PAPER NO. 233

'SHOCK IN REGIME CHANNELS'

BY

GERALD LACEY ESQR., B Sc., M. INST. C. E.

Recent analysis by the writer of authoritative Punjab regime channel data has shown that the slope in parts per thousand, S^*, on any regime channel subject to shock is given by the equation

$$S^* = 0{\cdot}391\ k^{\frac{5}{3}}\ m^{\frac{5}{6}} \Big/ q^{\frac{1}{3}} \quad \text{.....} \quad (1)$$

in which k represents a coherence factor, m the mean diameter of the bed silt particle in millimetres, and q the discharge in cusecs per foot run, or discharge intensity, Q/P.

2. Provided that the silt is always of the same coherence this equation can be thrown into a simpler form. The Punjab silt consists of sand for the greater part, and for discharges varying from roughly 100 cusecs to 1000 cusecs the coherence factor for the channels examined approximates closely to 1·76. If this value is substituted equation (1) reduces to

$$S^* = 1{\cdot}003\ m^{\frac{5}{6}} \Big/ q^{\frac{1}{3}} \quad \text{.....} \quad (1a)$$

for standard silt coherence.

3. The general equation has one very curious feature. The shock is implicit in the equation. Any increase in shock leads to the dissipation of more energy and an increase in eddying motion in the turbulent water. The eddying motion, whether due to perfect regime flow, or compounded of normal turbulence and shock turbulence, is reflected in the precise grade of silt exposed on the bed. Provided the coherence of the silt is known, (a factor obviously affecting silt behaviour), the mean diameter of the bed silt grade in a regime channel faithfully records turbulence in the same way that a thermometer registers heat. Shock is implicit in equation (1) and coherence is explicit.

4. Equation (1a) which is a modified form of the Bose-Malhotra equation

$$S^* = 2{\cdot}09\ m^{\cdot 86} \Big/ Q^{\cdot 21}$$

has a curious counterpart in the writer's earlier work. It was found that for regime channels free from shock the silt characteristic, or silt factor, f could be expressed by the equation

$$f = 0{\cdot}75\ V^2 / R \qquad (2)$$

the value of f being unity when the rugosity co-efficient N_a was equal to ·0225. It was found also that on all regime channels free from shock the general equation

$$V = 16{\cdot}05\ (R^2 S)^{\frac{1}{3}} \qquad (3)$$

held good. This equation is independent of the bed silt grade and the coherence.

5. If equation (2) and (3) are combined they result in the writer's general regime equation

$$S^* = 0{\cdot}391\ f^{\frac{5}{3}} \Big| q^{\frac{1}{3}} \qquad (4)$$

which should be compared with equation (1). The two equations show that when there is no shock, and the channel is in regime.

$$f_r = k m_r^{\frac{1}{2}} \qquad (5)$$

The suffix 'r' indicates regime, free from shock. It follows that when a regime channel is free from shock the value of f computed from equation (2) is equal to the value of f computed from equation (4) and this is evident from the manner in which equation (4) was derived.

6. Regime channels are very seldom entirely free from shock and for this reason the value of f computed from equation (2) usually differs from that computed from equation (4). When positive shock is present the value of f is less than that computed from the slope and q. This latter value of f will be denoted f_q. When f equals f_q there is no shock.

7. If equation (4) is to be written in a general form it is

$$S^* = 0{\cdot}391\ f_q^{\frac{5}{3}} \Big| q^{\frac{1}{3}} \qquad (4a)$$

If we assume that the introduction of shock into a channel sweeping forward a given silt charge does not change the coherence of that charge it follows from equation (4*a*) and equation (1) that

$$f_q = km^{\frac{1}{2}} \quad \text{.....} \quad (5a)$$

If by S is denoted the regime slope subject to shock, and by S_r the regime slope free from shock,

$$S/S_r = m^{\frac{5}{6}} \Big| \; m_r^{\frac{5}{6}} = f_q^{\frac{5}{3}} \; \Big| f_r^{\frac{5}{3}} \quad \text{.....} \quad (6)$$

It is however required to compute the shock in terms of f_q and f. This cannot be done without recourse to the writer's shock theory.

8. Equation (5) when plotted to a logarithmic base is represented by a straight line. Departures from this line indicate shock, and departures are measured *normal* to the regime curve. On this assumption, which constitutes the writer's shock theory the 'shock locus' is represented by the line

$$m^{\frac{1}{2}} f = m_r^{\frac{1}{2}} f_r \quad \text{.....} \quad (7)$$

This is a rigid inversion of equation (5).

Solving for f_r

$$f_r = k^{\frac{1}{2}} \left(m^{\frac{1}{2}} f \right)^{\frac{1}{2}}$$

substituting for $m^{\frac{1}{2}}$ from equation (5*a*)

$$f_r = (f \cdot f_q)^{\frac{1}{2}} \quad \text{.....} \quad (8)$$

9. The fact that the basic silt factor differed from the silt factor derived from the slope and q was in the past a stumbling block. The new equation shows how it is possible, without reference to coherence, or the bed silt particle, to calculate the regime value of the silt factor which would have obtained had there been no shock. Students

of the work of Mr. C. C. Inglis will have observed that some years ago he proposed an equation identical in form with equation (8) on empirical grounds. It must however be remembered that the standard of channel maintenance is often reflected in shock, and the degree of shock must therefore be computed. Substituting the value of f_r from equation (8) in equation (6) we have finally

$$S \Big/ S_r = m^{\frac{5}{6}} \Big/ m_r^{\frac{5}{6}} = \left(f_q \Big/ f \right)^{\frac{5}{6}} \quad \ldots\ldots (9)$$

10. It has thus been shown how to compute for a regime channel, of which values of V, R and S are alone known, the regime value of the silt factor and also how to calculate the degree of shock. When the value of the bed silt grade has been observed the coherence co-efficient can be computed from the equation

$$k = f_q \Big/ m^{\frac{1}{2}} \quad \ldots\ldots (5b)$$

11. As already stated the values of k computed for the Punjab data are very consistent over a large range. In the large canals the value of k increases indicating the possibility of silt waves, and in the small channels with an admixture of clay the value of k also increases. In Sind the method of silt grading by sieving and sedimentation probably involves a correction constant before comparison can be made with the Punjab data. The values of f_q for the Rohri Canal, R. D. 210,000 for the months of September 1937 and 1938 were respectively ·668 and ·669 and thus practically identical. The values of k were however 1·96 and 1·65.

The following are the data :—

Date	f	f_q	f_r	m	S*	k	q
24-9-1937	·643	·693	·668	·1244	·068	1·96	30·5
21-9-1938	·801	·558	·669	·1142	·046	1·65	33·2

It will be observed that in 1937 there was positive shock to a small degree and the value of the coherence factor was higher than 1·76 as to be expected. In 1938 the slope was much lower than in 1937, the shock was negative, and the coherence factor very low indicating that the bed silt in movement was highly mobile. If the observations are indeed correct and the channel was in regime the only explanation of the negative shock is that patches

of the bed were smooth and immobile. Positive shock is due to major irregularities in the banks or bed causing increase in eddying motion, and negative shock may be due to very smooth clay banks freshly dressed, or, in this particular instance, to major variations in cohesion, part of the bed of fine silt and clay being very smooth and practically rigid.

12. It is very evident that cohesion may be a dominating factor when the silt transported is very fine. It is clear also that the silt charge in suspension as also the percentage of fine clay in suspension are of importance. If it were desired, from the data of the Rohri Canal, to design a new canal transporting the same type and grade of silt, conservatism would demand that a value of f_q of 0·70 was adopted in calculating the slope, and a value of f of say 0·62.

