PAPER No. 184.

THE LACEY SLOPE FORMULA

By T. Blench, I.S.E.

The Central Board of Irrigation Research Committee, in July 1934, unanimously accepted the Lacey formulae as the best available for practical design. It is not, therefore, intended to attempt any justification of them ; but some discussion is necessary because any one channel may depart considerably from the average laws deduced by Lacey. Whence it follows that a very large amount of data is required to satisfy any independent inquirer of the truth of the laws, or, alternatively, to fix what is the true average regime condition of a set of existing channels.

To illustrate, the writer has had two years of a sub-division subject to a remodelling programme and has failed to collect enough data to prove the Lacey laws independently, although the whole sub-division can be explained in terms of them, and a single f obtained applicable to all channels.

This Paper is intended to show that the f of a large system of canals is most easily obtained by the slope formula of the title, *i.e.*, $S=f^{\frac{5}{3}}/1788 \quad Q^{\frac{1}{6}}$.

The value of the slope formula is that, once f is known the final regime slope (provided outlets are not set so absurdly high as not to get a fair share of silt) can be calculated, as can the channel shape.

Surveys for remodellings normally include surface width of channel, depth taken somewhere near the centre, waterlevels at falls or masonry works, and interpolated levels in between. Occasionally a few accurate discharge observations are added. Often breadth is omitted.

If the behaviour of the channel is studied from these, before remodelling, it is usually assumed that the overseer's interpolated levels are correct and the channel slopes uniformly between falls. The remodelled slope may therefore be made constant over impossibly long reaches and has to be maintained artificially. This is one source of non-regime channels.

Again, if breadths be omitted, a channel breadth very much different from the natural one may be designed and result in continued artificial maintenance.

Yet again, it is usually assumed that channels are trapezoidal, and their discharges are calculated on that assumption. For very small channels this is very much in error, as the sections are more nearly

elliptic, Consequently if v/v_0 as actually obtaining is estimated on this assumption it comes out too small. See Fig. 1, which shows actual sections and their data.

Again, slopes are often designed flatter than existing to "suit command"; or made the same as existing but with a reduced discharge. It is a foregone conclusion that these channels will silt.

Finally, channels may be remodelled with a new type of outlet set high above the bed : and if the bed cannot adjust itself readily (as in a large channel) the silt factor increases towards the tail giving silt trouble there.

It is clear, therefore, that most sub-divisions will contain some channels which can never be allowed to attain regime except at the expense of sanctioned water distribution.

Passing on from these man-made " non regime " channels we come to those which are in cutting in hard soil which never scours, *e.g.*, the Lower Jhelum Canal in its head reach where 1/6666 can be observed, although the filling reaches at the same discharge have a consistent 0·11/1000 slope.

Add to such cases those where continued neglect allows overgrown berms to drop into the bed, jungle to stick in the berms and bed, cattle to wallow in minor tails, and enough has been said to show that only a competent local officer can sift the large amount of improper data from that which is truly applicable for determining the silt factor of a system with a view to its remodelling.

The fact remains, however, that if a channel is free to form itself it will do so according to the Lacey rules ; if aided scientifically it will respond very rapidly ; and if constructed originally to the rules it will continue according to them.

The Research Department has done valuable work finding f from $d=\frac{f^2}{64}$ an approximate formula. Apart from the fact that it seems certain that quantity of silt must have some effect there is the difficulty of defining what is meant by d, the mean sand particle diameter; and also deciding whether to take mean silt over a section, or just the rolling silt. The investigation is, of course, the soundest one for relating river silt with canal silt—the problem in building a new headworks ; but it is far too rough for canal work when the canal has been running for many years and it is desired to remodel, or make new distributaries.

Another investigation has been done to find f from the fundamental formula $v=1.151\sqrt{fR}$. This has the defect that no one can possibly choose a true representative section ; and the percentage error in f is twice that in the discharge.

The formula $S = f^{\frac{5}{3}}/1788Q^{\frac{1}{6}}$ is mathematically the soundest because the slope automatically averages out the effect of all the varied sections in the reach over which S is measured.

It is practically the best because S is very accurate and is measured as a matter of course in all surveys; and f depends on $Q^{0.1}$, so that no ordinary error in measuring Q can have any appreciable effect at all. Looked at another way, we see that f can be estimated over a reach of a distributary in which there is 10% of draw-off to 1% accuracy. An abstract of data relating to Jahanian Sub-Division is given in Table 1. The observations were taken every 1000 feet from surveys done mostly by another officer prior to remodelling. The original data is too bulky to publish and was used for testing all the Lacey formulae and the Kennedy one, and the Kennedy diagrams : it exists for any one who is interested.

The results of the table are shown in Fig. 2. It will be seen that $\sqrt{f}$ is constant between badly designed minor offtakes, *viz.*, 1R/2R and 3R/2R. The reason for the low $\sqrt{f}$ in the head reach is probably a change in the system of regulating the head. This reach was the only one levelled specially for the writer. The old records showed its slope as 1/6000 which makes $\sqrt{f} = 0.91$.

It is interesting to note that the proportional distributor for 1L/2R distributes $\sqrt{f}$ equally.

The tail reach "average slopes" near R.D. 100.000 were too far from actual to be used, so $\sqrt{f}$ was found from the fundamental formula.

On Fig. 2 is also plotted v/v_0, found on the assumption of trapezoidal sections. It will be noted it is definitely a function of the discharge.

The channel was fitted with outlets sufficiently low to be assumed to get a fair silt charge and had not been interfered with for several years.

Table II is for 1L, a channel that had run without interference for many years, except at the remote tail. As it is very little in cutting and has good berms it is excellent for exact analysis. Also its outlets are set deep or the bed has risen to near crest level.

It shows the same remarkable approximate constancy of $\sqrt{f}$; and has the same $\sqrt{f}$ as its 850 cusec parent 10R ; and has the same $\sqrt{f}$ as its own minor which is fitted with a proportional head just above a fall.

Table III is for 10R and all its offtakes. The parent 10R gives about 30% of its discharge to direct outlets, 60% to distributaries, and 10% to absorption. It is mainly in filling, with adequate berms. Offtakes, both outlet and distributary, are set reasonably low, in general ; so it is not surprising that the $\sqrt{f}$ is practically constant from head to tail.

