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Orthotropic sandwich plates

Part 1: Dynamic relaxation treatment

Part 2: Analysis and application to multicell voided bridge decks

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Part 1 of the Paper presents a dynamic relaxation solution for the small deflexion behaviour of rectangular orthotropic and isotropic sandwich plates of uniform or varying cross section and subjected to any system of normal loading. The necessary theory for the treatment of any additional in-plane loading is also included. Any combination of simply supported fixed or free edges may be specified and advantage may be taken of symmetry about either or both centre lines when it exists. There is provision in the program for the inclusion of point supports so that multispan plates can also be treated. The accuracy of the method has been verified by comparison with existing solutions. A discussion on the numerical stability and convergence of the dynamic relaxation process in general is also included in the Paper with particular emphasis on the concept of pseudo densities.

The dynamic relaxation computer program is used in Part 2 to obtain the solutions for a number of normally loaded plates, both orthotropic and isotropic, for which solutions by other methods are already available. The present solutions have been shown to be in excellent agreement with the existing ones. The application of the sandwich plate analogy to the solution of certain other plates or plate-like structures undergoing appreciable vertical shearing deformations, viz. homogeneous thick plates and multicell and voided decks, is then discussed. Comparison of the test results given in reference 21 for a multicell deck (under patch loading) with the numerical results obtained by the present theory shows a very encouraging agreement.

For written discussion.

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A similitude framework of regime theory

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The Authors have been contributing forcefully and effectively to the inductive approach to quasi-steady flow, in uniform channels, of a mixture of bed-load material and water; their aid in restoring the necessary tool of dimensional analysis has been considerable. This approach demonstrates that popular engineering misunderstanding of channel transport is comparable to that of simple pipe flow before proper dimensionless statement of the case made necessary the plotting of data in the friction factor (Stanton) diagram, which showed different phases requiring different detailed formulations. However, transport phases are visible and more numerous and require a couple of independent equations each. I would respond to the Authors' direct and implied questions by giving an outline of recent similar work in North America, co-ordinated by the kind of 'logical model', or statement of a prototype case,⁴⁷ requested in § 33. Earlier original basic work must be mentioned for completeness and because recent advances use and re-interpret it.

54. Since the end of the last century, irrigation engineers in the Indo-Gangetic plan, geologists (mainly connected with the US Geological Survey), and associated laboratory workers have studied different aspects of this problem. The first two groups have followed the classic inductive approach of physical science by ascertaining facts, organizing them, correlating measurable variables associated with the phenomena, and leaving theories till later. The engineers concentrated, originally, on quantitative field matters, from the viewpoint of canal self-formation; the geologists concentrated on qualitative river ones supplemented by laboratory flume experiments that involved measurements. Their co-operation has increased over the years, and the latter have enlisted engineers working in laboratories at universities. Neither can measure bed-load transport effectively as a routine, in the field, and both realize the limitations of the laboratory. The third group has concentrated on measuring bed-load transport in laboratory flumes and has been influenced appreciably by theories based on the ideas and approximations of rigid boundary hydraulics of Newtonian liquids. Its laboratory-based formulae have appeared, in general engineering literature, to be almost incredibly divergent from field findings of the first two groups and from each other. Fig. 4 shows 100 times range in forecasts of bed-load transport according to well known laboratory based formulae. Fig. 5 shows how a regime-theory formula produces a slope analysis chart for sand rivers and large gravel ones within a 4 times bandwidth that allows for meandering and moderate bed-load intensities.

55. A 'prototype case'⁴⁷ for canals and flumes is now stated. A steady volumetric discharge Q of Newtonian fluid with kinematic viscosity ν and mass density ρ is diverted into a long horizontal rectangular flume of breadth b , whose sides behave as if they were hydraulically smooth. A granular material of fixed constitution is added to this discharge at a fixed rate, or 'charge', C , units of weight discharge per unit of fluid weight discharge. The granular material is selected with a constitution such that, when it has formed a bed, it will all move in the rolling and/or saltating manner recognized as *bed-movement*. Its median size is D and the mass density of every particle is ρ_s ; shapes and the distribution of sizes and shapes are susceptible to definition by a host of non-dimensional factors represented by X . The motion takes place in the earth's gravitational field, g .

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Fig. 4. Sediment discharge against water discharge

Adapted from Vanoni, V. A., Brooks, N. H. and Kennedy, J. F. Report KH-R-1, Caltech, January 1961.

56. When, and only when, all the above factors have been assumed, the channel will form a granular bed and adjust itself to have a unique space-mean depth d , and a unique energy degradation rate of gS per unit mass per unit length along the flow (corresponding to a surface slope S). There will be, also, a definite mean flow velocity V and, under many circumstances, bed waves of mean wavelength and amplitude λ , α .

57. Note that the same problem could have been stated from different *equivalent* viewpoints that would interchange dependent and independent variables; the present viewpoint is for sediment injection, but it might have been changed to suit circulation.

58. The stated consequences of the preceding experiment are, for five independent physical relations, covered by equations of the general type

$$d, gS, V, \lambda, \alpha = \text{fns} (Q, \nu, \rho, b, C, D, \rho_s, X, \dots, g) \quad (40 \text{ a, b, c, d, e})$$

Fig. 5. Specific slope classification chart

Defining b and d so that $Q = Vbd$ allows Q to be replaced by V and equation (40c) to be eliminated, then conventional dimensionless reduction^{45, 48} can yield the four independent relations

$$V^2/gd, V^2/gdS, \lambda/D, \alpha/D = \text{fns} [\sqrt[3]{vg} \cdot D/\nu, C, d/D, b/d, s, X \dots] \quad (41 \text{ a, b, c, d})$$

The prototype case and the set of numerics (dimensionless parameters) arose in regime-theory analysis⁴⁹ of the classic Gilbert data. The neglected velocity, $\sqrt[3]{vg}$, which is as old as the Froude Number, will be called 'Vig' (for viscosity-gravity), and the Reynolds number formed from it and particle size is then the 'particle Vig number'. For water, $s = \rho_s/\rho$ is the solid's specific gravity and $s-1$ would be the buoyant specific gravity. King's regime-theory slope equation^{23, 24, 50} calls for manipulation of the preceding equations (or restatement of the case to reduce algebra) into

$$V^2/gdS = \text{fn}(Vb/\nu, C, d/D, b/d, s, X \dots) \quad (42)$$

Further manipulation or restatement can replace gdS by $V_* C$ by $p/(\rho g V_* D)$ and the Vig number by $V_* D/\nu$ to suit Yalin's principal system.⁴⁵ Simons, Richardson and Albertson⁵¹ stated a broader prototype case, taking account of suspended load, and used yet another set of numerics. Settlement velocity can be introduced in

$$V_* = \text{fn}(D, \rho, \rho_s, \nu, g, x' \dots) \quad (43)$$

Of course, no amount of algebraic manipulation can alter the fact that, for a given case, there must be a certain number of independent equations containing a certain number of independent numerics, in general. All that is altered is the appearance of statements of this fact.

59. Clearly, an engineer wishing to design a canal with rigid sides must know the two specific equations corresponding to equations (41a) and (41b); so he will visualize channel behaviour in terms of two degrees of freedom^{23, 24} against that of a rigid channel without sediment. (The canals of regime theory had three degrees of freedom because breadth was self-adjusting.) Clearly, also, the task of formulating, or plotting, the data from laboratory flume or field canal observations is seven-dimensional, in general (counting $X \dots$, which might be called 'texture' as one for convenience); so success would be unlikely unless circumstances permitted only a couple of the independent variables to operate at one time. Here regime-theory canals had a tremendous advantage. Nature provided a bed-material of standard log-normal particle size distribution, standard grain shapes and fixed specific gravity, thereby fixing $s, X \dots$. Within one canal system a 50-times discharge range would correspond to a b/d range a little over two times, with the smallest value of b/d probably about six; this almost eliminated the effect from b/d . Nature also provided a C small enough

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not to make its presence discoverable in analysis. Running conditions were such that analysis, at nominal full supply conditions of data collected by systems over decades, from hundreds of miles of channels that had had plenty of time to attain quasi-equilibrium (in-regime) conditions, was virtually for steady flow of fluid with fixed (average) viscosity and fixed (average) suspended load. Also the standard duned beds indicated one phase of behaviour. For these reasons the Lacey task^{14, 15} was to find two equations of the form

$$V^2/gd, V^2/gdS = \text{fns} [\sqrt[3]{(vg).D/\nu, d/D}] \quad . \quad . \quad . \quad (44)$$

and Nature kindly decided that d/D did not matter in the first. Fig. 6 indicates independence from d/D by the lower horizontal band.

60. Hoque⁵² made an elementary analysis of world-wide flume data⁵³ for F_{b0} at values of b/d greater than 5.0, as a thesis project auxiliary to full analysis of F_b and S by Cooper and Peterson.⁵⁴ (F_{b0} , called the 'zero bed-factor'^{23, 24, 50} is the bed-factor $F_b = V^2/d$ for $C \rightarrow 0$, and is practically equal to the original Lacey 'silt factor'¹⁴ f (§ 67).) He accepted $F_{b0} < 2.0 \times 10^{-5}$ to replace F_{b0} . Essentially his procedure was to plot F_{b0} against d/D for various values of other variables. Fig. 7, prepared after the thesis, shows bands enclosing points of some individual experimenters and adds, for interest, two bands for 'initiation of motion'. The bottom band was curved deliberately to run into the regime-theory phase, as its points were for flumes of small regime-theory canal size and bed-material type; scatter of points would have allowed a wider band paralleling the rest. The available data suffered major disabilities. b/d varied from 1 to 60. Bed materials were, generally, neither standard nor natural, so $X \dots$ was significant and indeterminate. 90% of D values lay between 0.2 and 5.0 mm, which is larger than the range of regime theory but, because of other defects, is inadequate for finding the effect of particle Vig number. Obviously the task was to find

$$F_{b0} = \text{fn} [\sqrt[3]{(vg).D/\nu, d/D, b/d, X \dots}] \quad . \quad . \quad . \quad (45)$$

The plots, taken at their face value, indicated F_{b0} varied as $(D/d)^{0.8}$, all other factors being constant, outside the regime-theory range.

61. Because of association among the independent variables the plots cannot be taken at face value. Thus, in any one flume with one D , b/d is proportional to D/d and could have a range of 10 times. The effect of this association on the discovery of a formula, or even on making a plot, can be visualized by imagining a surveyor had to prepare a map of a mountain area by measuring land elevation, $z(F_{b0})$, at points $x(b/d)$, $y(D/d)$ that covered the area fairly, but was told that he must never stray from a narrow band round the line $y = 3x$! The effect on the band slopes of Fig. 7 is to make them considerably steeper than the true situation. When *all the data* are analysed—which implies having several surveyors working on various lines $y = mx$ to cover the whole area—the association between b/d and d/D becomes small, but a very strong association between d/D and D arises. In fact $D \propto (D/d)^{0.6}$ roughly, on an average, though the index is larger for larger D . This makes the plots have steeper slopes than their true values, just as for the b/d against D association for single flumes. Algebraically, the $(D/d)^{0.8}$ of the last paragraph might be really $(D/d)^{0.8-n} \times D^{n/0.6}$ with n having any value. Individual experiments and field observations can suggest hypotheses about n (§ 62). Clearly future laboratory experiments require co-operative scientific planning on several accounts.

62. Remaining valuable information on subcritical F_{b0} is from Kellerhals⁵⁵ who performed a complete regime-theory type of analysis for the three degrees of freedom of gravel canals and gravel rivers emerging from lakes and having D up to 1 ft; beds were flat. Data were unavoidably very scattered, but b/d varied randomly between about 20 and 30, and the association between d/D and D was not strong. Kellerhals' work, being new and comprehensive, must be read in full. Depending on the method of measuring D he found that

$$gdS \propto D^{1+n} \quad . \quad . \quad . \quad (46)$$

Fig. 6. Sub-critical phases of F_{b0}

Fig. 7. Individual experimenters' bands (Hoque)

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where n was zero or $-\frac{1}{2}$; scatter permits other statistically significant values including small positive ones. Various well-known flow formulae fitted his data, but he elected to use the Lacey regime-theory type for deducing formulae in terms of Q . Solving it with equation (46) gives

$$F_{b0} \propto [(D/d)^{1/2}(\sqrt[3]{(vg).D}/v)^n \dots \dots \dots (47)$$

Now Shields⁵⁶ and Meyer-Peter,⁵⁷ who both corrected for the effect of b before plotting and were probably not in a position to suspect or find the effect of particle Vig number, used $n=0$. The Meyer-Peter beds appear to have been flat, while the state of the Shields ones is not clear. So there is a chance that the world-wide data examined by Hoque (§ 60) might have given band slopes, at least at the top, of -0.5 instead of -0.8 if there had not been error due to association among variables; at the bottom there would have to be a transition of slope to the horizontal of the regime-theory phase. Negative n implies that, as d/D decreases, the lines of constant D coming from the regime-theory phase into the laboratory flume one in Fig. 6 would have to cross each other; there seems no physical reason against this possibility. Regime-theory²⁴ suggests that n might be $\frac{1}{2}$ on an average for sands and $\frac{1}{4}$ for gravels. Replacing the particle Vig number by V_a^2/gD does not help in making a decision, as that term scatters widely and does not seem to have the average constancy approximated by spheres of gravel size over a fair range. The truth will have to await large-scale experiments.