When designing the cross section. There is nothing in the Sind data repugnant to the writer's silt theory which on the contrary should be used for interpreting Sind channel behaviour.

13. The shock theory outlined in this Paper was evolved by the writer in December 1938, while engaged in collating his previous researches as a publication for the Central Board of Irrigation which is now in the press. The relationship

$$f_r = (f.f_q)^{\frac{1}{2}}$$

which the writer terms the Inglis relationship, since Mr. Inglis first put it forward on empirical grounds, is immediately derivable from the writer's shock theory and was so derived by the writer in November 1939. It is hoped that this equation will receive due attention, because it is very evident that it is the value of f_r as computed for all the channels in a regime system, which characterises the type of silt admitted at the head of the canal, whereas values of m or f, uncorrected for shock, may prove exceedingly misleading. The writer trusts that in future, in addition to measurement of the silt grade, values of f_r, m_r and k will be computed. These values coupled with a study of values of the shock ratio S/S_r should rationalize channel design. The writer would again emphasize the importance of recognizing that it may be necessary to employ two values of the silt factor, namely f_q and f in channel de-

sign, the first being employed to compute the slope, and the second to assign the cross sectional area. For the latter purpose the writer's equation $p = 2{\cdot}67\ q^{\frac{1}{2}}$ can be employed, associated with f, but the numerical value of the co-efficient can vary from 2·5 to 2·8 and much obviously depends on the ratio of the maximum discharge to the actual average discharge.

14. The theory has been briefly presented in this Paper in order that it may receive a full discussion by Members of the Punjab Engineering Congress many of whom have a specialized knowledge, not only of the subject, but of the channels on which the writer's theory is largely based.

The writer desires finally to acknowledge his debt to the empirical Bose-Malhotra equation

$$S^* = 2{\cdot}09m^{\cdot 86}/q^{\cdot 21}$$

which went far to confirming his own equation correlating the silt factor and the silt particle, and which first led him to investigate the elusive but very real problem of shock in regime channels.

APPENDIX A

List of Regime sites under observations.

Serial No.	Serial No. as per Punjab Report.	Centre with period.	Sites and R. D.	Q in cusecs.	$S \times 10^3$	V in ft. per sec.	P in ft.	R in ft.	$n \times 10^3$ in cm.	$P\sqrt{Q}$	$\frac{n \cdot 86}{Q^{\cdot 21}} \times 10^3$	q	f_q	$f = \frac{29.1 n^2}{R}$	$f_s = \sqrt{f \cdot f_q}$	$K = \frac{f_q}{1.12\, m}$	$818_s = \left(\frac{f_q}{f}\right)^{5/6}$	818_{Bose}
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
					Lower	Jhelum	Canal											
1	19	Satgharah February 1936 to June 1936	N.B.L.J.C.R.D. 73,500	1391·2	·17	2·52	10·84	5·46	35·30	2·71	·18	13·91	1·025	·967	0·943	1·79	1·149	·94
2	15	Ludawala September 1933 to January 1936	N.B.L.J.C.R.D. 163,000	1183·4	·13	2·45	9·93	5·24	31·29	2·65	·17	13·32	0·965	·86	0·864	1·543	1·041	·76
3	22	Gullapur September 1933 to November 1934	S.B.L.J.C.R.D. 102,500	777·0	·15	2·15	8·56	4·45	27·29	2·93	·17	9·52	0·882	·76	0·85	1·672	1·108	·83
4	16	Ludawala September 1933 to January 1936	N.B.L.J.C.R.D. 170,299	773·6	·16	2·32	7·4·30	4·46	30·26	2·67	·18	10·4	0·959	·90	·916	1·696	1·03	·89
5	24	Gullapur October 1933 to December 1934	Kisara Distributary R.D. 2,675	434·5	·30	2·11	6·33	3·41	28·52	2·90	·19	7·2	0·995	·979	0·987	1·86	1·013	1·05
6	23	Do. October 1933 to November 1934	Lallian Distributary R. D. 1,700	357·9	·16	2·23	4·83	2·77	27·56	2·37	·19	8·9	0·837	·802	0·89	1·69	0·996	·94
7	18	Ludawala October 1933 to January 1936	Sukh Branch R.D. 0,200	351·8	·20	2·16	5·6·9	2·91	29·95	3·00	·22	6·25	0·963	1·102	1·072	1·76	0·837	·91
8	20	Satgharah February 1936 to June 1936	Mathalah Distributary R. D. 4,000	153·2	·20	1·88	2·5·32	2·60	27·29	2·20	·23	5·28	0·98	·946	0·936	1·776	0·987	·87
9	17	Ludawala September 1933 to January 1936	Bhek Distributary R.D. 1600.	55·5	·28	1·56	2·0·60	1·72	24·77	2·77	·20	2·69	0·964	1·06	1·012	1·935	0·924	1·40
10	21	Do. February 1935 to April 1936	Fatehpur Distributary R.D 10,200.	44·6	·20	1·43	1·7·19	1·74	23·29	2·58	·27	2·6	0·81	·85	0·846	1·678	0·934	·74
													0·935		0·93	1·733	1·062	0·91
													± ·0617		± ·073	± ·103	± ·086	± ·176

Note.—I am indebted to Mr. G. C. Inglis, C. I. E., Director, Hydrodynamic Research Station, Poona, for the necessary computations in this Appendix, a labour which is gratefully acknowledged.

Attention is drawn to the four small channels of the Lower Chenab Canal system, transporting relatively fine silt and the high values of 'K' as computed. Also the relatively high values of 'k' on the five large channels.

The values of 'k' for the Lower Jhelum Canal are very consistent.

The computations are based on an assumption of perfect regime channels, *subject to shock.*

APPENDIX A.

Data from pages 60 and 61 (Table 17) of the Punjab Irrigation Research Institute Report for the year ending April, 1937.

List of Regime sites under observation.

