Appendix D

Expert Panel Review Comments

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Introduction

An Expert Panel was convened to review and provide input on several key documents during the HMP development process. The purpose of the Expert Panel is to help ensure that the HMP is scientifically defensible and embodies a sound approach to hydromodification management. The Expert Panel consists of Professor Brian Bledsoe, Ph.D., Colorado State University[1]; Professor Tom Dunne, Ph.D., UC Santa Barbara[2]; and Professor Matt Kondolf, Ph.D., UC Berkeley[3]. The panel has reviewed the Literature Review (September 2002), Technical Memorandum #1 - *HMP Assessment Method* (October 2002), the *Hydromodification Management Plan, Draft Interim Report, Assessment of the Lower Silver–Thompson Creek Subwatershed.* (July 2003), *Hydromodification Management Plan Report – Public Review Draft* (June 2004), and specific technical aspects of the HMP assessment method approach according to their areas of expertise. In addition, Mr. Craig MacRae, P.E. of Aquafor Beech Limited[4], and Professor Ted Hromadka, Ph.D., P.E., of California State University, Fullerton, and Hromadka & Associates[5], also provided valuable input on several elements of the methodology.

This appendix presents a summary of the Expert Panel's comments regarding the development and testing of the hydromodification assessment methods and management strategies. The actual comments can be found in a separate binder titled "*Expert Panel Review Comments, SCVURPPP, Hydromodification Management Plan*".

Table D-1 indicates the material reviewed and/or commented on by the Expert Panel and others. The first five line items represent requested peer reviews of specific work products. The last four line items represent personal communications not associated with a specific work product.

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Table D-1 - Listing Work Conducted By Expert Panel and Other Professionals

Task or Topic	EXPERT PANEL			Personal Communication	
	Brian Bledsoe, Ph.D.	**Tom Dunne, Ph.D.**	**Matt Kondolf, Ph.D.**	Craig McRae, P.E.	Ted Hromadka, Ph.D.
Task 2 – Review Literature Review	**Yes**	**Yes**	**Yes**	-	-
Assessment Method Tech Memo No. 1	**Yes**	**-**	**Yes**	-	-
Task 5 – Review Draft HMP Report	**Yes**	**Yes**	**Yes**	-	-
Technical Requirements and Geographical Areas for Hydromodification Management, Chapters 3 to 6	**Yes**	**Yes**	**-**	-	-
Task 10 - Hydromodification Management Plan, June 2004	**Yes**	**Yes**	**-**	-	-
Use of Index Method	Yes	-	-	Yes	-
Simple Approach, Discrete Events vs. continuous modeling	Yes	-	-	-	Yes
Critical Shear Stress for Bank Material	-	-	-	Yes	-
Layout of Cross Sections for Stability Assessment	-	-	-	Yes	-

Dr. Brian Bledsoe

Literature Review

Dr. Bledsoe thought the following concepts needed to be stated more clearly:

- Sand bed streams and gravel bed streams are fundamentally different in behavior and sensitivity to urbanization,
- Bed material type needs to be stated when discussing stream behavior and response,
- There is an important distinction between connected and disconnected imperviousness,
- Maintenance or recovery of geomorphic stability does not necessarily imply habitat maintenance or recovery,
- Inflowing sediment loads are a central control on channel response, but different stream types respond differently to inputs of sediment (size, amount, timing, network processes are important),
- Maintaining the energy dissipation features and processes in urbanizing watersheds is critical,
- Streams must be viewed as multi-scaled systems, and the spatial and temporal scales need to be defined when discussing stability,
- When discussing bank stability analysis “basal endpoint control” is an important concept, emphasizing the significance of a stable ‘toe’.

With respect to the risk-based modeling concept, Dr. Bledsoe emphasized that the $S(Q/D50)^{0.5}$ parameter was a first cut to show how simple indices can be related to stable versus unstable channel forms and that it could be improved with better descriptions of the channel boundary, energy dissipation features, continuous flow regime and regional calibration. Dr. Bledsoe suggested that the decision-based models of stream stability he developed should be extended using descriptors of key flow regime attributes, the condition of channel banks and riparian zones, geologic or wood influences, floodplain connectivity, developments style, etc.

Dr. Bledsoe also identified several essential references that he felt should have been included in the review.

Tech Memo #1

Dr. Bledsoe brought up the question of how to develop criteria and what information would be used for separating stable vs. unstable, healthy vs. unhealthy, quasi-equilibrium vs. habitat quality. Classification trees and logistical regression, using probability as the dependant variable, were suggested as statistical tools. It was also noted that streams may achieve some new quasi-equilibrium form after land use change and yet lack geomorphic heterogeneity and a desirable habitat structure.

Dr. Bledsoe emphasized the importance of time and channel evolution when defining thresholds. He also noted the difficulty in associating hydraulic parameters with severely unstable reaches because they have already gone through significant adjustments. He recommended identifying reaches at varying stages of adjustment and recognizing that sections which are incipiently unstable or in the early stages of evolution may provide some of the best information on how to set thresholds. Dr. Bledsoe suggested applying a channel evolution model to incorporate time, such as Schumm et al. or Downs. It was also noted that different models may be required for erosive and depositional situations.

