# Comment on 'Measurement of Sediment Control Impacts on Agriculture' by James C. Wade and Earl O. Heady

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The authors [*Wade and Heady*, 1978] are to be commended for attempting to solve a most complex problem. The linear programing approach is a good technique for this problem.

As the authors stated early in the paper, 'additional research would be required to establish the validity of the parameters of this model before specific policy recommendations could be made, since many of the parameters are estimated from limited data and by new procedures.' This statement, as well as errors in the paper restrict use of the paper to an illustration of what might be done.

The comments in this letter are directed toward major points of contention in the paper, and follow the organization of the original paper.

1. In the opening paragraph the authors stated that '. . . sediment is not easily controlled.' Actually, there is a wealth of information about controlling sediment; generally, the problem is doing it within the framework of a farmer's economics. We often have the knowledge, but not always the dollars. Besides construction costs, some erosion control systems, like terraces, are difficult to farm, particularly with large modern equipment. However, good land management is often more economical than traditional land management (such as no-till versus clean-tilled row crops) and is excellent for controlling erosion. Still another reason for lack of sediment control is that it takes an effort, and many farms suffer from benign neglect; farmers are simply too busy with the many other problems. Clearly, education is needed.

2. There is, apparently, a symbol missing in (5), between $X_T$ and 0.

3. Under the section entitled 'erosion sector,' the authors stated that the '. . . analysis and verification of the procedures used to estimate erosion rates are a major part of this article.' The USLE was used to estimate sheet and rill erosion, and they developed guidelines for estimating delivery ratios and sediment transport ratios. These methods were described briefly, but the analysis escaped us, and certainly no verification was presented.

4. Figure 1 under the section on the sediment transport

system has an inconsistency which could be very important. The schematic shows that gully and channel erosion exists only from the nonvariable land use. To be used correctly, this box should be inserted before the sediment-delivery-ratio box, and then the erosion from the variable and nonvariable land-use boxes will input to this box of the schematic. The authors in this section also discussed '. . . three distinct aspects of soil movement: Sources, delivery, and transport.' Most sedimentationists refer to these three components as erosion, transport, and deposition, or as upland soil displacement, delivery to channels, and transport in channels. We are not sure why the authors adopted their own concepts, rather than building upon accepted concepts.

5. In the section on sediment delivery, the authors stated: 'Conventional procedures for computing delivery ratios proved inadequate. . .' The concept of sediment delivery ratios is admittedly crude, and delivery ratio relationships have not been developed for much of the United States. However, the conventional procedures should be compatible with other gross estimates contained in the paper. It seems unlikely that measured sediment yield data were available for each of the production areas. Since measured sediment yield data are scarce, the method the authors used is not very practical.

Delivery ratios vary regionally, and even within regions (Figure 1 of the present paper). The answers for the sediment-control impact could vary appreciably, depending upon what relationship was used. This variability was apparently similar to that used by the authors. *Williams* [1977] described a useful procedure for developing sediment delivery ratio equations.

In (10), the authors used a term, $s_i^*$, to reflect the tons of gross soil loss for land uses not endogenous to the model. We question what proportion of the suspended sediment delivered came from such sources, and how it was estimated.

