

# DIRECT DETERMINATION OF RHEOLOGICAL CHARACTERISTICS OF DEBRIS FLOW<sup>a</sup>

Discussion by Yu Bin<sup>6</sup> and Zhao Huilin<sup>7</sup>

The authors present a method to determine the rheological characteristics of debris flow directly. According to the authors' experiments on the material debris flow that occurred on Moscardo Torrent (Friuli Region, Italy), the debris flow behavior can be represented by a Herschel-Bulkley model with  $n = 1/3$ . In the authors' experiments, the coarse fraction of debris flow was eliminated, even in the large-scale rheological experiments. In practice, however, the rheological characteristics of debris flow are affected by the coarse particles.

Zhao and Chen (1992) have researched the role of coarse particles in debris flow. In their research, the fine particle content of viscous debris flow is nearly the same as that of non-viscous debris flow, and different coarse particle content causes different concentration of debris flow. They gave a concept of effective concentration and pointed out that coarse particles reduce the space (or volume) occupied by fine particles and water, then raise the effective concentration of fine particle slurry, thus influencing indirectly the physical characteristics of debris flow. So the elimination of coarse fraction must affect the rheological characteristics of debris flow.

With the same aim as the authors, i.e., direct determination of the rheological characteristics of debris flow, the discussers have finished some rheological experiments on debris flow. The rheometer is HAKKE RV12, and the gap of the cylinder is 5.8 mm. The rheological curve can be obtained directly. The test material comes from a debris flow that occurred in Jiangjia Gully, Dongchuang, China, in 1989. The mixture of debris flows was obtained by adding successively to clear water, coarse particles derived from the sample of debris flow material. The first step is the test of fine particles (maximum particle diameter: 0.2 mm). The concentration (or effective concentration) was 0.1, 0.2, 0.25, 0.3, 0.35, and 0.4, respectively. The Bingham model was fitted to the rheological curves:

$$\tau = \tau_0 + \mu \frac{du}{dy} \quad (4)$$

where  $\tau$  = shear stress;  $\tau_0$  = Bingham yield stress;  $\mu$  = dynamic viscosity; and  $du/dy$  = shear rate. Then, adding coarse particles successively, the concentration of coarse particles was 0.1, 0.2, 0.25, 0.3, and 0.35, respectively, and the average diameter was 0.55, 1.01, 1.18, and 1.63 mm, respectively. The sedimentations were observed at the range of shear rate 20–56  $s^{-1}$ . The larger the effective concentration of slurry, the larger the shear rate of sedimentation. The larger the diameter of coarse particles, the smaller the shear rate of sedimentation. The model of O'Brien and Julien as described by Julien and Lan (1991) was fitted to the rheological curves of nonsedimentation:

$$\tau = \tau_y + \eta \frac{du}{dy} + \zeta \left( \frac{du}{dy} \right)^2 \quad (5)$$

where  $\tau_0$  = yield stress;  $\eta$  = dynamic viscosity; and  $\zeta$  = turbulent-dispersive parameter. The  $\tau_0$  and  $\mu$  can be determined by a test of slurry and (4). The  $\zeta$  can be determined by a test of slurry with coarse particles and (5), and  $\tau_y$ ,  $\eta$  can be obtained by correction of  $\tau_0$  and  $\mu$ , respectively, in (4) and (5). The concentration of slurry was the same as the effective concentration of slurry that has coarse particles;  $\tau_y$ ,  $\eta$ , and  $\zeta$  are the function of diameter of coarse particles, effective concentration of slurry, and concentration of coarse particles. The rheological parameter of debris flow can be determined by the test of slurry that has only fine particles, and its concentration is the same as the effective concentration of slurry in debris flow material. As the authors pointed out, the work of the discussers remains approximate and relatively difficult to use in practice, but it was an attempt to determine debris flow in rheological parameters.

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Closure by Philippe Coussot,<sup>8</sup>  
Dominique Laigle,<sup>9</sup> Massimo Arattano,<sup>10</sup>  
Andrea Deganutti,<sup>11</sup> and Lorenzo Marchi<sup>12</sup>

The writers are grateful to the discussers for their interest in the paper. They are pleased that an approach whose principle is similar to the method presented in the paper has already been applied by the discussers, leading to similar results. The characteristics of the material to which it was applied and the reference rheological model appear to be different, though. The point focused by the discussers—i.e., the influence of coarse particles—is essential and has probably not been fully addressed yet.

The addition of coarse particles may lead to two types of modifications concerning the behavior of the mixture:

- When adding coarse particles leads to a low content of fine fraction, interactions between grains prevail over viscous dissipations, and the mixture probably does not behave as a viscoplastic fluid.
- When adding coarse particles leads all the same to a large enough content of fine fraction, viscous dissipations prevail and the mixture is likely to behave as a viscoplastic fluid. In that case the addition of coarse particles leads to an increase of the rheological parameter values, the generic expression of the rheological model that applies to the fine fraction-water mixture being still valid for the bulk mixture. Coussot (1994) proposed using a Herschel-bulkley model ( $\tau = \tau_c + K\dot{\gamma}^n$ ) in that case.

Coussot and Meunier (1996) suggested a practical (thus approximate) criterion to classify natural debris flow materials

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<sup>6</sup>Asst. Prof., Chengdu Inst. of Mountain Disaster and Environment, CAS, P.O. Box 417, Chengdu, 610041, China.

<sup>7</sup>Asst. Prof., Chengdu Inst. of Mountain Disaster and Environment, CAS, P.O. Box 417, Chengdu, 610041, China.

<sup>8</sup>Res., Laboratoire des Matériaux et des Structures du Génie Civil, Cité Descartes, 2 allée Kepler, 77420 Champs-sur-Marne, France.

<sup>9</sup>Res., Cemagref, Div. Erosion Torrents Neige et Avalanches, Domaine Universitaire, B.P. 76, 38402 St.-Martin-d'Hères, France.

<sup>10</sup>Res., CNR, Inst. for Hydrogeological Protection in Po Basin (IRPI) Strada delle Cacce, 73-10135 Torino, Italy.

<sup>11</sup>Res., CNR, Inst. for Hydrogeological Protection in Northeastern Italy (IRPI) Corso Stati Uniti, 4-35127 Padova, Italy.

<sup>12</sup>Res., CNR, Inst. for Hydrogeological Protection in Northeastern Italy (IRPI) Corso Stati Uniti, 4-35127 Padova, Italy.

between these two categories of behavior: when the fine fraction ( $<40\ \mu\text{m}$ ) represents more than about 10% of the grain size distribution (which may greatly depend on the type of clay present in the material, though), the material belongs to the second category and is referred to as muddy debris. That is the case of the studied material (Rio Moscardo, Friuli, Italy). Thus, in a general sense, the proposed method only applies to materials that belong to this second category.

Coussot (1994) and other authors have proposed using the Herschel-Bulkley model (with  $n = 1/3$ ) because it applies on a wider range of shear-rates than the Bingham model classically used, and takes into account the shear-thinning behavior of most natural mixtures encountered in the field. This model has been validated in the field on the basis of a large number of experiments carried out with a large-scale rheometer (Coussot and Piau 1995).

Compared to the research carried out by the discussers, it appeared during our experiments on Rio Moscardo debris flow material, that sedimentation and turbulence could be neglected. Thus, in our approach, interest was focused on the evolution of the yield-stress ( $\tau_c$ ) and consistency index ( $K$ ) versus concentration of coarse particles. According to previous results (O'Brien et al. 1993) these parameters increase rapidly (seemingly exponentially) with the solid concentration, resulting itself from the addition of coarse particles, while the power-law index ( $n$ ) seemingly remains constant when the solid concentration is increased. However, as pointed out in the paper, some more work on the evolution of these two latter parameters would be necessary to fully validate the proposed approach.

As pointed out in the paper, the presented method remains approximate and still difficult to use in practice. However, the experimental techniques that were applied led to coherent results and made it possible to infer the rheological parameter values of muddy debris. At least, several of these techniques can be easily applied by field engineers and provide useful estimations of parameter values.

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## RIVER WIDTH ADJUSTMENT. I: PROCESSES AND MECHANISMS<sup>a</sup>

Discussion by S. V. Chitale<sup>2</sup>

### DEFINITION OF WIDTH

The paper is concerning the river width. This width could be low water width or high water bankful stage width; it could be in the bend portion or in a relatively straight reach at the crossing; the width could be on a single cross section at a

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<sup>2</sup>Consulting Engr., 425/14 T.M.V. Colony, Pune 411037, India.

specific location or the average width taking the mean of several meander lengths.

