Discussion.

Professor Dixon.

Professor S. M. Dixon remarked that much of Mr. Davies's Paper was devoted to criticism of Professor Gibson's Paper on siphon spillways, and it would appear that Mr. Davies found himself in opposition to most experimenters. In fact Professor Dixon found it necessary to defend himself, in case he might be included among some who, in Mr. Davies's view, had actually challenged the law of conservation of energy. The experiments to which he had referred in the discussion on Professor Gibson's Paper had been carried out with the greatest care by Mr. K. C. Wu, and from very close connection with those experiments he could say that he had no hesitation in relying on the accuracy of the results. He felt quite sure the work that had been carried out by other experimenters mentioned by Mr. Davies had been done with full knowledge of the difficulties of the experiments, and with just as much care as was described on p. 359. In his general remarks on the tests (p. 364) Mr. Davies mentioned that there was a difference of 18 per cent. between the results obtained in one of his experiments and a similar experiment at Manchester, and he said "the main factor is undoubtedly the polishing of the Manchester model"; and yet, in discussing friction of the walls, he stated (p. 367) that the loss due to friction was so small that it might be neglected. The point about the polishing of the Manchester model was an important one, because in models of such apparatus experimenters were compelled to use not the actual material of construction—which would not be fair—but a reasonable material for the purpose; and a material that was easily worked was timber. Experimenters were convinced that, for a model of about 1/12 scale, timber was a reasonable material to use, and the effect of the friction would be much the same as that in a well-finished masonry structure twelve or twenty times the size. Probably the difference generally between the results of Mr. Davies's experiments and those of Professor Gibson's was not great. That was noticeable particularly at the bottom of p. 368 and the top of p. 371, where Mr. Davies explained the results he had obtained as being due to the surging action that took place in the siphon. As Professor Gibson had said,[1] the flow was complex and subject to very wide variations in velocity and pressure across the various sections.

[1] Minutes of Proceedings Inst. C.E., vol. 231, p. 205.

Professor Dixon.

In other words, Bernoulli's law was not contradicted as Mr. Davies said, because in a siphon Bernoulli's law, when applied to average velocity at a section, really did not hold. There was no constant regime in the flow in the siphon, a fact which was seen from Mr. Davies's experiments just as well as from Professor Gibson's experiments. Mr. Davies seemed to think that there might be difficulty about taking up the design of these very useful engineering contrivances if there was any doubt—and Professor Dixon thought there was no doubt at all—as to the design. For first approximations in design it was always, he thought, implicitly accepted by designers that the flow of water through the siphon was free-vortex flow at the bends and something like pipe flow through the rest of the siphon. It would seem that the design of a siphon was in general carried out in somewhat the following way:—First the siphon was calculated approximately by the laws of pipe flow, assuming free vortices at the bends. Then a model was made, adopting the conditions noted in Professor Gibson's Paper as to scale. With smooth timber, friction effects would be approximately the same in the model as in a well-finished masonry siphon. In an actual case of a siphon designed by Mr. W. T. Halcrow, M. Inst. C.E., the model had been made to 1/8 scale in timber and tested in the laboratory of the Imperial College of Technology, with the following results:—The calculated discharge of the siphon under a certain head was 642 cusecs. The discharge of the model was 3·385 cusecs, corresponding to 613 cusecs on the full scale, an error of $4\frac{1}{2}$ per cent. Confirming some of the results given by Mr. Davies, it was found that variations in the head corresponding to 1 foot and 2 feet in the real siphon caused no changes in the coefficient of discharge. On p. 23 Mr. Davies stated some of the best reasons why experiments with models should be carried out. The word "efficiency" seemed to be a very vague term to employ in connection with siphons. He would suggest that the characteristic of a siphon be defined as the discharge per foot width for a given rise above the crest, divided by the square root of the working-head. That would enable the discharges of competitive designs to be compared. Mr. Davies pointed out that a curve of large radius gave smooth flow. In construction that form of siphon possessed several advantages. It was easy then to form the siphon so that the lower limb followed the downstream face of the dam, instead of being built into the dam with a horizontal leg through the dam. It was evident that the large hollow spaces of a siphon were out of place in the body of a gravity dam.

Mr. Braine.