Dr. Bledsoe liked the continuous modeling approach, but suggested identifying some collection of selected single events that could act as surrogate for the entire flow series to facilitate practitioner use. He also recommended using a distributed hydrologic model to account for the spatial distribution of imperviousness and land use over time, and suggested Dr. Glen Morgan's work at the University of Maryland.

Dr. Bledsoe recommended using a 10m DEM to map a coarse scale picture of stream power in the Basin. Using flow accumulation and DEM slope, $(Slope*DrainageArea)^{0.4}$ could be calculated and used as a surrogate for valley stream power. The calculated value could come in handy with regard to potential planform change in the predictive model.

Dr. Bledsoe uses a reference discharge (such as a regression relationship between drainage area and discharge) to get an idea of channel enlargement. Although there are many exceptions, Dr. Bledsoe noted that the ratio of $Q_{bf}/Q_{effective}$ seems to increase with $Q_2/Q_{meanannuaflow}$ (which is one measure of flashiness), indicating that sub-bankfull flows often become most effective in urbanizing watersheds.

Dr. Bledsoe mentioned the partitioning of shear stress is important in the use of power to "resilience" ratios because non-grain roughness may reduce bed shear and the resulting grain size measured on the bed. Some very stable reaches can have relatively high Shields parameters due to wood or other roughness elements resulting in smaller gravel.

Dr. Bledsoe also recommended the use of qualitative models such as Schumm's or Lane's Balance. In addition he suggested some preliminary sediment transport analysis, noting that simpler bedload equations such as Meyer-Peter-Muller can be used with the data on grain size, excess shear, velocity or unit discharge. He pointed out that a sediment transport relationship would need to be chosen to use the erosion indices described by himself and Craig MacRae.

Dr. Bledsoe pointed out that a clear identification of calibration and validation steps as well as the specific feedbacks used to refine the predictive models needed to be added to Figure 1. In Figure 2 it was suggested to use Excess Precipitation and note bed material/resilience as was done for bank material. In addition a box for proximity to thresholds could be inserted.

Draft HMP Report (July 2003)

General Comments

Overall Dr. Bledsoe felt that GeoSyntec had probably identified meaningful thresholds associated with channel degradation in the project watersheds. However, caution and judgment in interpretation were stressed due to the question of how much of the differences in cumulative work reflect the consequences of incision as opposed to the causes of incision. Dr. Bledsoe recommended trying to reconstruct the pre-development condition by adding a reasonable floodplain to incised sections or giving less weight to flows that would have gone overbank in the pre-development scenario. If widening had occurred, a reference width from hydraulic geometry could be used. Schumm's channel evolution model as modified by Watson et al. (2002) could be used to identify the conditions associated with beginnings of instability (CEM Type II). For overbank flows, Dr. Bledsoe uses two separate at-a-station effectiveness of overbank flows: one for sub-bankfull flows, and one for flows that go overbank.

Dr. Bledsoe also emphasized that the threshold at which currently stable streams become unstable may not be the same as the threshold at which unstable streams stabilize. He noted that the hysteresis is probably due in part to the increased effectiveness of flows greater than Q_2 in incised channels.

Dr. Bledsoe stressed that base level lowering, upstream headcut migration, and instability can result from downstream impacts that are not directly related to the flow regime in the reach of interest. Caution with imperviousness vis a vis the headwater/undeveloped segments was emphasized because even at low or zero imperviousness, the stability of upstream tributaries is often threatened by downstream incision if headcuts are not intercepted.

Dr. Bledsoe drew attention to the fact that one of the major benefits of developing continuous flow simulations is a better picture of the ranges of flows that are most effective in the current and future scenarios. He acknowledged the uncertainty in soil storage and other model parameters, but felt that nonetheless it is clear that the effectiveness of flows less than Q_2 is significant in these streams. Dr. Bledsoe felt that this point was very important for the stakeholders to understand if new development was to be managed effectively.

Dr. Bledsoe noted that even in "stable" fine-grained systems the ratio of $Q_{bankfull}/Q_{effective}$ often increases with $Q_2/Q_{meanannual}$ (i.e. it seems the flashier the system, the more work is performed by subbankfull flows and the cross-sectional dimensions do not correspond to the computed effective discharge). He asked how much the estimate of Q_2 magnitude would change if a partial duration series is used instead of the annual maximum series.

Dr. Bledsoe also noted that determining appropriate critical shear values is challenging for the bed materials of these channels. He referred to work by Jackson and Beschta (1984) and Wilcock and Crowe (2003), which suggest that sand /gravel mixtures are quite complex in terms of incipient motion. For the streams in this study, the D50s are the gravel and the % sand seems to vary from about 28-57%. In this range the threshold for mobility of the gravel can be greatly reduced by the sand component and the more general mobility threshold no longer applies. Based on Wilcock's recent paper, Dr. Bledsoe recommended something on the order of 0.021 for critical dimensionless shear of the gravel for the streams in this study.

Dr. Bledsoe mentioned that using a reference reach to set the denominator of the Ep analysis could be a little contentious. Because the D50 is fairly consistent and the threshold of erosion is exceeded by most flows, Dr. Bledsoe thought it might be feasible to simply use specific stream power (SSP) or shear without reference to critical values. Moreover, he suggested identifying the SSP of selected "indicator" events (less than Q_2 in the partial duration series) that perform well in discriminating between stable and unstable reaches.

