

### Discussion.

The Author introduced the Paper with the aid of a series of lantern slides. In 1938 he had been asked by the Central Board of Irrigation of the Government of India to summarize and analyse the data which had been received in reply to a questionnaire on the subject of meandering. An extraordinary diversity of opinion was evident among those who had sent in replies to the questionnaire, and the answers more or less cancelled each other out. The only point upon which there was fairly general agreement was that meanders were due to differences in the bank material and that a rock or other projection started a meander: but actually that was not correct; a meander was a natural physical occurrence which was not imposed by external conditions.

The replies to the questionnaire also revealed a wide diversity of opinion in regard to the meaning of various terms, and therefore the definitions given in the Paper had been prepared. There were three main types of meander, but the meander length bore the same relation to the discharge in all three, varying as the square root of the discharge. That was fortunate, because it was meander length which determined the position of groynes in river training:

Mr. W. L. C. Trench observed that the Paper was clearly the result of close study of the data and judgement in their application. Even in Great Britain some rivers exhibited extraordinary meanders, and although, as a rule, they might be described as incised rivers (flowing generally below the level of the surrounding country) sometimes, as in the case of the Ouse in Bedfordshire and Cambridgeshire, they flowed above the ground in times of flood, and the beds were extremely tortuous. Any basic theory of meander should also be applicable to those rivers.

He had been interested to see the application of the term "meander" to rivers, but with regard to "tortuosity" he suggested that, instead the phrase "meander length ratio," might be used. "Meander ratio" would then become "meander breadth ratio". The meander length ratio would be the ratio of the length of the river-bed to axial length, which would be the same as the tangential length between successive cusps of curvature.

The term "dominant discharge" had not been strictly defined. It meant apparently that discharge below which silt was deposited and above which scour occurred. That could be inferred from the Author's statements:

"This means that for any discharge below the dominant discharge the slope will be in defect for the dominant charge, so that material will deposit" (p. 7, *ante*). "But when the discharge exceeds the dominant discharge, the energy available exceeds that required to carry the dominant charge, so that excess

energy is utilized in increasing the charge by scouring the bed" (p. 7, *ante*).

"Where the banks are straight, and are tough enough to resist erosion, the excess energy which develops during floods is absorbed in scouring the bed" (p. 7, *ante*).

There Mr. Trench felt bound to join issue with the Author, because his experience was that the silting of a bed actually occurred in times of high flood, and not in times of low flood.

He could give three instances of that. One was from the ordinary discharge graph of the Indus at Kotri.

If that were plotted to a logarithmic scale of the discharge against the natural scale of gauge readings it would be found that the rising flood consisted of two nearly straight lines with a change of inclination at about 14 feet where the flood tended to cover the sandbanks in the bed and permanent banks, and that was what might be expected. The upper part of the graph, however, was steeper than the lower, and an inspection of individual successive discharges and gauge readings showed clearly that during the higher flood period there was a tendency for the gauge reading to rise more rapidly than could be accounted for solely by an increase of discharge and often without increase of discharge, thus indicating a rise in bed-level. That was confirmed by the fact that during the subsequent falling flood the gauge readings for the same discharges were higher—sometimes as much as 2 feet higher—than during the rising flood.

From October to May during the low-water period there was normally a reduction of level for the same discharge, indicating a slow scour, so that when the rise again began there was little change; but any balance of rise or fall of regime levels depended on whether a previous high flood had produced excessive silting or a low flood had produced little.

The second indication occurred between 1905 and 1912, during which period the rainfall in the Punjab and over north India generally was defective or uncertain. Taking the Kotri site, in Lower Sind, and plotting the specific discharge graph—that was, the gauge reading for a selected discharge through a number of years—a slow rise was observed prior to about 1905. That was followed by a fall and subsequent rise. The kink in the curve was due to years of very low floods, when there was less deposit of silt. The next discharge site up stream was at Sukkur, where the effect was masked because of a gorge, but a similar kink was found at a very large number of the Punjab gauge-reading sites. All tended to show that the effect of a series of low discharges was not to increase the silting but, in fact, to cause scour.

The third instance referred to canals which were tending to silt. In the Rice Canal at Sukkur Mr. Trench had succeeded in stopping silting, at all events temporarily, and inducing a very slow scour by reducing the gauge reading at the head. He did not know what the Author would call the dominant discharge of a canal, but presumably it was the discharge at

which the canal was designed to run. With other canals also he had known a tendency to scour to be developed by reducing the full-supply gauge reading by about 10 per cent.—a few inches on a small canal, and a foot or so on a big canal. He suggested that the effect on the Lacey number,  $f=V^2/d$ , might be that small reductions of  $d$  would increase the value of  $f$  rather than reduce it.

The Author had mentioned primary and secondary meander. Mr. Trench believed that the cause of eating into the banks in the flood season in a big river and the cause of the type of meanders which the Author had produced artificially were not the same, but were possibly even due to diametrically opposite causes. The cutting into the bank which occurred on a river such as the Indus in flood was almost like an hydraulic jet action. On one occasion he had been standing on a bund in the Karachi Canals area at a place called Belo, when that bund was under attack by what was called in Sind the "Main Stream" (which could be a very limited section of the river itself). It was eating into the bank as though it were an hydraulic jet; and then suddenly, for no apparent reason, the whole of the main current shifted about  $\frac{1}{2}$  mile out into the middle of the river, and the attack on that particular place ceased for the time. The embayment caused by that type of attack, which occurred only on rising floods, led to excessive meanders and "cut offs". The embayments were frequently deepened by reverse flow, especially if there was a patch of hard silt at the down-stream nose.

He had observed a perfect example of the other type of meander referred to in the Paper in about 1911, on the Jamrao Canal, Sind. For the first seven miles the bed of that canal was pure sand, whilst the sides were also sandy. From 1905 to 1911 very little water had been coming down in the cold weather, for the reasons to which he had already referred. The canal-bed was about 164 to 200 feet wide, and the depth of water was supposed to be about 8 feet. With the reduced discharge the depth became less than 2 feet, and immediately the meanders started, forming a perfect set of curves all the way down from the canal head to the seventh mile; they were most intense at the fourth mile, where the slope began to flatten, that corresponded with the experiments carried out by the Author at Poona, where meander was produced by having a steeper slope at the top and a flatter slope below.

The reason for that appeared to be as follows:—If a canal were 160–200 feet wide and the water-level fell so that the depth was, say, 1.5 foot, the slope to which the canal had been graded would be insufficient to provide the necessary energy in relation to the excessive wetted perimeter. The stream would then tend to concentrate in a channel of, say, 50 feet by 4 feet, thus increasing  $R$  and reducing the W.P. In doing that it swung from side to side. Thus it was lack of available energy, and not excess energy, which caused the meander.

In the case mentioned it was seriously considered whether large-scale

clearance work should not be undertaken. Fortunately, the river rose and when sufficient water was admitted to the canal the meanders disappeared.

The Paper indicated again the value of research of which so much was now heard and it was creditable to the Provincial governments of Bombay and the Punjab that they were willing to provide money for the work in the early stages when its practical application had still to be proved. Now, when the correct placing of a single groyne might easily save the whole cost of all the work done, the practical importance of the work could not be doubted.

Mr. F. J. Salberg observed that of the rivers mentioned in the Paper two were of a very different nature. The Kosi came down from the Himalayas and from a hill gorge discharged on to the Ganges plain. For that reason the river's plain, built in the past, had assumed the form of a portion of a cone, the discharge from the incised gorge being at the top of the cone. Any particular line of the Kosi's discharge, as soon as it had been built up a little above the rest of the surface of the cone, enabled the river, when conditions were suitable, suddenly to change its course to some other part of that cone. *Fig. 10* showed clearly the numerous channels which had built up the cone, any one of which at any moment might again become the main river. The Rupnarain, on the other hand, was a channel through the tidal plain of the Gangetic delta, and that river's plain was limited by the plains of other similar channels. Both rivers were liable to wide variation in discharge and silt load, but for different causes.

He considered that the Author had not sufficiently emphasized the fact that a river channel should not be dissociated from its plain, for the plain which the river and its silt had made was as much a part of the whole river system as were the water discharge and the silt load. In incised rivers that factor was not of much practical importance, but nearly all rivers which posed the problems under discussion were liable to large variations in flow and in silt load and hence all were meandering. The meander was the river's main mechanism for building up its plain as broad and as wide as circumscribing conditions permitted, and it was also an important part of the mechanism for "trading" the whole river system downstream. Taking a long view, therefore, it was necessary to regard the river and its plain as a progressive entity. The plain had been built by past floods and the meander and the channel by its integration of the flows, so that gradually but surely the whole river system of channel, slope, and plain was moving downstream.

When an engineer was required to construct a permanent work on such a river, he was rightly permitted to treat the section as he found it, as being in a state of stability. It should not be forgotten, however, that that was not the whole truth; the whole river system would be moving downstream past the work in question at a rate of possibly  $\frac{1}{2}$  mile in one hundred years, and in some cases possibly at an even higher rate. Therefore Mr. Salberg

was apprehensive of a certain limitation in the Author's bridge training solutions based on model experiments. It was generally known from experience that a spur could be placed so as to produce the required result with the channel as it was at the time ; but would that spur be equally satisfactory in fifty or one hundred years' time, when the channel and the plain had all moved downstream with regard to the bridge?