Furthermore, different authors have considered different widths in formulating relationships for morphological parameters. Blench (1957) considered the mean width (area/depth) while Chang (1985) adopted the effective width at the pool section, defined as that portion of the total width that provides power expenditure of the section, excluding the dead-water spaces at the channel margin from calculations. This width is equivalent to that given by Cherkaur (1973), who noted that the effective width more closely approaches the total channel width in riffles (crossings) than in pools (Richards 1976). Normally, however, the width considered in morphological adjustments is the water surface width at the crossings at the bankful discharge (formative) averaged over several bends, which appears to be an appropriate choice for the width parameter. The width needs to be defined in the Task Committee paper to remove ambiguity.

### TRACTIVE FORCE APPROACH

Alluvial rivers are rarely straight (Leopold and Wolman 1957). More often they are either meandering or braided. Unless there are constraints, natural or through human agency, bank erosion occurs during floods and meanders having low sinuosity travel downstream, as on the Ganga River in India, while those having high sinuosity form cutoffs as was experienced on the Mississippi River. In the case of the Ganga River, the period of completing downstream movement by one meander length has been found to be about 70 years at Mokameh and Mansi in Bihar state but only 30 years at Gaighat in the state of U.P. depending on the frequency, duration, magnitude of high flood, and erodibility of the bank material (CBIP 1989). In the Mississippi, the bends are sharp, having high sinuosity, and cutoffs result if bend erosion is not arrested. Below Cairo between 1765 and 1929, 19 natural cutoffs occurred (Ferguson 1939). In braided rivers also, bank erosion occurs and separating islands move downstream as observed on the Brahmaputra River by Coleman (1969). Thus the morphological equilibrium of alluvial rivers is a dynamic one (and not static), allowing bank erosion. On the other hand, bank erosion is avoided in case of the alluvial canals by restricting the design velocity or shear stress equal to or less than critical. The tractive force methods (explained on p. 884) are thus considered by the discussers to be applicable to alluvial canals, but not in the case of alluvial rivers.

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### Discussion by Erik Mosselman<sup>3</sup>

The thorough review provides a rich blend of field experience in various geomorphic contexts and state-of-the-art mod-

<sup>3</sup>WL/Delft Hydraulics, P.O. Box 117, 2600 MH Delft, The Netherlands.

eling, with an adequate research agenda. However, the discussor would have liked to see a stronger presentation of the existing knowledge on external hypotheses. Phrases such as “the last two decades” and “still not entirely clear” give the impression that extremal hypotheses are somehow modern and unexplored, thus holding promises for innovation. In reality, extremal formulations were prominent in previous centuries (Leibniz, Maupertuis) and have been superseded because of their limited explanatory power.

Extremal hypotheses do not have the generality of fundamental laws of physics, but they appear to be mathematically equivalent to equations deduced from fundamental laws for special simplified cases. For instance, Yang and Song (1979) show that the velocity distribution that satisfies a linearized momentum equation without inertia terms, is the one that minimizes the total rate of energy dissipation. Considering the applications to width adjustment in the companion paper (ASCE 1998), the minimization of deviations from uniformity in FLUVIAL-12 is still close to modern mechanistic concepts in which gradients provide the driving forces of processes. The validity of the minimization of energy dissipation in GSTARS can be expected to be more limited, because such minimization is not a general principle of nature. The limited validity is substantiated by Griffiths (1984) and Lamberti (1988, 1992), who demonstrate that, generally, conclusions from extremal hypotheses are not compatible with observations.

Another objection against extremal hypotheses is the teleological form in which they are presented. This form carries a connotation of anthropomorphism, as if rivers are living organisms with conscious goals. Admittedly, rivers often appear to be more like living organisms than inanimate systems (Kennedy 1983), and informal teleological explanations of river behavior are usually accepted as merely a way of speaking, without the premise of a conscious goal. However, formal teleological explanations are not accepted in mechanics where they do imply the connotation of anthropomorphism (Nagel 1961).

The fundamental equations for the geofluvial system form a determinate system that, theoretically, can be solved when the initial and boundary conditions are known. Mathematical indeterminacy arises when only the steady state is considered. Some researchers believe that, rather than the laws governing the transient process of width adjustment, some extra law of nature should exist to close the steady-state system of equations. Extremal hypotheses have been proposed to provide this extra closure relationship. But as extremal hypotheses appear to be just alternative formulations for simplified versions of fundamental laws already used, the only new information they add is equivalent to the conditions under which the adopted simplifications are valid. The existing equations may be incomplete due to limited process knowledge, but the idea that essential equations are missing is a fallacy.

Notwithstanding these arguments, it remains refreshing to look at geofluvial dynamics from a different perspective. There is still room for research on extremal hypotheses for reasons of purely scientific interest or sheer fun. The suggestion, however, that extremal hypotheses are new and relatively unexplored sends out the wrong message to funding agencies and young researchers starting their career. After all, the greatest progress in the last decades has not been obtained from the old-fashioned extremal hypotheses, but from mechanistic approaches, not only by means of more detailed process knowledge and development of numerical models (as reported by the Task Committee), but also by providing explanations on more philosophical issues such as the cause of river meandering and the inherent limitations to the predictability of morphodynamic systems.

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### Discussion by Emmett M. Laursen,<sup>4</sup> Life Member, ASCE

The Task Committee correctly states that width adjustments can have impacts on a number of things, but since the statement is so brief (because everyone interested knows) it fails to go into just what our interest is. I would suggest our interest is in the chance something of value will be damaged or destroyed in the next 10, 100, 1,000 years—or at all.

For this we do not need to worry about the rate of bank retreat (or erosion), but only the limit. The term bank retreat is used advisedly because one bank can retreat while the other side of the river becomes relatively inactive (virtually stagnant) with little or no width increase. The stable width of a channel for different discharges, however, is the clue needed to be able to make a reasonable prediction of what can happen.

The equivalent rectangular channel can be defined as a rectangular channel with the same discharge  $Q$ , the same sediment load  $Q_s$ , and the same slope  $S_o$  (and, of course, the same bed material and the same resistance as the natural channel).

Straub solved the long-contraction problem using the DuBoys transport equation and the Manning formula. For every width ratio, he found a depth ratio, and other flow characteristics can be found. Straub’s banks were implicitly assumed inerodible, but to the extent the boundary shear distribution is known, one can evaluate the bank erodibility for various widths. It is notable that most transport formulas will give results very close to Straub’s solution. (Those few that do not I would question.)

Using a variant of Straub’s concept, one can assume a  $Q$ , a  $Q_s$  (and a bed material), an  $S_o$  (and an  $n$  value), and a width  $B$  and solve one way or another for the depth and the bank shear. For another  $Q$ , the same concentration  $Q_s/Q$  (or another specified  $Q_s/Q$ ), the same  $S_o$  and  $n$ , and several  $B$ s, one can interpolate to find the  $B$  for the same bank shear. Enough computations provide the value (graphically) for the width  $B$  as a function of  $Q$ . According to Blench

$$B \propto Q^{0.5} \quad (5)$$

For the typical conditions in the Southwest.

$$B \propto Q^n \quad (6)$$

With  $n$  being about 0.8. As the Platte River has been described (knee deep and mile wide), the exponent could approach 1.0. Thus it would be expected that the width could almost double if the discharge doubled (and the return interval increased by

<sup>4</sup>Prof. Emeritus, Dept. of Civ. Engrg. and Engrg. Mech., Univ. of Arizona, Tucson, AZ 85721.

an order of magnitude). Note, however, that all of the increase could occur on just one side of the river.

The bank shear in an imperfect laboratory flume can vary along the flow by  $\pm 15\%$ . This is equivalent to varying the discharge and can cause bank erosion and retreat. In a 10-year flood a bank can retreat an amount equal to the stream width over a reach of several widths, and the flow can then switch to the other side and erode the opposite side.

A similar behavior can occur if a bank is slowly, almost imperceptibly, eroding until it reaches a weaker, more erodible alluvial deposit. The erosion rate then suddenly accelerates and the bank can retreat a stream width in an ordinary 10-year flood event.

Aerial photos and calculations such as suggested herein should be enough to predict what could happen in floods that could happen over the nominal life of whatever it is that could be damaged or destroyed. A bridge may have considerable value for 50 years, a real estate development with streets and utilities for over 100 years, a farm field for maybe only 10 years.

