14. G. M. Binnie, "Model-Experiments on Bellmouth and Siphon-Bellmouth Overflow Spillways." J. Instn Civ. Engrs, vol 10, p. 65 (Nov. 1938).
15. Sir Claude Inglis, "The Behaviour and Control of Rivers and Canals (with the aid of Models)." Research Publication No. 13, Parts I and II, Government of India, Central Waterpower, Irrigation and Navigation Research Station, Poona, 1949.
16. James Williamson, "Round-Crest, Syphon and Trumpet Shaft Spillways." Commission Internationale des Grands Barrages de la Conférence Mondiale de l'Energie; Quatrième Congrès, New Delhi, 1951; Question No. 12.
17. N. S. Govinda Rao, "Design of Volute Siphon." Commission Internationale des Grands Barrages de la Conférence Mondiale de l'Energie; Quatrième Congrès, New Delhi, 1951; Question No. 12.
18. A. H. Naylor, "Siphon Spillways." Arnold, London, 1935.

The Paper is accompanied by five photographs and eighteen sheets of drawings, from which the half-tone page plates and the Figures in the text have been prepared.

Discussion

Mr Roberts introduced his Paper with the aid of a series of lantern slides.

Professor Allen, introducing the second Paper on behalf of his colleagues and himself, explained that since writing their Paper they had reconstructed the 1 : 12-scale model of the 21-inch siphon almost entirely in Perspex and had had a ciné film taken to illustrate the priming of the siphon and the behaviour of the vortex chamber, as well as the spouting of a mixture of water and air from the top of the air-shaft connected to the crown of the vortex chamber, during priming and de-priming of the siphon. The film was then shown.

The President, in moving a vote of thanks to the Authors, remarked that the two Papers had provided a record of their work which would prove of great interest and use to the Institution. In the Annual Report published a few days previously by the North of Scotland Hydro-Electric Board, reference was made to the inauguration of the Glen Affric hydro-electric scheme by H.R.H. The Duke of Edinburgh on the 13th October, 1952, and a very good idea was given of the importance of the work which was taking place in Scotland, in which connexion the President would like to quote a few figures.

In 1949, the kilowatt capacity of the Board's hydro-electric stations was 87,000 kilowatts and that had been increased to 391,000 kilowatts at the present date. 322 million units had been generated in 1949 and that figure had now risen to 889 million units. A very important matter at the present time was the saving of coal by the installation of hydro-electric plants, estimated at 600,000 tons per year.

The Report also gave some indication of the tunnelling work which had been undertaken; during 1952, 11 miles of tunnel, or nearly one mile a month, had been driven in connexion with those schemes.

There was one other quotation which he would like to make from the Report, namely, that there was plant under construction capable of generating 287,000 kilowatts as compared with that already installed, which had an output of 391,000 kilowatts, and further schemes were projected to install plant capable of generating a further 416,000 kilowatts. From those figures it was evident that there would be a considerable amount of work for engineers in Scotland in connexion with hydro-electric schemes for some years to come.

Mr A. A. Fulton paid tribute to the excellent work which had been done by the engineers, Sir William Halcrow & Partners, and particularly to Mr Roberts and all his engineers for having brought the Affric scheme to such a successful conclusion. Mr Fulton also paid a tribute to Professor Allen for the help which he had given and for the siphon analysis which he had made. That had been of great help to the engineers in dealing with the problem described in the second Paper. Mention should also be made of the contractors, Messrs John Cochrane & Sons, who had carried out an excellent piece of work and who were now clearing the site, and also of the architect, Mr James Shearer, who had given a great deal of personal attention to obtain the result which had been sought, namely to satisfy the amenities and produce a scheme of which all connected with it could be proud.

Mr Fulton observed that it was gratifying to him personally that Mr Roberts had so quickly remedied the short-comings of his own Paper [19] the previous year, when he had been able to make only a very short reference to the Glen Affric scheme. It was a good thing to have had the additional information so soon.

The two Papers presented were complementary, for they illustrated the kind of co-operation which was desirable, and how far the experimental man could help the practical man. The features selected by Mr Roberts for his Paper indicated the limitations under which a great deal of the work had had to be accomplished. Those limitations arose from the need to respect amenity, the need to restrict capital expenditure, the importance of saving scarce materials, and the employment of greater mechanization. It had been recognized at an early stage that regard would have to be paid to amenity, or the scheme would never be sanctioned. For that reason it had been decided that the level of Loch Affric should not be interfered with. That meant that the impounding of the lower loch in the glen, Loch Benevean, should be restricted, by taking 30 feet of so off the height of its dam and putting it on the dam in the adjacent Glen Cannich. That was how Mullardoch became a major dam, and the one in Glen Affric

[19] A. A. Fulton, "Civil Engineering Aspects of Hydro-Electric Development in Scotland." Proc. Instn Civ. Engrs, Part I, Vol. 1, p. 248 (May 1952).

became less substantial than in previous promotions. It was the second thoughts about the decision, in the interests of lower capital expenditure, not to put the Mullardoch dam to its full height which had given Mr Roberts the opportunity of describing his method of heightening the southern half of that dam.

The same amenity considerations had determined that there should be no pipelines. Some people wondered why, if it were possible to do without pipelines at Affric, it was not possible to do without them everywhere. At Affric, however, the geology had been suitable and the tunnel of sufficient size to make it a practical proposition. In addition, the saving in steel had been quite substantial at a time when steel was particularly scarce. The question of what scarce materials should be saved had had to be carefully considered throughout. If steel were saved it had usually to be done at the expense of cement. By making Fasnakyle a pressure tunnel, steel had been saved, but the amount of cement used on the scheme was increased because the pressure tunnel had had to be concrete-lined. On the other hand, if Mr Roberts had chosen to use post-stressed steel rods to enable him to use the same section of dam for the higher head, he would have saved cement but used more steel. Those were the conflicting interests which had had to be faced all the time, and, depending on circumstances, changed in relative importance from time to time; it was now understood that steel was to be no longer scarce!

The post-war restrictions on capital expenditure had affected everyone and had always called for some sacrifice. If the proposal to reduce the top water level at Mullardoch had been adhered to, it would have meant doing without very valuable storage destined to save more of the coal to which the President had referred. Moreover, to see a contractor taking away all his plant with the idea that he might some day come back and do the work was quite unthinkable.

Then there was the effect of full employment. That had led contractors and everyone else to go in for more mechanization. Good examples of that had been seen in tunnelling. It had meant that contractors had turned to the use of lighter drilling rigs and to the use of special types of mechanical loaders to get rid of their muck; they were now using pneumatic concrete-placers to line tunnels. Mr Roberts had allowed the use of a pneumatic placer only on the roof, but Mr Fulton did not think that any contractor would be satisfied until he could place concrete all round the lining at one and the same time and with a pneumatic placer. So far the results with such placing had been somewhat mixed, and very patient experimentation would be needed to achieve the result desired—no cold joints and no segregation.

