

REPLY TO DISCUSSION
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“EROSION ON LOGGING ROADS IN REDWOOD CREEK, NORTHWESTERN CALIFORNIA”

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REPLY TO DISCUSSION

by Randy Klein, Vicky Ozaki, Greg Bundros, and Mary Ann Madej¹"Erosion on Logging Roads in Redwood Creek, Northwestern California"²Raymond M. Rice³

I hope that the questions raised by Klein, Ozaki, Bundros, and Madej will serve to alert the unwary reader to the limitations of "outdoor research." I was apparently not completely successful in my paper. I will try again.

They seem to believe that I was attributing all of the difference in erosion estimated between the earlier studies (in Copper Creek and Garret Creek) and my study to improved forest practices. That is not the case. In the last sentence of my "Introduction," after outlining some of the confounding factors affecting my study, I stated: "The reader will have to decide how much of the differences that will be reported should be attributed to differences in weather and how much to differences in road maintenance and construction practices." In the "Conclusions" I begin by saying: "The results of this investigation suggest that changes in forest practices have greatly reduced road-related erosion. . . ." The reader was free to accept or reject that suggestion.

The discussants claim that the storms of the 1980-1997 (and especially 1997) were inadequate to test the improved construction and maintenance practices. I find that peculiar, given that their own organizations funded a study of road-related erosion in Redwood National Park in response to the 1997 storm (Bloom, 1998) ". . . to see how rehabilitated roads behaved after being "tested" by a large flood." (Harris *et al.*, 1997). In my paper, I noted that the actual return periods of the 1995-1997 storms for the drainages tributary to each of the 100 plots are unknown. Also unknown is the erosional response of

the plots to different size storms. Therefore, the question is not "how big was the storm" but is "was it big enough to reveal weaknesses in the road system?" I contend that it was big enough to give useful insights into the importance of erosional mechanisms and the effectiveness of various mitigation measures.

Lastly, Klein *et al.*, questioned my use of simple random sampling and noted ". . . that the previous studies . . . (to which I compared my results) . . . measured the *entire population of erosion sites* . . ." I doubt that this is strictly true. Every field study must, as a practical matter, set limitations on what is measured. For example, no attempt was made in my study to estimate sheet erosion and Bloom (1998) only tallied features greater than 2.3 m³ (3 yd³). Such restrictions affect a study's results. Having a random sample, I can estimate that there is about a 4 percent probability that there is a site in my study area that produced more erosion than the largest site in my data (~2,400 m³ km⁻¹). However, the people working in the area do not believe such a site exists. On the other hand, had Bloom (1998) been measuring my study area, I predict she would have underestimated the total erosion by 37 percent because of not recording features displacing less than 2.3 m³. The choice of sampling criteria depends on study objectives. In my case, one of the objectives was an estimate of the contribution of even "minor" erosional mechanisms. The careful author needs to inform the reader of the sampling methods used and the careful reader must take the limitations of those methods into consideration when interpreting study results.

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