63. World-wide flume data were collected by Peterson and Cooper,⁵³ and have been given a preliminary regime-theory type of analysis by Cooper and Peterson,⁵⁴ according to equations (40). The unbiased system of plotting finally published is shown in Fig. 8, where V^2/gd is the ordinate and C is the abscissa. Each graph is for a value (actually the mean of a small range) of d/D , and the points are marked distinctively according to phase. The work was repeated with distinctive markings for ranges of particle Vig number, then for b/d , and finally for bed material grading. This gave 23 graphs. Another 23 were prepared with S as ordinate. The association between d/D and D was plotted for later use in partial correlations. In general the V^2/gd plots show changes of slope corresponding to the different visible phases described by Simons and Richardson⁵⁸ up to and including anti-dunes but, of course, they cannot show the more striking subcritical phase change of F_{b0} of § 62, although they contain it. Careful study shows changes associated with the transition from sand size to gravel size, but the trifling effective range of D hides the post-transition. The S curves show obvious change only between pre-dunes and dunes. Later studies seemed to indicate that, for sands, the functional form of the King slope formula (equation (48)) of regime theory will apply for some distance into the small d/D phase of Fig. 6; perhaps (§ 67) the application is until wave forms vanish from the bed. Simons and Richardson⁵⁸ have shown phase-change zones on a chart for sand in flumes of small canal size, in irrigation canals, and in rivers; the co-ordinates are different from those above and can be derived from equations (41a) and (41b) by eliminating C (which is unmeasurable in practice). Maddock⁵⁹ has shown plot changes in the Gilbert and other data using yet another system of numerics. Blench and Erb,⁴⁹ who had no computer, failed to show phase effect within subcritical flow in the Gilbert data. Their F_b analysis was in terms of $(F_b - F_{b0})/F_{b0}$, because that quantity might depend predominantly on C , but they had to use the regime theory 'rough qualitative formula' for F_{b0} because the data were not suitable for extrapolating to the true value. However, the change between sub- and super-critical plots was striking. Rottner's analysis⁶⁰ is most valuable for $C \rightarrow 0$.

64. Ignoring the possibility of different formulae for different visible phases must

Fig. 8 (right). Location of visible phases between V^2/gd , C and d/D_{50} , for the Colorado State University Experiments

Legend				
Plot No.	d/D_{50}			Phase
	Min.	Max.	Mean	
a	100.0	250.0	183.8	Plane bed
b	250.0	400.0	335.6	Ripples
c	400.0	600.0	486.3	Dunes
d	600.0	1000.0	758.0	Transition and sheet flow
e	1000.0	2510.0	1265.0	Std. wave, antidune and chute pool

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be partly responsible for the wild discrepancies among the formulae of Fig. 4. It may have encouraged rather abstract attempts to prove the regime formulae from first principles by means that give an apparently universal answer.^{19, 61, 62} My own attempt,⁶¹ which was mercifully published in abstract, appears now as no more than a selection of the type of functional form that, when operated on by a standard dynamical process, gives the desired answer! Such exercises are useful. They clear and survey tentative paths to the distant objective of discovering the message in factually derived formulae with apparently reasonable dynamical forms.

65. Important dynamical aspects of the Lacey equations tend to be forgotten. One is that his original equations can be interpreted to make three simple dynamical statements, *for the proper phase*, to the effect that channels with a given water-sediment complex ($D, C, X \dots, \rho_B, \rho, \nu$ and some suspension) tend to adjust themselves to equilibrium, where

- (a) the Froude number in terms of a representative depth is the same for all;
- (b) the dissipation of energy per unit mass per unit time is the same for all;
- (c) a measure of erosive attack on the sides is $\mu \rho V^3/b$ if the sides are hydraulically smooth.

Another aspect is that Lacey dimensionally analysed all his results and used ν, g to rationalize his $P = 2.67 Q^{1/2}$ into $P/R \propto V/(\nu g)^{1/3}$. Yet another is that the original Lacey equations were not general enough, so had apparent defects that have since been removed^{23, 24} by splitting his 'silt-factor', f , into parts to allow for the bed and sides being different, and introducing b/d and C (rather crudely) into the slope equation. None of these actions has altered either the indices of measurable variables in his derived formulae containing Q or the statements (a)–(c) above. The incongruities in the original equations are:

- (a) one factor, f , to deal with duned noncohesive beds and unduned cohesive sides;
- (b) unreal trapezoidal channel dimensions beneath about 4 cusecs;
- (c) equivalence of a semi-elliptic section with the major axis horizontal and the half of the same ellipse with the minor axis horizontal;
- (d) unique channel dimensions for a given Q and water-sediment complex (although engineers alter dimensions of in-regime channels within small but appreciable limits and obtain new in-regime dimensions);
- (e) when the actual dimensions of a set of in-regime channels were inserted in various derived formulae the values of f obtained differed according to the formula used, and were functionally related to each other instead of being independent of the formula used;
- (f) the roughness of channels, according to the slope formula, increased slightly with D , whereas the actual roughness for a duned bed decreases as D increases.

66. It is a remarkable testimony to Lacey's formulae that these incongruities were removed by actions comparable to finding the secondary factor causing scatter in a fairly tight band of scattered points; for many major practical purposes his original formulae are adequate for engineering design if used in a suitable sequence and with the understanding that C is implicit in f .

67. In my opinion, the regime theory discovery most important to science was King's,^{23, 24} i.e. that the original slope formula should be modified to the form

$$V^2/gdS \propto (Vb/\nu)^{1/4} \dots \dots \dots (48)$$

which is that of Blasius for a *smooth* boundary. Detailed discussion is impossible here. The points that arise are as follows. (a) The form of the Blasius equation is now verified in the region of Reynolds number into which the Moody diagram is grossly extrapolated from one set of laboratory data by using the Prandtl velocity distribution formula which, according to Prandtl,⁶³ was not rigorously applicable to finite pipes

(and, therefore, to finite channels). Engineering texts omit both his warning and the engineering data that disagree with the extrapolation.⁶⁴ (b) Belief in a 'universal' (dimensionless) velocity distribution formula for all boundaries implies belief in a 'universal' flow formula, since the latter can always be obtained from the former by integration. The belief is strengthened after making the bed and side factors (replacing Lacey's f) explicit in King's equation, which then turns into a *rough-boundary* type with the equivalent roughness height decreasing as D increases (as it ought for dunes); that is, the regime-theory boundary is 'rough' and 'smooth' at the same time, depending on point of view. (c) 'Smooth' in rigid boundary hydraulics means that a phase of flow near the boundary behaves sufficiently like a viscous laminar flow to permit boundary resistance to be expressed in terms of the fluid viscosity regardless of the rigid boundary; in the regime-theory phase the bed zone is differentiated out of the water-sediment complex. (d) Prandtl and Tietjens⁶⁵ report that a 'wavy' boundary produces a Blasius type flow (or friction factor) formula, and Einstein⁶⁶ has demonstrated periodicity in the so-called laminar film along a rigid boundary. So it would seem that the Blasius type of formula applies when a boundary zone can be formed by periodic fluctuations generated within the fluid, or imposed by waviness agreeing with natural fluctuations. All the preceding matters suggest that an improved approach to rigid boundary problems would be via the knowledge gained from mobile-boundary hydraulics. The situation is analogous to the improved understanding of algebra in terms of complex numbers instead of their lopsided 'real' parts.

68. Full answers to the questions, implied and direct, of § 32 are inherent in the preceding discussion. My summarized replies to the specific queries are (a) See reference 24, Chap. 12. (b) Answer is probably in plots of reference 54. (c) No. See references 14, 15, 16, 23, 24, 50. (d) See reference 23, § 3.7; reference 24, §§ 7.3, 7.6, 14.21, and reference 50, which contradict the last sentence of the question. (e) The practical range of variables cannot be covered in a laboratory, and C cannot be measured in the field. A major advance can be made when engineers create and observe field conditions to fill present gaps in knowledge. The answer to § 29 is that Nature does not create all situations to exhibit dynamical similarity. If she did then *every* numeric in equations (40) would have to be fixed and, as only three different scales can be chosen arbitrarily (by a theorem of dynamical similarity^{45, 46}), all scales would be expressible in terms of those for g , ρ and ν . As the first two have to be unity, then all should be expressible in terms of ν , as equations (16)–(19) show. Those equations state that any two channels with the same viscosity must be identical if they are dynamically similar. In reality the different laws of bed and side erosion prevent the b/d scale from being unity and, therefore, destroy dynamical similarity completely. The wording of the related § 28 might mislead. Its first relation is an original Lacey one, since amended to contain bed and side factors (§ 65). The second and third are partial Lacey forms; the full ones contained his silt factor f , which has since been split into components (§ 65). The incomplete forms are quoted legitimately only for special reasons, for example by geologists correlating river dimensions with Q when they are not in a position to record or use the nature of the water-sediment complex. However, they are aware of the omission and try to group data to avoid ill effects. The indices they find depend on the phase observed and on possible statistical associations.

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The Authors have made yet another effort to fit the regime equations into a theoretical framework compatible with modern fluid mechanics. Probably most readers who have delved into the regime literature would endorse their remark that it contains an unusually high ratio of works discussing concepts to works presenting actual field data. A regime channel is conceived as one that has attained a stable cross section

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Fig. 9. Various best-fit width/discharge curves for rivers and canals (surface width against channel-forming discharge)

and slope in response to imposition of a steady or cyclic discharge of water and of sediment, the latter comprising both bed-material and wash load. The concept is unimpeachable, but when we examine the derivation of the classical equations from field data, the question arises: did anyone know the sediment loads of the channels whose dimensions were correlated to yield the equations? Despite some hedging by regime writers on this point, the answer seems to be negative. The Authors remark that 'transport is usually assumed to be in the ripple zone', but regime writers are by no means unanimous about this. Blench usually writes of 'dunes', and I have seen a photograph of an empty Indian regime canal whose bed was covered with very large sand waves.

70. In the face of these uncertainties about the complete specifications of a regime channel (in the sense of a channel that satisfies the equations), one wonders if attempts to derive the equations 'theoretically' are worth the effort, and if the attitude of the theoretician should not be 'so what?'—at least until the specifications of a regime channel are further clarified by field measurement of the missing factors. Is it not time to abandon the word 'theory', and simply take the regime equations as empirical formulae for the design of a certain type of channel in a certain kind of geographical and geological environment, channel type and environment both being somewhat loosely specified?

71. I have long thought that the most puzzling of the regime relations is the one that seems to have received the least attention, namely the width-discharge relation. Lacey's original form, derived from Indian canals, may be approximated (for all but very small channels) as

$$W_s = 2.67 Q^{1/2} \quad (49)$$

where W_s = water-surface width in feet, and Q = channel-forming discharge in cu. ft/s.

72. Later regime writers have attempted to improve on this patently empirical formula by introducing other factors dependent on channel boundary materials, etc., but have not convincingly demonstrated that their modifications improved the data correlations. Meanwhile equation (49) has been tested against various other sets of

stable channel data, including small laboratory streams,²⁷ British rivers,⁶⁷ gravel-paved rivers and canals,⁶⁶ large canals²⁶ and a heterogeneous set of rivers in Alberta (Neill, 1968, unpublished). These sets cover a range of channel-forming Q of from less than 1 to over 100 000 cu. ft/s (Fig. 9). Although best-fit coefficients for the various sets vary from 1.65 (Nixon) up to over Lacey's value, there is little reason to depart from Lacey's exponent of $\frac{1}{2}$, and the scatter in most plots is (considering the circumstances) remarkably small. Thus it seems reasonable to conclude, firstly, that the form of the Lacey equation expresses a kind of natural law for stable channels, and secondly, that the numerical coefficient is relatively insensitive to such factors as boundary materials, channel alignment, and suspended load.

73. In a previous paper,⁶⁹ I have argued that if both straight and meandering channels obey the same width-discharge law, then it is difficult to accept bank erodibility as the physical explanation. In most meandering channels, one bank is eroding while the opposite bank is being built up by a process of point-bar formation out of bed load, followed by colonization by plants and trapping of suspended load. If outer bank recession rates vary from point to point along the river, as they usually do to a marked degree, then inner bank construction rates follow suit. Now, it is usually supposed that the meandering process is inextricably linked with spiral circulations. Is it then a reasonable hypothesis that secondary circulations may also play a part in determining the stable width of straight channels? If so, a satisfactory theoretical explanation for the regime equations may not be found until the secondary circulations can be predicted.

74. Leopold and Maddock's data²⁸ on the hydraulic geometry of American rivers, referred to by the Authors, have not been quoted in this discussion, because the discharges used in their correlations were in general much too small to be regarded as channel-forming.

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Notation

A	coefficient of transport
G	coefficient of transport
C	Chezy coefficient
I	slope
L	dependent length
T	transport
h	water depth
g	gravitational acceleration
e	base of natural logarithms
n	scale prototype/model
r	radius of curvature
v	velocity
Δ	relative density ($\psi_{\text{mat}} - \psi_{\text{wat}}$)
μ	ripple factor

The argument that there is a theoretical justification for the uniqueness of indices of the regime equation is certainly not upheld when transport formulae are considered. It is known that many theoretical conditions for the model can hardly ever be entirely met. There will be deviations in the model from the ideal situation in general, as the model transport of a material size practically to be manipulated and which is readily available must be taken into consideration. The ideal situation is that in the model, the time scale for the changes in the bottom is always and everywhere constant.

76. The non-integrated form of the simplified form of the equation of motion

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along and perpendicular to a streamline gives the flow picture of a laboratory study⁷⁰:

$$\begin{aligned}\frac{1}{2} \frac{\partial v^2}{\partial s} + g \frac{\partial h}{\partial s} + g \frac{v^2}{c^2 h} &= 0 \\ \frac{v^2}{r} + g \frac{\partial h}{\partial n} &= 0 \\ \frac{v^2}{2g} \Big|_1^2 + h \Big|_1^2 + \int_1^2 \frac{v^2}{C^2 h} ds &= 0 \\ h \Big|_1^2 + \int_1^2 \frac{v^2}{gr} dn &= 0\end{aligned}$$

For a stationary situation both equations, after integration, are as follows:

$$\frac{v^2}{2g} \Big|_1^2 + \int_1^2 \frac{v^2}{gr} dn = 0$$

From the above equation it appears that the difference in height between points 1 and 2, the surface and bottom of the channel respectively, is given by $\int_1^2 (v^2/gr) dn$. From the non-integrated equations of motion it follows that the height scale is to be equal to the square of the velocity scale.