Serial No.	No. as per Punjab Report.	Centre with period.	Sites & R. D.	Q in cusecs	$S \times 10^3$	V in f./sec.	P in ft.	R in ft.	$m \times 10^3$ in mm.	$P^{\frac{1}{2}} \sqrt{Q}$	$\frac{m^{-\frac{1}{2}}}{Q^{\frac{1}{10}}}$	$\frac{Q}{P^2} = q$	f_q	$f = \frac{13 V^2}{R}$	f_r	$K = \frac{f_q}{m^{\frac{1}{2}}}$	$S f_r^{\frac{1}{3}}$	$\frac{S \times Q^{\frac{1}{10}}}{m^{\frac{1}{2}}}$
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19
					Lower	Chenab		Canal.										
1	13	Sagar November 1934 to June 1936	Upper Gugera Branch R. D. 2,500	4552·0	·16	3·01	176·19	8·59	36·73	2·61	·15	25·8	1·121	·79	·942	1·85	1·339	1·07
2	8	Nanuana January 1935 to June 1936	Main Line L. C. C. R. D. 181,000	4410·6	·19	2·90	205·84	7·36	41·99	3·12	·17	21·3	1·198	·856	1·013	1·85	1·324	1·12
3	12	Sagar October 1933 to June 1936	Main Line L.C.C. R.D. 149,000	4387·9	·20	2·86	201·74	7·75	41·08	3·06	·16	21·8	1·24	·792	·993	1·932	1·452	1·25
4	9	Nanuana November 1934 to June 1936	Jhang Branch R.D. 7,260	2802·3	·21	2·63	143·93	6·83	41·92	2·72	·18	19·5	1·248	·875	1·045	1·925	1·304	1·16
5	1	Kot Khudayar September 1933 to June 1936	Jhang Branch R.D. 221,000	2069·0	·19	2·62	129·14	6·18	32·93	2·34	·16	16	1·128	·84	·975	1·97	1·28	1·19
6	10	Nanuana December 1934 to June 1936	Rakh Branch R.D. 7,260	1070·7	·20	2·55	90·61	4·63	39·84	2·77	·21	11·81	1·09	1·055	1·072	1·725	1·025	·95
7	11	Do December 1934 to June 1936	Main Ali Branch R.D 2850	551·1	·20	2·36	64·37	3·63	33·06	2·74	·21	8·56	1·02	1·101	1·061	1·775	·94	·95
8	6	Kot Khudayar September 1933 to October 1934	Nasrana Distributary R.D. 3,275	273·0	·17	2·28	39·94	3·00	26·03	2·42	·20	6·84	0·892	1·30	1·09	1·75	0·73	0·85
9	2	Do September 1933 to June 1936	Chiniot Distributary R.D. 2,909	163·2	·22	1·83	30·46	2·45	29·40	2·36	·24	4·18	0·955	1·025	0·99	1·76	0·95	0·91
10	3	Do October 1933 to June 1936	Sarangwala Distributary R.D. 6,300	59·5	·27	1·51	24·34	1·61	27·29	2·16	·28	2·45	0·96	1·061	1·01	1·83	0·922	0·96
11	7	Do September 1933 to October 1934	Khai Distributary R.D. 4,943	40·1	·29	1·51	16·86	1·58	21·73	2·67	·36	2·38	0·992	1·08	1·036	2·125	0·937	1·11
12	4	Do September 1933 to June 1936	Gugiana Distributary R.D. 7,400	28·8	·29	1·34	16·19	1·32	21·88	3·02	·29	1·84	0·942	·02	0·98	2·01	0·977	1·00
13	5	Do September 1933 to October 1934	Jamal Jath Distributary R.D. 2,360	26·8	·33	1·31	16·03	1·23	22·02	3·10	·29	1·67	1·00	1·003	1·003	2·13	1·00	1·14
14	14	Sagar November 1933 to June 1936	Hafizabad Distributary R.D. 3,050	22·6	·34	1·43	11·83	1·33	25·50	2·51	·35	1·91	1·05	1·15	1·100	2·08	0·928	0·97
													1·06		1·021	1·908	1·07	1·045
													±·1099		± 0·439	±·1306	± 202	±·115

FIGURE 1.

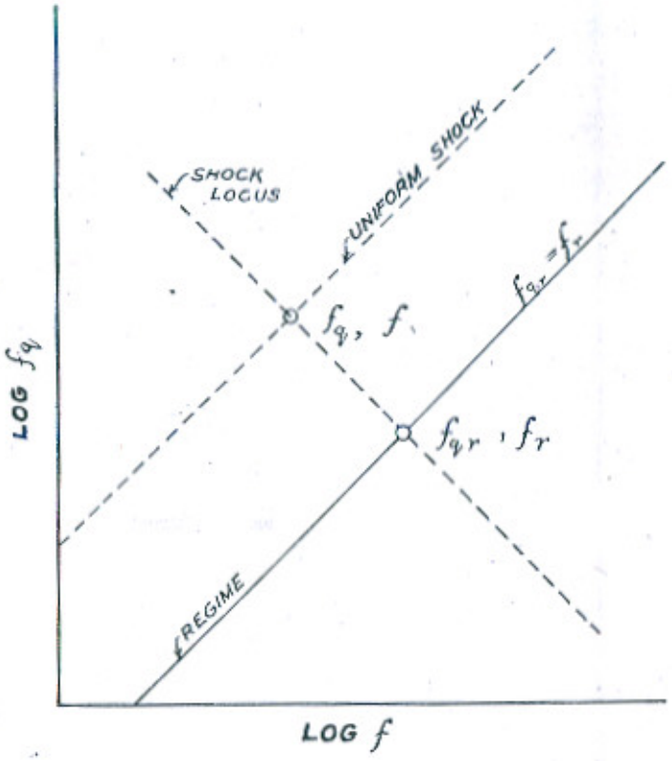
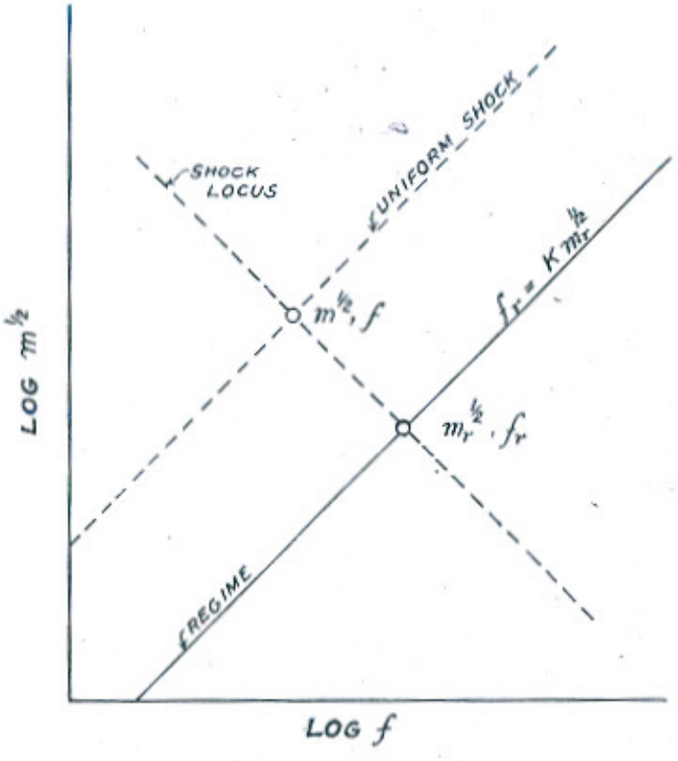


FIGURE 2



DISCUSSION

Mr. Gerald Lacey in introducing the Paper remarked that it will possibly facilitate discussion if I may be permitted to elucidate the mathematical treatment of the shock theory, which, from its brevity in the Paper, may appear somewhat obscure.

The first point is the nature of the Bose-Malhotra equation which may be written in a general form

$$S \propto m^a/Q^b$$

or as modified in the Paper

$$S \propto m^a/q^c$$

The statistical analysis carried out in the Punjab shows that this equation has a high degree of correlation. A study of the data also shows that there is great variation in the value of Kutter's N, which reflects "channel condition". This departure in channel condition, in channels otherwise in regime is due to "shock".

Whether there is shock or not, the degree of turbulence is exhibited by the slope adopted and the grade of bed silt exposed. When there is no shock, the turbulence is literally "straightforward" and can, therefore, be expressed in terms of the mean forward velocity and the hydraulic mean depth, that is to say in terms of the silt factor f as computed in terms of V and R, and also, ultimately, in terms of the forward velocity distribution.

To sum up, whether there is shock or not, the computed value of f_q is appropriate to the grade of bed silt exposed. When there is no shock the value of f as calculated from V and R will be equal to the value of f_q. The values of f and f_q can therefore, be employed to calculate shock. In order to develop a shock theory it is not necessary to introduce the diameter of the silt particle, although the physical background to the theory can only be brought out by studying silt distribution curves.

Effectively we are always presented with values of the silt factor assessed in two different ways. The method presents us with f_q, and the other with f. We have also to bear in mind that when there is no shock, and the channel is in ideal or perfect regime, the values of f_q and f are equal.

In Figure 1, f_q and f have been plotted to a log base. When any channel is free from shock, the two values will be equal. Hence $f_{qr}=f_r$ and the full straight line represents this equation. When there is positive shock, high values of f_q are associated with low values of f. The broken line represents a series of channels, all subject to the same degree of shock. The normal to the basic equation is termed in the Paper, a rigid mathematical inversion. It represents the departures which constitute "shock".