The questions then are how to reduce damage, how to protect structures of value, and how to best use the land adjacent to the riverbanks. Approach fills are relatively cheap; bridges are expensive (bridge failures are more expensive). Even low-cost housing is expensive when the development is large; agricultural levees a few feet high are cheap. If there is something of significant value in danger, a cost/risk analysis will almost always push the design to be for the maximum truly expected flood (in my experience), and every lesser flood will be covered also. However, if you settle for less than the worst case hoping the erosion will not go on to the limit, remember—the same magnitude flood could happen again the next year and go on for twice as long.

### Closure by the ASCE Task Committee on Hydraulics, Bank Mechanics, and Modeling of River Width Adjustment<sup>5</sup>

The writers welcome the interest shown in their paper by the discussers. Chitale correctly points out that the term “river width” is ambiguous. The writers agree that different authors have defined river width in a variety of different ways, depending on the context and application for their work, and Chitale uses some excellent examples to illustrate this point. To clarify the term for use in conjunction with the Task Committee’s paper, we broadly accept his proposed definition. We have, however, reworded it slightly for clarity. Hence, the writers’ proposed definition of river width with respect to morphological adjustments is as follows: The water surface width at bankfull (formative) discharge measured at meander crossings and averaged over several meander wavelengths.

The writers also agree that the tractive force approaches are most appropriate to alluvial canals. Chitale is correct in emphasizing that the theory behind the existing tractive force methods assumes the channel to be straight and the banklines to be static. This is the case in many canals, but in nature, most streams have meandering or braided planforms that are constantly changing and evolving. This would suggest that results based on tractive force methods cannot be applied to alluvial rivers. In practice, several researchers have, however, demonstrated good agreement between the channel dimensions

predicted by tractive force methods and the stable channel dimensions measured in laboratory experiments and in natural rivers that are relatively straight or exhibit low sinuosity [see, e.g., Vigilar and Diplas (1998)]. This agreement may be attributed to two properties of low sinuosity streams that allow their channels to be characterized using tractive force theory. First, the morphological effects of strong secondary currents are insignificant in straight and slightly sinuous rivers, so that the fact that they are neglected in tractive force theory is acceptable. Second, rates of lateral and downstream bend migration are slow, and therefore treating the banklines as static is an acceptable first approximation when predicting the stable channel dimensions. These approximations are, of course, inapplicable to streams exhibiting significant sinuosity, and the writers are in agreement with the discussers’ comment that tractive force methods are unsuitable for meandering (sinuosity  $> 1.5$ ) and braided rivers.

The writers welcome the insightful and innovative view of extremal hypotheses provided by Mosselman. His discussion builds usefully on the brief reference to these approaches included in the Task Committee’s first paper, and his remarks on the relative merits of the FLUVIAL12 and GSTARS models add to the commentary on modeling width adjustment in the second paper. The writers did not intend to give the impression that the concepts behind theories of energy minimization or maximization are new, and Mosselman is right to point out that they have been used in several disciplines for a long time. However, the writers retain their view that advocacy of the use of extremal hypotheses to predict stable channel morphology is a fairly recent phenomenon and would point out that the publication dates of the references cited by Mosselman clearly testify to this. Mosselman clearly understands and explains why extremal hypotheses were not dwelt upon in the Task Committee’s review, which is simply because they remain difficult to justify and explain from first principles—a view long held by established river engineers and scientists [see, e.g., Davies and Sutherland (1983)]. Conversely, there are situations where members of the Task Committee have found minimization concepts useful in explaining morphological evolution of unstable fluvial systems (Simon 1992; Simon and Thorne 1996; Simon and Darby 1997). Like Mosselman, we would not wish to discourage young engineers from pursuing research on extremal hypotheses. On the contrary, we would challenge them to further explore and enlighten us on the potential benefits and limitations to these approaches with respect to the analysis and design of alluvial channels.

Laursen suggests that our interest in width adjustment arises because of “the chance that something of value will be damaged or destroyed in the next 10, 100, 1,000 years—or at all.” While the members of the Task Committee appreciate that finding solutions to practical problems involving river width adjustment is of paramount interest to river engineers, the remit of the Task Committee called for a broader consideration of width adjustment processes and mechanisms. Specifically, we were charged with reviewing current understanding of the fluvial processes and bank mechanics involved in river width adjustment and with identifying current research needs to advance fundamental as well as applied research. No doubt approaches such as that advocated by Laursen (using the Straub’s solution of the long-contraction problem) may be useful, when applied with care, to the prediction of bank erodibility or stable width. Unfortunately, they tell us little or nothing about the fluvial processes actually responsible for width adjustments or the gravitational failures invoked when bank stability becomes limiting. In any case, we would argue that improved engineering analyses for bank protection and channel stabilization

<sup>5</sup>Colin R. Thorne, Dept. of Geography, Univ. of Nottingham, Nottingham NG7 2RD, U.K.; corresponding author. E-mail: colin.thorne@nottingham.ac.uk

schemes in channels with changing widths can only be achieved through a more complete understanding of the cause(s), nature, extent, and severity of the geomorphological problem (Thorne et al. 1995, 1996, 1997; Thorne 1998). In this context, it is indeed encouraging that fundamental research on width adjustment processes and mechanisms, and unrelated to “the chance that something of value will be damaged or destroyed,” continues apace [see, e.g., Simon et al. (1999) and Rinaldi and Casagli (1999)].

Laursen dismisses the need to “worry about the rate of bank retreat (or erosion),” instead advocating that it is only the *limit* of erosion that is of concern. This statement is inconsistent with modern approaches to bank erosion management, which rely on an assessment of the risk posed by bank retreat (Reed 1998). Risk assessment requires knowledge not only of the possible limit of erosion, but also of the bank retreat rate, in order that the threat posed to structures or assets adjacent to the river can properly be quantified and appropriate action taken to reduce the threat to an acceptable level. Certainly, where infrastructure (such as a bridge) or housing is threatened, minimum risk can be taken and an engineering solution using hard bank protection will be required (*Waterway* 1999). However, where agricultural land or recreational areas are threatened, it may be acceptable to reduce, but not remove, the erosion risk using an environmentally sensitive, nonstructural or management solution that decreases, but does not eliminate, bank retreat (Reed 1998; *Waterway* 1999). Clearly, reliable estimates and predictions of bank retreat and width adjustment rates with and without proposed engineering or management interventions are essential to the application of this novel approach.

In conclusion, the writers would like to thank the discussers for their useful contributions, which have each added significantly to the value of the Task Committee’s work and outputs.

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## RIVER WIDTH ADJUSTMENT. II: MODELING<sup>a</sup>

Discussion by Emmett M. Laursen,<sup>2</sup>  
Life Member, ASCE

Hydraulic models are imperfect, distorted analog models of their prototype that don’t necessarily give an adequately correct answer, but at least they perform the way they do because they have to . . . not because you tell them to. Numerical models (mathematical models, computer models) behave as you tell them to; therefore, they are not models. They are simply computation procedures. To call them models is to invest them with an authority that is undeserved and thus is dangerous—“but that is what the computer says.”

Even to be meaningful as a computing device or process, one must decide on the correct equations to describe the hydraulic behavior of the parts of the puzzle, how the puzzle is to be arranged, and all the data that are required, and finally to examine the final numbers and sense whether they are at all useful. It is my contention that numbers crunched in a computer have only a limited value, by themselves, to the problem at hand.

About 50 years ago at the Iowa Institute, I was asked to do a mobile-bed model study of scour in the floodplain downstream of low dam being built. The floodplain was wide, sandy, and covered with small pines and brush—with power lines and pipelines slashed through it here, there, without any evident rhyme or reason. I refused, but said that if they could tell where future lines would go, that I’d be able to predict where the scour would occur.

It thinks there is a similar difficulty here in determining whether a structure or development is vulnerable. If the valley is occupied by birds and bees and trees, there is no problem requiring a solution. In the long term (geological time), the river will have been all over the valley—sometimes here, sometimes there. In the short term (one’s lifetime), some short reaches of bankline will retreat and endanger facilities; some adjoining reaches won’t. Which do and which don’t depends on the local boundary shear over a relatively small area and the erodibility of bank material in this area. Knowing the character of the alluvium everywhere is manifestly impossible. Forecasting the probable, expected bank shear over the next 50 years is equally fazing.

Several times over the years I have attempted to hindcast a bank failure event through the examination of aerial photos from the past. The beginning of the erosion was generally so small as to be overlooked if the failure hadn’t already occurred. As time went by, the eroded area enlarged, but still might not have signaled imminent danger. Then when the current was sufficiently shifted or a slightly larger flood came along—failure, and a court case.

