THE BRAKE PAD PARTNERSHIP
Compilation of Technical Reviewers' and Stakeholders' Comments on
Watershed Modeling Draft Report
July 22, 2007

NOTE: Responses to comments are in Arial font.

The major focus of many of the reviewer's comments was on the lack of adequate calibration. All the reviewer's need to be aware that funding limitations precluded planned calibration efforts, and in fact the calibration budget was eliminated as part of the negotiation of the budget. That is why consistency checks were included as an alternative to 'no calibration', and we were fortunate that Jim Carleton of EPA volunteered to perform some minimal calibration runs on the Castro Valley Creek for copper simulations, to supplement our consistency checks.

Recommendations will be included to address many of the shortcomings of the current effort, including additional calibration efforts for sites where data is available, along with further investigation of reservoirs, improved spatial definition, error analyses, and both sensitivity and uncertainty analyses.

Background

On behalf of the Brake Pad Partnership, AQUA TERRA in conjunction with the US EPA has conducted watershed modeling of the environmental fate and transport of copper from vehicle brake pad wear debris and other copper sources in order to better understand how copper travels through the environment and what the relative significance is of copper from brake pad wear debris on copper levels in the San Francisco Bay. The watershed modeling effort is one of three interlinked modeling components at the core of the BPP's effort. An air deposition model was used to estimate the amount of copper from brake wear debris and other air releases of copper that is deposited in the watershed. The results of that model provided some of the inputs to the watershed model. The watershed model, a draft of which you are being asked to review, estimated the relative amount of copper from brake wear debris that is discharged from the watershed in runoff to the San Francisco Bay. That information will provide inputs to the bay model, which will estimate short- and long-term concentrations of copper in the South San Francisco Bay.

The work plan for the watershed modeling effort was reviewed and approved and is available at http://www.suscon.org/brakepad/documentArchive.asp. Final results for the air deposition modeling and for the source release inventory effort can be found at http://www.suscon.org/brakepad/documents.asp.

The Brake Pad Partnership Steering Committee is seeking an independent expert review of watershed modeling results for the environmental fate and transport of copper to ensure that the approach and results of this element of the Partnership's work are technically sound, to determine if there are feasible opportunities to strengthen the presentation of the results, and to help build in-depth understanding of and confidence in the technical studies on the part of the Steering Committee and the stakeholder communities.

Charge to Reviewers

With the aim of meeting these objectives, the Steering Committee is seeking comments that specifically address the following questions:

1. In your assessment, do the modeled results adequately estimate the relative amount of copper from vehicle brake pad wear debris that enters the San Francisco Bay in runoff from the watershed? What improvements, if any, do you recommend for the presentation of results?
2. The original intent of the technical studies was to develop information on the relative impacts of copper from brake pad wear debris in the bay. In your assessment, what factors would need to be taken into account when using the results presented in this report for assessing the relative contribution of brake pad wear debris copper in the creeks?
3. In your assessment, do the modeled results adequately present the level of uncertainty involved in the amount of copper from vehicle brake pad wear debris that enters the bay in runoff from the watershed? Do the modeled results adequately represent the uncertainty in the relative contributions of copper in runoff from brake pad wear debris and from non-brake pad copper sources?
4. During your review, did you identify anything that could render the modeled results inappropriate for subsequent use in the Bay model as described above? Did you identify any limitations on the use of these results in the Bay model?
5. In your assessment, what factors would be important when extrapolating the modeled results to other geographic areas?

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COMMENTS RECEIVED

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From Jim Pendergast, US EPA (July 17, 2007)

P1, second paragraph: This text seems better placed in section 1.2. It doesn't provide background but rather describes what model was used, which seems to be better placed in the discussion of the work.

We suggest moving back within Section 1.1 so that it is briefly summarized prior to the 'Summary of Watershed Model Results' in Section 1.2.

P2, Table: Suggest deleting the Total Loads column because it is confusing without adding discussion text. We know that we matched the high brakes with low non- BP contribution, etc., which resulted in no real change in total loading. But without saying this, readers will wonder why the total loads did not change. Since the table is designed to show that the BP contribution ranges from 10% to 35% depending on one's assumption on the variability in sources, that is all the table should show here.

We suggest leaving the totals in the table and expanding the discussion along the lines of Section 3.3 but with additional discussion

P18, Last paragraph: Did we decide not to pay for a BP high – non-BP high and BP low – non-BP low scenario? How much would it cost to run these scenarios? Given the insensitivity of the total watershed load to the bay to the three scenarios run, I'll guess that reviewers may want to see the high-high and low-low scenarios to check the sensitivity on the total load calculation.

This is addressed in the discussion of the three scenarios, below

P26, L2: Suggest not saying "fall back", but rather say "secondary".

Either 'secondary', or 'alternate' approach might be appropriate.

P26, Table: Need to fix the word wrapping.

This was caused by the Adobe pdf conversion; it is not present in the original Word file. We will check the final pdf for these types of conversion errors.

P26, Last sentence: Ought to give the reason why the Alameda Creek simulation was run.

The Alameda Creek simulation was run to both assess the reliability of assigning parameters without calibration, and assessing the impact of ignoring contributions from the reservoirs in the Upper Alameda Creek watershed. This will be added to the text discussion

P26, Last paragraph: Fix formatting to left justify.

Adobe pdf conversion error

Figures 3.2 and 3.3: Very good simulation of higher and middle flow ranges, but why the departure between observed and predicted at the lower flow ranges of Napa River near Napa and Alameda Creek near Niles? The Napa River is more troubling because the departure is significant for the lower half of the flows. Suggest additional text in the report describing this.

Additional comprehensive calibration might have been able to improve the agreement, but the focus of the consistency checks was on the high to mid-range flows important for stormwater runoff. Differences in the low end of the flow-duration curve will not have a significant impact on sediment and copper loads to the Bay. Additional discussion describing this will be added to the report.

P30, L3: Don't need to emphasize NO. Lower case will do.

Will correct

P31, Third paragraph, Last sentence: What reason do we give to consider these 6 points as outliers? An alternate hypothesis is that the model is missing a significant loading, because the observations are 4 to 5 times higher than the predicted values. I do suggest more text on this topic.

Jim Carleton has done additional plots (attached) demonstrating that those 6 points correspond to lowflow conditions and can reasonably be considered outliers.

P31, Last paragraph, L3: Suggest replacing “NOT calibrated” with “not generated by further calibrating the model”

Will do.

P36, Third paragraph: Is there a reason why the high end of BP loads is offset by the low end of non-BP loads? Since the loads were developed independently, this is only a coincidence, right? We ought to say this so that a reader doesn’t wonder if the non-BP loads were generated by subtracting the BP loads from a total watershed load. Remember that not all readers will read all the reports.

The scenarios were chosen with the intent that they explore the range of the relative contribution of copper from brake pads in runoff to the bay. The release estimates don't actually compensate for each other in the different scenarios. It's coincidental that the three copper loadings of Table 1.1 are so similar for the three different cases. There are sub-watersheds where the loads in runoff across the three release scenarios are not as close.

Total anthropogenic loads are actually much higher in the brakes-low case, where brake pad releases are reduced by a standard uncertainty and non-brake releases are increased by a standard uncertainty. It's only after these releases go through the modeling process that they offset each other, and that's partly because of the calibration that was done in Castro Valley but it's also somewhat expected because the non-brake releases are to pervious surfaces where they are much less likely to be entrained in runoff.

The word "relative" will be inserted between "possible range of" and "copper contributions" in the 2nd paragraph on p. 36. The third paragraph (the one that begins with "The concept is that…" is removed. The following is added to bullet #2, the brakes-high bullet, in section 3: "This provides an estimate of the upper bound of the relative contribution from brake pads." The following is added to bullet #3, the brakes-low bullet, in section 3: "This provides an estimate of the lower bound of the relative contribution from brake pads."

Some changes will be made to section 2.5 to make it more obvious which of the sources of copper are due to brake pads and which are not, and what the rationale behind the source loading scenarios is:

Column headings in Tables 2.5 to 2.7 will be as follows:

Sub-Watershed	Release to Roadway from Brake Wear (adjusted for street sweeping)	From Non-Brake Sources			
		Rain-Independent Releases to Storm Drains and Surface Waters	Rain-Dependent Releases to Storm Drains and Surface Waters	Releases to Agricultural Land	Releases to Developed Land

The second paragraph of section 2.5 will be replaced with the following three paragraphs:

Figure 2.6 provides a schematic of how the various copper flux estimates are represented in the HSPF watershed model for the Bay Area watersheds. There is a great deal of uncertainty in both the non-brake and brake release estimates, and taking that uncertainty into account when determining whether the contribution from brake pads is substantial was necessary. Thus, three cases of copper flux scenarios were modeled, one called brakes-high, one called brakes-low, and one called mid-brakes. These three scenarios were selected because results based on them adequately represent the range of relative contribution of copper released from brakes, and because they take the uncertainty in both brake and non-brake releases into account. One scenario is based on the point value presented in the copper release inventories for both brake sources and non-brake sources; this scenario is called the mid-brakes case. A second scenario, called the brakes-low case, explores the source term estimates from the perspective that the point values in the release inventory overestimate brake contributions relative to non-brake sources. The third scenario, called the brakes-high case, explores the source terms from the perspective that the point values in the release inventory underestimate brake contributions relative to non-brake sources of copper.

Tables 2.5 through 2.7 show the flux estimates provided by Rosselot for three scenarios – Midpoint Brakes, High Brakes, Low Brakes -- for both BPW roadway releases and non-BPW releases to various land uses included in the model. Rain-independent releases to storm drains and surface waters include copper in domestic water discharged to storm drains, copper released from pool, spa, and fountain algaecides, and copper used in algaecides used in non-agricultural rights of way, recreation areas, and for public health. Rain-dependent releases to storm drains and surface waters includes architectural releases of copper and copper in industrial runoff. Releases to agricultural land include copper in algaecides applied to agricultural water areas as well as copper in fertilizers and pesticides applied to agricultural land. Releases to developed land include copper in pesticides applied to urban land and copper in non-farm fertilizers as well as copper from pressure-treated wood used in residential and commercial construction.

Table 2.8 shows the wet and dry deposition fluxes supplied by Pun (2007) for each of the watersheds. Releases of copper from brake pads are responsible for the vast majority of these deposition estimates. The values in these tables provided the basis for quantifying the various copper contributions shown in Figure 2.6.

P37, Table: Check the total row. The sums for "Watershed Load" and "Load to Bay" should differ because not all watersheds modeled drain to the Bay.

The totals are correct. The watershed loads and loads to the Bay are the same for all watersheds except for North Napa and North Sonoma. These two watersheds drain to the Napa and Sonoma watersheds, respectively, so their watershed contributions are included in the total load to the Bay from the Napa and Sonoma watersheds. In the other words, the loads to the Bay from Napa and Sonoma include the North Napa and North Sonoma watershed loads. That's why both column totals are the same.

P40, Figure 3.9: Check the word wrap on the figure title.

Adobe pdf conversion error.