The distributary $\sqrt{f}$'s vary considerably as calculated from their head reaches ; and every case of variation has been found to have a sound and obvious cause at site, by the slopes not being regime ones. Were the true $\sqrt{f}$'s wanted it would be necessary to choose regime reaches.

Such excellent indicators of abnormality in construction or design are these discrepant $\sqrt{f}$'s in that it is worth giving a short history of the channels.

1L has been left alone for years and has, to all appearances, formed itself and settled down. Hence its $\sqrt{f}$.

1R has been steady for years but its head reach is in cutting and the slope not natural.

2L was steady till April 1933, in cutting for most of its first 6 miles. The soil was exceedingly hard. In 1933 remodelling, the channel was raised some 10" and it was assumed it would still run at its slope of 1/5000 far steeper than regime. This it has refused to do in spite of bushing and daffs and is now in for remodelling.

2R has already been discussed. It takes off a few hundred feet above a regulating fall and used to be regulated by working its gate. The writer stopped this practice and had regulation done at the fall in the interests of direct outlets. This seems to have made the head reach reduce its $\sqrt{f}$ to that of the parent.

4L was remodelled in 1931. It's $\sqrt{f}$ has been worked out from average slope and discharge. It takes off immediately above a fall with the same crest level as its head regulator karrie regulation is done at the fall occasionally, but never in the hot weather.

3R is mostly in filling. Its head crest is set about 0.6 channel depth. The $\sqrt{f}$ was calculated on the assumption of elliptic sections. The reach of 10R from which it offtakes has a width much wider than for final regime, probably due to a hard bed although the cutting is slight.

4R was remodelled in April, 1932 to a gentler slope and promptly silted back to what it was before. It is in regime at its old slope and the high $\sqrt{f}$ is due to it taking off immediately above a fall which is permanently blocked with karries to well above the head regulator crest level.

5R was remodelled in 1931 to about half its pre-remodelled breadth, and a slope of 1/4444 from head to 55,000 fall where Q is

about 10 cusecs. It is almost unnecessary to record that it has never run without continual bushing and rebushing.

6R was remodelled in 1931 and has flumes throughout fitted high above bed level. So the bed merely silts till it gets to about outlet crest levels and remains steady ; the water distribution being achieved by fitting roofblocks. Hence the high $\sqrt{f}$.

The tail reach of 10R is not too reliable as bushings are continually being done which scour out the bed and deposit the silt elsewhere.

The writer concludes from these observations :—

1. That f is a true physical constant of the silt content ; not like N or v/v_o which vary with discharge.

2. That no great abnormality of offtake design is required to ensure f being constant over distances of, say, fifty miles.

3. That proportional distributors divide silt content equably.

4. That regime can be tested for by working out f by the slope formula for several channels of an area and noting those whose f's agree.

The writer is well aware that the sub-division chosen is special, in that it is at the tail of some 130 miles of main line. It is to be expected, therefore, that seasonal variation of f would not be nearly so marked as at the head of a canal system. Thus at the head of the Lower Jhelum Canal a slope variation from 0.10 to 0.125 per thousand was noticed in the records of two years. This means f varying from 0.81 to 0.93 ; or $\sqrt{f}$ from 0.90 to 0.96. Whether it is the maximum or the mean that impresses itself further down is not known to the writer.

As a test of the approximate constancy of f throughout large areas the writer has asked chance met officers for the slope and discharge of some canal they know, and then informed them the slope to expect in a 10 cusec minor at its head. So far he has found no dissentients.

It would appear that the f's and slopes for 10 cusecs are as follows in large areas of the following canals :—

Canal.	f.	Slope per thousand for a 10 cusecs channel.
Lower Bari Doab Canal ..	0.64	0.18/1000
Derajat	0.83	0.29/1000
Upper Bari Doab Canal ..	1.00	0.38/1000
Upper Chenab Canal ..	1.10	0.45/1000
Lower Chenab Canal ..	1.20	0.51/1000

These chance figures are probably rather rough ; but they are very interesting in that they show where the legend that "there is no regime for minors" comes from. For there are clearly canals where the regime slope of small channels is so much greater than the natural slope of the ground that it is hopeless to have any but very short minors.

Another interesting fact is that water course slopes at the tail of the Lower Bari Doab Canal are about 0.30/1000 for 1.5 cusecs outlets ; so they would require to be about 0.90/1000 on the Lower Chenab Canal.

A last interesting point following from the Lacey formula is that the amalgamation of two silt-troubled outlets which are side by side may cure the trouble by reducing the watercourse slope. Conversely, reduction of an outlet discharge should be accompanied by designing it for a smaller working head.

TABLE 1. DATA OF 2R SYSTEM—0-110,000 TAIL.

From R.D.	0	20,000	41,000	50,000	60,000	72,000	81,000	98,000
To R.D. ..	14,500	29,000	49,000	54,000	67,000	80,000	87,000	100,000
Q average ..	173	123	103	81	65	40	33	18
S per 1,000 ..	0.125	0.18	0.19	0.196	0.212	0.18	0.20	?
$\sqrt{f}$	0.83	0.91	0.91	0.91	0.92	0.86	0.85	0.85
Name Work or Offtake ..	1R/2R	..	1L/2R & Fall.	..	3R/2R & Fall.	Fall	..	..
R. D. Work or Offtake ..	14,000	..	50,000	..	71,500	80,000	..	..
Q Offtake	35	..	15	..	15	..	..	..
S. Offtake/1,000 ..	0.278	..	0.26	..	0.425	..	..	..
$\sqrt{f}$ Offtake ..	0.98	..	0.91	..	1.02	..	..	..
Remarks on Channel ..	..	..	..	Between falls.	..	..	..	..
Remarks on Offtake ..	Outside of sharp curve crest below fall crest 14,500.		1L/2R crest level same as fall.		3R/2R crest level lower than fall and bad wings.			

TABLE 2. – 1L/10R SYSTEM.