Using computers to solve the equations that reveal the (approximate) relationship between river width and discharge, sediment load, slope, bed material, bank material, and whatever else matters significantly is a good thing to do. Ground truth: the new width of a river after a flood that was larger than the previous large flood is absolutely imperative to build confidence in what came out of the computer. The bank failures examined do not need to be at specific locations—they

<sup>a</sup>September 1998, Vol. 124, No. 9, by the ASCE Task Committee on Hydraulics, Bank Mechanics, and Modeling of River Width Adjustment (Paper 14418).

<sup>2</sup>Prof. Emeritus, Dept. of Civ. Engrg. and Engrg. Mech., Univ. of Arizona, Tucson, AZ 85721.

can be anywhere that isn't obviously dissimilar. Equations and the computer have a role; they can be used to guide the interpretation of what has actually happened in the past.

Rivers widen when the discharge increases because if the banks were just at the verge of eroding, now they are over the verge and will erode until the cross section enlarges and the velocity decreases—and the bank shear reduces to its former value on the verge of eroding. The opposite behavior, narrowing the river, is not as simple, and easy, to understand. The beginning of narrowing, however, can be a result of general bed deposition of the overload created by the bank erosion. The low flow following the flood carves out a new channel for itself. There is now a mini-floodplain within the enlarged channel, and deposition will gradually occur on this mini-floodplain. This disposition, this narrowing, should not present a threat in and by itself, but can in that it contributes to a change in the planform of the river.

Equations can be useful, but they need to be examined with skepticism. Almost all sediment transport equations will result in almost the same prediction of stream width, especially if set up in ratio form. Yet every so often somebody grabs onto the Brown-Kalinski or Brown-Einstein equation even though they were presented in the Sediment Transportation chapter of *Engineering Hydraulics* for the explicit purpose of demonstrating the inability of their original expressions to describe their own data when transport is general—well above critical tractive force conditions. Hardly indicating a skeptical examination of those equations when they are used.

### Closure by the ASCE Task Committee on Hydraulics, Bank Mechanics, and Modeling of River Width Adjustment<sup>3</sup>

The members of the ASCE Task Committee welcome the discussor's interest in the paper. His views on the applicability of hydraulic models to practical problems concerning natural rivers are based on extensive experience and, therefore, must be treated with respect. The writers agree with him that existing numerical models require extensive improvements before they can be used to predict, with a high degree of accuracy and reliability, either the future autonomous evolution of an alluvial stream, or its response to the impacts of major floods or human interventions in the fluvial system. In stating this we are, of course, reiterating points made in our paper. We do, however, take issue with two specific statements made by the discussor in his discourse.

The first is his contention that, "Numerical models . . . behave as you tell them to; therefore, they are not models. They are simply computational procedures." The writers are unconvinced that numerical models "behave as you tell them to" (although some of us sometimes wish they did so!) and would point out that numerical models and computational procedures are not actually the same thing.

*Computational procedures*, often referred to as "code," are the algorithms and programming functions used in a computer program to process the mathematical equations (the mathematical model) that represent the physical processes actually operating in the real river or fluvial system. A *numerical model* is the combination of this code together with the input data necessary to define the initial status and the boundary conditions of the river or system. The modeler is responsible for ensuring that the computational procedures have been pro-

grammed to represent the mathematical model correctly, accessing reliable data that are sufficient to define the initial status and boundary conditions, and entering those data accurately. Provided that these conditions have been met, then the resulting model should be able to simulate the behavior of the river system to the extent that the mathematical functions within the model faithfully represent the physical laws governing natural processes at the Earth's surface. On this basis, we are confident that the majority of hydraulic and river researchers would support our view that a numerical model is greater than the sum of its computational parts.

The writers would be the first to admit that numerical models are neither perfect or adequate at present, but we would also stress that the better models are based on physical principles, utilize appropriate mathematical and numerical techniques, and, where theory is weak or inapplicable, make good use of empirical functions based on laboratory and field data. Experienced modelers are able to use these models to generate useful knowledge and increase our understanding of river process and form significantly and cost-effectively. In the last decade, numerical models have not only been verified and validated by many approaches (Wang, unpublished paper, 1999), but they also have been applied with success to engineering analysis and design in schemes too numerous to list here. The fact that many successful applications have been reported in the pages of this journal means that there is no need to elaborate this point further here.

The writers must also take issue with the discussor's statement that "If the valley is occupied by birds and bees and trees, there is no problem requiring a solution." This suggests a narrow view of what constitutes a river problem at the end of the twentieth century. Traditionally, numerical models and engineering solutions have been applied to the solution of socioeconomic problems created when river erosion or sedimentation impinges on the activities of people dwelling and working on the floodplain. However, during the 1990s river engineers have increasingly been asked to develop approaches and strategies targeted precisely on floodplains that lack any built environment. Two examples may be used to illustrate this point. The first concerns engineering measures to protect ecologically valuable floodplain environments, and the second, schemes to restore previously damaged floodplains.

It is now recognized that human activities in one part of a watershed can perturb the fluvial system to generate complex response and adverse channel changes in other parts of the drainage network (Schumm 1977). Under these circumstances, engineers must intervene to protect from damage vulnerable aquatic, riparian floodplain environments that are rich in wildlife, but economically undeveloped. While there are wilderness areas in North America where pristine river-floodplain systems should be left entirely to their own devices, this is not the case in most watersheds, where subtle engineering interventions may be required to protect the remaining environmental capital from the destructive impacts of past and present human activities.

The number of river restoration schemes being built in North America has risen exponentially during the 1990s (Brookes and Shields 1996). While early schemes concentrated almost exclusively on restoration of natural forms and habitats in the river channel, experience shows that to realize the maximum environmental benefits it is essential to reconnect the channel to its floodplain (*Stream* 1998; Janes et al. 1999). In many cases, much of the floodplain has been developed and cannot be returned to the river. Under these circumstances true "restoration" of the river to its predisturbed state may be impossible (Downs and Thorne 1996). Engineers are, however, rising to the challenge of restoring the environmental function of the floodplain, while continuing to meet legitimate goals of

<sup>3</sup>Stephen Darby, Dept. of Geography, Univ. of Southampton, Southampton, SO17 1BJ, U.K.; corresponding author. E-mail: s.e.darby@soton.ac.uk

flood defense and land drainage for developed areas, using innovative approaches such as two-stage channels and floodways, to enhance river and valley wildlife habitats and create areas for occupation by birds, bees, and trees.

In summary, the writers firmly believe that physical and numerical models will continue to contribute to the advancement of our knowledge and understanding of river forms and processes in general, and width adjustments in particular. Nevertheless, the writers accept the discussor's point that it is imperative that numerical models should be interpreted in conjunction with empirical data and ground truth for the river being modeled in order that the capabilities and limitations of the model can be identified and properly accounted for in each particular application. He is correct to remind us that healthy skepticism is required in interpreting models, and in saying so, the discussor echoes a sentiment evident at several places in our paper.

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# STABLE WIDTH AND DEPTH OF GRAVEL-BED RIVERS WITH COHESIVE BANKS<sup>a</sup>

Discussion by Tai D. Bui,<sup>3</sup> P.E.,  
Member, ASCE

The authors' paper should be required reading for anyone interested in understanding the different physical processes occurring in gravel-bed streams. The authors discuss not only the fluvial entrainment and fluvial erosion processes, but also the mass failure mechanics associated with cohesive river banks.

A central theme of the paper is to outline an objective function for the stable channel design procedure/model for use with rivers with cohesive banks and mobile "coarse and noncohesive" bed. The authors also use published field data for verification purposes. Equally important is the authors' statement regarding certain simplifications that must be made so that the main thrust of the present work is not obscured.

Notwithstanding the above, certain additional information is required to prevent possible misuse of the approach and the numerical modeling. In addition, the discussor also proposes a simplified procedure that could be used to implement the approach set out by the authors.

## ABOUT THE APPROACH

First of all, the hypothesis (i.e., that for a specified slope, the channel will adjust its cross-sectional geometry such that

<sup>a</sup>October 1998, Vol. 124, No. 10, by Robert G. Millar and Michael C. Quick (Paper 14568).

<sup>3</sup>Consulting Engr., Weslake Inc., 10-120 Lancing Dr., Hamilton, ON, Canada L8W 3A1.

its capacity to transport bed load sediment is maximized) may or may not be applicable to a stream of interest. For non-equilibrium conditions, it is doubtful that the hypothesis can be validated.

Secondly, to estimate the bed load transport capacity, the authors use the Einstein-Brown relation. It should be noted that other formula or procedures can also be used to determine this quantity, including the Meyer-Peter and Müller formula and Yang's (1984) or Parker's (1990) gravel formulas for bed load or gravel transport.