6. In the sections on sediment transport and on sediment transport ratios, the authors stated that 'A basic assumption used is that in the long run sediment deposited in aggrading processes is offset by sediment removed in degrading processes.' This assumption contradicts the delivery ratio and transport ratio concepts, although it might be true on a large river system where equilibrium has been reached. Sediment not only is deposited in shallow upland flow, but also is deposited in flood plains where flow may be shallow, usually
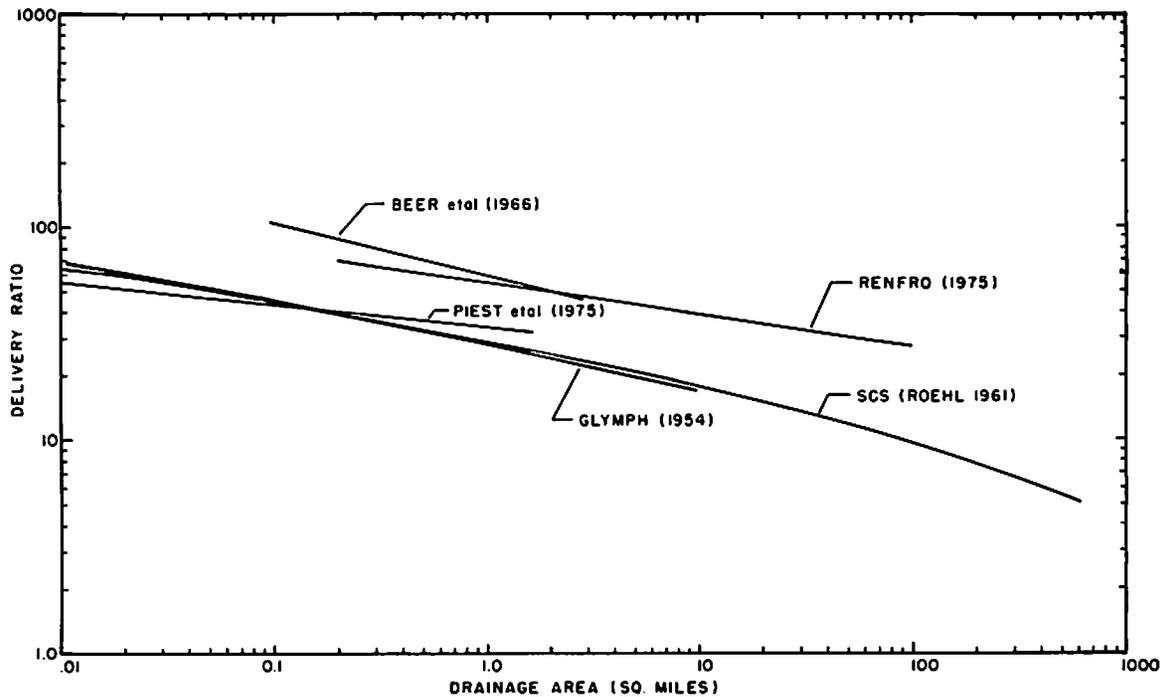
Fig. 1.   Sediment delivery ratio variations with drainage area as reported in the 1972 Sediment Yield Workshop [*U.S. Department of Agriculture*, 1975]. (Multiply mi$^2$ by 2.59 to obtain km$^2$.)

with low velocity. A sediment routing model [e.g., *Renard and Laursen*, 1975; *Williams and Hann*, 1978] could be used to determine the transport ratios. As in other sections, the authors' terms and reasoning confuse the reader.

The presentation of sediment delivery ratios (Table 1) and sediment transport ratios (Table 2) to three significant figures is certainly questionable. The values provided in the tables create a false impression of facts and knowledge. Similarly, the

six significant figures used in Table 4 imply unwarranted accuracy.