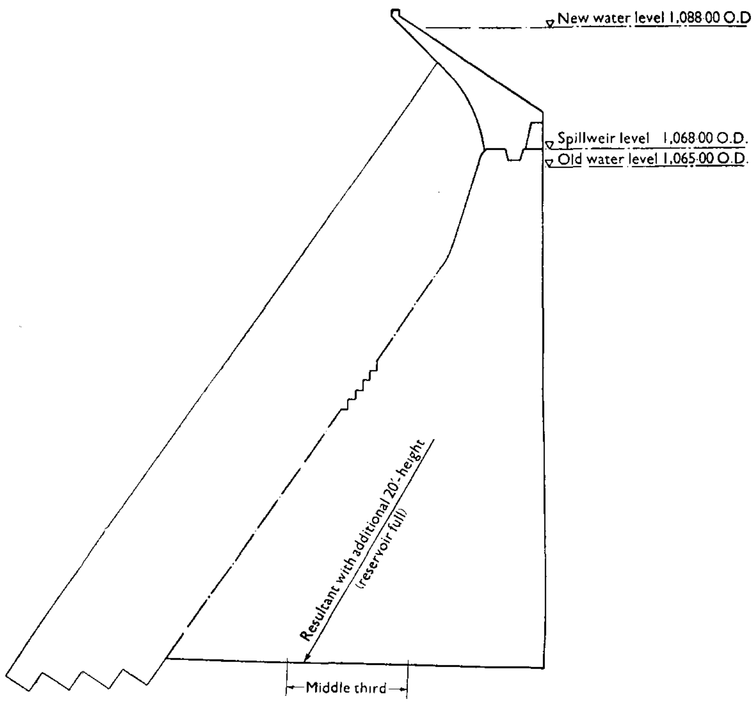
The development of "Colcrete" as a means of running grout into the voids of aggregate was, from the experience gained at Mullardoch, worth pursuing. The French were interested in it too. If it were successful, concrete-mixers would not need to handle anything like the same amount of material, and certainly not the bulkiest and hardest materials, all of which at the present time had to go through them.

Professor Allen had explained why it was necessary to keep air from going down into the tunnel. It was primarily to prevent air-entrainment and to ensure that the full value of the water was obtained. If air as well as water were put through the turbine, then the full weight of the water would not be available; the presence of air might also give rise to cavitation in the turbine. For those reasons, the complicated siphon arrangement involving small pipes and bends had been accepted. In some cases where water had been dropped straight down a shaft into a tunnel, the water had re-surged back because of an air-lock pushing the water back up the shaft, as had been illustrated in the film. For that reason, also, the system which had been adopted was of some advantage. To Mr Fulton, as an engineer and a Scot, it seemed a pity to have to destroy the energy once it got to the bottom. There was a nozzle there to dissipate the energy, which in one instance was equivalent to 40 kilowatts, and some means should be found to make use of that energy. Perhaps Professor Allen and his collaborators might suggest a solution whereby the water, when it reached the bottom, could be passed through a small turbine.

Mr H. D. Morgan said that he had been very glad to hear Mr Roberts refer to *Figs 20* (p. 546), because the diagrams were a little startling; but, as Mr Roberts had said, they were intended only to illustrate the extreme stresses which would be realized if a number of favourable factors were left out. It would be quite impossible in reality to have such abrupt changes in intensity of stress as were shown in those diagrams, and it was certain that the maximum stresses would be less than those shown. The diagrams were also, of course, based on the assumption of linear distribution, which was not correct.

The problem of raising the crest of large dams was of very great interest, and was a problem which had been facing engineers for a long time. Gravity dams had been raised, but always with great difficulty, although the solution looked very easy to a layman. Mr Morgan's firm had once been asked to consider the question of raising the crest of a very old dam which had been penetrated by a large number of cracks from which water was seeping. A scheme had been evolved for the purpose, which entailed putting a reinforced-concrete slab on the top and supporting it with buttresses (see *Fig. 1*, p. 588). On a statical analysis, that had been found to be all right, and water coming out from a number of points along the back of the old dam could drain away readily, whereas if a complete sheet of material had been put on the back there would have been a build-up of pressure on the contact plane. The cost, however, had been rather high, and the clients had decided that it would not be worth their while, so that nothing had come of it. An analysis had been made of the water seeping through; the quantity of lime in the water had been analysed by chemists and a calculation made based upon the velocity of flow and the amount of

Fig. 1



material carried. The result showed that about one-quarter of the dam ought to have gone already, so that there was evidently something wrong with the assumptions made.

Referring to the Paper on experiments on high-head siphons and vortex chambers, Mr Morgan said that he had become interested in that problem in the early 1930s when the Laggan–Treig works were being designed—the second stage of the Lochaber scheme. The main tunnel at Lochaber was then in operation and there had been some difficulty caused by entrained air. Some of the shafts, and particularly the No. 1 shaft, were very deep, and he thought that it was a matter not of 40 kilowatts but of something like 1,000 horse power which was dissipated at the bottom by the potential energy of the falling water. A number of schemes had been worked out for putting things right, but it was very difficult to do anything once the tunnel had been built and was in operation, and nothing had actually been done; but it had concentrated his mind on the problem. That had led him to suggest to Mr Roberts that a vortex chamber might be tried to look after his siphons. He appreciated the acknowledgement by Professor Allen and his co-Authors that the practising engineer had done the original thinking in that case. It had been anticipated that a vortex chamber situated as shown in *Figs 2* (p. 588) and level with the tunnel, might not turn out to be practicable and that it might be essential to have the outlet for the water in the bottom. It should be remembered, however, that the practising engineer was not making models in perspex and copper which could be erected in a laboratory in any manner he chose; his work had to be done underground. If the vortex chambers could have been placed as shown in *Figs 2* it would have been of great advantage and less costly. In fact it had been necessary to stope up and place the chambers above the tunnel.

The analysis of the flow of the siphon was very interesting and had seemed to strike a chord in his memory. He had looked up a file of old calculations and found that he had done the same thing for the siphon spillways on the Laggan Dam: the date of those calculations was 20th March, 1933—just 20 years ago. On that occasion there had been a large model of the Laggan siphon spillway at the City and Guilds Engineering College, and he had asked Professor Purser to measure the velocities and pressures at the throat to see whether the assumption of a free vortex was valid. The agreement had not been good; the velocity at the inner radius had been lower than it should have been for a free vortex, and also that at the outer radius did not correspond. He had thought at the time that the reason must be that the streamlines which approached the siphon hood had too short a distance in which to become modified to true vortex motion at the throat; but Professor White, to whom he had mentioned the matter recently, told him that in experiments which he was making on the scroll casing of turbines one did get a close approximation to true vortex motion and could measure velocities within 1 per cent of what they should be theoretically. That, however, was a case in which the flow was convergent right through, and that might have something to do with it. In the case in question, Mr Morgan thought that it was very probable that there was just not time in passing from the entrance to the throat for vortex motion to be set up. If the throat of the siphon were arranged as a corkscrew instead of a single turn, then that sort of flow might be achieved by the time it had been traversed. That was a point, however, of mere academic interest. In practice it was an advantage not to have too great a velocity at the inner surface of the throat bend, since an absolute pressure must be limited to approximately 10 feet absolute.

