Discussion.

The President. The PRESIDENT, in moving a vote of thanks to the Author, observed that the Paper was one of very great interest to all engineers who were connected with that class of work, and showed the extreme care and great ingenuity with which the Author had dealt with the various problems that had arisen in his work.

The Author. The AUTHOR exhibited several lantern-slides in further illustration of the subject.

Dr. Unwin. Dr. W. C. UNWIN, F.R.S., Past-President, remarked that the Paper contained such a mass and variety of quite new information that it was not an easy Paper to discuss. He believed the compressed-air installation on the Rand was the largest compressed-air installation ever built, and was peculiar in the fact that the air was compressed to a comparatively high pressure by turbo-compressors and was distributed continuously along a main over 30 miles in length to the various mines on the Rand. When the scheme was first developed a definition was given of the unit of air on which the charge was to be made, and that was theoretically quite a satisfactory unit, but there was at that time no means of determining how many units went to each mine. In order to obtain a meter which would directly show the units supplied to each mine, an exceedingly intricate scientific investigation had to be made. Seeing that the commercial interests were large, the investigation was carried out at an expenditure which would probably never have been within the reach of ordinary scientific investigators. The large three-cylinder standard meter, each cylinder 3 feet in diameter with 27 inches stroke, was a piece of apparatus very much beyond what could be ordinarily used in scientific research. The investigation had been carried out with great scientific knowledge. It had involved the invention of a large number of quite new appliances, and the insight which had been shown throughout the whole investigation was deserving of the highest praise. It had ramified out into a number of subsidiary inquiries of almost equal interest. In selecting a meter, there had been half-a-dozen methods of procedure more or less known, and the ultimate selection had been the Venturi tube, which had been selected no doubt mainly because in the Venturi-tube meter the loss of pressure was exceedingly small. In developing that meter it was a matter of extreme interest that it had been possible, by

quite simple means, to measure not only the pressure and temperature and velocity of flow but the logarithms of functions of those quantities, and by means of cams to add these functions of the pressure, temperature and velocity on one shaft, and re-convert by an anti-logarithmic cam the measure of the total air units passing. Besides the Venturi meter another meter was installed at each mine; this meter was of the hinged-gate type, an old form of meter that had always been considered a very rough appliance, because there was generally no means of properly calibrating it. There was no scientific law with regard to it, but it had to be calibrated in order to make its indications accurate, and it was for the calibration of such meters that it had been necessary to develop the very interesting method of calibration by means of water flows. This had involved a scientific research into the conditions under which the flow of water was to be used, the research having been carried out with very great acuteness and accuracy. The advantage of the use of water was that it was a much less expensive method of calibrating the meter than having a huge standard air meter for calibration. It had also been found necessary to develop a simple and accurate method of measuring the rate of flow of air or fluid in any main. For this purpose the Author had devised and calibrated in all their various sizes square-edged orifices such as were shown in *Fig. 11*. That reduced the measurement of the discharge from the compressor to as great simplicity as measuring the discharge from a water turbine by a V notch, but it required a determination of the precise conditions in which the orifice was to be used in order to be sure of its coefficient of discharge. There was nothing more striking in the Paper than the investigation of the conditions under which definite coefficients had been obtained for given forms of orifice. It was quite remarkable that the Author had been able to find a constant coefficient for orifices which varied from $\frac{1}{16}$ inch to 9 inches in diameter. It had long been known that in some way the rate of flow of air and water corresponded to the square root of the head, but the exact conditions under which that law was fulfilled had never been properly investigated before, and obviously an enormous amount of observation had been carried out in determining the data which were given in the Paper in regard to the conditions under which the coefficients for air and for water were practically the same. There was a very interesting meter described towards the end of the Paper for metering steam, a meter placed in the steam-pipe and giving directly the weight of steam passing through the pipe. An instrument of that kind might be of immense use in observing the behaviour of engines. The ordinary method used

Dr. Unwin.