Dr. Bledsoe recommended superimposing partial duration series frequency curves to see which range of flows are most magnified and then examining these as potential indicator events. Discriminating stable and unstable sites with the less than 2yr events may be reflecting more what happens as a consequence of incision if the predevelopment scenario has some semblance of a floodplain. He felt that one often gets a much better picture of hydrologic impacts by using a partial duration (peak over threshold) series and selecting key event magnitudes from that. The effects of urbanization are generally less detectable as the return period increases. Dr. Bledsoe suggested that an intermediate alternative might be to examine the effective work index for flows in the range from $x*Q_{1.5}$ to $Q_{1.5}$, where x is a fraction like 0.5.

Specific Comments and Questions

- Dr. Bledsoe was glad to see vegetation rooting depth to bank height ratio mentioned. This descriptor has really helped his group recently in explaining variability in channel widths observed in the field.
- How close are the banks to critical height in the various study segments?
- How were the flow bins set up?
- Were the critical values of erosion based on motion of some fine gravel size?
- Bagnold's approach for calculating critical specific stream power was recommended over using critical velocity * critical shear.

- MacRae's equation uses the integral of the post-development condition minus the predevelopment condition.
- Are the critical shear stresses in Table 5-1 from studies of uniform material? A critical shear stress value of 0.047 might be high for even uniform fine gravel.

<u>HMP Report (June 2004), plus the SCVWD Technical Requirements and Geographical Areas for Hydromodification Management, Chapters 3, 4, 5 and 9</u>

Dr. Bledsoe's initial comments were to commend the project participants for developing a fundamentally solid strategy for managing hydromodification. He feels that the method is based on the correct physical processes without becoming overly complex and cumbersome, and reflects the best available science to date.

Dr. Bledsoe believes that the method correctly and necessarily relies on continuous simulation to quantify the duration of erosive flows and geomorphic work done exceeding the threshold for erosion of the boundary materials. Dr. Bledsoe commented that this approach is essential for adequately managing hydromodification.

Dr. Bledsoe felt that although the method is based on a sound hydrologic and geomorphic understanding, there are still a number of details that need to be worked out to ensure physically reasonable and consistent parameterization of the models being used.

These include the following for example:

- Selecting correct model input parameters
- Selecting representative field samples
- Selecting the in-stream solution approach without proper geomorphic considerations

Dr. Bledsoe commented that clear guidance needs to be provided on selecting parameter values and conducting HMP studies. Given such needs, Dr. Bledsoe strongly supports the development of a model akin to the Western Washington model (WWHM) to provide consistency and reliability in analysis and results.

Dr. Bledsoe highlighted the importance of monitoring the effectiveness of this plan, and noted that performance criteria may require adjustment and updating as more experience is gained. He pointed out that the method does not account for changes in sediment supply for example, and that perfectly matching the pre-development flow regime does not unsure channel stability. Monitoring is critical for assessing the adequacy of the management strategies and for improving the robustness of the method.

Dr. Bledsoe also pointed out that there must be consideration of larger scale impacts arising from changed sediment supplies and headcut migration into upstream reaches that were adequately protected. In other words, allowing some degree of instability in lower reaches could have geomorphic implications that extend beyond a single destabilizing reach.

For these reasons, Dr. Bledsoe recommends continual monitoring and analysis. He also suggests these issues make compelling arguments for selecting a low "risk" of instability from the logistic regression curve.

Finally, Dr. Bledsoe provides a number of comments and raises questions relating to specific details of the equations, figures, and wording. These were addressed in the report as appropriate.

Dr. Tom Dunne

Literature Review

Dr. Dunne felt that although the report was supposed to be a literature review, there was no summary of the literature. He found the assertions largely undefended and did not find any quantitative, predictive capability that could be utilized as the basis for plans. Dr. Dunne was also not clear on what audience the literature review was intended for and was not sure how well the report would succeed in raising the knowledge level of local professionals. Nevertheless, in his opinion the review convincingly showed that GeoSyntec had thoroughly informed themselves about the current knowledge. The report showed that there is a lot of activity in watershed hydrology and geomorphology concerned with some ill-defined aspects of 'habitat' and that some of the activity is concerned with semi-arid landscapes. Dr. Dunne felt that the diagrammatic conceptual model needed clarification, documentation and to be incorporated into the report. He also felt that the report was rambling, repetitious and often went into a level of detail and speculation which invited unnecessary dispute and had the potential to get readers off-track.

Dr. Dunne felt that the literature review characterized the important physical processes in wildland and urbanized watersheds. He found the information on infiltration, percolation, slope stability, and related processes underlying watershed response useful as background information. However, he mentioned that these measurements do not scale up to the watershed and the knowledge is not often used to predict watershed runoff, erosion, channel response or habitat condition.

Specifically, Dr. Dunne recommended defining what hydromodification is likely to include in the context of the Santa Clara watershed. He also suggested emphasizing the spatial and temporal dimensions of the problem, noting the fact that urbanization spreads through a watershed over time, possibly avoiding areas. Dr. Dunne felt that information about the transformation of watersheds and how and when changes are likely to happen in the watershed would be useful for educating planners and managers.

Dr. Dunne also recommended elaborating on the relevance of physical characteristics for "habitat". Specifically he asked what is it that needs to be expressed quantitatively, explained, and predicted about the nature and functioning of a channel bed so that a biologist can take predictions of physical change and assess its biological significance.

