

Discussion.

The CHAIRMAN, in moving a vote of thanks to the Authors for their Papers, remarked that even those who had not given much attention to that particular branch of engineering must realize that the Papers were important contributions to its literature.

Sir MURDOCH MACDONALD, M.P., endorsed what the Chairman had said as to the importance of the Papers: the conclusions were very valuable, in that they practically settled the problem of the determination of the real discharge of a great river by means of current-meters. When he first came into contact with the subject, some years ago, that discharge was not known with any degree of accuracy. Neither the flood stage nor the low stage—which in Egypt was really the important stage—was accurately gaugeable. When he started to measure the water at Assuan, there was a discrepancy of up to 25 per cent. between the sluice discharges and the discharges then measured in the river; and, as far as irrigation was concerned, a difference of 25 per cent. might mean the difference between success and failure of any particular year's crops in a district. Having started those experiments and to some extent helped in the genesis of the work on which these Papers were based, it had been indeed a pleasure to hear them read. When, from the measurement of individual sluices discharging into a tank, the data obtainable were used to calculate that of the whole river, the final error appeared to be within 1 per cent. at the low stage. This was most unexpected and gratifying. Dr. Hurst had continued the experiments, with the assistance of Mr. Watt, in order to find out what occurred at the high or flood stage, because that question, though it had not been vital to Egypt in the past, would certainly be so in the future, as a great tract of country in the Sudan—which was incessantly demanding a share of the water of the Nile—might affect it, and the question of dividing the flood waters in some proportion might one day arise between the two countries. Accurate measurement of the water actually passing was thus rendered necessary. Turning to the details of the first Paper, he was not quite happy about *Fig. 7* (p. 84), in which there were drawn two lines running through the plotted results of a series of experiments made with an actual sluice, while an intermediate dotted line represented the results from the model. It did not seem to him that the Authors were fully justified in drawing the two solid lines. The lower line went beyond the actual

The Chairman.

Sir Murdoch
MacDonald.

Sir Murdoch
MacDonald.

experiments between R.L. 110 and R.L. 111 ; the upper line went down to an experiment at R.L. 107. He would suggest that, on the data presented, it was a little too much to ask for acceptance of the deduction that a sluice could have two different discharges under exactly the same conditions in all respects. He thought the Authors should continue their experiments and satisfy themselves whether it really was the case, or not, as was stated in the Papers, that a sluice could give two discharges differing by possibly 7 per cent. It might be that the departures from the model values were partly due to errors in the full-scale discharges. There were very few full-scale discharges at those particular levels, and it was not unlikely that that was the explanation. The practical result of all this very valuable work was the assurance that a model could predict with practical certainty the discharge which would be produced under natural conditions in an actual sluice. Had that fact been known before, a great deal of trouble and anxiety would have been avoided in the building of these big works. He noticed that Sir Wilfrid Stokes had adopted the suggestion he had made with regard to the Blue Nile dam, and he regretted that the figures found by Sir Wilfrid were not attached to the Paper. It really had not mattered at Assuan, when the dam was being built, whether the measurement of the discharge was very accurate or not, as there was such an obvious excess of opening ; but, in the case of the Blue Nile dam, it was much more desirable to know what would occur, because the number of sluices was relatively small, and there was much greater variation in the river itself. With regard to the second Paper, the accuracy with which the discharge could be recorded was shown on p. 138 to be practically 1 per cent., and from the point of view of an engineer dealing with a river, that was a remarkably small figure. Some of the observations on pp. 140-142, in connection with *Fig. 19*, certainly needed explanation ; the Authors said : " The error is probably not, as was at first thought, due to sounding, and needs further investigation. In view of the form of the results, it is almost certainly the current-meters and not the sluices which are at fault." With the current-meters, although accuracy could be achieved, a large number of observations were required. If one had to measure the discharge of a river with current-meters, the observations ought to be repeated a number of times and the mean taken, even if the instruments themselves were accurately calibrated. The three or four observations indicating a large factor of variation from the anticipated result at the high levels might be neglected ; as they were probably not repeatedly taken. Apart from this instance, the accuracy was very great, and the Authors were able to show that

the discharge of a great river could be measured within 2 or 3 per cent., even in its flood time, if a series of current-meter observations was properly made. The Authors deserved the thanks accorded to them by The Institution that evening for having demonstrated that the discharge of a great river could be measured to such a degree of accuracy.