Mr. C. D. C. Braine remarked that among the laws of siphon flow enunciated by Mr. Davies no mention was made of those governing the priming of a siphon, and it would add to the scope of his

Mr. Braine. Paper if he and others would give their views on that subject. Such views would be of value to any engineer called upon to design a siphon without the aid of a model. Models might be advisable—even essential—on big installations, but should not be necessary for small units discharging, say, 500 cusecs. When regarded in that light, the behaviour of model A was somewhat disconcerting; for it was a type of siphon said to be self-priming, and yet, with a depth of water over the crest approximately one-third of the throat-depth, it did not prime, and apparently altering the form of outlet did not make it prime; whereas at Manchester a similar siphon was usually self-priming and its priming qualities were influenced by the form of outlet. Here, then, was a pitfall into which a designer might be led. Was it always safe to assume that inclining the leg backwards, as on the Erie Canal siphon, so that the falling water impinged on the opposite side, was a certain and definite means of causing a siphon to prime when the water-level rose a little way over the crest, even if the outlet was not sealed? To what extent did that backward inclination affect the discharge? According to the Paper it would be supposed that it probably affected the discharge very considerably. Would Mr. Davies be good enough to state the criteria for a suitable coefficient of discharge for a siphon when no model was available, considering that the coefficients of such diverse types of siphon as the Erie Canal, Hetch Hetchy, and the San Roque, varied so little? The ordinary designer might say there was a substantial difference in those types and would expect a big difference in coefficients, but in actual fact, judging by models and disregarding the question of priming, the difference was not very serious. Could Mr. Davies also say what relation the vortices, such as those described in Test A8, bore to the working-head and to the velocity at entry? The statement that the coefficient, K, of loss of head in the lower bend, varied inversely as the square of the radius of the bend was not quite clear. Did not that law apply only where the outlet corresponded with the Section FF on that particular type of siphon, or did it apply to all siphons irrespective of type? If the siphon was symmetrical at each end it was difficult to see why the losses in the lower bend should be so much greater than in the upper, especially if the water seal was deep; and that was borne out by the American experiments on the Leaburg siphon, which indicated that the coefficient of discharge was approximately constant for similar heads when the siphon was tested lying horizontally or in its proper position.

Mr. Husbands. Mr. H. W. S. HUSBANDS remarked that Mr. Davies appeared to think that the chief losses on the Manchester model were due to a sharp bottom bend, but it would seem natural to expect more

Mr. Husbands.

loss on a bottom bend if it was not of larger radius than the crest bend, for the natural velocity of flow, if the head were free and not confined in a siphon, would be greater at the bottom of the lower leg. By analogy it would be expected that a larger bend would be needed at the bottom than at the crown, although actually in the siphon the water was constricted to a mean velocity on account of the suction drawing up the water from the intake and preventing the water in the lower leg from flowing as fast as it otherwise would. It was obvious that when the lower bend was left open to the air the water would at once be fully affected by gravity and would begin to flow much faster, and therefore there would naturally be more friction on the lower bend. In *Fig. 7* Mr. Davies showed that the great loss from eddies in the crest came, not evenly throughout the curve, but at the beginning and at the end. From B to B_1 and from C_3 to D there were very heavy losses, so that it would appear that even in connection with such a thing as a siphon there was more disturbance on entering and leaving the curve, just as was found to be the case with railways, etc. It looked as if the type of design would probably be improved by the introduction of transition curves. As Mr. Davies said, it was very difficult to make a wooden model accurately to within $\frac{1}{16}$ inch, although in one of his own models Mr. Husbands had had to work in parts to as little as $\frac{1}{64}$ inch. Mr. Davies sometimes used the term "loss" in a misleading sense; the only real losses were due to friction on the side walls and to the eddies. The loss due to conversion of head into velocity was not a real loss, and some work was done in drawing the water up above the mean hydraulic gradient. He did not see why there should be any serious loss in a siphon beyond the side-wall friction. Probably a great deal of the loss would be avoided if transition curves were adopted, and that would perhaps enable a smaller crest curve to be used without any difficulty, and probably with less loss. In his opinion the lower bend should if possible be made of a larger radius than the top bend. Whether it would be advisable to put a transition curve in there was doubtful. It was obvious that, if the siphon was to prime, some extra resistance must be put in the lower leg to allow the top to fill, and that resistance was often put in by means of the bottom bend. Perhaps it was advisable to give a little more resistance in the bottom bend in order to ensure priming.

Mr. Halcrow

Mr. W. T. Halcrow considered that the Papers contributed further useful information on matters which were always of great interest to hydro-electric engineers. He would not comment on the Paper on siphon flow, although he had done a good deal of investigation in that matter recently, in connection with a dam now being built. Professor Dixon had referred to a model which

Mr. Halcrow. he had had constructed for the purpose. It had afforded a good check on the calculations made, and the results had proved to be satisfactory.