Mr G. Ford observed that the Paper by Mr Roberts fulfilled in a remarkable way the requirements of good history writing. It was mainly a reasoned account of why certain designs had been adopted and why certain operations had been judged necessary. In many ways that was more useful to the engineer than a bare recital of what had been done.

From the contracting point of view, the job had been of great interest, because it afforded many opportunities for experimenting with novel plant and a new process of construction for a novel set of conditions. The use of pneumatic concrete-placers and of Colcrete grouting for mass concrete construction had had to be tried out since very little experience of them was then available in Great Britain.

The concrete-placers were of 1 cubic yard capacity with air supplied at 100 lb. per square inch, and for speedy and efficient working about 1,500 cubic feet of air per minute had been required for each machine. The speed of placing concrete with an experienced gang had been determined by the speed of feeding the mix into the placer. A few seconds had been sufficient to "blow" the concrete into a shutter, and it had become possible to place the concrete at 60 cubic yards per hour for short periods. In the low-pressure tunnel, when the gang became familiar with the plant, an 85-foot shutter had been filled at an average speed of 30 cubic yards per hour. It was found that the best results with the concrete lining were always obtained where the concrete had been placed at maximum speed. In the low-pressure tunnel, the walls and roof had been filled with the placer, but in the high-pressure tunnel the side walls had been filled by hand and the roof with the placer. Because of that, the average speed of lining the high-pressure tunnel had been only about 50 per cent of the rate of lining the low-pressure tunnel, which substantiated Mr Fulton's remark that it would be an advantage to persevere with that type of mechanized placing.

Mr Roberts had stated, when dealing with grouting, that it had been considered necessary to add a preliminary stage of grouting to fill the cavity between the rock and the concrete at the crown of the arch, and had also stated that the cavity required five times as much filling as the fissures in the surrounding rock. Mr Ford disagreed with that. In his opinion, keeping in mind that the concrete had been blown into the arch at 100 lb. per square inch pressure, voids in the roof had been practically negligible, and what was called cavity grouting had been in reality, in his view, the first stage of pressure grouting. He suggested that for future work, cement-mortar filling of the grout holes would be as efficient and effective as, and more economical than, screw plugs.

On p. 529 of the Paper it was stated that the level of the tunnel invert had been amended from the original design to give it a gradient of 1 in 50 upwards to the high-pressure shaft. That amendment had been made so that the depth of the high-pressure shaft could be reduced by about 100 feet. In practice it had been found that the gradient was too steep, and that 1 in 100 was much nearer the ideal for practical purposes.

An interesting feature regarding the design had been the readiness to change the type of design for the tunnel lining. It would be interesting to know whether that was wholly due to technical conditions or whether the reasons had been partly economic, because the tender prices had been less for type A than for type B linings. When departures of that kind to any major extent were made, there was always a possibility that a recovery by the contractor of adequate overhead charges might be prevented. In such cases it might be proper to schedule such charges separately and make any choice between alternatives depend on the basic prices of labour and materials.

With regard to the use of Colcrete on the Mullardoch dam, Mr Ford would add that the time taken from the mixing of the grout to the actual placing in position in the slot had been about 12 minutes. The width of the slot had been decided on the minimum working width for a man to fix shutters and place the concrete.

On p. 552, details were given of the tilt of the Mullardoch dam. Mr Ford believed that in the case of the Benevean Dam, the tilt had been found to be upstream when the reservoir was filled. Would Mr Roberts comment on that?

Mr G. R. Hoffman referring to the Paper by Mr Roberts, said that attention had been drawn to the fact that one of the advantages of the high-pressure tunnel design adopted had been that it saved a considerable amount of steel. If that were assumed to be the most important consideration, there seemed to be three ways in which the quantity of steel required could be very much reduced below the figure given in the Paper. Mr Hoffman had no doubt that those points had been fully examined, but thought it would be helpful from the point of view of future design if Mr Roberts would say why they had not in the end been adopted. It was possible that the danger of collapse from external pressure, with the pipe empty, had been the ruling criterion, but that was not mentioned in the Paper.

The first point concerned the factor of safety. On p. 526 was a list of possible methods of design for pressure tunnels in rock; in that list, (*a*) and (*b*) might be described as mathematical analyses, and (*c*) and (*d*) as rule-of-thumb methods. In practice, neither exact analyses nor rule-of-thumb methods gave a positive answer to the question of what loads would actually occur on the lining when constructed, since the conditions assumed were seldom realized in natural rock. Because of that uncertainty, comparatively large safety factors were normally used in the design of steel linings; but if safety margins were allowed in all the design assumptions, the ratio of the pressure at which failure would occur to the design pressure, that was to say, the overall safety factor, might become unnecessarily high.

Allowing for an efficiency of about 79 per cent for the riveted joint, the hoop stress of 10,600 lb. per square inch, mentioned on p. 530, gave a working stress for the steel of 13,500 lb. per square inch. That was the working stress normally taken for penstock pipes above ground, and had a safety factor of about 5 on the ultimate tensile stress. The steel pipe in the case in question, however, was covered by up to 200 feet of rock, and it could be assumed that the effects of failure would be much less serious than if the pipe were above ground.

Further, other safety margins were allowed in the design, so that there seemed to be some justification for increasing the working stress in the steel. For example, Mr Hoffman understood that the lined pressure tunnels of the Oberhasli scheme in the Swiss Alps, under heads of from 1,500 to 2,000 feet, had been designed for a working stress in the steel equal to the ultimate tensile stress. Such an extreme assumption might not be justified in the case under consideration, but a reduction in the safety factor from 5 to 4 might be reasonable, giving a working stress of 16,500 lb. per square inch.

In addition, a thorough grouting programme similar to that specified for the type A tunnel could be carried out. The resistance of the rock could then be counted on to act almost as soon as the pipe began to deflect under load, and the effect of the reduced safety factor would be partly or wholly cancelled. Grouting pressures would, of course, need to be adjusted according to the depth of rock over the tunnel. The usual factor of safety of 5 could apply where the pipe came near the surface of the ground at the power station.

Using a working stress of 16,500 lb. per square inch, therefore, a saving in steel of about 200 tons would be made. If the grouting were effective, it might also be said that as the depth of rock cover increased, the thickness of the lining should be reduced until it reached the minimum required for handling. A further large reduction in the weight of steel might therefore be possible.

The second way in which the quantity of steel might be reduced was by using a welded longitudinal joint instead of the riveted joint. The efficiency of shop welds could be assumed to be 100 per cent, and the consequent increase in the permissible working stress would lead to a saving of 250 tons of steel. However, he assumed from what was said on p. 530 that welded pipes had been rejected because of the long delivery time.