Channel.	10R.	1L/10R at 4,400/10 R.					1L/1L at 24,000 of 1L/10R.
From R. D.	0	0	5,000	24,000	31,000	39,000	0
To R. D.	9,000	5,000	24,000	31,000	39,000	Tail.	5,000
Q.	850	75	..	..	..	..	20
$\sqrt{f}$.	0.80	0.80	0.80	0.825	0.835	Vague.	0.83

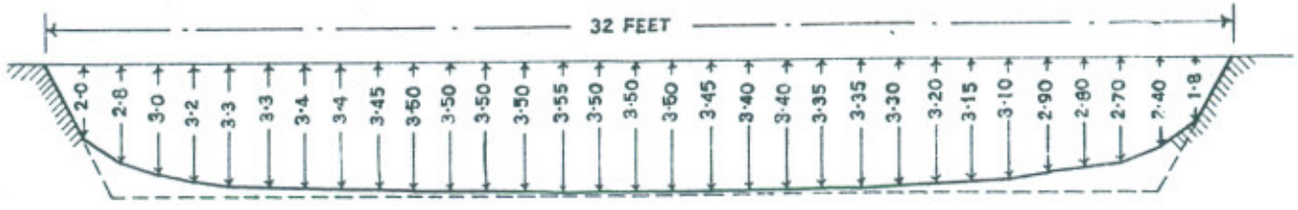
TABLE 3.—10 R SYSTEM O-TAIL 275,000.

R. D. of 10 R in 10^3 Feet	..	0	4.4	42.0	58.0	85.0	144.0	163.7	192.7	206.5	234.0	260.0
$\sqrt{f}$ of 10R nearly ..	..	0.80	0.80	0.80	..	0.80	..	0.835	0.825	0.81	0.82	†0.85
Name of Offtake ..	..	..	1L	1R	2L	2R	4L	3R	4R	5R	6R	..
Q of offtake ..	..	..	75	56	78	180	30	8	12	66	22.5	..
$\sqrt{f}$ Offtake by S formula ..	..	..	*0.80	0.925	0.91	*0.83	*0.835	*0.82	*0.915	?	0.89	..

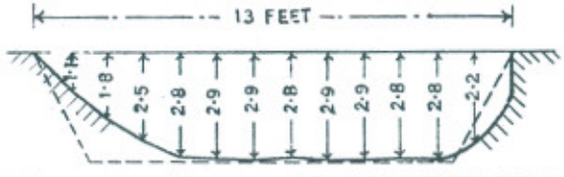
†Not very reliable.
*Regime channels.

FIG. 1.

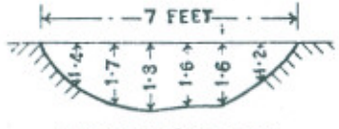
ACTUAL CHANNEL SECTIONS



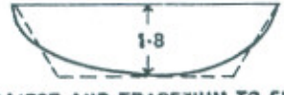
SECTION OF 2R/10R DISCHARGE SITE.



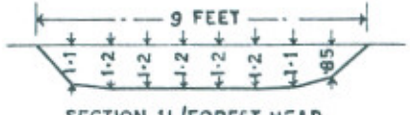
SECTION NEAR 1R/2R HEAD AFTER REMODELLING AND BUSHING.



SECTION 4R/10R HEAD.



ELLIPSE AND TRAPEZIUM TO SUIT.

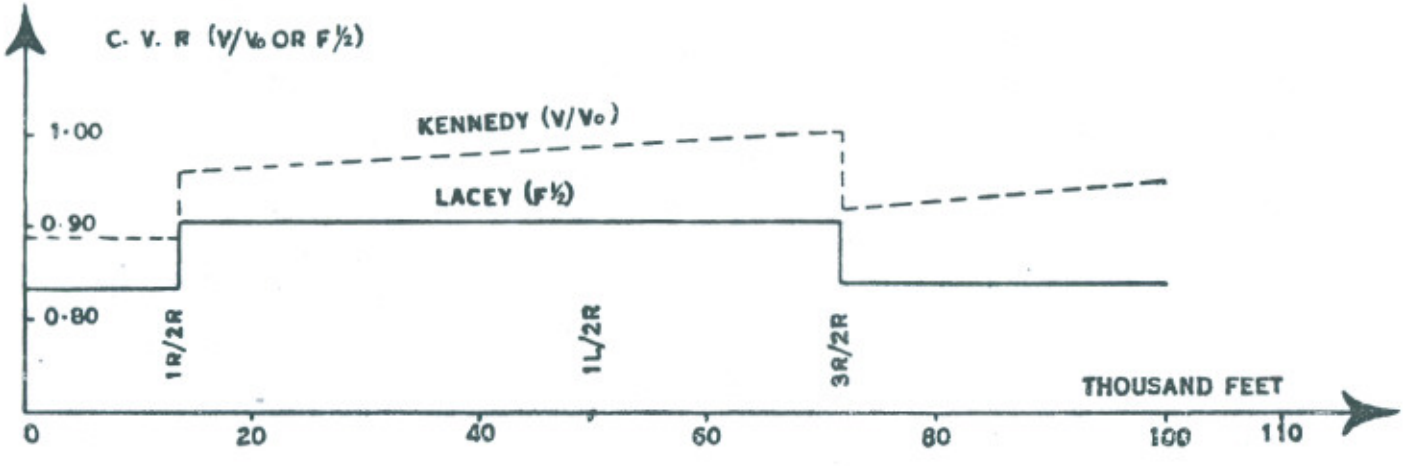


SECTION 1L/FOREST HEAD.

CHANNEL	ELEMENTS	OBSERVED VALUE.	TRAPEZOIDAL VALUE	ELLIPTIC VALUE
2R/10R.	A SQ. FT.	98·2	105·9	
	P = FEET.	35	38·1	
	R = FEET.	2·78	2·78	
	Q = CUS.	1·80		
1R/2R.	A = SQ. FT.	31·4	33	
	P = FEET.	16	16·5	
	R = FEET.	1·96	1·99	
	Q = CUS.	34		
4R/10R.	A = SQ. FT.	9·3	11	9·9
	P = FEET.	8·25	9·24	8·50
	R = FEET.	1·13	1·19	1·16
	Q = CUS.	12 DOUBTFUL		
1L/FOREST.	A = SQ. FT.	9·1	10·1	8·46
	P = FEET.	9·8	10·5	9·6
	R = FEET.	0·93	0·97	0·88
	Q = CUS.	12		

FIG 2.

