutant specific discussions of Section 3.3. Additional detail will be provided in these sections to ensure the source, period of record, number of records, and detection limits are clear. In addition, section references to the pollutant-specific discussions have been added to the introductory paragraph of Section 3.3.
1.110	pdf p. 24	App. II, page 22. The authors indicate that the previously calibrated models (assume it is LSPC models, but should be explicit) of the LAR and SGR were expanded in some way. How were they "expanded"?	The time periods associated with the LSPC models of LAR and SGR were expanded to cover as much time as possible (see response to **Comment 1.21**). These points have been

No.	Author	Comment	Response
		What does this do to the calibration?	clarified in the report. Because the original LAR & SGR model time periods (October 1988 through December 2001 and January 1, 1990 through March 1, 2004, respectively) covered most of the expanded time period of January 1995 - July 2005, no additional calibration was performed.
1.111	pdf p. 24	App. II, page 22. Does the POTFW parameter depend on pH for the metals? Or fraction of organic content for the toxic organics? If not, then this parameter does not truly represent the relationship between sediments and these toxics, and should be improved before using it this way. Are any reactions taken into consideration? If not, state this.	The POTFW parameter is the ratio of constituent yield to sediment outflow and does not take any reactions into consideration. This discussion has been added to Section 3.3.1. POTFW was used to represent metals, not organic compounds. This approach has been used for many other metals TMDLs in the region and several previous studies have validated the use of the POTFW parameter to represent metals loading (see **Comment 1.24**).
1.112	pdf p. 24	App. II, page 23. It is unclear whether the model output is total metal, dissolved metals, or metals in particulate. If only dissolved, how do you account for the load on the TSS? When comparing to observed data, are you comparing the correct fraction? This would make a huge difference.	Model output is in total metals. This clarification will be added to appropriate figure and table captions as well as in the text. The model output is correctly compared to observed total metals.
1.113	pdf p. 24	App. II, page 23. The authors mention that the comparison was graphical. They really mean visual, which as indicated above, is not scientifically acceptable. The authors mention that for these three metals the predicted concentrations are "slightly lower" than the observed concentrations. For Cu the simulated peak concentration is significantly less than half of the observed value. For Pb, there is a larger discrepancy. The least difference is for Zn, but there is still a significant error. The cumulative loads (integrating Figure 11) are seriously over predicted, which is not surprising given the error in flow and TSS. Thus, it is unclear what the authors consider to be "slightly lower" or "fairly close". Again, there is a statement that "these model results are within acceptable modeling ranges" which is rather unnerving. Just like in the previous "calibrations" only one storm event at one location was used to "calibrate" the model. Scientifically this is unacceptable.	See response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.106**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent. Additional description on the evaluation of model fit has been added throughout Section 3.3.

No.	Author	Comment	Response
1.114	pdf p. 24	App. II, Fig. 10. The small negative values for the simulation are an artifact of the graphing software, but should not be presented. They are not real.	Comment noted. The small dips into negative territory on these graphs have been investigated and are an artifact of the graphing software. They could not be corrected without compromising the model output used in the graphs; therefore, the graphs remain unchanged
1.115	pdf p. 24	App. II, page 26, Table 6. The percent differences for the largest subwatershed are around 84 to 87%. Clearly this model is not predicting the correct toxic metal concentrations or loads during wet weather. Given that it under predicts the concentrations, it would result in a higher risk to the environment and humans, since one would be misinformed in the actual levels.	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent. While the model could certainly be improved in future TMDL reopeners with the incorporation of additional data, all of the model parameters associated with the Maritime Museum site are based on the regionally calibrated values and the limited amount of data available for validation are not sufficient to warrant re-calibration of these regional values.
1.116	pdf p. 24	App. II, page 26. Are these EMCs flow-weighted? Unclear, and very important.	The EMCs are flow-weighted. This fact has been clarified in Section 3.3.1.1.
1.117	pdf p. 24	App. II, page 28. These results clearly indicate that this model is not valid. The results are not "well within the ranges of observed data."	See response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.103**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent.
1.118	pdf p. 24	App. II, Fig. A-16 to A-27. Without a scientifically valid measure of goodness of fit, it can't be stated whether the model predicts the metal concentrations well or not, but in general it appears that the model under predicts them substantially at most locations, most of the time.	See response to **Comment 1.95** and **Comment 1.06**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent.
1.119	pdf p. 24	App. II, page 33. This sensitivity analysis is terribly simplistic. These two sediment parameters are important, but there are many others that may play a role in determining the metal concentrations. A thorough review of the hydrologic, sediment and metals parameters in LSPC should be done, and then those that result in the highest sensitivity	KEIM and JEIM are two important parameters representing sediment washoff from impervious surfaces. The calibration and validation subwatersheds are highly impervious areas; therefore, performing sensitivity analyses on these parameters is justified.

No.	Author	Comment	Response
		should be considered. The error bars for the EMC are of interest, but the most important calculation is the load for each metal, not the EMC. The current sensitivity analysis is not scientifically acceptable. The authors are referred to Chapra's book on "Surface Water Quality Modeling", to learn how a sensitivity analysis is performed.	
1.120	pdf p. 25	App. II, page 36. The authors indicate that "Final EMC values for SGR and Coyote Creek were obtained by averaging the three storms EMCs and their respective standard deviations for each reach." Frankly, this sentence makes no sense.	This sentence has been clarified in Section 3.3.2. Essentially, EMCs for three storms were available for both the San Gabriel River and Coyote Creek. A representative EMC was determined for each reach (SGR and Coyote Creek) by averaging the three EMCs in their respective waterbody.
1.121	pdf p. 25	App. II, page 36. The authors indicate that McPherson et al. (2006) "state that in most cases, the total load estimated using EMCs for long-term simulation can have similar accuracy as more complex models." While this is a statement, this has not been proven. The use of EMCs has its place where insufficient data is available, in which case using a more complex model is not going to improve the result. If that is the case, then what was the point of setting up LSPC/HSPF for these watersheds when a simpler calculation could be performed?	LSPC was required for this analysis to estimate flow from the watersheds, which is required to calculate loads into the receiving water model. As stated in the report, a simpler approach (EMCs) was used to estimate the PAH concentrations; LSPC was only used to estimate flows.
1.122	pdf p. 25	App. II, page 36. These sensitivity analyses were again based on just a perceived "most sensitive parameter" without any formal evaluation of other parameters at all. While one can generate different values using different EMCs, it is not valid to assume that this represents the widest range of probably values.	Concentrations and flow were the only values used to calculate PAH EMCs; therefore, no other values were available for sensitivity analyses. While these sensitivity analyses may not represent the widest range of possible values, they do provide an indication of the general range of concentrations and how these values compare to observed measurements.
1.123	pdf p. 25	App. II, Fig. 17. Most of the observed values are outside the plus/minus one standard deviation range according to the model. This indicates that the model does not adequately predict the actual range of concentrations that will be observed. Again, only one storm event is evaluated.	For watershed loading estimates, a balance must be achieved to represent existing conditions using the available data and information. While the observed concentrations are generally higher than the EMC-based predictions at the Forest subwatershed, they are generally lower at the Pier A subwatershed. The average of all observed EMCs for these

No.	Author	Comment	Response
			two subwatersheds is 1,757 ng/L. The average of the Forest and Pier A EMCs is 1,633 ng/L. These average values are within 10%, indicating that the use of EMCs to determine PAH loading is representative of the overall watershed existing conditions. This has been clarified in Section 3.3.2.
1.124	pdf p. 25	App. II, Fig. 18. In this case , most of the observed values are below the lower range based on one standard deviation, so the model over predicts at this location.	See response to **Comment 1.123**.
1.125	pdf p. 25	App. II, Fig. 19. No observed data, so no way to know if the model over or under predicts.	Limited available data made comparison between predicted and observed values impossible.
1.126	pdf p. 25	App. II, page 40. These results for "total PAHs" are only valid for the aggregate, and not for specific PAHs. Since each PAH has its own toxicity and fate and transport, the results are not useful for predicting the actual toxicity of the discharges. The reader should be made aware of this.	A discussion on the nature of specific PAHs vs. total PAHs has been added to Section 3.3.2.
1.127	pdf p. 25	App. II, page 46. The method is acceptable, except that the TSS values used for the calculations are incorrect, so the results are not valid.	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent.
1.128	pdf p. 25	App. II, Fig. 24. Define "Port DL"	Port DL (detection limit) has been defined in Figures 24-26.
1.129	pdf p. 25	App. II, page 50-51. As far as one can gather, for the LAR the authors used observed flow data, but for the SGR they used LSPC modeled flows. Given that the LSPC cannot model dry weather flows, it is unclear how one can use it for some but not all. There is no clear explanation for the inconsistent approach.	LSPC is capable of modeling dry flows; however, separate wet and dry weather approaches were used in the TMDL. Observed wet and dry flow data were used to represent LAR since they allowed for a better fit during salinity calibration in the receiving waters. The predicted SGR flows were determined applicable during receiving water calibration. Both the wet and dry LSPC predicted flows were used since the dry flows included (and were dominated by) the continuous point sources (the best available DMR data were