Finally, the approach presented by the authors restricts the computational procedure to sediment median size (i.e.,  $D_{50}$ ) on the bed. By so doing, the accuracy will be compromised since "size-class" calculations would provide a better result.

## NUMERICAL MODELING

A plethora of equations is included in the paper. Therefore, some typographical errors or omissions with respect to the limitations of certain equations may have been overlooked. In short, some discrepancies/limitations can be pointed out as follows:

- In Eq. (7), Einstein-Brown's bed load (by weight) equation, the left-hand side of the equation should read  $g_b/F_1 g[\rho(s-1)D_{50}^3]^{1/2}$  instead of  $g_b/F_1 \rho s \sqrt{(s-1)gD_{50}^3}$ .
- The value of 0.82 is used for  $F_1$  in the paper. It should be noted that if the water temperature is significantly above or below 16°C, this value might not be appropriate.

## SIMPLIFIED PROCEDURE

Using Microsoft Excel or MathCad computer software, a simplified procedure can be established. This procedure can be used to carry out the optimization scheme included in the original paper. Essential steps included in the recommended procedure are as follows:

1. Input the values of all independent factors as per the specific design problem.
2. Prepare the "cells" (in Microsoft Excel) to compute the bank shear stress, friction factor, discharge, stability factors, etc.
3. Define an objective function. In this case, the formula related to the bed load transport capacity,  $g_b$ .
4. Use Solver to maximize the objective function subject to the appropriate constraints, i.e., discharge, bank stability, side slope (via  $\theta$ ), bed perimeter, etc.

The above procedure should facilitate designers in obtaining a set of solutions without going through the iterative process.

The present discussion is intended to provide readers with conditions in which Millar and Quick's paper may apply. A typographical error is also pointed out, and it is hoped that this discussion will assist in any effort in applying sound scientific knowledge to stable channel design. A simplified procedure is also included to assist designers with this type of practical problem.

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## Discussion by Emmett M. Laursen,<sup>4</sup> Life Member, ASCE

Stable channels (width, depth, slope) are in essence both simple and perplexing—perplexing because certain elements are not known adequately; simple in that for every discharge,  $Q$ , sediment load (of a particular bed material)  $Q_s$ , and a bank material of some known erosion resistivity, there is a width, depth, and slope, which is the limiting, stable condition. Since the flow and the independent variables vary with time and/or space, the stable, limiting geometry is probably seldom attained in nature, but it is for several reasons the best guide we can have for evaluating the vulnerability of structures and developments near riverbanks.

Consider a flume with a sand feed and a sand trap. Fill the flume with a sand and gravel bed material; establish a discharge and rate of sediment supply. After a time the sand trap will show a sediment-transport rate equal to the rate of feed. The flow and sediment load should now be in equilibrium. The water surface and bed profiles should no longer change with time. The depth and velocity of flow can be measured as well as the bed and water surface slopes—which should be parallel. The back wall of the flume should be inerodible and of a surface texture resembling a cohesive soil. The boundary shear on this “bank” can be measured with a calibrated Preston tube. If this back wall could be replaced with a cohesive soil having a resistance to erosion of that measured shear value, it should not erode.

If the rate of sediment supply is now increased, the depth should decrease, the velocity should increase, the slope should increase, and the boundary shear on the wall should increase. A range of  $Q_s$  and a range of  $Q$  will permit a plot to be made of wall shear as a function of  $Q_s$  for different constant values of  $Q$ . Another set of curves can be plotted for the slope as a function of  $Q$  and  $Q_s$ .

Note that in the above “mind experiment” the width of the flume is constant. Although we may be certain that if the wall was erodible, it would erode, and the higher the shear, the more it would erode, more experimentation is needed to find out how much more. More flumes of different widths are needed. Repeating the set of experiments permits one to go from one set of curves to another to find the same value of bank (wall) shear for the same values of  $Q$  and  $Q_s$  but different values of width and slope—or of the same values of width and  $Q$  and slope, but different values of width and  $Q_s$ .

The overall slopes of streams do not ordinarily tend to change markedly with discharge or time intervals of the order of the life of a structure. The bed of a stream is more likely to rise as a result of the mouth extending farther into the sea than by the slope increasing with the width of the stream. (Many things can happen, however—a few predictable, most bewildering.)

What would be apparent from these pages of families of curves is that the stable width of a channel is dependent on the discharge, the sediment load, and the erodibility of the banks. No optimums, no maximization of sediment load, no minimization of energy loss are evident. Simply that an increase in any of the independent variables, discharge, sediment load, bank erodibility (not strength, erodibility) causes a certain wide stable channel.

All of those flumes (channels) should be wide and should not be small; runs should be long, not short; measurements should be made with care, and should be repeated. An actual

project like this would cost a pretty penny and would take a considerable time. (I have neither, and besides I am sure I know qualitatively what the result would be.)

Computer computations (not computer models, of which there are really none) can be used to find these pages of families of curves. The primary difficulty here is not the time and money, but the selection of equations: What equation to use for  $Q_s$ ? What equation for the bank shear? All equations in the literature are not acceptably good approximations of reality. For example, the Einstein-Brown relation for sediment load was originally presented in *Engineering Hydraulics* in 1950 expressly to demonstrate that the data used by Einstein did not track his formula except for very low rates of transport—hardly a recommendation for the use of either Einstein’s formula or what has become known as the Einstein-Brown relation. (The same can be said for the Kalinske-Brown relation.)

There are at least three areas where our knowledge is not sufficient to attain a satisfying, reliable solution to the problem at hand:

1. The configuration of the bed, the dune size, the  $n$  value.
2. The sediment load and character of the bed material at different flows and times (ignoring the fine, transitory particles is a partial solution).
3. Evaluation of the erodibility, or resistance to erosion, of the bank material.

Nevertheless, pages of families of curves such as described can be used as a guide to interpreting what can be seen on old aerial photographs.

As for mass failure of banks (landslides, slumping banks), they can be spectacular—even frightening if one is watching while standing at the top of a nearby bank. But that slumped material must be removed by the flow if there is to be a further widening by collapse after collapse of that same piece of bank. Thus the limiting, stable width is a fluvial problem, not a soil stability problem.

## Closure by Robert G. Millar<sup>5</sup>

The discussers are to be thanked for their interest in the paper. A number of issues have been raised, including several that require no further comment. In the interest of brevity, I will address four key issues: (1) Presence or absence of any optimum; (2) equations and solution technique used in the model; (3) fluvial erosion versus mass failure; and (4) limitations and topographic errors.

### OPTIMUM OR NOT?

The most significant issue raised by Laursen’s “mind experiment” is the absence of any evidence for optima, maximization of sediment load, or minimization of energy loss. His argument suggests that, given adequate equations to describe flow resistance, sediment transport, and bank erosion, a completely deterministic solution for the limiting stable geometry would be possible, without recourse to an optimization scheme. This raises an important and fundamental question regarding the basis of our approach.

<sup>4</sup>Prof. Emeritus, Dept. of Civ. Engrg. and Engrg. Mech., Univ. of Arizona, Tucson, AZ 85721.

<sup>5</sup>Asst. Prof., Dept. of Civ. Engrg., Univ. of British Columbia, Vancouver, BC, Canada V6T 1Z4.

Let us continue with the mind experiment, firstly by holding the slope of the channel constant. Consider a channel that is initially narrow and deep with unstable, erodible banks. Over time the banks will erode, and the channel will become progressively wider and shallower until the limiting bank stability is reached. After the limiting bank stability is reached, no further widening will occur. The channel would now be considered stable, and corresponds to the minimum stable width. Now, imagine that the channel is manually widened further through excavation of the banks, so that it becomes wider and shallower than the self-formed minimum stable width. For this overwidened channel, the banks would remain stable; however, the sediment transporting capacity would be less than for the minimum stable width. Thus, for a specified slope, the maximum sediment transporting capacity develops at the minimum stable width.

If the channel slope is also permitted to adjust in order to accommodate an imposed sediment load, the slope that develops at the minimum stable width must also correspond to a minimum. The overwidened channel would require a steeper slope in order to maintain the same sediment transporting capacity. The wider and shallower the channel, the steeper would be the required slope. Viewed in this way, the minimum stable width can be considered to represent an optimum that corresponds to either the maximum sediment transporting capacity or the minimum slope, depending upon whether the slope is fixed or free to adjust.

