



USDA  
Forest Service  
Pacific Southwest  
Research Station

Redwood Sciences Lab.  
1700 Bayview Dr  
Arcata, CA 95521

Tel: (707) 825-2900  
Fax: (707) 825-2901



August 20, 1999

Ed Salminen  
Hydrology Team Leader  
4491 Woodworth Drive  
Parkdale, OR 97041

Dear Mr. Salminen:

Thank you for requesting that we review the Hydrologic Change Module (HCM) for PALCO lands dated August 12, 1999, posted at <http://w3.gorge.net/salminen/hydro/0812meth.doc>. In general, it appears to be a well-considered and reasonable approach to estimating changes in peak storm runoff from forest harvesting. Our comments that follow are not comprehensive, but focus primarily on Sections 3.0 and 4.4, which are largely based on research at Caspar Creek that Bob Ziemer and Jack Lewis have authored.

The assumptions in Section 3.0 seem appropriate except items 9 and 10, which deal with the effects of partial cutting. In interpreting the results of statistical hypothesis tests, it is important to remember that non-detection of an effect is *not* equivalent to the detection of no effect. Acceptance of the null hypothesis of no effect is only appropriate if it has been determined (by statistical power analysis) that the probability of the test statistic falling in the acceptance range is adequately small under an alternative hypothesis representing minimal (hydrologically unimportant) change. It is not recommended practice to declare that a hydrologically significant difference exists when it has not been found to be statistically significant. However, if an estimate of the size of an effect is needed, it is equally inappropriate to use zero when an understanding of the processes imply there is a non-zero effect, and data are available to estimate its size. The confidence interval around that estimate may include zero, but zero is probably not the best estimate.

The first assumption in Section 3.0 states that the primary mechanisms by which forest harvest on PALCO lands may cause significant effects on peak flow magnitudes is through reduced ET and interception. Since these processes are largely controlled by leaf area, and leaf area should be highly correlated to canopy cover, any reduction in canopy cover would be expected to reduce both ET and interception.

The following data are from Table 2 in Ziemer (1981). NPFLOW is the peak flow in the North Fork (control), and PFLAWSFT is the relative difference in peak flows, SPFLOW/NPFLOW - 1. We have replaced standard deviations with standard errors of the mean and added columns 2, 3, and 8-11.

NPFLOW (m <sup>3</sup> s <sup>-1</sup> )	Midpoint (m <sup>3</sup> s <sup>-1</sup> ha <sup>-1</sup> )	T <sub>r</sub> at midpoint	PFLAWSFT 1963-1971		PFLAWSFT 1972-1975		% incr in South Fk	NF60% clearcut	NF50% clearcut	NF40% clearcut
			Mean	s.e.	Mean	s.e.	Mean (range)	% incr	% incr	% incr
0.08-0.33	0.00043	0.099	0.420	0.071	1.491	0.241	76 (51,102)	49	39	30
0.34-0.78	0.0012	0.14	0.333	0.071	0.716	0.241	29 (5,55)	41	33	26
0.79-1.97	0.0029	0.28	0.205	0.055	0.263	0.116	5 (-9,20)	34	27	21
1.98-8.50	0.011	5.1	0.054	0.039	0.152	0.071	9 (-1,20)	15	13	10

Recurrence intervals at the midpoint of each class were calculated using equations (4) and (5) of the HCM. The percent increase in the South Fork was estimated as  $100 * ((\text{PFLOWSFT}_{1972-1975+1}) / (\text{PFLOWSFT}_{1963-1971+1}) - 1)$ . The “range” indicates the possible outcomes of this calculation for the means of PFLOWSFT +/- one standard error. For comparison, the predictions of the peaks model (Lewis et al., in press) for 40-60% clearcut in the North Fork are shown for “average wetness” 2.5 years after logging. The spreadsheet equation provided with the HCM was modified for this purpose. These last 3 columns of the North Fork results were included because removal of 60% of the volume in the South Fork is likely to have left a canopy coverage somewhat greater than 40%. While the percent increases in the South Fork do not agree precisely with the predictions in any of the last 3 columns, the ranges based on +/- one standard error do include all the predictions for the second and fourth classes. It would seem reasonable (and not in conflict with the data) to assume that peak flows respond to partial cuts with  $p\%$  canopy reduction similarly as they would respond to areas in which  $p\%$  was clearcut. Such a model is consistent with the first assumption in Section 3.0 and agrees with the data better than a model in which peak flows are entirely unaffected by partial cuts with less than 50% canopy reduction.

Some sort of indication as to the magnitude of uncertainty in the predictions should be included in the Module. Because much of the uncertainty is due to variability in antecedent wetness, this has been partly addressed by considering both “average” and extreme minimum wetness conditions. This could be amplified by also presenting predictions for relatively wet conditions, e.g. for the 90<sup>th</sup> percentile of the 59 wetness values shown in Figure 5. But there is additional unexplained variability even when the wetness index is known. An indication of the magnitude of this uncertainty is given by the prediction intervals shown in the attached figure (from Lewis et al., in press). We would not object to inclusion of that figure in some form in the HCM.

Table 1 seems incorrect. Should not the entries for row G be intermediate in size since they are weighted averages of values from the polygons contained within HAU G? The same comment applies to HAU E and F.