77. From the equation of the slope of water level perpendicular to the streamline it follows that the variations in depth must equal $\int (v^2/gr) dn$. From this follows as a demand for rendering the curvature on the length scale (a demand for affinity of the flow-picture) that the scale of variations in the height of the water level must be equal to the square of the velocity scale. If the non-integrated differential equation is used as a starting point, this means that height scale of the model is to equal the square of the velocity scale. However, if the variations in the height of the water level are small in comparison with the depth, the latter may be accepted as being constant, and the result will be the simpler demand mentioned above.

78. The changes in the height along the stream lines as a result of the Bernoulli factors in the equation of motion are also proportional to the square of the velocity. However, the difference in height between two points of an orthogonal line is also determined by the factors of resistance in the equations of both stream lines.

79. Then from the equation of motion along the stream lines, it follows that $n_L/n_B n_C$ must be 1. This demand can be complied with without having to lay obligations on the velocity scale. Should it be chosen in such a way that the scale of the slope no longer equals the distortion, then a special tilt may be given to the model except in the case of tidal models with opposite flow directions.

80. In § 79 it has been mentioned that the ideal situation would be that the transport scale on all points of the model should be the same for all the different velocities.

81. The Authors mention that their recent work sets out the restrictions inherent in the conventional bed-load formula, and although I agree with this, I feel that using the Kalinske-Frijlink formula as a starting point the transport can be fairly accurately calculated if it is done by using certain modifications derived from laboratory experimentation.⁷¹

82. The formula describes the transport as follows:

$$T = 5d(ughI) \exp(-0.27\Delta d/uhI)$$

where hI can be written as v^2/c^2 . Then the transport can be written as

$$T = 5dvC^{-1}\mu^{1/2}g^{1/2} \exp(-0.27C^2\Delta d/\mu v^2)$$

In order to analyse now the variation of the transport scale over the model, the proportion of the transport on the various points to the mean transport is studied. To do this various factors of the transport formula have to be modified, and a modified value can be arrived at:

$$T = Adv \exp(-G\Delta d/v^2)$$

Now this can again be modified for the point (imaginary if so) where for every quantity the average value holds

$$T = \bar{A} \bar{d} \bar{v} \exp(\bar{G} \bar{A} \bar{d} / v^3)$$

From this formula an equation between the ratios is deduced and consequent upon this, again an equation can be found between the scales in these ratios.

83. Then the ratio is written

$$n_T = n_A n_d n_v n_e$$

If now the variation in the transport in the model wholly equals the valuation in the prototype (the case of the ideal situation) n_T should be 1.

84. At the present level of knowledge and research, the developed methods have by no means reached the stage that the value of scale can be definitely judged. Using the regime theory gives the basis for calculating this transport movement, but careful laboratory work gives modified but more correct values to the indices and therefore a true answer. Much research has still to be done in the sediment transport field, but using the past work on regime concept as a basis should allow researchers to develop modified formulae more suitable to their respective problems.

Mr K. V. H. Smith, University of Southampton

The regime method, or theory, is intended to apply to systems of alluvial channels, that is, channels in which the fluid is always water and the bed material is normally grains of quartz. This is of course recognized by the Authors in § 27 of the Paper and elsewhere, but it is perhaps not made sufficiently clear that a part of the Paper, including equations (15)–(19), does not refer to such real alluvial channel systems. Reference is really being made to channel flows, over movable bed materials, in which complete similarity exists, and this in general requires different fluids and bed materials in each channel, in order that fluid viscosity and density ratios can be chosen to satisfy the similarity conditions.⁷²

86. A valuable part of the Paper is given in equations (24)–(26), which relate the empirical regime equations for alluvial canal systems to the equations for movable bed systems having complete similarity. These equations are stated to demonstrate the basis for a critical evaluation of the indices for depth and breadth. However, such an evaluation must still involve an empirical correlation of the available field data. These field data exhibit considerable scatter and, in addition, are usually lacking in one vital observation, namely, the total sediment discharge in the channel. It seems hard to believe that the revised evaluation procedure would produce anything much more conclusive than has been obtained previously. The Authors' fuller explanation of this point would be appreciated.

Saburo Komura, Associate Professor of Civil Engineering, Gifu University, Gifu, Japan

In § 41 the Authors stated that 'the regime equations can only be derived assuming charge remains constant'. This assumption means that the charge remains constant with respect to time. However, as stated in a paper,⁷³ the conditions on the regime and tractive force theories are as follows: the regime theory requires that the sediment load passing through a cross section of the upstream end be equal to that of all points along the channel; whereas the tractive force theory requires that the critical shear stress on the bed be constant at all points along the channel. Therefore, some discrepancies and incompatibilities arise from these two different conditions. I should be glad to have the Authors' explanation on the discrepancies and apparent incompatibilities.

DISCUSSION

Dr John Allen, Queen Mary College, University of London

The Authors have made a valuable contribution to the literature dealing with the complex subject of regime. In particular, the method of applying dimensional analysis to the problems of similitude between model and prototype is both rigorous and comprehensive.

89. It seems apparent, as the Authors state, that the indices derived by Lacey and others will not have universally constant values irrespective of the cross-sectional shape of the channel. However, where there is not excessive distortion of vertical scale in the model, formulae of the type given by Lacey are still capable of yielding reasonable agreement. I consider that the true values of the indices cannot be obtained by dimensional analysis alone, since they depend to a large extent on the velocity distribution and not simply on the mean velocity. In his paper on the subject, Lacey⁷⁴ compares two channels of different widths and suggests that the narrower one should be given an additional slope 'in order to preserve the correspondence of the velocities and the paths followed by the sediment particles with reference to the water'. Since there is no guarantee that the velocity distributions are in agreement, there appears to be no reason why the scour and deposition should correspond in the two cases.

90. Similarly the Authors do not appear to have attempted the difficult task of analysing the effects of the velocity distribution patterns in the model and prototype.

91. In my opinion each individual case has to be treated on its merits, and laboratory experiments undertaken to evaluate the indices in the regime equations. Furthermore, in practice it is extremely rare to encounter long straight reaches of channel with uniform flow conditions applying, so that to be strictly correct a new set of indices should be derived for each elementary length; a task which would clearly take far too long to be feasible.

Dr D. M. McDowell, Professor of Hydrodynamics and Hydrology, University of Manchester

The Authors have used the method of dynamic velocities developed by Dr Barr in an attempt to study the theory of regime of channels with alluvial beds. It seems to me that the use of dynamic velocities contributed little if anything to the result shown in equations (6)–(10), all of which could have been obtained by conventional dimensional analysis together with the method of compounding used by the Authors in § 20. I am not convinced that the method of dynamic velocities which follows from the use of a *general linear variable* has anything to offer to the study of *similitude*, while on the other hand its introduction may add confusion to an already difficult problem. The concept of a velocity that would be developed in a fluid under the action of a force over a chosen length in unit time gives the impression that a more fundamental approach is being made to the problem than the mere selection of possible variables that affect a given situation. My objection to this is that the dynamic velocities do not follow from any straightforward application of the laws of physics.

93. In Table 1 a list of active forces is given in the first column and coefficients of active forces in the second column. The first item, viscosity, is a passive force rather than an active one; moreover the coefficient of viscosity is not a force coefficient. The other items in the Table are based on gravity which is a force (per unit mass) and may be regarded as constant. The use of the term 'dynamic velocity' is also open to criticism. In the first place none of the so-called 'dynamic velocities' are velocities at all. The terms all have the units L/T which correspond to speed, but they have no direction; they are scalars, not vectors. In what sense can v/l be regarded as dynamic?

94. One of the biggest difficulties with the method of dimensional analysis in any of its forms is that it is non-discriminatory between variables which have the same dimensions. It is immaterial whether length dimensions represent vertical height, breadth, or depth of flow or whether forces act vertically like gravity or in any other

direction. Dimensional analysis works best with geometrically similar situations in which the uncertainties can be reduced to a minimum and the functions of dimensionless quantities are few enough to make analysis possible.

95. The concept of regime was developed as a means of reconciling measurements in real canals and rivers which had various cross-sectional shapes, gradients and bed materials. Geometrically similar shapes were seldom present, and one of the facts to emerge quite clearly from the observations was that streams in regime had different proportions of breadth to depth according to the volume of flow per second, other things being equal. Barr and Herbertson's conclusion which is embodied in equations (15)–(19) suggests that regime is impossible for two streams carrying different flows and having the same viscosity if they carry the same charge. They said, moreover, that streams carrying the same charge must also be geometrically similar both in their cross-sectional shape and in the ratio of particle size to stream depth. I have checked the effect of keeping slope constant but varying discharge by means of the Einstein–Brown equation, which applies quite well to the uniform size grains necessary to the Authors' analysis; this equation does confirm that constant charge between systems is quite consistent with the changes in B , H , D and ν suggested by the Authors. This appears to be so much at variance with experience with real rivers that either the Authors' analysis or the traditional regime concept, or both, must be wrong.

96. Barr and Herbertson's work has the serious defect that the grading of particle size is not included in the analysis. They also point out in § 21 that it is unwise to assume wide channel conditions but they do not discuss the reason, which is that it takes no account of three-dimensional flow or of variations in cross-sectional shape. They then proceed to modify equation (8a) to give equation (8b) by introducing Q and B , but in reality equation (8b) is identical to equation (8a) because $Q_s = q_s \times B$; the Authors' equations (8b), (9b) and (10b) contain all the inherent weaknesses of equations (8a), (9a) and (10a). Does this not account for their conclusion that all regime channels which carry the same charge must be geometrically similar?

Dr T. M. Prus-Chacinski, C. H. Dobbie & Partners

During the 40 years since Lacey formulated his correlations known as 'the regime theory', so much has been written about it that the time has come to clarify our thoughts and possibly to 'liberate' the regime theory from the colossal burden of misleading, or often simply wrong, interpretations and 'information'. The Authors would seem to apologize for writing one more paper on the regime theory, but if they had in mind a gradual removal of the accumulated misinterpretation they should be congratulated for a capacity for hard and often thankless work. Against this general background there are a few points which, in my opinion, should be mentioned.

98. Among the very numerous references quoted by the Authors I have not found the US Geological Survey works by Leopold *et al.*,^{75–80} which demonstrate that by using the concept of stream power as stated by Bagnold⁸¹ it is possible to derive the indices to Q of the principal regime relationships by very general considerations. A case equivalent to Brebner and Wilson's derivation is only one of the cases investigated in the US works noted above. The indices as quoted by Lacey were in general confirmed quite well.⁸² Perhaps a study of these references could have a significant influence on the concept of the Authors' Paper.

99. The Authors would seem to demonstrate in their Paper that the regime indices cannot be derived purely by similarity considerations, at least as they are understood at present. This could probably be appreciated *a priori* because it has been already demonstrated by very many authors that (a) a real similitude is not obtainable in liquid flow and (b) that the geometry of small laboratory channels flowing in sand contains a large 'scale effect' if compared with their natural prototypes. This, of course, follows from (a). Innumerable references could be quoted but the Authors' references 27–45 and reference 83 may be taken as typical.

100. The Authors would seem, therefore, to answer in their Paper at least one

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question posed by them in § 32 (e): what is the possibility of practicable laboratory studies of true idealized channels' regime systems, and will there still be a large gap between these and full scale practice? The answer would seem to be 'no' to the first part and 'yes' to the second part.

101. Indeed the only place to study a river is the river itself, and the beginning of § 32 would seem to indicate their general agreement with this trivial statement. A laboratory experiment or a purely theorizing exercise can elucidate some details, but it cannot reveal the general mechanics of the behaviour of natural or full-scale artificial channels.

102. With reference to the regime indices, Lacey and his numerous followers have found that the width of the full-scale channel or river varies approximately as $Q^{1/2}$. This relation is of course of a statistically most probable nature, as is everything else in the physical world. It cannot be proved or disproved purely by hydraulics, because many other factors must be taken into account. The relationship for width seems to be one most stable in the regime relationships and this is fundamental. It is really quite easy to observe that the range of the detritus sizes is relatively small and that because of the relative instability of banks it is much easier for a river to get wider than to get deeper (unless of course it flows across hard rock). Therefore, even if a real hydraulic similitude in water flow were obtainable, the required similitude of regime theory could not be achieved without taking into account the complex factors of soil mechanics, vegetation, climate, etc. The same remark can, of course, be made about laboratory experiments, at least at the moment.

103. I consider the Lacey regime theory to be a very powerful engineering tool, even with its present imperfections. This tool must be used with a considerable degree of skill but, after all, skill is necessary for any engineering tool. The regime theory may, and one hopes will, be greatly improved by the observations of full-scale rivers and canals and by the necessary collection of climatic and geophysical data. Laboratory experiments and theoretical considerations may be helpful, but perhaps at present their role is of a secondary importance.

104. I should like to observe that in my opinion the great merit of the Authors' Paper is that in § 2 they have reminded everybody that the Reynolds number can (or should) be written as the ratio of velocities. Professor Barr has mentioned the same also.¹ It is perhaps correct to say that so far not much attention has been paid to the very interesting question of what really is the significance of the 'dynamic' velocity $\mu/\rho d$. But the recently discovered facts indicate that the frictional resistance in the flow of water is greatly decreased with a small percentage of polymeric solution.^{84, 85} This may perhaps indicate that the general revision of the theory of 'friction' in liquids and particularly in water flow may not be very far in the future. This may not greatly affect the engineering design, but it may perhaps very greatly affect the similarity concepts in respect of the flow of water, and particularly the multiphase flow of water (as in sand, etc.). Therefore, at present the similarity would seem to be a rather unstable base for any theories. A more general approach similar to the one applied by the US School would seem to be much more promising.