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From Bob Peters, Akebono-USA (July 18, 2007)

Page 3, Paragraph 3. I'm not familiar with the report cited but it seems to me that those estimates would be based on roughly the same thinking and same modelling that this report is based on. Additionally, it had major involvement from the same individual, so it's no real surprise that the estimates are similar. I don't think this adds much to the credibility of the model.

The estimates of 44% to 53% for lower San Francisco Bay 'cited' (referenced) in the Carleton and Cocca paper are from a 1994 Woodward-Clyde report; they were not developed from the work by Carleton and Cocca.

Page 6, Section 2.1 - when the number of paths from a watershed to a bay have been reduced from many to 1 for the purposes of modeling - how is the background copper concentration (I think in the sediment) of the many small creeks comprehended, or maybe more appropriately, compensated for?

The current modeling effort does not account for sediment and copper deposited and/or scoured from the many streams, creeks, storm drains that lead to the Bay. The instream modeling was setup to maintain relatively stable channels and deliver essentially all the sediment and copper from the watershed to the Bay. To account for the highly spatially variable process of scour and deposition in each of the connecting streams would have required a much larger effort and more data. This is one of the shortcomings of the current model and is one of the recommendations for future work and refinement.

Page 9, Table 2.1 - This isn't the only example - but it illustrates my point. Rain gage measurements are reported to 0.001". I doubt any rain gage is that accurate. I think significant digits need to be considered throughout the report.

You are correct, rain is usually measured down to 0.01 inches. We will review our tables and make an effort to show only significant digits.

Page 17, Paragraph 2. Minor point - just wondering if the assumption about irrigation efficiency is a good one. I'm pretty sure my lawn watering isn't that efficient.

The WUCOLS manual suggests an efficiency of 0.85 for a well-designed urban irrigation system (mostly microjet/drip nozzles), and we have used a value of 0.75 for agricultural systems based literature for Ventura County, so we selected a midpoint of 0.80 since we combined irrigation applied to urban and agriculture. That is we developed a single irrigation timeseries for each of 6 different regions covering the study area, and that irrigation was applied to both developed and agricultural land. Lower values of the efficiency would result in more irrigation water being applied but this is mostly during the summer months (i.e. largest applications amounts) and would not have a major impact on the runoff volumes.

Page 18, Section c. "BPW" is used as an acronym for <u>B</u>rake <u>P</u>ad <u>W</u>ear - I think it would be more accurate to call it BPWD - since it really is the debris we're concerned with.

Will do.

Page 26, Table 3.1. Text and table are interfering with each other.

Adobe pdf conversion error.

Page 32, Figure 3.4. I assume the line is a correlation line and my read is that correlation is pretty poor based on that graph. What is the correlation factor r^2? How can this be considered "good" correlation?

The correlation is discussed on page 31, 3rd para. The slope of the line is 0.99 and the r^2 is 0.66 when the 6 highest concentrations are treated as outliers. See attachment.

Page 32, Figure 3.4 The plots (not just on this page) are very difficult to decrypt. For the benefit of the folks that aren't used to viewing the plots - I suggest the legends and axes are labeled in English, avoiding technical abbreviation wherever possible.

This was a rough draft. We will insert proper labels and do our best to improve the graphics in the final.

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From Wayne C. Huber, Oregon State University (July 18, 2007)

Donigian, A.S., Jr. and B.R. Bicknell, *Watershed Modeling of Copper Loads to San Francisco Bay*, Aqua Terra Consultants, Mountain View, CA, June 21, 2007.

I am pleased to provide this review of the report. Overall, it looks like a good job to me. I will make a few general and specific comments about the report and then address the specific questions to the reviewers. The majority of my general comments are really questions for clarification and do not reflect generally upon the good quality of the modeling.

<u>General Comments</u>

1. I like continuous simulation for the purpose of defining loads to the Bay. HSPF is an appropriate model, run by good modelers. The tools of this study are appropriate.

2. I agree with the "weight of the evidence" means for model credibility in the absence of more data and greater study resources. Once in a while I might quibble with what is "good agreement," but I generally accept the opinion of these qualified modelers.

3. Loads developed from HSPF are driven by the loadings supplied by others. HSPF associates copper mass with runoff events and develops hydrographs and pollutographs for Cu. But I expect that the loads are driven most strongly in the model by the deposition modeling of Pun (2006) and copper release estimates of Rosselot (2006a,b). (I have not reviewed those reports.) In other words, if the loading estimates that drive the model are on target then so will be the HSPF estimates. The fact that HSPF Cu loads are in the same ballpark as spreadsheet estimates (Davis et al., 2000) is good. The good agreement of HSPF TSS loads with estimates of McKee et al.(2003) is also good.

4. Having said this, I do not see a good discussion of sensitivity analysis in the report. What really drives the copper loads in the opinion of the authors?

The load sources are the primary driving forces for the watershed loads with the atmospheric deposition (wet and dry) significant, but relatively smaller than the direct BPWD roadway release and other non-BPWD releases. Attached is the Copper Fluxes diagram from the report with numbers quantifying the various flux terms.

5. Since load (mass or mass/time) = concentration x flow (volume or flow rate), the long-term hydrographs are also very important. The quality of the observed vs. predicted hydrographs is difficult to judge from Figures 3.8 – 3.11, but looks generally good. The flow-duration curve comparisons (Figures 3.2 and 3.3) look good to me, and a maximum volume error of 9.0% over a 24-year simulation (Table 3.2) is very good.

We can readily provide additional daily flow hydrographs and at smaller spatial scales to make them more readable (perhaps in an Appendix), but the reviewers need to be aware that our minimal calibration efforts on flow did not focus, nor try to reproduce, the daily flow hydrographs – we looked only at flow-duration curves and mean annual volumes during those pseudo-calibration efforts, and only checked the daily timeseries to make sure nothing 'unusual' was occurring. We need some guidance from the Steering Committee members as to whether we need to include daily hydrographs for each year for each of the 8 watershed, i.e. about 90 pages of graphs, or some reasonable subset.

6. I like the use of best available topographic, land use and other spatial data, e.g., National Hydrography Dataset (NHD), coupled with GIS. I would appreciate a few more references for datasets such as NHD with which I have not worked personally.

We will add web site links for each of these into the final report.

7. P. 8, re. volume-stage-discharge functions: Were actual rating curves available for the various streams that would serve the same function? And couldn't stream width be measured directly or estimated from photos or GIS coverage? For instance, some of the "streams" are rectangular concrete channels. Just asking.

Rating curves are usually available from the USGS for their stream gages and that is the normal procedure for site-specific watershed model applications in the range of a few hundred square miles, or more if resources are available to support the associated effort needed to obtain and apply the rating curves for use in the HSPF model. For this effort an expedited approach was needed as less than 100 labor hours were budgeted for the initial model segmentation and input development for all 22 watersheds for hydrology simulation. We feel the results shown support an assessment that the hydrology is adequately represented even with this expedited approach.

Concrete channels are not common for the major streams we modeled, with some possible exceptions for limited portions of the streams in the South and East Bay.

8. Regarding precipitation input (p. 8), the report indicates hourly values were used. Were 15-min data available and if so, why not use them?

No, the NCDC stations did not have 15 minute data available for a significant number of the 20 stations we used; we only had hourly data for about 8 stations, and had to use daily data disaggregated to hourly for the rest.

However, with large PERLND and IMPLND subareas, maybe HSPF does not respond much differently to hourly vs. 15-min rainfall. But in most urban modeling scenarios, the shorter time increment rainfall data, the better.

The large scale of these watersheds, ranging from 10 to over 600 square miles, and the long lengths of the stream channels, ranging from about 5 to over 30 miles, are more consistent with the use of available hourly rainfall data than attempting to generate 15-minute data from limited 15-minute observations.

9. I wonder if 15-min rainfall vs. hourly would also affect sediment washoff? My recollection is that sediment is generated in HSPF on the basis of rainfall energy. Would higher intensity, 15-min rainfall generate more sediment than lower intensity, hourly rainfall?

Yes, 15-minute rainfall data used with a 15-minute timestep in the model would likely produce somewhat higher runoff intensities and resulting sediment washoff. However, the lack of 15-minute rainfall data and the scale issues noted above still severely limit the practical application of 15-minute data throughout the study area of 3500 square miles.

10. I really don't think this matters much here, but Thiessen weighting of nearby gages loses temporal patterns as weighted gages are combined. Another option is to use the closest gage so that temporal resolution is retained.

Thiessen weighting was only used from 1 watershed, Upper Alameda Creek, and a significant portion of that watershed was non-contributing due to reservoirs. The Thiessen analysis was used to assign the appropriate rain gages to each watershed, and then develop an adjustment factor (MFACT in Table 2.1) to adjust the point rainfall for watershed-wide effective rainfall, based on the isohyetal pattern. We will add some additional explanation in the text to further clarify these procedures.

11. I like the use of the California Irrigation Management Information System (CIMIS) data for evaporation estimates. In the Pacific NW, we have something similar called AgriMet (U.S. Bureau of Reclamation). I am not clear how the CIMIS data were combined with pan evaporation data for modeling purposes.

We have longterm PET data at San Jose (actually monthly pan evaporation converted to PET with monthly pan coefficients) and CIMIS has tables of mean monthly and annual ETo (Reference ET) for each of its zones. San Jose is in Zone 8, and we assigned each of the 22 watersheds to one of the CIMIS zones shown in Figure 2.3. Then we develop ratios of the CIMIS annual values for each zone compared to Zone 8, and used these ratios to adjust the San Jose data for each of the 22 watersheds, i.e. we used constant multipliers to adjust the San Jose PET for each watershed

If there were a way to use the data directly in the model, I'd choose CIMIS ET estimates based on the Penman equation and good local meteorological data over pan data in San Jose.

HSPF has traditionally used PET data derived from either pan evaporation or formulars such as Penman or Hamon, or others. The CIMIS data is for ETo, which is Reference ET, and that differs from both PET and pan evaporation. In this study we stuck with the traditional approach and used PET derived from the pan data in San Jose.

12. Pp. 16-17, re. irrigation. Great idea to include this in the model. How is irrigation introduced into HSPF? As additional rainfall?

Irrigation options in HSPF allow the use to apply an irrigation timeseries in a variety of ways, as additional rainfall representing spray irrigation, (so it undergoes canopy interception processes), as under-canopy spray or drip irrigation (so it bypasses interception), or directly to the soil as flood or subsurface irrigation. In this effort, we applied it as spray irrigation.

If so, does this show up on impervious as well as pervious areas?

No, it is only applied to the pervious areas where vegetation is growing and an ET demand has been calculated.

Probably not, but I'm interested in how this works.

13. P. 18. paragraph c: Does the "impervious" land use category include both roadways and non-roadways?

I believe so, but need to check the URS study from which the values were derived

Is imperviousness "directly connected impervious area" (DCIA)?

Yes, see bottom of page 12.

It is not clear to me whether loadings differ for roadways and, say, rooftops, and whether such a difference is accounted for, or whether it matters at all in this modeling. I probably just don't understand exactly how the loadings, land uses, and model land covers are coupled. The model is likely calibrated to whatever assumptions were made in the loadings. But I'd assume that roadway surfaces need to be separated from other imperviousness if brake pads themselves impact mainly roadways?