Dr. Dunne noted that relatively few studies have been conducted in semi-arid environments and questioned the transferability of studies done in other environments to the Santa Clara Valley. He noted that the report did not supply any guidance on how to apply studies/classification systems developed for other environments to the Santa Clara Valley. The idea that many of the processes documented in these studies are computable by models that can be calibrated needed to be emphasized in the report. Otherwise each study appears to be confined to the area in which it was conducted. A model based analysis allows the results from these disparate environments to be transferred to semi-arid watersheds. However, Dr. Dunne still thought the importance of the semi-arid climate in assessing the effects of urbanization in the Santa Clara Valley needed to be explained.

Dr. Dunne thought concluding with a review of risk-based approaches was wise. However he felt that instead of portraying it as a way around the problem of parameter uncertainty, it is a way of avoiding the issue of effective discharge. Dr. Dunne noted that stochastic approaches to prediction are also necessary for integrating over the entire probability distribution of rainfall events and antecedent conditions.

Dr. Dunne felt that more use could have been made of the literature concerning agricultural and wildland environments. The forestry and rangeland management literature contains a lot of basic information on runoff and erosion processes in semi-arid environments including mountainous watersheds. Dr. Dunne recommended a few chapters and papers, including Branson (1981) on urbanization effects on runoff, Trimble (1997) on channel erosion in the suburbs of San Diego, and Haible (1980) on channel changes along a small stream in the Bay Area.

<u>Draft HMP Report (July 2003)</u>

General Comments

Dr. Dunne's overall impression of the stability assessment was positive. He thought the report would be strengthened and clarified if the purpose of the computer modeling and graphics was described, along with examples of potential uses beyond occasional references to "planning" and "design". Dr. Dunne thought the presentation was logical for an insider, but probably would lose many a non-technical reader in its sequential, incremental style. Dr. Dunne suggested telling the readers upfront what needs to be calculated at the planning and design stages, and then briefly telling how the results are going to be provided.

For the methodology, Dr. Dunne questioned how much field data collection would be necessary for model calibration, but presumed it was related to how precisely the answers needed to be known at the design stage. He also felt that the intended use of the methodology for the planning and design stages needed to be explicitly stated.

Dr. Dunne was also unsure whether the report was intended to be a general methodology or a restoration plan for particular creeks. He wanted to know why the report was so focused on geomorphologic concepts developed for alluvial rivers, and not the rivers depicted in the Santa

Clara Valley. Dr. Dunne also asked what the underlying strategy for the restoration was. He assumed that the restoration plan was not to revert to the original channels which petered out somewhere in the valley floor, instead he assumed the plan was to design channels that would stop incising and would be in balance with the new sediment loads and flow spectrum. If so, the fact that options for reducing watershed runoff, the sediment budget, channel form roughness to take out some of the flow energy, etc, are not in the correlations that the stability assessment makes between accumulated work and the indicators of channel instability. Dr. Dunne noted that the stability assessment, although it uses the vocabulary of alluvial geomorphology is more relevant to the incisional (usually bedrock) parts of drainage basins. Extension to designing alluvial channel regimes requires additional thought and Dr. Dunne thinks further work needs to be done at the design stage to make the E_p useful.

Specific Comments

In Section 5.3, Dr. Dunne recommended only stating the final equation for work. He indicated that the introduction of the first two equations would expose the report to diversionary criticism, and were really not equations for work. Dr. Dunne suggested calling them “indices of work” rather than “work”, if all three equations were used.

Dr. Dunne questioned the fact that HEC-2 was used, but backwater effects were not considered. Since HEC-2 is based on the step-backwater method this did not make sense. Dr. Dunne also questioned the computations using normal flow assumptions, and asked if there was a normal flow option in HEC-2.

Dr. Dunne was curious how the methodology could be used at the design stage. Without extensive calibration and adjustment of τ_c and v_c, it is easier to imagine using the methodology for planning purposed rather than design. He reiterated that the report did not lay out the broader strategy of how the assessment was going to be used in the restoration.

Dr. Dunne stressed that the original concept, from Wolman/Leopold/Andrews, that the flows which carry most of the sediment are responsible for shaping the channel referred to alluvial channels, rather than to rapidly eroding, incising channels. He stated that even in the original incarnation the mechanism behind the concept is essentially unknown. He added that transferring a poorly understood concept outside of the range of its empirical base is unwise. It was not clear to him that such a concept should “be used in the design of in-stream solutions” in the case under investigation.

Dr. Dunne did not think the cumulative curves were a very clear or helpful way to highlight the critical flows. Instead he felt the original figures 5-5 and 5-6 were better.

Dr. Dunne stated that the probability calculation was an important step and needed further explanation. Dr. Dunne also felt that the calibration of the rainfall-runoff model needed explaining, and it may be necessary to defend hydrologic parameter choices.

Dr. Dunne thought an expanded description was needed of how the assessment would be applied in various forms of analysis. As an example, he recommended running a set of calculations to illustrate how urbanization increases the frequency of moderate flows (the Booth effect), and how introducing runoff control in the urbanizing area would change the distribution of the erosion potential.

Report (June 2004), plus the SCVWD Technical Requirements and Geographical Areas for Hydromodification Management, Chapters 3, 4, 5 and 9

Dr. Dunne's initial comments were to refer to the approach as an attractive procedure that is broad and systematic, and one that improves the prediction capability of the effects of land surface alteration and hydrologic changes and its effects on channel networks.