Professor J. L. Bogárdi, Professor of Hydraulic Engineering, Technical University, Budapest, Hungary

Essentially the methods and relationships associated with the concept of regime theory do not form a separate theory, since basically a great number of field observation data are processed, rules and empirical relationships being then derived therefrom. In my opinion, the Authors are justified in grouping papers on sediment transportation in watercourses into studies in laboratory flumes, into those relating to the regime theory, and those concerning alluvial plain-land rivers. Additional emphasis is perhaps needed of the fact that the sediment transportation in rivers, river regulation, the 'stable channel' and the regime theory are substantially all concerned with sediment transported by the moving water, with sediment settling out

from water, or with that scoured from the bed. In this interpretation of these physical phenomena they include the theoretical relations concerning the determination of critical conditions of sediment movement and even those applying to the silting of channels and reservoirs.

106. On reviewing earlier research on the regime theory, considerable achievements can be recorded. Still it appears to me that no complete solution can be found as yet. In fact, the number of factors to be taken into consideration is extremely great, and although an almost complete list of the significant hydraulic factors has been presented by Lane,^{86,87} it may be contended that not all have been included. Obviously it was owing to the very great number of hydraulic factors that graphical and numerical methods were resorted to for determining empirical relations of local validity.

107. I am in full agreement with the objectives and the subject of the Paper, as well as with the methods employed therein.

108. The general interpretation of similitude as used by the Authors sounds slightly strange to me. If a relationship applies equally to physical phenomena of different scales and properties (in the present case to different canals), then it can be adopted evidently as a criterion for similitude. However, rather than ensuring similitude, the purpose here is to determine relationships and criteria, by which 'non-scouring, non-silting canals' can be designed in an economical manner under various sets of sediment and other hydraulic conditions.

109. I am in agreement with the method of 'linear proportionalities' and its application. Care should be taken, however, as all quantities of the dimension of length, such as D , H and B , cannot be characterized by a single linear quantity, even in the simplest cases. The phenomenon of sediment transportation is, in fact, affected in different ways by different linear dimensions, which do not change in the same proportion in canals and natural watercourses. Of course this has been allowed for by the Authors, yet equations (6) and (15) could lead to misunderstandings, even if they serve to illustrate a principle only.

110. In the present situation it is impossible to describe the regime theory with the help of explicit equations, unless sediment is transported at a constant rate. This may be the cause of the majority of discrepancies and greatest controversies. For this reason, however difficult this may appear, the inclusion of varying rates of sediment transportation is deemed inevitable.

111. In connexion with the 'synthesis' applied by the Authors and the method referred to by them as 'linear proportionalities' it is proposed to describe the mathematical method developed by myself on the basis of the potential dynamic velocities introduced by Dr Barr.

112. Fundamental importance should obviously be attributed to the selection of variables involved in relationships describing a physical phenomenon. Regardless of the method adopted, an error will inevitably be introduced by neglecting one or more of the variables bearing on the phenomenon. Depending on the significance of the variable neglected, the error will vary in magnitude. Careful selection of fundamental quantities is consequently a prerequisite for the success of every method. This condition alone, however, is insufficient, since from among the fundamental quantities selected only those unrelated to each other must be taken into consideration. The fundamental quantities are usually combined in dimensionless groups and the relationship itself is derived in a dimensionless form as well. Dimensionless numbers again involve the possibility of interdependent groups appearing as independent ones. This contingency assumed particular importance in recent times, since the use of dimensionless numbers has greatly increased during the past 15 years, and as a result the same dimensionless number or group has been used repeatedly in different forms and under different names.^{88,89} Considering the forms of expression, applications and analytical forms of dimensionless numbers used in sediment transportation, a considerable lack of systematical approach is indeed experienced. Attempts to select

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independent dimensionless numbers by exact mathematical methods are consequently fully justified.

113. The term 'potential dynamic velocity' has been introduced by Dr Barr, and these velocities were used to determine relationships on a similarity basis by introducing independent dimensionless groups, selected on the strength of previous considerations. In the present method dimensionless groups are determined on an analytical foundation.

114. Consideration must first be given to the concept of potential dynamic velocities and to the generalized implications thereof as introduced by myself.^{6,90}

115. Every phenomenon can be described by dimensional and dimensionless physical quantities. Diverse mathematical operations can be applied to these to form quantities having the dimension of velocity. These are, of course, not actual velocities and are by no means identical with the resultant velocities occurring during movement, but represent only possible, i.e. potential velocities.

116. This term appears to be acceptable since these velocities depend on physical quantities controlling flow and thus define a certain capacity, possibility, or potentiality. It should be pointed out in this context that shear velocity $U_* = \sqrt{(gHS)}$ introduced earlier in hydromechanics is obviously a similar potential dynamic velocity.

117. Depending on the kind and number of dimensional and dimensionless physical quantities which are required, or desired for describing the phenomenon, different potential dynamic velocities can be formed. It should be clear, however, that if all potential dynamic velocities are formed, there will be some among them which can be derived from one or several of the other velocities, so that not all potential dynamic velocities will be truly independent. Once the independent ones are found, these will give a true description of the phenomenon. Where it is desired to state the relationships characteristic for the phenomenon in a form reduced to a resultant velocity, the independent potential dynamic velocities can be used directly. But there is, naturally, no objection to deriving a dimensionless relationship. Indeed, the ratios of independent potential dynamic velocities yield independent dimensionless groups and define thus in an analytical manner the independent dimensionless numbers by which the phenomenon is uniquely described.

118. The development of potential dynamic velocities and the new analytical method will be introduced in connexion with the phenomenon of sediment transportation.

119. The notation adopted is as follows.

g gravitational acceleration, m/s^2

H hydraulic radius or mean depth, m

D particle diameter of sediment, m

ν kinematic viscosity of the fluid, m^2/s

ρ density of fluid, kg/m³

ρ_s density of sediment, kg/m³

S energy gradient

120. Forming the dimensionless ratios

$$\frac{\rho_s - \rho}{\rho} = \rho' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (50)$$

and

$$\frac{\rho_s - \rho}{\rho_s} = \rho'' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (51)$$

it is desired to describe the phenomenon of sediment transportation with the help of four dimensional and three dimensionless quantities. The channel will be assumed to be very wide in comparison with depth, and sediment to be adequately described

by the diameter D and the ratios of equations (50) and (51). Essentially this implies that under conditions outlined above the phenomenon is adequately defined by the seven basic quantities.

121. The four dimensional basic quantities g , H , D and ν are independent from each other. However, from among the three dimensionless ratios

$$\frac{\rho_s - \rho}{\rho} = \rho', \quad \frac{\rho_s - \rho}{\rho_s} = \rho'', \quad \text{and } S,$$

only two are independent from each other, since both ρ' and ρ'' can be expressed uniquely with the help of the ratio $a = \rho_s/\rho$.

122. From the four independent dimensional quantities, three independent potential dynamic velocities can be formed, since all the remaining potential groups with the dimension of velocity are already defined by the three velocities selected *a priori* arbitrarily and regarded as being independent.

123. From among the three dimensionless numbers, only two may be regarded as independent, and, consequently, any operation and combination with the three independent velocities cannot yield more than two additional independent potential dynamic velocities. It is to be seen that if it is desired to describe under the conditions outlined above the phenomenon of sediment transportation, then *altogether five independent potential dynamic velocities* can be formed, but these may be grouped in different ways. Besides these five independent velocities, a great number of other velocities may naturally be formed, but these will no more be independent. Taking into consideration that there is no reason whatsoever to calculate the groups which can be formed within the three dimensionless numbers, a total of 15 potential dynamic velocities can be derived. Indeed, from the quantities g , H , D and ν , the five velocities $\sqrt{gH}$, $\sqrt{gD}$, ν/H , ν/D , and $\sqrt[3]{g\nu}$ are obtained, each of which can be multiplied or divided individually by the two dimensionless numbers, or arbitrary powers thereof (whereby it is immaterial whether division, or multiplication is used). In this manner 10 additional velocities result. The number of potential dynamic velocities is thus limited to 15, but of these, as has been demonstrated, only five are independent from each other.

124. By adopting, besides the four above dimensional quantities, S and ρ' as the two independent dimensionless numbers, the 15 velocities may be compiled into the following five columns and three rows:

$$\sqrt{gH}; \quad \sqrt{gD}; \quad \nu/H; \quad \nu/D; \quad \sqrt[3]{g\nu} \quad . \quad . \quad (52)$$

$$\sqrt{gHS}; \quad \sqrt{gDS}; \quad (\nu/H)S; \quad (\nu/D)S; \quad \sqrt[3]{g\nu S} \quad . \quad . \quad (53)$$

$$\sqrt{gH\rho'}; \quad \sqrt{gD\rho'}; \quad (\nu/H)\rho'; \quad (\nu/D)\rho'; \quad \sqrt[3]{g\nu\rho'} \quad . \quad . \quad (54)$$

125. These 15 velocities can be combined into a wide variety of groupings to obtain five independent velocities.

126. The number of fifth-class combinations of 15 quantities is $\binom{15}{5} = 3003$.

However, five independent velocities are not always involved in each of the above combinations. In fact there will be some combinations which *a priori* involve three independent quantities only, yet the total of 3003 also includes combinations containing not all of the four dimensional and both dimensionless basic numbers, and cannot, consequently, form five independent velocities as far as basic quantities are concerned.

127. In selecting the truly independent five-term velocity combinations the following rules should be observed:

- (a) In the velocities selected, each of the six basic quantities g , H , D , ν , S and ρ' should figure at least once.
- (b) From each row no more than three velocities may be selected, but these must be independent.

- (c) The velocities selected must satisfy the condition that none of the ratios which can be formed from them should yield identical values. In other words the values S , ρ' , S/ρ' , or their inverses should occur no more than once.

128. Any of the five-term velocity groups selected in the above manner is suited to describe the physical phenomenon in a unique manner. Since any of the independent groups containing five velocities can be used to form four independent velocity ratios, this statement holds true for velocity ratios as well. It will be appreciated from these considerations that it is immaterial which independent velocity group is used for describing the phenomenon, and differences will be formal ones only.

129. Returning now to the previous example, it is seen that any of the independent velocity groups can be used for describing the phenomenon. Since, however, the method developed in this manner points out the velocities, or velocity ratios on which the phenomenon may mathematically depend, it may occur that one of the velocities, or velocity ratios, will be eliminated from the relationships on the basis of experimental results. (This implies naturally that even dimensionless quantities may be eliminated, since both may be produced as velocity ratios.)

130. Continuing and concluding the example adopted for introducing our method, let us select from the 15 potential dynamic velocities given in equations (52), (53) and (54) the group comprising the following five independent velocities:

$$\sqrt{(gH)}; \quad \sqrt{(gHS)}; \quad \sqrt{(gDS)}; \quad \sqrt{(gD\rho')}; \quad v/H \quad . \quad . \quad (55)$$

131. The five independent velocities of expression (55) can be utilized in two ways for determining the relationships which describe the phenomenon.

132. If the velocity pertaining to the incipient motion of sediment of particle size D and specific gravity ρ_s , the so-called critical velocity, is v_c , then this velocity can be expressed analytically as a function of the five independent velocities of expression (55) in a direct manner. By writing the ratio of v_c and one of the potential dynamic velocities, the dimensionless group thus obtained may be expressed obviously in terms of the four independent velocity ratios which can be formed from the five velocities.

133. *In the first case* the critical velocity is described by the relationship

$$v_c = f [\sqrt{(gH)}, \quad \sqrt{(gHS)}, \quad \sqrt{(gDS)}, \quad \sqrt{(gD\rho')}, \quad v/H] \quad . \quad (56)$$

so that under the conditions assumed when the problem was outlined, v_c may depend analytically on the potential dynamic velocities involved in equation (56). The actual relationship itself and the form thereof must, of course, be determined experimentally.

134. The detailed analysis of the theoretical relationship expressed by equation (56) would exceed the scope of this discussion. Still it is deemed advisable to remember that according to the classical impact theory substantially

$$v_c = f [\sqrt{(gD\rho')}] \quad . \quad . \quad . \quad . \quad . \quad . \quad (57)$$

135. *In the second case*, for deriving the dimensionless relationship, the four independent velocity ratios must be formed.

136. For writing the four independent velocity ratios $\sqrt{(gHS)}$ should be related to the remaining four velocities. Taking the potential velocity $\sqrt{(gD\rho')}$, the critical velocity v_c can be described by the relationship

$$\frac{v_c}{\sqrt{(gD\rho')}} = f \left[S, \frac{H}{D}, \frac{\tau_0}{(\gamma_s - \gamma_w)D}, \frac{U_* H}{v} \right] \quad . \quad . \quad . \quad (58)$$

137. It should be noted in this context that considerable importance should be attached to the choice of the independent velocity ratios included in the investigation. In fact, some of these may fundamentally affect the phenomenon considered, while others may be ignored without introducing an appreciable error.

138. A comparison of potential dynamic velocities and dimensionless numbers in widespread use will reveal obvious interrelations between them. The relationship

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148. In some instances the introduction of potential dynamic accelerations may result in certain advantages. Acceleration as a kinematic quantity is in general of a significance inferior to that of velocity, yet its application may be justified by its intimate relation with acting forces. It needs no further explanation that any acceleration may be regarded as a force acting on unit mass and, considering the dominant forces of the phenomenon, this circumstance may be extremely beneficial.