I believe that rooftops were NOT included in the DCIA percentage but URS may need to respond to this question.

14. Page 30, paragraph b: Another reason for deviations in flow-duration curves at low flows might be baseflow resulting from irrigation and the many different kinds of urban discharges that occur during dry weather.

Agree

15. Is there a reason for dropping the six high observed Cu concentrations in Figure 3.4? Agreement between simulated and observed concentrations is not so good when they are retained.

See response to Jim Pendergast's question on this, and the attached plots of outliers by Jim Carleton.

16. Finally, what is the rationale for the three scenarios (Table 1.1), in which non-BP loads are increased/reduced as BP loads are reduced/increased? Yes, this results in three copper loadings of similar magnitude (56,000 kg/yr), but I don't understand why the scenarios assume compensating loads.

The release estimates don't actually compensate for each other in the different scenarios, it's coincidental that the three copper loadings of Table 1.1 are so similar for the three different cases. There are sub-watersheds where the loads in runoff across the three release scenarios are not as close. Responses to Jim Pendergast's comments might address this comment.

There may be some confusion about whether this table represents the copper load in runoff to the bay or copper releases to the watershed (these releases are often referred to as "loads" or "environmental loads"). The title of Table 1.1 will be changed to "Table 1.1 Summary of Mean Annual Copper Loads in Runoff to SF Bay for Alternative Scenarios."

Specific Questions to Reviewers

1. In your assessment, do the modeled results adequately estimate the relative amount of copper from vehicle brake pad wear debris that enters the San Francisco Bay in runoff from the watershed? What improvements, if any, do you recommend for the presentation of results?

I believe the relative amount of copper from brake pads vs. other sources is strongly a function of the loadings applied to the different land uses in the model (see comments 3 and 13). Hence, I believe the HSPF modeling itself is secondary to the loading estimates, at least as far as producing total loads to the Bay is concerned. As noted in the report, the earlier spreadsheet modeling (66,000 kg) comes close to the HSPF annual loads (56,000 kg). The HSPF modeling increases in utility as control alternatives are considered and spatial and temporal patterns might affect them. Regarding the spatial distribution of loads, this is also strongly a function of the land use and loading data entered into the model and also, presumably, the spreadsheet. I don't see any special improvements for presentation of the result, other than, perhaps, some higher resolution (shorter time span) presentation of some of the observed vs. simulated hydrographs.

2. The original intent of the technical studies was to develop information on the relative impacts of copper from brake pad wear debris in the bay. In your assessment, what factors would need to be taken into account when using the results presented in this report for assessing the relative contribution of brake pad wear debris copper in the creeks?

I gather this question deals with the relative impact of brake pad wear on the Bay vs. creeks. Factors that might need to be considered include: point sources of Cu (e.g., industries) that might be located along a creek (greater Cu downstream); sorption and speciation of Cu along the creek; relative runoff from a subbasin directly out of a creek into the Bay vs. runoff distributed elsewhere along the shoreline (discussed in the report a little bit on p. 8).

3. In your assessment, do the modeled results adequately present the level of uncertainty involved in the amount of copper from vehicle brake pad wear debris that enters the bay in runoff from the watershed? Do the modeled results adequately represent the uncertainty in the relative contributions of copper in runoff from brake pad wear debris and from non-brake pad

copper sources?
How to assess uncertainty is a bugaboo in most modeling studies. Presumably this was considered in this modeling study through the stated uncertainty in the dry and wet deposition fluxes of copper given in Table 2.8. But the scenarios seem to consider only compensating loading variations (see my comment 16), so I don't really understand how uncertainty is considered here. And I don't know how this would be done other than arbitrarily to vary model parameters, which is often done to produce a range of estimates. On the other hand, I'm not sure what this would prove. A better sense of model sensitivity would help here (see my comment 4), to see where additional focus might be needed.

Sensitivity analyses will be one of the recommendations from the report.

Responses to Jim Pendergast's comments address the "compensating" loading variation question.

4. During your review, did you identify anything that could render the modeled results inappropriate for subsequent use in the Bay model as described above? Did you identify any limitations on the use of these results in the Bay model?
Most of my general comments reflect more of a need for clarification than any negative criticism of the HSPF modeling. I think the modeling reflects the spatial and temporal patterns of copper and sediment load to San Francisco Bay to the extent that such estimates can be supported by the data. This is to say, I believe these results are credible. I also believe annual loads might be produced just as well by the spreadsheet model. But HSPF provides the opportunity for good temporal resolution and many more options for controls.

Also, annual loads could not be used to realistically drive the Bay Model.

If brake pad contributions of copper are on the order of 10-35% of the total loads to the Bay, it will be interesting to see whether reductions in brake pad copper make much difference in the health of the Bay's sediments. For instance, it isn't possible (I assume) to get rid of 100% of brake pad copper, given the prevalence of older vehicles and long life of brake pads in general. What if BP contributions were reduced by 50%? Could the impact of this be seen in the Bay?

Control options will also depend on the particle size distribution and speciation of copper entering the Bay. If it's all attached to sediment, it will be more easily removable than if more is in a dissolved form.

5. In your assessment, what factors would be important when extrapolating the modeled results to other geographic areas?
Loadings (atmospheric and other) have to be the key since they are the driving parameters in determining loads to the Bay or wherever a similar model is run. Good land use data will also be necessary since loadings are determined as a function of land use. Point sources and upstream sources of Cu will also matter. HSPF will do a good job of modeling the hydrology most anywhere. It's the parameters that drive the quality processes that are most important. And calibration/verification data must always be a part of the study as well.

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Kirsten Rosselot, Process Profiles (July 19, 2007)

General comments:

I found this to be a well-written report that was a pleasure to read.

The portion of the copper load to the Bay that is due to brake pads, non-brake pad anthropogenic sources, and sediment is going to be of central interest to many of the Partnership's stakeholders. Could you include a discussion of this in your final report? I tinkered around with Tables 3.3 and 3.4 because I wanted to see what portion of the anthropogenic copper loads were due to releases from brake pads. I used a concentration of 25 mg Cu/kg sediment to eliminate the nonanthropogenic load so I could get the total estimated anthropogenic loads to the Bay and from there get the the portion of the anthropogenic loads that is due to brake pads. Is this a reasonable way to determine the anthropogenic fraction?

Yes, we believe so.

When I do this, I get that the total contribution from brake pad wear debris towards total anthropogenic loads of copper to the Bay for the mid-brakes case varies from 15% (for the Sonoma sub-watershed) to 57% (for the Upper Colma sub-watershed). For the rural sub-watersheds, the brake pad contribution is much lower than for the heavily urbanized sub-watersheds. There are six sub-watersheds whose total copper load to the bay is larger than 4,000 kg/y (for the mid-brakes case). They are Contra Costa Central, East Bay Central, Napa, Petaluma, Santa Clara Valley West, and Sonoma. These six sub-watersheds contribute about 60% of the total copper load to the Bay. It's interesting that some of these sub-watersheds have their largest contribution from sediment (Napa, Petaluma, Sonoma)), some have their largest contribution from non-brake pad anthropogenic sources (Contra Costa Central, East Bay Central), and one has its largest contribution from brake pad sources (Santa Clara Valley West).

Another thing that's of interest to folks who might want to use this study as a springboard for understanding copper in areas other than the Bay area is the fraction of releases that winds up in runoff for various types of land use. Could you discuss average runoff factors for the various land use types in the Bay area?

We have developed this information and will include a discussion and table of values in the report

I would like to see more mass balance-related discussion in the report. For example, I think the runoff factors for releases to directly-connected impervious surfaces depend heavily on the build up and washoff parameters that were developed using monitoring data in the Castro Valley sub-watershed. What happens to the copper released to these surfaces after the buildup function value is satisfied? Physically we know that this copper blows off or is blown off the impervious surface, but is it modeled as if it blows off the impervious surface and lands on an adjacent pervious surface where it is treated like a release to pervious surface, or is it modeled as if it goes to a sink and becomes unavailable?

It is assumed lost from the system – this may be another area where we can provide recommendations to investigate adding this 'lost' portion to the pervious areas and see how it impacts the final loadings. We will include a recommendation to address this

Could you prepare a mass balance of copper in the watersheds based on the source term inputs and the modeled outputs?

Yes, see the Copper Fluxes diagram attached.

Specific comments:

First two paragraphs, page 3: Does this agreement occur because the model was calibrated to produce these results?

No, this comparison was made after the limited calibration exercises and model scenarios were completed.
Or does this agreement demonstrate that using parameters that were developed from calibration exercises in one watershed throughout the greater watershed produced results that are in agreement with other research?

The agreements noted in these paragraphs simply indicate that our load estimates are consistent with other entirely different methods of estimating annual loads to the Bay. The results shown in Figures 3.5 through 3.7 show that the parameter extension from the Castro Valley Creek to the Quadalupe, San Lorenzo Creek and Alameda Creek produce values consistent with observations.

This issue returns in the 3rd and 4th full paragraphs of page 36.

Table 2.1 page 9: There's an asterisk in the second column heading that doesn't have a footnote.

Will clarify that the rainfall values in that column were obtained from the isohyetal map

What does the "(S)" in the third column heading mean?

Will remove.

Please define "MFACT" and "CIMIS" in footnotes.

Yes, we will

What is the Zone 8 MFACT (the last column)?

This is the ratio of the CIMIS Zone ETo for each watershed to the CIMIS Zone 8 ETo, and is used to adjust the PET evaporation time series from San Jose for use on each watershed.

I see that it looks like Gage MFACT normalized to Zone 8 but I don't know what it was used for, unless it's the multiplier instead of Gage MFACT.

The Gage MFACT adjusts the rainfall at the gage used for each watershed to better represent effective rainfall over the entire watershed.

Paragraph just below Table 2.1, p. 9: define "ET." "ET" shows up again on page 16 and I think it should be defined again there.

ET is evapotranspiration.

Third paragraph, p. 12: Could you discuss whether the choice of land use dataset would be expected to influence the results in any particular direction?

More recent land use might show more developed area, i.e. an increase in urbanization, and therefore increased loads associated with the developed areas.

First paragraph, p. 17: 0.8 doesn't seem small enough, observation indicates that more overwatering than that occurs in urban landscaping. How sensitive are the modeled results to this number? IIf this number is too high, what effect would that have on the modeled results? Would it make the runoff from developed land too low? Is this factor the same for crops as it is for landscaping on developed land?

See response to Bob Peters question.

Item d(e) on p. 18: Sediment associated losses don't actually show up in Tables 2.6 to 2.7. I would suggest you switch to plain bullets or something besides lowercase letters for the items in this list.

Will do.

Item e on p. 18: I want to make sure these releases are being treated as a non-constant point source load direct to the streams and that there is no buildup function involved.

Yes, that is correct.

Items 1, 2, and 3 at bottom of p. 18: There needs to be a paragraph or so explaining the rationale for choosing to run the cases this way.

The response to Jim Pendergast's comment addresses this comment.

Second full paragraph p. 31/Figure 3.4 p. 32: I think there should be some discussion of the uncertainty in the measured copper concentrations.