A natural representation on a grand scale of the principles of Mr. J. R. Bell's "bund" system—namely, impregnable guide banks and impregnable approaches—existed at Gauhati, on the Brahmaputra, where the river ran through a granite hill gorge. There it was possible to see how in past times the river had swung from side to side for many miles upstream, and how it had approached the gorge from every possible angle.

At a site where there were no fixed points it was necessary to consider not only the bridge itself but also the approach banks leading up to it, and Mr. Salberg considered that the Author's proposal for a spur such as that constructed for the Rupnarain river would not be able to meet the conditions which would occur in the distant future when the meander had moved downstream. For instance, in the case of the Rupnarain, was it not possible that when the left bank embayment had gradually worked out eastward and downstream, the approach bank leading to the bridge would be threatened?

His own experience had been gained in Eastern Bengal and Assam, while working on the Assam-Bengal Railway. There the practice was to provide some stone pitching protection when it became necessary, but it was always set out in such a manner as to permit future development into a continuous "Bell" guide-bank. On the other hand, the neighbouring railway, the Eastern Bengal State Railway, normally provided its bridges with a complete Bell bund system when they were being built. On the Assam-Bengal Railway, any piece of protection work done was recorded in a "Hidden Works Register", which meant that the work had to be inspected and reported on once a year. That was necessary (because usually a successful piece of protection soon became silted up and buried) to ensure that in the future no fresh work might be undertaken without knowledge of the existence of the old work ; or, on the other hand, further work might be required to maintain or improve the efficiency and scope of the old work, and should be carried out in continuation of the work already done.

There could be no question of which method was the cheaper, but it would be invidious for him to give an opinion on which system gave the greater safety. The point at issue was that both methods allowed for the movement of the whole river system downstream. In the period to which he referred there was never any question of the use of spurs. At that time such a proposal would have been entirely unacceptable, and he still found it difficult to visualize how such spurs could be worked into a final permanent protection scheme.

The penultimate paragraph of the Paper appeared to foresee certain difficulties. Those might be the difficulties to which he had drawn attention ; but, whether they were or not, he felt sure that the Author had some means of meeting them, and a statement of his ideas and proposals would be of great value.

Professor C. M. White observed that in river-mechanics most of the theories lacked satisfactory foundation. The Author had suggested the principle of minimum dissipation as a general basis, but that seemed too far from the facts ; it seemed to imply smooth and straight channels as the norm, whereas those freely formed in Nature were tortuous without exception. Actually, all courses from the hills to the sea descended through precisely equal heights, and involved equal total dissipation ; therefore arguments based on least dissipation could hardly be expected to predict which course the river would ultimately follow.

The theory of turbulence offered a more promising basis. Already it yielded quantitative predictions for the transport of solids which were valid in many rivers, although as yet it had not explained bed-forms nor width/depth ratios. In the meantime reliance had to be placed mainly on field records, brought together and recorded as empirical formulae.

There were many such formulae, but, regarded as river formulae, the majority were basically unsound in what they attempted. Usually they expressed one local river-dimension, such as speed, in terms of one or more other local river-dimensions, such as depth or slope, rather than in terms of some fundamental causative agent such as run off. However, the Author had quoted three formulae of rather a different type, due to Mr. Gerald Lacey, M.I.C.E. The second and third contained a coefficient,  $f$ , for which several conflicting definitions had been published. The Author had defined  $f$  as  $V^2/d$ , in which case it involved two local river-dimensions, and those two formulae then became difficult to discuss.

However, the first, the breadth formula,  $b = 2.7 Q^{\frac{1}{2}}$ , was excellent in conception. The boldness of its simplicity compelled admiration, though, as the Author had pointed out, there was a certain inconsistency in its dimensions, which could be remedied in a particular way. Professor White thought that that and similar formulae could be included in a more general treatment based on the idea that any river-dimension, such as the breadth  $b$ , depended primarily upon the magnitude of the flow  $Q$ , moving under the action of gravity  $g$ . In building a formula, he thought it desirable first to get the chief variables correct dimensionally, and that could be done only by putting  $\frac{2}{5}$  against  $Q$ , and  $\frac{1}{5}$  against  $g$ , giving

$$b = \alpha Q^{\frac{2}{5}}/g^{\frac{1}{5}}$$

where  $\alpha$  was a variable coefficient depending firstly upon the magnitude of the flow in relation to the size  $k$  of the solid particles, and secondly upon the size of those particles in relation to the molecular structure of the water, and the latter was represented by the kinematic viscosity,  $\nu$ . It followed

that  $\alpha$  depended firstly upon  $Q/k^{2\frac{1}{2}}g^{\frac{1}{2}}$  and secondly upon  $k/\nu^{\frac{1}{2}}(e^{\frac{1}{2}}g)^{\frac{1}{2}}$ . Such relations should be regarded as interpolation formulae, for which equations of the type  $\alpha = \alpha_1 x^{n_1} \cdot y^{n_2}$  were usually sufficiently accurate, provided suitable values were assigned to  $n_1$  and  $n_2$ . The breadth formula then became

$$\frac{b}{Q^{\frac{1}{2}}/g^{\frac{1}{2}}} = \alpha_1 \left( \frac{Q}{k^{2\frac{1}{2}}g^{\frac{1}{2}}} \right)^{n_1} \left( \frac{k}{\nu^{\frac{1}{2}}(e^{\frac{1}{2}}g)^{\frac{1}{2}}} \right)^{n_2}$$

which was complete, or nearly so, except for the omission of the quantity of solids in relation to the quantity of water. With regard to  $n_1$  and  $n_2$ , available data showed that  $b$  was nearly proportional to  $Q^{\frac{1}{2}}$ , therefore  $n_1$  would approximate closely to 0.1. That brought in the particle, but Lacey had stated that particle size did not affect the breadth formula; if that were true, then  $n_2$  would, of necessity, be 0.25, and the complete interpolation formula could be written as

$$\frac{b}{Q^{\frac{1}{2}}/g^{\frac{1}{2}}} = \alpha_1 \left( \frac{Q}{k^{2\frac{1}{2}}g^{\frac{1}{2}}} \right)^{0.1} \left( \frac{k}{\nu^{\frac{1}{2}}(e^{\frac{1}{2}}g)^{\frac{1}{2}}} \right)^{0.25} \left( \frac{G}{Q} \right)^{n_3}$$

where  $G$  denoted the quantity of solids conveyed, and  $n_3$  was probably small.

Professor White considered that most river-dimensions could be expressed by similar formulae; for example, depth  $d$ , area  $a$ , slope  $s$ , and meander wave-length  $\lambda_M$ , yielded the following:

$$\begin{aligned} \frac{d}{Q^{\frac{1}{2}}/g^{\frac{1}{2}}} &= \alpha_2 \left( \frac{Q}{k^{2\frac{1}{2}}g^{\frac{1}{2}}} \right)^{n_4} \left( \frac{k}{\nu^{\frac{1}{2}}(e^{\frac{1}{2}}g)^{\frac{1}{2}}} \right)^{n_5} \left( \frac{G}{Q} \right)^{n_6} \\ A \frac{Q}{Q^{\frac{1}{2}}/g^{\frac{1}{2}}} &= \alpha_3 \left( \frac{Q}{k^{2\frac{1}{2}}g^{\frac{1}{2}}} \right)^{n_7} \dots \dots \dots \text{etc.} \\ s &= \alpha_4 \quad \text{''} \quad \text{''} \quad \text{''} \\ \frac{\lambda_M}{Q^{\frac{1}{2}}/g^{\frac{1}{2}}} &= \alpha_5 \quad \text{''} \quad \text{''} \quad \text{''} \end{aligned}$$

Those expressions, which looked rather formidable, readily simplified into practical forms. When testing a proposed formula, it was a necessary precaution firstly to regard it in its complete form and to test its parts, before simplifying it for use in design.

The meander length for the Orissa rivers was summarized by the Author in the formula  $\lambda_M = 27Q^{\frac{1}{2}}$ . From that and the breadth formula it followed that the ratio  $\lambda_M/b$  would approximate to 10, which was curious in comparison with some other values which Professor White had analysed. When  $\lambda_M/b$  was plotted against the frequency of the number of observations, he found no rivers with  $\lambda_M$  less than 3.8 times their breadth and none with  $\lambda_M$  more than 37  $b$ . All rivers fell within those extremes, but the variation

of  $\lambda_M/b$  did not follow an ordinary probability curve; instead it had a double peak with two strongly preferred values of  $\lambda_M/b$ . The most frequent value was 15.5, and the other nearly as frequent value was 7.0. The value 10 corresponding to the Orissa rivers in the Paper was not exactly missing, but was found only one-quarter as frequently. If many values, some around 15 and others around 7, were averaged, then the result became 10, but he did not believe that was the explanation.