149. Considering again the phenomenon of sediment transportation already referred to for the generalization of potential velocities, from the four dimensional basic quantities g , H , D , and ν , three accelerations, including the two dimensionless basic quantities, S , ρ' , and two additional accelerations, can be formed, i.e. altogether five independent potential dynamic accelerations can be formed. As in the case of potential velocities, a great number of accelerations can be developed. Thus from the four dimensional basic quantities, the accelerations

$$g; \quad g \frac{H}{D}; \quad g \frac{D}{H}; \quad \frac{\nu^2}{H^3}; \quad \frac{\nu^2}{D^3}$$

can be formed, preferably, and thus in combination, with S and ρ' and observing the restrictions mentioned previously, 15 potential dynamic accelerations can be written:

$$g; \quad g \frac{H}{D}; \quad g \frac{D}{H}; \quad \frac{\nu^2}{H^3}; \quad \frac{\nu^2}{D^3} \quad . \quad . \quad . \quad . \quad (64)$$

$$gS; \quad g \frac{H}{D} S; \quad g \frac{D}{H} S; \quad \frac{\nu^2}{H^3} S; \quad \frac{\nu^2}{D^3} S \quad . \quad . \quad . \quad . \quad (65)$$

$$g\rho'; \quad g \frac{H}{D} \rho'; \quad g \frac{D}{H} \rho'; \quad \frac{\nu^2}{H^3} \rho'; \quad \frac{\nu^2}{D^3} \rho' \quad . \quad . \quad . \quad . \quad (66)$$

150. Potential dynamic accelerations lead obviously to exactly the same results as the potential dynamic velocities.^{91, 92}

151. It should be noted finally, that for reasons of practicability the introduction of potential dynamic lengths besides velocities and accelerations may be justified in certain instances.

152. Returning again to the example on sediment transportation used for the generalization of potential velocities, the four dimensional basic quantities may be combined, preferably into the potential lengths

$$H; \quad D; \quad \frac{\nu}{\sqrt{(gH)}}; \quad \frac{\nu}{\sqrt{(gD)}}; \quad \frac{\nu^{2/3}}{\sqrt[3]{g}}$$

of which no more than three are independent. Introducing further the dimensionless numbers S and ρ' , altogether five independent potential lengths can be derived.

153. With the restrictions mentioned before, in this case again 15 potential dynamic lengths can be written.

$$H; \quad D; \quad \frac{\nu}{\sqrt{(gH)}}; \quad \frac{\nu}{\sqrt{(gD)}}; \quad \frac{\nu^{2/3}}{\sqrt[3]{g}} \quad . \quad . \quad . \quad . \quad (67)$$

$$HS; \quad DS; \quad \frac{\nu S}{\sqrt{(gH)}}; \quad \frac{\nu S}{\sqrt{(gD)}}; \quad \frac{\nu^{2/3} S}{\sqrt[3]{g}} \quad . \quad . \quad . \quad . \quad (68)$$

$$H\rho'; \quad D\rho'; \quad \frac{\nu\rho'}{\sqrt{(gH)}}; \quad \frac{\nu\rho'}{\sqrt{(gD)}}; \quad \frac{\nu^{2/3}\rho'}{\sqrt[3]{g}} \quad . \quad . \quad . \quad . \quad (69)$$

154. In selecting the independent five-term length groups, the rules described in connexion with velocities and accelerations must be observed.

155. It will be appreciated that by using potential dynamic lengths, relationships can be derived in the same manner as in the case of velocities and accelerations. These quantities, or more correctly the ratios thereof, which I term 'potential dynamic lengths' are referred to by the Authors as 'linear proportionalities'.

Mr G. E. Hartley, University of Strathclyde

I was at one time actively engaged in the organization of the removal and disposal of considerable quantities of silt from the irrigation canals of the then United Provinces.

157. I was instructed and confirmed by observation that:

- (a) the effect of silt in channels was to raise the upstream levels so that the outlets at the head of the canal discharged more water than those at the tail, but that the increase in irrigated area on the head outlets did not compensate for the loss of irrigation at the tail; consequently all heavy silting channels must be repeatedly cleared;
- (b) since silt clearance was expensive, repeated silt clearance should be followed by the redesign of the channel.

158. It was argued that since steep slopes were known to scour, there must for a given discharge be some slope (and channel dimensions) at which silting would become unimportant, and if, therefore, the channel slope was increased in small steps, no harm could result and ultimately there would be improvement. There are, of course, disadvantages to this procedure. Remodelling is expensive, and in order to find additional head while maintaining command it may be necessary to alter the designed full supply level for miles upstream, with consequent adjustment of the outlet levels. Step-by-step remodelling therefore involves repeated expense and, more important, upsets the customers.

159. It was at this time that Mr Lacey produced his silt theory. Although I (and I suspect others also) but imperfectly understood the original papers, design charts were available and the system worked.

160. However, it was at once obvious that if all the channels could in turn be made non-silting, then any silt brought into the canal must be discharged through the outlets to the cultivators' water courses.

161. I endeavoured to accelerate this process by fixing outlet pipes with their mouths at or near bed level and sloping upwards and outwards to restrict the operating head.

162. This, however, could only be a palliative for, at best, the device merely transfers the work of removal and spreading silt from the project to the cultivator. The material is perhaps of some manurial value, but to what extent this recompenses the cultivator for his labour is problematical. In the very long term, spreading silt on the fields must tend to reduce command, and by 1940 many irrigation engineers were satisfied that there was a case for settling out some silt where the canal system takes off from river and either dredging or scouring it back into the river downstream of the headworks.

163. Mr Lacey's silt formulae have been summarized,⁹³ and the main objections thereto are stated to be 'The formulae only contain a parameter that indicates the size of the bed material. There is no parameter that allows for the quantity of bed load that is introduced into the canal.'

164. Recently I have had the opportunity of observing a length of flume (Aberdeen sand) which is not accreting. The bed load moves down the flume in a series of rows reminiscent of the movement of sand dunes by bulldozers. The movement is intermittent, a series of pulses at irregular intervals, so that work done or power utilized is a statistical average. Similarly the roughness profile as it appears to the observer is changing constantly, and there is no reason to suppose that if the profile at any instant could be frozen in position it would cause the same energy losses as now obtain.

165. What is observed is turbulence doing work, and there must be an equivalent static roughness (Kutter's N)⁹⁴ requiring the same slope to maintain a steady state. Mr Lacey first tied in his silt factor with the roughness co-efficient, but separate co-efficients would appear to be necessary for the bed and for the berms. The Authors

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quite properly stress shape for the roughness of the berms, though different from the equivalent roughness of the bed, will affect the flow pattern and hence the apparent roughness of the bed.

166. In this flume, the bed was not accreting, so that the quantity being moved was the amount being fed, and this by the nature of the movement would appear to be a function of the bed width. By the analogy of the bulldozer, I would expect the resistance to motion and energy utilized to be determined by the quantity being moved, the size and shape of the particles, and their distribution.

Sir Claude C. Inglis, Formerly Director, Hydraulics Research Station

The Authors have treated the subject of regime as a theoretical exercise and have aimed at casting doubt on the validity of the regime concept.

168. The Authors state in § 8, 'A study of the literature leads the Authors to propose two definitions for aspects of the regime theory:

- (a) Regime principles apply to all quasi-uniform flow of fluid, in any shape or form of conduit, over a bed of non-cohesive material in active transport.
- (b) Regime practice is confined to the design of straight stable irrigation channels in alluvium. Depending on various factors, including method of construction and wash load, the side slopes of these channels may be steep, consisting of cohesive but erodible material. However, the channel bed is incoherent and erodible.'

169. Those two definitions are unacceptable,

- (a) because it would change the meaning of the regime concept, which has proved so successful in solving practical problems since it was originated and developed by irrigation engineers in India, and
- (b) because regime practice most certainly is not 'confined to the design of straight stable irrigation channels in alluvium'.

170. The definition of regime theory in § 6 is also unacceptable. This reads 'Regime theory is basically a set of rules for the design of loose boundary canals such that a known quantity of water and water-borne sediment will be passed along the canal without either erosion or deposition.' This implies that the canal will be in full cutting and will be run at once with full supply and assumed sediment charge and grade of bed sediment. Not one of these assumptions is justifiable. Actually, the canal, when first opened, will be run with a discharge much less than the designed full supply discharge, and a charge and grade of bed sediment which is extremely unlikely to be the same as predicted. Furthermore, most canals are designed with their full supply level considerably higher than the land through which they pass, to give 'flow irrigation', and high banks are constructed to contain the flow, the width between these banks being greater than the designed surface width of the canal at full supply level after silt berms have formed by deposition. These berms are valuable as they greatly reduce losses by percolation which occur owing to water being headed-up at cross regulators in order to pass flow into off-taking channels at their design levels.

171. The Authors' definition is therefore topsy-turvy, and the definition of regime as stated by E. S. Lindley 50 years ago²¹ is, on the other hand, precise and correct. This reads: 'When an artificial channel is used to convey silty water, both bed and banks scour or fill, changing depth, gradient and width until a state of balance is attained, at which the channel is said to be "in regime"'. In other words, discharge and charge and grade of sediment are the independent variables, and width, depth and gradient, the dependent variables.

172. The next question for consideration is whether the exponents in the Punjab equations which the Authors 'attempt to refute' are justified. In the equations Lindley worked out, based on 786 observations in branch surveys of the Lower Chenab Canal, the data show that, measured in accordance with later practice, widths

were proportional to $Q^{0.50}$ and depths proportional to $Q^{0.33}$, i.e. exponents were the same as those of Lacey, when he was specially deputed in 1933 to study regime equations and evolve working principles for the design of irrigation channels. Lacey did not make any observations himself, but analysed the existing data. It was then decided by the Central Board of Irrigation, India, that canal data should be collected from regime sites by the Irrigation Research Organizations of the various provinces to check Lacey's relations. In the Punjab, the work of collecting data in the alluvial channels was entrusted to the Punjab Irrigation Research Institute, who selected 43 sites with discharges from 5 cusecs to 6000 cusecs. The observations were carried out in accordance with research methods developed at the Punjab Irrigation Research Institute as recorded in their Annual Reports.

173. Among the equations then worked out by Drs Bose and Malhotra were:

$$\text{wetted perimeter} = 2.8 Q^{0.50}$$

$$\text{hydraulic mean depth} = 0.47 Q^{0.33}$$

It will be seen that the exponents for width and depth (which are easy to measure and not subject to short-term oscillations) were identical in these three independent investigations for channels in which the bed sediment was in general between 0.3 and 0.4 mm dia., and the discharges in the channels were maintained constant at their heads when they were in flow. Though there were limited periods when the concentration of fine sediment in suspension was high, the effect of this at the selected gauging sites was small and temporary, and had a negligible effect on the overall annual dominant flow conditions on which the regime values were based. These several circumstances rendered possible the high degree of consistency in those early sets of equations.

174. In the foregoing, the discussion has been restricted to channels in which the bed sand is in the medium sand range, 0.2–0.6 mm. It is now known that different regime equations hold for channels in which bed-sand is finer than 0.15 mm, and V is proportional to rs , compared with V proportional to $\sqrt{(rs)}$ in channels with medium sand beds,^{95,96} and that there is a third regime in which the bed-sand is coarser than 0.6 mm.⁹⁷ Much work will have to be done to establish the coarse sand equations, and field confirmation of Lacey's fine sand equations is also desirable.

175. The Authors state in § 5: 'Many correlation methods and transport equations have been proposed and a large amount of data has been accumulated. Although some of the correlations and equations have been based on theoretical considerations, the conventional approach is essentially empirical and extrapolative.' Unfortunately, in the case of flume experiments, conditions were imposed which precluded the development of natural regime, and in the case of field observations, much of the data that has been accumulated does not contain information about all the factors which determine the behaviour of channels, particularly as regards periodic variations in the charge and grade of sediment. A great advantage of the work done in India, based on data collected in large channels as well as small, was that undesirable extrapolation was unnecessary.

176. Lacey included in his equations a term to cover the effect of size of bed material on the dimensions and slopes of channels, and I have obtained the relationship (§ 34)

$$\frac{V^2}{gH} \propto \left[\frac{xw}{(gv)^{1/3}} \right]^{1/2}$$

by combining the Lacey equations with an extension of White's ideas,⁹ the effect of charge being based on experiments carried out under my direction at the Central Water and Power Research Station, Poona, in 1943 and 1944.

177. This relationship showed that the Froude number is dependent only on the sediment properties, because it indicates that the combined effect of sediment charge, which involves values of charge and grade which fluctuate and are difficult to measure,

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can be replaced by an expression involving terms which vary little, and are relatively easy to measure.

178. The Authors state in § 35 that this relationship does not hold because 'the Froude number is constant for a given charge and sediment grain size, provided kinematic viscosity and either channel width, depth or slope are also maintained constant'. This once again emphasizes that the Authors fail to realize that regime, in the sense that it is used by irrigation engineers, envisages the development and maintenance of stable natural flow conditions in a channel, not the imposition of conditions which, though they may remain stable, like those in flume experiments, would not reproduce the correct secondary currents which, in natural channels, determine the cross-sectional shape. The Authors are also apparently not aware of the conditions that have had to be dealt with to achieve the high degree of success obtained in interpreting the behaviour of canals, rivers and estuaries, and planning their control.^{98, 99}

Dr F. V. A. Engel, Consulting Engineer

In the synopsis of their stimulating paper the Authors refer to a 'new approach to the delineation of the similitude framework appropriate to the circumstances of regime channels'. §§ 17–21 present a short account of the similitude theory for sediment transport. However, without a careful study of previous investigations by the Authors, in particular references 3, 11, 12 and 13, it is difficult to follow the arguments developed in the Paper.

180. The first question concerning the system of linear relationships is whether it includes any redundant terms or whether in the strictest sense the system is complete. In particular, this question refers to equations (7) and (8a). Assuming that both relations are complete, is it meaningful to compare them with empirically established, incomplete relations? In many known relations of regime 'theory' it is likely that neglected phenomena and terms are lumped together in the indices, which from the very beginning makes any analysis impossible.

181. Regarding redundant terms, it is puzzling that three terms in equations (7) and (8a) include kinematic viscosity. Other essential components of the three terms are already included in the third expression. Is it necessary to include all three terms in the system, or should one or the other term be represented on a different basis excluding kinematic viscosity?