Dr. Dunne commented that the power of this method come from the combination of systematic field survey's, computed assisted modeling, and capacity to efficiently examine alternative control strategies. The field surveys provide local calibration and model verification. The model provides reproducibility by various analysts, and the ability to evaluate cumulative watershed wide effects and solutions.

Dr. Dunne noted that the methodology is largely based on modeling and that more field verification is needed to develop a strong empirical base. He therefore strongly recommends continual monitoring to keep track of unforeseen changes.

Dr. Dunne commented on the logistic regression technique, referring to it as innovative and crucial for assessing risk of channel destabilization. He suggests that follow-up work be conducted to evaluate the sensitivity of the results to the uncertainties implicit in the method. He sites examples, such as selecting parameters in the flow models, and identifying and quantifying field conditions as potential process variables to be analyzed.

Dr. Dunne mentioned that there is some ambiguity in deciding whether the critical shear stress of velocity is used from the bed or banks. The text was modified in the report to make it clear.

Like Dr. Bledsoe, Dr. Dunne also commented on the fact that the method does not address the effects of changed sediment supplies. He acknowledged that the method necessarily avoids the need to develop sediment transport routing models, which he states are not adequate (sediment transport models) for watershed scale analyses. The *index* method was in fact developed so that sediment transport modeling would not be needed.

Dr. Dunne thought that it would be valuable to express more fully the likely consequences of future build-out and the impact to streams. So if the Ep is 1.2 and the chance of instabilities is 1 in 7 streams, what does this mean exactly? What type of change and impact do we expect to observe? How many miles of stream does this come too? Which ones?

Dr. Dunne concluded by saying that this method is a useful prediction tool for planning and design, but moreover, it has provided a valuable database of systematic channel surveys throughout each of the three test watersheds. These data provide valuable information to evaluate effectiveness and changes observed by future monitoring efforts. He strongly recommends a database be developed to keep track of this data.

Dr. Matt Kondolf

Literature Review

General Comments

Overall Dr. Kondolf thought the literature review was a good compiling of points from the literature relevant to the topic, but that the citations were relatively few. He suggested several additional sources. Dr. Kondolf noted that many statements in the literature review were not supported and stressed the need to support all statements with citations or arguments from earlier points.

Dr. Kondolf suggested a table summarizing the literature on urban changes in hydrology, as an efficient way to convey the range of experience reported in the literature and to make the point that most of the research has been in different climates and landscapes. A similar table for studies of channel changes in urbanized streams was also recommended.

Dr. Kondolf noted that some sections, such as bed mobility thresholds, were not directly linked back to the purpose of the literature review. He suggested a flow chart showing how some of the cited studies fit in, with a simple statement linking each section back to the problem.

Specific Comments

Dr. Kondolf noted that it is difficult to avoid increasing stormwater discharge duration if the rate is limited (as by detention).

Dr. Kondolf also advised defining interflow versus groundwater flow and providing a reference for the distinction between the terms. He recommended adopting the terminology of Dunne and Leopold (1978). Dr. Kondolf also suggested clarifying that the unconfined, water table aquifer is being referred to when discussing groundwater elevations relative to the stream. Also it may be necessary to distinguish between groundwater recharge to deep aquifers and water held in shallow soil and transpired or evaporated. Dr. Kondolf also made the point that many stream channels are perched, e.g. channels flowing across proximal reaches of alluvial fans, where the water table could be tens of feet deep a short distance away from the channel, but where fine-grained sediments clog the gravels directly under the channel, maintaining perennial flow.

Dr. Kondolf pointed out that urbanization can also increase drainage density by causing more channels to be cut, and that it may be helpful to emphasize the role of urbanization in converting subsurface flow to surface flow and implications for flashiness of runoff. Dr. Kondolf also recommended clarifying that an intense local rain will produce the highest peaks from small basins, while longer duration rains produce the big peaks from larger basins.

Dr. Kondolf asked for an explanation of why small floods are more affected by urbanization. He suggested a figure from Leopold (1968) relating percent urbanized and percent served by storm sewers to increased peak flow. The use of the term "drainage networks" as increasing the drainage density was unclear.

Dr. Kondolf also emphasized that not all channels were in equilibrium before human activities, referencing tectonics, climate change, periodic big floods etc, especially in semi-arid environments.

Dr. Kondolf thought the discussion of slope adjustment may be misleading to some readers, as spatial and temporal scales are not defined. He recommended Schumm's (1977) distinction of independent versus dependant variables at different temporal scales.

Dr. Kondolf recommended additional literature of Wolman and Gerson (1978) regarding relative importance of frequent versus infrequent events in semiarid environments, as well as Gar William's paper on bankfull discharge.

Dr. Kondolf did not think that dynamic equilibrium implied widths, depths and slopes as functions of discharge. Instead he thought the equilibrium channel idea may be better suited, as illustrated by Lane's stable channel balance. Dynamic equilibrium refers to landscape evolution, the idea that mountainous areas are like that because they are uplifting, and the erosion rates will tend to be greatest there. The idea was proposed by Hack (1960) as an alternative to the Davison notion that uplift occurs once and landform evolution occurs by gradually wearing down the uplifted blocks (with no further uplift).