7.   In Figure 2, the schematic of the sediment parameter estimation procedure, the *P* factor (conservation factor) of the USLE should have been included as another decision variable, along with the *C* factor and the RKLS factor. The average sediment load is apparently the measured value (?) which then is used with the estimated total gross erosion to determine the
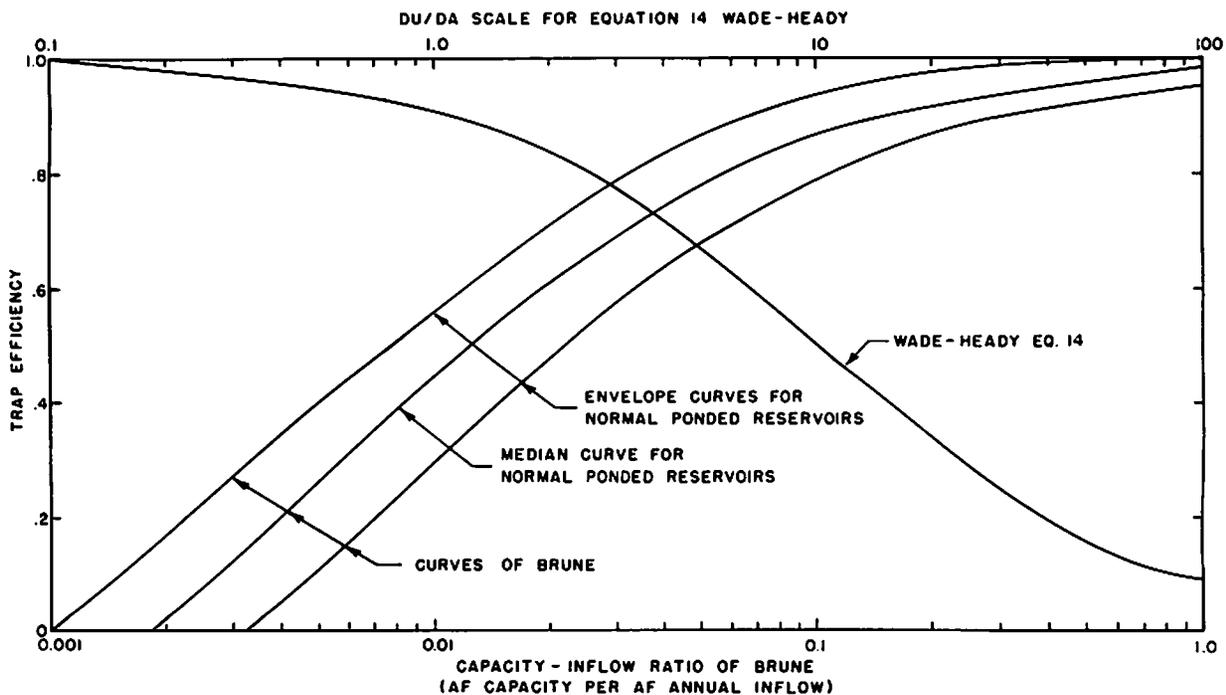


Fig. 2.   Reservoir trap efficiency relationships.

delivery ratio. Thus the delivery ratio developed depends upon having an estimated sediment-transport ratio along with the other factors affecting the gross erosion estimate.

8. Using 5% trap efficiency for reservoirs seems to be very low where data are inadequate. Reference to Brune's classic work on reservoir trap efficiency illustrates an important difference between his work and the formula presented as (14). Figure 2 of the present paper illustrates that the trap efficiency relationship shown by the authors is almost opposite the relationship shown by Brune. Although the abscissa scale is different for the two concepts, the assumption that Brune's annual inflow is directly proportional to drainage area makes both abscissae have the same relative trend. Thus the author's concept disagrees with Brune's.

The concept of relating the reservoir trap efficiency to the drainage area, as the authors' did, is poor, because runoff (the transporting mechanism) is not simply related to drainage area. Figure 3 of the present paper illustrates that the relationship of runoff to drainage area is quite different for different physiographic areas; (14) would not afford this flexibility.

Equation 14 shows that if drainage area is fixed, increasing the reservoir size decreases the trap efficiency, a concept that is unrealistic, because the residence time of the runoff and sediment in the larger reservoir would increase. The source of (14) is not presented, and in any case, the equation is wrong.

About the only time one would use a 5% sediment trap efficiency is with a very low-head sill in a channel, or when the sediment entering a reservoir is composed of very fine clay and the reservoir has a large bottom outlet. Even reservoirs that have lost most of their capacity to sediment deposition have trap efficiencies larger than 5% because the inflow is still spread over a larger cross section than what the normal stream would have. The reduced flow velocity allows the larger sediment particles to settle out.

9. The authors stated that they tested the sediment delivery ratios before applying them in their model, and that their verification gave a successful appraisal of their system. It would be very helpful if they had shown their testing and verification so that a reader could make his own judgment as to the success of this system. It is not evident how their model can work, unless there are compensating errors, such as high sediment transport ratios, offsetting the low values of reservoir-sediment trap efficiency. Throughout the paper, the allusion to DA's, MR's (drainage areas and market regions), and river basin groupings of PA's is very confusing. The points of the analysis would have been clearer with an example.