The equation to this normal is

$$f_{qr} \quad f_r \quad = f_q \quad r$$

and since f_{qr} is equal to f_r we obtain the equation

$$f_r = \quad (f_q \; f)^{\frac{1}{2}}$$

which is that derived in the Paper.

This simple direct solution eliminates the silt grade, but only recourse to silt grade can give the equation a physical background.

It is true that we could obtain an approximate value of f_r by taking the arithmetical mean of f_q and f instead of taking the geometrical mean. The method is faulty. If we have two observations of the same function measured in the same manner, the correct method certainly is to take the arithmetical mean. When, however, we are attempting to measure the same function by two different methods and the departure is *not* due to error, but to a known hidden variable, shock, the departure is functional and the mathematical treatment different.

If shock is calculated in the manner given in the Paper, by plotting equation (5), care must be taken to plot values of the log of f against values of log $m^{\frac{1}{2}}$. If this is done the normal to the curve will represent shock. If values of log f are plotted against values of m, the shock locus is not given by the normal. I am indebted to Dr. Malhotra for pointing out this difficulty. In Figure 2 the correct presentation of the shock theory, in terms of f and $m^{\frac{1}{2}}$, is given.

In Figure 1 no difficulty can arise, and the departures are measured by the most obvious method, the

normal. The shock theory is essentially a physical explanation of departures on the assumption that they are not due to error or to chance, but are functional.

Mr. Radha Krishna Khanna said that yesterday while speaking on Mr. Malhotra's Paper, Mr. Lacey took exception to my considering his silt factor f an imaginary conception. A word of explanation is necessary from me as to why I consider his silt factor to be an imaginary conception. Part of the explanation was already given by me, when I pointed out yesterday that, inspite of elaborate preparations for designing Haveli Canals to a silt factor of ·8, it was physically impossible to adhere to this value of f on many canals, such as, Fazilshah, Kurranga, etc. Moreover, whatever be the value, of f in the Main Haveli Canal, it could have no bearing on the values of f of canals taking off at Sidhnai on account of the value in the four or five miles stretch of the River Ravi below the outfall of the main canal. This is between ·4 and ·5, as pointed out by Dr. Bose. Again, since general orders were issued in the Western Administration of the Punjab Irrigation Branch that all the channels to be remodelled should have ·8 or ·9 as the minimum value of f I have seen many harassed Executive Engineers calling in the help of their head draftsmen to discover a method for determining the value of f for existing channels. There is only one section of a Lacey Channel for a given discharge and silt factor, and the sections of existing channels are, as a rule, very different from the Lacey section.

For the above reasons I maintain that Mr. Lacey's silt factor is a myth and the present attempts of Mr. Lacey to explain away some of the glaring inconsistencies in his theory by his new 'shock theory' are not convincing. His shock theory in a word amounts to this, if f should differ from the supposed correct value, we have to ascribe the difference to another imaginary factor "shock", and if the wetted perimeter should differ from the standard value $2{\cdot}67\sqrt{Q}$, we have again to ascribe it to shock.

After reading Mr. Lacey's Paper on the Shock Theory, the following questions suggest themselves to me :—

What exactly is the physical significance of shock in flowing water, and how it is measured ?

Is it there a resistance or back pressure to the forward flow of water, and can we not treat it as a part or component of the general resistance to flow ?

Can we ascertain a similar conception of shock, as we have of turbulence ?

What part does shock play in our understanding of the behaviour or characteristics of flow of water in earthen channels?

Another important question is what can possibly be the significance of negative shock assuming that positive shock is in the nature of turbulence or some sort of resistance. Is negative shock in the nature of acceleration in the flow of water.

One more work I have to say in connection with Mr. Lacey's equation $V=16{\cdot}05\ (R^2s)^{\frac{1}{3}}$. In paragraph 5 he says that this equation holds good on all regime channels free from shock. In the next paragraph he says that regime channels are very seldom free from shock. It follows from this that his equation very seldom holds good. I say that it does not hold good anywhere because my equation is $13R^{\cdot 6}S^{\cdot 3}$ which holds good everywhere, without reservations of regime and shock, etc., and gives results about 30 per cent. less than Mr. Lacey's. If one of these two equations is right, the other must be wrong, and the right one is obviously that which holds good in all cases.

Mr. Blench commented that, as Mr. Lacey had made an addition to the Lacey Theory, he would state his views on how it affected earlier work so that, were he incorrect, Mr. Lacey could clear the position.

The Lacey theory, up to date, gave three fundamental equations which fitted the average behaviour of channels that had formed in their own transported material. These equations are correct for an ideal channel in which the sides and bed are of the same incoherent

nature ; and there is good reason for believing them dynamically correct. Actual channels have sides different from the bed, so for this, and a variety of reasons, few real channels obey the equations exactly ; but, for practical design, this deviation from perfection is of small consequence. Mr. Lacey's present addition to his theory attempts to explain one major cause of deviation from perfection and need not, therefore, affect principles of design now in use for remodelling where there are channels to act as a guide to the silt factor to be adopted.

From Lacey's fundamental equations others can be deduced and, in practical remodelling, it is advisable to use $S=0{\cdot}391\ f^{\frac{5}{3}}\ q^{\frac{1}{3}}$ to find f, and then use that f for new slopes required by changes of discharge. The use of this value of f ensures that a remodelled or new channel will not silt.

To find channel sections, there is not usually any objection to using the same f, although the "shock" element would require that a smallar f (in general) should be used for a real channel.

Where the addition to the theory comes in useful is when an entirely new canal system has to be designed, and the only guide to f is the mean diameter of silt likely to enter the canal. If Lacey's f, m relation be used to assess f, it assesses the f for ideal conditions, and this is generally less than what is required for designing slope. Appendices A and B would then be useful in deciding how much increase to give to the ideal f to find the value suitable for the slope formula.

Incidentally it appears that attention to channel maintenance can affect slope appreciably.

For the addition to the theory, regarded theoretically, it was noteworthy that orthogonality of deviation from the ideal f, m line on log paper does not imply orthogonality on an ordinary f, m plotting. The dependence of orthogonality on a particular type of plotting naturally suggests that there may still be room for improvement in the new theory ; but there seems little doubt that the new idea is on the correct lines, and merits test. Practically, there is little difference between the geometric

mean now recommended, and the arithmetic mean, and if some deviation other than the orthogonal proved correct, the designer would hardly detect the difference.

The new theory brought out the reason why the Punjab Irrigation Research Institute had not been very successful in establishing Lacey's f, m relation (which was based on so wide a range of m that variations of m within one canal system—or between systems much alike—would be of small consequence). Now that an explanation of deviations within a system had been given it was to be hoped that the Punjab would consider the official adoption of the Lacey theory, in place of that of Kennedy which allowed channels to be designed in an infinite number of ways without any consideration of what the channels were trying to settle down to permanently.

Dr. Bose remarked that Mr. Lacey in his Paper on Stable Channels in Alluvium, first gave out the idea of the silt factor "f" and proposed the following relationship.

$$f = 8\sqrt{d} \qquad (1)$$

where d is the average silt diameter in inches.

In the Punjab Irrigation Research Institute observations were made to verify this. Not only did we fail to do this but it was further found that all the f's introduced by Mr. Lacey in his different formulæ were dimensionally different and gave for the same channel in regime different numerical values. Various attempts have been made by Messrs. Lacey, Blench and others to get over the dimensional incongruity. In his Paper Mr. Lacey has introduced a new idea of "shock" by means of which he has attempted to get over the quantitative difficulties.