A third possible way of saving steel would be to replace the three 8-foot-4-inch pipes by a single pipe 700 feet long, with the trifurcation point 100 feet from the power house.

To compare those two alternatives, the friction loss might be assumed to be the same in each, and the diameter of single pipe required would then be 12 feet 7 inches. At that diameter, riveted longitudinal joints were impracticable, because the pipe would be too thick. Using a working stress of 13,500 lb. per square inch, therefore, the substitution of a single pipe with welded longitudinal joints would give a saving of about 500 tons of steel.

He appreciated that, with that arrangement, the erection cost per ton would be greater than with the three smaller diameter pipes, and the excavated diameter of one of the small tunnels would have to be increased so that the larger pipe could be drawn into position. Also, the organization of work in the tunnel might be more difficult; but, if economy in steel were the main consideration, then using the higher working stress as well as welded joints and 700 feet of single pipe, the quantity of steel could apparently be reduced by a total of about 650 tons.

Mr C. D. C. Braine said that he proposed to refer only to siphons, but, since there were probably many hydro-electrical engineers present, a comment made by a Continental friend of his who had just returned from America might be of interest. Having seen a job or two in Scotland, his friend commented on how extraordinarily good the workmanship was compared with anything he had seen in America, and how extraordinarily competent the men on the site were compared with anything that he had seen abroad, but added that compared with America there appeared to be no plant on the job at all, and said that he had found one group of men arguing about 25 tons of steel, whereas any American would have said: "Don't argue about that; put in for 50 tons. We can't afford the time to argue." Wages were so high in America that small savings were not considered.

Mr Braine went on to say that he had been designing low-head siphons for many years and that it would probably be of interest to consider the application of some low-head siphon devices to high-head siphons such as those described in the Paper by Professor Allen and his co-Authors. Before doing so, however, he could not help wondering why, if air entrainment had been found objectionable, siphons had been used at all. Near the Ashford works, with which the President was concerned, there was a very deep shaft carrying a very large flow; there had been no trouble with air entrainment and the system worked very smoothly. A series of down-pipes discharging into small water-cushions set in succession below each other at about 18-foot intervals and occupying a space of perhaps 8 feet by 4 feet in a shaft would do all that was required. An arrangement of that type had worked satisfactorily for years discharging large flows down a deep shaft on the West Middlesex main drainage scheme. It had been described in two previous Papers [20, 21] and after 15 years, it was still very satisfactory.

When the outlet of a siphon was deeply submerged, the rate of rise of bubbles in water might be crucial. Experiments carried out by Holroyd and Parker,[22] showed that the upward velocity of small bubbles in a more or less unconfined space was about 1·25 foot per second, as compared with Scouller and Nixon's [23] value of 0·75 foot per second. Both figures might be misleading when applied to siphons, for each had been derived from experiments in connexion with the activated-sludge process of sewage

[20] David M. Watson, "West Middlesex Main Drainage." J. Instn Civ. Engrs, vol. 5, p. 463 (April 1937).

[21] C. B. Townend, "West Middlesex Main Drainage—Ten Years' Operation." J. Instn Civ. Engrs, vol. 27, p. 351 (Feb. 1947).

[22] A. Holroyd and H. B. Parker, "Investigations on the Dynamics of Aeration." J. Inst. Sewage Pur., 1949, Part 3, p. 292. See p. 310.

[23] See reference 2, p. 583.

purification, in which the aim was to use the smallest possible size of bubble. The former seemed to be the safer value to use. The difference between the two might perhaps be explained best by the analogy of a small stone falling through a cylinder of water, when it had been found that the rate of fall was very quickly affected by the diameter of the cylinder used. To get true values, cylinders had to be surprisingly large in relation to the stones concerned; otherwise there might be very serious errors.

Fig. 10 (facing p. 565) showed that the bubbles there were far too large in relation to the surrounding tube for true bubble velocities to be applied. That, no doubt, explained why those bubbles had been swept out when the mean velocity in the tube had been only 0·60 foot per second instead of something like the figure of 1 to 1·25 foot per second given by Holroyd and Parker. Those same bubbles in a tube of 21 inches diameter would probably behave quite differently. There was a probable safeguard there, however, because, judged by the size of bubbles released at the foot of small waterfalls, Mr Braine imagined that many of the bubbles in the prototype would be much larger than those seen in the model.

When dealing with siphons, the outlets of which were deeply submerged, he had successfully used sucking siphons. Those might be quite small, but they evacuated air at an astonishing rate from a large siphon. For instance, one 6 inches by 5 inches at the throat, operating under a head of 1·85 foot, had produced a suction of — 3 feet of water. A high rate of evacuation could therefore be obtained from the use of a small quantity of water.

With a sucking siphon there would be no need for the ring, which was not a happy contraption, and the main siphon would prime very rapidly. The sucking siphon would not, of course, prevent most of the air plug from being carried down into the tunnel. The answer to that was surely that the vertical leg should be expanded, probably including some form of water-cushion, so that the downward velocity in the expanded portion of the leg would be insufficient to carry the bubbles farther downwards. The air released could escape to atmosphere either direct or, perhaps, via a connexion to the sucking siphon. With that arrangement, the nozzle would be located downstream of the air-trap and, if it were built into the side of the tunnel, the energy in the jet could perhaps do a modicum of useful work. He saw no reason why a horizontal air-trap at the level of the tunnel should not be as effective as a vertical trap, but it would almost certainly need to be longer.

Incidentally, it was difficult to believe that it was wise to locate the nozzle at the inlet to the vortex chamber, because the jet must surely turn the chamber into an inferno of turbulence, with so much eddying that it was not surprising that the chambers did not prove successful as de-aerators. Perhaps the Authors would say whether they had ever tried placing the jet downstream of the vortex chambers and, if so, with what result.

It was interesting to speculate on what would happen if a sucking siphon were to be used on a high-head siphon with its long leg on a slope of about 45 degrees instead of vertically. As the leg filled it would do so from the bottom upwards and thus virtually expel the entrapped air into the sucking siphon. Moreover, as the downgoing water would be flowing along the invert of the pipe instead of plunging vertically, air entrainment might be so reduced that possibly the air-trap could be dispensed with. The effect of tilting the siphon leg backwards beneath the inlet might also repay study. If those various devices had already been tried out and discarded, he hoped that some details about them would be given.

With regard to the effect of in-drawn air on the performance of a siphon, some rather crude experiments of his own had shown that, as the volume of air carried increased, the discharge of the siphon decreased steadily until the siphon crest merely acted as a weir. A change in the general scheme had put an end to the experiments, but it had already been confirmed that a siphon, once it had primed, would carry away a surprising quantity of air; thus, by adjusting the quantity of air being admitted, it was possible to exert some control over the discharge. Presumably the same principles could be applied to a high-head siphon fitted with a proper air-removal device, in which case it would be possible to arrange, if desired, for the siphon to operate semi-continuously at a reduced, but more or less steady, flow, thus lessening the general wear and tear on it.