With regard to the Paper by Professor Gibson and Mr. Cowen, observations made on surge-tank models showed close agreement with the results obtained in actual installations. Similarly it had been found that observations on models agreed with calculations made. There had been close agreement in both cases as far as he had found, applying to cases of reduction of load, though not necessarily to cases where load was put on. Some years ago, when designing a surge chamber for a tunnel 15 miles long, a model had been constructed on rather a larger scale than Professor Gibson and Mr. Cowen described in their Paper, the pipe being about 50 feet long and having a diameter of 4 inches, and the model of the surge chamber having a pipe 6 inches in diameter. In that case it was found that the model gave results which agreed closely with the calculations when the load was reduced, but the same degree of accuracy was not obtained under the conditions of putting load on. Perhaps the Authors could give some explanation of that. Subsequently, when the work was completed, a few tests were carried out for the purpose of comparing the actual conditions with those calculated. In the case of reduction of load, which was a reduction in flow from about 400 cusecs to 60 cusecs in a short period, it was found that the results obtained from observations agreed closely with the calculations. The first upward surge rose to a height of 39 feet above the steady water-level, and agreed with the calculated figure to within 1 foot. Similarly, the calculation of the minimum level agreed to within 1 foot. It was noticed, however, that the actual periodic time of oscillation was longer than that given by calculation. The calculated time was about $10\frac{1}{4}$ minutes, whereas the observed time of oscillation was $13\frac{1}{3}$ minutes. In that case the time of the first half-oscillation differed by only 1 minute from the calculated time, the major part of the disagreement being during the second half-oscillation. Only one test had been carried out. Owing to the conditions under which the British Aluminium Company's power-house operated it was not very easy to arrange a thorough series of tests, but it was hoped some day to do a good deal more on those lines if it could be arranged, and the results of the tests would be communicated to The Institution. In all the calculations friction had been assumed to be proportional to the square of the velocity, but in practice it usually varied according to some slightly less power of the velocity. For a long time he had been anxious to obtain the actual coefficient for the 15-mile tunnel, because it would be of great value. He had not, however, been able to do that with any degree of accuracy, for the following reason. In the first stage of development the quantity of water flowing through the tunnel had been comparatively small and the velocities low—less than 2 feet per second. Consequently the levels of the water in the various intake shafts had differed only slightly, and it had been difficult to measure precisely the difference in level. It would be appreciated that calculating precise levels over mountains was not a very simple operation, and there might be small inaccuracies. Also, taking the level of the water by measuring down the intake shafts was not an easy thing to do; and the result was that, when the difference between any two shafts was small, the error was a high percentage of the difference of level. As soon as the whole scheme was completed and the installation was working up to its full conditions with higher velocities, there would be bigger differences between the shafts, and therefore the percentage of error would be smaller. It was hoped, when that time came, to arrive at some useful data. The method given on p. 343 for arriving at the correct shape of a surge tank of minimum volume for dead-beat operation was interesting Such a tank, however, was of limited application in practice, since it required the water-level in the reservoir to be more or less constant. Moreover, as the Authors indicated in the last paragraph of the Paper, the cost of that type of tank would be high. Some years ago, as the results of investigations carried out by Mr. R. S. Cole, Assoc. M. Inst. C.E., who had submitted a Paper on them,[1] he had come to the conclusion that a chamber having the shape of a trumpet was ideal under certain limited conditions. Where there was considerable variation in the level of the water that shape was impracticable, and the form frequently adopted was a cylindrical shaft having an enlargement at the top, usually with provision for overflow. It had occurred to him that should the Authors intend to continue their investigations on experimental models, it would be very helpful if they would investigate the conditions arising in a surge tank built on the lines he had indicated. Many installations had been built with comparatively small vertical shafts, with a large chamber at or near the top, and for practical reasons they formed a very usual type. Mr. Halcrow.

Mr. E. BATCHELOR remarked that he was interested in the matter mainly from the theoretical point of view, not being a professional engineer; and it seemed to him somewhat surprising that there should be such difficulty in dealing with what was essentially one of the simplest of hydraulic devices. A siphon was little more than Mr. Batchelor.

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[1] "The Surge-Chamber in Hydro-Electric Installations." Inst. C.E. Selected Engineering Paper No. 55 (1927).