According to Mr. Lacey the relation between f and m, the average silt diameter, given for regime conditions without "shock" by

$$f_r = K m_r^{\frac{1}{2}} \qquad (1)$$

which Mr. Lacey calls the regime curve—a parabola whose parameter remains constant so long as the coherence of the bed silt charge remains constant.

If f_q is given by

$$S^x = 0{\cdot}391 \frac{f_q^{\frac{5}{3}}}{q^{\frac{1}{3}}} \qquad (2)$$

and m is the average diameter of the bed silt of channels in regime but not free from "shock", then f_q and m will lie on this parabola.

If now f is given by

$$f = 0{\cdot}75 \frac{V^2}{R} \qquad (3)$$

for the same channel subject to the shock then (f, m) will not lie on the parabola, but will be displaced normal to the regime curve, *i. e.*, it will lie on a curve orthogonal to the parabola. This orthogonal curve is defined by Mr. Lacey as the "shock locus' and is represented by

$$m^{\frac{1}{2}} f = m_r^{\frac{1}{2}} f_r \qquad (4)$$

where (f_r, m_r) are points on the regime curve to which f, f_q and m will converge as the "shock" disappears.

Here Mr. Lacey has made a mistake. Curve (4) called as "shock locus" is not normal to the regime curve (1). If curve (1) is plotted on a log base with log f and log $m^{\frac{1}{2}}$ as variables then curve (4) will be normal to it but if (1) is plotted on the natural scale or any other scale even on a log base such as log f and log m, curve (4) will not be normal to it. Hence the normality of curves (1) and (4) is a geometric delusion and not an algebraic fact. Of course Mr. Lacey may disown all normality and inversion and say that his "shock locus" is given by (4)

$$m^{\frac{1}{2}} f = m_r^{\frac{1}{2}} f_r$$

as much as his regime curve is given by (1)

$$f_r = K m_r^{\frac{1}{2}}$$

then eliminating K he finds

$$f_r = (f . f_q)^{\frac{1}{2}} \qquad (5)$$

a relation which was produced by Mr. Inglis in his criticism of my Congress Paper No. 192 in 1936. Hence in this Paper Mr. Lacey has not given us anything further than what was given by Mr. Inglis in 1936.

Mr. K. K. Framji said that in Sind anyone who has done any practical redesigning of channels agrees with Lacey. Nothing has been discussed to equal it. We would not employ imitations of the Lacey theory which appear in some of our periodicals. We attempted to introduce our own modifications of Lacey's theory but they were all empirical, and did not satisfy any physical or mathematical analysis. We have now returned, very wisely I should say, to Lacey. This does not mean that we have given up the investigation of our difficulties for ourselves. As a matter of fact, it was Sind which, I trust I am right in saying, was the first explicitly or specifically to point out the differences between the values of the Lacey silt factor obtained by visualising it from the different hydraulic criteria. It was then that Mr. Inglis igeniously found a way out of the divergence, so similar in form as to be identical with what now Mr. Lacey has derived *ab initio*.

We are still in Sind contending with a suitable silt factor, and Mr. Lacey will be, I believe, the first to agree, our problem in a more complex one due to the coherent nature of our silt resulting from the large and varying percentage of colloidal matter. With us, it is not merely '*m*' the weighted mean diameter of the silt particles which appears to govern '*f*' and the behaviour of channels, but some other, as yet unknown, factor of coherence, due to the fact that there is one settling velocity at which silt particles are dropped and another and higher scouring velocity at which these particles are picked up again; that is, there is a V_o for silting and an $n\ V_o$ velocity for scouring, where n is >1, this is believed to be affected by the coherence of our silt.

Thus, for example, Mr. Lacey's equation (1) of the Paper was tested by me for the site, mentioned by him in his para. 11, namely R. D. 21,000 of Rohri Canal (*vide* statement I) K, the coherence factor, for the same site is found to vary widely from 1·43 to 2·05. For the particular values chosen by Mr. Lacey, even for the same period of the season, the coherence factors are 1·96 and 1·65. For those values of the data, where the discharge

and slope are practically constant (S. Nos. 6 and 9), the coherence factors are, however, nearer, being 1·56 and 1·54 ; similarly for S. Nos. 8 and 13 with practically equal discharges and slopes, the coherence factors are 1·61 in both cases.

This may mean either that the degree of shock is different, or that the coherence factor varies from season to season, even for the same site in regime. I would request Mr. Lacey's opinion on this, so as to enable me to make a start with the problem of the part coherence plays in channel behaviour and on regime channel sections.

The Author explains in his para 6 that when f (derived from V.-R. equation 2) equals f_q (derived from equation 4) there is no shock. But it is not clear what are the physical conditions to judge whether a channel is free from shock, before using the equations which alone are applicable. For example, Mr. Lacey uses the Upper Bari Doab, the Lower Chenab and the Madras Godawari Canal Data for obtaining his equation

$$f = 0{\cdot}75 \frac{V^2}{R}$$

applicable only to channels "free from shock". How did he decide these channels were so in practice, as he himself points out regime channels are very seldom entirely free from shock.

Analysing the site at R. D. 21,000 of Rohri Canal, quoted by Mr. Lacey, in his para. 11 for which he has shown $f_r = \sqrt{f.f_q}$ constant, in spite of the variations of f and f_q, which he attributes to different degrees of shock, I have observed the following difficulties for which I request Mr. Lacey's explanation.

Mr. Lacey has selected observations (2) and (10) for the same period of a year, namely September 1937 and 1938 presumably because the silt charge, distribution and coherence are expected to be similar, as the silt distribution observed over different months is known to follow some periodical curve, resembling a sine curve. But these observations unfortunately have different discharges and varying slopes : the discharges being 8710 and 9497

cusecs and the slope ·068 and ·046 parts per thousand, respectively.

If we take observations practically equal in discharge and slope, such as items 6 and 9, (where discharges are 8751 and 8758 cusecs and slopes ·042 and ·046 parts per thousand) we have f_r as ·583 and ·596, respectively. Similarly for items 8 and 13, with practically the same discharges, 9171 and 9071 cusecs, and nearly equal slopes, ·046 and ·048 parts per thousand, the f_r work out to ·647 and ·61, respectively.

Generally for the same site, for various observations spaced through two years f_r varies from ·482 to ·680, being as would be expected, highest in the abkalani and minimum in the winter.

I should like Mr. Lacey kindly to explain whether this divergence is due to the unknown coherence factor or degree of shock, and how to set about determining it or eliminating it by some correction factor, so as to have, if possible, one "f" for our channels, instead of having to work out slope from the f_q and the section from basic f, as he has recently suggested. It will appeal more generally and be possible of application correctly by all if such distinction in f is eliminated.