No.	Author	Comment	Response
			used to represent these point sources). This has been clarified in Section 4. In addition, see response to **Comment 1.25**.
1.130	pdf p. 25	App. II, page 51. The dry weather flows are apparently based on 1 or 2 days of flow monitoring. How do we know those were typical days? The load analysis is being extrapolated to thousands of dry days based on this sample size?	While it is uncertain how "typical" the dry weather sampling days were in the original study used to determine the regression relationship for dry weather flow vs. urban area (Stein and Ackerman, 2007), the study authors did visit each storm drain several times during the month before sampling. These visits would have likely identified "typical" dry weather conditions. The sample size may appear small when considering the total number of days sampled; however, multiple drains within six watersheds were sampled 1-2 times each during dry weather to characterize dry weather flow. Given the entire sample size and the strong relationship established between dry weather flow and urban area, these data were determined suitable to calculate dry weather flow in ungaged watersheds.
1.131	pdf p. 25	▪ App. II, page 51, Table 13. Are the data log-normal? This should be made explicit. The standard deviation seems to be much larger than the mean, so if the data are normal, then the mean minus one standard deviation would be a negative value. Are these total metal or dissolved metal concentrations? Was there enough flow at these locations to mobilize sediments during dry weather flows?	The dry weather data were heavily influenced by a handful of very high concentrations, resulting in high standard deviation values. Analyses were performed to remove these outliers; however, all data were ultimately included because the conditions at the time of sampling were unknown so it was difficult to form a basis for exclusion of specific samples. The mean minus the standard deviation does result in a negative value, but were presented as zero in the report since a negative concentration is impossible. This will be explained as a footnote to Table 13. The data are in total metals, which has been clarified in the report. Flow data were available for some dry weather samples, but it is unknown whether they were enough to mobilize sediments.
1.132	pdf p. 25	App. II, page 51, Table 14. Given the scarcity of data, this approach is adequate for dry weather flows. However, it should be clearly stated that these estimated loads have a high degree of uncertainty, given that they are based on very few observations. The high range may not reflect the	Additional text has been added to section 4.1 to discuss the uncertainty in the loads. See response to **Comment 1.21**.

No.	Author	Comment	Response
		variability in flow, and the "mean" value is not known to the degree indicated in these values (3 to 4 significant digits). At best it is an order of magnitude estimate with one significant digit.	
1.133	pdf p. 26	App. II, page 54. Is there any study that can support the assumption of the sediment composition? Surely the soils in this area have been studied by others in the past.	Sediment composition estimates were developed during TMDL development in the Ballona Creek watershed by SCCWRP. The same estimates were used, here.
1.134	pdf p. 26	App. II, page 55. The "sensitivity analyses" performed are not true sensitivity analyses.	See response to **Comment 1.20**, **Comment 1.91**, **Comment 1.119**, and **Comment 1.122**.
1.135	pdf p. 26	App. II, page 55. To the dry-weather flow and load predictions, add that these are based on data from one day only.	Additional detail regarding the dry weather dataset has been added to the assumptions in Section 5, as per response to **Comment 1.130**.
1.136	pdf p. 26	App. II, page 55. There was no presentation of the estimated the point source and non-point source loads separately. Since this will be needed in the TMDL, this is an important flaw in the presentation. It is not clear that these were actually calculated separately.	Load and wasteload allocations are related to the EFDC model results, as described in Appendix III. Additional information has been added to Section 6 to indicate that the presented loads are total loadings (point and nonpoint source) and references are included for Appendix III for further information on load and wasteload allocations.
1.137	pdf p. 26	App. II, page 55. Similarly, the final results do not separate the dry and wet weather loads for each pollutant. Instead, only "average daily" loads are presented in the figures. Since management actions may be different during these days, lack of this information is a major flaw in the presentation of results. A table presenting the average dry and wet weather loads is needed.	Wet and dry weather loads associated with the watershed output are presented in Appendix I. To minimize duplication between various report sections, references to these tables in Appendix I have been added to Section 6.
1.138	pdf p. 26	App. II, page 55. There is no formal estimate of the uncertainty in the loads. Figures 30-35 should present the error bars that reflect the uncertainty in load estimates. Clearly, given the poor calibration basis (one storm event) and the poor calibration results (as discussed above), there is a considerable amount of uncertainty in the estimated loads. This	See response to **Comment 1.20**, **Comment 1.91**, **Comment 1.119**, and **Comment 1.122** in reference to the comments on the sensitivity analyses and **Comment 1.24**, **Comment 1.95**, and **Comment 1.96** in reference to the comments on calibration.

No.	Author	Comment	Response
		information is very important for the TMDL. These sensitivity analyses do not adequately reflect the uncertainty in the calculations.	
1.139	pdf p. 26	App. II, page 55. The EMCs and other land use based load estimates have been considered for the "industrial" land use as if this was a typical mix of industries. However, near the harbors there are many facilities which are clearly "heavy industry", including refineries and other chemical processors, which are likely to generate much higher loads than light industry, or even a mix of industrial sources. One could look at the Toxic Release Inventory information for the facilities in this area to have a much better idea of the types of sources. These sources are very close to the waterways and harbors, so the transport pathway is short. Since this has not even been mentioned in the report, or particularly in these modeling assumptions, it is likely that this was not taken into consideration by the authors. Thus, the load estimates are likely to be incorrect.	See response to **Comment 1.23** and **Comment 1.85**. The model was generally populated based on regionally calibrated parameters and very limited local data were available and did not justify refinement of these parameters. In addition, data were not available on hot spots for inclusion in the LSPC model as point sources. If these data become available in the future, revisions can be made to the LSPC model during TMDL reopeners in the future.
1.140	pdf p. 26	App. II, page 56, Figure 29. Label hard to read.	The legend of Figure 29 has been increased for better readability.
1.141	pdf p. 26	App. II, page II-i. Typo: heavily "rely" not "reply"	The typo on page II-i has been corrected.
1.142	pdf p. 26	App. II, page 51. Typo: Change "verses" to "versus"	The typo in the caption for Figure 27 has been corrected.
1.143	pdf p. 26	**Comments on Appendix III** App. III.1, Page 3. The time period for the EFDC model was 2002 to 2005, while the best observed data is from 2006. This does not make sense, and the explanation for truncating the simulation in 2005 is that the LSPC models were simulated from 1995 to 2005. Why not extend the LSPC simulations to 2006?	The LSPC watershed modeling was completed in 2006 – thus the modeling period went through 2005 and incorporated the available data to date at that time. The modeling period for the EFDC receiving water model was based on the watershed modeling period since this output was required as EFDC input. The 2006 observed data became available later in the modeling process and in an effort to continue making progress on the technical aspects of these TMDLs with finite resources, the final modeling period was not adjusted to include these data for calibration/validation (rather they were used to represent

No.	Author	Comment	Response
			initial bed conditions to improve sample size and monitoring station distribution).
1.144	pdf p. 27	App. III, Page 4 and others: as in the rest of the report, tables and figures should be numbered so that they can be referenced in the text, and some interpretation of the information in each table and figure should be provided in the manuscript.	Comment noted.
1.145	pdf p. 27	App. III, Page 4, Waterbody Information table. The deposition rates are not known to such precision, and should thus be reported only to the degree that the calculations justify. I doubt there are more than 2 or 3 significant figures, but not 7 (e.g. 1,564,089 kg/yr). Good scientific practice requires one to report the correct precision. In the caption it mentions "TMDL waterbody", but in reality these are "TMDL zones".	See response to comment 1.21 The caption has been modified to refer to TMDL zones.
1.146	pdf p. 27	App. III, Page 4, Sediment Concentration Information table. It is unclear if these are total metal concentrations, dissolved or adsorbed. For the toxic organics it is clear that these are total concentrations, so it is even more confusing.	The table includes total concentrations for both metals and organics. This has been clarified in the table and associated text.
1.147	pdf p. 27	App. III, Page 5. The text reads "The areas and percentages below are…" Percentages of what? Should be clear that these are percentages of freshwater inputs.	The percentages refer to the percentage of total area draining to that waterbody. This has been clarified in the table and associated text.
1.148	pdf p. 27	App. III.2, Page 7. The threshold for wet weather days is inconsistent. For the Dominguez Channel the 90th percentile flow is used, while for the near shore watersheds, the 50th percentile flow is used (Appendix II, page 4). This can make a significant difference in the load considered for different watersheds, given the different approaches used for dry and wet weather. The load duration curves were apparently developed using only the wet days. Given the relative few wet days in this region, this may bias the analysis. No effort seems to have been made to determine the impact of this decision. Although the flow and loads in dry days is smaller, the cumulative contribution to the harbor waters can be quite	As indicated above (**Comment 1.87**), the dry weather flow cutoff for the nearshore areas should read 90th percentile. This typographical error in Appendix II has been corrected. Only the wet days were used in the load duration curves because the Dominguez Channel freshwater TMDLs for metals are for wet weather only. The text in Appendix III.2 has been updated to ensure this point is clear.