Mr. Batchelor. a cylindrical tube with a portion of its length bent in the form of a semicircle, and with inlet and outlet fashioned with a view to avoid eddies and secure the ingress and egress of the water in parallel vertical filaments. No doubt conditions at the actual site, and also considerations of cost, necessitated modifications of that simple design; but it did not appear to him that the designs he had seen, and the structures depicted in the various Papers referred to by Mr. Davies, introduced modifications which necessitated any departure from the laws of flow ordinarily adopted. Still, it was obvious from Mr. Davies's Paper that there was considerable difference of opinion as to certain conditions, e.g., the measurement of head; and he suggested that it would enhance the value of experimental work if observations were made under agreed similar conditions and conventions as to head, etc. He thought that models should in the beginning be made with the smoothest possible surfaces, in order to reduce friction and eddy losses to the minimum. They should also be at first of the simplest type, and from those models the experiments should proceed to models of actual installations. Further, this comparatively new branch of engineering would gain considerably by the adoption of agreed definitions of the special terms of which it stood in need. In 1927–28 he had occasion to visit the Central Provinces in India, and from such information as he had been able to obtain it was clear that the behaviour of the siphon at the Maramsilli reservoir, constructed in a very remote part of the Central Provinces, had been awaited with considerable uncertainty. Mr. Davies might justifiably be proud of the great success which had attended works constructed on so large a scale and under such great difficulties, and of the fact that he was the pioneer, both in the design and in the execution, of such works in so remote a part of the British Empire.

Mr. Williamson. Mr. JAMES WILLIAMSON remarked that, in a contribution [1] to the Correspondence on the Paper on siphons by Messrs. Gibson, Aspey and Tattersall, he had ventured to suggest that the problem had by no means been solved by the experiments described therein. He had not been inclined to agree with some of the conclusions, and he had expressed the hope that that Paper, and the discussion on it, would induce further efforts towards the solution and understanding of the problem. Mr. Davies's Paper went some way towards clearing up some of the obscure points. One point which still persisted in that Paper was the apparent contradiction of the usual laws of energy in the flow of pipes, so far as the nature of the flow round the bend in a siphon was concerned. It was conclusively proved that there

[1] Minutes of Proceedings Inst. C.E., vol. 231, p. 275.

was something approaching free-vortex flow, that was to say, the flow Mr. Williamson. round the bend was nothing like uniform. The velocity was lower at the outside of the bend than at the inside. The lower velocity at one part involved a higher velocity at another, and as the energy of the water was proportional to the square of the velocity, that meant that the total kinetic energy of the water was really being increased. That fact would completely explain the apparent departure from the laws of flow. Considering two square pipes, each 1 square foot in cross section, one carrying water at 5 feet per second and the other at 15 feet per second, the quantity of water passing through the two was obviously equal to that flowing uniformly through a pipe 2 feet by 1 foot at 10 feet per second. In the one pipe 5 cubic feet flowed per second, and in the other 15 cubic feet, and the energy per second in the two cases was proportional to $5 \times (5)^2$ and $15 \times (15)^2$, that was 125 and 3,375, the total energy being 3,500. But the energy in the case of the pipe of 2 square feet section, passing the same total quantity of water with a velocity of 10 feet per second, would be proportional to $20 \times (10)^2 = 2{,}000$. That was to say, the kinetic energy was increased by 75 per cent., owing to the variation in velocity of the two portions of the total flow. Applying this to one of the points dealt with in the Paper, at section DD, Model A, the conditions of which were represented by *Fig. 1*, as the velocity was inversely proportional to the radius, the velocity at the inside of the bend was approximately three times that at the outside of the bend. Integrating the kinetic energy across the width, it would be found that it was about 28 per cent. in excess of what would be arrived at by taking the mean velocity of flow. As the radius of the bend became easier the difference diminished. With radii in the ratio of 2 to 3, which occurred in one of Mr. Davies's examples, the excess kinetic energy was only 8 per cent. *Fig. 6* (p. 369) represented the apparent losses from eddies as calculated by Mr. Davies. It might safely be said that any losses from eddies were absolute losses, and the energy so lost could not be recovered. Point D in *Fig. 6* was at the end of the upper bend in *Fig. 1*, point C was at the crown, and point B was at the commencement of the bend. Assuming that the water entered the upper bend with the centre of the energy in the middle, as vortex conditions developed there would be concentration of flow towards the inside, and the centre of the energy would probably be at about 0·7 of the depth from the top. It would be appreciated that the stream of energy would change gradually from one line to the other, and no doubt the water made its own transition. The figures given in the Paper seemed to indicate that on the downstream portion of the

