Streambank stability and cattle grazing in southwestern Montana: A viewpoint

Kenneth G. Renard

The JSWC *received the following unsolicited commentary on the research report "Streambank Stability and Cattle Grazing in Southwestern Montana" by Clayton B. Marlow, Thomas M. Pogacnki, and Shannon D. Quinsey [* JSWC *42(4): 291-296]. The commentary is followed by a response from the authors.*

The authors are to be commended for their attempt to treat a problem of considerable importance in the western United States, namely, the control of streambank erosion resulting from cattle grazing. Unfortunately, the way the authors designed their experiment and completed their analysis leaves much to be desired.

Kenneth G. Renard is a research hydraulic engineer, Aridland Watershed Management Research, Agricultural Research Service, U.S. Department of Agriculture, 2000 East Allen Road, Tucson, Arizona 85719.

1. The sketch in figure 1 is apparently of a conceptual nature rather than an attempt to faithfully reproduce a topographic map. Experience with natural streams would not lend credence to the smooth flow line indicated for Cottonwood Creek. This leads to the question: "Where were the cross-sections relative to bends in the stream?" Geomorphologists and river engineers have long treated problems of this nature, although admittedly often on larger discharge streams. Gregory's book (*1*) includes four chapters in the section titled "Channel

Journal of Soil and Water Conservation 43(2):204-206 www.swcs.org

Geometry Changes" and includes another chapter on the subject "Rates of Erosion on Meander Arcs" directly applicable to the current problem. Thus, the location of cross-section information (e.g., Figure 2) with respect to changes in flow direction may explain some of the differences the authors report in table 1. One can only assume that the cross-sections used to develop table 1 were free of the influences of meanders.

2. The authors report, "Flume/recorder units were positioned on the downstream boundary of the study area: at three locations along the creek in the grazed area and at the downstream boundary of the ungrazed paddock. Measurements were made during the study period each year and converted to centimeters for comparison with channel alterations." Furthermore, they state, "Recorders were mounted on H-type flumes. Each flume was equipped with a stage-height indicator calibrated in tenths of feet (1 ft/sec. maximum)."

Several conclusions can be made from these quotes:

a. The stage recorder does *not record* 1 foot/second maximum. At best, the H-type flume might have had a maximum recording capacity of 1 cubic foot/second.

b. The recorder operating to the nearest 0.1 foot implies tremendous error in the recording of discharge rate. Assuming a 1-foot-deep H-flume was used, the maximum error at peak capacity (± 0.05 foot) would be 1.92 cubic foot/second $\pm$ 11% (see p. 93 of reference 4). When such errors are projected to the data like that in table 3, the numbers are not significant to the level reported.

c. Inserting a 1-foot H-flume in a channel, such as is illustrated in figure 2, would severely upset the channel equilibrium. Furthermore, the H-flume invert that should have been above the normal stream bottom would create a hydraulic jump downstream and could conceivably affect the equilibrium of the downstream sections, at least for a distance until some new natural channel control was established at a point of a geologic outcrop, channel constriction associated with a log or tree roots, etc.

d. The heights of streamflow (cm) reported in table 3 are difficult to understand. How were they obtained? Do they represent the flow depth at some point along the stream, or did they represent runoff from the intervening area/paddock expressed as depth over an area? This latter interpretation is questionable because it would be extremely difficult to obtain without additional measurements or without the use of a physically based analytical model. The most useful data would have involved measuring the runoff from each paddock and then subtracting the base flow to report surface runoff per paddock.

e. The reporting of height of streamflow in centimeters at five locations along Cottonwood Creek is misleading at best and really not helpful, except that it indicates a decrease in streamflow in time for each of the 4 years. Discharge, Q, is more helpful (with units of L^3), and for an H-flume, Q (L^3/T) is proportional to Y^m, where the exponent (^{m}) of depth (Y) is a number larger than unity (see reference 4, pages 93-95).

3. Channel cross-section changes *should not* be expected because of the hydraulic forces acting on the stream boundary under normal flow conditions. Most changes due to hydraulic/hydrologic conditions occur primarily on storms having a frequency larger than the annual peak discharge. It seems likely that little sediment would be transported for the depths shown in table 3, assuming they represent depths recorded in a 1-foot H-flume. Thus, the cross-section changes reported in table 1 probably represent bank sloughing resulting from the cattle hoof action in descending to the stream. I would expect an accumulation of material near the stream bottom (material sloughed from banks), with subsequent removal downstream by larger discharges, such as those associated with the spring snowmelt or the occurrence of thunderstorm runoff during the study period.

Because the authors report the flow depth on a bimonthly basis (Table 3), it is difficult to know what the data in table 2 indicate. Tables 4.10 and 4.11 of *Sedimentation Engineering* (5) illustrate the role of a single storm in affecting the average annual sediment yield, which, in turn, is the sum of all erosion sources/types and sedimentation. A significant amount of the erosion may well be bank erosion.