If consideration were given to the question of how meander originated, it would be seen that the mechanism did not permit of two possible solutions. Many models tested at the Imperial College before the war proved that when water and solids were fed perfectly steadily and symmetrically to a straight channel, the bed-load eventually congregated into long undulations or mounds of saw-toothed type, sometimes visible only with oblique illumination. Each mound was unstable when on the centre line of the channel, and went over to one side, which automatically caused the next to go to the other side, and so on. A very regular pattern of staggered shoals eventually developed, and the bank of the river tended to erode slightly opposite each mound. The resulting additional detritus was deposited on the next mound downstream, that was, *on the same side*, and once that deposition began the whole system rapidly went into meander with a regular and predetermined wave-length. The wave-length changed only slightly as the process developed, and there was no fundamental change in action, except that the cross flows referred to in the Paper could now begin, though they were not easy to detect in a small model. In the primary stage there was no crossing of material or water whatsoever, only the formation of the shoals; in the secondary stage the banks were cut, and the detritus from the cuts was deposited on the same side; and it was not until the final or tertiary stage that true crossing of material and the diving flow referred to by the Author could occur. At the Imperial College the whole sequence occurred under ideally steady conditions, and did not arise from fluctuations in feed, as in the Author's experiments, though the primary stage could be greatly accelerated by superimposing fluctuations of suitable periodicity.

The average meander wave-length,  $\lambda_M = 27Q^{1/2}$ , given by the Author for the Orissa rivers was valid, at least approximately, for a remarkably wide range of flows. The models at the Imperial College were tested with flows of as little as 0.05 cusec, yet the results agreed in substance with the Author's formula, which was therefore true for a range in which the largest flow was more than 10 times the least. Nevertheless something might also be learned by comparing the data in the table on page 33.

From that comparison Professor White surmised that particle size influenced the wave-length to some extent. He thought that in Nature certain sizes of sand and gravel were more abundant than others, and that might repay study in connexion with preferred meander values. However, the major factor determining  $\lambda_M$  was clearly the flow, and the Author's

Rivers in order of magnitude.	Bed particle- size : millimeters.	$\frac{\lambda Mg^2}{Q^3}$
{ Mississippi Mississippi Bengal rivers Orissa rivers	0.2	260
	0.5 some patches of large gravel	160
	not stated	280
	not stated	220
Rhine { Orissa rivers Bengal rivers Inn	100	160
	not stated	170
	not stated	200
	55	210
Poona models S. Kensington models	0.2	130
	0.7	90

formula gave that part of the relation, and did so for an extremely wide range of diverse conditions.

Mr. J. T. Evans observed that the Author had dealt principally with two classes of rivers in India which he called "rivers on flood plains" and "incised rivers." It would be helpful to have more fully explained the relation between those two classes because most English rivers seemed to fall between the two. They flowed for part of their course through flood plains of alluvium which they had themselves laid down in times past and it was unusual for a year to pass without their overflowing. On the other hand, their cross-sections could readily be described by the word "incised"; they carried less silt than the Indian rivers and changed their course more slowly, having reached positions of virtual equilibrium. Their meander lengths corresponded to those of the Indian rivers, but for fully developed meanders the belt widths were narrower, approximating with some uniformity to three-quarters of the meander length.

The Author had stated that in Nature every condition existed between the completely unstable flood plain rivers and incised rivers in virtual equilibrium. Was it possible to correlate those rivers to show a gradual change from one type to the other? If so, was it possible to select from the many factors associated with meandering some more purely causal factor and perhaps a more fruitful one than the degree of incision?

In England the trouble with rivers was not so much that they changed their course as that proposals were continually being made to interfere with them by bridges, wharves, gravel dredgings, and other alterations, and it was not always easy to be sure whether the proposals would be beneficial, of little consequence, or so harmful as to require opposition. Mr. Evans suggested that the "simplifying concept" of the dominant discharge might be used in a modified form in those nearly stable rivers to help in deciding such questions and also to help in reconciling the occasionally conflicting requirements of navigation and drainage in the design of a self-regulating channel.

At the beginning of a programme of river training a discharge would be selected increasing appropriately downstream, such that it would scour out as many as possible of the existing reaches to the depth required and do the same elsewhere with the minimum of bank stabilization and other alterations. When, however, some alteration was necessary and a choice was available between two different schemes of training, that one would be adopted which put the river there more nearly into the shape of its modal meander.

Although it might be many years before the whole of the river would be improved into conformity with the selected discharge, some benefit would accrue as soon as that discharge had been selected, for proposals for interfering with the river could then be judged according to their conformity with it.

As the training programme continued over the course of years more of the reaches would be scoured to the required depths by the selected discharge and more of the bends would be stabilized closer to the mode. The selected discharge would tend to act as the dominant discharge of the river thus artificially altered and stabilized.

Further benefits would then follow. Periodically the river would restore itself to its designed condition. If the bed had been scoured before the dominant discharge period, then the scoured parts would tend to fill up to the required depth, whereas, if there had been a period of low water, with silting, the shoals would be scoured out again. During a period of drought, again, a part of the river might silt so much that the question would arise whether it should be dredged out. The alternatives could then be stated quite simply. To dredge out the shoal would cost a certain sum of money and the work would take, say, twelve months. If left alone, the shoal would be scoured out again by the river itself in due course and probably within, say, two years.

**Mr. E. N. Fenwick** observed that the first sentence of the "Introduction" (p. 4, *ante*) read: "The basic fact regarding flow in alluvial channels is that there is a never-ceasing tendency to attain equilibrium", and thereafter the use of the word "tendency" became almost monotonous. That did not denote any defect in literary style, but rather a realization by the Author that no other word would suffice, because wherever there were conditions subject to the natural laws with which the Author had dealt "tendency" would be found insinuating itself between cause and effect. It was desirable, therefore, not to miss any opportunity of recognizing and learning from any of the phenomena which arose out of those natural laws.

One phenomenon, which was all too often allowed to pass unnoticed, might be referred to as the "phenomenon of reluctance", because it showed, as it were, considerable reluctance to change. Its nature was a tendency to stabilize matter at rest and the state and condition of things as they existed and its effect was to create zones in which transition could

take place in an orderly manner and to ensure against a continuance of those unstable conditions which were associated with transition. Therefore its manifestations were zonal, and he would attempt to show that "stabilizing zones" existed in Nature. For example, water showed no willingness to change into either ice or steam. It was only at the expense of considerable heat-losses that water was prepared to change its physical state. That reluctance was not in one direction only; there was a similar urge in Nature for ice to remain ice and for steam to remain steam. That stabilizing zone, guarding the transition from one physical state to another, existed and was measurable in terms of heat units.

In considering the transition from one condition of flow to another, Reynolds' experiments provided, perhaps, the best example. Starting with streamline flow and a steady increase in velocity, the flow conditions changed at the higher critical velocity, through unstable conditions, to turbulent flow. The reverse process occurred through those unstable conditions at the lower critical velocity. Whichever condition existed in a zone would persist throughout that zone, and that stabilizing zone was measurable.

Another instance was provided by water flowing over a weir. If the weir was of the broad-crested type, such as the open-flume module, it was known that the same flow condition obtained throughout modularity up to a drowning-ratio of 85 or 90 per cent. Above that, unstable conditions were evidenced by signs in the standing wave to break down, and then the drowned weir condition prevailed; and only by reducing the drowning-ratio to about 70 per cent. could modularity, with its attendant standing wave, be established, again through unstable conditions in the transition region. If the weir were of the narrow-crested type the zone would be much more pronounced, and whichever condition of flow existed would persist throughout the range of drowning-ratios from 0 to 66 per cent. He had drawn particular attention to those unstable conditions, because they might be said to bracket the stabilizing zone; that exemplified the kind of change-for-change-sake conditions which would exist except for the stabilizing zone.

With regard to the tendency to stabilize matter at rest, it was elementary that the force required to move a body from rest was greater than that necessary to keep it on the move in the same plane and at the same acceleration. That excess force was the measure of the tendency for the body to remain at rest. A combination of the two forces—the inherent tendency of flowing water in a channel to maintain its existing flow characteristics, and the tendency of the bed silt to remain at rest—gave a fair expectation of a stabilizing zone at transition between the two contending conditions of silting and scouring. Experience supported that, since considerable difficulty was sometimes experienced in upsetting the regime of a channel. For instance, the water surface slope could be increased without any change in the bed, whereas a further small increase would bring about the

total expectation in regard to bed scour. Regime had all the characteristics of a stabilizing zone.

The application of that concept to the Paper was that the dominant discharge was not a single discharge but a range of discharges. Its wider application was that all formulæ showing the relationships between the different dimensions of a regime channel, such as the Lacey formulas referred to in the Paper, should in graphical form show not one line but two lines, between which would lie the stabilizing zone. That would have the effect of giving a more factual representation, and it might well embrace much of the observed data which at present showed discordance with the single line.

Mr. W. P. Thompson observed that alluvial rivers of the type considered by the Author flowed through country of their own making. Taking one example, say, between Karachi where the river entered the sea and Jhelum where the river emerged from the foot hills, the country had a slope of about 1 foot per mile. Thus there was a self-made slope by a river carrying suspended matter brought down from the hills.