10. It is difficult to draw conclusions from the results in Table 5. Sediment delivery varies with physiographic conditions, such as soil type, slope, and the location of the farm relative to the stream point where pollution may be a problem. If sediment is not delivered to a point of damage, it is not a pollutant. However, sediment lost from an individual farm can also be a severe economic loss in terms of the production potential of that soil. In some instances, limited soil depths can severely restrict soil moisture storage, and in other instances, losses from the surface can remove sizeable quantities of essential plant nutrients that are costly to replace [see *Willis and Evans*, 1977]. Thus sediment control at a point in the stream may not always provide the greatest economic benefit.

11. In the section on sediment loads the authors stated, 'In some areas the proportion of sediment from noncropland sources is so high that reducing cropland erosion has no major consequence on total loads.' Further elaboration on this point
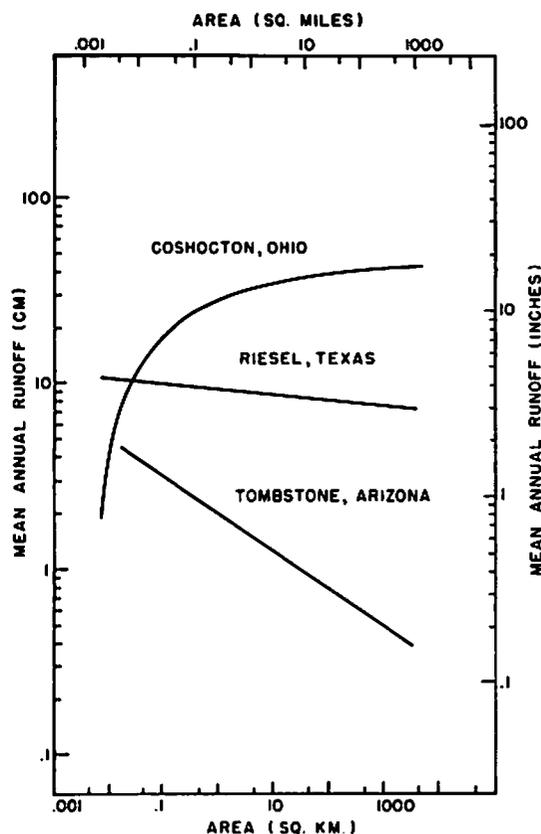


Fig. 3. Mean annual runoff versus size of drainage area for several vicinities [*Glymph and Holtan*, 1969].

seems warranted. In the mountainous areas of the western United States or in the rangeland areas, the statement may be valid, but we feel it is an exception rather than the rule.

Thus we feel that although the authors' effort is a commendable example of what might be done, there are enough questionable items in the paper that we hope decision-makers will neither consider it as a final product nor reach any conclusions based on the use of the material it presents.

REFERENCES

Glymph, L. M., and H. N. Holtan, Land treatment in agricultural watershed hydrologic research, in *Effects of Watershed Changes on Streamflow, Water Resour. Symp. 2*, pp. 44–68, University of Texas Press, Austin, 1969.

Renard, K. G., and E. M. Laursen, A dynamic behavior model of an ephemeral stream, *J. Hydraul. Div. Proc. Amer. Soc. Civil Eng.*, *101*(HY5), 511–528, 1975.

U.S. Department of Agriculture, Present and prospective technology for predicting sediment yields and sources, *ARS-S-40*, 285 pp., Washington, D. C., June 1975.

Wade, J. C., and E. O. Heady, Measurement of sediment control impacts on agriculture, *Water Resour. Res.*, *14*(1), 1–8, 1978.

Williams, J. R., Sediment delivery ratios determined with sediment and runoff models, Proceedings of the Paris Symposium, July, *IAHS-AISH Publ. 122*, Int. Ass. Hydrol. Sci., 1977.

Williams, J. R., and R. W. Hann, Optimal operation of large agricultural watersheds with water quality constraints, *TR-96*, 152 pp., Tex. Water Resour. Inst., Texas A&M Univ., College Station, 1978.

Willis, W. G., and C. E. Evans, Our soil is valuable, *J. Soil Water Conserv.*, *32*(6), 258–259, 1977.