No.	Author	Comment	Response
		significant over time. There does not appear to be any indication of the relative contribution of dry and wet days to the total load.	
1.149	pdf p. 27	App. III.2, Page 8. The "Allowable Loads" presented in the table do not match those presented in page 11. The difference is quite significant. There is reference to a section in the entire report where these Allowable Loads are calculated. Since this is critical for the TMDL calculation, it should be a transparent presentation. What is the uncertainty in the calculation of these Allowable Loads? Clearly there are many data gaps, so there must be some sense of the major uncertainties. Again, reporting these values to a high precision gives the false impression of certainty.	The wet weather metals TMDL for Dominguez Channel freshwater is presented as a concentration multiplied by wet weather storm volume. The values presented on page 11 are an example based on the 90th percentile flow volume. The average annual loads on page 8 are based on the average allowable loads (values that fall below the load capacity curve) over an 11-year simulation period. These account for flows greater than the 90th percentile flow volume, so they are expected to be higher than the average daily flow example provided on page 11. Additional description has been added to Appendix III to clarify the use and calculation of these values (including appropriate references to the staff report). See also response to **Comment 1.20** and **Comment 1.95** regarding the comments on uncertainty.
1.150	pdf p. 27	App. III.2, Page 9-10. The y-axis labels are unreadable, and the numbers in the x-axis are also unreadable.	The axes for both of these figures have been corrected.
1.151	pdf p. 27	App. III.2, Page 11. A 10% explicit MOS is considered. No justification is given. Given the data gaps, it is very hard to justify such a small MOS.	See response to comment 1.43.
1.152	pdf p. 28	App. III.3, Page 13. The method for determining the initial concentrations is not discussed at all. Given that this is a complex calculation based on data from several years and locations, and that it is crucial for an adequate estimate of the concentrations of toxics over time, it is a major deficiency in the report. Was equal weight given to all data? If not, what were the weights?	Initial concentrations were based on observed data as per Section 7.1 in Appendix I. Specifically, observed concentrations were used at their specific location and then the concentrations in between the individual data points were estimated by interpolating between the known concentrations. The model is very insensitive to sediment and contaminant concentration in the water column since this is a dynamic tidal environment. Initial water column sediment and contaminant concentrations wash out quickly due to higher inflows in early 2002. The text in Section 7.1

No.	Author	Comment	Response
		How can one use data from 2006, past the simulation period, to determine the initial concentrations in 2002? There is no scientific basis for doing this, since the only method for back calculating the concentrations from 2006 to 2002 is the model that is being calibrated. The authors have a serious problem with circular logic. In addition, there is no scientific basis for reporting the concentration to such a high precision. Laboratory results do not have such precision. Perhaps the authors could take a look at a few lab reports to understand the actual precision of such data.	has been clarified to describe the assignment of initial bed concentrations and appropriate section references will be added to Appendix III. See response to **Comment 1.33** for more discussion on the data used for initial conditions. The organic compounds are listed here in mg/kg dry, while the analytical detection methods are 1000fold lower, so it is OK to have such precision. The metals results are also in mg/kg dry wt., Staff concur the precision is more than analytical methods. See also response to **Comment 1.21**.
1.153	pdf p. 28	App. III.6, Page 50, Table 2. Correct notation is $\mu g/m^2$-day, not $\mu g/m^2/day$. Also, it is better to present a range of values, or some other measure of their variability. Clearly, their sources are different and meteorological conditions play a major role. There is no discussion of these considerations; one must assume that this was not taken into account.	The notation has been corrected.
1.154	pdf p. 28	App. III.6, Page 51. There is no calculation of the uncertainty in these estimates. Since these loads are an important part of the TMDL calculation, it is important to determine the uncertainty.	Error bars for the LA Harbor air deposition data are presented in Appendix III.5.
1.155	pdf p. 28	App. III.8, Page 2, Figure 1. Hard to read label.	The figure legend size has been increased to improve readability.
1.156	pdf p. 28	App. III.8, Page 2. A four-day average was considered. Is there a regulatory or scientific basis for this selection? What is the objective of such averaging? This tends to smooth out peaks in concentrations, which may under protect organisms that are exposed to such peaks.	The four-day average was a useful comparison as it is directly comparable the chronic CTR water quality criteria. Water column concentrations were initially evaluated using the CTR criteria, while subsequent comparisons focused on

No.	Author	Comment	Response
			the sediment concentrations.
1.157	pdf p. 28	App. III.8, Page 4, Table 2 & 3. While the comment is made that almost all of the TMDL zones exceed the criterion (34 mg/kg) even when all the upland sources are eliminated, it is clear from Figure 3 that this is a matter of time. The simulation ended after 4 years, but if additional time was taken into consideration, in fact most zones would eventually meet the criterion. Some may take too much time, and thus additional actions may be needed. However, the current analysis does not point out this important finding. Eliminating or reducing upland sources does have a very positive effect, as expected. The current text implies that there is little or no value in doing so, since the criterion is still exceeded. The authors could have done further analysis to determine why there are some locations that respond very rapidly and some that almost do not respond, to guide the development of the TMDL.	Figure 3 shows sediment concentrations of copper dropping with time in Dominguez Channel and Consolidated Slip in response to elimination of upland loads. For the upland load elimination, the sediment load is not reduced (just pollutant loads), thus the response in these two zones is due to dilution in the bed due to deposition of clean sediment. Note that Cabrillo Beach and Fish Harbor show no change since they receive very little clean sediment. Additional discussion has been added to describe the impacts of the clean sediment and simulation time period on the results.
1.158	pdf p. 28	App. III.8, Page 5, Fig. 2. Why use negative values in the x-axis? What is the significance of starting day 0 in the middle of the simulation? Why not use actual dates?	The model simulations originally spanned 2003 to 2005 with time zero set to 1 Jan 2003. Year 2002 was subsequently added but the original time origin was retained. The axis titles for all of these similar figures have been updated include actual dates.
1.159	pdf p. 29	App. III.8, Page 11. These two figures are very similar, if not identical. Is this a mistake? It is hard to understand how the PAH concentrations would decrease so rapidly in the Base Scenario. If this was the case, one would not need to do anything but wait. Also refers to Figures 10-11.	The interpretation for these Figures is that the high levels of PAH in the sediment bed pore water in relation to the water column drive a significant diffusion flux of PAH from the top of the bed. The flattening of the curve indicates that equilibrium is being approached. Additional discussion has been added to the text to describe this.
1.160	pdf p. 29	App. III.8, Page 15. The authors mention that “copper hot spots within all zones were reduced”. How were they reduced? There was no mention of this previously in the report. This could be quite important to know. How was this information considered in the TMDL calculation?	As indicated in the memo, the concentrations of hot spots were reduced until the average zone concentration achieved the sediment criteria. This scenario was part of additional TMDL implementation scenarios performed. The text in this section has been revised to more clearly describe the various scenarios performed and how they were included in the

No.	Author	Comment	Response
			TMDLs.
2	Patrick Brezonik (Professor Emeritus), University of Minnesota		
2.1	pdf p. 1	**Overview** My initial impression upon starting to read the TMDL document was favorable. It was clear that a very large effort went into the development of the document and its associated appendices. The Introduction and Problem Statement sections are well written, and the analysis of impairments identified in 303(d) lists, as well as the assessment of findings for each water body is thorough. Unfortunately, as I continued to read the report, my opinion became less positive. The writing in key sections on numeric targets and the TMDL development (sections 3 and 6) was unclear, and I had difficulty understanding the scientific basis for some numeric targets and TMDLs.	Comments noted and responded to in detail, below.
2.2	pdf p. 1	My opinion further declined as I read the two appendices related to the critical modeling components. Although the models that the authors used are widely used and represent the state-of-the-art in watershed and hydrodynamic modeling, the calibrations were poor to mediocre. Similarly, although an attempt was made at model validation for some of the contaminants, it was not successful. As a result, to the extent that the models were used to generate the TMDLs, WLAs and LAs, I do not think that much confidence can be placed in the numbers.	See response to **Comment 1.24** and **Comment 1.95** for a discussion on the regional modeling approach and further calibration and validation performed in the nearshore watersheds.
2.3	pdf p. 1	A broad framework is provided in the TMDL document for the implementation plan, which includes a monitoring program. Actual details of the implementation plan and monitoring program are left to the responsible parties to develop. Additional monitoring of water and sediment quality is critically important, not only to gather information on the extent to which compliance with the TMDL objectives is achieved, but equally important to provide more and better data to calibrate and validate the models on which the TMDLs were based.	Additional sediment monitoring is required by responsible parties subject to the TMDL and may be used to update the model if the TMDL is reconsidered for that purpose.
2.4	pdf p. 1	An analysis of costs to implement the TMDL is provided at the end of	Comment noted; staff notes that economic impact is not a