VARIATION OF C. V. R. AND F½FOR 2R/10R



DISCUSSION.

Mr. Blench, in introducing the paper, apologised for what was really rather scanty data for the purpose, although it represented a tremendous amount of labour for one officer. He would not have published it were it not that (*i*) he knew that valuable confirmation had been obtained in other provinces and (*ii*) the Lacey Formulae gave a solution to problems until recently insoluble.

As examples he quoted :—

1. The Lower Jhelum Canal, by still pond regulation introduced 2 years ago, had reduced " f " in the head reach from 0·92 to 0·64. What would be the effect on channels free to scour in the irrigating tracts ? They will scour to very nearly half the slope corresponding to " f " of 2 years ago (if that " f " were 0.92). How long this will take is uncertain ; but it is believed that a heavy silt wave took five years to pass through the Upper Chenab Canal.

2. A 20-cusec minor, at present with the " f " of its parent, and a properly designed head is just on the point of drowning its head, and is always drowned at the tail where silt clearance is frequent. The outlets also require constant silt-clearance. It is required to remodel it for 10 cusecs. The parent has a good fall two miles above the offtake. How is the remodelling to be done ?

The remodelling is normally done by raising the tail and flattening the slope—a remedy that will give tremendous trouble continuously. Actually, the reduced discharge will require about 12 per cent. greater regime slope on the channel and watercourse. The only sound solution is to raise the tail, allow 0.40′ at least more working head on all outlets than at present, and allow the steeper slope on the channel. This will mean raising the parent about 1.50 feet—which is feasibl on account of the proximity of the fall.

Mr. S. L. Malhotra said that the author had, on the authority of the Central Board of Irrigation, taken it for granted that the Lacey Formulae were universally accepted. The formulae were, however, still questioned in the Punjab Irrigation Service which was still using Kennedy's Tables.

The author had selected the slope-discharge formula for calculating the value of " f " and had given no convincing reasons for preferring this to the fundamental Lacey Formula based on channel sections, from which the slope-discharge formula had been derived. The author explained the unreliable nature of hydraulic surveys carried out for remodelling schemes and had apparently worked out his C.V.R.'s by assuming channel sections as trapezoidal. The speaker remarked that for deriving laws of flow no assumptions should be made. Actual channel sections should be surveyed and

accurate data collected. The speaker enquired how the author observed the discharges of channels described by him in the paper. If he had been making assumptions as mentioned by him, his results were certainly far from correct. In some channels the value of "f" was as variable as the C. V. R. The values of "f" had been calculated from two of the Lacey Formulae and these differed widely from each other. The speaker requested the author to elucidate the following points :

(*i*) Why should the value of "f" from the two formulae differ if they were mathematically the same ?

(*ii*) Why should "f" from the slope formula only be adhered to ?

(*iii*) How does the author connect "f" and quantity of silt which was admittedly the guiding factor in design of channels.

Hydraulic surveys of channels remodelled on Routh's system was the easiest. Slopes and discharges were seen from outlet and meter gauges and channel sections could be accurately observed. The author should have observed a stable channel of that type and studied the value of "f" from accurate data instead of depending upon old unreliable surveys with assumed values of discharge. His way of working out C.V.R.s was liable to inaccuracy and so were the conclusions drawn therefrom.

Dr. N. K. Bose while criticising Mr. Blench's paper remarked that the author had said that if a channel was free to form itself it would do so according to Lacey rules ; and of those rules the author preferred the formula $S=f^{\frac{5}{3}}/1788\ Q^{\frac{1}{6}}$ and he thought that it was mathematically the soundest because the slope automatically averages out the effect of all the varied sections in the reach over which S was measured.

As the author had raised the question of the comparative accuracy of the various formulae proposed by Mr. Lacey it would be well if the engineers should examine his method of deducing those various relationships.

In his first paper Mr. Lacey derived his first relation :

$$V=1.17\sqrt{fR}$$

from a number of hydraulic data. In this he had introduced a factor which he called "Silt-factor" *f*, most probably with the idea that all the variations in the quantity

$$\frac{V}{\sqrt{R}}$$

were due to variations in silt and as such could be covered by a single

factor ' f.' Having thus introduced ' f ' he calculated it from a number of hydraulic data and plots Af^2 against Q the discharge and gets :

$$Af^2=3.8V_0^5$$

$$Qf^2=3.8V_0^6$$

These were his second and third relationships.

In his more recent publication Mr. Lacey deduced a new relationship which was based on the Lower Chenab Canal data utilised by Mr. Lindley. This was

$$V=K\ R^{\frac{3}{4}}\ S^{\frac{1}{2}}$$

and by a line of reasoning not shown by him puts this :

$$=\frac{1.3458}{Na}.\ R^{\frac{3}{4}}.\ S^{\frac{1}{2}}.$$

Subsequently he equated Kutter's Na to his ' f ' and got :

$$Na=0.022\ f^{\frac{1}{4}}$$

the reasoning in that case also was not clear.
However, he got a new relationship

$$V=\frac{1.3458}{0.0225\ f^{\frac{1}{4}}}.\ R^{\frac{3}{4}}.\ S^{\frac{1}{2}}$$

By eliminating V between this relation and his first one

$$V=1.1512\sqrt{f\,R}\ \text{ he got }\ S=370.68\times10^{-6}\ f^{\frac{3}{2}}R^{-\frac{1}{2}}$$

Again after eliminating R and V between this relationship and his first and third relationships :

$$V=1.1512\sqrt{fR}\ \text{ and }\ Qf^2=3.8V_0^6$$

he got the relation

$$S=\frac{f^{\frac{5}{3}}}{1788\ Q^{\frac{1}{6}}}$$

claimed by the author as the soundest mathematically.