The river seemed to find that the slope was too steep for the purpose of carrying the load through that deposited matter and consequently it created for itself a flatter slope and an increased length by meandering from side to side.

In the Paper "meandering" was defined in about seven different ways, but he considered that the way suggested by him was the simplest.

In the Punjab, the flood discharge of a river might range from 100 to 150 times its winter minimum discharge. That was a very wide range, and it was difficult to say which discharge in that range controlled either the slope of the country or the meandering course of the river. Except in those reaches where the river overflowed one bank or both, high floods would determine the meander belt.

From experience on canal construction it was known that fairly small canals would flow with a slope of 1 foot per mile whilst for the very large canals it was necessary to make the slope about one foot in 2 miles. It was more difficult to know, however, what occurred in Nature, especially as a river cross-section was difficult to determine. The river, when in flood, was unapproachable, and it was necessary to wait until it became very low before a survey could provide the details, so that the cross section of the river in flood was an unknown factor.

Formulae applicable to the flow of water in canals had been derived from a study of constructed channels which had been made to flow with just the slope required to discharge the water and its contents, flowing fairly uniformly. A condition of uniform flow was comparable with a body at rest, where a state of equilibrium existed; but a river in flood formed a complete contrast. It was impossible to imagine a greater state of disequilibrium, because at any one section, or from section to section, variation of energy, content, and area was proceeding all the time, so that

*prima facie* the formulae which had been evolved for flow in a channel were hardly applicable to the extraordinary conditions which prevailed with a river in flood.

The study of rivers, either on the site or by models, was important because it enabled the engineer to construct his works with greater accuracy and to maintain them with greater efficiency and at lower cost. When engineers first started building bridges and barrages across the rivers, they were disinclined to interfere with the width of the river, because they did not know what awaited them, and so very long barrages and very long bridges were built. Later, greater knowledge had enabled those same bridges to be reduced to about half their length; the girders were used elsewhere and the cost of maintenance was considerably reduced.

\* \* **Mr. Gerald Lacey** observed that the Paper would make a very strong appeal to those, who like himself, held the view that Nature's laws, when once elucidated, were apt to prove remarkably simple. The Paper had the additional merit of providing a very useful running commentary on river behaviour with which most research workers in India would be in full agreement.

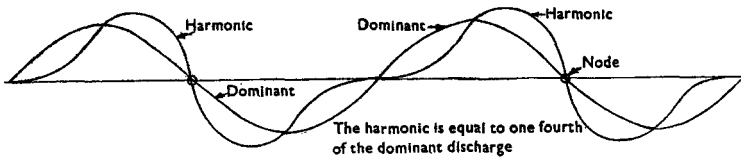
The traditional explanation of meandering in great alluvial rivers was that the flood discharge demanded a flatter slope and that the surplus energy of the river was dissipated by a lengthening of the channel by the formation of loops or meanders. Possibly some confusion existed between cause and effect; nevertheless the explanation would appear to be correct when the flood discharges persisted for a sufficiently long period, the river was in a true alluvial plain, and the material was fine. In such rivers not only would there be a region in which meanders were generated, but also still nearer the sea, where the outfall slope approached zero, there should be a final reach in which the river straightened before entering the delta. The Mississippi and some of the rivers in South America exhibited that tendency.

Although it might be undesirable to press the harmonic analogy too closely, considerable progress should ultimately be made in elucidating meander theory by that means. It was very significant, as the Author had pointed out, that for a given discharge the meander length was of the same order in a flood plain and an incised river. That would suggest that the meander, or wave length, was independent of the resistance offered by the banks and further, by inference, that it was independent of the "persistence" or duration of the generating discharge. Resistance and persistence would, however, both enter as factors in determining the rate of longitudinal progression of the wave. In ideal circumstances simple harmonic waves proceeded in an orderly manner towards the sea. Loops and cut-offs might occur primarily because of some interference in that process. The meander width, or the amplitude, should be a function of the resistance, and by inference, of the persistence.

\* \* \* This and the following contributions were submitted in writing.—Sec. I.C.E.

The Author's reference to the Jumna River railway bridge at Delhi was of considerable interest to Mr. Lacey, who for some years had been responsible for the river training works which extended from the railway bridge to the headworks of the Agra canal at Okhla. He then discovered that not only was there a dominating discharge, determining meander length, which was rather less than the maximum flood discharge, but also there appeared to be a critical discharge of much less volume, but greater persistence, which was set up at comparatively rare intervals and was responsible for very heavy and somewhat alarming localized action. Thus, during one flood season Mr. Lacey had found that a persistent minor flood of the order of 50-60 thousand cusecs led to heavy action, the flood or "dominating" discharge for the river being something more than 200,000 cusecs. The explanation would appear to be harmonic resonance. The small but persistent flood discharge was a higher harmonic—in that instance the octave—with half the meander length generated by the dominant. *Fig. 13* showed a simple harmonic, of the type obtained by the Author

*Fig. 13.*



MEANDER PLAN.

experimentally, modified by superimposing a higher harmonic of one-half the wave length, and, therefore, one-quarter of the dominant discharge.

The plan exhibited a type of meander that appeared to occur more frequently than the simple harmonic. Other higher harmonics would be associated with discharges so small that the effect would be masked. Discharges which did not differ greatly from the dominant had a forced wave length. Aberrations in river behaviour, and unexpected local attacks and embayments, suggested the reality of resonant discharges and the accentuated effects they produced.

The Author had cited Mr. Lacey's empirical equations, and indicated the dimensions involved. The subject had recently advanced and Mr. Lacey hoped to deal with it in his reply to the correspondence on his own Paper on "A General Theory of Flow in Alluvium."<sup>1</sup> One equation, however, that for the slope in terms of the silt or sand factor, was so simple that it might be given and compared with the early expression for regime flow given to the Author.

<sup>1</sup> J. Instn Civ. Engrs, vol. 27 (1946-47), p. 16, Nov. 1946.

Briefly, the following four dimensionless arguments were involved in alluvial flow ;  $V^2/gR$  ;  $V^2/gRS$  ;  $P/R$  ;  $RV/v$ .

Of those arguments any one could be expressed in terms of two out of the remaining three. Dealing with the first three,

$$V^2/gRS = \frac{1}{2}gR/V^2.P/R$$

or 
$$V^4 = g^2AS/2$$

and 
$$S = 2V^5/g^2Q.$$

The last expression did not contain any criterion for sand grade or sand charge. The product ( $VS$ ), however, was proportional to the rate of energy destroyed for every unit of the liquid—in that instance water—transported. The slope equation therefore could appropriately be rewritten

$$S = 2^{1/6}(VS)^{5/6}/(g^2Q)^{1/6}.$$

It would be seen that the turbulence factor of  $V^2/R$  had been replaced by the square root of the energy criterion ( $VS$ ). The prime characteristic of every alluvial channel was the product ( $VS$ ), or effectively its “vertical velocity.”

The Author had presented a mine of information based on skilled observation and should prove of great value to all river engineers.

**Mr. A. St. George Lyster** observed that, as Chief Engineer for Irrigation in the Punjab, it had fallen to him to state the problem to be examined on the model and eventually to decide from the investigators' reports what work should be carried out and what expenditure incurred.

The problem that occurred most frequently in the Punjab was that of the behaviour of the main rivers at the immediate approach to the barrages which enabled the water of the river to be directed into the canals. The river-bed was usually no more than a wide, shallow depression in light alluvial soil and sand and allowed great latitude of movement to the river, in which the flow might vary from 10,000 cusecs to 400,000 cusecs or more ; whilst its natural fall of about 1 foot in 1 mile was seriously disturbed by the barrage. The investigation equipment was extensive. In addition to the indoor laboratory at Lahore, with its several glass-sided flumes and devices for studying subsoil flow, an outdoor model-ground, several acres in extent, enabled half an acre or more to be devoted to a single model, and the predictions arrived at were often realized with an accuracy and reliability that was all that a Chief Engineer could desire. Models which did not attain sufficient reliability on which to base expenditure were of mainly academical value to the engineers responsible for works.

The Author had stated (p. 7) that where banks were of tough material the depth (on a curve) to which bed scour occurred was increased to about twice that in a straight channel. Mr. Lyster had often noticed that

phenomenon in distributary channels of various sizes ranging from 50 cusecs to 250 cusecs but, so far as he knew, the corollary that any masonry works should be given correspondingly deep foundations did not appear in any text-book. Such curved banks were often undermined, when the discharge was falling, by the washing out of a layer of sand which had been saturated while below the water-surface, leaving the superimposed clay unsupported. That layering of material was not to be expected in artificial channels, but it was common in river banks in alluvial soil.

Factors affecting Meanders.—The Author had concluded that the meander length was of the order  $25Q^{\frac{1}{2}}$  to  $30Q^{\frac{1}{2}}$ . The discharge  $Q$  would be in cubic feet per second, or similar units but, in the absence of an original relation between  $M_l$  and  $Q$ , it was difficult to assign a definite value to the meander length.

In the two tables on p. 10 of values of  $M_l$  and  $M_b$  in terms of  $W$  and  $Q$ ,  $M_b$  was greater than  $M_l$ , but that was not usually the case in Nature. In the experimental results given on p. 13 the opposite was the case, and  $M_l$  was greater than  $M_b$ .