Will need input from Jim Carleton on the sources of the data and a discussion on the uncertainty based on how the data were collected and analyzed.

Also, the modeled results appear to be much flatter than measured concentrations. The slope of the modeled results does not appear to be near unity, either – based on an eyeball assessment that slope looks greater than one.

The vertical and horizontal scales are different, but clearly the lines are the 1:1 lines, e.g. 60 mg/l is the same on both scales – actually should be ug/l!.

I think there may be source term reasons why the modeled results are flatter than measured and I would like to see some discussion about these reasons, which include 1) the releases were applied evenly throughout the year when in reality releases can be seasonal, episodic, or vary by time of day, and 2) releases were modeled as if they were applied evenly throughout the land use types for each sub-watershed when in reality they may have occurred more in one part of the subwatershed for that land use area than another. The effect of these temporal and geographic variations in releases might be captured in grab samples, but they wouldn't be in the modeling. That doesn't mean the model didn't do a good job of estimating annual copper loads to the bay, but it may be important to understand that the way things were modeled may mean that modeled peak concentrations would not be expected to match measured.

Will add some discussion along these lines.

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From Kelly D. Moran, TDC Environmental (July 19, 2007)

This memorandum contains my own comments as well as comments from a BASMAA reviewer on the draft watershed modeling report prepared for the Brake Pad Partnership (BPP).
Overall, the way the watershed modeling was implemented appears to provide a reasonable estimate of copper loads conveyed from San Francisco Bay Area watersheds to San Francisco Bay. When supplemented with additional information to address our comments below and any substantive comments from the BPP Scientific Advisory Team, we believe that the watershed modeling report should be suitable to serve the needs of the BPP.
Our substantive comments are listed below. During our review, we identified a few minor editorial items that we expect AQUA TERRA will wish to address while completing its report. These are listed in an attachment.

- Anthropogenic copper contributions. We would appreciate it if the report were amended to include additional information to clarify the portion of the copper loads attributable to anthropogenic sources. Copper control strategies necessarily focus on anthropogenic copper. Clarification of the anthropogenic contribution will help BPP stakeholders understand the relative importance of the brake pad copper contribution. (We appreciate that the BPP project manager has already initiated efforts to address this comment.)

<u>See response to Kirsten's question and the attached graphic</u>.

- <u>Approach to modeling of brake pad releases in relationship to other copper sources</u>. The approach to addressing the range of the uncertainty of the copper source estimates is based on an assumption that does not seem reasonable—that the errors in the release estimates for copper sources other than brakes are directly linked—and inversely proportional to—the errors in brake pad copper release estimates (called "anti-variance" below). Despite our concerns about this approach, we hope that it is possible to obtain the information necessary for the BPP from the existing modeling runs. To address this issue, we suggest the following actions:
    - Consult with the Bay modeling contractor (URS) to determine whether an approach to handling the Bay modeling scenarios without assuming anti-variance in release estimates can be developed with existing information.
    - Clarify the reason for the selection of the "anti-variance" assumption in the text.
    - Revise or remove the text in the third full paragraph on Page 36, which relies on the anti-variance assumption.Discuss the additional potential release estimate variance options (e.g., high-brakes and high-non-brake sources, variance of individual non-brake sources) qualitatively.

Responses to Jim Pendergast's comments address these comments. The modeling scenarios were developed in consultation with the bay modeler. It was not assumed that the errors in brake sources were any way linked to errors in non-brake sources when developing the modeling scenarios; there was no anti-variance assumption. Instead, the scenarios were chosen with the intent that they explore the range of the relative contribution of copper from brake pads in runoff to the bay. Exploration of the source loading where both brake and non-brake sources are high would not provide further information about the relative contribution of copper from brake pads in runoff to the bay. It would provide information about the sensitivity of the model to variations in source loadings. This information can also be gleaned from modeling results for individual sub-watersheds and it is not necessary to determine the sensitivity of the entire watershed.

    - Consider additional description of the error estimates for the primary copper load estimate comparison value, which is from Davis et al. 2000. In particular, we believe that it would be helpful to provide the estimated range of the load estimate ("low" and "high" estimates can be obtained mathematically from information in Table II-7 of Davis et al. 2000). We also suggest further clarification of the preliminary nature of the SFEI copper load estimates, e.g., the SFEI report states, "[t]he simple model used in this report was intended only to provide preliminary estimates of emissions….(Davis et al. 2000, page 69).

We will add the further stipulation that Davis' estimates are 'preliminary'. Also, Table II-20 (page 49) provides lower and upper bounds loads for both TSS (170,000 and 670,000 kg/yr) and copper (36,000 and 110,000 kg/yr). We feel these are better estimates, since they were developed by the authors, than attempting to use the information in Table II-7 which only shows percentage variances by land use used in sensitivity analyses, and would need to be further interpreted and calculated.

- Inherent shortcomings of modeling approach. The report should list and discuss the implications of the inherent shortcomings of the modeling approach. The most important shortcomings to list are those that are likely to be relevant to (a) the Bay modeling or (b) interpretation of the report by stakeholders. For example:
    - The modeling systematically underestimates peak copper concentrations/loads.

This conclusion is not consistent across reviewers – one reviewer feels the model consistently overestimates loads.

> This result is an expected shortcoming of the modeling approach, which entails averaging out of sources that may occur as "events" (e.g., swimming pool discharges). In conjunction with the Bay modelers (URS), the BPP should explore what this means for the Bay modeling and for comparison to Bay water quality criteria, and the BPP should ask AQUA TERRA to include appropriate recommendations in its report (e.g., explain how to adjust for this shortcoming and/or or to understand its implications).

A section on recommendations will be included to address identified shortcomings.

- Similarly, the modeling design was focused on providing information that will allow Bay modelers to assess annual or seasonal changes in the Bay—it is not appropriate for evaluating short-term copper concentrations in creeks.

We agree.

> Because the modeling was approached on a Bay-wide basis, the values for individual modeling watersheds are necessarily approximate. (More locally-specific release estimates and/or modeling parameters could significantly affect results for higher-resolution modeling at smaller geographical scales or time steps).

We agree.

We believe that the following changes would improve the clarity of the report:

- Table 1.1, Summary of copper loads for modeled scenarios. This table does not connect well to the text, which refers to 6 model runs. This table would be more useful and clearer for the reader if it specifically showed the three non-brake runs. While it is possible for a clever reader to tease this information out of the table, the summary would be clearer to stakeholders if it were formatted such that it is obvious that there were 6 model runs. Table 3.3 is a good example of how the six runs can be clearly presented.

Table 3.3 is a more detailed presentation of results for each watershed, and we didn't feel this was appropriate for a summary presentation of results. We would like some guidance from the Steering Committee as to whether this change should be made.

- "Best Estimate." It would be helpful to label the "mid-brakes"/mid-other copper sources scenario in a manner indicating that it is the central estimate of loads from all copper sources (e.g., the "Best Estimate" or "Central Estimate" scenario).

We would like to get a consensus from the Steering Committee as to whether this change is generally acceptable.

- Explaining the value of BPP-conducted studies. In Section 1.3, it would be helpful to provide a short explanation of the framework of the BPP studies (i.e., the boxes in Figure 1.2) that identifies the studies that were specifically prepared to provide input data for this watershed modeling report. We believe that is important to explain that the BPP studies were completed with the cooperative oversight of the BPP Steering Committee and were peer reviewed by the BPP Scientific Advisory Team. The cooperative approach to preparation of these input data distinguishes the BPP-supplied studies from other information sources that happen to have relevance to this work. The cooperative approach and the peer review lend credibility to the BPP-supplied inputs.
  When BPP-funded studies are mentioned for the first time in the body of the report, it would be helpful to identify them as BPP studies.

Will do.

For example, where the report draws on the BPP studies in the first sentence of Section 2.5, it would be helpful to link back to the explanation of the credibility of the source (i.e., by adding the phrase "…as described in Section 1.3").

The following discussion will be added to Section 1.3

The Brake Pad Partnership's source release inventory, water quality monitoring, and air deposition monitoring studies were specifically prepared to provide input data for this watershed modeling report. Other BPP studies, such as air deposition monitoring, procurement of a representative sample of brake pad wear debris, and physical and chemical characterization of brake pad wear debris, indirectly provided information that supported this modeling effort. The BPP studies were completed with the cooperative oversight of the BPP Steering Committee and were peer reviewed by the BPP Scientific Advisory Team.

- Clarification that runoff from the delta is not part of the modeling (Page 6, Section 2.1, first paragraph). It would be helpful to clarify that the model does not address runoff flowing into the study area from the Sacramento/San Joaquin River delta.

We will include a clarification that loads from the Delta are not part of the Watershed Model effort but are separately accounted for in the Bay modeling effort.

- Clarify references to Bay Area Hydrology Model (Page 25, letters “a” and “d”). We believe that this would be more clear for Bay Area readers if these two items were combined into one and included the context (i.e., “HSPF parameter development for the Bay Area Hydrology Model design tool through calibration studies in Castro Valley and Upper Alameda Creeks (AQUA TERRA 2006) and in two Santa Clara County creeks (Clear Creek Solutions in preparation; parameters listed in Clear Creek Solutions 2007).”), with appropriate editing of the first sentence in the following paragraph to reflect this change. To be completely accurate, it would be appropriate to add a footnote after this mention of “Upper Alameda Creek” clarifying that the subwatershed referenced is a subset of the BPP modeled watershed with the same name.

We will add these clarifications and changes.

- Clarify data source (Page 31, first paragraph, last sentence). Please clarify the source(s) of the “limited available instream copper concentrations” and add appropriate citations to the reference list.

We will add appropriate citations; Jim Carleton provided this information and we will request the source information from him.

- Clarify references to watersheds. BASMAA asks that text, figure captions and/or tables use the term "BPP modeled watershed(s)" (as was done in Dufour and Cooke 2005) to minimize potential confusion with different watershed areas delineated by other organizations that have identical or similar names.

Will do.

ATTACHMENT

The following minor editorial items were identified during our review:

- Page 1, first paragraph, second sentence. Delete “San Francisco Bay Area”. The project is the “Brake Pad Partnership,” not the “San Francisco Bay Area Brake Pad Partnership.”
- Page 1, first paragraph, last sentence. It would be preferable to use a word other than “deposition” to refer to the quantity of copper transported to San Francisco Bay, because “deposition” is the term used for the air pathway in this work.
- Section 1.2. Consider using this section to create an Executive Summary. The summary is quite helpful, but we found its placement confusing. We found the text in this summary a bit confusing to follow (it seemed to jump back and forth between

assumptions, results and validation/calibration comparisons with outside datasets); perhaps more paragraph breaks or subheadings could be added to assist the reader.

- Page 2, Section 1.2, second paragraph, third sentence (and throughout the report). Please replace "Alameda study" with "study prepared for the Alameda Countywide Clean Water Program (ACCWP)."

Will do

Similarly, please clarify other ambiguous uses of the term "Alameda" (e.g., page 26, paragraph 1, which could be interpreted as referring to Alameda Creek, another geographical area in the East Bay, or to several possible agencies).

Will do.