With regard to the mathematical derivation of his equation (7), which he calls a rigid inversion of equation (5), I frankly confess I have spent hours without success in attempting to derive it. It is not very clear from the brief five or six lines in para. 8 how Mr. Lacey has arrived at the form

$$m^{\frac{1}{2}} f = m_r^{\frac{1}{2}} f_r$$

of the equation for the "shock locus,"

If he means a curve always normal to the regime curve

$$f_r = K m_r^{\frac{1}{2}} \qquad (5)$$

then the correct expression obtained from solving the differential

$$\frac{d f_r}{d m_r} = - \frac{2}{K} m_r^{\frac{1}{2}}$$

would be I think of the form

$$f_r = C - \frac{4}{3K} m r^{\frac{3}{2}}$$

and not the one obtained by the Author. If, on the other hand, he means a line normal to the straight line representing (5), when plotted to a logarithmetic base, I feel it is possible for the equation to assume the form

$$m_f^{\frac{1}{2}} = m^2 f r^{\frac{1}{2}}$$

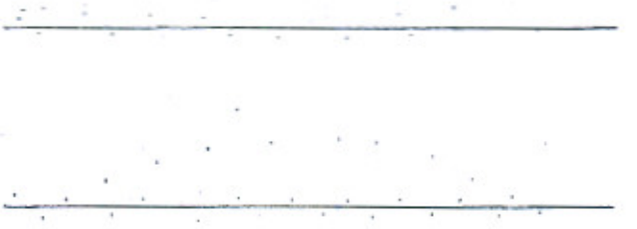
and not equation (7), namely

$$m^{\frac{1}{2}} f = m_r^{\frac{1}{2}} f$$

Mathematicians and statisticians may dispute the rigid accuracy of using a logarithmetic plane to give a true picture of the results. Similarly strict mathematical analysis, even accepting equation (7) as correct, and combining with (5) would mean that the two curves (5) and (7), *i. e.*, the regime and the shock locus will interest at (7)*a*,

$$f_r = K^{\frac{2}{2}} (m^{\frac{1}{2}} f)^{\frac{1}{2}} \ldots\ldots\ldots\ldots (7a)$$

But this is contradicted by the original assumption that all points representing any measure of shock must necessarily be situated at a fixed distance from the regime curve, as



evident from the sketch. Will Mr. Lacey explain to me where I am wrong.

SITE ROHRI CANAL R. D. 21,000.

Serial No.	Q	P	S (observed).	m	Log $(m^{\frac{5}{6}} q^{-\frac{1}{3}})$ = log (Z).	Log S (observed).	Log S − log (Z) = $\frac{5}{6}$ log K + log ·391.	K
1	2	3	4	5	6	7	8	9
1	8568	282·44	0·066	0·1105	−2·7088	−2·8195	+·1107	2·046
2	8710	285·37	0·068	0·1248	−2·7519	−2·8385	+·0806	1·963
3	8099	285·00	0·044	0·1393	−2·7752	−2·6435	−·1317	1·464
4	7454	285·25	0·048	0·0974	−2·6848	−2·6812	−·0036	1·748
5	8024	285·35	0·064	0·1229	−2·7583	−2·8062	+·0479	1·877
6	8751	285·32	0·042	0·1107	−·077	−2·6232	−·0845	1·563
7	8013	285·27	0·04399	0·1283	−2·7740	−2·6434	−·1306	1·467
8	9171	281·92	0·04599	0·1191	−2·7258	−2·6627	−·0631	1·610
9	7858	285·38	0·046	0·12721	−2·7581	−2·6628	−·0953	1·540
10	9497	285·30	0·046	0·11452	−2·7083	−2·6628	−·0455	1·650
11	8629	283·49	0·048	0·1441	−2·8044	−2·6812	−·1232	1·482
12	6681	284·93	0·050	0·14627	−3·8477	−2·6990	−·1487	1·431
13	9071	285·57	0·048	0·12438	−2·7451	−2·6812	−·0639	1·608
14	8561	285·53	·52	0·15428	−2·8313	−2·7160	−·1153	1·498
15	844643	285·82	0·04801	0·09394	−2·6539	−2·6813	+·0274	1·824

STATEMENT II.

Serial No.	Date.	Site and R. D.	Q in cusecs.	$S \times 10^3$	V in ft. per sec.	P in ft.	R in ft.	$m \times 10^3$ in mm.	$P/\sqrt{Q}$.	$2{\cdot}09\frac{m^{1{\cdot}86}}{Q^{0{\cdot}21}}$.	q	f^n	$f = \frac{1{\cdot}75v^2}{R}$.	$fr = \sqrt{f}\,\overline{fq}$.	$K = \frac{fq}{m^{\frac{1}{3}}}$.
1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16
1	22-7-37	Rohri ...	8568	·066	3·02	282·44	10·05	11·05	3·050	·607	30·3	·6805	·6808	·6806	2·046
2	24-9-37	Canal ...	8710	·068	2·97	285·37	10·27	12·48	3·058	·052	30·5	·6936	·6443	·6685	1·963
3	23-11-37	R. D. ...	8099	·044	2·78	285·00	10·24	12·93	3·166	·054	28·4	·5266	·5659	·5461	1·464
4	18-1-38	210000 ...	7454	·048	2·56	285·25	10·22	9·74	3·303	·043	26·1	·5455	·4808	·5122	1·748
5	18-2-38	Site ...	8024	·064	2·72	285·35	10·35	12·29	3·185	·052	28·1	·6580	·5363	·5940	1·877
6	17-3-38	No. 1 ...	8751	·042	2·99	285·32	10·25	11·07	3·050	·047	30·7	·5200	·6543	·5833	1·563
7	22 4-38	...	8013	·044	2·76	285·27	10·20	12·83	3·186	·054	28·1	·5254	·5602	·5425	1·467
8	19-5-38	...	9171	·046	3·20	281·92	10·18	11·91	2·944	·049	32·5	·5556	·7544	·6474	1·610
9	17-6-38	...	8758	·046	2·98	285·38	10·29	12·72	3·049	·053	30·7	·5493	·6473	·5964	1·540
10	21-9-38	...	9497	·046	3·29	285·30	10·12	11·45	2·927	·047	33·3	·5582	·8023	·6692	1·649
11	18-10-38	...	8629	·048	2·97	283·89	10·24	14·41	3·052	·059	30·4	·5624	·6463	·6030	1·481
12	10-1-39	...	6681	·050	2·37	284·93	9·88	14·63	3·485	·063	23·5	·5471	·4263	·4820	1·430
13	10-2-39	...	9071	·048	3·03	235·57	10·48	12·44	2·908	·051	31·8	·5672	6·569	·6105	1·609
14	10-3-39	...	8561	·052	2·93	285·53	10·24	15·43	3·085	·063	33·0	·5884	·6290	·6084	1·498
15	11-5-39	...	8446	·048	2·76	285·92	10·73	9·39	3·110	·041	29·6	·5591	·5325	·5456	1·824

Mr. Inglis congratulated Mr. Lacey on bringing his formulae yet another stage nearer to finality by introducing shock and cohesion, which explain why channels which are stable may yet not be in true regime. In this the chief criticism which Mr. Lacey had previously to face has been removed.

Mr. Inglis said he was naturally pleased that a formula which he introduced four years ago has now been shown to fit in with the Lacey formulæ.
The formula

$$f_r = \sqrt{f\mathrm{V}\mathrm{R}\ \ f_q\mathrm{S}}$$

makes it possible to determine the true regime silt factor and then to allow for any degree of divergence from true regime which experience showed to be desirable.

Mr. Inglis held that as there is a high degree of shock in all channels, the Author's terms free from shock, positive and negative shock were incorrect and misleading, and he suggested the adoption of the terms "Normal, super-normal and sub-normal shock."

Mr. Ishar Das said that a new slope formula $S = 0{\cdot}391\ K^{\frac{5}{3}}\ m^{\frac{5}{6}}\ q^{\frac{1}{3}}$ is now introduced by Mr. Lacey.

How does Mr. Lacey define the mean diameter of the bed silt as mean m changes from time to time. Does he also take into account the proportion of fine silt to the course silt or is this immaterial for his purpose ? Evidently Mr. Lacey has obtained this formula by working on Dr. Bose's formula. Will he explain in his reply how he arrived at it.

Mr. Kennedy's theory of $V_o = {\cdot}84\ d^{{\cdot}64}$ has been attacked, and I think rightly, because Mr. Kennedy by neglecting the width B threw overboard the R, the hydraulic mean depth.

Taking his formula (1a)

$S = 1{\cdot}003\ m^{\frac{5}{6}}\ q^{\frac{3}{3}}$. For practical purposes a small mistake in m or q will result in a wrong slope and I feel in practice this formula will prove to be useless.