No.	Author	Comment	Response
		the report. The authors indicated that such an analysis was not a requirement of the TMDL process but presented it anyway. I found the analysis to be largely superficial, but if one accepts the numbers generated in that analysis to be even roughly correct, it is clear that the implementation will impose a large economic burden on the region. It is not within my role as a reviewer of the scientific merits of the TMDL report to make judgments on the economic impacts relative to the need or desirability of various components of the implementation program. In my opinion, however, it is within my purview to state that given the high projected costs, the science behind the analyses leading to the TMDLs (and thus the necessity for implementing BMPs and sediment remediation) needs to be sound and the results need to be reliable. I conclude that unfortunately the current TMDL document does not meet this standard.	scientific or technical matter requiring peer review.
2.5	pdf p. 2	**Responses to Major Issues** **1. Appropriateness of selected … targets …** The numeric targets were based largely on state and federal water quality standards and criteria. These standards and criteria were developed over many years based on the best scientific information available, and I do not have any basis for criticizing them. Even if I did, I think that the authors of the report were constrained legally to use these values. The TMDL document notes that there are no numeric standards for sediments (called sediment quality objectives) in the California Toxics Rule (CTR), but the TMDL document relied on guidelines in a 2006 study on the development of California's 303d (impaired waters) list to develop the sediment quality guidelines (Table 2-4 of the document) that were used to assess whether sediments were impaired or not. This approach seems reasonable.	Staff agrees the approach is reasonable.
2.6	pdf p. 2	Nonetheless, I found Section 3 "Numeric Targets," (pp. 43-52) very difficult to follow and understand (see my detailed remarks regarding these pages in the section of this review titled "Other Comments"). The section on numeric criteria for chronic toxicity (pp. 44-45) lacks clarity. For example, I don't understand what the authors mean when they say	Comment noted and also responded specifically in responses, below. TUc = 100% ÷ the sample concentration, derived using hypothesis testing, to cause no observable effect, with the

No.	Author	Comment	Response
		"sample concentration was expressed as a percentage" (p. 45, below equation 1). Percentage of what? In the end, I was unable to make a firm conclusion regarding the scientific validity of the specific numeric targets because of the lack of clarity and details in the section.	"sample concentration expressed as a percentage" means the percentage of the water tested i.e. the dilution. See also response to **Comment 1.79.**
2.7	pdf p. 2	**2. *Development of the sources and linkage analysis …*** The authors of the TMDL document clearly expended considerable efforts in gathering background data for their modeling efforts. This included extensive historical information on water and sediment quality in the subject water bodies, as well as data on fish tissue levels of contaminants. The analysis of existing conditions appears to be thorough and credible, and the remaining uncertainties regarding the degree of impairment in the water bodies and their sediments reflect the absence or inadequacies of past monitoring programs rather than insufficient efforts on the part of the authors.	Staff agrees. In addition, see response to **Comment 1.17.**
2.8	pdf p. 2	To run the contaminant loading models (LSPC and EFDC), the authors obtained detailed information on point and nonpoint pollution sources in the watershed, detailed watershed information needed to configure the models to the complicated set of watersheds in the study, and a variety of meteorological and water data needed to calibrate the basic hydrodynamic components of the model and the water quality (pollutant transport and fate) components of the model. As the authors note, the models they used are widely used in the environmental engineering community for surface water modeling in complicated systems, and they are accepted and supported by the U.S. EPA.	Staff agrees.
2.9	pdf p. 2	I have no criticism of the models per se except to note that such models do much better at simulating the movement of water itself than they do in modeling/predicting the transport and fate of non-conservative substances (e.g., pollutants) in the water. This is because the physics of water movement is well understood and can be described quantitatively by mathematical equations with physical coefficients that can be determined with fair accuracy. In contrast we simply do not understand how organic pollutants or metals behave sufficiently to write analytical	Staff generally agrees with these comments. Modeling sediment and contaminant transport and fate in this and other studies is limited by numerous factors including the high level of variability in sediment and contaminant levels. The observational data for sediment and contaminants used in this study is far from robust for modeling use. It is noted that all of the data were collected before the modeling study was conducted; therefore, the goals of the data collection were

No.	Author	Comment	Response
		equations with coefficients that are truly fundamental. In spite of their apparently "analytical nature," when models like EFDC are used to simulate the environmental behavior of non-conservative chemicals or biological components, they become inherently empirical, meaning that the accuracy of their simulations depends strongly on the availability of a robust set of calibration data.	different than if the data were collected specifically to support a modeling effort.
2.10	pdf p. 3	The calibration exercises conducted by the authors for the TMDL study showed that the model generally did a good job in simulating water flows (at least insofar as water surface elevations at a NOAA tide gauge appear to be close to observed water levels. Results were not quite as good for modeled versus measured salinity, but part of the problem here is that many of the stations do not show substantial variations over time in salinity. In contrast, modeled trends generally did not accurately fit observed values for concentrations or loads of the three heavy metals (Cu, Pb, Zn) either in the subwatersheds used to calibrate the model or (even more strongly) in the subwatersheds used for model validation. The authors state several times (e.g., Appendix II, p. 15) that the differences between observed and modeled results were small and well within acceptable modeling ranges, but I simply do not agree with this statement. Furthermore, the validation results that are presented in the TMDL document and Appendix II really do not "validate" the accuracy of the model nor do they demonstrate that it is able to predict the behavior of the metals in the system with sufficient accuracy for the purposes needed in the TMDL analysis. (Just because one conducts a validation exercise does not mean that a model has been validated.)	As this comment refers to model calibration, please see response to **Comment 1.95** and **Comment 1.99**. Overall, the limited amount of new data for calibration and validation did not justify the re-calibration of the LAR, SGR, and Ballona parameter values, which were based on much larger datasets. The Port Activities land use was the only land use parameterized during this study, which had very limited data available for model comparison.
2.11	pdf p. 3	There are at least two reasons why the calibration/validation exercise failed. First, as the authors point out, there was a paucity of data that could be used for calibration and validation purposes. This was especially the case for the organic pollutants, for which within-event calibration data were almost completely lacking. Perhaps this can be rectified by establishing a monitoring program (which is part of the implementation phase). Second, the model itself simply may not be sufficiently defined and refined to simulate the behavior of the pollutants	Comment noted. Staff agrees with the comments regarding the use of constant values of equilibrium partitioning. The EFDC model has the capability to accommodate more sophisticated specification of space and time variable equilibrium partitioning such as three-phase partitioning or incorporation of spatially variable solids and dissolved and particulate organic carbon dependence. Evaluation of information from this site did not support use of these

No.	Author	Comment	Response
		in this system. The equations describing the behavior of metals in the model are not described in any detail in the TMDL document or modeling appendices, but my impression from the latter documents is that metal behavior is modeled primarily in terms of a partition coefficient, K_p, that quantifies the amount of metal in the dissolved state and that sorbed onto suspended particles. The behavior of the former presumably is modeled by water transport and the latter is modeled by equations intended to predict the settling and scouring of suspended particles. This certainly is a simplification of the complicated chemical and biological processes that affect behavior of the metals in aquatic systems, but it may be adequate if two conditions are met: (1) sorption/desorption to/from suspended sediments is the dominant process, and (2) this process can be quantified in terms of a single value for K_p. The results presented in Appendix I, Figure 31 (p. 47) clearly show that the latter is not the case. Values of K_p exhibit a wide range for all three metals, and they do not show a predictable relationship with the concentration of suspended solids. Consequently, the use of a single (average) value of K_p in the modeling effort is inappropriate and may account for much of discrepancy between modeled and observed concentrations and loads.	options. Likewise there is a research version of the model with non-equilibrium partitioning which, of course, requires even more information available only from long-term laboratory studies.
2.12	pdf p. 3	Use of the complicated hydrodynamic model may have been intended to give the impression that the authors used a sophisticated modeling approach, but given the lack of fit and inadequacy of calibration data, the results are no more reliable than if the authors had used simpler, more empirical approaches (e.g., plug-flow and completely-mixed reactor models) to conduct their loading and transport studies.	The hydrodynamic model was a useful tool as it can incorporate dynamic loadings from the watersheds to represent both existing conditions as well as possible load reduction scenarios. It can also be used to evaluate sediment bed concentrations upon clean-up of specific toxic hot spots. This will prove especially useful during implementation and as new data become available to update the loadings during future TMDL reconsiderations.
2.13	pdf p. 3	For further comments on this topic, see comments for pp. 69-80 of the TMDL document and all the comments for Appendices I and II in the section of this review title “Other Comments.”	Comment noted and responded to in detail, below.
2.14	pdf p. 3	3. Calculating loading capacity (TMDLs).	Because 1 TUc represents water with, essentially, no chronic