Mr. Williamson. upper bend the main stream of water tended to leave the inner curve before point D, overshoot the centre line slightly, and gradually

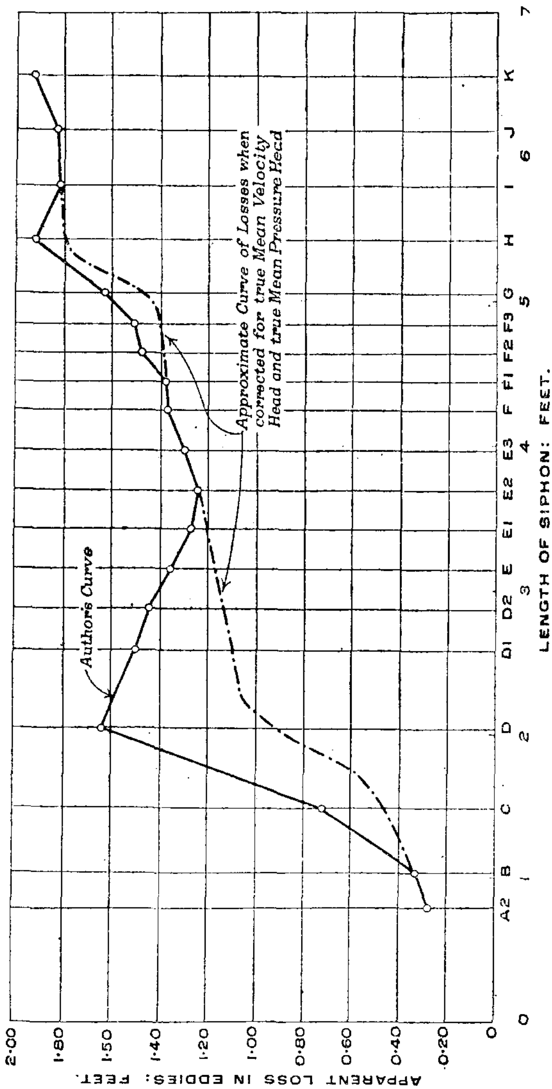
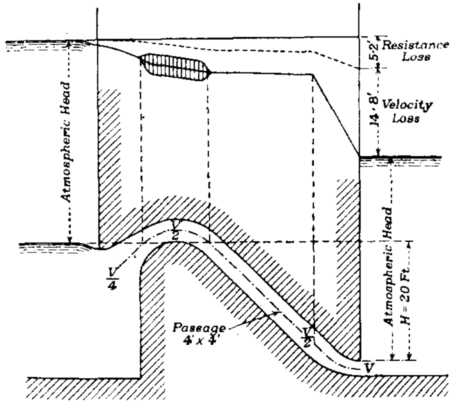


Fig. 1.

get back to a condition of uniform flow at a point a considerable distance down the straight. The velocity head as given by Mr. Davies in his calculations was about $2\frac{1}{2}$ feet of head, calculated on the mean velocity. To get the actual velocity head something like 28 per cent., or 0·7 foot, had to be allowed for. Looking at *Fig. 6*, 0·7 foot deducted at point D, test A, would get rid of all the apparent excess, and having applied this for several points he had found that Mr. Williamson.

Fig. 2.



Available up to 70 Ft. Head

SIPHON TYPE A.—TABLE OF LOSSES.

Part.	Velocity.	Coeff.	Loss of head.
Outlet	V	1·00	$1{\cdot}00\dfrac{V^2}{2g} = 14{\cdot}8$ ft.
Lower bend . . .	Mean $0{\cdot}75V$	$0{\cdot}3 \times 0{\cdot}75^2$	0·17 ,, 2·5 ,,
Upper bend . . .	$0{\cdot}50V$	$0{\cdot}4 \times 0{\cdot}50^2$	0·10 ,, 1·5 ,,
Passage	Mean $0{\cdot}50V$	$0{\cdot}3 \times 0{\cdot}50^2$	0·08 ,, 1·2 ,,
		Total . .	$1{\cdot}35\dfrac{V^2}{2g} = 20{\cdot}0$,,

$$V = 8\sqrt{14{\cdot}8} = 30{\cdot}6. \qquad Q = 16\frac{V}{2} = 245 \text{ cusecs.}$$