For purposes of design m, too, is an abstract thing. It is difficult to measure accurately as it changes from time to time. Like the Author's previous slope formula $S = \frac{f^{\frac{5}{3}}}{1700\ Q^{\frac{1}{6}}}$ it is extremely liable to error.

A formula S in $10^3 = \frac{Q}{32\ R^{\frac{10}{3}}}$ we derived for regime channels by manipulating Lacey's and other formulae about seven years ago. Now more authentic data of regime channels is available and by their examination the co-efficient may have to be changed. This formula may be more useful for practical purposes than any of the slope formula advanced by Mr. Lacey.

On page 100 of the paper, Mr. Lacey has put forward his perimeter equation with two more co-efficients for variance, *viz*, 2·5 to 2·8. In my improved Kennedy's diagrams I used these equations years ago and to the users of these they have to give a good zone of selection.

The Author said yesterday, in reply to my comments on Paper No. 234, that it was possible to design all channels on a canal system with one particular f. This is possible only if it is agreed for a particular discharge there will be only one slope. All 20 cusecs channels will have one particular slope and all 50 cusecs channels will have another alone and all 200 cusecs another and so on. There will be no common slope for channels of different discharges.

Soil is not so uniform as to admit of Lacey's assumptions Some lands are low and others are high, some slope rapidly and others gently, the slope of the country is seldom uniform. It is for the irrigation of such lands we construct channels and we must try to command them. At times a limited fall in certain lengths is available and we cannot increase it or it may be that we must have certain falls and certain slopes for the proper command of the land. Under such handicaps it is not for us to enforce Lacey's ideal slopes. We, as practical engineers, are called upon to design regime

sections for the given slopes for channels. It is in order to meet such cases that I gave a new relationship.

$$S \times 10^3 = \frac{Q}{32 R^{\frac{10}{3}}}$$

and said that it fitted remarkably the actual conditions. As I said yesterday it is derived at by combining Lacey's formulæ with Manning. This formula, connecting as it does S directly with Q and R and dispensing with any unknown entity such as f or N, would seem to be useful for practical purposes. It is simple, direct and straightforward.

Such relationships can be derived purely from Lacey's formulæ as well.

The formulæ so derived are :—

(1) $S^{\frac{10}{3}} = \frac{Q^{\frac{3}{2}}}{60\ R^5}$ or $= \frac{0167\ Q^{\frac{3}{2}}}{R^5}$

(2) $S^{\frac{10}{3}} = \frac{Na^2}{12{\cdot}1844} \quad \frac{Q}{R^{\frac{7}{3}}}$

The first connects S with Q and R without any silt factor, the second though it dispenses with the silt factor f, the Lacey's NA, co-efficient of rugosity, is there and may serve a useful purpose.

The **Author** in replying to the discussion thanked those members who had taken part in the discussion and replied very briefly in the short time available. The following is a more detailed written reply after he had been able to study the arguments and equations put forward by the various speakers.

The Author was somewhat disappointed with the criticism of Dr. Bose. Briefly, Dr. Bose failed to comprehend the reality of shock as a new, and very important concept in regime flow. Dr. Bose drew attention to what he considered a defect in the silt factor, the fact that it could be assigned different dimensions in the different equations into which it entered, and it was clear that Dr. Bose considered "shock" not as a reality, but merely a means of overcoming quantitative incongruities.

Dr. Bose criticised the Author's definition of shock, claimed that the Author had made a mistake, and finally remarked that the Author had not advanced the subject beyond the stage it had reached in 1936 when Mr. Inglis first suggested correlating the values of the silt factor obtained from the cross-section, and the slope, respectively. Had Dr. Bose adopted a more constructive attitude, his criticism would have been more valuable ; Dr. Bose would also have obtained an insight into one very remarkable incongruity in the Punjab equations, for which Dr. Bose was responsible.

The following comparative table of equations would facilitate discussion.

S. No.	Author.	Dr. Bose
1	$P=2{\cdot}667\ Q^{\frac{1}{2}}$	$P=2{\cdot}800\ Q^{\frac{1}{2}}$
2	$R=0{\cdot}472\ (Q/f)^{\frac{1}{3}}$	$R=0{\cdot}470\ Q^{\frac{1}{3}}$
3	$V=1{\cdot}155\ f^{\frac{1}{2}}\ R^{\frac{1}{2}}$	$V=1{\cdot}120\ R^{\frac{1}{2}}$
4	$S^*=0{\cdot}542\ f^{5/3}\ /Q^{\frac{1}{3}}$	$S^*=2{\cdot}09\ m^{\cdot 86}\ /Q^{21}$

It was clear to the dispassionate observer that the two sets of equations bore a remarkable "family resemblance" when it was borne in mind that Dr. Bose obtained his equations by means of a rigid statistical analysis. There was, however, as in many families, a "family quarrel", and it was well to know what it was. The Author insisted, whatever the precise correlation between the hydraulic mean depth and the mean velocity in regime channels might be, that the grade of the bed material *must* enter as a variable. Adopting the ratio $V/R^{1/2}$ as a criterion of grade, the following table made the fact that there must be *some* correlation very clear.

Dr. Bose criticised the Author's definition of shock, claimed that the Author had made a mistake, and finally remarked that the Author had not advanced the subject beyond the stage it had reached in 1936 when Mr. Inglis first suggested correlating the values of the silt factor obtained from the cross-section, and the slope, respectively. Had Dr. Bose adopted a more constructive attitude, his criticism would have been more valuable ; Dr. Bose would also have obtained an insight into one very remarkable incongruity in the Punjab equations, for which Dr. Bose was responsible.

The following comparative table of equations would facilitate discussion.

S. No.	Author.	Dr. Bose
1	$P=2{\cdot}667\ Q^{\frac{1}{2}}$	$P=2{\cdot}800\ Q^{\frac{1}{2}}$
2	$R=0{\cdot}472\ (Q/f)^{\frac{1}{3}}$	$R=0\ 470\ Q^{\frac{1}{3}}$
3	$V=1{\cdot}155\ f^{\frac{1}{2}}\ R^{\frac{1}{2}}$	$V=1{\cdot}120\ R^{\frac{1}{2}}$
4	$S^*=0{\cdot}542\ f^{5/3}\ /Q^{\frac{1}{3}}$	$S^*=2{\cdot}09\ m^{\cdot 86}\ /Q^{21}$

It was clear to the dispassionate observer that the two sets of equations bore a remarkable "family resemblance" when it was borne in mind that Dr. Bose obtained his equations by means of a rigid statistical analysis. There was, however, as in many families, a "family quarrel", and it was well to know what it was. The Author insisted, whatever the precise correlation between the hydraulic mean depth and the mean velocity in regime channels might be, that the grade of the bed material *must* enter as a variable. Adopting the ratio $V/R^{1/2}$ as a criterion of grade, the following table made the fact that there must be *some* correlation very clear.

$V/R^{\frac{1}{2}}$	Channel.	Description of silt grade.
0·732	Ismailia Canal, Egypt.	Very fine silt.
1·056	Upper Ganges Canal, distributary.	Fine sandy silt.
1·120	Bose data.	Bed silt, 0·20—0·40 mm.
1·460	Missouri River, U. S. A.	Coarse river sand.
1·620	Miani River U. S. A.	Gravel, sand and clay.
2·430	Rhine at Basle.	Gravel and shingle.
2·919	Isar River, Bavaria.	Coarse gravel and detritus.
4·281	Kander River, Berne	Very coarse detritus.