No.	Author	Comment	Response
		In many cases explanations in the section on TMDLs are given considerably after the results are presented or are not given at all, making it very difficult for readers to understand what was done and what the basis for the TMDL really was. Overall, this section of the report was difficult to follow and understand. As a result, I am not able to provide a firm conclusion about the validity of the final results. One example regarding the lack of clarity involves Table 6-1, which provides WLAs and LAs based on toxicity criteria. It would seem that the various loads would be additive to the overall toxicity of the receiving water and thus the TUc values should be distributed fractionally among the dischargers. Perhaps I just don't understand what was done and how the calculations were made, but I do not think the report provides an adequate description for me to develop this understanding. Similarly, I was not able to figure out how the wet-weather loading capacities in Table 6-2 were obtained.	toxicity, it is not feasible to partition this allocation amongst several dischargers; i.e., a discharger cannot have less than no toxicity. Section 6.2.1 provides the equation for calculating the wet weather metals TMDLs in Dominguez Channel freshwaters. On any given day two variables may change: (1) Daily storm volume, which is contingent on amount of stream flow through/past stream gage S28 within 24 hours; and (2) the numeric target, which is metal-specific and hardness dependent, along with site specific conversion factor for translating to total recoverable metals. Wet weather loads in Table 6-2 were calculated by using 62.7 cfs/day (or 1.5×10^8 L) and the numeric targets (based on hardness = 50 mg/L and conversion factors in Table 3-2)
2.15	pdf p. 4	It is clear from previous sections of the TMDL document that large uncertainties exist in the modeling and analyses and that the available data is not sufficient in many respects. Given this situation, it seems to me that the small margin of safety (10%) provided in Table 6-4 is unrealistic. The values reported in Table 6-8 presumably represent 95 percentile values of historical data, but the text is not clear regarding how they "translate" to either a WLA. Similarly, the meaning of the TMDL values and allocations for bed sediments in Table 6-10 is not clear, and with regard to the first note at the bottom of this table, it is not obvious why no reductions in atmospheric deposition of Cu, Zn and PAHs should be anticipated. If atmospheric sources are contributing to the problem, they should be subject to regulation just as much as land-based point and non-point sources.	See response to **Comment 1.43** and **1.45.** The allocations are based on concentration, so when the 95% is calculated, that number is directly the allocation. Atmospheric source are not regulated by the Rgeional Board, so the TMDL makes the conservative assumption that atmospheric sources will stay the same.
2.16	pdf p. 4	It also is not obvious why an implicit margin of safety exists in the final allocations to Dominguez Channel estuary and the greater Harbor waters (Section 6.5.3) just because multiple numeric targets were selected.	See response to **Comment 1.43** and **1.45.** Additionally, the targets were selected based on water quality objectives, sediment quality guidelines, and fish contaminant goals

No.	Author	Comment	Response
		They all could be "unprotective."	known to be protective of beneficial uses.
2.17	pdf p. 4	Finally, I wonder whether the tiny values listed in Table 6-12 for DDT and PCB WLAs are meaningful. Could one actually make measurements to show that a discharge was in compliance with a WLA of 0.35 g/yr? In general, the numbers in the table seem unreasonably low.	A TMDL is required to calculate the appropriate allocation.
2.18	pdf p. 4	**4. Development of a proposed monitoring program to assess effectiveness of the TMDL and attainment of water quality goals.** The proposed monitoring program is an essential component of the TMDL implementation. The data that will be obtained will be critically important not only for compliance purposes but also for improving the database available for calibrating transport and fate models. In an adaptive management context, this will allow improvement of the analyses conducted originally as part of the TMDL study, thus likely allowing modification and improvement of the implementation plan, as well as the TMDL targets themselves.	Staff agrees.
2.19	pdf p. 4	The water parts of the monitoring program appear generally to be sound. In particular, the requirement to monitor two wet-weather and one dry-weather events each year, including the first major wet-weather event of the season, is reasonable. The monitoring plan described in section 7.6, starting on p. 116 of the TMDL document, does not provide sufficient information, however, on the nature of the sampling frequency within the wet and dry events. This may be spelled out in the SWAMP protocol and various MRPs and QAPPs, but it would be appropriate for the document at least to specify that sufficient samples should be taken within events to define the "pollutograph"—that is, the concentration and load versus time over the period of the event. In addition, the report does not provide specific information on the number and location of storm drain sites that will need to be monitored. I believe the report easily could be modified to present this information, which would make it much easier to evaluate the adequacy of the monitoring program. Finally, it is not clear what is meant by a dry-weather "event." It would be useful for the report to clarify this terminology and also the timing	Staff agrees and notes that the implementation schedule requires responsible parties to develop the detailed monitoring plan(s) for Executive Officer approval.

No.	Author	Comment	Response
		and duration of a dry-weather sampling program.	
2.20	pdf p. 4	I doubt that it makes sense to require analyses of filtered water samples for dissolved DDT, chlordane, PAHs and PCBs at all sites. It is known from many studies in the literature that these highly hydrophobic substances occur on particulate phases rather than in the dissolved phase, and prior work in these watersheds (described in the document) has shown that levels generally are undetectable in the water itself. It may be appropriate, however, to require collection of dissolved natural organic matter (NOM) and analysis of this material for the above mentioned pollutants if dissolved organic carbon (DOC) concentrations in the stormwater are known to be high; this usually is done by passing water samples through columns of resins like DAX-8 and extracting the sorbed NOM. It is well known that organic pollutants sorb onto macromolecular NOM, which operationally is a part of the "dissolved" fraction when water samples are filtered using conventional filters. Given the geological and climatic conditions in the Los Angeles region, I doubt that DOC (and dissolved NOM) is high enough in surface waters of region to represent a significant transport medium for the pollutants, but aquatic chemists in the region should be able to evaluate this.	EPA methods require measuring DDT and PCB compounds in unfiltered water samples. This is collected and analyzed because of the hydrophobic nature of these chemicals and their high affinity for particulate matter. Staff agrees that there is little reason to suspect DOC values to be above normal for urban stormwater in the Los Angeles Region watersheds.
2.21	pdf p. 5	Sampling of sediments and fish within the various units of Dominguez Channel and the Greater Harbor also is a component of the monitoring program. Although the proposed sampling frequency of every five years may be sufficient for compliance purposes, in my opinion, it is not sufficient to improve the database needed for better calibration and validation of the transport and fate models. Therefore, I recommend that sampling and analyses of sediment and fish should be undertaken at least every two years for an initial period—until sufficient data are obtained to improve the models. It may be possible for this sampling to be done at fewer sites than needed for the five-year compliance monitoring, but sampling will need to be based on the requirements to achieve the goal of improved scientific understanding of pollutant distributions and dynamics in sediments and fish of the system rather than on compliance issues. The TMDL document does not necessarily	While sediment triad data is to be collected every five years, in fact, fish tissue samples are required to be collected every two years. See page 119 of the Staff Report. See also response to **Comment 2.19**.

No.	Author	Comment	Response
		need to include details on the exact sites to be included in this more frequent sampling, but it should be modified to address the need for more and better data to achieve the aforementioned goal.	
2.22	pdf p. 5	**5. Evaluation of implementation plan and allocations.** Insofar as I lack confidence in the results of the EFDC model used to generate the proposed implementation plan and allocations, I must conclude that the TMDL report does not provide a sufficient scientific basis for the proposed plan and allocations. That said, the report does provide a sound general approach to implementation that involves five broad processes: 1) implement and evaluate the effectiveness of BMPs and source control in conjunction with remediation to remove contaminated sediments; 2) evaluate the effectiveness of controlling sediment loading from major river sources (Los Angeles and San Gabriel Rivers and Machado Lake) through implementing effective TMDLs; 3) conduct compliance monitoring; 4) determine whether reductions in loadings from controllable sources in the Los Angeles and San Gabriel Rivers will be required and addressed through revision of the TMDL; and 5) re-evaluate the WLAs and LAs, as necessary.	For the EFDC comment, see response to **Comment 1.37**. Staff agrees that the implementation strategy is sound.
2.23	pdf p. 5	Overall, the implementation plan provides a general framework for implementation rather than specific details, which are left to the "responsible parties" (local agencies and governmental units in the affected area) to develop. The implementation plan also is not prescriptive in stating specific activities, including BMPs, that should be undertaken to achieve the WLAs. In one sense, this approach is good in that it allows for local decisions to be made based on local knowledge. On the other hand, the approach adds uncertainty and vagueness to the implementation phase.	While agreeing with the reviewer on the pros and cons of the implementation approach, staff notes that the Regional Board is prevented legally from prescribing the methods of compliance.
2.24	pdf p. 5	The implementation plan is also described as consisting of three phases. Phase I includes incorporating interim limits into NPDES permits and waste discharge requirements, implementing BMPs in the watersheds, implementing TMDLs for the Los Angeles and San Gabriel Rivers and Machado Lake, and developing and initiating a monitoring program.	Staff agrees with the phased implementation approach and notes the comment on adaptive management.

No.	Author	Comment	Response
		Phase II extends the implementation to clean-up of high priority areas, including sediment removal in harbor areas, implementation of additional BMPs, and other targeted source reduction activities identified in Phase I. Plans for Phase III are very sketchy and simply state that secondary and additional remediation actions as necessary will be implemented to insure compliance with final load allocations by the end of the implementation period. Table 7-2 (p. 122) indicates that Phase I should last five years, Phase II ten years, and Phase III an additional five years of the total 20-year implementation plan. Overall, the idea of a phased approach makes sense, and although the report does not use the term "adaptive management," the implementation plan does have many elements of adaptive management. Considering the very large costs associated with implementation of this TMDL, I agree that a phased approach is appropriate, and I also recommend that the implementing agencies develop an implementation approach that specifically follows the principles of adaptive management.	
2.25	pdf p. 5	Clearly, the implementing agencies will need to develop more detailed plans for the three phases than are presented in the TMDL document. Although it is not feasible at the outset to provide as much specificity for Phase II as for Phase I, the plan at least should describe the mechanism and timing for formulating a detailed Phase II plan, and a similar requirement should exist regarding Phase III plans.	More detail has been added to the Staff Report and Basin Plan Amendment regarding Phase II and III implementation plans.
2.26	pdf p. 6	Processes 2 and 4 in the implementation plan involve actions outside the domain of this TMDL, specifically, the development of separate TMDLs for the Los Angeles and San Gabriel Rivers and Machado Lake. The latter is a small and apparently impaired water body between Wilmington and Harbor City and west of I-110, just north of Los Angeles Harbor. Given the proximity of Machado Lake to the harbor and the fact that it drains into the harbor, it is difficult to understand why this water body was not part of the present TMDL or at least why it was not described in more detail in the TMDL document. Its location is not even noted in Figure 2-1, although I believe it is present on the map as an unnamed water body just northwest of the Los Angeles Inner Harbor.	Comments noted. In addition, a toxics TMDL for Machado Lake has been completed and staff anticipates final approvals for the TMDL this year.