182. To demonstrate the inadequacy of many regime relations two relations have been selected from an investigation by Herbertson (equations (24a) and (25) of reference 13), namely:

$$\Phi \left(\frac{B}{D}, \frac{R}{D}, \frac{q_s}{(sg)^{1/2} R^{3/2}}, \frac{sg}{g'_w}, \frac{\rho_s - \rho}{\rho}, \frac{g^{1/2} D^{3/2}}{\nu} \right) = 0 \quad . \quad . \quad (70)$$

(i) (ii) (iii) (iv) (v) (vi)

and a version of equation (25) rearranged so that some important specific terms appear in the same position as in (24a), namely:

$$\Phi \left(\frac{q}{\nu}, \frac{R}{D}, \frac{q_s}{q}, \frac{g'_w}{sg}, \frac{\rho_s - \rho}{\rho}, \frac{g^{1/2} D^{3/2}}{\nu} \right) = 0 \quad . \quad . \quad . \quad (71)$$

(i) (ii) (iii) (iv) (v) (vi)

Herbertson has expressed the basic bed load functions in terms of non-dimensional components of equations (70) and (71) respectively. These terms have been indicated in various columns of Table 3.

183. It is of interest to note that none of the conventional bed load formulae include the Galileo number, column (vi). This is an unexpected result as, in connexion with silt transport, it was already implicitly developed by Lacey as early as 1934 (reference 15, 441–443). The Authors state in a reference to the Inglis regime

Table 3. Comparison of basic terms in bed load functions

Authority	Non-dimensional terms as given in equation (70)					Remarks
	$\frac{B}{D}$ (i)	$\frac{R}{D}$ (ii)	$\frac{q_a}{(sg)^{1/2} R^{3/2}}$ (iii)	$\frac{sg}{g_w}$ (iv)	$\frac{\rho_a - \rho}{\rho}$ (v)	$\frac{g^{1/2} D^{3/2}}{\nu}$ (vi)
Meyer-Peter and Müller .			o	x	x	
Kalinske		ox	o	x		
Einstein		x	o	x		
Shields		x	ox	o		
Yalin		ox	x	x		

The marks (o and x) indicate that the term occurring in the heading of the Table is a basic expression in the relationship established by the authorities. These relationships had been analysed by J. G. Herbertson.¹³ The mark o indicates that the term occurs on the left hand side of the relation, while the mark x indicates that it occurs on the right hand side.

In accordance with equation (71)
Complex expression, terms represented by the marks indicated

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theory, § 34, that an essential expression in the relationship established by Inglis can be replaced by the Galileo number. From Leliavsky's book (reference 31, 231) in which the Inglis regime relations are confronted with those by Lacey, the pre-dominance of a kinematic viscosity expression becomes evident. It is suggested that the Authors may show how the Inglis and Lacey regime relations could fit into Table 3 or how they could be represented by their equations (7) or (8a)?

184. Regarding the interpretation of the Galileo number, some comments are required. It is a combined group representing two laws of dynamic similarity. The Froude law and the Reynolds law are incompatible with respect to alterations of length dimension scales in model and prototype, but also when comparing dissimilar length dimensions of river and channels which formed the basis of the various empirical regime relations. Furthermore, in view of the fact that the pertinent regime relations originated from experimental research conducted in India, USA and Switzerland it is not realistic to assume a constant value of the kinematic viscosity. Therefore, the term D^3/ν^2 cannot be uniquely defined. A constant value of the Galileo number may characterize a large range of unspecific and dissimilar flow conditions.

185. There is another point which must be raised in view of the importance rightly attached by the Authors to the Galileo number. Dr Barr³ writes: 'If the fluids are . . . allowed to flow in acceleration in certain geometrical similar systems, $\nu^{2/3}/g^{1/3}$ is the necessary measure of size to be applied to these systems. This is as basic and rigorous as are the Navier-Stokes equations, but $\nu^{2/3}/g^{1/3}$ does not appear to be a measure which can be *directly* extracted from the governing equations which have so far been devised for viscous incompressible flow under the influence of gravity.' Have the Authors attempted to start from more appropriate forms of the momentum equation, for instance from equations which are used in investigations of transport phenomena in chemical engineering¹⁰⁰? In a heterogeneous system, mass transfer should be considered by an additional term in the momentum equations, taking into account the net influx of momentum into the system by mass transfer across the bounding surface.

186. The Authors, in contrast to many authorities who define non-dimensional groups as ratios of forces or energies, introduce non-dimensional terms on the basis of length ratio or velocity ratio concepts. I do not understand the significance of shifting emphasis from energy or force ratios to velocity or length ratios. The disadvantage of such a procedure is that the non-dimensional groups lose their identity. Main objections to these two ways of defining non-dimensional terms are that the array of quantities, as presented for instance in equation (7), does not distinguish between mere geometrical or property ratios, genuine groups of the laws of dynamic similarity, and combined groups. I prefer^{101, 102} a clear definition of the laws of dynamic similarity, and to separate from these groups geometrical parameters, property parameters and combined groups. Each individual group of the laws of dynamic similarity is characterized by a property, e.g. viscosity, surface tension, heat conductivity, density, and the velocity of sound, and furthermore, by physical quantities, namely frequency and acceleration. These are expressions of a scheme of macro-molecular Newtonian Mechanics which complete the system of dynamic entities, like velocity, force and energy.

187. As the Authors present some suggestions in the concluding paragraph of the Paper for further research to elucidate the problems of regime conditions, it is warranted to draw their attention to the advantages of the *analogy method*, which also makes extensive use of non-dimensional groups of dynamic similarity. In my opinion, the analogy method is superior to any method of 'inspectional analysis' which in a wider sense also includes the Authors' method of synthesis.

188. The method of analogy has made important contributions to some domains of engineering and science and has achieved considerable progress in the fields of momentum, heat and mass transfer. It would not have been possible to establish the

Table 4. Specific critical Mach numbers and Froude numbers related to upstream sections of contracted fluid streams

	Axisymmetrical convergent/divergent nozzle flow	Side contracted open channel flow*
Non-dimensional groups, general design	$(Ma)_1 = \frac{u}{\sqrt{\left(\frac{\gamma p_1}{\rho_1}\right)}}$	$(Fr)_1 = \frac{u}{\sqrt{\left(\frac{gh_1}{n+1}\right)}}$
Specific critical numbers . . .	$(Ma)_1 = m \left[\left(\frac{2}{\gamma+1} \right) \left(1 + \frac{\gamma-1}{2} (Ma)_1^2 \right) \right]^{\frac{\gamma+1}{2(\gamma-1)}}$	$(Fr)_1 = B \left[\frac{2n+2}{2n+3} \left(1 + \frac{1}{2n+2} (Fr)_1^2 \right) \right]^{\frac{2n+3}{2}}$
Rule of transformation . . .	$\gamma = \frac{n+2}{n+1}$	$n = \frac{2-\gamma}{\gamma-1}$

* The geometry of the parabolic channel cross section is defined by $y = sz^n$, where s = size factor and n = shape factor. $n=0$ denotes a rectangular cross section, $n=1$ a triangle, all other values of n represent parabolas. The geometry of the upstream and throat sections is 'similar' and is given for each individual case by different values of the size factors s_1 and s_2 respectively. The ratio of the size factors is denoted by $B = s_2/s_1$; it corresponds to the area ratio m of the nozzle. $\gamma = c_p/c_v$ is the ratio of specific heats.

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basic relationship of heat and mass transfer by applying conventional methods of dimensional analysis. Essential groups of dynamic similarity and other well known non-dimensional parameters form variables in these relationships.

189. The two following examples may demonstrate special features of the method of analogy. In general it enables one to transfer all terms in an equation which has been established in one field to another one, which may considerably differ in many respects from the original relation. The mathematical design of the equations remains the same. However, following certain rules of transformation, the physical meaning of the individual terms in both equations may be quite different.

190. The first example, the gas dynamic and open channel flow analogy,¹⁰³ is mainly concerned with phenomena in nozzle flow characterized by a gas property, namely the velocity of sound. With reference to open channel flow, the criterion is the velocity of the propagation of a long wave of small amplitude in the throat section of a side-contracted open channel. In both cases disturbances occurring downstream of the contracted, critical flow section cannot be transmitted upstream. In these critical sections the Mach number and the Froude number, respectively, attain a numerical value of unity.

191. Under those conditions the upstream Mach number or Froude number differs considerably from unity. Analogy requires that the mathematical design of the specific Mach number and Froude number, both related to the same 'area ratios', should be identical. However, the component terms in each expression may be defined by absolutely different quantities or qualities (see Table 4).

192. The velocity term, both in the Mach number and in the Froude number, is an essential feature of any of the genuine groups related to the laws of dynamic similarity. Velocity of sound in the Mach number is a gas property under prevailing plant conditions. It is the only genuine group which does not include a length term. This would exclude the Mach number from one of the systems advocated by the Authors. The celerity term in the denominator of the Froude number is merely dependent on channel geometry, the depth of the liquid stream, and acceleration due to gravity.

193. A particular feature of this example is that no 'geometrical similarity' exists between the two analogous systems. However, it is channel geometry, in combination with h , the depth of the water stream, which can affect the numerical value of the Froude number. The geometrical shape factor n is the only available free variable. These features demonstrate the uniqueness of both the Mach number and the Froude number.

194. The second example, the analogy between momentum, heat and mass transfer originated with an early paper by Osborne Reynolds (1874) and was advanced considerably by T. von Kármán.^{104, 105} Basically the analogy covers the total momentum transfer by molecular and turbulent exchange and the amount of heat transferred by molecular heat conduction and by turbulent heat transfer. This means that, besides kinematic viscosity, other fluid dynamic properties, namely eddy viscosity and thermal diffusivity, specify the problem. Kármán's significant contribution was the introduction of a buffer layer between the laminar sub-layer next to the pipe wall and the core of the turbulent fluid stream. Flow in this buffer layer is characterized by both molecular and turbulent transport. In the sub-layer molecular transport phenomena exist, and the core of the fluid stream is characterized by the Reynolds analogy. For further information the original papers should be consulted.¹⁰⁴⁻¹⁰⁷ An extension of the Reynolds-von Kármán analogy to flow in smooth and rough pipes is due to Hofmann.¹⁰⁷ The heat transfer equation is mentioned here to contrast its mathematical design with the usual pattern of the regime relations. Besides the friction factors λ_s and λ_r for smooth and rough pipe wall surfaces, it comprises several non-dimensional terms, namely:

(St) Stanton number	These are non-dimensional 'dependent variables' applicable to heat transfer calculations
(Nu) Nusselt number	

(*Re*) Reynolds number

(*Pe*) Péclet number

(*Pr*) Prandtl number: a fluid property parameter

195. An interesting feature of the Hofmann equation

$$(St) = \frac{(Nu)}{(Pe)} = \frac{\lambda_s/8}{1 + 1.5(Re)^{-1/8}(Pr)^{-1/6}\{[(Pr)\lambda_s/\lambda_r] - 1\}}$$

is its design, which is not based merely on a product of variables with fractional indices.*

196. Fractional indices which are not simple terms, like $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$ etc., may be due to empirical 'curve fitting' procedures adopted in deriving the relations. It appears unlikely that they represent a law of nature. Simple fractional indices were suggested recently by Gill¹⁰⁹ in an attempt to correlate Einstein's with Lacey's regime equations. However, it should be pointed out that the indices of the Froude number in Table 4 can assume fractional indices of any numerical value between 1.5 and 2.0 depending on channel geometry.

197. In my opinion it is a fallacy to assume that numerical values could be obtained by means of deduction from a mere array of non-dimensional terms. Concluding, I should like to quote Bridgman,¹¹⁰ one of the very few reliable authorities on non-dimensional analysis. We should understand that the method of dimensional analysis 'has nothing esoteric about it, that it gives no information which we did not have already in the less available form, and that therefore an adequate physical grasp of each individual problem is necessary. No refinement of mathematical argument will give correct results if our physical grasp of the broad features of the problem is inadequate.'

Mr G. Lacey, Consultant, Sir M. MacDonald & Partners

The Paper was of great interest to all who have studied the regime of loose boundary, self-formed rivers and canals. The contention of the Authors that regime theory implied departure from dynamic theory was linked with the more vital issue, the mode of sediment transport, and its corollary the shape of the channel cross section which the mode imposed.

199. The mode of sediment transport was highly controversial. Yalin, Danel *et al.* accepted saltation as a primary means of transport whereas Einstein and Kalinski rejected this contention and suggested sliding and rolling on the bed.¹¹¹ It was for the Authors to state their views on this subject; it would appear from their trapezoidal flume approach that they adopted the bed load mode.

200. In a 'regime' channel the isotachs (US isovels) were curved and confocal and dictated the shape of the boundary. In a trapezoidal flume with a heavy bed load the isotachs were parallel to the bed, and this led to a uniform value of the Author's *H* and, as a sequel, their vertical slice theory. This concept was inapplicable to channels which transported sediment by saltation. The variable *H* was a length and no more. The regime mean depth, *D_m*, was in an entirely different category. The mean depth consisted in dividing the cross-section area, *A*, by the water surface width, *W_s*, and was thus implicit in the ratio *A/W_s*. Regime theory was essentially three dimensional.

201. The 'proof' of the regime indices based on the vertical velocity of the water, *V_s*, and on the settling velocity of the particles was admittedly somewhat inept, but that did not necessarily prove that the indices were invalid. Lane¹¹² in 1935, and

* Nunner attributes the general design of the equation in § 195 to Hofmann. However, the introduction of the ratio of the friction factors for smooth and rough pipes respectively is due to Nunner.¹⁰⁸

Fig. 10. Typical example of a channel in process of siltation

Blench¹¹³ in 1937 working independently were the first to assign a physical dynamical meaning to the parameter VS . In effect Lane and Blench contended, to quote Blench,¹¹⁴ that 'channels of the same water sediment complex tend to acquire the same value of gVS , the energy dissipation per unit mass per unit time.' If Lane and Blench were correct it would appear that they had proved that the regime indices did not depart from the principles of dynamic similarity. The Authors' views on this subject would be of great value.