The speaker said that he was afraid that the process of reasoning might appear complicated and involved to the audience. For their benefit he summarised the line of reasoning in the following way :

From a number of hydraulic data Mr. Lacey derived two relationships :

$$V\ =\ 1.1512\ \sqrt{f\ R}\ \text{ and}$$

$$V\ =\ K\ R^{\frac{3}{4}}\ \ S^{\frac{1}{2}}$$

which he transforms subsequently to

$$V = \frac{1.3458}{.0225\ f^{\frac{1}{4}}} \cdot R^{\frac{3}{4}} \cdot S^{\frac{1}{2}}$$

from which by eliminating V, he got

$$S = 370.68 \times 10^{-6}\ f^{\frac{3}{2}}\ R^{\frac{1}{2}}$$

Now if his line of reasoning be mathematically sound the values of 'f' derived from these three relations should agree. Not content with this slope relationship Mr. Lacey brings in his third relationship:

$$Qf^2 = 3.8\ V_o^6 \quad \text{and got } S = f^{\frac{5}{3}}/1788\ Q^{\frac{1}{6}}$$

Now there were four relationships containing 'f', two involving *V* and two involving *S*. Now if it could be shown that those four 'f's were the same qualitatively then they were mathematically sound and if they were the same quantitatively they would be sound from the hydraulic point of view. It did not require very much mathematics to show that all those 'f's were mathematically different from each other —f_1 in $V = 1.1512\ \sqrt{f_1 R}$ had the dimension of an acceleration and 'f_2' in

$$V = \frac{1.3458}{.0255\ f_2^{\frac{1}{4}}} \cdot R^{\frac{3}{4}} \cdot S^{\frac{1}{2}}$$

had the dimension of inverse length, f_3 in

$$S = 370.68 \times 10^{-6}\ f_3^{\frac{3}{2}} \cdot R^{\frac{L}{2}}$$

the dimension of cube-root of length, and f_4 in

$$S = f_4^{\frac{5}{3}}/1788\ Q^{\frac{1}{6}}$$

had a dimension which had not so far been assigned to any physical quantity. That was so far as mathematics could go.

The speaker further added that the author had calculated the value of 'f' from the last relation from a number of hydraulic data relating to channels in the Jahanian sub-division. As the author had not given the values of R and V, it was not possible to calculate the values of f_1 and f_2. The speaker had therefore taken a number of hydraulic data collected by the Research Institute, from six distributaries taking off from the Jhang Branch near Kot Khudayar and from the Jhang Branch itself. Mr. Gunn was of the opinion that those channels were in regime before February, 1934. These data all referred to November, 1933. The three values of 'f' calculated from the same channels vary from 1.39 to 0.63 they did not show any approximate constancy. As the author had suggested, the value of 'f' had been calculated for the Jhang

Branch from $S = f_3^{\frac{1}{5}} / 1788\ Q^{\frac{3}{6}}$

and the corresponding slopes worked out for the distributaries. The existing slopes were in all cases, excepting one, flatter than the calculatedones. Yet all those channels were in regime.

For the last year and a half hydraulic data from regime channels had been collected by the Research Institute. When these data were analysed it might be possible to say if there was any regime in channels or not. Meanwhile the bed silts in those channels had been analysed by the Siltometer developed by Dr. Vaidhianathan of the Research Institute and a silt characteristic called the Schocklitsch number, K, had been worked out. Though this number fluctuated for different parts of the year, yet a statistical average could be worked out for each channel. The Schocklitsch numbers had been compared with Mr. Lacey's 'f_3' and did not appear to show any correlation.

Channel.	Jhang.	Cheniot.	Sarang-wala.	Gujiana.	Jamal-jatti.	Khai.	Nasrana.
K. ..	1.14	1.03	.70	.45	.45	.60	.70
f_3. ..	1.09	1.39	.87	.73	.82	.74	.63

The author referred to the work that was done some years back in the Research Institute. It was then thought that if Mr. Lacey's 'f' had any physical significance or bearing on the bed silt in canals and river, it should be possible to get some sort of correlation between his 'f' and the diameters of silt particle obtained from the Siltometer curves developed in the Institute. As a matter of fact Mr. Lacey had also worked out a relationship between the diameter of silt particles and his 'f' :—

$$d = \frac{f^2}{64}.$$

The speaker did not not know how Mr. Lacey arrived at this equation. The speaker failed to get any correlation between any of Lacey's 'f's and the corresponding diameters of bed silt.

More recently some work on the above line had been started in Institute on the hydraulic data collected from various sites for the last year and a half and it appeared that it might be possible to get a real characteristic of silt, not a derived characteristic like Mr. Lacey's 'f' and to

to correlate this with some other hydraulic constants of canals and rivers. The idea on which the speaker at that time was working had been inspired by some model works of Prof. Gibson at Manchester and Prof. Rehbock at Carlsruhe. The speaker said that as he got more data it might be possible to obtain a sound working relationship between the silt a channel carried on its bed and its other constants.

KOT KHUDAYAR SECTION.

November, 1933.

	Canals.	Discharge.	Velocity.	R.	$S \times 10^3$	K	f_3	f_2	f_1	Calculated $S \times 10^3$ with f_3 as standard.
		Cu/Sc .	ft./Sc.	ft.						
1.	Jhang Branch.	2149.71	2.70	6.26	.18	1.14	1.09	1.14	.88	..
2.	Chiniot.	174.05	2.11	2.26	.31	1.03	1.39	1.16	1.15	.27
3.	Sarang-wala.	60.18	1.50	1.70	.33	.70	.87	1.11	1.00	.33
4.	Gujiana..	26.95	1.30	1.30	.28	.45	.73	.91	.98	.37
5.	Jamal Jatti.	26.12	1.34	1.26	.34	.45	.82	1.02	1.08	.37
6.	Khai..	38.04	1.49	1.55	.27	.60	.74	.94	1.08	.35
7.	Nasrana	277.06	2.27	3.09	.15	.70	.63	.78	1.26	.25

$$S = f_3^{\frac{5}{3}} / 1788\ Q^{\frac{1}{3}}$$

$$S = 370.68 \times 10^{-6} \times f_2^{\frac{3}{2}} \times R^{-\frac{1}{2}}$$

$$V = 1.1512 \sqrt{f_1 R}$$

Mr. S. Majid Ullah said that the author had stated that Lacey's formulae had been accepted as the best available for the practical design of earthen channels and had given his verdict that if any channel was free to form itself it would do so according to Lacey's rules. The author, had, however, not given a single instance in support of his statement. Even for his investigation he had confined himself to only one of the many formulae of Lacey, viz., $S=f^{\frac{5}{3}}/1788\ Q^{\frac{1}{6}}$. Did the Author think that the other formulae of Lacey were less reliable and gave less accurate results ? Had he also investigated the value of ' f ' by the other formulae of Lacey and if so with what result ? Had he also observed the channel sections and found them obeying Lacey's Law.