An optimization routine is required only if the bank angle is permitted to adjust. In Laursen's mind experiment (and the above continuation) the bank angle was assumed to be fixed at vertical. With the bank angle fixed, the minimum stable width can be readily defined, and as Laursen has indicated, the limiting geometry can be determined without recourse to optimization. However, if the bank angle is permitted to vary, then definition of the minimum stable width becomes less clear. A narrow and deep channel with unstable vertical banks can often be rendered stable by reducing the angle of the banks, as opposed to widening. When the bank angle can vary as in our formulation, there is no readily definable minimum stable width, and a solution can only be obtained using an optimization routine.

## SELECTION OF EQUATIONS

Both discussers raise issues regarding representativeness of the specific equations used in the model formulation, their ability to adequately describe processes such as bed material transport and flow resistance, and the use of alternate equations and solution techniques. The basic idea underlying our work is that self-formed alluvial channels develop an optimum geometry that is constrained by discharge, bank stability, and sediment supply. We consider this optimum to be equivalent to the equilibrium or regime geometry. Model formulation requires equations to describe and characterize flow resistance, bank stability, and sediment transporting capacity. There are many existing equations that could have been used. The Einstein-Brown transport relation has been used to model bedload transport only because it is simple and continuous. Under certain conditions and ranges it is probably just as valid as other relations. Bui mentions the Yang (1984) and Parker (1990) transport equations. These could also have been used, although the Yang equation contains a critical stream power term, which introduces some difficulty to the optimization scheme. Incidentally, reformulation of the model using the Parker (1990) relation is currently under way in order to investigate logging impacts to gravel-bed rivers including the adjustment of armor layer composition.

Laursen questions whether any of the currently available equations for sediment transporting capacity, bank erodibility,

or hydraulic roughness are acceptable approximations of reality. There is of course scope for significant advances to be made in each of these areas of hydraulics. As our understanding increases and new relations are developed, these can be incorporated into the same optimization framework that we have proposed.

Bui also demonstrates that alternate optimization schemes including those now standard in commercial spreadsheet packages can be readily used to obtain a solution. The basic requirements are the same. A set of design variables, constraints related to the flow resistance, discharge capacity and bank stability, and an objective function, in this case the maximization of bedload transport. The output will yield a solution for the dependent variables.

## FLUVIAL EROSION VERSUS MASS FAILURE

Laursen is emphatic that the limiting width is determined solely by bank erodibility and fluvial erosion, and not shear strength and mass failure. Our formulation states that for a channel to be considered stable, the banks must be stable with respect to both fluvial erosion and mass failure processes. That is, the solution must satisfy both the bank-shear and bank-height constraints. Returning again to the mind experiment, consider an initially narrow and deep channel where the shear stress acting on the banks is less than critical, but the bank height exceeds the maximum stable height. The banks would be stable with respect to fluvial erosion, but would be liable to fail through mass failure. Clearly, the channel cannot be considered stable. We have assumed in our formulation that any material that slumps into the channel will be eroded and removed by the flow.

## TYPOGRAPHIC ERRORS

Bui has raised two instances of typographic errors and limitations. He claims that there is an error in (7) in the formulation of dimensionless bed-load transport rate per unit channel width,  $g_b^*$ . The formulation as it appears in the paper is correct;  $g_b^*$  can be configured with unit transport rate in terms of volumetric, mass and weight units:

$$g_b^* = \frac{q_s}{F_1 \sqrt{(s-1)gD_{50}^3}} = \frac{g_b}{F_1 \rho s \sqrt{(s-1)gD_{50}^3}} = \frac{i_b}{F_1 \rho g s \sqrt{(s-1)gD_{50}^3}}$$

where  $q_s$ ,  $g_b$ , and  $i_b$  = transport rates per unit width by volume ( $m^2/s$ ), mass ( $kg/s/m$ ), and weight ( $N/s/m$ ), respectively.

Bui also questions the value of parameter  $F_1$  for temperatures significantly different than 16°C. According to the Rubey equation used to calculate  $F_1$  (Vanoni 1975, p. 169), the value of  $F_1$  for gravel sediment ( $D > 2$  mm) is not affected by slight temperature-induced variations in viscosity. In practice, a value of  $F_1 = 0.82$  is appropriate for gravel sediment regardless of water temperature.

## APPENDIX I. REFERENCES

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## APPENDIX II. NOTATION

The following symbols are used in this paper:

- $i_b$  = bedload transport rate per unit channel width by weight ( $N/s/m$ ); and  
 $q_s$  = bedload transport rate per unit channel width by volume ( $m^2/s$ ).

# LIMITATIONS AND PROPER USE OF THE HAZEN-WILLIAMS EQUATION<sup>a</sup>

Discussion by B. A. Christensen,<sup>2</sup>  
Member, ASCE

The author has concluded that the usage of the Hazen-Williams formula should be strongly discouraged. The discussor is indeed in agreement with the author, as will be seen from the following analysis. However, a range does exist where this formula may be applied, but most practical pipe flows are outside that range that will be defined by the analysis. Most hydraulic engineers are applying the Hazen-Williams formula in pipe flow ranges where it is not valid at all. To make the formula give reasonable results in these ranges, researchers have introduced  $C$ -values as functions of pipe diameter and equivalent sand roughness. Even the author has followed this line of thought by using the empirical Colebrook and White formula for the friction factor in his analysis. This formula covers all three turbulent ranges as shown in Fig. 6. The transition range is indicated by the following range of wall Reynolds numbers

$$3.29 < R_w = v_f \epsilon / \nu = 70 \quad (10)$$

where  $v_f$  = friction velocity =  $\sqrt{g R_h S}$ . While the upper limit of this range is based on Nikuradse's experimental data, the lower limit may be found by intersecting the lines representing smooth and rough ranges. The equations of these lines are shown in Fig. 6. See Schlichting (1955) or a later edition.

The Hazen-Williams formula is a power formula of the form

$$V = K R_h^a S^b \quad (11)$$

where  $a$  and  $b$  are dimensionless exponents.  $K$  is a constant that must depend on the kinematic viscosity and acceleration due to gravity alone in the smooth range where all roughness elements are completely covered by the viscous sublayer and therefore have no or only minor influence on the turbulent flow and the friction factor. In the rough flow range all roughness elements are protruding into the turbulent flow and the equivalent sand roughness together with the acceleration due to gravity will have influence on  $K$ . In this flow range the Manning formula usually serves as a good approximation in pipes as well as in open channels.

In the transition range the thickness of the viscous sublayer is decreasing with increasing values of the Reynolds number, exposing more and more the roughness elements until the rough range is reached. By and large the transition range is relatively short. It may therefore be safe to extend Nikuradse's formula for the friction factor in the rough range across the entire transition range.

A logarithmic regression analysis of Nikuradse's formula for the friction factor in the smooth range yields

$$f = 0.1079/R^{0.16} \quad (12)$$

which, between  $R = 10^5$  and  $R = 10^8$ , approximates the original Nikuradse formula's  $f$ -values with a maximum error of 5%. This corresponds to a maximum error on the prediction of the cross-sectional velocity not exceeding 2.5%. Since Nikuradse's friction factor formula may be derived from Prandtl's classic mixing length theory [Schlichting (1955) or

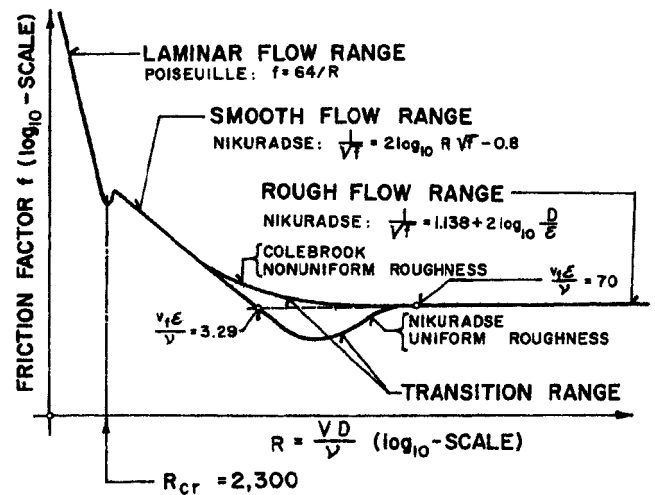


FIG. 6. Friction Factor  $f$  as Function of Reynolds Number  $R$  for Constant Relative Roughness  $\epsilon/D$

a later edition] it may be safe to apply (12) beyond the original experimental range of  $4 \cdot 10^3 < R < 3.2 \cdot 10^6$ .

