Review of Draft Volume Depletion Approach Study

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Introduction

My background is in forest hydrology, catchment rainfall-runoff processes and streamflow transit time calculation. I provide feedback in this review as my expertise allows. While I now work outside of the USA, I spent some years working on streams near the study region, namely those draining the Elk River watershed that drain into Humboldt Bay south of Eureka (Sayama et al., 2011). I will make reference to that study in my review comments and suggestions below.

At the outset, I should state that compared to other scientific reviews that I have conducted, the request for external review instructions were rather obtuse and difficult to follow (for me). The paragraphs sent to me to respond to in Attachment 1 of the October 16, 2013 memo include passages like this: "*The volume depletion approach study evaluates the protectiveness of this policy section in regards to minimum bypass flow, and may recommend further conditions, including those on bypass, for use in the alternate criteria. This conclusion will be revised prior to peer review to include the conclusion of the study regarding minimum bypass flow: that is either supporting the requirements of Policy section A.1.8.3 provided; or proposing modifications to the alternative criteria in Policy section A.1.8.3; or documenting that A.1.8.3 is not protective of fisheries in regards to minimum bypass flow*". The request is replete with such text—perhaps text for insiders that makes sense but to an outsider or external reviewer, reads as rather confused and impenetrable and thwarts a clear, transparent discussion of the issue(s) at hand. Plain language works in journal article peer review. Clear, less jargon-filled language would aid outside transparent review of results.

So, as I understand my charge, it is to determine if the conclusions vis-à-vis the 3 diversion/bypass proposals as summarized on pages 6-14 and 6-15 "are based on sound scientific principles". There was further, a so-called Big Picture request, to (a) "provide any additional scientific conclusions that are part of the scientific basis for the proposed rule not described in the document", and (b) "to provide feedback on the scientific portion of the proposed rule based upon sounds scientific knowledge, methods and practices". Lastly, I was "encouraged to focus on feedback on the scientific conclusions that are relevant to the central regulatory elements being proposed". I frame my remarks below as best that I can around these requests and my background as stated above.

Review feedback on "the soundness of scientific principles followed"

The overall report develops additional protective conditions necessary under A.1.8.3 for maximum cumulative volume depletions greater than 5% but no more than 10% of the seasonal unimpaired flow volume. In other words, what additional restrictions should be made to ensure that fisheries resources are minimally affected by diversions. The summary table 6-1 summarizes the findings on protectiveness for each stream class and level of maximum cumulative volume depletion. It states that for Class III streams that the A.1.8.3 guidelines are regionally protective for maximum cumulative volume depletions ranging from 1-10% and that no additional conditions are required. For Class II streams, the A.1.8.3 guidelines were not found to be protective across all cases. Table 6-1 summarizes the additional conditions that the State Water Board may consider in future policy revisions developed from the report.

Overall, the scientific approach, methods and data as presented in A.1.8.3 appear sound. The field data appear to have been collected using appropriate methods. The model work follows standard practice in a consultant report. The flow chart (Figure 6-6) and summary section 6.3 of the report outline a logical and sound framework for decisions for depletion and diversion based on the analyses completed and the model work executed. **Overall, I would say that the report is based on sound scientific principles.**

Notwithstanding this positive endorsement of the consultant's work, I do have some concerns related to the field work and modeling:

- The projections in the rating curves beyond the highest measured stage/discharge value are highly uncertain. Measurement uncertainty is high overall (as in any field-based effort like this). This should be addressed in the context of the diversion/bypass recommendations.
- Likewise, uncertainty in the model analysis is not discussed. How well identified were the parameters? How much uncertainty is there in the model output? Is this a big number or a small number? I suspect the former given the large number of parameters in HSPF. This should be addressed in the context of the diversion/bypass recommendations.

Such treatment of uncertainty seems essential to any policy document. Other uncertainties in our understanding of how the watershed systems work seems warranted here also. Which take me to my main feedback on this work: the big picture.

Review feedback on “the big picture”

Point 1

My concern with any study approach like this (using the model as predictive tool) is that it implies a false sense of understanding of how the system works. These watersheds systems are complex with strong non-linearities: thresholds, hysteresis etc. The model structure in something like HSPF does not parameterize some of the key controls on flow regime, for example the underlying geology. My first “big picture question” is “how do differences in geology/geomorphology across the study region influence flow regime”? In the Sayama et al (2011) study we found that this had a very large effect whereby streamflow response during the Fall and early winter “wetting up” was strongly controlled by storage properties set by topography and geomorphology/geology.