The conclusion that where the meander was held at a point the meander-length varied considerably during the flood season and from year to year, was of the greatest importance to bridge engineers, and to all who had to construct barrages and similar devices; but it needed some clarification. As it stood, it seemed to be at variance with the Author's earlier statement, on p. 14, that rivers were constantly seeking a steady regime determined by their existing conditions. It was beyond dispute that the conditions usually kept changing before the river had had sufficient time to adjust itself to them, but the return to steady flow which occurred in most rivers in Northern India in between flood seasons should result in the reproduction of closely similar conditions and, therefore, the same meander length year after year. In Mr. Lyster's experience in the Punjab that annual regularity was usually attained.

The Paper threw considerable light on a very intricate subject and Mr. Lyster hoped that the Author's work would be carried on until prediction attained the degree of accuracy so desired by the engineer who had to plan the work to be carried out.

Mr. A. M. Sims observed that the Paper was of great interest to Railway Engineers in India, particularly to those of the North-Western Railway for whom it was necessary to have a knowledge of meander lengths and widths in the vicinity of railway bridges. Probably many engineers would be surprised to learn that meander widths, as ascertained from experiments in America, were 2.86 times the meander lengths (p. 10, *ante*) and that that exactly confirmed the figures from 1863 relating to the delta of the Ganges. From the figures given,

$$\frac{M_b}{M_l} = \frac{84.7Q^{\frac{1}{2}}}{29.6Q^{\frac{1}{2}}} = 2.86.$$

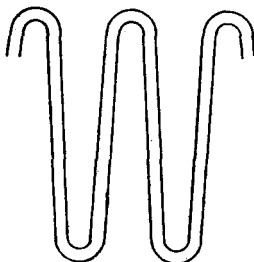
That meant that, instead of the shape of the meander being as shown in *Fig. 1*, the configuration should be as in *Fig. 14*.

It had to be admitted that the more general conception of the shape was given in *Fig. 1*, and Mr. Sims would welcome the Author's views on that point, particularly as most engineers bore in mind an "arc-to-chord" cut-off ratio of about 2.0 when the bed intervening between meanders began to be covered with flood water. There was also considerable discrepancy between the value of 2.86 and that obtained from the experimental results at the Research Station, Poona. From the first four experimental results (p. 13, *ante*) the average values for  $C_1$  and  $C_b$  were 33.87 and 15.41, giving a ratio of 0.46 for meander width to meander length. Could the Author explain that appreciable difference?

Railway Engineers were most interested in the limits of the meander belt widths and maximum flood levels in rivers.

Graphs showing specific discharges plotted against gauge readings had

*Fig. 14.*



been prepared by the Punjab irrigation Branch. They indicated that Indus and Punjab river water-levels were rising gradually by from  $\frac{3}{4}$  inch to about  $2\frac{1}{2}$  inches per year. The North-Western Railway Administration were more interested in the trend of the maximum flood-levels and had prepared graphs for the bridges over the Punjab rivers showing periodic similar times for successive years plotted against the gauge readings at the various bridges, and agreement with regard to a general rising tendency had been obtained; but the main object had been to attempt to forecast probable flood-levels in the years ahead with a view to determining when the raising of any bridge should be taken in hand.

Experience on the North-Western Railway during recent years had shown increasing meander limits and higher flood-levels. Examples of increasing meander width limits were the Chenab above the railway bridge at Wazirabad; the Sutlej between Bahawalpur and Samasata and also at Gidarpindi and, from the latest information available, at Hussainiwala near Ferozepore; and the Indus near Ruk, and just above Sukkur, Budapur, Tirth Laki, and Kallur Kot. With regard to higher flood-levels, some years ago the railway bridge at Jhelum was raised by about 4 feet, and very high levels in the Punjab rivers culminated in record levels in the Indus

just above Sukkur in 1942, with a further record in 1943. What were the causes of these increasing meander limits and rising flood-levels? The Author had referred (p. 15, *ante*) to the lack of knowledge regarding meander belts; but was the cause due in any way to increasing withdrawals from the rivers for irrigation purposes? The sills of some barrages had already been raised.

With the rising of regime levels and the construction of marginal bunds, there was a tendency for rivers to run on ridges in comparison with the level of the surrounding country. At Rohri, just below Sukkur, the ground by the river was already about 15 feet above the level at Ruk, and it sloped away also on the left bank towards the new 15-mile diversion of the main line of the North-Western Railway.

What was the ultimate end to be expected when rivers raised their regime levels and beds, and a system of marginal bunds forced the river slowly up to a ridge? In time, might not such a system of bunds have to be applied to the Punjab rivers? The appalling disaster of 1942 in Sind was a warning not only to Irrigation Engineers but also to Railway Engineers.

The Author's views on those points would be appreciated by Railway Engineers.

The Author, in reply, observed that the discussion had produced a wealth of ideas. Mr. Trench's statement that the bed of the Indus tended to rise during floods was true to a large extent, but the rise was due to the fact that scour had been taking place upstream until a charge was put into suspension which could not be carried when the river reached Sind. The Author should have made it clear that the rule applied down to the point where the river was saturated with silt; below that a tendency existed for silt to be deposited.

Mr. Trench had suggested replacing the term "tortuosity" by "meander-length ratio"; but tortuosity was a measure of intensity of meandering, whereas "meander length" referred to the length of a single meander. It followed that tortuosity might increase down a river while the meander length decreased slightly—as had occurred in the case of the Indus in Sind.

Mr. Trench had observed that the term "dominant discharge" had not been strictly defined. The Author had, however, stated in his Introduction (p. 6).

"In order to prevent this question becoming so complex that it cannot be comprehended, it will be assumed that there is a dominant discharge and its associated charge and gradient, to which a channel returns annually. At this discharge, equilibrium is most closely approached and the tendency to change is least. This condition may be regarded as the integrated effect of all varying conditions over a long period of time."

It was impossible to define "dominant discharge" more clearly,

because it was not an absolute value, but a long-term average equilibrium value, which was varying slightly all the time.

Mr. Trench had also stated that he felt bound to join issue with the Author ; but in quoting, he had omitted important portions of the Author's statements, shown in italics below. Mr. Trench presumably would not dispute the accuracy of the statement :

“ But when the discharge exceeds the dominant discharge, the energy available exceeds that required to carry the dominant charge, so that excess energy is utilized in increasing the charge by scouring the bed, *eroding the banks, and developing meanders.*”

The other quotation, when fully stated read—

“ Where the banks are straight, and are tough enough to resist erosion, the excess energy which develops during floods is absorbed in scouring the bed, *thus increasing the charge downstream.* Where, however, banks are not tough enough to withstand the excess turbulent energy developed during floods, the banks erode, and the river widens and shoals.”

The only place in the Indus in Sind where the banks were tough enough to resist erosion was at Sukkur Gorge where, as Mr. Trench knew, the bed of Sukkur Gorge had scoured to a great depth and maximum scour occurred in the Gorge during high floods, the bed filling up again as the floods fell.

Mr. Trench had, it was clear, looked upon the Kotri banks as “ tough enough to withstand erosion ” ; but the Kotri banks had eroded many centuries ago to the width required to pass the maximum discharge. The point which the Author had wished to bring out was that in material which resisted scour, a channel scoured to a deep narrow section, whereas in material which eroded easily, the channel became wide and shallow. That fact had been demonstrated in models and was not open to doubt ; nor did the Kotri case go in any way counter to the Author's statement.

The three cases quoted by Mr. Trench were interesting, but it was not correct to assume that a rise of gauge was necessarily accompanied by a rise of bed, or vice versa. After the approach channel at Sukkur, which had been designed to exclude sand from the North-Western canals, was completed, the bed of the Rice Canal scoured, but the water-level rose. That was due to the material exposed on the bed becoming coarser as the finer grades were removed by scour, so that the bed roughness increased.<sup>1</sup>

Mr. Trench apparently agreed with the Author regarding the two types of meandering he had defined ; but he considered that the meandering which had developed with small discharges at the head of the Jamrao Canal in Sind about 40 years ago was of a third type, in which meandering was due to “ lack of available energy, not excess energy.”

<sup>1</sup> “ The Behaviour and Control of Rivers and Canals (with the aid of Models) ” (in the Press), Chapter 7, para. 7.03, 7.04, 7.05. Manager of Publications, Government of India, Delhi.

The Author had been in charge of that section of the canal in 1909-11, and had found that the water slope with low discharges was steeper than the slope at full supply, and that the water entering the canal with small supplies was clear, so that the energy exceeded that required to maintain equilibrium. That caused bed scour, which naturally tended to be greatest in the deeper parts of the channel, and the excess energy increased the charge, giving rise to conditions which developed meandering—for reasons explained in the Paper.

Mr. Trench's statement that "where sufficient water was admitted to the canal the meanders disappeared," was not correct. The meander channel still persisted in the bed, although not to such a marked extent as with lower discharges, because the slope was less and the charge more. Thus Jamrao Head was in no way an exception to the rule.