Dr. Kondolf asked if "quasi-equilibrium" was a term of Booth's and if so it needed a citation. Also he recommended reminding the reader that these adjustments occur in alluvial channels, while bedrock (or otherwise constrained) channels may accommodate increases in Q without changing dimensions.

Dr. Kondolf noted that streambed material is not usually thought of as "soil". He also questioned the use of the Sacto County example. If is used, then he suggested using specific streams and how their reaches differ, how they have adjusted, and providing values of width in feet etc. The significance of alluvial streams having variable stratigraphy was also unclear. Dr. Kondolf was not convinced about the differences posited between permeable and impermeable soils; it seemed contradictory to Schumm's findings in Great Plains streams. Also he wasn't sure if Zenter would be a suitable authority on it.

Dr. Kondolf asked how general the statements about 40-70 percent of change in sediment storage were. If it was only for the Andrews (1982) study, it needed clarifying. If it was more general, support and citations needed to be provided for such generalizations.

Dr. Kondolf thought that the effects of large woody debris (LWD) were a separate and broader topic from stressed vegetation getting into the channel. He also noted that citations on LWD were needed.

Dr. Kondolf noted the need to link the classification system discussion back to the purpose of the literature review. He also mentioned that there have been lots of classification systems and it should be made clear that the report only discusses a few of them. Also, Phankuch may not be a channel classification. He mentioned that many restoration projects built based on the Rosgen classification system have failed.

Dr. Kondolf felt that the points on bed mobility thresholds need to be related back to the purpose of the literature review. He also recommended the review paper by Buffington and Montgomery (1997).

Dr. Kondolf also suggested critical citations for the discussion of equal mobility, including Parker et al. (1983), Parker and Klingman (1983), as well as more recent work by Peter Wilcock on the effects of grain mixtures on bed mobility such as Wilcock and McArdell (1993).

Dr. Kondolf thought the erosion thresholds might be useful for the SCVWD planning, but wondered how well the thresholds, developed elsewhere, would apply to the Santa Clara Valley, and what kind of input data would be needed to make them work. He asked if field data collected in the SF Bay would allow documentation of channel changes as a function of percent basin imperviousness. He asked what the data sources were for McRae's index. It was not clear to Dr. Kondolf that the index was such an advance, and he suggested that once the necessary data were collected a lot more could be done than the simple index.

<u>Tech Memo #1</u>

Overall, Dr. Kondolf felt that the memo correctly identified the salient variables to be considered and laid out a logical approach to identifying thresholds for bed and bank erosion. He also thought that the proposed development of the method on one test watershed and then application to two other watersheds in different physiographic regions of the basin made sense. He appreciated the recognition of the importance of geological and historical influences. However Dr. Kondolf was concerned about the feasibility of implementing the program over the entire Santa Clara Valley. He felt that completing a good field reconnaissance and historical study over the entire area, while absolutely essential, would be the equivalent of several masters theses and would require an investment.

With respect to cause and effect, Dr. Kondolf questioned the criteria for method selection. He emphasized that while the criteria were desirable, there is no *a prior* reason to assume that such an ideal method exists. Determining the causes of channel change is a complex scientific question, and it is easy to reach a conclusion based on the data available, which may be incomplete and thereby lead to mistaken conclusions. Dr. Kondolf warned that a highly standardized approach to predict when erosion will begin, based on GIS data and modeling and applied to all channels, may fail if it does not account for local effects which can determine whether a channel incises or not. He also noted the difficulty in finding good reference reaches, and that in order to determine suitability the reference reaches themselves may need to be studied well to understand their character and history, rather than simply assuming they are healthy because they appear unaffected by channel erosion.

Dr. Kondolf recommended three simultaneous distinct efforts for phasing the assessment: 1) walking the length of each major stream and all its tributaries, 2) collecting historical data, and 3) building the GIS necessary to determine the percent urbanized for each reach in the stream network. Based on the results of 1 and 3, and the partial results of 2, a more informed strategy for collecting additional data could be developed. Based on the application of these studies to the test watershed, conceptual models specific to the Santa Clara Valley could be developed to guide data collection elsewhere, but differences among drainages must be considered before extrapolating.

Dr. Kondolf stressed the importance of the walking reconnaissance. He noted that while it is a significant effort, it is critically important, and that conclusions cannot be drawn about why the bed and banks are eroding where they are, or about stability thresholds without knowing the locations of grade control structures, bank protection, bedrock/hardpan exposures, and type of bank material exposed.

Dr. Kondolf recommended that the historical data collection include evidence of past channel planform, channel bed elevations, channel bank conditions and riparian vegetation distribution, land-use conditions in catchments, and human alterations to the channel. He also suggested examining the historical photos multiple times, iteratively, as insights are gained from other lines of evidence such as the field reconnaissance, to guide further field and historical studies.

Dr. Kondolf thought the GIS analysis should provide a framework within which to prioritize data collection and to delineate areas long ago urbanized and channels transformed, areas in which channels have been recently affected and may continue to adjust, and areas in which such adjustments are just getting underway.