- Page 3, last paragraph, second sentence. The final verb should be "are" rather than "is". ("…while the values for the East Bay watershed are 48% to 53%.")

Will correct.

- Page 4, Section 1.3, last sentence. This sentence contains a "he" that should be a "the."

Will correct.

- Page 5, Figure 1.2. Recommend addition of a source for more context for this figure (at a minimum, a link to the BPP web site).

Will do.

- Page 8, Section 2.2., first paragraph, first sentence. The verb should be plural (i.e., "include" rather than "includes.")

Will correct

- Page 9, Table 2.1. This table has a few formatting issues. It may need footnotes. There is a "*" in the header of the Rainfall column that doesn't connect to a note below the table. There are two columns for "Gage Annual Rainfall" and "Gage MFACT," with two values for Upper Alameda watershed, and one value for the other watersheds. It appears that 2 rain gauges were associated with Upper Alameda watershed. A short footnote to the table would be helpful to clarify whether this was to fill gaps in individual gauge records, or for other purposes.

See responses to Kirsten's questions on this.

- Page 26, last two paragraphs. Paragraph formats need fixing (one wraps around table; the other is centered, rather than left justified.)

Adobe pdf conversion error.

- Page 31, first line. Replace “Cleanwater” with “Clean Water” (two words).

Will do.

- Pages 43-44, references. At least one reference is missing (ACFCWCD 2005).

Will include

- Page 43, references, final reference on page. Correct spelling of “Leatherbarrow”.

Will correct

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From Lester McKee, San Francisco Estuary Institute (July 19, 2007)

P = Page number, Par. = paragraph number, L. = line number

P2, section 1.2. Please make it clear in the opening paragraphs if you are modeling total copper or dissolved copper.

We are modeling Total Copper. However, the instream model does model the fractionation between dissolved and sorbed forms, and both Total and Dissolved loads have been provided to the Bay model.

P2, Par4. Given the model objective if to estimate loads of Copper from urban areas, I am hopeful you included the gauges that represent greater urbanization (only two come to mind: Guadalupe River at San Jose/Hwy 101, San Lorenzo Creek at San Lorenzo).

Yes, see Table 3.1 and Figure 3.1, and Figures 3.5 and 3.6

P6, Par5. …small streams and creeks (add and storm drains).

Will do.

P8, Par1. Define major. For example there are 14 “major reservoirs in the Bay Area that capture >20 sqmi area (see SFEI report Davis, McKee et al., 2000).

We identified 10 reservoirs that are generally larger than 20 sq mi drainage areas, and are excluded from the model. The Davis study included 2 reservoirs that do not drain to the Bay, and the other 2 reservoirs were not included in our hydrography/waterbody coverage. We will be including recommendations that the impacts of the reservoirs should be further studied in any future efforts.

P9, Par1. …with similar rainfall total. Do you mean daily rainfall totals.

Yes.

Did you check the results of this methodology by trying to predict the hourly rainfall of a station with hourly rainfall data? This would provide you with an understanding of your %error both in timing and in magnitude.

Our limited calibration was focused on the flow duration curves and the mean annual runoff volumes, so any differences in hourly timing would have no identifiable impact in our results. These procedures have been used for the past 20 years or more in watershed modeling and have been codified in the BASINS WDMUtil Manual. We can provide a variety of references and documentation if the review so desires.

P9, Par3. Pan evaporation. This section needs better description so that the reader is not left feeling disconnected from your modeling procedure. How exactly did you estimate evaporation on an hourly basis?

Monthly PET from San Jose was adjusted for each watershed (see response to Huber's questions). Then the CIMIS data was used to distribute the monthly values to daily, and then the daily totals were distributed over daylight hours for this latitude using procedures in WDMUtil.

What about impervious surfaces?

Same PET applied to impervious surfaces.

How did you cope with roofs, roads and parking lots?

Roads and parking lots included in the DCIA percentage – see response to Huber's question regarding rooftops.

Did you check the outcomes?

We checked annual water balances for all watersheds.

Please demonstrate the methods and assumptions gave reasonable input data for the model. Evapotranspiration is likely 70% of the total water budget so I suspect it is a really important term in the model. Was the range of the estimated data reasonable over a single day and over the longer term? If not, what are the potential consequences of the assumptions and errors?

We believe we demonstrated that the model results are reasonable in the Draft Final Report; if the reviewer would like access to all the model files and results, they will be provided to ABAG and the BPP for distribution.

P12, Par3. …and associated characteristics. What characteristics? Please be specific. …and associated activities. Please add some references to the end of the paragraph so the reader can follow from where you are deriving your assumptions.

We will add some additional explanatory text, but the level of detail that this reviewer is requesting is beyond the scope and budget allocated for all reporting in this effort.

P12, Par4. I agree – impervious surfaces along with evapotranspiration are very important parameters in your model. Again, I am left having to just trust that you mode reasonable decisions. Please add some more detail in this very important section. How did you actually estimate total imperviousness and disconnected impervious surfaces? Did you use land use data, if so which data and how was it classified? You reference Defour and Cooke, 2006. Please add their table 10 in this report so the reader can see what assumptions where made and how all the calculations were done. Please discuss the potential impacts of these assumptions to the model outcomes.

We will add additional detail to the extent possible.

P16, Par1. 23% seems low. Do you mean the developed area that received irrigation? Overall based on ABAG land use data from 1995, the area in urban and ag use in the BA was more like 44% (see SFEI report Davis, McKee et al., 2000). Perhaps the differences in the model area are the cause of the apparent discrepancy?

The ‘developed’ category is 23%, comprised of 5% impervious and 18% pervious, and agriculture is 8%, based on 1992 NLCD data – Table 2.2 shows the breakdown. The different year and domain of the Davis study may be the cause for the differences.

P17, Par4. Please add the units from Pun and the units you converted to for HSPF in to the text and better explain how you did the conversion. Please comment of the errors and potential effects on the model output of the assumptions that you had to make.

Table 2.9 shows the units provided by Pun, and we can provide this reviewer with the spreadsheet of the conversion process, but we don’t feel that is appropriate for display and inclusion for the audience of this report.

P19. Figure 2.6. Given that the model objective is to understand urban copper runoff, more effort needs to be put towards disaggregating the urban source and transport characteristics and less effort on the 4 non urban classes (Ag, Grass, Scrub and For.)

The model objective is to understand the relative contribution of copper from brake pads in runoff to the Bay, not just urban copper runoff.

P23, Table 2.8. These are lower that Tsai et al, 2000 (RMP study). Please comment on how did your estimates of loads compare with the work of Pam Tsai et al. (see http://www.sfei.org/rmp/reports/air_dep/tracemetals/AD_TMFinalReport.pdf). I realize Pam’s work was primarily focused on deposition to the Bay not to watershed surfaces but she did estimate deposition to watershed surfaces as well. And her concentrations in rainfall are certainly valid.

The BPP reviewed Tsai's work and decided that the air deposition modeling should be used for atmospheric inputs to the watershed model.

P24, Table 2.9. Your concentrations in wet deposition here appear lower than Pam Tsai's work by a factor of 2 or 3 (see page 8 in http://www.sfei.org/rmp/reports/air_dep/tracemetals/AD_TMFinalReport.pdf). Please add a comment of why that is so and the potential impact to the model outcomes.

If you look at page 49 of the Tsai report, you will see that the mean concentrations in rainfall ranged from 380 ug/l for the South Bay, to 900 ug/l in the North Bay to 1800 ug/l in the Central Bay. The South and North Bay sites were located somewhat inland from the bay shore, while the Central Bay sites was on Yerba Buena island, and that Central Bay site's value of 1800 raised the mean of the 3 sites to 1200 ug/l. Thus there may be some question about the values obtained from that site.

The values in Table 2.8 are entirely consistent with the range of 380 to 900 from the land based sites.
The Copper Fluxes diagram (attached) shows that the atmospheric contributions are significant but a relatively small portion of the overall watershed load to the Bay.

P28, Figure 3.2. I was hoping to see a better calibration of the urbanized San Lorenzo system. Please comment on the potential impacts of the over estimation in the two urbanized systems (San Lorenzo and Guadalupe).

We don't feel that a 5-6% over-estimation of annual volumes (see Table 3.2), and the slight differences in the flow duration curves for these 2 sites is significant, considering that a complete calibration was not possible due to funding constraints. It is our opinion that the flow simulation is likely to be the least significant source of error in this effort.

P30, Par b, c, d. Please define "good" in this report. I see the model results for hydrology being out be 30 – 50%.

Please show your calculations for this

I see both over and under estimation that is apparently based on degree of urbanization.

Again, please clarify.

I realize that +/- 50% is not considered bad, but I am left to ponder what causes the over or under estimation and therefore I am not sure I agree with "very good" without some further commentary.

Regarding how we define 'good', the following table has been used for the past 2 decades in HSPF workshops as guidance for model users. This reference is cited in the report.

Table 1 General Calibration/Validation Targets or Tolerances for HSPF Applications (Donigian, 2000)

	% Difference Between Simulated and Recorded Values		
	Very Good	Good	Fair
Hydrology/Flow	< 10	10 - 15	15 - 25
Sediment	< 20	20 - 30	30 - 45
Water Temperature	< 7	8 - 12	13 - 18
Water Quality/Nutrients	< 15	15 - 25	25 - 35
Pesticides/Toxics	< 20	20 - 30	30 - 40

CAVEATS: Relevant to monthly and annual values; storm peaks may differ more
Quality and detail of input and calibration data
Purpose of model application
Availability of alternative assessment procedures
Resource availability (i.e. time, money, personnel)

P31, Par1. It would be more helpful to see scatter graph of observed versus modeled data for TSS so that the reader could see if the model is performing consistently over the range of observed concentrations. Please add the scatter plot. P31, Par 2 come part way to achieving this.

We will respond to the extent possible.

P31, Par3. Do you mean San Lorenzo Creek?

Yes, 'river' is a typo.

San Lorenzo River is not within you model boundaries (also Figure 3.6). I have collected three years of high flow data collection to total Cu at Hwy 101. JC used this data to verify his model. Is that factored in to your model as well?

See Figure 3.5, if the reviewer's data is included in this simulation for 1988-2003, we will include a citation.

If so please reference SFEI and my reports and provide more detail about what was done.

We will add clarification to the extent possible.

P31, Par4. …will provide sound technical basis for performing the alternate scenario runs. You may be right, but I am not convinced. I think adding some more scatter plots would help.

However the main addition that would help would be a sensitivity analysis of errors. I suggest that such an analysis should be completed to provide validity to you statement.

A sensitivity analysis will be part of our recommendations.

P37, Table 3.3. The loads for Santa Clara Valley Central and West represent an area of about 335 sq mi. You loads of sediment and Cu are 75554 tonnes and 7037 kg. The loads of sediment and Cu I have measured during WY 2003 (about average flow) in Guadalupe River at Hwy 101 were 10,806 tonnes and 820 kg for an area of 160 sqmi about 91 sqmi of which is downstream from reservoirs. No matter which way I scale up the sediment or copper loads, it appears your model over estimates Cu load for that part of the Bay Area. Sediment is over estimated by at least 1.9 times and Cu is over estimated by at least 2.3 times (see McKee et al., 2005 for the Cu loads http://www.sfei.org/watersheds/reports/409_GuadalupeRiverLoadsYear2.pdf). Please comment the cause of the discrepancy and on how this might impact the usefulness of the model.