*** **Mr Cyril Parry** observed that before any lining operations commenced in the low-pressure tunnel, it had been pointed out that it would be an unfavourable application of the Pressweld pneumatic placer, on account of the permitted close proximity of rock points to the lining forms which made it impossible to employ the machine correctly.

The usual teething troubles were met on the application of the machine to the first two or three lengths, but from those lengths, which might be regarded as inexperienced results, exhaustive tests were carried out by the cutting of chases around and along the tunnel and the removal of cores for various tests, the chief test being to ascertain whether segregation was taking place or not. When stop-ends were removed from the first application of the form, the hacked work was found to contain a correct distribution of the aggregate.

Following those tests, the completion of the lining of the tunnel proceeded with the Pressweld placer, and it could be concluded that results were satisfactory.

The Contractor was compelled to operate with the discharge end of the placer pipe blowing the concrete into free space, which was the wrong way to employ the machine. However, in spite of that, the general finish throughout was unquestionably good.

The high-pressure tunnel presented an ideal application for the correct use of the machine, because there the engineers had provided a 10-inch

*** This and the following contribution were submitted in writing upon the closure of the oral discussion.—SEC. I.C.E.

clearance between the shutter and rock points, enabling the machine to work with its discharge pipe buried in the previously placed concrete. The rate of discharge of a cubic yard of concrete from the placer into the shutter was rather less than 15 seconds.

Cold joints, mentioned in the Paper, occurred in any form of concrete placing, if the placing was not continuous, and were equally objectionable, regardless of the method of lining. Whether those cold joints were horizontal or inclined could not possibly make any difference. What was required was a homogeneous mass of concrete in the lining of the tunnel and the Pressweld placer, owing to its high rate of placing of concrete, could achieve that.

Mr Parry said that it might be of interest to point out that there was at present no difficulty in delivering concrete into the linings of those tunnels at 60 cubic yards per hour. Average rate of placing over a volume of between 160 and 200 cubic yards was 40 cubic yards per hour, with a surprisingly small number of men. The machine was ideally suited for either intermittent or continuous methods of lining. In the case of intermittent lining, care had to be taken in connexion with the first few batches of concrete delivered, but the attention required was no more than the attention needed with any system of placing concrete. When once there was some volume of concrete in the shutter and particularly when the delivery pipe was embedded, scarcely any attention was required, owing to the terrific amount of work which was put into each batch as it was delivered into the lining of the tunnel.

Two machines were supplied on that particular contract which involved the placing of several thousands of cubic yards of concrete, with no major repairs required on the machines. Indeed, any repairs that were dealt with were of a very minor nature, which paid tribute to the high quality of the American equipment.

Mr E. S. Crump observed that his interest in the Paper by Professor Allen and his colleagues arose from his having been responsible for the design and/or construction of a number of siphons in the Punjab and the Union of South Africa. There he had been mainly concerned with saddle-siphons of the spillway type in which, with an air vent in the hood at about priming level, once the siphon had primed, the air vent took charge and, with a relatively small variation in the upstream surface level, automatically regulated the inflow of air, so that the siphon adjusted itself to accommodate any discharge offered to it. After priming, there was, at every stage of discharge, a steady flow through the siphon of both air and water and, if properly designed, the siphon never attained its full potential capacity, namely, that with " black " water and no air.

The Paper dealt with siphons having a behaviour basically different from that of the spillway type. In those, with the air vent considerably below priming or crest level and air inflow excluded, the siphon, after it had primed, operated at its full black-water capacity, and, that being always greater than any discharge offered, the upstream surface was drawn down to air-vent level when air entered the siphon in sufficient quantity to "break" the water column and completely stop the flow of water through the siphon. The upstream water surface then rose until priming level was attained and the cycle of operation, essentially intermittent, was repeated at intervals depending upon the discharge offered.

An essential condition for the effective priming of either type was that the head and flow over the internal crest or lip must be enough to evacuate air imprisoned in the siphon by ejecting it, in the form of entrained bubbles, through the tail-water seal. With the inlet sealed, a slight evacuation of air caused a relatively large increase of head and flow over the crest weir, and that in turn increased the rate of air-ejection ; so that a regenerative process set in, resulting in the rapid build-up of flow that was a characteristic of all siphons.

The Authors had adduced evidence indicating that entrained bubbles tended to have a standard size giving a rate of rise—in still water—of about 0·70 feet per second, and their experiments had shown that in a vertical limb, when the mean downward velocity attained that value, regenerative priming set in, regardless of the depth of the tail-water seal to be traversed. That important conclusion seemed to imply that in a vertical limb there was little or no tendency for the standard bubbles to coalesce and form larger bubbles which would rise more quickly. What was true for a vertical limb was not necessarily true for a limb inclined to the vertical, and in the latter case it seemed likely that descending bubbles would gradually move over towards the sloping roof, coalesce, and ascend against the flow. If that happened, the longer the limb to be traversed, the greater would be the mean velocity required for effective priming.

With a view to early priming, Mr Crump had normally arranged for a very light initial seal by incorporating a tail-water control-weir in his designs. He had generally found that a discharge-intensity of about one cusec per foot of width sufficed to ensure that air bubbles entrained in the inclined downstream limb were carried forward by the submerged expanding jet, and delivered beyond the seal in sufficient quantity to start regenerative priming. In the case of his 6-foot gulpher-siphon, priming level was precisely 6 inches above crest level. That meant that the full-bore mean velocity was less than one quarter of the competent velocity of 0·70 feet per second suggested by the Authors. A further example was provided by a so-called "mushroom" siphon installed at Howbery Park for maintaining constant level in a header-tank. That siphon was similar to the one shown in *Figs 12* (see p. 565), except that—to render it a self-regulating spillway—the drum carried a ring of air vents a little above spillweir crest level. The downpipe of 14 inches diameter dipped only one inch into lowest tail-water. The siphon became operative at a discharge of less than 0·25 cusec, that being only one-third of the discharge required to give a competent velocity of 0·70 feet per second. An explanation of the advantage gained by low back-pressure was furnished by *Fig. 10* (facing p. 565) which showed that although the velocity was less than competent, bubbles were driven down by the expanding jet to a depth of about $7\frac{1}{2}$ inches below the full-bore surface before being returned to the surface by flotation and the circulation set up by the jet.

He would suggest that air carried into the tunnel from the vortex chambers might never reach the turbines. Long before reaching the surge-shaft, all such air would have risen to the roof of the tunnel along which it would be carried forward in large pockets, which, on reaching the surge shaft would rise to the surface and escape through the slowly moving column of water in the upper portion of the shaft.