4. The authors state (pages 293 and 294): "Although early grazed paddocks with short measurement intervals appeared to have more transects with significant changes, regression analysis showed little association between measurement interval and the number of transects with differences."

Again, this result should have been expected. As shown in table 2, most of the transects measured on the lower end (also early in summer) evidenced net erosion, whereas those later had deposition (those further upstream); this deposition probably resulted because there was insufficient flow to transport the upstream erosion as well as the erosion from bank sloughing in the paddock being grazed out of the experimental reach of Cottonwood Creek. Although not mentioned by the authors, one must assume the permanent transects were adequately referenced on each end so there was no frost heaval of the monuments.

5. Table 2 is difficult to interpret. The footnote states that each of the nine paddocks had five channel transects, yet only the early July 1982 and late June 1984 period report data for each transect. Why weren't the others reported? Furthermore, the table lists in column 2 the average measurement interval. Presumably, these represent the interval following some reference measurement in early May. Thus, it is impossible to tell whether the erosion in any paddock has erosion/deposition due to the hydraulic/hydrologic occurrences prior to the grazing period or whether the erosion/deposition phenomenon result from the specific paddock grazing period. For these reasons, items 1 and 2 above also become important.

6. It would be helpful if the authors had provided some indication regarding the general status of annual streamflow for the 4 years studied. Were the reported values in table 3 above, below, or average runoff years? Despite the confusing units used to report streamflow as mentioned earlier, the authors do report, "High levels of streamflow were significantly related to the amount of change in the channel profile 2 of the 4 years (Figure 4)."

Despite this evidence, the authors attempt to relate soil moisture to the observed channel cross-section changes.

They then state, "Each moisture level used in the correlation analysis was the average of only two observations, while channel change data points were the average of five transects" [again the illusive five transects and missing data in table 2]. They further state, "If streambank areas corresponding to each channel transect had been sampled for moisture conditions, a higher correlation between soil moisture and channel change may have been reported."

Although this may be true, one would intuitively expect a high correlation between channel soil moisture as well as upland soil moisture and channel flow. Such a correlation was not reported as being investigated by the authors.

7. The authors state in the conclusion, "The negative relationship between time and channel change reinforces the point that the magnitude and extent of change are a function of the season of grazing."

Although the statement of the negative relationship (it's not apparent where the statement comes from) between time and channel change may be true, the reason for the relationship is undoubtedly incorrect. The reason is, as the authors say in the text, "...it is possible streamflow dynamics may have played a role in bank erosion patterns" to which I add amen!

As a final observation, streambank stability is certainly a major problem with cattle grazing and access to the channel, but certainly not the only problem. Increased nutrient concentrations and bacterial pollution also result with grazing pattern and intensity. For example, Stephenson and Street (2) found that typical rangeland cattle operations, such as are encountered in southwestern Idaho, result in coliform bacterial pollution along various reaches of streams draining rangeland. Total coliform counts varied more with change in streamflow than did fecal coliform counts. In fenced, summer range allotments, under deferred grazing management, the effects were the same, except that bacterial counts were not as high or persistent. The reduction in bacterial concentrations at several downstream sampling sites indicated that certain stream segments were self-purifying. The presence or absence of livestock along the streams overshadows any effect variations in chemical concentrations of the water might have on bacterial concentrations.

In an extension of earlier work, Stephenson and Rychert (3) suggested that the elevated fecal coliform indicator counts observed in streams are mostly the result of a suspension of stream bottom sediment and organic matter, rather than from sources extraneous to the stream at the time of increased runoff. They suggested also that the organic matter content of the sediment may have a critical influence on the survival and/or multiplication of the bacteria. Their results showed *E.coli* concentrations of bottom sediments to be from 2 to 760 times greater than from the overlying water. For these reasons, and depending upon the ultimate use of runoff originating from rangelands, such considerations are critical to any water development plan.

Thus, it seems that a more viable alternative to water quality problems (sediment, chemical, and bacterial) might be to fence paddocks so that cattle cannot have direct access to a channel with intermittent or perennial flow and to develop watering systems that move runoff from the channel to upland sites.

REFERENCES CITED

1. Gregory, K. J., ed. 1977. *River channel changes.* John Wiley & Sons, New York, N.Y. 648 pp.
2. Stephenson, G. R., and L. V. Street. 1978. *Bacterial variations in streams from a southwest Idaho rangeland watershed.* J. Environ. Quality 7(1): 150-156.
3. Stephenson, G. R., and R. C. Rychert. 1982. *Bottom sediment: A reservoir of* Escherichia coli *in rangeland streams.* J. Range Manage. 35(1): 119-123.
4. U.S. Department of Agriculture, Agricultural Research Service. 1979. *Field manual for research in agricultural hydrology.* Agr. Handbk. 224. Washington, D.C. 547 pp.
5. Vanoni, V. A., Ed. 1975. *Sedimentation engineering.* Am. Soc. Civil Engrs., New York, N.Y. 745 pp. □