Figure 1 shows how watersheds in the Elk River system store differing amounts of Fall rainfall from the start of the wet-season (around Nov 1), similar to the Nov 1 to March 31 period examined in this study (but different years of course). The Y-axis is change in storage (dV) calculated from the watershed water balance. What is notable, is that the storage in these watersheds (whose area is shown in parentheses for each line, in km^2) vary from ~200-500 mm. How much water the watershed stores then influences the flow regime for rainfall inputs throughout the rest of the wet season (and later in the hydrologic year for that matter). Figure 2 shows a plot of two of these watersheds with different topography/geology: one watershed, the 6 km^2 number 533 watershed has very sensitive flow response to rainfall inputs; the other 3 km^2 watershed number 534 has a very different storage-discharge response.

Such storage-discharge characteristics have a first order control on flow regime—and the appropriateness I would think of potential withdrawls or diversions. In short, some watersheds are more sensitive than others. I would argue that such sensitivity is not captured in an HSPF analysis. I would therefore question how regionalization of model results across the study area (for policy implementation) could be accommodated within the context of the Elk River findings. I would encourage thinking on how the topographic and geomorphic/geological conditions across the study region affect watershed storage and flow regime. Important work has been done elsewhere (e.g. Malcolm et al., 2003; 2005) that links such groundwater-surface water interactions (mediated through storage) to salmonid egg survival etc.

More specific to the report findings and recommendations, I would question the logic of Scenario A1 where adding a diversion season policy element “*protects sensitive passage and spawning in October and November and shifts the impacts to December and January when there are substantially more days with flow conditions conducive to* *each habitat need such that reductions in flow due to diversions have less biological significance with respect to protecting diversity in life history strategies*".

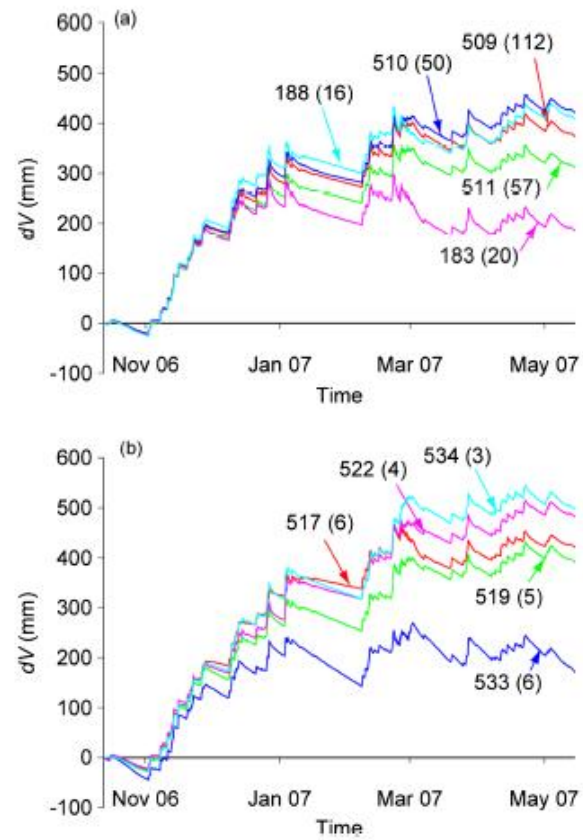

Figure 1. Storage change (dV) increases for 10 watersheds in the Elk River watershed, CA through the wet season (from Sayama et al., 2011).

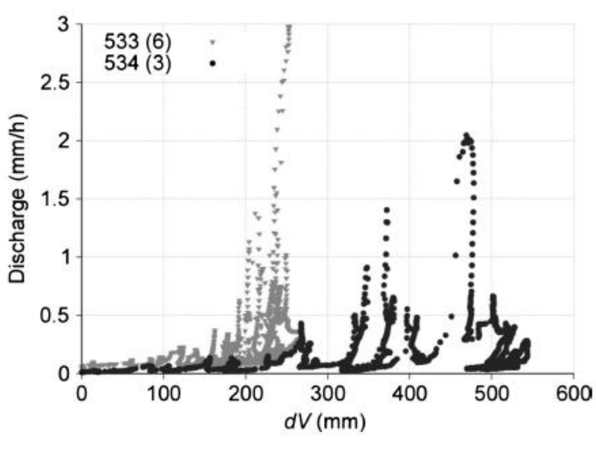

Figure 2. Discharge dynamics for 2 watersheds in the Elk River watershed, CA (from Sayama et al., 2011) showing very different flow dynamics.

Figure 2 suggests to me that the runoff behavior across watersheds in the study region is highly variable—some store a lot of water and release it in small bursts and others store much less water and release it in larger, more frequent flushes. Again, this is controlled by the watershed storage characteristic—characterization of which is beyond the scope of HSPF. The Elk River work showed that median watershed slope gradient explained over 70% of the variance of watershed storage. That is, the steeper the watershed, the more storage there was due to the permeable nature of the underlying geology. One last example to perhaps further drive home this point is the very important work by Tague and Grant (2004) in the Oregon Cascades (admittedly, different geology to the study area of interest, but the point is still instructive) that shows just how important such geological controls are also for summer lowflow. In fact, they were able to explain >75% of the variance of summer low flow, or more precisely the log of mean August streamflow flow (Figure 3) with a geology map: no model necessary. I encourage the group to step back from the gory details and precision implied with the current approaches and consider the power of such analysis.