Sir Murdoch
MacDonald.

Professor S. M. DIXON remarked that the value of the Papers would be apparent to everyone. In commenting on them, he intended only to criticize the methods and not the results. The Authors stated at the beginning of the first Paper that they had such confidence in the method that it was no longer necessary to carry out full-scale experiments. Fortunately the investigations described in the second Paper had been carried out, which showed that the discrepancies between current-meter observations and measurements on full-size sluices were only 1 or 2 per cent. The value of experiments on models was really qualitative and not quantitative, in that it was possible to introduce modifications in a model and to examine their effects, at far less cost, and with much less trouble, than would be involved if the conditions of an actual structure were varied. He thought the Authors should be content to rest on that claim. In fact, once or twice they showed that they realized just where the model failed, namely, in dealing with the influence of the roughness of the surface. That roughness was very important in sluice measurements. Dr. M. A. Hogan, Assoc. M. Inst. C.E., had pointed out, in his last report on the measurement of water over weirs, that the discrepancies which occurred in the well-known weir formulas were due to the roughness of the upstream part of the weir. Experiments with small models alone would never determine that effect, because one could only experiment with a model of one roughness. He thought the Authors showed that in their Paper. They also showed that the discharge through the models was quite different from that through the weirs. The discharge through the models was a clear flow, whereas the discharge of the actual sluices was like snow, a vast quantity of vapour and spray passing through the sluice. Therefore they realized the limitations of the model. He would like to ask if the Authors had had any difficulty with the hook gauge they described. His experience showed that with a meniscus of water against the glass it was difficult to get consistent readings. He was not certain what degree of accuracy was required in those observations. The difference between the two sets of experiments was about $\frac{1}{2}$ inch on the model. Any well-constructed hook gauge would read certainly to 0.001 inch, and would seem to be sensitive enough.

Professor
Dixon.

With regard to the extremely interesting method described in the second Paper, he wondered if the Authors had thought of using a Pitot tube instead of a current-meter. A Pitot tube was so easily calibrated that it would be interesting to try that additional method of measuring the discharge.

Mr. G. S. BAKER said he would confine his remarks to the instruments used by the Authors, and to the comparison of model and full-size work. If he spoke more of their discrepancies, and did not say much on the general results obtained, that was not because he did not appreciate the great value of the work done, but because experimental discrepancies were always the more interesting part of any such work. With regard to the use of current-meters for the measurement of the velocities in the sluices, he had tested a fair number of current-meters, and for all moderate velocities down to about $\frac{1}{2}$ foot per second the observations plotted on a straight line. At quite low velocities, however, the current-meter was not to be relied upon at all. At very high velocities, too, there was an indication that it would fail, and the Authors had found a degree of change which was rather surprising. Their pitch values varied by as much as 7 per cent.—and even a little more—between the low velocities and the high velocities. That was a perfectly genuine change, due, not to friction as the Authors suggested, but to hydrodynamic changes in the pitch value at different velocities—to what was called “scale effect” in the hydrodynamic flow. He suggested that that was probably the reason why the pitches of the Authors’ meters changed steadily as the velocity changed. He thought, too, that the explanation of the wild plotting of velocities measured with such meters, with high pressure-heads, was to be found in the changes of pitch value of the meter and not in its mechanism. When water pressure-heads comparable with atmospheric pressure were obtained, a breakdown might occur due to cavitation behind the pitch blade; and he thought it was quite possible that these meters would break down at a pressure-head of 30 to 40 feet. With regard to the hook gauges, he used them continually in his work, and they worked to an accuracy of about 1 in 1,500 as plain hook gauges. He rather agreed with the last speaker in doubting whether, if a glass mirror was added, the meniscus could be obtained in a reliable way, when the water-level was moving up and down. He thought the meniscus would move as well, and the calibration would vary according to whether the water was steadily increasing in depth, or whether it was falling. As to the discrepancy between models and full-size work, he thought that, whatever might be said as to the qualitative nature of the work, the Authors had shown by