Possible causes of the differences are:

1. One year of data versus 25 years of simulated values; average flow does not translate into average load, and I'm sure the reviewer recognizes that sediment loads are highly non-linear with flow
2. Watershed model is not calibrated to account for deposition in streams/creeks, and essentially assumes all watershed load is delivered to the Bay.
3. Assumptions on constant annual copper sources which are likely to demonstrate seasonal behavior
4. Model simplifications needed to complete this effort with limited resources and time.

Clearly further investigation is needed, along the lines of the various recommendations that will be included in the report, to improve the model results. However, we do feel that the current model results provide our current best estimate of watershed loads for use in the Bay modeling effort.

P38, Table 3.4. Please clarify that each column represents the low best and high scenarios.

Yes, we will add clarifying labels to the columns.

Please comment on any other literature that estimates BP contribution to urban runoff. For example, perhaps the modeling done in Stockholm made these estimates.

Please provide citations if possible

Is 35% reasonable? Given you have potentially underestimated atmospheric contributions, perhaps 35% may be the upper estimate?

P43. The report lack a discussion of potential errors associated with the assumptions. It would benefit from a sensitivity analysis, a conclusion, and section describing data gaps that would improve future modeling efforts for the Bay Area.

We will respond to the extent possible with remaining resources and time.

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From Robert Ambrose, US EPA (July 19, 2007)

Reviewer Response

I'll start with some general comments and questions about the report and the modeling work before addressing the specific charge questions. First, I want to express my great confidence in the technical expertise of the report's authors, Tony Donigian and Brian Bicknell. They are certainly among the best in the world in applying HSPF to analyze watershed hydrological and pollutant response. Second, I acknowledge the coordinator's observation of the strong effort to get this draft report together in time for review, and that certain items would be added and corrected in the final report.

I have two or three main technical questions about this application.

Modeling of Land Use
The report makes it clear that individual land uses were considered, but it does not make clear if they were modeled explicitly or implicitly in the assignment of watershed parameter values. I have always thought of HSPF as a lumped parameter model as applied to individual subwatersheds, but I understand that there are ways to model individual land uses. Were land uses lumped and modeled explicitly within each watershed? That would make sense to me, but it is never stated that this is how the modeling was conducted. If land uses are not modeled this way, how is the information in Table 2.4 used?

Each of the land use categories shown in Table 2.3, including both pervious developed and impervious, was modeled explicitly, and we will add clarifying statements to the report.

Assignment of Hydrological Parameter Values
On page 13, the last paragraph states that model parameters for each watershed and land use category were assigned based on the soil classification (one of 4 HSGs). In the last paragraph on p. 25, the report states that hydrology model parameters were based on soils and slopes. These two paragraphs imply to me that the model would use the same parameters for forested, grassland, developed, and agricultural land uses if the HSG and slope is the same.

No, model parameters were assigned based on combinations of land use, soils and slope; predominant soils (HSG) and mean slopes within each watershed were determined (as described in the report) and then parameters for each land use were assigned using this soils and slope information

That wouldn't seem right to me. The report should state clearly the methods used to account for individual land uses within each watershed. A table summarizing model parameters by land use and watershed would be helpful.

There are really too many parameters, and insufficient resources, to develop complete summaries. Complete model files will be provided at the conclusion of the effort and should be available from the BPP.

Modeling Copper Fate in Watersheds

There is no clear description of how the model handles the fate of copper loads deposited to the watershed. A section should be added summarizing how this is modeled. This could mostly be copied from the HSPF technical manual. The report should also list the sediment and copper parameters used in this application, and describe how their values were derived. Text on p. 31 mentions washoff parameters, but no equations or values are given.

We will add some clarifying text.

On p. 18, item "e" states that "rain-dependent releases are represented in the model as originating from impervious surfaces based on a calculated concentration and the runoff volume from those surfaces for each watershed." Though it's not entirely clear, I assume that these releases are not meant to represent runoff and erosion of atmospherically-deposited copper from the watershed (such as wet and dry deposition and BPW roadway releases), but rather the "architectural and industrial 'rain-dependent' releases" mentioned in item "d.d." This should be clarified.

Will do.

On p. 18, item "c" states that roadway releases of copper were adjusted to account for losses due to street sweeping. The adjustment method (equation, algorithm, parameter values, etc.) should be described. The amount of copper reduced due to street sweeping must go somewhere in the watershed. How were those loads handled? Were they added to wastewater loads somewhere?

Item c is expanded to say: "Copper removed via street sweeping was approximately a tenth of the copper released direct to roadways. Street sweepings are securely landfilled and the copper they contain is not expected to be entrained in runoff." The watershed modeling report did not provide a review of many BPP work products, such as the release estimates and the air deposition modeling. A review of the methodology for street sweeper removal will not be included either.

Modeling Results

As noted, the flow calibration seems good to very good. The general pattern TSS and copper loading in Castro Valley Creek, Guadalupe River, and San Lorenzo River seem good, except that high loadings in Castro Valley Creek are not captured. I don't believe these are "outliers," as stated on p. 31, as there are 6 of these values. The implications of this apparent bias should be addressed.

See response to Jim Pendergast's questions and attached plots.

Charge Questions

1. In your assessment, do the modeled results adequately estimate the relative amount of copper from vehicle brake pad wear debris that enters the San Francisco Bay in runoff from the watershed? What improvements, if any, do you recommend for the presentation of results?

I would judge the modeled estimates of the relative loadings to be adequate. Previous estimates were that brake pad depris constitutes about half of the copper loading to the Bay, whereas this study centers on about a quarter. I do have questions about how copper lost in street sweeping was accounted for, and whether it might enter the Bay in other waste streams.

2. The original intent of the technical studies was to develop information on the relative impacts of copper from brake pad wear debris in the bay. In your assessment, what factors would need to be taken into account when using the results presented in this report for assessing the relative contribution of brake pad wear debris copper in the creeks?

I would like more information on the differences in loading estimates in this report, which centered on 23%, versus earlier work by Carlton and Cocca that centered on about 50%. I also would need to know more about how street sweeping was handled.

3. In your assessment, do the modeled results adequately present the level of uncertainty involved in the amount of copper from vehicle brake pad wear debris that enters the bay in runoff from the watershed? Do the modeled results adequately represent the uncertainty in the relative contributions of copper in runoff from brake pad wear debris and from non-brake pad copper sources?

I believe that the results showing high, medium, and low estimates give an adequate representation of the range of uncertainty in the relative contributions of copper from brake pad wear (10% to 35%). I'm not sure about the accuracy of the overall loading estimates, since the modeling took overall loading estimates as given. I'd be a little concerned about the under-simulation of high copper loads from Castro Valley Creek.

4. During your review, did you identify anything that could render the modeled results inappropriate for subsequent use in the Bay model as described above? Did you identify any limitations on the use of these results in the Bay model?

One of my main concerns is how land use patterns affect results. These concerns might be alleviated by the authors with some further explanation of procedures. At present, I'd have to caution use of this model to explore scenarios with different land use patterns.

5. In your assessment, what factors would be important when extrapolating the modeled results to other geographic areas?

At present I couldn't recommend extrapolating modeled results to other geographic areas. I would need to understand more about how the model was applied to individual land uses, and how parameter values were derived. If important (sensitive) model parameters represent lumped processes and are calibrated, then the model must be considered descriptive of present conditions in the modeled watersheds. This is still useful, of course.

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Ken Schiff, Southern California Coastal Water Research Project (July 19, 2007)

The stated goal of this paper is to develop a watershed model of flow, sediment, and copper loads to San Francisco Bay. The uniqueness of this project is the linkage with atmospheric deposition, and more specifically, the atmospheric deposition associated with brake pad debris wear. A wonderful question and fun challenge indeed! Below are a few of the major items I saw that could improve the report.

I assume that this watershed modeling is an extension of the original watershed modeling conducted by Carleton for Castro Valley Creek. If so, a quick comparison of the two models would be helpful. Overall, the model setup is very straightforward and the typical model parameters are from the regular sources (i.e., NHD+, DEM, local rain gages with extrapolation, NLCD, etc.). What's unique are the BPDW source terms. To the person who has not read the atmospheric deposition component by Pun and Rosselot, a good summary here would be very helpful, especially since these terms are not specifically defined. Roadway release, although variable, appears to be empirical data. Atmospheric terms are modeled.

See earlier responses to this question.

Two issues that are not addressed in the report:

1) Are there non-BPDW deposition rates for both wet and dry conditions?
2) Of the non-BPDW releases, a very large portion may be from historically deposited BPDW that was either resuspended (for atmospheric terms) or washed off (for nonatmospheric terms). This is an important element as we have found a large fraction of deposition is from nonspecific dusts.

The modeling calibration and validation deserve a bit more effort. The authors begin the most important section (3.0 application and results) with a paragraph of excuses. This does not give the reader confidence in the results. The fact that such a high profile project would not provide adequate time and resources (in the authors opinion) for thorough calibration and validation is startling. In fact, looking at the "QA checks" leads me to believe that some calibration and validation would definitely help.

This will be included as a recommendation.

Some of the larger items I noticed:

1) Flow duration curves match well (fig 3.2, 3.3), but little other hydrologic validation is provided that could significantly influence sediment or copper concentrations (i.e., daily mean flow, daily max flow, time to peaks, etc.)

See earlier responses to calibration questions, e.g. by Huber.,

2) Units in table 3.2 need to be examined.

Units are inches over the watershed, fairly standard in hydrology modeling reports

3) The TSS and copper plots do not show a "remarkable degree of consistency with observations" to me.

See response to Jim Sansalone's question below

I do not see much relationship in the simulated concentration and observed data points in the long-term simulations. In fact, the correlations of observed vs. simulated data for copper in Fig 3.4 looks doubtful, even with the outliers removed. I would have the authors double-check the slope and r^2.

See earlier responses to these questions, and the attached plots with outliers identified

4) The observed copper data vs. long-term copper simulations in figs 3.5-3.7 are insufficient for me to evaluate validation. I recommend more scatter plots and/or summary statistics of accuracy, precision and bias.

Validation was not performed, as noted earlier, due to resource limitations. These are shown as consistency checks and were generated without any calibration effort.

Although I am somewhat harsh in some of my criticisms, I do think this is a worthwhile and important effort. I encourage the authors to spend some more time performing the necessary calibration and validation steps. If this extra effort requires additional resources and time from the sponsors, it will be a valuable investment. Even if the numbers change little, the extra confidence it provides the reader will be important.

This will be included in recommendations.

One last issue, albeit nontechnical. Who is it that will decide that the BPDW fraction is large enough to warrant management action? Is 35% too much? Is the action threshold lower (say 10%) or does BPDW have to be the majority (>50%) of the load? These policy decisions are often times equally as important as the scientific information. Sometimes agreeing to action levels beforehand can help avoid miscommunication at the end of the study.