No.	Author	Comment	Response
		Overall, this situation (i.e., three additional TMDLs being required to fully implement the TMDL for Dominguez Channel and the Greater Harbors) represents an unfortunate complication, but I understand that this may reflect legal requirements and is not necessarily an issue relevant to the scientific review of the TMDL document.	
2.27	pdf p. 6	**Responses to overarching questions** **(a)** ***Are there additional scientific issues that are part of the scientific basis of the proposed rule not described above?*** There may be other issues, but I believe that my major concerns with the proposed rule and its scientific basis have been addressed in responding to the above five issues and in the comments included below, which were developed as I observed issues and problems while reading the report and associated appendices.	Comment noted and responded to in detail, below.
2.28	pdf p. 6	**(b)** ***Taken as a whole, is the scientific portion of the proposed rule based on sound scientific knowledge, methods and practices?*** The authors of the report show clear evidence of detailed familiarity with scientific knowledge about the environmental problems in Dominguez Channel and Los Angeles and Long Beach Harbors and about the scientific bases for addressing these issues. In addition, the scientific portion of the proposed rule relied on generally accepted and sound scientific methods. For example, the models used in the study are generally accepted as "state-of-the-art" and are widely used by both government agencies and scientists and engineers in the private sector. The application of sound scientific practices was not always followed, however. Examples of instances where there was a lapse of sound scientific practices range from small statistical issues, such as using regression analysis when the basic assumptions inherent in the method were not present in the data (e.g., see comments on pages 52 and 53 of Appendix II below), to much larger issues like the continued use of the EFDC model to determine transport and fate of pollutants in the system in spite of the fact that the calibrations and validations showed that the model did not come close to matching the observed values.	See response to **Comment 2.96** and **Comment 2.97** below and **Comment 1.37** above. The EFDC model could be greatly improved with the collection of additional field data, especially if the design of the field program has significant input from the modelers. As noted, the observational data used was collected in advance of the modeling study and likely with no anticipation of its use for modeling.

No.	Author	Comment	Response
2.29	pdf p. 7	**Other Comments** Page 31, Line 4 from bottom: The text makes reference to summary tables for all the data but does not indicate where these tables are located.	The locations have been clarified in the Staff Report. We have removed reference to these summary tables of sediment quality information.
2.30	pdf p. 7	Page 39, Paragraph 2.6.3 and subsequent ones: No summary statement is provided regarding conclusions on what is impaired, as was done in paragraphs 2.6.1 and 2.6.2.	A summary of the impairments for each waterbody-pollutant combination is found in Table 2-18..
2.31	pdf p. 7	Page 39, Paragraph 2.6.5 and subsequent paragraphs: I don't understand what the authors mean by "certain DDT and PCBs…" As far as I am aware there is only one kind of DDT, although there are several DDT degradation products.	The statement has been clarified.
2.32	pdf p. 7	Page 43, Line 2 above Table 3-1: I don't understand what the authors mean by "…the CTR vice…"	The statement has been changed to "...the CTR versus..."
2.33	pdf p. 7	Page 43, Table 3-1: The relevance of including a water quality criterion for mercury in water based on protection of human health is not obvious. Previous text did not establish that there was any problem with mercury concentrations in the water column of any of the water bodies.	Mercury is an impairment in both the Consolidated Slip and Fish Harbor.
2.34	pdf p. 7	Page 44, Table 3-2: It is not obvious to this reviewer how the freshwater wet weather metal targets in this table were obtained, nor is it clear what is meant by "translators," or why this was done.	The "translator" is the hardness-specific conversion factor, as required by CTR, to accommodate the differing toxicity of metals at different water hardnesses.
2.35	pdf p. 7	Page 47, Paragraph 2 of Section 3.2.2: Insufficient information is provided on the benthic invertebrate indices, including their nature and references to literature on them.	The Staff Report which supported the Sediment Quality Plan (July 18, 2008) includes the references for the benthic invertebrate indices. It was not the intention of this Staff Report to repeat the science used to develop the Sediment Quality Plan.
2.36	pdf p. 7	Page 47, Second last paragraph: The text states that the combination of the four benthic invertebrate indices provides more information than any single index. I am not convinced that this is the case if all one uses is the	The science supporting the Sediment Quality Plan was separately developed and subject to its own peer review.

No.	Author	Comment	Response
		median value of the four indices. If anything, use of the median value will decrease information on extreme conditions that the individual indices may provide. I do not think that this approach yields results that are helpful in deciding whether the sediment benthos is impaired or not.	
2.37	pdf p. 7	Page 48, Last paragraph: Proper names of organisms should be italicized. The second last sentence is not clear and needs further elaboration.	The Staff Report has been corrected.
2.38	pdf p. 7	Page 49, Last paragraph: At best this paragraph is unclear, but it seems to me to represent circular reasoning.	The paragraph has been edited for clarity.
2.39	pdf p. 7	Page 57, Second paragraph under Section B: If the analytical methods were not sufficiently sensitive to detect the pesticides and PCBs, how can the authors know that the discharge is a minimal source of these contaminants to Dominguez Channel and the harbor waters?	The paragraph has been edited. Additional studies (performed by other groups) have detected measurable amounts of pollutants within this watershed. (SCCWRP, 2003)
2.40	pdf p. 7	Page 58, Third last paragraph (and many other places in the report): The report is sloppy with regard to citing references. Including the date of the Stenstrom et al. report in the text would tell the reader that the authors are citing a reference that can be found in the bibliography or reference section.	Additional citations have been included in the revised draft Staff Report.
2.41	pdf p. 7	Page 58, Third paragraph: the mean values given for copper, lead and zinc are very high (> 1 mg/L), and I wonder whether these are correct.	The mean values presented are correct.
2.42	pdf p. 7	Page 62, Table 4-3: It is difficult to evaluate the significance of the numbers in this table. Reporting the results as areal based loads (g m-2 yr-1) would be more useful.	This air deposition information was compiled by dischargers and they reported in units of g/yr.
2.43	pdf p. 7	Page 66, Table 4-6: Same comment applies.	This air deposition information was compiled by dischargers and they reported in units of g/yr.
2.44	pdf p. 7	Page 68, Third last paragraph: By this point in the analysis, the authors should not have to resort to weak statements like "…atmospheric	The statement has been clarified.

No.	Author	Comment	Response
		deposition may be a potential nonpoint source of metals, DDT and PAHs to the watershed….” Is it or isn't it? Data sources were cited earlier that should have allowed a more conclusive statement than this.	
2.45	pdf p. 7	Page 69, Section 5.1: The terms LSPC, LAR, and SGR were not defined previously and are not in the list of acronyms. Authors should define these terms and describe how the models work.	The terms have been defined in the Staff Report.
2.46	pdf p. 7	Page 73, Mention of the three appendices much earlier in the section would have been helpful to the reader in understanding where to look for more information about the modeling approach.	The Staff Report has been modified to address this comment.
2.47	pdf p. 8	Page 77, Figure 5-2: It is impossible to distinguish the modeled results from the actual data in the black and white printed version of the report sent to me to review. The reader must accept on faith that the figure actually shows both.	Staff apologizes for not including a color copy in the reviewer's printed copy. The copy on the Regional Board's website includes color figures.
2.48	pdf p. 8	Page 77, The second last "sentence" actually is not a sentence and does not express a complete thought.	The Staff Report has been corrected.
2.49	pdf p. 8	Page 78, Figure 5-3: One cannot distinguish which data point and line represent the bottom water and which represent the surface.	Staff apologizes for not including a color copy in the reviewer's printed copy. The copy on the Regional Board's website includes color figures.
2.50	pdf p. 8	Page 78, First paragraph under Figure 5-3 states "As can be seen from the comparisons indicated in the above figures, the hydrodynamic model provides a good foundation …." This is not really the case. Without presenting any statistics, the authors cannot make such a conclusive statement.	As indicated in Appendix I (page 23), due to the extreme scatter of the data, lumped error statistics are not particularly meaningful. Therefore, time series plots of the modeled and observed salinity values were presented for twenty stations in Appendix A. These plots illustrate that the model does represent the general response to high freshwater inflow events. A reference to these comparisons has been added to Section 5.2.1 of the TMDL report.
2.51	pdf p. 8	Page 78, Section 5.2.2, first paragraph: The first sentence is not clear.	Due to data limitations, model validation using an