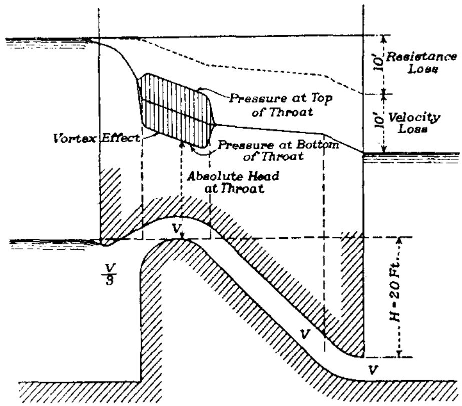
$$V \text{ for 20 ft. head} = 35{\cdot}7 \text{ ft. per sec.}$$

$$\text{Coefficient of discharge } \frac{15{\cdot}3}{35{\cdot}7} = 43 \text{ per cent.}$$

the curve for losses, taking account also of the true mean variation of pressures, would be like *Fig. 1* (p. 402). That showed that the apparent excess loss which persisted round part of the bend and at and beyond point D was balanced by excess kinetic energy which was not lost but was regained as increased pressure head in the pipe

Mr. Williamson. below. It afforded also an explanation of the inconsistencies in the factors which were given in the text-books for the losses of head in bends, as the evaluation must be made at a point a considerable distance beyond the bend. In his remarks on the previous Paper he had mentioned the question of the significance

Fig. 3.



Available up to 30 Ft. Head

Siphon Type B.—Table of Losses.

Part.	Velocity.	Coeff.	Loss of head.
Outlet	V	1·00	[$1{\cdot}00\frac{V^2}{2g} = 10{\cdot}0$ ft.]
Lower bend . . .	V	0·3	0·30 ,, 3·0 ,,
Upper bend . . .	V	0·4	0·40 ,, 4·0 ,,
Passage	V	0·3	0·30 ,, 3·0 ,,
		Total . .	$2{\cdot}00\frac{V^2}{2g} = 20{\cdot}0$,,

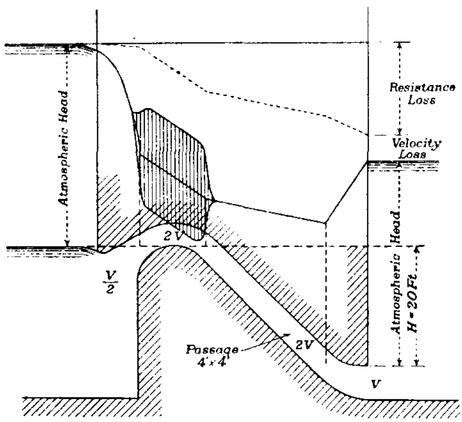
$V = 8\sqrt{10} = 25{\cdot}2.$ $Q = 16 \times V = 403$ cusecs.

Coefficient of discharge $\frac{V}{8\sqrt{20}} = \frac{25{\cdot}2}{35{\cdot}7} = 70$ per cent.

of the term "efficiency," and had suggested that it had little to do with the engineering efficiency of the apparatus. He had also suggested that it was a little dangerous to apply the results from any model to full-scale work unless it was known exactly what the conditions were at the points of lowest pressure. The efforts of Professor Gibson had been directed towards obtaining a very efficient type of outlet based on theoretical calculation of the efficiency of flow. *Figs. 2–5* illustrated a method of carrying out the investigation in a manner which gave the atmospheric head its due value in the siphon problem. The atmospheric head was really equivalent to the addition, at sea-level, of 33 feet head of water to the reservoir

Mr. Williamson.

Fig. 4.



Available up to 17 Ft. Head

SIPHON TYPE C.—TABLE OF LOSSES.

Part.	Velocity.	Coeff.	Loss of head.
Outlet	V	1·00	$1{\cdot}00\frac{V^2}{2g} = 4{\cdot}5$ ft.
Lower bend . . .	Mean $1{\cdot}5V$	$0{\cdot}3 \times 1{\cdot}5^2$	0·68 ,, 3·0 ,,
Upper bend . . .	$2{\cdot}0V$	$0{\cdot}4 \times 2{\cdot}0^2$	1·60 ,, 7·1 ,,
Passage	Mean $2{\cdot}0V$	$0{\cdot}3 \times 2{\cdot}0^2$	1·20 ,, 5·4 ,,
		Total . .	$4{\cdot}48\frac{V^2}{2g} = 20{\cdot}0$,,

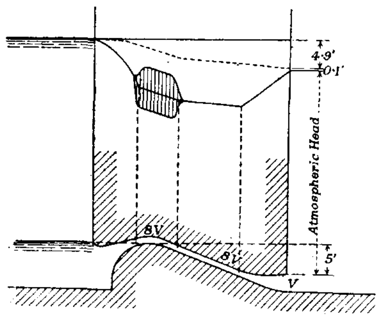
$V = 8\sqrt{4{\cdot}5} = 17{\cdot}2$ $\qquad Q = 16 \times 2V = 550$ cusecs.