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John Sansalone, University of Florida (July 22, 2007)

The reviewer examined the report in the context that the draft report will be a component step towards building a comprehensive calibrated/validated model of copper loads to San Francisco Bay. In the report the authors clearly indicate the constraints they worked with; specifically lack of data, lack of resources and lack of funds. As a result, the HSPF model developed in the study was not calibrated and validated. It is assumed that all parties accept that the results and conclusions are developed from an uncalibrated/unvalidated model due to the reality of the constraints identified. The report title should reflect these limitations accordingly. As a draft report, the report appears to be a reasonable draft summary, although the original scope and constraints are not directly known by this reviewer.

Since the study and report represents an important evolutionary step that builds a foundation from the previous literature and data collection, it is of critical importance to fully details the input parameter values and ranges and their specific sources in a series of tables within the report.

See earlier response to Bob Ambrose questions.

This is an organizational exercise since all of these input parameters were required to run the model. Large databases such as rainfall should have their specific source identified, and the actual digital data should be provided to the sponsor in an organized and clear format on a CD or similar.

This will be done.

(This may be part of the deliverable package to the client, but is not clear from the report). Input parameters that were assumed or estimated should also be identified in an organized format of tables based on model algorithms for watershed or catchments and references and rationale identified. The use of input data and parameters is discussed throughout the report but without the ability of the reviewer or client to assess the validity and basis of input data and parameters.

In general the reviewer finds the report language replete with lack of precision and lack of clarity for an engineering document intended to build the knowledge base. There are a number of grammar and punctuation corrections that are required; and can be identified through careful editing. Again, this observation is important if the report is to have lasting value in the evolutionary process. As with the language, the graphical results could also need improved clarity. The graphics are not report quality.

Some deterioration is due to the Adobe pdf conversion, but we will strive to improve the graphics where possible.

There are a number of specific comments. These comments are generally organized from the beginning to the end of the report.

The authors indicate that HSPF is considered a premier model. Can the authors provide an independent reference to demonstrate "premier".

Will review wording

The authors should concisely describe the other models they considered and why these models were not selected in favor of HSPF. This rationale is important as others undertake parallel and future modeling efforts.

Use of HSPF was specified by the BPP approved work plan for the watershed modeling effort.

The authors have properly included an expanded database with 20 precipitation gages given the micro-climate variability of the watersheds. However the watershed components do not appear to reflect a database of equal quality and detail in order to translate rainfall to runoff. It would be helpful if the authors create input parameter tables with quantitative values and units, parameters ranges, reference sources so that the client and future investigators can recognize input parameter strengths and weaknesses. Future work can rely on the defensible strengths and work towards improving weakness in the input parameters.

See earlier responses to Bob Ambrose's questions.

It is unclear if the authors used GIS data to generate a carefully disaggregated set of hydrologic functional units (HFU) for each land use. Each HFU can then have hydrologic properties identified such as depressional storage, infiltration parameters based on physical soil properties, etc. There should be tables of HFUs and a drawing delineating and quantifying HFUs for each watershed.

Watershed boundaries were defined and approved by the BPP Steering Committee, as discussed on page 6.

The authors indicate the lack of data and resources. The authors should identify the specific data needs and resource needs for the benefit of future investigations. Again this can be best carried out in tables and with a description of these needs.

Recommendations will address data needs.

The authors should quantitatively describe their methods of limited calibration and the quantitative results of limited calibration.

We believe this was done in Section 3.2

The authors should quantitatively describe their "consistency" and QA checks in terms of both methods utilized and quantitative results that illustrate the consistency and QA checks. Some of these are illustrated in Figure 3.2 and 3.3.

See response above.

It is noted that while two modeling methods provide similar load results, that does not necessarily imply that this represents reality without an objective yardstick.

This is only included to show consistency with prior studies, nothing more, as there is no 'true' answer to what are the actual watershed models to the Bay.

The text and figures are inconsistent regarding suspended sediment and TSS. What is being measured is particulate matter in aqueous solution. Suspended sediment may be measured as TSS (a method of measurement) but TSS is not an accurate representation of suspended sediment. The proper terminology is suspended sediment measured as TSS. In addition suspended sediment should not be confused with the SSC methodology, which I suspect was not utilized in most of the data collection.

We will attempt to clarify this issue.

There should be a list of symbols and abbreviations at the front of the report with definitions and units provided.

Very if any symbols are used in the report, and we will include an acronym list if resources allow; otherwise we will be sure to define acronyms frequently within the report.

Nearly all of the figures need graphical and clarity improvement in order to provide clear, report quality figures.

We will improve to the extent possible.

This is especially true for the area maps, which in a number of cases are missing scales and north arrows. In some cases map boundaries and sub-delineations should be shown more clearly. This should not be a problem given the GIS capabilities. Each map caption should provide references for the sources of information.

How do model results respond to various levels of watershed and loading parameter discretization? This parametric evaluation is important not only for copper loadings, but for hydrologic parameters and watershed hydrologic functional unit parameters.

Addressing this issue was not possible within the resource constraints fo this effort. We will include this in our recommendations.

The study talks about data types – define data types.

Will do.

The authors should indicate why it is necessary for the model setup, "by necessity" to aggregate all watershed drainage.

BPP defined the watershed boundaries to be used which aggregated all the drainage into a single stream and outlet to the Bay. This was part of the BPP approved work plan for the watershed model

The use of indefinites and personal pronouns, at least in this reviewer's opinion, are not appropriate for an engineering report.

Will attempt to remove to the extent possible.

The use of a single level value for multiple streams should be justified with a reference. This is the case for many, many assumptions made in the report and model.

Many of the assumptions are the result of the watershed boundaries and spatial definition (or lack thereof) specified by the BPP.

The storage characteristics of the reservoirs should be summarized in a table.

Further investigation of the reservoirs is included in our recommendations. Storage values were not tabulated in this effort

Identify how many of the study stations were updated with respect to the total number of stations utilized.

Not sure what this referes to

The HSPF hydraulic parameters should be provided in table format with references.

See response to earlier question from Bob Ambrose.

Briefly describe methodology for "actual simulated" ET.

Will clarify

Also briefly describe what procedures were used from the Alemeda County study and why they are applicable to this study.

Will do, the procedures are applicable because Alameda County is within the Study Area.

Describe infiltration algorithms and how these were related to soil properties and moisture conditions. For example the use of physical soil properties to determine gravitational and soil suction components of soil hydraulic conductivity. How were soil properties delineated across catchments?

HSG were defined within each watershed, and the percent distribution was calculated to establish the dominant HSG

Is the mean the representative statistic for slope – are slopes normally distributed; if not, use the median.

Mean slope of the overland flow plane is a required parameter within the overland flow routing algorithm of the model.

Provide a basis and reference for the irrigation efficiency factor of 0.8, this appears high. Identify the copper loads from the irrigation water.

See earlier response to Bob Peters.

Are annual historical precipitation values normally distribution, so that a mean annual value is representative of the distribution?

No, they are usually log-normally distributed, but mean annual precipitation is a standard statistic used in hydrology.

How is the overall long-term mean annual precipitation shown to demonstrate how the mean varies?

The mean is used to adjust point rainfall at a gage to watershed-wide rainfall applied in the model.

Describe and quantify how losses due to street sweeping were determined.

This comment was addressed in a response to a comment from Bob Ambrose.

How was a “best estimate” of copper contributions determined?

See response to Jim Pendergast’s question

Figure 2.6 could use more work; I cannot put my hand on it, but it is not particularly useful in the present form.

That’s disappointing as it was well-received, after some further refinements, at the BPP Stakeholder Meeting. A copy of the figure with flux numbers is attached.

Tables should be report quality.

Some of the formatting is the results of Adobe pdf conversion errors. Those will be corrected, but otherwise the tables will not change unless a different format is requested by the BPP.

Describe and illustrate statistical comparisons.

These are fully described in the cited references, since these tests/comparisons were not possible in this report it would not be useful to elaborate here.

While weight-of-evidence approaches are described in the Guide, demonstrate how the approach was used in this study and why it is deemed valid in lieu of calibration/validation.

Will add some clarification

Be specific, what are the sediment and copper parameter values?

Will add some additional discussion

Figure 3.2 and 3.3 are important but the graphical quality is weak. How do authors reconcile the large differences are the far more frequent low flows?

Low flows have little impact on stormwarter runoff of sediment and copper – also see earlier responses to Jim Pendergast's questions.

Consistency and QA checks are very unclear in the report and not detailed or quantified.

Most of the consistency checks are purely graphical, as there was insufficient data or resources to do quantitative comparisons, other than for flow at the identified USGS sites.

Demonstrate the decision to throw out 6 of the data points is statistically valid; there are specific test to demonstrate this. Why 6, why not 5 or 7? If the data cannot be thrown out statistically they should be included.

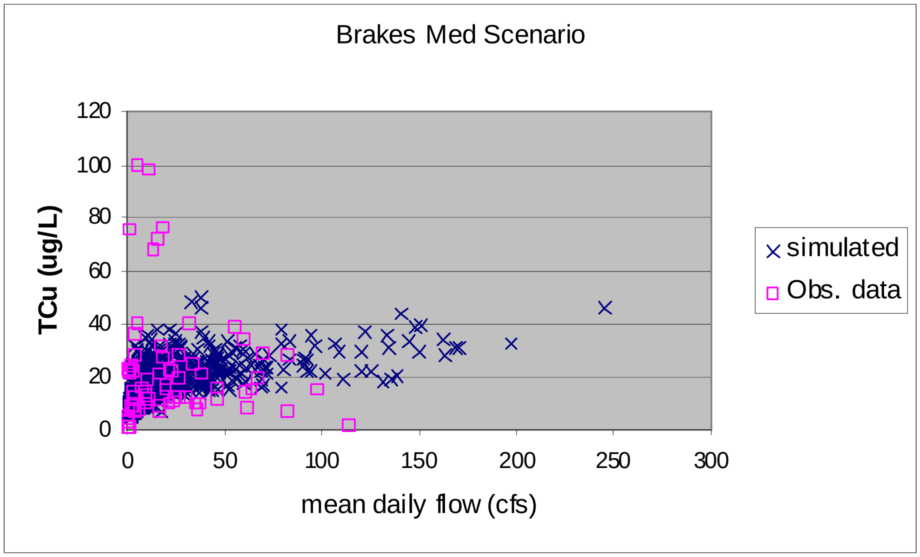
See responses to Jim Pendergast's questions, and attached plots.