In Fig. 2 the author had plotted Kennedy's V/V_o and Lacey's $\sqrt{f}$ and had shown that while Kennedy's V/V_o varied with the discharge. Lacey's ' f ' remained constant. The result was rather interesting as Lacey himself maintained that for velocities not exceeding 3 feet per second V/V_o was more or less equal to the square root of ' f ' and that he regarded this as a convenient link between the old classification of silt and his new silt factor.

The speaker said that it was not clear how the author had arrived at the values of the C.V.R. given in Fig. 2 of his paper. Had he calculated those from the observations made by him or his predecessor or had he merely taken those out of Kennedy's Diagrams ? If he had merely taken those out of Kennedy's Diagrams then his deductions were neither valuable nor helpful. A recent set of observations for 2R/10R is given below for comparison with the observations quoted by the author. It would be seen that $\sqrt{f}$ was not constant and varied appreciably, rising with the reduction of the discharge and thus followed more or less the C.V.R. curve shown by the author and not the horizontal line as given by him for $\sqrt{f}$.

Reach.	0 to 14500	20 to 29000.	41 to 49000	50 to 54000	60 to 67000	72 to 80000	81 to 87000	98 to 100000
Q ..	137	92	77	57	49	33	31	17
S per 1000 as observed now.	.135	.16	.193	.20	.29	.204	.24	.28
Slopes as given by Mr. Blench.	.125	.18	.19	.196	.212	.18	.20	3
Value of $\sqrt{f}$ taken from regime slope diagram of Mr. Lacey.	.84	.85	.89	.9	1.0	.89	.92	.94
Value of $\sqrt{f}$ as given by Mr. Blench.	.83	.91	.91	91	.92	.86	.85	.85

As regards the conclusions drawn by the author, the speaker said, that they were based, in his opinion, on data which was too meagre and insufficient to be convincing. The first conclusion that 'f' was a constant and did not vary with the discharge was not supported by the observations quoted above.

The speaker said that he would like to ask the author as to how he had arrived at his second conclusion. Were not some of the minor heads he had mentioned fitted with silt excluding devices.

Mr. N. D. Gulhati stated that if a channel was free to form itself, it would do so accordingly to the Lacey's rules. That was a hypothesis the correctness of which need not be questioned, for it could never be put to test.

The author stated that the formula $S=f^{5/3}/1788\ Q^{1/6}$ was mathematically the soundest, because the slope automatically averaged out the effect of all the varied sections in the reach over which S was measured. But actually the slope averages out many factors besides the change in section.

In general, with the silt content remaining the same, if the equilibrium of a channel was disturbed by any cause, the first thing that was affected was the slope. Silting or scouring of the bed followed as a result of the change. Slope was thus the least steady factor in a channel. It was not a measure only of the 'f' of a channel but it was governed by the total resistance offered to flow, which included many other factors besides the silt content. The slope could thus be different even if Q and 'f' remained constant. The slope formula could not therefore be adopted for determination of 'f'.

The slope was never constant over long lengths. If it was measured over short lengths, the difference in levels was so small, that the observational errors were high. Unless the water levels were observed at a large number of points say 100 feet apart, and the slope determined by drawing a mean line between those levels plotted on an exaggerated scale, no reliance could be placed on the value of slope.

Again the slope formula advocated by the author was not a general relationship between Q, *f* and S. It was a formula, which according to Mr. Lacey himself, gave the regime slope in the limited conditions of those channels which had their bed width and depth determined by the Lacey formula, and as such, it could not be used for channels not complying with that condition.

The only general formula by which 'f' could be determined was Lacey's fundamental formula $V=K\sqrt{fR}$.

The author stated that regime could be tested by working out 'f' by the slope formula for several channels of an area and noting those

whose 'f's were constant. That was not correct. Two channels having different values of 'f' might have the appropriate slope and sections and be thus still in regime.

Mr. Kanwar Sain said that he was afraid that he could not agree with all the conclusions drawn by the author. He agreed that 'f' was a true index for comparison of silt content, but the exact physical significance of 'f' had yet to be discovered. It must be definitely related to Kutters' N.

$$N_a = .0225\, f^{\frac{1}{4}}$$

and it was from that relation that the Lacey Regime Slope formula had been derived. The author therefore could not say that N varied with discharge, while 'f' did not.

N was believed to depend on the nature of the surface. In the Punjab canals and distributaries it was found to vary from 0.018 to 0.025. From the relation given by Lacey for that range of N, '*f*' would vary from 0.41 to 1.51.

The speaker believed $\frac{V}{V_o}$ could be left out of consideration, if the true value of N or *f* could be found.

In his conclusions 2 and 3, the author did not appear to be on firm ground. If the statement made by the author "that it seems certain that quantity of silt must have some effect on the value of 'f' was a fact, then any off-takes which distributed silt differently must cause variations in the values of 'f' for those channels.

On the Jhang Branch of the Lower Chenab Canal, the speaker had some accurate observations made in stable reaches, the value of 'f' from the slope formula was 0.91 to 1.1. On two small distributaries fitted with A. P. M. off-takes, the value of 'f' from the slope formula came to 1.3 and he remodelled those distributaries assuming the same 'f'.

Mr. G. R. Sawhney said that he would like the author to realise that whether they follow Lacey or Kennedy in designing their channels, the regime of those channels would also always be affected by unavoidable winter closures.

The speaker was afraid that the author could not very well get out of doing the unpalatable artificial maintenance unless he spent the rest of his career looking only after rivers and weirs.

Mr. K. R. Erry pointed out that Lacey had developed four formulae from which 'f' could be calculated. The values of 'f' derived from these formulae were all different. He said it was difficult to decide which value of 'f' to adopt.

Mr. Ishar Dass said that it was for the first time after Lacey advanced his theory regarding laws governing stable silt transporting channels that any of his formulae had come for discussion before this Congress. Hydraulic engineering owes a debt of gratitude for the great advance made by Lacey in their conception of science of hydraulics.