We did not attempt to check every term in the spreadsheet formula (in 0812modl.xls), but it does seem to work correctly for the examples we tried. If you implement our suggestion regarding partial cuts, you will need to include the cutarea variable ( $c$ ) in the formula as a surrogate for canopy reduction. We would have found the formulas easier to work with if the absolute reference to return period was made into a relative reference, so when the formula is copied to a new row, it would use the return period in the new row rather than the return period in row 4.

Now, for some detailed comments on the August 12 draft (page and line numbers are based on the document posted at <http://w3.gorge.net/salminen/hydro/0812meth.doc>).

Page 2 Lines 25-28: We suggest the following rewording “Loss of canopy interception was also hypothesized to be responsible for differences in soil moisture conditions between logged and unlogged sub-watersheds throughout the storm season, as well as having a significant effect on peak flows because of increases in throughfall during storm events (Ziemer 1998).

Page 3 Line 3-5: Based on the points raised above, this should be revised to read something like “We might expect that the reduction in canopy interception and evapotranspiration associated with partial (selective) cutting would also result in increased peak flows. A study of partial harvest that removed 60% of the stand volume in the South Fork Caspar Creek was unable to detect a statistically significant increase in peak flows that occur on average 8 times per year (Ziemer 1981; Wright et al. 1990; Ziemer 1998). In interpreting the results of statistical hypothesis tests, it is important to

remember that non-detection of an effect is *not* equivalent to the detection of no effect. The reason for the lack of statistical significance is often because of high variance. In the South Fork, the reported differences in peak flow were numerically greater in all flow classes after logging.”

P3L13: The discussion of ROS events introduces unnecessary confusion. It should be clearly stated that a ROS event is a meteorologic process that can result in large streamflow peaks and floods, independent of land use. Since such events are rare and the hydrologic details poorly documented, it is difficult to separate the land use effect from the meteorologic event. The 1964 flood in California was clearly exacerbated by a ROS event. What is less clear is the influence of forest cutting on a flood of this magnitude. [Work by Pitlick (1994) suggests that the magnitude of floods in drainages greater than 10 km<sup>2</sup> can be predicted well using only drainage area and precipitation, while slope, drainage density, and percent forest cover did not significantly improve the relationship.] Consequently, although lines 22-24 are correct in stating ROS played a role in the 1964 flood, this does not necessarily mean that land use played a substantial role in either the 1964 flood or in the ROS contribution of that flood. The confusion is introduced by using the term “ROS effect” to mean both the hydrologic response to ROS *and* the land use relationship between ROS and runoff. For example, on Page 4 Line 2 the statement “it is unlikely that ROS effects will affect peak flows on PALCO lands” is probably incorrect if it refers to the hydrologic response to a ROS meteorologic event (ROS events most certainly do affect peak flows on PALCO land), but is probably correct if it refers to the incremental influence of tree cutting on the ROS response in this area.

P4L8-11: Roads also can capture water “intended” for one drainage and route it to another drainage. This can reduce streamflow in one channel and increase it in another. Such capture is most important to the stability of channels in the smallest drainages. The effect becomes muted as drainage area increases. Consequently, if the area considered is large, this process is often overlooked and not detected by streamgages located in a large watershed below where the two small tributaries merge.

P4L26: There is a serious typo here. The sentence should read “In one headwater swale at Caspar Creek Ziemer (1992) reported that peak pipeflow was 3.7 times greater *after logging than* prior to logging.

P11L20-22: Restate this point to make it clear that this is discussing the effect of harvest on ROS, not the importance of ROS meteorologic events in generating large peaks. (see discussion above for P3L13)

P12L17 and L25: The two numbers are not consistent. Line 17 states 12 years; Line 25 states 15 years.

P12L23-29: Revise based on the points raised in the first two pages of this review. To summarize, numeric increases were reported by Ziemer (1981) in all size classes, but they were not statistically significant. To conclude that there was *no* increase would be incorrect without a power analysis, which was not done. A better approach would be to use point estimates from Ziemer (1981, Table 2) as a guide for modeling the effect.

P13L1-10: Revise based on the points raised in the first two pages of this review. Assumption 1 (Page 11 Lines 14-19) would imply that the line in the figure should go from (0%, 1) to (100%, 0).

P18L18: What is the variable “f” in the equation?

P21L7-9: The Lewis et al. paper has now received technical review and the methodology and conclusions did not require changes.

P21L10-15: Delete this paragraph based on the points raised in the first two pages of this review.

P22L16: The watershed is HEN not HIM.

P23L4: In the statement “is equal to a  $(X - X_j)$ ” it is not clear that “a” is a variable.

P23L11-12. MLE estimates are not computed from the mean and standard deviation, but from the whole data set.

P24L1: The watershed is HEN not HIM.

P24L6: “Recurrence intervals were determined for the 59 storm events from the HI control watershed.” – using equations 5 and 6?

P24L17: “ ... using values associated with the annual series” – using equations 4 and 6?

P24L19-21: You might also report predictions in a relatively wet condition, e.g.  $w_{90} = 90^{\text{th}}$  percentile of wetness data (~400). This would give a better indication of the range of likely effects.

P30L21: Why is G the highest? It should be intermediate since it is a weighted average of the polygons that it contains. Check also the values for E and F.

We hope that you will find our review comments and suggestions useful.

Sincerely,

Jack Lewis  
*Mathematical-Statistician*

Robert R. Ziemer  
*Chief Research Hydrologist*

Enclosure: 1 figure

cc: Ray Rice  
Matt O'Connor  
Pete Cafferata, CDF  
Sharon Kramer, NMFS  
Chris Heppe, EPA  
Dave Parson, SWRCB