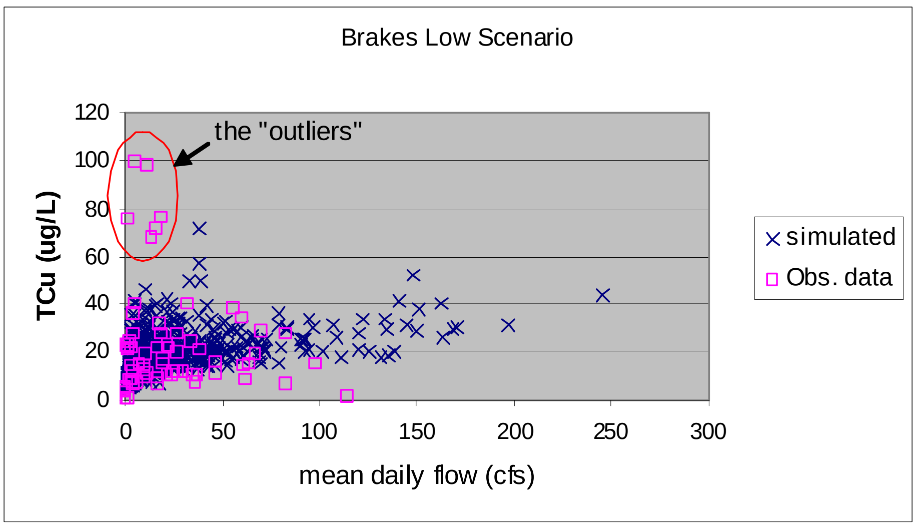
Describe all calibrations quantitatively.

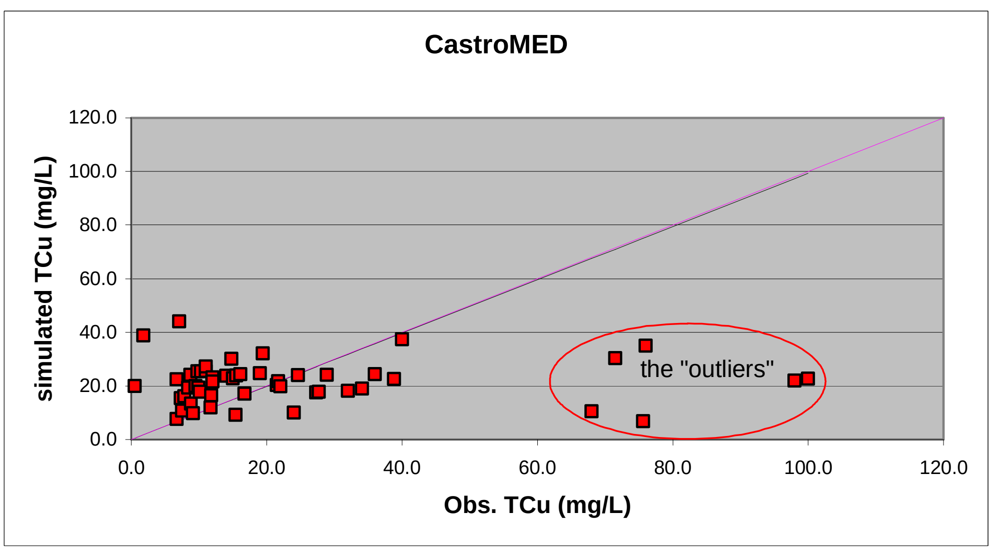
What is a "remarkable" degree of consistency? This is probably inappropriate for a quantitative engineering report.

Will consider alternative wording – the extent to which the results are 'remarkable' is that they easily fit on the same page using an arithmetic scale, not a log scale, were generated with little to no calibration, and used average annual source terms developed from separate independent studies . Anyone who has done extensive watershed modeling for water quality would appreciate the degree to which these results are 'remarkable' – but we will consider alternative wording.

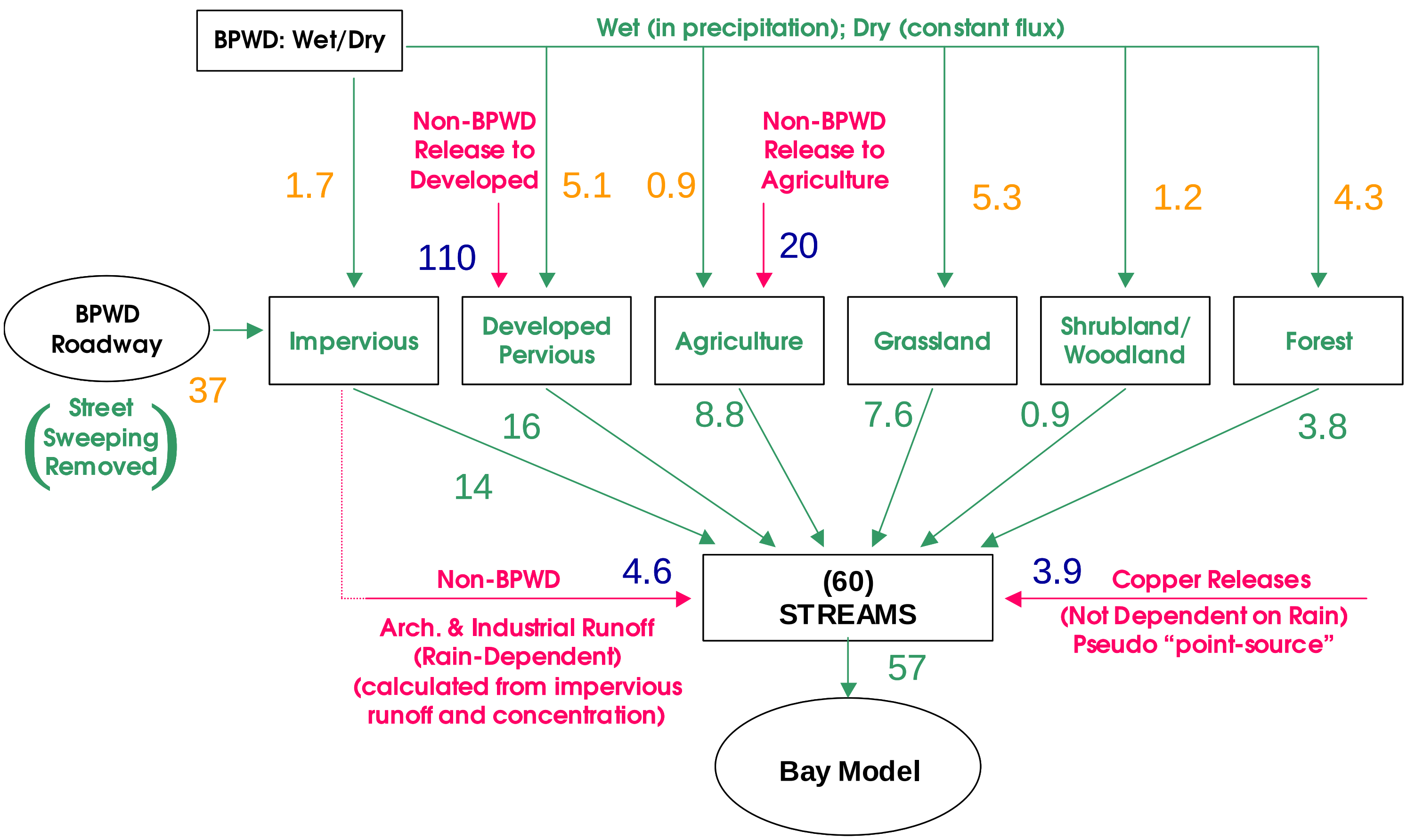
ATTACHMENTS

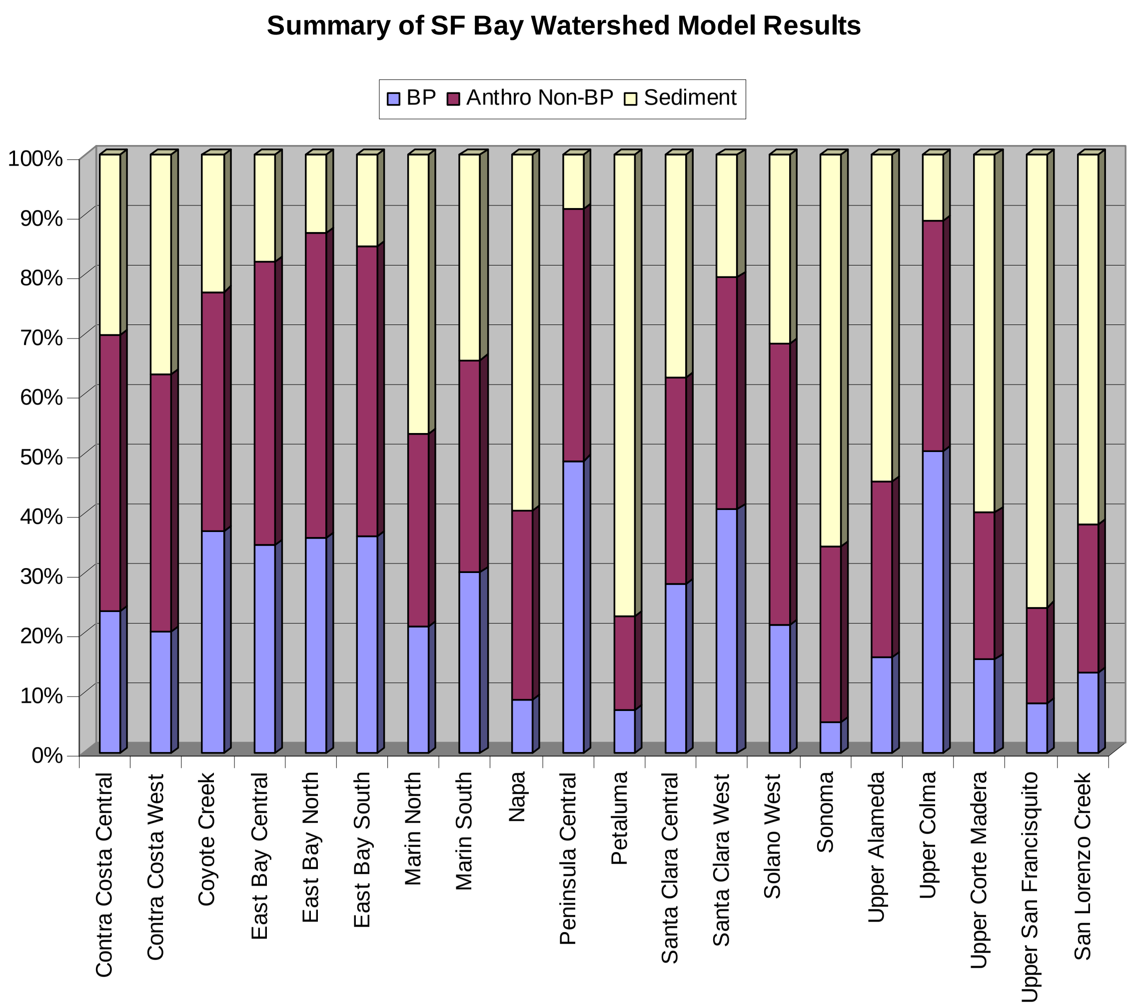






MEAN ANNUAL COPPER FLUXES IN THE WATERSHED MODEL (1000 kg/yr)




Summary of SF Bay Watershed Model Results
BP
Anthro Non-BP
Sediment
100%
90%
80%
70%
60%
50%
40%
30%
20%
10%
0%
Contra Costa Central
Contra Costa West
Coyote Creek
East Bay Central
East Bay North
East Bay South
Marin North
Marin South
Napa
Peninsula Central
Petaluma
Santa Clara Central
Santa Clara West
Solano West
Sonoma
Upper Alameda
Upper Colma
Upper Corte Madera
Upper San Francisquito
San Lorenzo Creek