The Author could assure Mr. Salberg that there was very little danger of the river moving downstream; if it were held at two points it could not do so. Many examples could be given of sites where rivers were permanently held, where there was rock or even lime kunkar on two sides of a river. The most typical site was at Benares, where lime kunkar was exposed on the left bank. That was gradually being eaten away, but it was sufficient to hold the river, whilst a few miles upstream the river was held at the other bank by another outcrop; it had been like that since the beginning of history, and there was no indication that it had ever been anywhere else. Another case was the proposed site for the Lower Sind barrage, 25 miles south of Kotri. The river there never shifted because hard material was present on both banks.

Mr. Salberg, speaking of the Kosi, had stated that:

"Fig. 10 showed clearly that numerous channels which had built up the cone, any one of which at any moment might again become the main river."

That was incorrect. The Kosi had been moving westwards for centuries, building up the plain to the east. It never returned to old channels, which silted when the river had passed to form part of the high-level plain.

Mr. Salberg was insistent that "the whole river system of channel, slope, and plain was moving downstream." But that concept was an over-simplification which led to wrong conclusions. If it were correct, the country-level would rise at a rate equal to  $(L \times S)$  (where  $L$  denoted the rate of meander movement downstream, in feet and  $S$  the slope per foot), whereas actually the plain might not rise at all. Again, if the plain moved downstream the river slope at a point would gradually increase, whereas the slope actually decreased very gradually—due to recession of the hills.

Mr. Salberg had expressed the view that:

"the Author's proposal for a spur such as that constructed for the Rupnarain river would not be able to meet the conditions which would occur in the distant future when the meander had moved downstream"

But it was that downstream movement which had been successfully stopped by the repelling groyne in the Rupnarain,<sup>1</sup> and there was not the least reason to suppose that when the river next moved downstream—as it undoubtedly would after a cycle of change—the groyne would not again have the same effect as it had had in 1945, when the river moved 2,100 feet from the eroding bank to flow along the other bank. The effect of the repelling groyne in the Rupnarain was somewhat similar to that caused by guide banks constructed on the Bell system. But a repelling groyne had many advantages: it was much cheaper in first cost and cost of maintenance; it was much quicker in action—the change taking place in a single season; and the tremendous action which occurred at the head of one guide bank and along the face of the opposite guide bank, which had sometimes led to catastrophe and always to great anxiety and much expenditure of money, was avoided.

Mr. Salberg was apprehensive of “a certain limitation of the Author’s bridge training solutions based on model experiments”; but where a river had been properly double-pegged by training works or by natural out-crops—such as those at Benares on the Ganges and at Kotri and Hajipur on the Indus—neither the plain nor the meander moved downstream, because the river was permanently held.

The Author would again emphasize that the design was not based on model experiments, but on experience, the model being used merely to demonstrate the suitability of the method and as an aid in working out details.

His opinion that training works should always be designed taking into account all changes in the river which experience had shown might take place in years to come, had been stressed again and again in earlier Papers, wherein he had stated that river models were an aid to engineering skill which, however, they could never replace.

Professor White had suggested that a student who had read the Paper would expect to see a river in the form of a straight channel flowing from the hills to the sea. He would, in fact, do so if the discharge and the charge of material were constant. In other words, as was stated in the Paper, meandering was due to fluctuating conditions.

Professor White had also observed that, “the Author had suggested the principle of minimum dissipation, but that seemed to imply smooth and straight channels as the norm, whereas those formed in Nature were tortuous without exception.”

The Author had, however, been at great pains to emphasize that where the discharge and charge varied—that was to say, nearly everywhere in Nature—the channels would be tortuous. That was in agreement with

<sup>1</sup> “Training Works constructed in the Rupnarain River in Bengal—after Model Experiments—to prevent further Bank Erosion endangering the Bengal-Nagpur Railway Line linking Calcutta with Bombay and Madras.” Instn Civ. Engrs, Maritime Paper No. 3 (1946).

the principle of minimum dissipation, and Professor White would remember that where variations in the discharge of the Mississippi became relatively smaller than upstream, near the sea, the river was almost straight and uniform in section.

The Author agreed that turbulence was the agency causing movement of material down a river. That was Nature's way of approaching equilibrium: but simultaneously Nature was continuously reducing the energy of flow, and were it not for tectonic and other counteracting factors, the hills would eventually be removed.

It was true that "all courses from the hills to the sea descended through precisely equal heights and involved equal total dissipation"; but, although the total dissipation of energy would be the same to start with, the proportion of the energy dissipated in overcoming friction and erosion, and in carrying the load, would be different. Hence the rate of denudation and the slope, shape, and course of the channels would also be different.

Professor White's argument assumed an imposed difference of level in a finite length, whereas in the case of alluvial rivers—which was what the Paper primarily dealt with—the rivers were flowing on a self-made plain, the slope being generated by the varying discharges and charges. The essential point was that the energy-gradient in an alluvial river was the minimum required to carry the varying charge with the varying discharge available. That could be seen much more clearly in the case of canals which, if designed with too steep a slope, flattened their gradients until the slope was the minimum necessary, under field conditions, to carry the charge.

With regard to Professor White's statement that, "At the Imperial College the whole sequence occurred under ideally steady conditions, and did not arise from fluctuations in feed, as in the Author's experiments," some misunderstanding appeared to have arisen; meanders were also produced at Poona with steady flow, downstream of where the slope was in excess of the equilibrium slope, causing movement of coarse material.

The Author agreed with Professor White's view that the formation of long undulations on the bed led to meanders. Similar effects were noted at Poona when the slope was sufficient to cause heavy bed scour—as a result of which a heavy charge of bed material was in movement.

It was satisfactory to find that Professor White agreed that  $M_1 = 27Q\frac{1}{2}$  held approximately over a wide range of conditions; because it was on that relation that successful river training depended.

It would have been helpful if Professor White followed Mr. Gerald Lacey's habit of quoting his data in full. That would be particularly interesting with regard to meander data. Professor White had stated that: "The theory of turbulence had yielded quantitative predictions for the transport of solids which were valid in some rivers"; but he had neither given data nor references. The making of statements without quoting authorities made his opinions much less authoritative than they

would otherwise be—especially when, as in that case, exhaustive studies in India had shown that there was no practical direct method of measuring the movement of bed sand in rivers.

Similarly Professor White had not supported by data or references his statement that :

“ the variation of  $\lambda_M/b$  did not follow an ordinary probability curve ; instead it had a double peak with two strongly-preferred values—15.5 and 7.0.”

Had references been given an explanation might have been possible ; but without references all that could be said was that that statement did not tally with the Author's experience.

The formulae suggested by Professor White were important, and they were dealt with on p. 51, *post*.

The Author agreed with Mr. Evans that nearly all rivers were incised in the upper part of their length and flowed on a flood plain in the lower part.

The causal factors determining whether a river was incised or emerged on the flood plain were determined by meteorological and geological conditions. For that reason, each case should be considered on its merits.

The “ phenomenon of reluctance ” referred to by Mr. Fenwick linked up with the opinion of Mr. T. R. Sethna, A.M.I.C.E., Research Officer, Irrigation, Sind, that there were two states of flow in canals, in one of which the bed of the channel was smooth and

$$V = CRS,$$

whilst in the other

$$V = C\sqrt{RS} ;$$

$C$  in both cases being determined by the size of bed material.

Certainly the Sind data showed a zone in which the type of flow changed from what the Author had called “ ripple turbulence ” to “ rough turbulence,” the data of channels tending to be near the two limits of the zone.

Mr. Fenwick's idea that the dominant discharge also represented a zone rather than a single discharge, meant that there would be a change of state of flow. That point merited further study.

Mr. Thompson's view that :

“ the river seemed to find that the slope was too steep for the purpose of carrying the load through that deposited matter and consequently it created for itself a flatter slope and an increased length by meandering from side to side ”

was unsound, because an alluvial river developed its own slope, which was natural for the dominant discharge. What the river had to do was to develop an autogenous course which best fitted a wide range of varying conditions of discharge and charge ; but a local rise of country level, due to earth movement, would cause an abnormal slope and increased meandering.

The Author could not agree with Mr. Thompson that except in those reaches where the river overflowed one bank or both, high floods would determine the meander belt. In high floods, flow tended to short-cut over the shelving bed near the convex banks even though the banks were not overtopped ; but he agreed that in the case of incised rivers the meander belt would be larger, owing to resistance to downstream movement of meanders being much greater, and to the dominant discharge being relatively greater.

Mr. Thompson's statement that :

“ *prima facie* the formulæ which have been evolved for flow in a channel were hardly applicable to the extraordinary conditions which prevailed with a river in flood ”

indicated that he had misread the Paper. All that was stated was that, at dominant discharge, the canal formulæ applied roughly to rivers—with, of course, different coefficient values.

The question of dominant discharge was very important and very difficult. The Author agreed with Mr. Thompson that the same conditions could not be expected in a river as in a canal ; but, provided one thought in terms of the dominant discharge, the same formulæ applied to both rivers and canals—not the same coefficients, but the same laws. In other words, the dominant discharge was a clarifying idea, and without that concept the Author could not see how it would be possible to obtain any result from the study of rivers.