Coefficient of discharge $\frac{2V}{8\sqrt{20}} = \frac{34{\cdot}4}{35{\cdot}7} = 96$ per cent.

on the upstream side and to the tail water on the downstream side. If for the particular elevation at which the siphon was to be used a head of water was plotted above the reservoir-level corresponding to the atmospheric head, and from that upper line the resistance loss was plotted downwards, and if there was added to that the

Mr. Williamson. velocity head lost at the various places, a line would be obtained which apart from vortex effects would, by its closeness to the outline or upper surface of the siphon, give a measure of the approach to vacuum conditions. The first case (*Fig. 2*) was the type of siphon with a contracted outlet, which with suitable contraction could be used up to almost any head. The resistance loss was small in relation to the total, and the velocity loss, except at the outlet,

Fig. 5.



Available up to 10 Ft. Head

MODEL SIPHON TYPE D.—TABLE OF LOSSES.

Part.	Velocity.	Coeff.	Loss of head.
Outlet	V	1·00	$1{\cdot}00\frac{V^2}{2g} = 0{\cdot}1$ ft.
Lower bend . . .	$4{\cdot}5V$	0·3	6·0 ,, 0·7 ,,
Upper bend . . .	$8V$	0·4	25·6 ,, 2·8 ,,
Passage	$8V$	0·2	12·8 ,, 1·4 ,,
		Total . .	$45{\cdot}4\frac{V^2}{2g} = 5{\cdot}0$,,

$V = 8\sqrt{0{\cdot}11} = 2{\cdot}64$. Coefficient of discharge $\frac{8V}{8\sqrt{5}} = \frac{21{\cdot}7}{17{\cdot}8} = 122$ per cent.

was also small. The diagram had been worked out for 20 feet head, and it was not until about 70 feet total fall was reached that the loss of velocity head at the upper bend gave rise to an approach to vacuum conditions. The shaded area on the diagram was meant to represent the effect of centrifugal forces produced by the water passing round the bend. This should be distributed half above and half below the velocity line. The effect of the centrifugal force was to give an increase of pressure on the outer surface and a decrease of pressure on the inner surface at the bend. *Fig. 3* **Mr. Williamson.** showed a siphon with a parallel outlet, which could be used up to about 30 feet head. In the case of a siphon with an expanded outlet (*Fig. 4*), starting from the same available head of 20 feet, it was found that the total reduction of head due to resistance plus velocity came much closer to the crown, and if the vortex variations were included it would be seen that something less than 20 feet was the limit to which that type of siphon could be used. *Fig. 5* was a siphon of the type with a large outlet expansion tested by Professor Gibson, except that for the purpose of making a diagram it was expanded horizontally. This case had been taken for 5 feet head only. It would be seen that the pressure line came nearly half-way down towards the top of the siphon, and a head of 10 feet would therefore be nearly the limit for reliable working. Thus the range of application for a siphon with a widely expanded outlet was extremely small, and the model type tested by Professor Gibson could not be applied for any large head. A head of 8 or 9 feet would practically be the maximum, and if the siphon were at a considerable altitude the head would be even less. With a more moderate expansion, say, from unit velocity at the outlet to twice as much in the throat, there was a possibility of getting up to 16 or 17 feet of head. In that case, with the vortex pressure distributed about the lower line, it would be seen that with 20 feet head there would be an absolute vacuum at the upper bend. In his opinion there was little left to be determined in the design of siphons. He would like to see some more detailed investigation of the resistance of bends, particularly with reference to the conditions at the end, and the redistribution of flow and regain of head beyond the bend.