It might be of interest to compare the discharges shown in Table 1 with those obtained by applying the principle of critical flow to the circular throat of the two siphons. Those discharges were for a total head T at the throat equal to the radius r of the throat-section. The general criterion for critical flow was :—

$$V = \sqrt{2gh} = \sqrt{g \cdot \frac{A}{w}}$$

For a circular section of radius r, if the surface subtended an angle 2θ where $\theta = 74$ degrees 21·75 minutes he found that :—

The depth of flow : $d = r(1 - \cos\theta)$ was $0{\cdot}7304 \times r$

The sectional area : $A = r^2\left(\theta - \frac{\sin 2\theta}{2}\right)$ was $1{\cdot}0383 \times r^2$

The surface width : $w = 2r \sin\theta$ was $1{\cdot}9260 \times r$

The kinetic head : $h = A/2w$ was $0{\cdot}2696 \times r$

That gave $T = d + h = 1{\cdot}0000 \times r$ as required

$$V = \sqrt{2gh} = 4{\cdot}166 \times r^{\frac{1}{2}}$$

and $$Q = V.A = 4{\cdot}326 \times r^{\frac{5}{2}}$$

Discharges for the two siphons calculated from the above relationship were 0·765 and 3·10 cusecs respectively as compared with 0·726 and 2·95 cusecs given in Table 1.

Figs 2 (p. 558) showed that the two siphons had been designed with the centre-line of the throat at spillweir crest level. With water in the forebay standing at that level, the priming discharges of Table 1 could only be realized by maintaining atmospheric pressure at the throat by means of an air-release pipe. The original design of *Figs 2* showed no provision for air release ; but the Authors' preliminary tests had clearly demonstrated the vital necessity of such provision, and it was doubtless for that reason that the Authors had dealt somewhat cursorily with the effects—in the absence of arrangements for air release—of air compression. In any case, such effects observed in a $\frac{1}{12}$-scale model could predict prototype behaviour only if the model were operated in an atmosphere of barometric pressure $\frac{1}{12}$th that of the prototype. Assuming a barometric pressure-head of 34 feet (of water) and regarding the pipe diameter as negligibly small compared with axial dimensions, Mr Crump had applied Boyle's Law to study compression effects in the An Cam Allt prototype with tail-water at O.D. 735. Under those assumptions he had found that starting—as was unavoidable on the first occasion—with no water in the trap and the siphon initially full of air at atmospheric pressure, water in the forebay would have to rise from O.D. 804·5 to O.D. (816·0+3·13) before water topped the throat and formed a seal at the bottom of the trap, and thereafter to O.D. (816·0+11·83) before water topped the crown of the trap and priming set in. It was also found that water left in the trap after a previous depriming could facilitate the next priming operation. For instance, under the most favourable hypothetical conditions, hardly likely to be realized in practice, a residual plug of water extending from the bed to the crown of the trap would leave an upper pocket of air at an atmospheric pressure-head of 34·0 and a lower pocket at a vacuum pressure-head of (34·0—7·5). In those circumstances the throat would be topped and priming would begin with a forebay level of O.D. (816·0+3·82). That was only slightly higher than the level of O.D. (816·0+3·73) that would be required for priming if the downpipe were straight and had no trap.

In the light of the above figures, and more especially in view of the Authors' criterion of a competent velocity, it appeared that a trap served no useful purpose, and could with advantage be omitted.

The Authors had described a number of alternative arrangements for air release. It would be of interest to know what final arrangements had been adopted, and at what forebay levels the siphons were found to prime.

Mr Roberts, in reply, said that he proposed to confine his remarks mainly to the subject-matter of his Paper—the raising of a portion of the Mullardoch dam, and the Fasnakyle high-pressure tunnel.

Mr Fulton had mentioned the pneumatic placer used for the roof of the high-pressure tunnel. It had also been referred to by Mr Ford and Mr Parry, and Mr Braine had mentioned mechanization in America. The particular placer which had been used, called the "Pressweld" placer, had originated, Mr Roberts believed, in America, and he could not help associating that with Mr Braine's remark about the quality of workmanship.

Mr Parry had stated that it could be concluded that the use of that machine in the low-pressure tunnel had produced satisfactory results. Mr Roberts considered that that could not be concluded until the tunnel had been in service for a number of years. He could not agree that the wider space provided in the high-pressure tunnel between the shutters and the rock had removed the main objection to the use of the machine, the principle of which he considered fundamentally wrong. With the intermittent system of placing, whatever the space available, it was not possible to avoid letting the concrete dribble down the sides of the shutter until a proper flow line from the point of delivery was obtained. That initial "wedge" of concrete in each length was therefore liable to be unsatisfactory. Concerning the "cold joints," Mr Roberts could not agree that any form of concrete-placing would be equally liable to produce them. He maintained that if concrete was placed in horizontal layers, being worked through ports in the shutters, there would be no need to accept cold joints as a result of hold-ups. The surface of the concrete could be prepared and extra mortar applied if necessary before proceeding further with placing. With the Pressweld placer, which involved flowing the concrete down sloping surfaces, a cold joint seemed inevitable with each hold-up. Mr Roberts was prepared to admit that there might be some hope for that machine with the continuous method of placing, provided that the concrete was flowing with a shallow slope and no hold-up occurred. It had to be admitted also that it did place a good arch; Mr Roberts thought that the explanation lay in the fact that the concrete in the arch did not fall over such a great height as it did for the side walls.

Mr Morgan's remarks about the probable stresses on the base of the thickened portion of the Mullardoch dam were fully appreciated, and Mr Roberts would like to thank him for his helpful criticism during the design stage and later in reading the proof of the Paper. He was glad to find that Mr Morgan agreed that the stresses shown in *Figs 20* were not likely to be exceeded. They were well within the strength of the materials used and Mr Roberts took comfort from that fact, apart from any views which might be held about the actual stress diagrams.

Mr Ford had mentioned the deflexions in the Benevean dam, the other main dam of the Affric works, and was quite correct in thinking that in the case of that dam, which was shorter than the Mullardoch dam, observations of deflexions along the crest had been taken from shore stations, using a theodolite, and had shown a deflexion upstream of up to $\frac{5}{8}$ inch during the filling of the reservoir. Mr Roberts would suggest that that was probably pure tilt, the bed of the reservoir being compressed slightly by the water. That phenomenon had frequently been observed when filling reservoirs. It could be further influenced by the fact that the water rising on the upstream face of the dam tended to cool the concrete relatively to that on the downstream face, which resulted in an upstream curvature.