experiment that a measurement made on a model of scale $1/n$ Mr. Baker. did give a reliable estimate of the quantity of water which would go through a full-size sluice, provided n was not too great. The discrepancies were extremely small—too small in most cases to be even reliable. The chief one was that shown by *Fig. 7* (p. 84), mentioned by Sir Murdoch MacDonald, which was worthy of close examination. It would be found, when the diagram was examined, that the upper line did not extend below the level of 106 or 107 metres, and the lower one did not go above about 110. Also, it was stated in the Paper that at 106 metres the narrow part of the sluice at the back of the gate commenced to run full. In other words, at 106 metres a water-lock would begin to form on the rear side of the gate, when large air-bubbles came out in the discharge on the down side of the full-size sluice. That in itself was a suggestive cause of the change-over, from one curve to the other. The air-lock which was formed in the actual sluice might have a different effect from the air-lock in the model: the pressure-changes which were taking place in the model were all relatively small compared with atmospheric pressure; but on the large-scale sluice the pressure-changes were not only comparable with, but in some cases equal to, the atmospheric pressure. The water-lock produced conditions at the rear of the gate which were different in the model from what they were on the full-scale sluice, unless all connection with the atmosphere at the rear of the gate was rigidly broken in both cases. In practice there were grooves for chains, etc., and this might explain why different results were obtained. In the model the change was too small to be seen, and a fair curve resulted; but, in the large-scale sluice, a definite reduced pressure was formed at the rear of the gate, and the flow through the gate was thereby increased. The more perfectly that was done, the greater was the effect; and, at last, when the water-lock was quite perfect, the second type of flow would be stabilized. When it was imperfect, the other type of flow might take place. There was no doubt at all, despite what Sir Murdoch MacDonald had said, that it was quite feasible to get two types of flow under what appeared to be precisely the same initial conditions. He had experienced it with ship-models, and he was quite prepared to believe that the Authors could have realized it on the Assuan dam. The most interesting result in the model work was the effect of roughening the walls. It rather surprised him to learn that the roughness of the walls only produced an average of about 5 per cent. difference in the discharge through the sluice. That showed what a relatively small part the skin-friction of the walls of a sluice had in the discharge of that sluice. Therefore,

Mr. Baker. inferentially, there was no need in these sluices to struggle for any remarkable smoothness of surface. The one thing in ship-models which had given great trouble—trouble beyond 5 per cent.—was the vertical laps of the plates of the ship. He thought the vertical joints of the stones in the sluice were about the only thing which really required consideration. If those were not good flat joints, it was possible that considerably more than 5 per cent. might be added to the difference in the discharge of the sluices.

Mr. Boulton. Mr. NORMAN BOULTON remarked that the Papers were of special interest to him because he was carrying out a series of experiments on the flow over models of broad-crested weirs under the direction of Dr. F. C. Lea, M. Inst. C.E., who had asked him to express his regret at not being able to attend that meeting. The first object of their experiments was to investigate the discharge over particular forms of broad-crested weir suitable for hydro-electric works, and to find their coefficients. Secondly, they were attempting to determine how the results departed from the fundamental principle of similarity. In comparing the departure from similarity with the friction term $KR/\rho V^2$, using the notation in Paper No. 4450, the value of $R/\rho V^2$ was found from the Chézy formula instead of from the published results of Dr. Stanton and Mr. Pannell. The advantage of determining the friction term in that way was that any effect which surface tension might have on the flow was taken into account. The coefficient c in the Chézy formula was found from the results of Bazin's experiments. He would expect the results of the experiments proposed (p. 107), to investigate the effect of temperature on the discharge, to be of a negative nature, owing to the following considerations. In the Appendix on p. 109, Professor Thomson's principle of similarity was referred to, and attention was drawn to its approximate nature when the principle was applied to turbulent flow. When, however, the principle was deduced from the fundamental equations of motion for a viscous fluid, it was seen to hold exactly for turbulent motion, provided certain conditions were satisfied. In addition to the model being geometrically similar to its prototype, the conditions required the viscosities of the water flowing over the model and prototype to be in a given ratio. For exact similarity the ratio of the kinematic viscosities must equal the scale-ratio raised to the power 1.5. The results of Dr. Hurst's work had shown that when the viscosity-ratio was approximately unity, the temperature being approximately the same for the prototype and the model tests, the maximum departure from similarity was never greater than 4 per cent. If the temperature of the water flowing over the model were 50° C. and the temperature of the water

flowing over the prototype were 15°C. , the kinematic viscosity ratio Mr. Boulton. would be about 2. To obtain exact similarity this ratio would have to be 190 if the scale-ratio were 100/3, namely, the scale-ratio raised to the power 1.5. Therefore, he concluded that a variation of temperature from 15°C. to 50°C. would have only a negligible effect on the flow over the weir.