Dr. Unwin. in the past for getting at steam-consumption had been to condense the steam and measure it through an orifice, or over a notch, a method which required considerable modifications in the ordinary arrangements of the engine and could only be carried out with considerable trouble. If it was possible to place so simple a meter as had been shown in a steam-pipe and to note at any time the quantity of steam the engine was using, it would be a very great gain to steam engineers. It was not quite clear to him in what way, in the steam meter shown in *Fig. 21*, the correction was made for variations in the steam-pressure. The meter appeared to register directly the weight of steam passing, and to do that, not only had account to be taken of the velocity of the steam in the pipe, which was indicated by the diaphragm, but there had to be some further arrangement for correcting those indications to give the weight corresponding to different pressures. He had found the Paper of extraordinary interest; but it was not a Paper which was easily discussed. He thought it was one of the most creditable Papers that had been brought before The Institution.

Dr. Brightmore. Dr. A. W. BRIGHTMORE thought the Author was to be heartily congratulated on evolving correct results from such complicated processes as he had explained. His object in rising was simply to ask the Author to be kind enough to clear up one or two points which he felt he did not understand in connection with Appendix II. With regard to Equation 8 in Appendix II, the Author stated that, the equation being cumbersome to use, its evaluation might be simplified by combining it with the less complex formula obtained on the assumption that W , the density, was constant, and he purported to combine the latter formula with the previous one, to obtain Equation 12. As far as he could see, Equation 12 was identical with Equation 8, which was simply divided into two parts, the first multiplied by $\frac{n^2}{n^2 - 1}$, and the latter part divided by the same thing, so that the two equations seemed to be identical. Equations 14 and 16 were obtained in a similar manner from Equation 8. It appeared to him, therefore, that the introduction of that approximate formula quite unnecessarily cast a doubt on the exactness of the theoretical deductions, and he would be glad if the Author would say whether that was not correct.

There was one matter he would like to mention which perhaps hardly bore directly on the Paper, and that was an extremely simple form of water-meter, which, so far as he knew, had never been made use of, although it existed in considerable quantities—

namely, a curved pipe. The difference in pressure at the two ends of the horizontal diameter of a curved pipe, through which water was flowing, was proportional to the square of the velocity through the bend, and although it probably would not constitute a very exact form of meter, there might be cases, in which exactness was not of very great importance, where it might be used. He thought probably the reason why it had not been applied was that it was generally considered that in a bend the motion of the water was of an eddying nature to a greater extent than in ordinary pipes, but he thought the result of experiments showed that such was not the case, and that the loss of head in the bend was not greater than in the same length of straight pipe. The loss really occurred in the portion of the pipe beyond the bend, because in the bend the water flowed round in a free vortex, with the maximum velocity on the inside, so that when the water came to the end of the bend it was necessary for the velocities to re-arrange themselves, and the loss of head took place there. In Vol. clxix of the "Proceedings," some experiments on 4-inch diameter cast-iron pipes, not varnished, were described, in which the radii of the bends varied from four to ten times the diameter of the pipe, and the difference in pressure on the two sides of the horizontal diameter at the end of the bend varied from 0.3 inch of water for a velocity of $2\frac{1}{2}$ feet per second to 4.7 inches of water for a velocity of $10\frac{1}{4}$ feet per second. Although the differences were not very great, plotting those and the intermediate values, which were, for $5\frac{1}{2}$ feet per second, 1.4 inch, and for 8 feet per second, 2.8 inches of water, they were found to lie very nearly on a parabolic curve. He drew attention to that matter because of its extreme simplicity, but he did not suggest it would be useful except in cases where extreme accuracy was not necessary.

Dr. J. Bright,
more.

