Discussion.

Mr. BRUCE, President, proposed a vote of thanks to the Author for his Paper, which he said contained a carefully prepared series of experiments that would, he had no doubt, be of great value, and lead to an interesting discussion. Mr. Bruce.

Mr. WILLANS wished to add that the figures given in the Paper, with reference to the trials made by Professor Kennedy, Professor Unwin, and Mr. Hartnell, had not been furnished by those gentlemen, but were his own figures from the diagrams which had since been sent to them. He had heard from Mr. Hartnell that the values obtained by him were practically identical with those given in the Paper, and he hoped that those of Professor Kennedy would not be far different. Mr. Willans.

Mr. W. ANDERSON (Erith) said that the Paper was one which it was exceedingly difficult to discuss, as it required pencil and paper to do it justice. The mechanical details of the engine were not in question; the engine, in fact, had not been described. The Paper was a purely theoretical one, and as such it was of great interest. He had been fortunate enough, at the invitation of the Author, to see the works and the manner in which the experiments had been conducted, and he could not speak too highly of the care which he had bestowed upon them in order to produce trustworthy results. One of the most important deductions obtained, he thought, was the complete confirmation, on a large scale, of the accuracy of Regnault's experiments on the latent heat of steam. This had been arrived at by allowing a jet of steam to issue from an ordinary tubular locomotive boiler, and to be condensed in a vessel holding 2 tons of water. Regnault's deductions, like those of most chemists and physicists, had been based upon experiments on a small scale. The Author had sent Mr. Anderson the particulars of the experiments which he had witnessed, and on working them out he found that the results agreed with Regnault's within ½ per cent., which was of course a permissible error in experiments of that kind. He would ask whether the Author could explain the differences of the indices in the formulas for the pressure curves. The usual formula for the saturation curve was $pv^{\frac{17}{16}}$, but that given in the Paper was $pv^{\frac{7}{6}}$, and one by Professor Kennedy $pv^{\frac{10}{9}}$. He did not know whether the equations were made to fit the curves, or on what other account they were made to vary. There was one point about the single-acting engine to which he wished to draw attention, namely, that from the nature of the case, the steam entering Mr. Anderson.

Mr. Anderson. only at the upper end, and the steam-chest being on the top of the cylinder, he should expect to find that the top end of the cylinder was hotter than the bottom end. He should be glad to know if any experiment had been made to ascertain whether that was so in practice or not. It had this important bearing, that the steam, as it expanded from the higher to the lower pressure, had to yield up the heat which was converted into work; and, unless the steam were condensed, it must take that heat from the walls of the cylinder, from the piston and from the trunk; and if one end of the cylinder were hotter than the other, the difference of temperature between the sides of the cylinder and the steam would be less on the whole than in a double-acting engine, in which both ends were equally hot, and where the middle of the cylinder only was probably somewhat cooler than the two ends. In that way there would be less interchange of heat between the steam and the sides of the cylinder, and he should expect more condensation in consequence. That fact was of great importance and especially in an engine which, he thought, would before long attract a good deal of attention, the Hon. Charles Parsons' turbine. There the steam ran in succession through about forty pairs of turbines placed in a line; the steam parted right and left from the centre, and as it worked it fell in temperature and in pressure. The turbine was not a reciprocating engine, and so, as the temperature of the steam fell the temperature of the whole engine fell likewise from the centre, where the work began, to the ends where the steam was given out. Mr. Parsons had told him a fact which had surprised him very much, namely, that although very good duty had been obtained, the steam came out dry, there being no great amount of priming. The quantity of heat in 1 lb. of dry steam increased with the fall of pressure and temperature; it followed, therefore, that the heat converted into work must be taken either from its own condensation, or from the heat conducted along the sides of the engine, that was along the outer case, and along the massive inner revolving part; and he fancied it would be quite possible that the large mass of metal would conduct sufficient heat from the centre to be converted into the work which the engine had to perform. The question as to the temperature of the steam working in an engine was an exceedingly important one, but it was extremely difficult to ascertain. He did not know whether the Author had attempted any experiments with a view to determining what the temperature of the steam really was. He had tried himself, but the results were contradictory on account of the great difficulty of following the steam through the cylinder. The Author evidently had his whole heart in the business, and probably, on some future occasion, when his trials of condensing-engines were completed, the Institution might be able to get some information from him on the point. Mr. Anderson.

Dr. JOHN HOPKINSON wished to point out how easily a precisely accurate formula, to which Mr. MacFarlane Gray's was an approximation when the range of temperature was small, could be deduced from the principles of thermo-dynamics. Consider 1 lb. of water; let it be heated from temperature B to temperature A, let it then be converted into steam at that temperature, let the steam be expanded adiabatically till its temperature had fallen to B, let it then be wholly condensed the temperature remaining constant. The cycle of operations was a perfectly reversible cycle. Heat was taken in at all temperatures from B to A and then at temperature A, and it was given off at temperature B. If a quantity of heat $d\,\theta$ was taken in at temperature θ, and the part of it not converted into work was given off at temperature B, the work obtained from it would be expressed in heat-units $\frac{d\,\theta\,(\theta - B)}{\theta}$. Dr. Hopkinson.