He also ventured an explanation of the results shown in *Figs 27* and *28* of the Paper. The former showed the backing slab, before grouting, rising in relation to the old dam, whereas the opposite might have been expected owing to the shrinkage of the slab. In spite of the views expressed in the Paper, second thoughts suggested that that might have been the result of the time lag, caused by creep, in the action of the pressure of the water, which had risen sharply to 770 feet O.D. and levelled off while the slab continued to rise. In addition, during that period, the cooling of the slab had been less rapid than that of the dam. *Figs 28* showed the movement of the "Colcrete" filling in relation to the backing slab, both at the bottom and at the top of the slot. At the bottom, the filling had dropped and had then risen sharply, whereas at the top the opposite had occurred. The explanation of that might be that the "Colcrete" at the bottom would tend to be compressed as the upper part of the slot was being filled, causing the initial small downward movement observed. It would then rise under the effect of the increase in temperature which had still been taking place. At the top of the slot there had not been any superimposed load and only the upward movement due to hydration would be felt, followed by a downward movement caused by the subsequent cooling. In that connexion, Mr Roberts wished to pay tribute to the Resident Engineer for the Mullardoch works and his staff, for their initiative in designing and fabricating certain strain gauges on the site. Had there been more time, the difficulty with moisture penetrating the insulation would probably have been overcome. Credit was also due to the contractor for his ready assistance in arriving at the most suitable method of introducing the "Colcrete" into the slot between the old dam and the backing slab.

Mr Ford had referred to the cavity grouting on the arch of the concrete-lined portion of the high-pressure tunnel. It was incorrect to say that the cavity grouting had been carried out at 100 lb. per square inch. That had been the pressure for the first stage of grouting. The pressure used for cavity grouting had been 50 lb. per square inch; it was not advisable to use a higher pressure for fear of blowing the roof down. Mr Ford had contended that a certain amount of grout was bound to have entered the fissures in the rock. Mr Roberts did not deny that but felt that it was difficult to be dogmatic about it. The main purpose of the cavity grouting had been to fill the gaps which inevitably occurred in placing a concrete arch in a tunnel. Mr Ford had also suggested that the screw plugs in the grout pipes might have been replaced by cement mortar. Where the threads had been damaged it had been necessary to resort to that method of plugging, and it had to be done carefully. Mr Roberts thought that the method would be satisfactory if the grout was very stiff, almost dry, and was literally hammered into position; otherwise there was the danger of the plug shrinking and ultimately dropping out. Such a method would, however, be perfectly satisfactory in the invert of a tunnel.

Mr Ford had also referred to the amendment of the tunnel profile from horizontal to a 1-in-50 slope. That had been done, not only to reduce the depth of the high-pressure shaft, but also to assist the contractor. That the slope was in fact too great was something which the contractor had found out by experience and would be noted in future designs. As to the change in design involving the elimination of type B tunnel, one could sense from Mr Ford's remarks that he would have preferred not to have the change for the sake of the overheads involved. In fact, it had been made to help the contractor in carrying out the work. When the ground had been opened and the quality of the rock realized, it had been felt that the complication of placing the steel membrane and the reinforcement should be avoided and the construction simplified if possible. The suggestion of introducing items in a bill of quantities to cover overheads separately from items covering work might have certain advantages. One disadvantage that Mr Roberts could see, from a promoter's point of view, was that a contractor could get regular payments while doing no work at all!

In reply to Mr Hoffman, it should be stated that the pipes of type C conduit had, in fact, been designed without regard to any support from the ground, just as if they had been surface pipelines, with the added precaution that they had been designed to resist the full hydrostatic pressure applied externally with the pipes empty. That had been felt necessary because of the uncertainties, and because of the dangers inherent in any failure. The stresses allowed for in the design were the following: the maximum permissible working stress was 6 tons per square inch or about 13,500 lb. per square inch, as assumed by Mr Hoffman; the actual stresses obtained with a maximum head of 550 feet for various conditions were :—

Stress in walls of pipe when still new	10,600 lb. per square inch
Stress in walls of pipe after the assumed loss of $\frac{1}{16}$ inch of thickness through corrosion	11,230 ,, ,,
Stress at riveted joint with a joint efficiency of 86·5 per cent, calculated on the basis of the actual riveting lay-out used	13,000 ,, ,,

Although the latter stress was less than the permissible stress adopted, it would have exceeded it if the next smaller thickness of plate had been used. Whilst Mr Roberts appreciated the figures which had been quoted for possible saving in steel, it had been felt unwise to go below the factor of safety indicated in the Paper. The welded longitudinal joint suggested by Mr Hoffman would have been adopted had it been possible to get a manufacturer to undertake such a joint, but all the quotations received had involved delivery dates far beyond that which could be allowed, and the only supplier who had been willing to meet the requirements insisted on the riveted joint. The suggestion that the thorough grouting programme of the rock would justify reducing the thickness of the pipes had also been considered but rejected, since it had been felt that grouting could only improve the compactness of the rock but not its weight, and it had been the weight of the rock cover which had determined the point at which a "full strength" lining should start.

In connexion with Mr Hoffman's further suggestion of putting in a single pipe and trifurcating 100 feet from the generating station, the problem involved had been one of access as much as anything else. The concrete tunnel had to be lined from the power station end rather than down the shaft, and it had been necessary to keep the work going whilst at the same time erecting the pipes. If a single pipe had been adopted, once the pipe erectors had been brought in everything else would have come to a standstill. By having three tunnels, there was a chance of erecting the pipes in one, spraying in another, and retaining access through the third; that design provided more flexibility for carrying out the work.

With regard to the siphons, it had been very clearly shown in the film how the air was discharged almost explosively from the vent pipes. In full-scale practice that was precisely what did occur with intermittent spouting at frequent intervals while the siphon was in operation. Air presumably accumulated at the top of the vortex chamber until it could overcome the weight of the column of water in the vent pipe; it would then blow the water out and spend itself, and the cycle would be repeated, producing the periodic bursts of mixed air and water from the pipe. It was fortunate that, at Affric, the head-works were such that no particular damage could result, but it might be a matter to be taken into consideration elsewhere.

Professor Allen, in reply, said that before the experiments had been started there was practically no information available regarding the damage which would result if air did go along the tunnel supplying the turbine in projects of the kind in question. Obviously, on general grounds, such entrained air, would be very undesirable, and Mr Fulton had explained that there might be difficulties from cavitation, apart from the reduced efficiency brought about by the machines being supplied with an emulsion with a density less than that of water; the Authors had failed—it might be entirely their own fault, in not consulting the right literature—to find any substantial amount of information on the actual effect of entrainment of air on the performance of machines and in other ways in power projects of this kind.

The analysis of the flow in the siphon of which Mr Morgan referred, and in particular the calculations about the free vortex and the degree of agreement or disagreement between theory and observed practice, recalled their own approach to this matter. They had seen references in various places to calculations of pressures in siphon spillways of rectangular cross-section in which the theory of the free vortex had been used and had been shown in certain cases to give approximately quantitative agreement with observation. They had at first thought that in the circular section there would be a corkscrew spiral motion to a larger degree than in the rectangular section, so that any hope of the free-vortex theory applying would be remote. There was also one other feature, though a comparatively minor one, namely, that the actual integration involved in the free vortex calculation for the circular section was more troublesome. For a time that fact, combined with their doubts about whether it was any use to do it at all, discouraged them from even trying it. However, they did ultimately try it, and to their surprise the results did agree quite closely, especially in the most pronounced suction effects at the critical points. That was very important.