Mr. HARRY E. JONES observed that the Author had met, as Mr. Jones Professor Unwin had pointed out, a rather novel problem. As he himself had been connected with the measuring of gas for many years, he could say that one would hardly venture to conceive of any system of measuring the enormous volume concerned in the air deliveries by any means that were employed in gasworks; the ordinary meter with a drum was out of account altogether; it was useless for dealing with anything more than a few hundred thousand feet per hour. In the case of compressed-air supplies to large centres of power production, it was obvious that it was necessary to measure and control and indicate the flow of volumes out of all comparison with those which the ordinary gas-engineer had to deal with. In gasworks the Venturi meter had been introduced,

Mr. Jones. and in other cases the well-known law that an orifice of a certain size would pass a volume of fluid in the ratio of the square root of the head of pressure upon it had been applied; but that was only a very short way on the journey that the Author had gone. The Author had had to contend with the alteration of density due to variation of pressure at the different points. In gasworks another difficulty had been met with which, up to the present, had even baffled the Author's ingenuity, namely, that in ordinary cases the specific gravity of the gas varied. In conclusion, he considered that the Paper showed an amount of scientific research, mechanical capability, and resourcefulness that commanded the greatest respect, and it would enrich the "Proceedings" of The Institution.

Capt. Sankey. Captain H. RIAL SANKEY remarked that the subject of the Paper was of considerable interest to him, but, with Dr. Unwin, he felt it was difficult to discuss. The first impression on looking at the Paper, an impression which was strengthened on reading it, was that it dealt with a very complicated and difficult subject, which was confirmed by what Dr. Unwin had said. Notwithstanding all the complication and difficulty, the accuracy aimed at, as mentioned on the first page of the Paper, was a very modest ± 3 per cent. He might have missed it in reading the Paper, but he did not find that the Author mentioned what degree of accuracy he had actually obtained, and if that figure could be given, it would be of value; it was however stated that the variation in a steam-meter was 1 per cent. It was obvious that there must be some reason why the measurement of air, steam, and gas, although effected by such complicated machinery and apparatus, should only be practicable to that low degree of accuracy. Commercial electrical measurements, carried out with much simpler apparatus, could be made accurately within 0.25 per cent. quite easily; with the same care and trouble as had been applied to the measurements mentioned in the Paper, an accuracy of 1 part in 100,000, and even in some cases 1 part in 1,000,000 could be reached. One obvious reason was that in the case of electrical measurements the flow was uniformly distributed over the whole cross section of the conductor, whereas in the cases under discussion it was anything but that; it varied at every point of the cross section, and there were also eddies. Curiously enough, in electrical measurements when high-frequency currents were dealt with, the distribution was no longer uniform; difficulties were then met with in measuring, and the degree of accuracy was nothing like that obtained with ordinary electrical measurements. The Author had nowhere attempted to use the Pitot tube. Judging from some

experiments that had been carried out in America, an account of Capt. Sankey. which he had read, he thought the Author had been very well advised, particularly in the case of pulsating flow. In that connection the Author's investigation in Appendix IX was very interesting, but he noticed it depended on the assumption that the gate had a small moment of inertia, and that the reaction due to the flexible coupling was negligible. Was that actually so? [The AUTHOR replied that practically the reaction was negligible.] The apparatus devised by the Author for measuring gas had been used substantially at atmospheric pressure; in America, where gas had to be measured under very high pressures, considerable errors had been found, one being due to the fact that Boyle's Law no longer held good. In Vol. 38 of the "Journal of the American Society of Mechanical Engineers," there was a long account of extensive experiments that had been made in that connection, and a formula was given which had been devised to take account of the variation in Boyle's Law, when the analysis of the gas was known. In the case of air under pressure, he did not suppose the variation in Boyle's Law was sufficient to cause any error, and, if it did, that error would be taken up by calibration; but, in the case of coal gas under high pressure, a more complicated gas, the departure from Boyle's Law might introduce serious error.