Dr. Kondolf expressed concern about the amount of field work and data analysis required and questioned whether it was feasible within the given deadlines. He also mentioned the considerable data requirements for the hydraulic modeling and stability assessment and stressed the danger in using models with widely spaced cross sections, which interpolate channel conditions between cross sections. Such models may miss the local features that influence channel response. In developing the method on one watershed, results can be compared to actual field conditions, allowing evaluation of model utility. Dr. Kondolf also brought up the issue of exactly how the tractive force equations are applied to already affected reaches, as the existing channel cross-section does not reflect the channel at initiation of incision. He asked if pre-incision channel geometry was inferred and if flows were routed through the inferred channel geometry.

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Specific Comments

Dr. Kondolf questioned how typical the selected cross sections were, and if results were skewed by using "problem sites" that may have more active erosion than elsewhere. He also recommended explaining how local field slopes were adjusted and questioned how using different slopes would affect the results. Dr. Kondolf also suggested that there may be a problem with the bed material sample size, citing Wentworth (1926) and Church (1987) who recommend bulk sample sizes based on the largest particle. He also recommended stating the method used for measuring bedload.

Dr. Kondolf found it difficult to consider the individual sites outside of their longitudinal context, and suggested including both a longitudinal profile and a table showing stationing upstream of confluences. He also questioned the longitudinal relationship between stable and unstable reaches, and recommended looking into what prevents incision from propagating upstream. Dr. Kondolf noted that in the case of regressive erosion, the channel could be affected even if the upstream catchment has not been hydrologicaly altered. He specifically mentioned Reach D, which is apparently stable. He questioned if this reach was protected by some grade control from regressive erosion and if it was passing increased sediment loads from upstream without becoming unstable? If this is the case, then he recommended further study in the reach because it may hold clues to controls on channel stability in the system.

Dr. Kondolf stressed the need for closer study of the areas in between the study reaches as well as surveying the entire longitudinal profile, capturing all the grade controls, not only bedrock outcrops, but also pipeline crossings or other such infrastructure, piles of concrete rubble, etc. He emphasized that these features are critical to understanding why certain reaches are incising and others are not. Dr. Kondolf also highlighted one key, unstated, assumption underlying the analysis is that the stability (or lack) at a given site can be explained wholly on the basis of upstream flow (and sediment loads). This may ignore important influences of past management activities at the site scale, propagation of incision upstream, etc.

Since increased flows from urbanization are the driver behind the channel changes, and thus the entire modeling exercise, Dr. Kondolf recommended calibrating the hydrologic model to actual hydrographs, not only to regional regression equations.

Dr. Kondolf suggested mentioning that Wolman and Miller (1960) developed the magnitude-frequency and effective flow concepts, and that Wolman and Gerson (1978) noted that while $Q_{1.5}$ might be useful in some climates, less frequent flows are relatively more influential in semi-arid climates.

Dr. Kondolf noted that the effective work concept of Wolman and Miller (1960) was developed primarily to determine which flows had the most effect over time and thus the most influence on channel form. In the HMP example, the lower flows always have the mode, with very long, extended tails towards the larger floods. Thus, there is no clearly apparent effective discharge in the examples shown.

Dr. Kondolf explained that for the purposes of Wolman and Miller (1960), the absolute magnitude of the actual stresses exerted did not really matter, it was only the relative importance of large versus small floods and that calculating total work over time is quite another thing. Since Dr. Kondolf was not familiar and had some reservations with the McRae approach, he suggested presenting the approach as experimental.

Given all the estimates, approximations, assumptions, and interpolations involved in modeling shear stresses in the channel over a wide range of flows and in study sites distributed over a wide area, Dr. Kondolf felt that it would be a miracle if the model actually worked to accurately predict shear stresses and resultant channel adjustments. He also noted that selecting the dimensionless critical shear stress from a range of 0.03 to 0.06 could introduce more variability than the change in flow regime. Moreover, he noted that using a threshold of mobilization approach can introduce further imprecision because sites near the threshold will be lumped into the stable or unstable category. He also mentioned that values from the ASCE manual are very general and selecting values for a given site in the study area could introduce comparable variability in results. On top of this, Dr. Kondolf highlighted the fact that runoff rates were produced by an uncalibrated model; routed using the Manning equation with the associated uncertainty in the selection of ‘n’.

Dr. Kondolf recommended presenting the model in a way that emphasizes that the testing will not only be to determine whether its predictions of instability correspond to actual areas of instability but also to determine if the predictions of runoff, stage, and resulting shear stresses correspond well with actual values. Dr. Kondolf noted that the model is deterministic with considerable uncertainty at each step of the long complicated process from infiltration and runoff to channel incision. He stressed that the uncertainty was not currently quantified at each step, nor carried forward in the analysis and that if it was the results would be sobering.

The report indicated that the differences among sites would mostly be a question of slope and widths and Dr. Kondolf expected this to show the most effects in steep and narrow reaches that will have greater flow depths for a given flow.

Dr. Kondolf questioned whether the model predictions would be any better than the predictions by Booth of erosion resulting from percent urbanized. He realized that a model is needed to model the effects of increased runoff from urbanization as well as the potential benefits of detention basins and other stormwater management practices, and that there is a need to predict variations in channel impacts spatially and temporally. However, Dr. Kondolf recommended taking a more skeptical, experimental tone in the presentation and if possible quantifying the uncertainties and carrying them forward in the analysis.

Craig MacRae

Mr. MacRae provided guidance on field data collection. He indicated that to predict the impact of urbanization on channel form, only the riffle data may be required to use the Enlargement and Relaxation Curve approach, which shows how big the channel will get in response to a disturbance in the sediment-runoff regime. However both pool and riffle data are necessary if the objective is to determine what is going on in the channel today and how it will respond to a disturbance within a specific length of channel and what its ultimate form will be.