202. The contention of the Authors that regime theory was merely a set of rules for the design of straight canals was an understatement. They quoted the range of canal data from 5 to 5000 cusecs, but ignored the correlation made in 1930 of the width and discharge of rivers with a range of discharge from 200 000 to 2 500 000 cusecs.¹⁴ Also my reply to the discussion on reference 14 appeared to have made no impact on the Authors, particularly in respect of model scales, and the phenomenon in Nature of exaggeration in the vertical scale, and the fact that Osborne Reynolds' first model of the Mersey estuary, because of his singularly happy choice of scales, confirmed the regime indices.

203. Early irrigation canals excavated in earth with steep side slopes and horizontal beds, which were narrow from motives of economy, might remain stabilized if the earth were tenacious. The fact remained that the principle of exaggeration in the vertical scale demanded that the bed width should bear a constant ratio to the water surface width. The ideal canal should be made wide enough and deep enough for it to flow within an envelope of deposited sediment. Fig. 10 provided an excellent example of this process. The canal was the Kheri Branch of the Sarda Canal and the photograph was taken, looking upstream, from a recently constructed regulator. The purpose of the regulator was to restore 'command' to a distributory which took off from the Kheri Branch immediately upstream of the regulator. The white line was painted on the cross section during a canal closure. The branch was originally excavated with side slope of 0.5 to 1.0, horizontal to vertical, and the bed was horizontal. The writer was at that time in charge of the canal. The actual discharge was 757 cusecs, and the actual slope 1/10 560. The deposition of sediment was fostered by the fact that the regulator raised the water surface and increased the depth.

204. The Authors' equation (24) which was none other than that derived by Inglis

was of great interest. There was, however, some doubt in respect of the power of $\frac{1}{2}$. I have recently obtained by plotting hydraulic data the analogue

$$\frac{V_s^{1/2}}{m^n} \propto \frac{V/R^{1/2}}{(R^{1/2}S)^n}$$

This implied that the general equation took the form

$$V^2/(gD_m) \propto (XV_s)/(vg)^{1/3} \propto X[gm/(vg)^{2/3}]^n$$

I regret that time did not permit expatiating on this theme particularly in respect of moving bed tractive force. I hope that the Authors will be tempted to re-examine the credentials of the 'regime' theory.

Professor Barr and J. G. Herbertson

It is apparent from the considerable volume of discussion received, that regime theory is a subject of much interest. This is truly justified and reflects very favourably on the pioneering studies of many workers in the field of regime.

206. Contrary to the remarks made by Sir Claude Inglis we have *not* 'aimed at casting doubt on the validity of the regime concept', and are of the opinion that the Paper tends to support the existence of a regime condition. What we do not accept is that there is any theoretical justification for the uniqueness of the indices of the regime equations. Herein the regime concept is taken to mean that discharge may be related to channel width, depth and slope.

207. Several contributors have made the point that dimensional analysis (or any other form of partial or fractional analysis—adopting Kline's¹¹⁵ definition of such methods) cannot be used to predict the numerical values of the indices of the regime equation. This is obviously true and at no point in the Paper have we attempted to perform this operation. What we have done is to deduce the numerical values of the indices for equations relating to dynamically similar channels. Dynamically similar channels do not exist under natural conditions and any relationship between channels subject to natural adjustments will show deviations from dynamical similarity. However, it is considered that by comparing naturally adjusted (or regime) channels with the corresponding dynamically similar (hypothetical) channels a convenient, universally applicable basis of comparison may be established. Some of the disagreement appears to have resulted from a confusion of regime and dynamically similar channels.

208. The importance of sediment charge has been stressed in the Paper and in many of the discussions. Attempts have been made by other workers to relate sediment discharge to catchment rather than river cross section properties. One of the most recent by Fleming¹¹⁶ has led us to consider the following system.

209. The discharge of water (or sediment) from a catchment results from climatic, vegetation, gravity and viscous actions on the catchment which may be characterized by the square root of its area A , some representative slope S , and sediment size D . It is thought that $\sqrt{A}$ provides a convenient measure of the catchment size and that climatic and vegetational parameters may be combined on the basis of cause and effect. This leads to equations of the form

$$\phi(\text{climatic term}, D, \sqrt{A}, \frac{Q_s^{2/5}/(Sg)^{1/5}}{4^*}, \frac{Q_s^{2/5}/g^{1/5}}{4^*}, \frac{Q_s^{2/5}/(g'w)^{1/5}}{4^*}, \frac{Q/v, v^{2/3}/(Sg)^{1/3}}{4^*}, \frac{v^{2/3}/g^{1/3}}{4^*}, \frac{v^{2/3}/(g'w)^{1/3}}{4^*}) = 0 \quad (72)$$

and a similar equation with Q_s appearing in place of Q . From these equations a non-dimensional functional equation can be formed.

$$\phi\left(\begin{array}{c} \text{group incorporating} \\ \text{climatic term,} \end{array} \frac{Q_s}{Q}, \frac{Qg^{1/3}}{v^{5/3}}, \frac{g'w}{g}, \frac{A^{1/2}g^{1/3}}{v^{2/3}} \begin{array}{c} \text{term incorporating} \\ \text{grain size} \end{array}\right) = 0 \quad (73)$$

Neglecting viscous and sediment density variations, equation (73) leads to a correlation among Q_s/Q , Q , $\sqrt{A}$, climatic or vegetational effect and sediment size effect.

DISCUSSION

Fleming's correlations take account of yearly suspended sediment load, mean yearly discharge and vegetation type. The statement of the problem is applicable to a somewhat idealized catchment and climatic influence. However, the simple model may serve as a first approximation to the problem and as a base for future study.

210. Before replying to the discussers in turn, we have the following general points:

- (a) One of our objectives, that of arousing a 'state of the art' discussion has amply succeeded, and we will not comment on each single point made.
- (b) Nothing has been said which meaningfully contradicts our proposition that the conventional values of regime indices are based on a misconception, and that instead of making comparison with 0.33 and 0.5 relating to depth and breadth respectively, the true interest should be in departures from the 'dynamically similar' value of 0.4 for both cases.
- (c) With reference to § 45, a tilting flume of triangular cross section and variable vertex angle has now been built, and we consider that understanding of regime principles will accrue from the running of this flume with appropriate sand and feed arrangements. All that has been said in the discussion will be taken into account in the consideration of the significance of the experiments in hand. The effective cross section of this channel under test conditions will be rigid sided, mobile bottomed trapezoidal.
- (d) In following up the proposition regarding plotting of data which derives from the Authors' equations (20)–(22), it is important to recognize that the Authors' exposition is based on the existence of a pair of ideal sets of data. Each individual set of ideal data may be erroneous to a greater or lesser extent, and it must not be assumed that the data for the least flow can be taken as a base for the improved form of plotting. However, this difficulty is easily overcome; one simple approach is to plot depth and breadth data against $Q^{0.4}$ which immediately rationalizes the intent of the plot so as to seek departures from the dynamically similar 0.4 index.

Dr Blench draws attention to the incredible divergence of laboratory based formulae and field findings and presents most useful additional background to any future work with a regime bias. The comprehensive study of laboratory data by Cooper and Peterson,⁵⁴ to which he refers is in general very closely related to the Authors' work. Blench emphasizes the importance of the particle Vig number ($\frac{2}{3}(\nu g)D/\nu$) and of the velocity measure $(\nu g)^{1/3}$ which also appears in the Addendum to Lacey's 1933 Paper.¹⁵ We regret that Blench has not given more details of the earliest appearance of this group. Engel, on the basis of work by Herbertson,¹³ has pointed out that terms of this form of non-dimensional group do not appear in any of the formulae derived by laboratory workers. We agree that the effect of phase of bed configuration is important and that it is possible that one set of equations will be relevant only to one configuration. There is some confusion in the literature regarding nomenclature: Blench refers to beds as duned, as is noted by Neill, but Lacey prefers to classify the bed as being in the ripple zone. The importance of the adopting of a standard nomenclature for bed forms should be stressed. The American Society of Civil Engineers have taken an initiative in a recent publication.¹¹⁷ Such attempts at categorization are essential in order that full use may be made of varied data from field and laboratory studies.

Mr Neill emphasizes just how little is known about 'a channel that satisfies the equations'. We cannot agree that the theoretician's attitude should be 'so what'. The equations have been used extensively with varying degrees of success and are undoubtedly a valuable tool in the analysis of practical problems. Neill's valuable discussion on the width/discharge relation does in itself stimulate the inquiries of the theorist. Neill considers that some justification is necessary for the effort required

to produce the Paper. Prus-Chacinski provides the answer in the statement that 'so much has been written about it (regime theory) that the time has come to clarify our thoughts and possibly to liberate the regime theory from the colossal burden of misleading, or often simply wrong, interpretations and information'. Coupled with this, statements (made even by Neill) that some or all of the Lacey equations express 'a kind of natural law for stable channels' must surely be either proved or disproved.

Dr Kramer's discussion is mainly concerned with the challenging problem of model scaling. Space does not allow us to do justice to such a complex subject and we would in reply refer to a recent paper by Herbertson¹¹⁸ in which the problem of scaling of loose boundary models is considered in detail. We most certainly agree with the remarks made in § 84 regarding the need for further study, but we would emphasize the importance of adopting a rational basis of experimentation and also of correlation of experimental data. These themes have been stressed elsewhere.^{11, 12}

Mr Smith draws attention to the fact already mentioned herein that a considerable part of the Paper is concerned with channels in which complete similarity exists and that these should not be confused with regime channels. We agree with Mr Smith that not much will be gained if we restrict our future efforts to the replotting of existing incomplete data. The correlation procedure suggested is intended more for new data which it is hoped will incorporate at least an estimation of sediment discharge. Perhaps the most significant feature of equations (24)–(26) is the relatively small difference between the indices for dynamical similarity and those for the conventional regime condition considering the marked differences between the two conditions.

Professor Komura rightly observes that it is essential to regime theory that the sediment load passing through all cross sections be the same while the flow remains the same. Is not this condition also essential to the practical design of canals by the tractive force theory? One must design for the real condition set by the problem, and not accede to a particular theory. Thus when approaching the problem from tractive force theory, the excess force must be constant along the elementary length of the channel where sediment and water flow remain constant.

Dr Allen. We would go further than to say that the regime indices were governed by cross sectional shape. Mr Neill has listed several other important factors including geographical and geological environment. Climatic influence will also be important as it affects both hydrological and vegetation factors. Regarding the choice of scales for models we would again refer to Mr Herbertson's Paper.¹¹⁸ As mentioned by Mr Lacey some well known models have been run successfully with scales chosen in accordance with the regime equations but as noted by Professor Allen³⁴ not all successful models obey the regime rules.

217. Dr Allen raises an interesting point about the effect of the velocity distribution. In dynamically similar channels we would assume similarity of velocity distribution but in a regime channel the superimposition of the 'wrong' sediment size and fluid viscosity will have an effect on the velocity distribution. The velocity distribution and also the secondary circulation mentioned by Mr Neill will undoubtedly have a profound effect on the shape of stable channels.

218. We agree that each individual case should be treated on its merits. This is the ideal situation. However, in view of the successful applications of regime theory claimed by Mr Lacey and Sir Claude Inglis it would seem reasonable to assume that consideration of a set of representative situations would enable extrapolation to be made for practical cases. The picture need not be as dismal as painted by Dr Allen.

Professor McDowell has stated that the results obtained in the Paper could have been obtained by conventional dimensional analysis. This we would have expected. The advantage of the method of synthesis is that the complete range of possible forms of equation are displayed in the initial stages of the procedure. Professor Bogardi makes this very clear in the presentation of his form of the method of dynamic velocities. The nature of a dynamic velocity is perhaps best described by Bogardi's nomenclature potential dynamic velocity.

220. What has been more interesting to us is that they can also be obtained by a dimensional analysis method which parallels the similitude method used in the Paper and which now appears to offer considerable advantages over conventional dimensional analysis. This applies to both its expediency and its ability to demonstrate physical significance. The new approach to dimensional analysis has already been outlined^{119, 120} and the theory has been given in some detail.¹²¹ We cannot agree that dynamic velocities are scalars because the whole ethos of similitude depends on the initial proposition of geometrical similitude. Detailed treatment of this objection must await a full discussion of the splitting of length into co-ordinate directions as has been proposed by Huntley.¹²² We consider that Huntley's approach has been much less than helpful and that criticism of the co-ordinate direction approach may be at the root of Professor McDowell's objection to the concept of dynamic velocities. Further justification of the nomenclature is given elsewhere.¹²⁰

221. In § 93 Professor McDowell raises questions on terminology. The main objection appears to be the classification of viscosity as an active force. In the present context all that is meant by the term 'active forces' is that certain forces are acting either on or within the system considered, as compared with the reactive inertial forces. The action of gravity acts on the system and the internal resistance to motion or friction (characterized by viscosity) acts within the system.

222. In reply to the comments made in § 95 we would refer to the introductory remarks included herein and also to Mr Smith's contribution in which it is stressed that equations (15)–(19) do not refer to real channels but to hypothetical dynamically similar channels. It is interesting that the Einstein–Brown equation yields similar results. Under these circumstances, neither the Authors' analysis nor the regime concept is wrong.

223. We agree that no account is taken of the grading of particle size. This is common to most works relating to sediment transport. A few attempts have been made to define a suitable parameter, e.g. Inglis⁹ introduces fall velocity, Egiazaroff¹²³ and Komura and Simons¹²⁴ adopt a ratio of particle sizes within the grading curve. To our knowledge no satisfactory solution to the problem has been found. If Professor McDowell knows of one he should have quoted it. The method of synthesis in common with conventional dimensional (as used by Yalin⁴⁵) requires the specification of the channel cross section. In the present case the ideal channel is specified either as rectangular or trapezoidal. The reason for introducing total discharge and width into equations (8b), (9b) and (10b) is that the need for the arbitrary wide channel condition may be eliminated. Thus removing at least one uncertainty for the ensuing analysis. The reply to Professor McDowell's final question is that we did *not* conclude that all regime channels which carry the same charge must be geometrically similar. This is a most unlikely event.