Mr Braine's contribution had raised or emphasized many points which had troubled Professor Allen and his co-Authors on the subject, and to which they had actually made some reference. They had made no claim that experiments of the kind which they had carried out would afford *strictly quantitative* results in all respects. They had in fact made a point of stating that there were dangers in this kind of experiment. First of all, atmospheric pressure was not being scaled down. The size of the bubbles was another point to be borne in mind. They had, however, used different scales of model, perhaps not over a very wide range, though when comparing a 1-inch pipe with a 2-inch pipe there did seem to be a considerable difference in practice. It was true to say that the general behaviour had been similar, and that had given them some confidence in extrapolating to the full size, though they made no claim that the agreement would be quantitatively exact in every detail.

Reference had been made to the nozzle used upstream of the vortex chamber. In the design as submitted to them, he understood that the object of the nozzle was to reduce the suction which would otherwise be found at the critical point high up the siphon, and the nozzle did that by virtue of the kinetic head discharged from it. Another feature of the nozzle, in combination with the vortex chamber, was that their experiments suggested that under a given total head the discharge in the siphon was increased slightly (he thought by about 4 per cent) by use of the vortex chamber, which they attributed to the conversion of the kinetic head to pressure.

Professor Allen then made further reference to Mr Braine's remarks concerning the vortex chambers. Possibly there was a misunderstanding, but Mr Braine's remarks appeared to imply that the chambers did not fulfil their purpose. The experiments indicated, however, that with the design finally evolved, the chambers would ensure that almost all the air would be removed and very little indeed find its way into the tunnel. The ciné film had demonstrated that conclusively and supported the contention that it was correct to place the nozzle upstream of the chamber; the jet issuing from it with high velocity in fact promoted the motion necessary to make the chamber effective.*

Both Mr Braine and Mr Crump had referred to the Authors' statements concerning the velocity of air bubbles. The source of the Authors' information, and the confirmatory evidence derived from their own experiments, were set out in their Paper, but they had since found additional data quoted in "The Essentials of Fluid Mechanics" by L. Prandtl (Blackie & Sons, Ltd, 1952), pp. 329, 330; to the effect that:

* A photograph illustrating the effectiveness of the chamber may be seen in the Institution Library.—Sec. I.C.E.

(i) If d denoted the diameter in millimetres of a sphere equal in volume to the air bubble, then the velocity of ascent in water was about $0{\cdot}127\sqrt{d}$ metres per second, if d was between 1 and 2·5 millimetres. Thus, taking the upper limit ($d = 2{\cdot}5$), the velocity would be 0·200 metre per second (0·65 foot per second).

(ii) If d was between 3 and 8 millimetres the shape of the bubble changed so that the velocity remained constant at about 0·21 to 0·22 metre per second (0·69 to 0·72 foot per second).

(iii) If d was greater than 12 millimetres, the velocity was about $0{\cdot}068\sqrt{d}$. Thus, taking $d = 25$ millimetres, the velocity would be about 0·34 metre per second (1·1 foot per second).

(iv) For a very large bubble, occupying virtually the whole cross-section of a pipe of diameter d milimetres, the velocity of ascent was about $0{\cdot}035\sqrt{gd}$ metres per sec.

Those results indicated that the Authors' criterion of a velocity of the order of 0·75 foot per second should be safe over a wide range of bubble sizes, although it was true that *absolute* pressure might affect the *shape* as well as the size of the bubbles and the velocity was largely influenced by shape. In any event, however, Mr Braine's contention that 1·25 seemed to be a safer value (taking that to imply also a *better* value) to adopt than 0·75 foot per second appeared to be inconsistent with the Authors' observations on such widely differing examples as the 1-inch-diameter glass pipe bent to the form of a circular trap (*Figs 10* and *11*, facing p. 565) and the 2-inch copper pipe (*Figs 12*, p. 565).

The Authors agreed with Mr Crump that what was true of the vertical limb was not necessarily true of an inclined limb. With regard to the Howbery Park " mushroom " siphon, they wondered whether it was *fully* primed under the low discharge mentioned; the very small back pressure also rendered it quite dissimilar to the conditions necessarily applying in the Glen Affric siphons. Mr Crump's calculations of the discharges likely to occur with a total head at the throat equal to the radius were very interesting and instructive and certainly gave values in reasonable agreement with the Authors' experimentally determined results. Owing to the curvature of the centre-line of the vertex of the siphon, however, the application of the theory of critical flow would not have appealed to the Authors without experimental evidence.

The suggestion that any air admitted to the tunnel would eventually escape up the surge-shaft could only be partially true. The solubility of the air in the water would be enhanced by virtue of the pressure, and even supposing that some appreciable quantity of the air would rise to the roof of the tunnel, it seemed to be at least possible that a good deal of it would be swept past the opening at the bottom of the shaft since the mean velocity of the water in the tunnel could be as high as 12 feet per second.

The Authors did not agree that the circular trap could with advantage be omitted. Given some effective form of air vents, the trap ensured that priming of the siphon would occur under a relatively low head, and once the upper portion was fully primed, the lower part was swept clear of air by the resulting heavy discharge.

The actual siphons had been provided with an air-release arrangement similar to that shown in *Figs 13* (p. 566), with $\frac{3}{4}$-inch piping to make the connexions A to B and C to atmosphere. It was understood that the siphons had worked well with that arrangement. One of the Authors had recently visited Allt Blar na Gamnha siphon and had observed that it was priming (as evidenced by the sudden draw-down in the forebay) when the water in the forebay rose to a level about 6 inches below the crest of the overflow weir. It was believed that it had been decided to construct that intake with a weir crest level of 787·0 feet O.D. and a siphon-throat centre-line at 786·5 feet O.D. On the same ocasion, the pipe connected to the top of the vortex chamber had been seen to spout several feet into the air.

Mr Crump's written communication had added greatly to the interest of the discussion.

Professor Allen repeated the statement he had made when introducing the Paper that the Authors claimed no credit for the *idea* of the siphon or vortex chamber; their function had been to study the tentative designs in the laboratory and to advise as to the prospects of their satisfactory operation in practice and to submit detailed proposals concerning air vents, position of nozzle, size of vortex chamber, and other features.

Since writing the foregoing reply to the discussion, the Authors of Paper No. 5888 have received the following information from Mr C. M. Roberts, the Author of Paper No. 5926.

Siphon	Minimum priming level in headpond: feet above O.D.	*Invert* level of throat as constructed: feet above O.D.
Allt Blar na Gamhna . .	786·10	785·86
An Cam Allt	809·41	808·87

Correspondence

Sir William Halcrow observed that, in principle, the design of the Affric scheme was the same as that outlined by his firm for the Report of the Water Power Resources Committee in 1918. He had made one important change, however, by dispensing with the pipeline, and as would be seen from the drawings the aqueduct was now entirely in tunnel from the reservoir to the power station. That had been done in the interests