With regard to cross section location, Mr. MacRae recommended a feature based approach to facilitate data interpretation, stream comparisons, and easy of repeating site measurements. In addition, measuring features avoids the problem of changes in channel form. Mr. MacRae emphasized the key to a feature based approach is consistency in the protocol used for site selection.

Mr. MacRae pointed out that he analyzes pools and riffles separately, due to the differences in shape and the magnitude and distribution of forces applied to the boundary. He also noted that it is difficult to determine the magnitude and distribution of the excess boundary shear stress on the banks through a pool section, and more difficult to interpret the physical meaning of these forces on channel form unless the forces can be calibrated to lateral migration rates derived from historic surveys or aerial photography. If the latter data is not available, Mr. MacRae recommended using empirical models to estimate migration rates, although the confidence level declines. He noted that the problem may be minimized by applying parallel lines of investigation, such as balance of force models, Enlargement Curve assessments, Rapid Geomorphic Assessment, sediment mass balance assessment, and Brookes specific stream power methods. Confidence in the findings can be gained if the independent means of assessment converge on a unique solution. If they don’t converge then the interpretation of the processes operating within the system needs to be revisited.

Mr. MacRae also outlined his methods for determining critical shear stress for bank and bed material. To determine critical shear stress for bank material, bank stratigraphy units are mapped and soil consistence tests are applied to each stratigraphic unit where fine grained sediments are found. Soil samples are collected from the strata located with a zone at about one third the bankfull depth and tested for Atterberg Limits and percent silt and clay. Vane shear stress tests are also completed on all strata from the bank toe to approximately ½ bankfull depth. The soils consistence tests are quantified to determine a SCORE value, which is the sum of the values for stickiness, plasticity and firmness. The SCORE value is entered into an empirical relation to determine critical shear stress. The vane stress strength meter provides a direct measurement of the critical shear stress. The particle size data together with the Plastic Limit value from the Atterberg Limits tests are used in a multi-variate relationship to predict critical shear stress. The soils are also classified and compared to values published in the literature. Root diameter, density, and distribution are also mapped by stratigraphic unit and a root binding correction factor is applied to estimates of critical shear stress where applicable. Mr. MacRae noted that each of these approaches has strengths and weaknesses and the appropriate value is usually determined by comparing the value of excess boundary shear stress for all stratigraphic units with stream stability and mode-of-response findings generated from the Rapid Geomorphic Assessment as well as Brookes specific stream power approach.

The bed materials are sampled using the pebble count approach if the bed is covered by coarse alluvium. Where the channel is composed of fine grained materials a bed sample is collected and submitted for particle size and possible Atterberg Limits. The critical shear stress for the bed materials is determined using a variety of methods, such as the modified Shields method, various empirical relations and values reported in the literature. For bedrock channels the materials are classified and literature values are used to estimate the critical shear stress. Mr. MacRae recommended papers by Peter Allen and Keith Tinkler.

Mr. MacRae stressed that the estimation of critical shear stress is difficult, but that the selected values and resulting excess boundary shear stress estimates should reproduce observed channel form and process. For example, if a channel is degrading but not widening the bed should be sensitive to scour while the banks are not. Similarly, if the channel is widening but not degrading the banks should be sensitive to scour while the bed is not. Mr. MacRae noted that through the use of this kind of logic, the excess boundary shear stress can be calibrated using observed channel form and mode-of-response.

Dr. Ted Hrodmadka

Dr. Hrodmadka provided feedback on practical methodology for the hydrology-hydraulics-stream stability analysis. In his opinion, the major underpinning of using a continuous simulation approach was using a constant AMC, despite the fact that knowledge of the soil moisture conditions is very important for the most frequent and smaller events. He added that determining the “right” AMC would be difficult and lead to a biased model, either over or under predicting or runoff quantities.

Dr. Hrodmadka indicated that Multiple Decision Event approaches were useful in identifying what threshold of return frequency event impacts a particular watercourse. Using the above information, one would only then consider discrete events in excess of the threshold design event storm.

Dr. Hrodmadka mentioned that while both the continuous simulation and multiple discrete event approaches would involve statistical inference to estimate rare event response, each approach would probably produce different conclusions.

Dr. Hrodmadka pointed out that depending on the treatment type envisaged for a watercourse, the post treatment response to any of the hydrology methods may be moot: only the most severe test is relevant. Therefore, he felt that the selection of the hydrology approach may be primarily useful for determining where and when treatment is necessary and if so, a single event test, of a rare nature, may be sufficient. He stressed that passing the rare single event test is also necessary, regardless; and therefore, the design rare event should be applied uniformly everywhere.

Dr. Hrodmadka suggested two approaches; (1) a single event test, with response adjusted by a safety factor to simulate the effects of a continuous series of events over time, (2) a multiple discrete event test, with the results synthesized by a summation of weighting factors. Also for each discrete event, an average AMC representing those types of storms would be used. He pointed out the advantage of using actual point estimates was not having to use statistics to leverage off of historical data limited by the record length and period. In addition, decisions being made are perhaps more consistent with the flood control hydrology manual that the Agency would perhaps still be measured against in the event something were to go wrong.