Mr. J. S. WILSON observed that it was mainly due to Sir Murdoch Mr. Wilson. MacDonald, who had initiated the experiments, that the work had been carried out; and in work of that magnitude and importance it was the initiation which counted. The importance of the results could not be overstated. The references to *Figs. 15-18* (pp. 131-134) were not at all clear. The context referred to the approach being normal in the one case and sloping in the other, and did not particularize which was which. It was interesting to examine the curves of velocity over the cross section of the sluice. The current-meter was placed in a rather unfortunate position. All the contours, apparently, were carried right up to the edge of the sluice, but, in the ordinary way, velocity could not be measured right at the edge. The fact that this had been achieved was due to the current-meter having been placed so that its axis was at the edge of the recess containing the rollers. He rather thought that that fact must damage the value of these results to some extent. A pocket of that kind appeared to reduce the friction very considerably, because, as was evident from those curves, the velocity was highest at the ends of the travel of the current-meter. Dr. Hurst was a keen statistician in matters of measurement of discharge, and was inclined to be a little severe on those who did not follow that method. Naturally, the principle could be applied to some subjects but not to others. He mentioned that, because Dr. Hurst's predilection for that method had led him, in discussing the recent Paper by Mr. Buckley,¹ to make some rather sweeping statements about the accuracy of his figures, merely because Mr. Buckley had not adopted the statistical method. Mr. Wilson was surprised to note that the method did not bring out the anomaly which had already been referred to, namely, the double value of the discharge at certain heads. That anomaly might have been mentioned in the earlier Paper, which was frequently referred to in the present Papers. It was now stated that the fact had been realized, but it was only in the later Paper that it was mentioned. If, in that earlier Paper, it had been stated that these two results, differing by 5 per cent., had occurred without apparently any reason, rather less importance might have been attached to the

¹ Minutes of Proceedings Inst. C.E., vol. cxxvi, p. 257.

Mr. Wilson. resulting accuracy of 0.5 per cent. That brought forward a rather fundamental point with regard to this research work, which he did not think could be mentioned too often, namely, the importance of giving all the data. In this Paper the Authors mentioned that they were going to investigate the question of temperature, and that they had the records; but they did not give them. Very important work had been done in the past on Bazin's experiments. In Bazin's work a vast number of results were given; and anybody could use them to test his views on the ideal formula. Mr. Buckley had achieved a good deal in that way, and he had given practically all his figures. In a Paper of scientific importance, he thought as much information should be given as suitable economy in the matter of printing-costs would allow. He did not wish to give the impression that he was criticizing the work of these Papers adversely. He realized that the work done by the Authors was of the greatest importance, and he would like to associate himself with every statement that had been made with regard to its value.

Correspondence.

Mr. Buckley. Mr. A. BURTON BUCKLEY remarked that it was stated (p. 142) that the four observations above 9,000 cubic metres per second discharge, which departed considerably from the sluice observations, indicated that the velocities given by current-meters at Khannaq were too high, and that it was almost certainly the current-meters and not the sluices which were at fault. Referring to the rating of the special current-meter used by them, the Authors stated that the velocities in the C and D sluices ranged from 3 to 6 metres per second; this appeared to be an error, as the velocities in the D sluices were shown to be as high as 7.6 metres per second. The rating curve, *Fig. 3* (p. 117), showed no experimental determinations above a velocity of 6.6 metres per second; the Authors gave this value as 6 metres per second, and stated that the rating curve was linear. As the plotted values at the higher limit of the rating curve showed a tendency to rise above the straight line AB assumed by the Authors for the rating curve (*Fig. 1*, p. 153), the alignment AC seemed as probable as AB, and it might well be that with the higher velocities the current-meter apparatus used in the sluices became capricious. In fact, in the circumstances, the extrapolation of the rating curve beyond the observed velocity of 6.6 metres per second appeared to be unwarranted. This was especially so in view of the obliquity