Dr Prus-Chacinski very rightly mentions the valuable works of the US Geological Survey personnel. The premise on which the work is based is that the processes associated with the flow of fluid in a channel will occur in such a way that the rate of work done is a minimum. For the application of the principle to alluvial channels, two relationships are required, one supplied by the minimum energy degradation rate and the other in some way defining the flow system. The difficulty in using a procedure of this type is in the choice of the second relationship. Langbein,⁷⁶ for example, assumed that the variance in shear end friction was equally divided, i.e. that there was a uniform distribution of internal energy. There appears to be very little theoretical evidence to either prove or disprove Langbein's assumption. Brebner and Wilson¹⁹ in reply to discussion of their paper state that 'in seeking a derivation for the regime formulae, there was a risk in using as a basis other empirical relationships applicable only to flow with a movable bed'. Although taken somewhat out of context, this remark emphasises the difficulty inherent in the minimum work rate procedures.

225. In answer to §§ 99, 100 and 101 we would restate § 33 of the Paper—'Regime studies are vastly complicated and thus no real advance can be made until a

logical model exists for even the simplest, idealized case'. We have found no such model in the literature and have considered the hypothetical dynamically similar systems as logical models. Working from this it should be possible even in the laboratory to evaluate the relative effects of charge, grain size, temperature, etc. Without a framework, whether this be a similitude or a complete mathematical one, comparison and correlation of results becomes meaningless.

226. We fail to see why a revision of the theory of friction in fluids should have adverse effects on similitude theory in particular. Regardless of the internal actions in a body of fluid (mono or multiphase), surely the overall force actions can be characterized by gravity and a viscous or resisting force. An analogy can be drawn with the difficulties which arise in density current studies where small thermal density differences are associated with temperature differences in the vicinity of the point of inversion. Provided that the same temperatures are kept between a small and a large body of fluid, and hence the same density differences, there is some scope for obtaining dynamical similarity between a large and a small system provided that the large system has an artificially increased viscosity to compensate for the increase in size, i.e. the normal densimetric fluid Reynolds number or Archimedes number criterion of scaling would be applied.¹¹⁵ However, if there is too great a departure from 'geometrical similarity' of the shape of the density temperature relationship as between the two systems, the patterns of normal exchange flow with superimposition of streamers due to mixing will be lost.¹²⁵

227. Without specific details, it would appear that experiments with polymeric solutions would have more bearing on channel systems transporting fine, cohesive or semi-cohesive materials than on regime type systems transporting larger sized non-cohesive sediments. However, we do agree that the effects of sediment charge are of considerable importance and that further investigation is required in order to extend Sir Claude Inglis's⁹⁷ work.

228. We are grateful to Professor Bogardi for so adequately defining the nature of similarity methods and of dynamic velocities in particular. It is encouraging to learn that another worker in the field of sediment transport has found the concept of dynamic velocities of great convenience. However, we do not agree that, as inferred in § 113, Bogardi's manipulation of the basic dynamic velocities is any more fundamental or analytical in nature than that proposed by Barr.¹¹⁷ We would prefer to consider the methods as alternatives.

Professor Bogárdi (§ 108) finds our general interpretation of similitude strange. This results from the confusion of the dynamically similar and the regime systems. We would most certainly agree that discrepancies have occurred in practice due to fluctuations in sediment charge and that attention must be paid to the extension of Sir Claude Inglis's⁹⁷ work. In this particular aspect it may be possible to establish a firm link between the regime and tractive force approaches. Further the strictures and cautionary remarks regarding the use of dimensionless numbers cannot be emphasized too strongly nor can the possibility of a considerable number to correct definition equations.

Mr Hartley's addition of practical details to the discussion was very valuable and we find his comments most interesting. The bulldozer analogy very vividly describes the behaviour of bed load transport. We agree that the quantity of sediment transported and the channel bed width will be very closely related. This is a factor which has not received much attention.

231. It is appropriate to say here that we had discussions with Mr Hartley during the writing of the Paper and it was what he said that led us to consider that a first approach to the elucidation of regime theory in the laboratory could be attempted with rigid sides to the channel. It may be that the clue to comparison between regime channels in different geographical locations will be the evaluation of a characteristic side slope which takes into account the extent of the cohesive capacity of the wash load and its ability to increase the effective angle of repose of the base material. At

present, we hope to have laboratory studies with different side slopes in the rigid sided channel.

Sir Claude Inglis. We would assure Sir Claude Inglis that he has misinterpreted our purpose in subjecting regime theory to a critical examination. We accept and recognize the theory as a remarkable achievement and fully comprehend the difficulties in analysing and interpreting field data. As applied to conditions obtaining in the Indian Sub-Continent, the indices of the regime equations are indisputable but unless the same equations and indices occur universally the theory is not a complete theory. It then remains to find a physical basis for sediment transportation into which conventional regime theory can be fitted as a special case. It is made quite clear by Sir Claude Inglis in § 173 and § 174 that special conditions do in fact occur in the Punjab and that recent work indicates that the form of the equations changes as grain size changes. If this is the case it seems equally likely that the equations will also change depending upon such factors as climate and sediment charge. Hence there appears to be a case for regional regime theories. Raudkivi³³ sums up the situation — 'One can find local regime theories developed in every area where irrigation is practised.'

233. Despite Sir Claude Inglis's objections we do not consider that the definitions of regime principles and practice are either misleading or unjustifiable. We would define the application of regime theory to natural rivers and estuaries as being in the realm of regime principles rather than practice. Regime theory was developed from the study of irrigation systems for the purpose of evolving 'working principles for the design of irrigation channels'.

234. As a result of work done after publication of the Paper we wish to modify the statements made in § 35 concerning the Froude-Inglis number relationship from equation (28)

$$\text{Froude number} = \phi \left(\text{charge}, \frac{g^{1/3} D}{\nu^{2/3}}, \left| \begin{array}{c} B/D \\ \text{or} \\ H/D \\ \text{or} \\ S \end{array} \right| \right) \quad \dots \quad (28)$$

it can be seen that (as already stated) the Froude number is constant for a given charge and sediment size provided kinematic viscosity and either channel width, depth or slope are the same in compared systems. However by plotting the Froude number-charge relationship for medium sized sediments (around 0.4 mm) there is some evidence to support the conclusion that the relationship becomes independent of the H/D ratio if $H/D > 400$. This adds weight to Blench's²⁴ statement that regime theory is applicable to channels with H/D ratios greater than 250. Thus a relationship of the form (of equation (27)) proposed by Inglis appears to be justified under the particular conditions prevailing in the Indian Sub-Continent. Finer sands exhibit a different tendency, i.e. it would appear that the significance of the H/D ratio is more marked.

Dr Engel's question concerning the completeness of the analysis is answered by Professor McDowell who states the same results could be obtained by conventional dimensional analysis. Thus if a solution obtained by dimensional analysis is considered to be complete then one obtained by the method of synthesis is no less complete. Attention was drawn in the Appendix to Yalin's⁴⁵ comprehensive and correct use of conventional dimensional analysis. The validity of comparing empirical incomplete relations with complete theoretical relationships is questioned. Surely the incompleteness of the empirical relations can only be judged by comparing them with the corresponding complete ones. How else can we be sure that the empirical relations are incomplete?

236. Dr Engel also raises the question of redundancy in the linear proportionality equations (7) and (8a) and doubts the wisdom of including kinematic viscosity in three terms of these equations. The answer to this may be found in references 3, 11, 12

and 13 and in Bogardi's detailed discussion of the Paper. Essentially kinematic viscosity is used to provide the necessary link between the other variables. Other forms of equations (7) and (8a) are possible but in all cases the action of each active force must be entered at least once. As can be seen there is no question of redundancy in equations (9a) and (10a) derived from the former equations.

237. The importance of the Galileo or Vig number has been stressed by both Engel and Blench. It is indeed strange that regime workers should be strongly aware of this term while the tractive force school totally ignore it. Considering one of the Inglis formulae—

$$H \propto \frac{\nu^{1/3} Q^{1/3} D^{1/6}}{g^{1/18} (Cw)^{1/3}} \quad \dots \quad (74)$$

in which $C = Q_s/Q$.

Equation (74) can be written as

$$\frac{H}{D} \propto \left(\frac{Q}{g^{1/2} D^{5/2}} \right)^{1/3} \cdot \left(\frac{Q_s}{Q} \right)^{-1/3} \cdot \left(\frac{w}{g^{1/3} \nu^{1/3}} \right)^{-1/3} \quad \dots \quad (75)$$

For any given value of the ρ_s/ρ ratio

$$\frac{w}{g^{1/3} \nu^{1/3}} = \phi \left(\frac{g^{1/2} D^{3/2}}{\nu} \right) \quad \dots \quad (76)$$

Thus equation (74) becomes

$$\frac{H}{D} \propto \left(\frac{Q}{g^{1/2} D^{5/2}} \right)^{1/3} \cdot \left(\frac{Q_s}{Q} \right)^{-1/3} \cdot \left(\frac{g^{1/2} D^{3/2}}{\nu} \right)^{-1/3} \quad \dots \quad (77)$$

It is not appropriate to attempt to fit equation (77) into Table 4 since the latter applies to rectangular channels. Equation (77) may be compared with equation (13). i.e.

$$\phi \left(\frac{H}{D}, \frac{Q^{2/5}}{g^{1/5} D}, \frac{Q_s}{Q}, \frac{g^{1/3} D}{\nu^{2/3}}, T, \frac{\rho}{\rho_s} \right) = 0 \quad \dots \quad (13)$$

Four of the six groups of equation (13) are included in equation (77), and the last two are implicit in the conditions of regime theory. Although the groups are included in equation (77) they appear in a compounded form. In effect equation (77) contains two independent terms in contrast to six in the corresponding complete equation.

238. We have not attempted to start from chemical engineering types of equation as we consider that the Navier-Stokes equations as well as applying, in theory, to both turbulent and non-turbulent flow are equally representative of a system transporting a single sediment grain over a continuous solid bottom or a large number of grains over a bed of the same material. It is immaterial whether velocity, length, force or energy ratios are used in a similarity equation. The identity of the groups is not entirely lost in a system expressed in terms of velocities or lengths. Is it important that [the identity of the groups be maintained? Has there not been a tendency to cast non-dimensional functional equations in terms of conventional non-dimensional numbers? In so doing many less conventional but more convenient forms of the equations have been discarded. Each problem must be considered on its own merits. The linear term form of the method of synthesis is particularly suited to problems of flow in open and closed conduits.

239. We would agree that it is impossible to determine indices for practical systems from an array of non-dimensional terms but is quite justifiable to determine indices for hypothetical dynamically similar systems. In performing this step we are doing no more than applying reasoning similar to that employed in the determination of flow in a Froudean hydraulic model except that in the ideal case both Froude and Reynolds criteria are satisfied. Bridgman's¹¹⁰ description of dimensional analysis or for that matter of any form of partial or fractional analysis is indisputable and cannot be emphasized sufficiently.

240. The value of the analogy method is readily apparent and we thank Dr Engel for bringing it to our notice. There would appear to be great difficulty in determining the appropriate analogy. We consider that a more basic procedure, such as the method of synthesis, is less prone to misuse.

Mr Lacey. We would emphasize that the great advantage of both dimensional and similitude analysis is that the question of the mechanics of sediment transport could be ignored in the basic analysis. Obviously the actual numerical factors and indices and even the significance of non-dimensional groups will vary depending on the predominant mode of sediment transportation. Our problem is to determine bases of correlation; details of modes of transportation although extremely important must be considered of a secondary nature.

242. Mr Lacey draws attention to the difference between the mean depth of flow, as measured from the water surface to the mean bed level and the regime mean depth, obtained by dividing the cross section area by the surface width. In the case of a trapezoidal channel the regime mean depth (D_m) is related to the mean flow depth (H) as follows:

$$D_m = H - \frac{TH^2}{B}$$

in which T is the side slope and B is the surface width.

For $B/H=5$ and $T=\frac{1}{2}$

$$D_m = 90\% \text{ of } H.$$

As width to depth ratio increases D_m approaches H . In most practical cases the difference between D_m and H will be small. We agree that regime theory is three dimensional but not by virtue of introducing D_m . It makes little difference whether D_m or H is used in the vertical slice theory—it is as Lacey has admitted 'somewhat inept'. This does not invalidate the indices.

243. It is indeed interesting that Lane and Blench found that 'channels of the same water sediment complex tend to acquire the same . . . energy dissipation per unit mass per unit time'. This does not prove that the channels are dynamically similar for the simple reason that geometrical similarity is a necessary condition for dynamic similarity and as has been stressed in this reply conditions of dynamic similarity do not exist in nature. They can be enforced in the laboratory, as shown by Yalin.⁴⁵

244. In § 202 Mr Lacey quotes values of discharge in a greater range than given in our Paper. However, we are interested in the range of values which were used in the evaluation of all the regime equations. As noted by several discussors the width equation has been verified over a very large range of values. Reynolds' model conforms to the regime equation but as Professor Allen³⁴ has observed not all successful models have been run in accordance with the regime rules.

245. It is unfortunate that Mr Lacey has not been able to expand the argument leading to the general form of Froude-Ingilis number. We sincerely hope that he will find time in the near future to present the ideas embodied in this discussion and also in the discussion of Kellerhals's¹²⁶ paper more fully. Further consideration of the significance of the velocity measure $(vg)^{1/3}$ would be particularly valuable and appropriate since Lacey first used this term in the addendum to his 1933 paper.¹⁵ Reference to the latter was unfortunately omitted from § 2 of our Paper. Thus the potential value of $(vg)^{1/3}$ as applied to sediment transport systems was recognized by Mr Lacey 36 years ago.

246. In conclusion we would assure Mr Lacey that we accept that many questions about regime theory remain unanswered. In a search for the answers we and many others will doubtless continue to re-examine the original statement of the regime theory. Thereby understanding of the whole question of channel geometry may eventually be greatly improved.

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