Dr. T. E. STANTON wished to call attention to a point which he Dr. Stanton. considered was of some importance. On p. 129 the Author stated the condition under which the coefficient of discharge was the same for any given meter, whether air or water was passing through it, which was that the pressure difference ($p_1 - p_2$) on the two sides of the orifice (Fig. 17) should vary as the square of the velocity of the flow. Dr. Stanton thought that this condition was not quite sufficient. Thus, if v were the velocity of flow, d the diameter of the orifice, ρ the density of the fluid, ν its kinematical viscosity, and if for any particular meter a curve (Fig. 31) were plotted, of which the ordinates were values of $\frac{p_1 - p_2}{\rho v^2}$, and the abscissæ the corresponding values of $\frac{vd}{\nu}$, then for identical values of the coefficient of discharge—i.e., identical values of $\frac{p_1 - p_2}{\rho v^2}$, such as P M in the figure—the values of $\frac{vd}{\nu}$ (OM) must also be identical. That this might happen, since d was the same for each fluid, it followed that the values of $\frac{v}{\nu}$

Dr. Stanton, should be identical, and since the kinematical value of air was about 13 times the value of that of water at the same temperature, it was evident that for the same coefficient of discharge, the speed of the air should be 13 times that of the water. Thus, if the meter were calibrated with water at 2 feet per second, and were found to have a coefficient of discharge, C , then this would be the correct value to use for air flowing through this meter at 26 feet per second. What the Author called a really good meter was one in which a considerable part of the curve in the figure, such as AB , was a straight line parallel to the horizontal axis, but it was very essential that the limits of speed of flow between which the coefficient of discharge was constant in value should be clearly defined for

Fig. 31.

different fluids. For example, suppose that when calibrated with water the curve for a given meter were found to be horizontal between speeds of flow of 1 foot and 5 feet per second, then the only range of velocities for which this meter should be used for air, so that the coefficient of discharge would be the same as found in calibration lay between 13 and 65 feet per second. That, to his mind, was the all-important thing, and if it were made clear in the Paper, it might possibly prevent some misunderstanding by readers who were not familiar with these conditions. Finally, he might say that he could not agree with the Author's practice of taking the values of the pressure-ratio $\frac{p_1}{p_2}$ as the criterion of the condition

of flow through the orifices. The real criterion of the nature of Dr. Stanton. the flow was the value of $\frac{vd}{v}$.

* * A single reply to the Discussion and to the Correspondence is given at the end of the article.

Correspondence.

Mr. A. A. BARNES observed, with regard to that portion of the Mr. Barnes. Paper which dealt with the discharge through square-edged orifices, that careful attention should be paid to the footnote to Table III, and to the limits which this note placed upon the results given in Appendix III. It was true that the Paper purported to deal with the measurement of air, gas and steam only, but since in the two instances cited reference was made to the coefficients for the discharge of water through orifices, it was important that the differentiation should be observed between the two distinct cases in which an orifice was installed (*a*) in a parallel-sided pipe under pressure, and (*b*) in the side of a tank or at the end of a channel with free discharge into the air. For case (*a*) the Author's value, 0.608, for the coefficient was most useful, as constituting a standard by means of which the quantity of water flowing in a pipe might be ascertained by the insertion of a diaphragm of almost any convenient size, merely by noting the drop in pressure, provided that were taken at the orifice (Case IV., *Fig. 17*). Since it was stated that this value 0.608 was based upon the results of experiments on diameters ranging from $\frac{1}{16}$ inch up to 9 inches, Mr. Barnes would ask the Author to give a few more figures for the larger diameters, similar to those in Appendix III for the very small orifices. With regard to case (*b*) for orifices discharging water into the air, it was found that orifices of different diameters did not possess the same discharge coefficient. The discharge in this case still varied with the square root of the pressure or head, but it also varied with the diameter. Mr. Barnes had found that for circular orifices discharging water into the air, the power of the diameter with which the discharge varied was 1.985, and if *H* represented the head in feet from still-water level to the centre of the orifice, and *D* the diameter of the orifice in feet, then *Q* the discharge in cubic feet per second was given by the relation

$$Q = 3.73 D^{1.985} \sqrt{H}$$