With regard to the Paper by Professor Gibson and Mr. Cowen, the question of dead-beat surge tanks was of more academic than practical interest. He had made a comparison, in relation to one or two recently-designed surge tanks, of the minimum size for dead-beat operation, as compared with what had actually been provided. In one case, for a conduit 4,000 feet long and 20 feet in diameter, a tank 100 feet in diameter standing above the ground had been provided. Calculating from the dead-beat formula, he had found that to get dead-beat operation the tank would require to be 480 feet in diameter, and the cost of such a tank of the necessary height would be between £250,000 and £500,000. In another case, with a higher head, he had found that for dead-beat operation the diameter would have to be about 90 feet according to the formula, as compared with the actual size of 24 feet. Therefore purely on the score of cost, in cases such as he had mentioned, dead-beat operation would be quite out of the question. Dead-beat

Mr. Williamson. operation simply meant that with a sufficiently large tank there was no second surge. On the question of the general shape of a surge tank as affecting economy, his experience had been that in deciding on the type of tank to be used on a pressure tunnel the location of the tunnel and the arrangement of the surge tank were correlated. There was a lower limit to the size of the vertical shaft of the tank, determined by turbine-governing conditions, and what was looked for was a site on a hill-side at the proper elevation which would enable the storage-capacity required against upward surge to be provided at very little cost by using some of the excavated material to form an embanked pond. In one case a vertical surge shaft had been excavated in the rock, with a connection to the upper side of the tunnel, and the pipe-line was led down to the power-station on a head of about 400 feet. There was 40 feet of fluctuation in the storage reservoir to be dealt with first of all, and large upward and downward surges in addition. The most economical arrangement, so far as quantity of work was concerned, was to keep the diameter of the tank to the minimum necessary for good governing, and throw the surface storage as high as possible. If it were necessary to get extra storage accommodation for water to deal with the downward surge, it should be done by excavating a separate chamber down below, the reason being that to stop the flow of water in the case of an upward surge due to a shut-down, or to start it in the case of a downward surge produced by throwing on load, the final result would be most economically reached if the rise or fall were quite rapid, as it would be in a narrow shaft, until the place was reached where there was a large amount of storage, i.e., the upper storage-pond or lower storage-chamber. From Thoma's formula, which was generally used to determine the minimum diameter for stable governing ($F_m = AL/2ghc$, where F_m was the minimum area of surge-tank cross section, A the cross-sectional area of the conduit, L its length, and h the minimum head on the machine), it might be thought at first sight that the size was proportional to L. It was, however, independent of L, because the factor c (friction characteristic of the conduit) was proportional to the length of the conduit and already contained the term L. The only material factors therefore were the conduit cross section, the minimum head, and the friction coefficient. The lower the friction coefficient, i.e., the smoother the surface of the conduit, the larger would be the tank-diameter required for stability.[1]

Professor Gibson. Professor GIBSON, in reply, remarked that apparently some of Mr. Halcrow's experimental results had not coincided so well with

[1] $c = v^2 f$, where f is the total frictional loss of head in length L, or $= L \times$ friction coefficient.—J.W.

calculations when the load was thrown on as when the load was thrown off. He and his colleague had obtained similar results, which they had always attributed to the fact that while it was easy to close almost instantaneously the needle valve which regulated the outflow, it was not so easy to represent the exact programme of operations of throwing on a load. Supposing it were desired to open it uniformly in, say, 2 seconds, to represent a load being thrown on uniformly in the corresponding period, it was not easy to do that in the model with the same degree of accuracy. With regard to the calculated period of oscillation being usually less than the observed period, he thought that was due to the fact (referred to in the Paper) that the actual length of column which was oscillating was the length of the pipe-line plus the length of the column in the surge tank itself, and, owing to the vertical exaggeration of scale, the length of the latter column in the model was relatively greater than in an actual installation ; so that it was to be expected that the time in the case of the model would be slightly greater than that in the full-size installation, or as given by calculation. They had calculated that in their own model the difference was of the order of 3 per cent. He agreed that the dead-beat surge tank was rather an academic question, and could only be justified in an exceptional installation. However, they had thought it worth investigation, having a model available. He was at a loss to understand the implication of the formula attributed by Mr. Williamson to Professor Thoma. Applying it, for example, to the case of the Tallulah Falls installation, referred to in the Paper (p. 328), the formula made the necessary surge-tank area 144 square feet, which was actually less than the area of the conduit, whereas the actual area of the surge tank installed was 2,130 square feet ; and even with that area surges of 32·3 feet had been experienced in the field tests. As the magnitude of the surge increased rapidly with a diminution in its area, it was evident that with a reduction from 2,130 square feet to 144 square feet the surge would be very great. In the case of the Walchensee development, also referred to in the Paper (p. 330), the calculated area would be 252 square feet, whereas the actual area was 5,020 square feet, and with this area surges of more than 12 feet were experienced. Professor Gibson.

*** Mr. Davies's reply will be found at p. 483.—Sec. Inst. C.E.