The speaker although accepting all the Lacey's Formulae as substantially correct differs from the view such as expressed by the author of the paper that " a single ' f ' obtained is applicable to all channels." Studying Table I of the paper under discussion, the speaker remarked that it will be found that values of $\sqrt{f}$ on one channel alone range from .83 to 1.02. How could, then, there be one value of ' f ' for the whole system. The speaker also did not agree with the other conclusions arrived at by the author.

The determining factor for the value of ' f ' was to be found in the section of the channel which took a suitable slope to fit in with the shape of the section and discharge.

The speaker thought that stable channels for any discharge can be obtained with all slopes. It is the section that matters. For this Lacey's wetted perimeter formula $P_w = 2 \cdot 67\sqrt{Q}$ is the best guide. When the curve for this is plotted on Kennedy's hydraulic diagrams, it shows the dimensional design of channels for any discharge and any slope.

Mr. Radha Krishna Khanna said that in the opening words of his paper Mr. Blench has remarked that the Central Board of Irrigation Research Committee in July 1934 unanimously accepted the Lacey Formulae as the best available for practical design. It is not quite clear from this statement whether all or some selected formulae of Mr. Lacey have received the unanimous approval of the Research Committee ; but if the slope formula should be on the approved list, then much more convincing arguments must have been adduced in support of the formula before the Research Committee than Mr. Blench has done in this paper.

Mr. Blench shows his own diffidence about the slope formula when he says that some discussion is necessary because any channel may depart considerably from the average laws deduced by Lacey ; whence he concludes that a very large amount of data is required to satisfy any independent enquirer of the truth of the laws.

Mr. Blench admits that in his two years' search after the truth about Lacey's laws he has failed to collect enough data to prove the Lacey laws independently, and yet he believes and asks others to believe that all the channels could be explained in terms of those laws. He congratulated Mr. Blench on the immensity of his belief, which is nothing short of the order of religious beliefs, in the Lacey laws, and sincerely wish that in course of time he will collect enough data for proving those laws independently

Until such a happy moment arrives, however, it will not perhaps be amiss to remark that the ordinary conception of any law is that it will satisfy the generality of conditions covered by it and that some few exceptions, if any, should be clearly explainable. In this case the entire paper of Mr. Blench enumerates the channels where for one reason or the other the Lacey laws do not apply ; and yet at intervals of a few short paragraphs, he enjoins the readers to put their implicit faith in those laws. Yet the successful application of the Lacey slope and other formulae, and for that matter, similar empirical formulae proposed by Mr. Khanger, Mr. Sharma and others, a great deal of imagination has to be exercised in assigning meanings and values to *f*, N and such other abstract terms. Mr. Lacey's '*f*' in the velocity formula functions in a diametrically opposite manner to that in the slope formula. For whereas in the velocity formula increase of 'f ' tends to increase of mean velocity of flow, in the slope formula it tends to increase of frictional resistance to flow. What the real implication of ' f ' is, and what its approximate values in any particular cases are likely to be, no one knows except perhaps the author of the formulae, or the author of the present paper, who tells us that the regime can be tested for by working out ' f ' by the slope formula for several channels of an area, and noting those whose ' f 's agree.

If Mr. Blench is determined to stick to his position that the truth of Lacey laws must not be questioned, then there is no question of argument. But if he is open to discussion then any amount of data could be produced to show that all the Lacey laws and formulae are arbitrary and incorrect.

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CORRESPONDENCE.

Mr. Gerald Lacey remarked that he very much regretted that he had not been able to take part in the discussion at Lahore on Mr. Blench's paper. Mr. Blench was to be congratulated on an exceedingly practical aper which demonstrated the utility of the silt factor as a basis of design. The silt factor, in the form $f^{\frac{1}{2}}$, replaced the Kennedy critical velocity ratio, and Mr. Blench's method of deriving it, from the writer's slope formula, was undeniably the best.

One advantage of measuring the slope as Mr. Blench had pointed out was that it automatically averaged all the varied sections in the reach over which the slope was measured. Another advantage was that the slope so measured took into account the slight element of shock which differentiated a working slope from a regime slope, and thus produced a working silt factor of great value.

In addition to the negligible error in measuring Q there was also the slight error due to assuming that the wetted perimeter varied as $2.67\ Q^{\frac{1}{2}}$ whereas in practice this was an average result and in extreme cases the constant might be studied by examining the slope formula afresh. Supplying the constants from which the equation was derived the correct expression was

$$S = \frac{(2.67 \times 1.17)^{\frac{1}{3}} \times .0003826\ f^{\frac{5}{3}}}{Q^{\frac{1}{6}}}$$

The variation in the wetted perimeter constant was equivalent to a variation in the calculated regime slope of five per cent. in a very extreme case and therefore negligible.

Mr. Blench had pointed put a very common error when he drew attention to the futility of adopting an existing slope with a reduced discharge.

A study of Kennedy's diagrams showed that for a channel with a given slope and given bed width the critical velocity ratio as determined from Kennedy's equation was sensibly constant over a fairly wide range. Such as a fact was not the case. The best proof was the manner in which escapes, designed for large discharges, silted with low discharges, despite the fact that they were provided with notched falls.

Experience on the Sarda Canal had shown that if small distributary channels barely in regime were silting slightly, running them for two or three weeks at a reduced discharge put them completely out of action as they silted up solid with the berms. Once the fact that regime slope varied inversely as $Q^{\frac{1}{6}}$ was appreciated the phenomenon was at once understood. As the discharge diminished the regime slope increased with rapidly cumulative effect.

Mr. Blench's remarks on the regime slope being set up in filling reaches was of great interest. Errors in slope in large channels were not usually detected owing to the slopes being so small and falls fairly close to one another. When there was a long reach without a fall the error was shown up.

The Kheri Branch of the Sarda Canal had been given a slope of 0.64 feet per mile, approximately 0.12 per thousand in a reach 17 miles long, the discharge being about 800 cusecs. The result had been that below the regulator at the head of the reach the water surface had dropped by no less than two feet. The actual slope was a little less than 0.10 per thousand. The silt factor on the Sarda Canal appeared to lie between 0.6 and 0.7 and the conditions were therefore very similar to those on the Lower Bari Doab Canal. The country slopes in the lower part were so flat that many of the smaller channels were silting heavily. Fortunately some of these channels were provided with falls to " preserve " command, and as it was now realised that it was better to " preserve " the discharge such falls were being lowered or demolished.