Mr. Lacey, with his gift for terse exposition, had put much that was contained in the Paper into a few lines, in his observation that :

“ It was very significant, as the Author had pointed out, that for a given discharge the meander length was of the same order in a flood plain and an incised river. That would suggest that the meander, or wave length was independent of the resistance offered by the banks and further by inference, that it was independent of the ‘ persistence ’ or duration of the generating discharge. Resistance and persistence would, however, both enter as factors in determining the rate of longitudinal progression of the wave. In ideal circumstances simple harmonic waves proceeded in an orderly manner towards the sea. Loops and cut-offs might occur primarily because of some interference in that process. The meander width, or the amplitude, should be a function of the resistance.”

The Author agreed with Mr. Lacey's explanation of what appeared to be “ a critical discharge of much less volume than the dominant discharge, but greater persistence, which was set up at comparatively rare intervals and was responsible for very heavy and somewhat alarming localized action.”

He had, in fact, mentioned such half-length meanders several years ago, calling them “ sub-meanders with semi-meander length.”

Mr. Lacey's new slope equation :

$$S = 2\frac{1}{2}(VS)^{\frac{5}{8}}/(g^2.Q)^{\frac{1}{8}}$$

where  $(VS)$  was proportional to the rate of energy destroyed for each unit of the liquid transported, could be re-written in various forms ; for example,

$$S = 2V^5/g^2Q ; \text{ or } V = (S.Q)^{\frac{1}{5}}.g^{\frac{2}{5}}/2^{\frac{1}{5}} ; \text{ or } S = 2V^4/g^2A ;$$

so that

$$S = 2Q^4/g^2A^5$$

That was a good formula for estimating velocity from slope and area, but it was not a good slope formula, because it necessitated knowing the values of  $V$  to a high degree of accuracy, as it was present in the formulae to high powers, so that a small error in observing  $V$  was greatly magnified in  $S$ .

Mr. Lyster had rightly pointed out that one of the Author's statements required clarification. The word "downstream" was omitted. The correct statement was

"Where the meander was held at a point the local meander length downstream of the fixed point varied considerably during the flood season and from year to year."

Above the fixed point there was no such effect. In fact, there was an opposite effect.

Mr. Sims had asked some very pertinent questions, he had called attention to the fact that the meander-ratio of 2.86 given for American rivers was much greater than was illustrated in *Fig. 1*, and greater even than the arc-to-chord cut-off ratio of about 2.0 which held approximately for the Indus ; and that it also varied widely from the experimental results at the Research Station, Poona ; and he had asked for some explanation of the difference. *Fig. 15* showed some typical, highly developed, meanders of the Mississippi ; but even those had a much smaller meander-ratio—about 1.6—than the original value of 2.86. The Author suggested that the discrepancy was probably owing to meander belt being taken in America to cover the full width of swing of the river over a period of years, and not merely its extreme limits at any one time, which was the Author's definition.

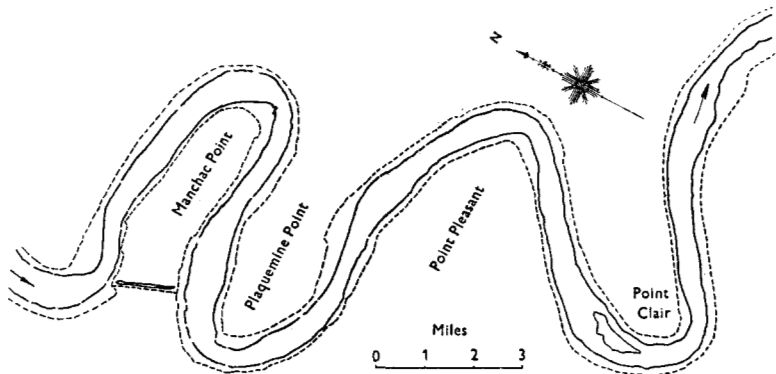
Mr. Sims had also inquired as to the causes of increases in meander limits and rises in flood levels. In the Author's opinion, meander limits were determined mainly by the degree of resistance to downstream progression of meanders ; and a great increase in resistance was provided just upstream of a barrage, where downstream progression of the meander was checked.

The effect of a barrage did not, however, end there ; because when water was headed at a barrage, that caused deposition of bed sand at the upper end of the pond which, in turn, caused the water-level to rise, causing accumulation of silt on the river berms. That rise was greater than the bed rise—because bed accretion was restricted owing to the barrage gates

being opened in high floods, which scoured the bed. As a result, the bed became gradually deeper, with an increase in dominant discharge. That slowed down the rate of meander progression and allowed the meander to develop its belt to a greater extent.

The rise of water-levels caused by afflux had been dealt with at length by the Author in his Report on "The Behaviour and Control of Rivers and Canals (with the aid of Models)." Roughly stated: heading up water at a barrage threw down bed sand upstream. At first that reduced the coarser material carried through the barrage and retrogression occurred downstream; but after a few years, sand began to be washed through the

Fig. 15.



MEANDERS OF THE MISSISSIPPI RIVER DOWNSTREAM OF BATON ROUGE, LOUISIANA.

barrage as a supercharge when the gates were opened to pass flood discharges, and that deposited immediately downstream, where the velocity was lower than upstream. That effect was magnified, owing to the relatively clear winter water—which previously had carried some of the sand down the river—being diverted into the canals. Thus deterioration occurred downstream of the barrage, which prevented material deposited upstream of the barrage being carried through the barrage to the same extent as before; so that the effect of the barrage was upstream deterioration, followed by downstream deterioration, causing further upstream deterioration. That process continued, though at a decreasing rate, until heading up at the barrage during the period when sand was in movement, became unnecessary owing to the water level above the barrage being high enough, under such conditions, to supply the canals without heading-up water; but the river downstream would still deteriorate at a greater rate than before the barrage was built, owing to most of the clear water flowing in the river in the fair season being drawn into the canals, instead of flowing down the river to improve it.

Thus heading-up of water at barrages and the withdrawal of sand-free water by canals both caused accelerated rise of water-levels in rivers. That fact had been epitomized by Mr. A. M. R. Montagu, M.I.C.E., lately Chief Engineer, Punjab Irrigation, in one sentence: "The construction of a barrage signs its own death warrant."

Viewed on a long-term basis, alluvial rivers would—unless conditions altered—continue to raise their flood-levels. In the past, Nature reduced the rate of rise by avulsions, the whole flood plain being gradually built up; but where river avulsions were prevented by flood embankments, the rate of rise increased correspondingly. The only remedy was to reduce fluctuations of discharge and charge passing down the river.

The obvious course would be the construction of retention reservoirs; because those would be used to reduce fluctuations in discharge and charge; and they would also catch bed-sand for many years, thus preventing its passage down the river. This would cause retrogression of bed-levels, owing to the slope required to carry the relatively sand-free water being much less than before. Such reservoirs would not, at first, check deterioration far downstream of the reservoir. There would, in fact, be a length of river in which deterioration would occur due to coarse material being scoured and washed downstream, and improvement might take many years.

For maximum improvement the regulation would have to be on a carefully conceived plan; but it would, in time, provide a definite remedy—though at a cost which might be prohibitive, even if the reservoirs were used for multi-purpose development; but where conditions were favourable—as in the case of the Tennessee Valley Authority scheme in America—floods and deterioration could be brought under complete control for many years.

Professor White had presented a set of dimensional formulae of the type

$$\frac{bg^{\frac{1}{2}}}{Q^{\frac{1}{2}}} = \alpha \left( \frac{Q}{k^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{n_1} \left( \frac{k(c^1g)^{\frac{1}{2}}}{V^{\frac{2}{3}}} \right)^{n_2} \left( \frac{G}{Q} \right)^{n_3} \dots \dots \dots (A)$$

where  $b$  denoted breadth,  $m$  size of solid particles, and  $G$  the quantity of solids conveyed.

The right side of that formula should, however, take the form:

$$\alpha \left( \frac{Q}{m^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{n_1} \left( \frac{XV_s}{(\nu g)^{\frac{1}{2}}} \right)^{n_2} \dots \dots \dots (B)$$

where  $m$  denoted the effective diameter, in millimetres,  $X$  the charge =  $\frac{\text{load}}{\text{discharge}}$ , and  $V_s$  the effective terminal velocity of fall of particles in water.

When  $n_1$  and  $n_2$  in formula (B) were linked with the exponents in Mr.

Gerald Lacey's formulae for regime channels,<sup>1</sup> restated in terms of  $Q$  and his silt factor  $f$ , a complete set of formulae could be derived, as shown below.

The justification for linking  $X$  with  $V_s$  and hence ( $\nu_g$ ) was twofold :

- (i) An investigation carried out at Poona under the Author's direction had shown that the rate of deposition of material in water was constant so long as the product ( $X \cdot V_s$ ) remained constant, that was, so long as  $X$  varied inversely as  $V_s$ .<sup>2</sup> That was the case where the material was quartz sand and  $m$  was greater than about 0.15 millimetre and less than about 0.5 millimetre ; but for grains smaller than 0.08 millimetre Stokes's law held and  $V \propto m^2$  ; and for grains coarser than about 0.7 millimetre  $V_s \propto m^{1.3}$
- (ii) Experiments initiated by the Author at Poona, when he was Director of the Indian Waterways Experiment Station, had shown that when the " grade " of material was kept constant and the " charge " varied, the exponents were similar to those in the new set of formulae when those exponents were based on Lacey exponents.