Introduction of (12) together with  $D = 4R_h$  in the Darcy-Weisbach equation and solving for the spatial mean velocity  $V$  yields

$$V = 11.71 (g^{0.54} / \nu^{0.087}) R_h^{0.63} S^{0.54} \quad (13)$$

to be used in any consistent unit system.

The exponents  $a = 0.63$  and  $b = 0.54$  clearly indicate that (13) must be the Hazen-Williams formula. The constant  $K$  in (11) is seen to depend on kinematic viscosity and acceleration due to gravity as expected. It is not a function of pipe size or roughness. In Fig. 7 the shaded area indicates where (13) is applicable. This diagram was developed as follows. The upper limit of  $R_w$  in the smooth range is 3.29. It may be written

$$3.29 = R_w = v_f \epsilon / \nu = (v_f \epsilon / \nu) (V/V) (D/D) \quad (14)$$

or

$$3.29 = (VD/\nu) (v_f/V) (\epsilon/D) = R (v_f/V) (\epsilon/D) \quad (15)$$

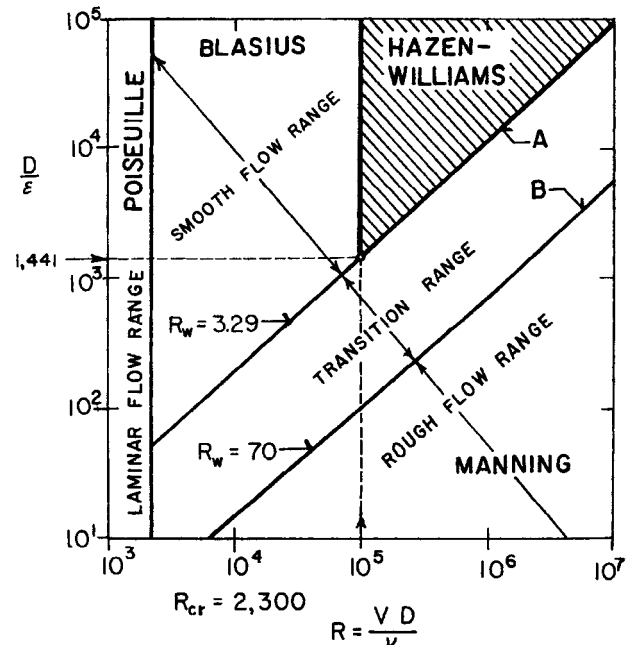


FIG. 7. Validity Range of Hazen-Williams Formula

<sup>a</sup>September 1998, Vol. 124, No. 9, by Chyr Pyng Liou (Paper 16176).

<sup>2</sup>Prof. Emeritus, Univ. of Florida, 10826 NW 15th Place, Gainesville, FL 32606-5455.

in which

$$v_f/V = \sqrt{f/8} \quad (16)$$

Hence

$$3.29 = R\sqrt{f/8}(\epsilon/D) \quad (17)$$

or by introducing Nikuradse's formula for  $f$  in the rough range and solving for  $R$

$$R = 9.306[1.138 + 2 \log_{10}(D/\epsilon)](D/\epsilon) \quad (18)$$

The similar equation for the beginning of the rough range, i.e., for  $R_w = 70$ , is found in the same manner to be

$$R = 198.0[1.138 + 2 \log_{10}(D/\epsilon)](D/\epsilon) \quad (19)$$

Eqs. (18) and (19) are plotted in Fig. 7 as lines A and B, respectively. The area between these two lines represents the transition range with the smooth range located above line A and the rough range below line B.

The range where (12) is valid, i.e., from  $R = 10^5$  to  $R = 10^8$  and above line A is shaded in the figure. The Hazen-Williams formula should not be used outside this area. For  $R < 2,300$  laminar (viscous) flow prevails. Here the Hagen-Poiseuille formula should be used. Below the  $R_w = 70$  line the Manning formula applies.

From Fig. 7 it appears that the smallest pipe diameter allowing the use of the Hazen-Williams formula (13) is

$$D = 1.441\epsilon \quad (20)$$

A roughness of  $\epsilon = 1$  mm is quite common in aged pipes. This corresponds to a minimum pipe size of 1.441 m, a substantial pipe size. Introducing a  $K$ -value that is a function of  $D = 4R_h$  will change the exponent  $a = 0.63$ , and the formula will lose its identity as the Hazen-Williams formula. Considering the Hazen-Williams formula's popularity among consulting engineers, there can be no doubt that the formula has been used in by far smaller pipes in the past.

The discussor must therefore agree with the author's conclusion that usage of the Hazen-Williams formula should be strongly discouraged. An exception, of course, is if the flow falls in the shaded area of Fig. 7.

## APPENDIX I. REFERENCE

Schlichting, H. (1955). *Boundary layer theory*. Pergamon, New York, 41–421.

## APPENDIX II. NOTATION

The following symbols are used in this paper:

- $a$  = exponent;
- $b$  = exponent;
- $K$  = constant;
- $R_w = v_f \epsilon / \nu \equiv$  wall Reynolds number; and
- $v_f = \sqrt{gR_h S} =$  friction velocity.

## Discussion by Frederick A. Locher<sup>3</sup>

With the proliferation of user-friendly computer programs for solution of pipe resistance and network analysis problems, the author's reminder of the limitations of the Hazen-Williams formula is certainly in order. It is all too easy to select a value of the Hazen-Williams  $C$  from a table without recognizing the limitations of an empirical equation that is valid over a rather

limited range of Reynolds numbers and relative roughness and whose application can often lead to ludicrous results.

The author's method for determining relative roughness from the Hazen-Williams  $C$  is indeed awkward, as he has stated in the paper. Diskin (1960) plotted the Hazen-Williams formula on the Moody diagram in a manner that clearly shows the relationship among  $C$ , the Darcy-Weisbach  $f$ , the Reynolds number  $R$ , and the relative roughness  $\epsilon/D$  as shown in Fig. 8.

The introduction of the equivalent sand grain roughness,  $\epsilon$ , in the author's equations (4) and (5) is an artifice that should be avoided, since it brings in a dependence on  $\epsilon$  that is not there and obscures the true nature of the Hazen-Williams formula. Instead, the relationship among the variables  $C$ ,  $f$ ,  $R$ , and  $\epsilon/D$  should be solved for  $f$  without the artificial introduction of  $\epsilon$ . Following Diskin (1960), the author's equation (7) can be solved for the Darcy-Weisbach  $f$  as given for SI units only in

$$f = \frac{133.84}{C^{1.85} R^{0.148} D^{0.0185} \nu^{0.1481}} \quad (21)$$

Since the Hazen-Williams formula is valid for water only, we can take the kinematic viscosity,  $\nu$ , equal to  $1.133 \times 10^{-6}$  m<sup>2</sup>/s, as the author did, and arrive at

$$f = \frac{0.201(100/C)^{1.85}}{R^{0.148} D^{0.0185}} = \frac{K}{R^{0.148}} \quad (22)$$

$$K = \frac{0.201(100/C)^{1.85}}{D^{0.0185}} \quad (23)$$

Eq. (22) indicates the true nature of the Hazen-Williams formula. It is nothing more than an exponential relationship between the Darcy-Weisbach  $f$  and the Reynolds number  $R$ , with the factor  $K$  being a constant for a given pipe diameter and roughness as expressed by the value of the Hazen-Williams  $C$ . For a given pipe diameter and  $C$ , the Hazen-Williams formula plots as straight lines on the Moody diagram, as shown by Diskin's (1960) Fig. 2, reproduced here with minor modifications as Fig. 8.

The discussor believes that Diskin's (1960) plot (Fig. 8) supplements the author's Figs. 1–4 and helps clarify the limitations of the Hazen-Williams formula. Diskin's (1960) plot clearly shows the following:

1. The sloping lines plotted for the Hazen-Williams formula, (22), indicate that this empirical expression was derived from experimental and field data taken in the transition range between laminar and fully rough turbulent flow. The Hazen-Williams formula is really valid only in the range of Reynolds numbers and  $C$  where the plotted lines are more or less parallel to the curves of constant  $\epsilon/D$  on the Moody diagram. It should also be noted that the line of demarcation between the transition region and full rough turbulent flow given by the author's (6) should be attributed to Rouse (1942) and not Moody (1944).
2. The factor  $K$  is an attempt to account for the relative roughness, since as the pipe diameter,  $D$ , increases for a given  $C$ , the Hazen-Williams curves move toward the smooth pipe curve. Note that as one follows the Moody diagram in Fig. 8 for  $\epsilon/D = 0.001$ , all of the curves for relative roughness between  $\epsilon/D = 0.001$  and the smooth pipe curve become very close to each other in the range of Reynolds numbers where the Hazen-Williams formula truly applies. This means that it is difficult, if not impossible, to derive an accurate determination of  $\epsilon$  from data taken in the transition range of Reynolds numbers. Perhaps the author could show his plot to estimate the

<sup>3</sup>Chf. Hydr. Engr., Bechtel Corp., P.O. Box 193965, San Francisco, CA 94119-3965.