Dr. Bose had entirely failed to establish any correlation between the mean velocity, hydraulic mean depth, and the silt grade. The fact, however, that he obtained a *negative* result was no warrant for *positive* conclusions. Dr. Bose was not perturbed by his failure, but had he sought causes, the research might have proved profitable. The most interesting feature of the Punjab data was that very low silt factors were associated with high grades of silt, an inversion so remarkable that it demanded an explanation that Dr. Bose had not afforded.

Dr. Bose's failure was due to two causes. The first that he was dealing with a limited range of silt grade, the second that the variable element of shock was so great that his data were hopelessly scattered. Sooner or later Dr. Bose would realize the importance of shock, and when he introduced it as a variable, he would discover that '*m*' entered into more than one equation. As soon as this happened, Dr. Bose would also discover that '*m*' had different dimensions in the different equations, and would further be faced with quantitative difficulties due to varying coherence and silt charge.

It was in 1920 (Proceedings Inst., Civil Engineers, Vol. 216, page 272) that Messrs. Lindley and Crump dis-

covered independently that the rugosity co-efficient N on main lines was inversely proportional to Kennedy's critical velocity ratio. It was common knowledge that, other things being equal, Kennedy's critical velocity ratio was a direct function of silt grade, or rugosity, and the fact was immediately evident, if any further proof was required, that "shock" or effectively "channel condition" could convert a direct relationship into an inverse one.

Dr. Bose had challenged the "shock normal" of the Author. It was hoped that the introductory remarks of the Author made this point clear. It was necessary *to plot the logs of both sides of an equation* and not to plot the variables f and m. When this was done, the shock locus was that given by the Author. It had to be borne in mind that the departures were not statistical and due to error but functional aberrations due to a hidden variable. The method was original and it was not surprising that Dr. Bose had found no precedent in his statistical work.

The Author was indebted to Messrs. Blench and Framji for criticism of a very constructive character. Not only did inattention to channel maintenance affect slope very considerably, it also affected cross-section. Thus a badly maintained channel would be subject to shock, and as a result, the value of fq would be increased. Further, it would be necessary to run such a channel with an increased gauge for a given discharge. That meant that the cross sectional area would be increased and the value of f reduced. If it was anticipated that a large branch would be subject to shock, the amount should be computed and the branch designed for the values of fq and f obtained. The Author hoped that he had made clear to Messrs. Blench and Framji, in his introduction, the method of obtaining the "shock locus". There was only one method of obtaining the normal and that was to plot logs of both sides of the equation, the log scales being made equal.

Two members, Messrs. Framji and Inglis, had enquired what exactly was meant by a channel free from shock. When the Author stated that channels were seldom entirely free from shock, he meant that they very seldom fell *exactly* on the line given by his general

regime equation which represented the normal or average of a very large number of channels. As Mr. Inglis had contended, it was, properly speaking, more correct to refer to normal shock. It was suggested by Mr. Framji that the equation

$$f = 0{\cdot}75\ V^2/R$$

applied only to shock free channels. This equation in form was obtained from the Madras and Punjab data but the constant followed from the general regime equation. It would be found that when the hydraulic mean depth was equal to one metre, this value of the constant made N, whether of Kutter, Manning or the Author exactly equal to ·0225. The equation defined the grade of sand to be expected in India in a channel free from shock and with a hydraulic mean depth of one metre.

The seasonal divergencies in the value of f_r as computed from f and f_q were due to variations in silt grade, silt coherence, and in all probability, silt charge. It must be remembered that f_r represented the bed silt if a channel were in perfect regime, and that this value must vary throughout the season since it was a function of the silt admitted to the canal.

The objections raised by Mr. Ishar Dass to the use of m, and q the discharge intensity, in designing channels appeared to have little force. It appeared quite incorrect to state that small errors or mistakes in assessing the silt grade or discharge could make a large difference in computing the slope and it was difficult to find a better means of calculation.

The suggestion that error might occur arose from Mr. Ishar Dass's preference for his own equation

$$S^* = Q/32\ R^{\frac{10}{3}}$$

Unfortunately Mr. Ishar Dass had made a serious mistake ; he had treated the Manning equation as if the value of N did not matter and had reproduced it in the form

$$V = 67\ R^{\frac{2}{3}}\ S$$

which obviously could apply to only one particular grade of silt and to no other. By means of this assumption, which was quite incorrect, he got rid of the silt grade. The correct method was to write the Manning equation in the form

$$V = \frac{1{\cdot}486}{N} R^{\frac{2}{3}} S^{\frac{1}{2}}$$

and Mr. Ishar Dass was then left with the trouble of assessing the value of N which was just as difficult to assess as the value of '*m*'.

The remarks of Mr. R. K. Khanna did not appear to warrant a detailed reply. His first question was concerned with the nature of shock. It appeared perfectly natural to conclude that any portion of the banks which was ragged and rough would cause local retardation, destruction of additional energy, and positive shock. Similarly any portion of the banks abnormally smooth, in clay freshly trimmed, would cause a local relative acceleration, the destruction of less energy and negative shock. The second question, which Mr. Khanna asked himself, was whether the conception of shock had any reality. He admitted that if shock was a real phenomenon, it must be regarded in the nature of a discovery of great importance to hydraulicians. Mr. Khanna, however, suggested that shock was of no significance other than a means of explaining away the contradictions and inconsistencies of the Lacey silt theory. The Author had already made clear that the shock theory explained the somewhat remarkable failure of the Punjab Research Institute to introduce silt grade as a variable in the correlation of the mean velocity and the hydraulic mean depth. The statement of Mr. Khanna that the Khanna equation

$$V = 13{\cdot}0\ R^{\cdot 6}\ S^{3}$$

held good everywhere without reservation of regime, shock, etc., was grossly inaccurate. If the statement meant anything at all, it meant that all data, when plotted, must lie on the Khanna curve.

The expression did not apply with any degree of accuracy even to the collection of 38 observations quoted by Kutter from which Mr. Khanna asserted that he had derived his equation. The formula was the worst fit to data of any extant. Had Mr. Khanna plotted his data correctly, he would have obtained the equation

$$V = 9{\cdot}44\ R^{{\cdot}510}\ S^{{\cdot}255}$$

Anyone who studied carefully the diagram (Sheet 5) reproduced by Mr. Khanna in his "Theory of Flow of Water" would immediately recognise that Mr. Khanna's curve was a very bad misfit. That fact was that Mr. Khanna's curve did *not* fit, in the very least, the Kutter data on which Mr. Khanna placed so much reliance, but by pure chance, and for no other reason, happened to be a somewhat close approximation to the better known Lacey general regime equation. The Khanna equation had nothing to recommend it.

The statement of Mr. Khanna that his own equation gave results 30 per cent. less than the Lacey equation was a wild mis-statement of fact. With velocities of 2·0′ per second, the Khanna equation gave *identical* results and within the entire range of normal velocities on canals the results did not differ by five per cent. either way.

Since the writer's Paper had been read, Dr. Malhotra, at a research meeting at Simla in July, 1940, had drawn attention to a very simple method of computing the value of S_r direct from V, R and S.

If for the regime velocity V_L were written

$$V_L = 16{\cdot}05\ (R^2\ S)^{\frac{1}{3}}$$

it could easily be deduced that

$$S/S_r = (V_L/V)^{\frac{3}{2}}$$

From this equation Dr. Malhotra derived the very simple equation

$$Sr = \frac{V}{R}\ \frac{(VS)^{\frac{1}{2}}}{64{\cdot}2}$$

a form very suited to computation as no cube root was involved.

It would be found that all the values of S_r given in the Appendices of the Author's Paper could be readily checked in this manner.

The Author fully realized that as the concept of shock was new it was very difficult to present it in a simple form which would win ready acceptance. It was however considered desirable to publish it on an early a date as possible in order to invite discussion. In conclusion he would again thank all those members who had contributed to the discussion.