The full set of derived formulae were

<i>New formulae.</i>	<i>Lacey equivalents.</i>
$b = \alpha_1 \cdot \frac{Q^{\frac{2}{5}}}{g^{\frac{1}{5}}} \cdot \left( \frac{Q}{m^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{\frac{1}{10}} \cdot \left( \frac{XV_s}{(\nu g)^{\frac{1}{3}}} \right)^{\frac{1}{4}}$ $= \alpha_1 \frac{Q^{\frac{1}{2}}}{g^{\frac{1}{5}} \cdot \nu^{\frac{1}{12}}} \cdot \left( \frac{XV_s}{m} \right)^{\frac{1}{4}} \dots \dots \dots (C)$	$P = 2.67Q^{\frac{1}{2}}$
$A = \alpha_2 \cdot \frac{Q^{\frac{1}{2}}}{g^{\frac{2}{5}}} \cdot \left( \frac{Q}{m^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{\frac{3}{10}} \left( \frac{XV_s}{(\nu g)^{\frac{1}{3}}} \right)^{-\frac{1}{2}}$ $= \alpha_2 \cdot \frac{\nu^{\frac{1}{6}}Q^{\frac{5}{8}}}{g^{\frac{7}{8}}(m \cdot XV_s)^{\frac{1}{2}}} \dots \dots \dots (D)$	$f = 1.76m^{\frac{1}{2}}$  $A = 1.26Q^{\frac{5}{8}}/f^{\frac{1}{2}}$
$V = \alpha_3 \cdot \frac{g^{\frac{1}{8}}}{\nu^{\frac{1}{6}}} Q^{\frac{1}{6}} (m \cdot XV_s)^{\frac{1}{2}} \dots \dots \dots (E)$	$V = 0.7937Q^{\frac{1}{6}}f^{\frac{1}{2}}$
$d = \alpha_4 \cdot \frac{Q^{\frac{2}{5}}}{g^{\frac{1}{5}}} \cdot \left( \frac{Q}{m^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{-\frac{1}{5}} \cdot \left( \frac{XV_s}{(\nu g)^{\frac{1}{3}}} \right)^{-\frac{1}{3}}$ $= \alpha_4 \cdot \frac{\nu^{\frac{1}{6}}}{g^{\frac{1}{5}}} \cdot \frac{Q^{\frac{1}{6}}m^{\frac{1}{6}}}{(XV_s)^{\frac{1}{3}}} \dots \dots \dots (F)$	$R = 0.4725 \left( \frac{Q}{f} \right)^{\frac{1}{3}}$
$S = \alpha_5 \left( \frac{Q}{m^{\frac{5}{2}}g^{\frac{1}{2}}} \right)^{-\frac{1}{5}} \cdot \left( \frac{XV_s}{(\nu g)^{\frac{1}{3}}} \right)^{\frac{5}{2}}$	

<sup>1</sup> " Stable Channels in Alluvium," Min. Proc. Instn Civ. Engrs, vol. 229 (1929-30, Part I), p. 259.

<sup>2</sup> Central Irrigation and Hydrodynamic Research Station, Poona. Annual Reports (Technical), 1940-41, p. 50 ; 1941-42, p. 33.

<sup>3</sup> Sir Claude Inglis, " The Behaviour and Control of Rivers and Canals (with the aid of Models)," Chapter 3 (in Press).

$$S = \alpha_5 \cdot \frac{(m \cdot XV_s)^{5/2}}{\nu^{5/2} g^{1/2} Q^{1/2}} \dots \dots \dots (G) \quad S = 0.000547 \frac{f^{5/2}}{Q^{1/2}}$$

$$(S.V.) = \alpha_6 \cdot \frac{g^{1/2}}{\nu^{1/2}} \cdot (m \cdot XV_s)^{1/2} \dots \dots \dots (H) \quad SV = 0.000434 f_s \nu^2$$

$$\frac{b}{d} = \alpha_7 \frac{Q^{1/2} \cdot (XV_s)^{7/2}}{g^{1/2} \nu^{7/2} \cdot m^{1/2}} \dots \dots \dots (I) \quad \frac{P}{R} = 5.65 Q^{1/2} f^{1/2}$$

$$M_l = \alpha_8 \cdot \frac{Q^{1/2}}{g^{1/2} \nu^{1/2}} \cdot \left(\frac{XV_s}{m}\right)^{1/2} \dots \dots \dots (J) \quad \text{---}$$

$$\frac{V^2}{gd} = \alpha_9 \left(\frac{XV_s}{gD}\right)^{1/2} \text{ (Froude number)} \dots \dots (K) \quad \frac{V^2}{R} = \frac{4}{3} f$$

$$\frac{Vd}{\nu} = \alpha_{10} \cdot Q^{1/2} \cdot \frac{g^{1/2}}{\nu^{1/2}} \cdot \left(\frac{m}{XV_s}\right)^{1/2} \text{ (Reynolds number)} (L) \quad \frac{VR}{\nu} = 0.375 Q^{1/2}$$

It was interesting to compare those formulae with the two of Mr. Gerald Lacey's formulae which he had recently put in dimensional form <sup>1</sup> :

(i)  $P/R = \frac{1}{2} (V^2/gR)^{1/2} \cdot (RV/\nu)^{1/2} \dots \dots \dots (i)$

which reduced to  $P = \frac{1}{2} \frac{VR}{(g\nu)^{1/2}} \dots \dots \dots (ia)$

$$P^2 = \frac{1}{2} \frac{Q}{(g\nu)^{1/2}}$$

or  $P = \sqrt{\frac{Q}{2(g\nu)^{1/2}}} = 2.78 Q^{1/2} \text{ (at 30 degrees C.)} \dots \dots (ib)$

(ii)  $V^2/gRS = \frac{1}{2} (V^2/gR)^{-1/2} \cdot (RV/\nu)^{1/2} \dots \dots \dots (ii)$

or  $V = g^{1/2} \nu^{1/2} \cdot R^{1/2} S^{1/2} \dots \dots \dots (iia)$

$$= 15.8 R^{1/2} S^{1/2} \text{ at 30 degrees C.} \dots \dots \dots (iib)$$

Although those two formulae were dimensionally homogeneous, they omitted the effect of charge and grade.

Thus  $P = \sqrt{\frac{Q}{2(g\nu)^{1/2}}}$  should be replaced by

$$b = \alpha \frac{Q}{g^{1/2} \nu^{1/2}} \cdot \left(\frac{XV_s}{m}\right)^{1/2}$$

When  $R^{1/2}$  and  $S^{1/2}$  in the second formulae were rewritten in terms of the new equations (F) and (G),

$$V \propto \frac{\nu^{1/2}}{g^{1/2}} \cdot Q^{1/2} \cdot \frac{m^{1/2}}{(XV_s)^{1/2}} \dots \dots \dots (M)$$

<sup>1</sup> *Nature*, vol. 159 (1947), p. 408 (22 March 1947).

but, as pointed out by the Author in 1936,<sup>1</sup> that formula should have been put in the form :

$$V = 16 \cdot 0 R^{\frac{2}{3}} \cdot S^{\frac{1}{3}} \left( \frac{f_{Va}}{f_{dS}} \right)$$

where the silt factors  $f_{Va} = 0.75 V^2/d$

and  $f_{dS} = 192 d^{\frac{1}{3}} S^{\frac{2}{3}}$

So that equation (M) should be rewritten

$$V \propto \frac{\nu_s^{\frac{1}{3}}}{g^{\frac{1}{3}} \tau_s^{\frac{1}{3}}} \cdot Q^{\frac{1}{3}} \cdot \frac{m^{\frac{1}{3}}}{(XV_s)^{\frac{1}{3}}} \cdot \frac{g^{\frac{1}{3}}}{\nu_s^{\frac{1}{3}}} \cdot \left( \frac{XV_s}{m} \right)^{\frac{1}{3}} \dots \dots \dots (N)$$

$$V \propto \frac{g^{\frac{1}{3}}}{\nu_s^{\frac{1}{3}}} \cdot Q^{\frac{1}{3}} (m \cdot XV_s)^{\frac{1}{3}} \dots \dots \dots (O)$$

which was identical with equation (E).

Thus grade and charge should not be omitted, as in Mr. Lacey's second formula ; though they came in to such a low power that they had a negligible effect in the sand region where  $V_s \propto m$ , and not much effect even in the regions where  $V_s \propto m^{\frac{1}{2}}$ , or  $m^2$ .

The remarkable fact emerged from the foregoing—that Mr. Gerald Lacey's formulae, evolved by him some eighteen years ago from very little data, required no modification for use by practising engineers and were also fundamentally sound.

The addition of the effect of charge would, however, be found very valuable in analysing channel conditions.

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<sup>1</sup> Discussion on Dr. N. K. Bose's Punjab Engineering Congress Paper 192, "Silt Movement and Design of Channels."