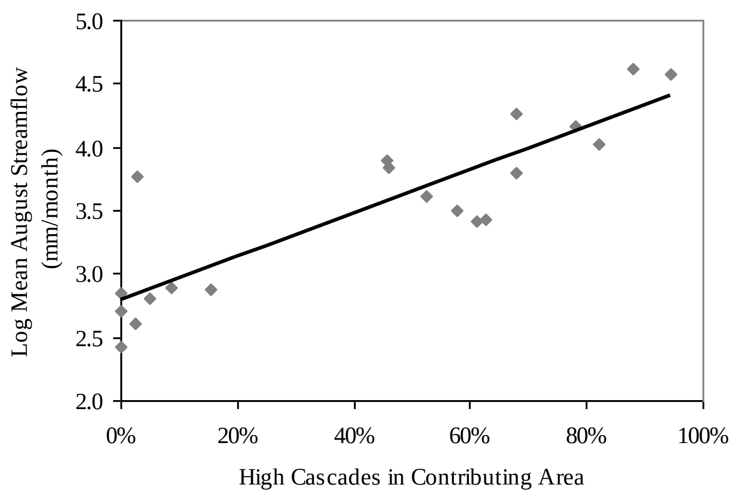

Figure 3. Work of Tague and Grant (2004) showing the importance of geology for defining summer low flow conditions in Oregon.

<u>Point #2</u>

A second “big picture question” relates to the Class III stream insensitivity assumption and how much watershed area is necessary to sustain perennial flow in the area. How variable is this? Class III watersheds often exhibit very high variations in flow (high and low flow) where until one scales up to larger watershed scales—where the variability in watershed characteristics (soil depth, slope angle, geomorphic zones) become representative of the continuum—flow-per-unit-area variance is very high. Woods et al. (1995) have made measurements of these so-called representative elementary area effects in similar systems in New Zealand. I would recommend that such measurements be made in the study region to again probe some questions of how these systems work (in a way that complements the measurement/modeling approach) but gets at basic behaviors of small Class III streams where variability is inherently high. I would say that a plot like this for sites throughout the study counties could be very helpful for seeing big picture controls on flow regime (ideally done at different flow conditions throughout the hydrological year).

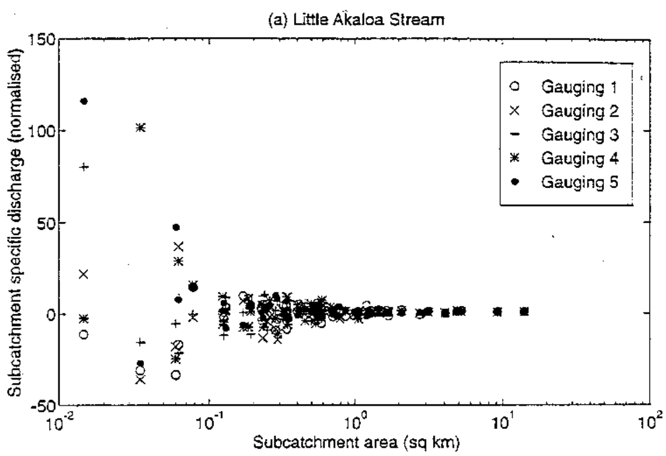

Figure 4. Work of Woods et al (1995) from streams draining watersheds on the South Island of New Zealand. High variability in flow per unit area (y-axis) for watershed areas less than 10^{-1} to 10^{0} km^2.

Conclusions

My overall assessment is that the scientific approach as followed in the report is solid and that the methods and data provided appear sound. The Policy section A.1.8.3 and its summary in section 6.3 and distillation in Figure 6-6 outline a logical and sound framework for decisions for depletion and diversion based on the analyses completed using this standard approach. My main critique of the methods as followed was that uncertainty (in the field data, especially the rating curves and the model output) was not examined.

For me, the big picture questions are the important ones to consider going forward. As a complex system, these watersheds may be easy to mimic in a model like HSPF but that such work can lead to a false sense of understanding of how such systems operate and might respond to changes in climate and landuse in the coming decades. I would encourage the powers that be to engage with their local university counterparts and fund studies of system functioning. There are many papers out there from the study region already and that the current report does not cite (e.g. Dhakal and Sullivan, 2012). I see that there is work being done at Humboldt State University (e.g. Huggett, 2012) using a model better suited to capturing these subsurface storage issues (the DHSVM model). I would encourage use of a model like that, going forward, where the subsurface can be described in a way that better captures its influence on flow at the catchment scale and how spatial differences in soil depth, bedrock permeability etc can be incorporated, as noted in my big picture comments.

References

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