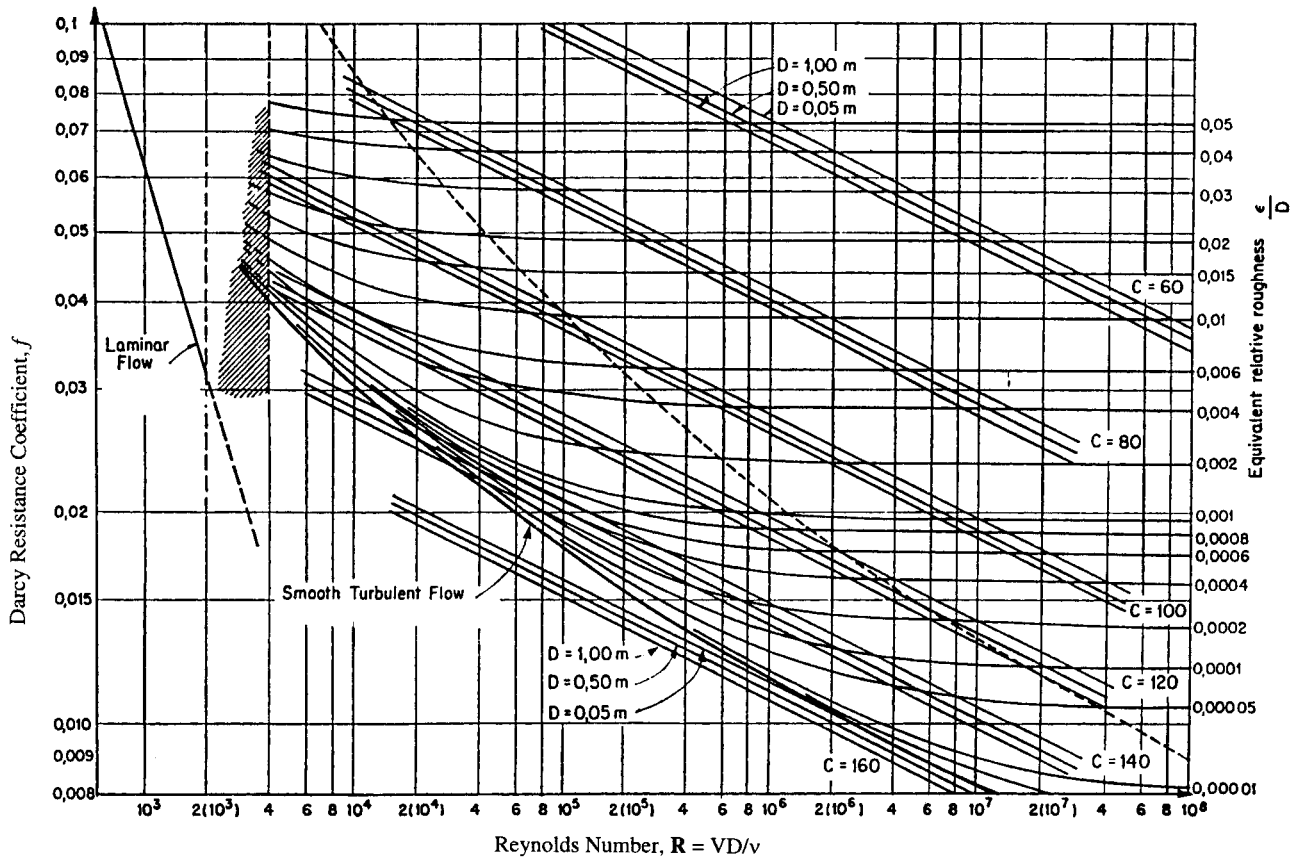


FIG. 8. Hazen-Williams Formula Plotted on Moody Diagram (Diskin 1960)

pipe roughness from which he obtained the value of  $\epsilon = 0.0003$  m by a least-squares fit so that the reader can judge the scatter in the field data.

3. The Hazen-Williams formula is not valid for values of  $C$  less than 100. As can be seen from Fig. 8, any field test data that result in a value of  $C$  less than 100 really are valid only at the point at which the data were taken. For example, suppose that one derives a value of  $C$  of 80 from a field test of an old water main, and then makes the erroneous assumption that this value of  $C$  is characteristic of a certain age and type of pipe. Applying this value of  $C$  to the same pipe with a greater flow rate than tested not only gives a wrong answer, but the head losses determined with this value of  $C$  are not conservative. By using a constant value of  $C$ , the pipe has become relatively much smoother!

As a final note, the discussor was involved with determination of capacity of a 2-m diameter gravity flow stormwater line conveying  $19 \text{ m}^3/\text{s}$  in a very flat area where pipe resistance losses were the critical factor in the calculation of capacity. The pipe was lined with a plastic liner that the manufacturer insisted had a Hazen-Williams  $C$  of 155. A plot of the operating point in Fig. 8 showed that the value of Darcy-Weisbach  $f$  for this condition lay below the smooth pipe curve on the Moody diagram. What wonders can be achieved with empiricism!

The author's conclusion that the usage of the Hazen-Williams formula should be strongly discouraged is therefore heartily endorsed. With modern computers, there is absolutely no sense in carrying the empiricism of the late 19th and early 20th centuries forward into the 21st century. There are enough difficulties in determining pipe resistance losses without the relationship used in the calculation of these losses also being part of the problem.

## APPENDIX. REFERENCES

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- Rouse, H. (1942). "Evaluation of boundary roughness." *Proc., 2nd Hydr. Conf., Univ. of Iowa Studies in Engrg. Bull. 27, Univ. of Iowa Publ. 1943*, J. W. Howe and H. Rouse, eds., 105–116.

## Discussion by Prabhata K. Swamee<sup>4</sup>

The author has to be congratulated for demonstrating the inaccuracies of the Hazen-Williams equation. Not only inaccurate, the Hazen-Williams equation is conceptually incorrect. Eq. (1) as written for pressure drop  $\Delta p$  is

$$\Delta p = \frac{6.824 \rho g L V^{1.85}}{C^{1.85} D^{1.167}} \quad (24)$$

where  $\rho$  = fluid mass density. Eq. (24) leads to the illogical inference that in pipe flow the pressure drop depends on the gravitational acceleration. On the other hand, the corresponding form of the Darcy-Weisbach equation (2) is

$$\Delta p = \frac{\rho f L V^2}{2D} \quad (25)$$

where  $f$  can be obtained by the following equation valid for  $0 \leq R \leq 10^8$  covering the laminar flow, the transition, and the turbulent flow (Swamee 1993):

<sup>4</sup>Prof. of Civ. Engrg., Univ. of Roorkee, Roorkee 247 667, India.

$$f = \left\{ \left( \frac{64}{R} \right)^8 + 9.5 \left[ \ln \left( \frac{\varepsilon}{3.7D} + \frac{5.74}{R^{0.9}} \right) - \left( \frac{2,500}{R} \right)^6 \right]^{-16} \right\}^{0.125} \quad (26)$$

Eqs. (25) and (26) indicate that  $\Delta p$  does not depend on  $g$ .

The dimensions of  $C (=L^{0.37}T^{-1})$  are not reflected in the paper. Instead the author gave two values of the coefficients associated with  $C$ . The dimensionally consistent form of (7) is

$$C = 4.1(g/f)^{0.54}(\nu R)^{-0.08}D^{-0.01} \quad (27)$$

Similarly, the dimensionally consistent form of (9) is

$$\frac{\varepsilon}{D} = 3.7 \left\{ \exp \left[ -\frac{0.3116}{g^{0.5}} \left( C^{10} R \nu \right)^{0.0926} \right] - \frac{5.74}{R^{0.9}} \right\} \quad (28)$$

It is ironic that in spite of the awkward dimensions of  $C$ , and a limited data base, the Hazen-Williams equation is the most popular pipe flow resistance equation among the users. On the other hand, the Darcy-Weisbach equation, which involves physically conceivable resistance parameters  $\varepsilon$  and  $\nu$ , and has an extensive data base, has remained confined mostly to academicians.

## APPENDIX. REFERENCE

Swamee, P. K. (1993). "Design of a submarine oil pipeline." *J. Transp. Engrg.*, ASCE, 119(1), 159–170.