Mr. Blench, in reply to comments and correspondence, expressed his satisfaction that the discussion gave him the opportunity to remove some popular misapprehensions, and dealt mainly with Dr. Bose's remarks, as these were definite and mathematical, and covered most of the others.

Firstly, however, he had to point out that Mr. Lacey never held out his $d=f^2/64$ formula as anything but a " very rough qualitative formula for the diam. in inches of the predominant type of silt transported," to quote his own words in his I. C. E. Paper No. 4736. Obviously he gave this formula as a suggested line of advance from his theory of Stable Channels in Alluvium, to the problems of how to link up river conditions (which are not stable) with canal behaviour. The engineer on running canals need take no notice of the formula at all, as he can find f from ordinary survey data by the accepted Lacey formulae. The Research Department are naturally interested in the formula as (*i*) it is the only one they can check from first-hand laboratory data and (*ii*) the information might be very useful for a new canal project.

Secondly, he found no difficulty in following Mr. Lacey's mathematics. Mr. Bose has not found a flaw in the mathematics, but tries to condemn the results because f has different dimensions from different formulae. This arises from the fact that the constants are dimensional, and is quite normal to any student of physics. As a parallel case one may take that of electricity, which suffers from the same defect, that no one knows what it is ; but the science has been built up from observation of its effects. Consequently, one finds that the capacity of a condenser has the dimensions of length. More advanced study shows that the dielectric constant has to be given dimensions to explain the wave theory of the various kinds of electromagnetic waves. Similarly, no one knows exactly what

" f " is, except that it is obviously a physical quantity that defines regime channel conditions, and the formulae Lacey got by experiment merely define it in terms of functions that we can measure. There is no more need for us to know the true nature of f, than there was for Lord Kelvin to know the true nature of the dielectric constant when he worked out the theory of electromagnetic wave propagation.

To give a more homely illustration, Mr. Bose's argument could be turned to condemn the Kennedy Formula because it gives velocity as having the dimensions $L^{0.64}$. The fallacy lies, once more, in the fact that constants in experimentally derived formulae are often dimensional.

Thirdly, there is the vexed question why certain Lacey formulae, particularly the fundamental one, are not suited for finding f. This can be answered very easily in terms of the Theory of Error. If the numerator and denominator of the quotient of two observed quantities are each subject to the same probable error, the quotient has a p.e. of $\sqrt{2}$ times that of either. Now, the fundamental formula gives $\sqrt{f}$ proportional to Q/A (neglecting the effect of R, which the paper has shown to be small). But Q, as every canal officer knows, is none too accurate except at a few selected sites; and even then, it must be remembered that current meter discharges may have a constant error of about 5% from velocity rod ones, owing to the meters being rated in a very narrow tank. (Official confirmation of this will be found in the Egyptian Ministry of Public Works Physical Department Paper No. 14, by by P. Philips D. Sc.; but the 5% figure is based on the speaker's observations). If we take 7.5% p. e. for Q, and the same for A (*vide* Fig. 1) this means a p.e. of 10.5% in $\sqrt{f}$, and 21% in f. So it would require 441 observations averaged to determine f correct to 1%. And this takes no account of the fact that some of the observations may be marred by the sections being subject to disturbing factors—or perhaps not being regime ones at all. It is quite obvious that no one but a Research Officer, with an enormous amount of data could find f from the fundamental formula.

But given f from a more suitable formula, the fundamental one is suited for finding the ideal sections; and it will be found that these agree with site conditions, or the site conditions can be made to agree easily, and remain. The non-mathematieal members will appreciate the point if they consider the well known Fane's curves for coefficient against Drowning ratio for Flumes. In the flat range of the curve the ratio can be found with great accuracy if the coefficient is given; but if the Ratio is given the coefficient is very rough.

Fourthly, the necessity of data being collected by an experienced officer is well illustrated in Mr. Majid's comments. He chose to quote the writer's own 2R against him. Now the writer took his data pre-remodelling. Mr. Majid analysed the channel after the discharge was reduced 20% (about), the pre-remodelling slopes retained almost

throughout (a non-permanent condition), the head regulator radically altered, and the heads of the two excessively silt drawing minors mentioned in the text radically altered to cure that defect—and this in spite of the fact that he has been X. E. N. in charge of the channel for two years. Nothing but hopeless inconsistency can be expected from such an analysis. Similarly the speaker is well aware that one of Mr. Malhotra's channels, at least, is of several hundred cusecs without a single minor offtake for miles. The direct outlets have to be fitted so high above bed level that they had no hope of ever taking their fair share of silt (quantity or quality); and such a channel is also unsuited for analysis. A careful reading of the Paper should have prevented such errors.

It seems worth mentioning specially, however, that a permanent channel is not necessarily a regime one. Channels in cutting in hard soil may never scour down to regime slopes. A regime channel is one that has *formed itself in its own silt*, and then remained permanent. Probably not more than half the total length of Punjab channels are regime ones.

It is one of the strong points of Lacey's data that it is largely from men who had rich experience, and were interested neither in proving nor disproving his results. They were after what they considered to be regime channels in the exact sense.

Leaving Dr. Bose, there is little to answer except Mr. Majid's comments on the data. The data was submitted for publication ; but was too bulky. Mr. Majid was shown it when he was the speaker's X. E.N. ; but did not show any interest. The S. E. of the time, however, inspected 2R with the data with him, and not only found no fault with it but was able to estimate what the slopes would be, from his experience of other channels—a first class vindication of the formulae. The data is still available. It shows f found by three ways ; and v/v_o by two. v/v_o from Kennedy's diagrams is not consistent with that from Kennedy's formula because it is marred by the not-too-trustworthy Kutter's formula ; and the assumption of trapezoidal sections. The pure Kennedy gives more reasonable results as may be expected from the fact that its data is embodied in Lacey's.

The speaker was glad that Mr. Lacey's comments show that he has not misrepresented him in any way ; and thanked Mr. Kanwar Sain for his support.