No.	Author	Comment	Response
		What is meant by "only a calibration effort"?	independent set of data could not be performed in addition to calibration. This sentence in Section 5.2.2 has been clarified.
2.52	pdf p. 8	Page 80, Second paragraph: No data are presented here or cited to support this statement.	Section 5.3 is a summary section and the support for the statement is in the linkage analysis section as a whole.
2.53	pdf p. 8	Page 82, Paragraph below Eq. 1: Sample concentration is expressed as a percentage, but it is not clear or obvious what this means (percentage of "what"?).	The percentage is the dilution of the water being tested. See, also, response to **Comment 1.79.**
2.54	pdf p. 8	Page 82, Table 6.1: are not the various loads additive? If so, shouldn't the final TUc values be allocated fractionally among the permittees?	The loads are not additive. See response to **Comment 2.14.**
2.55	pdf p. 8	Page 84, Table 6-2: It is not at all clear to this reviewer how the numbers in this table were obtained.	See response to **Comment 2.14.**
2.56	pdf p. 8	Page 85, Paragraphs 6.2.2.1 and 6.2.2.2: The term "MOS" in the equations is not defined.	The term has been defined for those equations in the Staff Report.
2.57	pdf p. 8	Page 86, Table 6-4: Given the large uncertainties in the data, modeling and analyses leading to the allocations listed in this table, the margin of safety (10%) seems unrealistically small. I note that MOS finally is defined, after the fact, in Table 6-4.	See response to **Comment 1.43** and **1.45.**
2.58	pdf p. 8	Page 87, Section 6.3.2: The wet-weather allocations given here seem reasonable given the lack of data, but one wonders why there are no data.	Previous permits and sampling programs have not provided sufficient data for site specific calculations of allocations for discharges to Torrance Lateral.
2.59	pdf p. 8	Page 89, Table 6-8: It is not clear what the numbers in the table mean. Based on the text at the bottom of page 88, I assume that they are 95 percentile values of historical data, but the text is not clear regarding how they "translate" to either a TMDL value or a WLA.	These data are interim allocations, set at a level of current conditions. Interim allocations are especially necessary due to the long implementation schedule of this TMDL, before final allocations must be met.
2.60	**pdf p. 8**	Page 91, Third paragraph from bottom: The paragraph, particularly the	The TIWRP discharges tertiary-treated effluent to the Outer

No.	Author	Comment	Response
		last sentence, strikes me as a bit of “hand-waving.”	Harbor and this POTW is in the process of eliminating their discharge into surface waters.
2.61	pdf p. 8	Page 92, Table 6-10: The document is not clear on what TMDL values mean for bed sediments.	The Staff Report and Basin Plan Amendment have been edited to add additional clarity on allocations for bed sediments.
2.62	pdf p. 8	Page 94, Note under Table 6-10: it is not obvious why no reductions in atmospheric deposition of Cu, Zn and PAHs should be anticipated. If atmospheric sources are contributing to the problem, they should be subject to regulation just as much as land-based point and non-point sources.	Atmospheric sources may be reduced in the future as air quality improves in the harbors areas but staff note that atmospheric sources are not regulated by the Regional Board but by other regulatory agencies.
2.63	pdf p. 8	Page 98, First sentence in section 6.5.3: It is not obvious why an implicit margin of safety exists in the final allocations to Dominguez Channel estuary and the greater Harbor waters just because multiple numeric targets were selected. They all could be “unprotective.”	All the targets, those based on CTR, sediment guidelines, sediment objectives, and fish tissue are designed to be protective of beneficial uses individually. Each of these sources has been based on science and peer reviewed on their own. It is unlikely that any of the individual targets are insufficiently protective.
2.64	pdf p. 8	Page 98, Table 6-12: One wonders whether the tiny values listed in the table for DDT and PCB WLAs are meaningful. Could one actually measure a WLA of 0.35 g/yr? In general, the numbers in the table seem unreasonably low.	The WLA are low as they are based on the low targets necessary to protect the beneficial uses.
2.65	pdf p. 8	Page 101, In many cases explanations in the section on TMDLs are given considerably after the results are presented, making it very difficult for readers to understand what was done and what the basis for the TMDL really was. Overall, this section of the report was difficult to follow and understand.	Several edits for clarity have been included in this section.
2.66	pdf p. 8	Page 106, Third paragraph: The sentence that forms this paragraph is garbled and difficult to understand.	The paragraph has been edited for clarity.

No.	Author	Comment	Response
2.67	pdf p. 8	Page 124, Overall, the cost analysis is very superficial and inadequate.	The cost analysis meets the requirements of a TMDL and a Basin Plan Amendment. Staff notes that cost analysis is not a scientific or technical matter requiring peer review.
2.68	pdf p. 8	Page 124, Second paragraph: It does not seem appropriate to simply average the two widely disparate estimates of dredging costs.	See response to **Comment 2.67.**
2.69	pdf p. 8	Page 126, The cost analyses for sand/organic filters and vegetated swales also are superficial and inadequate.	See response to **Comment 2.67.**
2.70	pdf p. 8	Page 128, Table 7-7: Even by today's standards, these are huge cost estimates. Although it is readily apparent that a large effort was expended in developing the TMDL document and associated appendices, the large uncertainties associated with the modeling analyses lead me to be very skeptical that the work provides a sufficient scientific basis for the expenditure of such large amounts of money.	See response to **Comment 2.67.**
2.71	pdf p. 9	**Appendix I** Page 14, 15, Figures 5 and 6: One cannot distinguish the "observed" and "predicted" lines in the black and white versions of these figures in the printed document. Authors of reports need to avoid using color for lines unless they are certain that the report will be printed in color.	Staff apologizes for not including a color copy in the reviewer's printed copy. The copy on the Regional Board's website includes color figures.
2.72	pdf p. 9	Page 20-22, Figures 8-11: The same comment applies to these figures.	See response to **Comment 2.71**.
2.73	pdf p. 9	Page 26-27,Figures 14 and 15: The two different sets of data in both figures cannot be distinguished by the symbols used in the figures, nor is it obvious which line refers to the "bottom fit" and the "surface fit."	See response to **Comment 2.71**.
2.74	pdf p. 9	Page 28, Last paragraph: Although there may be an entirely reasonable explanation for not including physical bed data from inside the breakwater for years prior to 1997, no explanation is provided, leading me to be concerned about whether this was an arbitrary decision.	Physical bed data inside the breakwater collected prior to 1997 were excluded from the initial conditions to ensure that data associated with areas that had subsequently been dredged were not included. Given that these data were somewhat limited and several studies with useful physical

No.	Author	Comment	Response
			data were available prior to 2000, the time period for physical data extended further back than those associated with the contaminant concentrations (individual stations that were known to have been dredged were excluded). The discussion about these data in Section 7.1 has been expanded to explain the use of data after 1997.
2.75	pdf p. 9	Page 29, Second paragraph: Similarly, no explanation is included for eliminating sediment metals data from inside the breakwater prior to 2000, leading to concerns about arbitrariness. In addition, the text is not clear on how initial concentrations of metals and organic contaminants in the sediments (displayed in the maps in Figures 23-28) actually were estimated.	Contaminant concentration data inside the breakwater collected prior to 2000 were excluded from the initial conditions to ensure that data associated with areas that had subsequently been dredged were not included. The Ports of Los Angeles and Long Beach provided maps to help identify dredged areas and associated dredging dates. The discussion about these data in Section 7.1 has been expanded to explain the exclusion of data prior to 2000. The distribution of initial concentrations in Figures 23-28 were based on the observed data. Specifically, observed concentrations were used at their specific location and then the concentrations in between the individual data points were estimated by interpolating between the known concentrations. The text in this section has been clarified to describe the assignment of initial bed concentrations.
2.76	pdf p. 9	Page 32, Figure 18: No r^2 values are given in the plots to demonstrate the level of precision of the predictive equations, nor is it clear whether the outlier value in the upper figure was included in the regression analysis.	All data were included in the regression analysis. The 2003 data had an $R^2 = 0.76$, while the 2006 data had an $R^2 = 0.50$. These values have been added to the report.
2.77	pdf p. 9	Page 47, Figure 31: The data shown in the three plots of Figure 31 are all "over the map," leading to two conclusions: (i) there is no predictive relationship between the partition coefficient (K_p) for heavy metals and total solids concentrations, and (ii) use of a mean value of K_p for modeling purposes would result in large uncertainties in predicted results because of the large range in K_p values.	See response to **Comment 1.31**. The data show a very weak decrease of K_p with increasing solids, consistent with previous findings. Additional information such as AVS might provide more insight however AVS measurements are highly specialized and were not included in the 2006 sampling event (the modeling did not guide the field program).

No.	Author	Comment	Response
2.78	pdf p. 9	Page 50-51, Figures 34-36: The same comment applies to K_p values for organic contaminants.	See response to **Comment 2.77**. K_p for organics show somewhat less scatter in relationship to TOC, but no detectable relationship.
2.79	pdf p. 9	Page 64-65, Figures 41 and 42: There is virtually no relationship in the scatter plots for copper in the figures. All that one can conclude is that the predicted numbers are in the same order of magnitude as the observed values. I suspect that the latter fact reflects "tweaking" associated with the calibration effort. I conclude from the figures that the model cannot be used to predict effects of changes in external loading on sediment concentrations with any degree of accuracy or reliability and that it would be even worse in predictions of the effects of other environmental/management variables on sediment levels of copper.	As indicated in Appendix I, these plots do show extensive scatter, but the model predicted levels are within the range of observations. The predicted and the observed values had considerable variability, generally in the same range. The simulated values used for TMDL or existing loading rate calculations were annual averages. Given that the model is in the range of observed values and averages are likely similar, the model is being appropriately used to determine loading estimates. In most modeling studies at major contaminated sediment sites, Figures like 41-46 use log scales and the rule of thumb is that a factor of 2 agreement between observed and predicted is acceptable. The study used best available parameters for sediment erosion/deposition and contaminant partitioning as well as the best available information for watershed loading.
2.80	pdf p. 9	Page 66-67, Figures 43 and 44: The results for lead and zinc are even worse in that the predicted sediment concentrations exhibit a much large range than the observed values for two of the three lead plots and all three zinc plots. As such the results suggest that the model may produce differences or trends in concentrations of Pb and Zn in runs where environmental or management-related parameters are varied even though such differences or trends may not occur in reality.	When compared to the copper results, the modeled ranges for lead and zinc do exhibit more variability than their respective observed ranges. Copper was ultimately found to be the most sensitive metal and implementation practices for all three metals will likely be similar; therefore, achieving the copper reductions will likely result in achieving the targets for lead and zinc. Adequate observational data were not available to calibrate the sediment transport model and this is reflected and amplified in the contaminant transport and fate predictions. Likewise observations were taken during the dry season rather than during high flow events which provide a stronger signal for calibration. In addition, see response to

No.	Author	Comment	Response
			Comment 2.79.
2.81	pdf p. 9	Page 68, Figure 46: The same comment as above applies to the PAH plot.	See response to **Comment 2.79** and **Comment 2.80**. Limited data associated with PAH initial concentrations and PAH watershed loading makes additional calibration of these values difficult. Only average values from the model are ultimately used in the TMDL calculations and the average observed and predicted values are likely similar, justifying the use of the model in the TMDL calculations.
2.82	pdf p. 9	Page 69, Paragraph 2: This is an honest appraisal of the adequacy of the data for modeling purposes, but I am not convinced that the statement in the third paragraph "…it has been demonstrated to respond appropriately to load reductions and is therefore considered useful for load reduction scenarios…" is true or accurate. I certainly would not be surprised if the model produced simulations in the right direction—i.e., a reduction in load produces a reduction in concentrations; the model would have to be seriously flawed not to do that, and I do not think that the model itself is that flawed. Nonetheless, one cannot conclude that the model is adequate for the proposed purposes just because it gets the direction of change correct. The results presented in preceding pages do not lead me to think that it can do more than that.	The model results used for TMDL or existing loading rate calculations were generalized as annual averages. Given the averaging of results and that the model is responding appropriately to load reductions, the model is determined to be sufficient to perform general load reduction scenarios to determine the relative differences in loadings (i.e., estimated percent reductions) rather than absolute loading rates. As previously noted, the EFDC model could be greatly improved with the collection of additional field data – both for receiving water configuration and calibration as well as data to better characterize watershed loading. The modeling effort made use of the best available data at the time the modeling was conducted.
2.83	pdf p. 10	**Appendix II** Page 1, Figure 1: The map does not "work" in gray scale. The watersheds simply cannot be distinguished in the b/w printed version of the report.	Staff apologizes for not including a color copy in the reviewer's printed copy. The copy on the Regional Board's website includes color figures.
2.84	pdf p. 10	Page 15, Last paragraph: The authors are disingenuous in stating that "the predicted flow for the Forest subwatershed has a similar pattern, but slightly (italics added) higher peaks than the observed flow at the POLA/POLB stormwater sampling station." The second simulated peak is twice as high as the observed peak; I do not consider that to be a "slight" or "small" difference, and I don't consider that to be "well within acceptable modeling ranges."	See response to **Comment 1.95** and **Comment 1.96**.

No.	Author	Comment	Response
2.85	pdf p. 10	Page 16, Figure 5: The comparison of modeled and measured flows in this figure also is not impressive. The modeled results completely miss the two-peak nature of the observations.	See response to **Comment 1.95** and **Comment 1.96**.
2.86	pdf p. 10	Page 17, Figure 6: Modeled versus observed peak flow for the subwatershed in the figure differ by a factor of five. At least, the text (bottom of p. 16) acknowledges the lack of fit, but the results certainly to not provide validation for the model.	See response to **Comment 1.95** and **Comment 1.96**.
2.87	pdf p. 10	Page 17, Statement in the first sentence: "Once the model was calibrated and validated…." This statement makes it seem that everything worked, but as the previous comments indicate, the model really was not validated. I don't think one can say that a model was validated simply because one ran a validation exercise. If the simulation didn't fit the observed data in the validation exercise, one cannot conclude that the model was validated.	As this comment refers to both the regional modeling approach and the calibration/validation, see response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.100**.
2.88	pdf p. 10	Page 19-20, Figures 7 and 8: The modeled trends in TSS and measured data are not even close in Figures 7 and 8, and I do not consider these results to be "well within acceptable modeling ranges" as the report states at the end of the first paragraph. The same comment applies to the "validation" in Figure 9 (p. 21).	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent.
2.89	pdf p. 10	Page 21, First paragraph: Plots like those in Figures A-2 to A-15 in the appendix are almost useless in evaluating the validity of the model. The range of the TSS data is so large that it would be amazing if the model didn't predict TSS concentrations "generally within the range of the observed data."	See response to **Comment 1.95** and **Comment 1.96**. These responses focus on the simulation of flow, but are also applicable for the TSS results since the modeling approach was consistent. In addition, achieving the range of observed data is useful to show as the model results were ultimately averaged for use in TMDL calculations and the average predicted values are similar to the average observed values.
2.90	pdf p. 10	Page 24, Figure 10: Overall, the modeled versus observed concentrations of copper, lead, and zinc in Figure 10 do not represent	See response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.106**. These responses focus on the

No.	Author	Comment	Response
		acceptable fits of the data. The modeled results largely under-predict the initial high concentrations and the double peaks of the modeled results are not to be found in the observed data.	simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent.
2.91	pdf p. 10	Page 25, Figure 11: Similarly, the second modeled peak is not found in the observed data in Figure 11, but the first peak roughly captures the initial observed data.	See response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.106**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent.
2.92	pdf p. 10	Page 26, Table 6: Are the EMCs flow-weighted or simple averages?	The EMCs are flow-weighted. See response to **Comment 1.116**.
2.93	pdf p. 10	Page 27, Figure 12: The comparisons of modeled and measured EMCs are actually quite good for two of the three sites shown in Figure 12 (and awful for the third site), but it is difficult to understand how the EMCs can be as close as shown given the poor match of modeled and measured results in the preceding Figures 10 and 11, from which the bars for the Forest Industries site in Figure 12 were based.	Since the EMCs are flow-weighted, the over-prediction of flow and under-prediction of metals balance out with similar observed and predicted EMC values for the storms in the Forest and Pier A subwatersheds.
2.94	pdf p. 10	Page 29, Figure 13: Same comment applies here as for Figure 10 (p. 24).	See response to **Comment 1.24**, **Comment 1.95**, **Comment 1.96**, and **Comment 1.106**. These responses focus on the simulation of flow and TSS, but are also applicable for the metals results since the modeling approach was consistent.
2.95	pdf p. 10	Page 31-32: Given that the authors showed previously that they were not able to simulate flows for the Maritime Museum subwatershed, one wonders why they even bothered to model the metal concentrations and loads. Clearly, they were unsuccessful in doing those as well.	Results were shown for comparison with all available observed data.
2.96	pdf p. 10	Page 52, Figure 27: The one data point at the right side of Figure 27 is the "tail wagging the dog." That is, this one datum is driving the regression and is largely responsible for the high r^2. The distribution of the data does not fit one of the basic assumptions of regression analysis—that data are distributed roughly equally across the range of the independent variable.	The values used in the regression analysis are averages representing a single value for each watershed studied. The data point on the right side represents LA River watershed, which has a large urban area. When all individual samples were included in the analyses (which is not accurate since some of the watersheds were sampled more than others, so

No.	Author	Comment	Response
			their flows would carry more weight in the analysis), the r^2 value was still high (r^2=0.84). In addition, please see response to **Comment 1.130**.
2.97	pdf p. 10	Page 53, Tables 13 and 14: From the magnitude of the standard deviations relative to the means in Table 13, it is clear that the data are highly skewed and not normally distributed. Mean values are not appropriate in such cases. The authors should have log-transformed the data, which likely would have yielded at least close to a normal distribution. As a result, the values calculated for the "low range" and "high range" in Table 14 are not correct.	See response to **Comment 1.131**.
2.98	pdf p. 11	Page 54, No basis is presented for the statement "Trace metals were bound to a particle during wet-weather wash off until they dissociated upon reaching the receiving water body." This may or may not be true, depending on dissolved metal concentrations in the receiving water body, the kinetics of desorption, and the mode by which the metals are bound to particles. Not all metals are bound by reversible (ion-exchange-like) processes.	To maintain consistency with the regional modeling approach (**Comment 1.24**), metals were assumed to be associated with particles. Additional detail has been added to this assumption in Section 5 to describe the potential uncertainty identified by the reviewer.