The heat taken in raising 1 lb. of water from θ to $\theta + d\,\theta$ was $d\,\theta$, and the work to be obtained from it was therefore $\frac{d\,\theta\,(\theta - B)}{\theta}$. Hence the work obtained from the heat applied to raise the temperature of the water from B to A was—

$$\int_B^A \frac{d\,\theta(\theta - B)}{\theta} = A - B - B \log \frac{A}{B}.$$

The heat absorbed in evaporating 1 lb. of water at temperature A was $(1438 - 0{\cdot}7\ A)$, and the work obtainable therefrom, working to a lower temperature B, was $(1438 - 0{\cdot}7\ A)\ \frac{A - B}{A}$. Hence the whole work obtainable was $(1438 - 0{\cdot}7\ A)\ \frac{A - B}{A} + (A - B) - B \log \frac{A}{B}$. It was to be remarked that this was not the maximum efficiency working between temperatures A and B; to attain this, the steam must not be wholly condensed at temperature B, but it must be so far condensed that on compressing it adiabatically, it should be raised to precisely the temperature A, when it was wholly condensed. The next point on which he desired to remark was the question of condensation on the surface of the cylinder of a steam-engine. Looking at it from a purely theoretical point of view, assuming that the whole of the loss was by condensation upon the

Dr. Hopkinson. surface of the cylinder, the loss per stroke would be proportional to the surface, proportional to the differences of temperature, proportional to the square root of the conductivity multiplied by the specific heat, and to the square root of the periodic time. Comparing that with the figures in the Paper, it appeared, in many of them, to be more nearly proportional to the time itself. It was only in the simple engine that it was anything like proportional to the square root of the time. He could not attempt to explain what the reason might be. He confessed he felt a certain degree of faith in the assumption that the loss was mainly due to condensation on the surface of the cylinder, and yet that was not consistent with the results put forward by the Author. He did not know what the cause of the discrepancy was; but an approximate idea could be formed of what the actual loss would be from the known conductivity, and known specific heat of iron. Consider a surface of iron, the thickness of the iron being considerable. Let its surface experience a periodic variation of temperature, the periodic time being T, say $\theta = A \sin \frac{2 \pi t}{T}$; if k was the conductivity and c the specific heat per unit volume of the iron, the equation of motion of heat in the iron would be—

$$k \frac{d^2 \theta}{d x^2} = c \frac{d \theta}{d t},$$

θ being the distance from the surface. The solution of this equation, having regard to the surface conditions, was—

$$\theta = A \epsilon^{-\sqrt{\frac{\pi c}{k t}} \cdot x} \sin \left(\frac{2 \pi t}{T} - \sqrt{\frac{c \pi}{k T}} \cdot x \right)$$

the rate at which heat entered the surface was—

$$- k \frac{d \theta}{d k} = k A \sqrt{\frac{2 c \pi}{k T}} \sin \left(\frac{2 \pi t}{T} + \frac{\pi}{4} \right)$$

Integrating this during a half period, the heat entering in one period $= A \sqrt{\frac{2 c k T}{\pi}}$. Use now the known values of c and k, the units being the centimetre and gram—

$$c = 0 \cdot 1124 \times 7 \cdot 8 = 0 \cdot 88$$
$$k = 0 \cdot 199 \, (1 - 0 \cdot 002874 \cdot \theta)$$
$$= 0 \cdot 124 \text{ taking the temperature} = 135^\circ \text{ Centigrade,}$$

whence—

$$\sqrt{\frac{2 \, c \, k}{\pi}} = 0 \cdot 25.$$

To change the units to the lb. and foot, multiply by $\frac{2\frac{1}{5}}{1000} 930 = 2 \cdot 05$, Dr. Hopkinson.

whence $\sqrt{\frac{2\,c\,k}{\pi}} = 0 \cdot 52$; the change in the unit of temperature made no difference in the formula, for the unit of heat was altered in the same ratio. Take as an example the case in Table V for S $\frac{110}{4 \cdot 4}$ at 406 revolutions per minute. The range of temperature was 119° Fahrenheit, hence A = $59\frac{1}{2}$°, and the heat missing per stroke per square foot—

$$= 59\tfrac{1}{2}^\circ \times 0 \cdot 52 \times \sqrt{\frac{60}{406}} = 11 \cdot 8.$$

The water condensed on the surface exposed at cut-off, 0·508 foot, would therefore be 6·0 lbs., leaving 3·4 lbs. to be accounted for by the end of the cylinder. This calculation was, of course, only intended to give an idea of the order of magnitude involved, it was really only a very rough approximation. The end of the cylinder would have a greasy coating which would interpose a great obstacle to the passage of heat. If the whole of the walls were well coated with oil, the loss of heat per stroke would vary as the periodic time. Again, so soon as a small quantity of steam was condensed on the walls of the cylinder, there would be a sensible difference of temperature between the surface of the film of water in contact with the steam, and that in contact with the iron, for the conductivity of water was only one-hundredth of that of iron. The effect of a steam-jacket was to prevent condensation on the surface, but without imparting much heat to the expanding steam on account of the very low conductivity of steam for heat.

Mr. Henry Davey had been curious to calculate roughly the efficiency of the engine as a heat-engine compared with other engines of which pretty certain data existed, selecting the Author's high-pressure engine, working with an absolute initial pressure of 170 lbs. per square inch, the Cornish engine, working with an absolute initial pressure of 40 lbs., a good ordinary compound engine, working with an initial pressure of 75 lbs., and the Author's condensing engine, working at 170 lbs. If they were all perfect heat-engines their relative efficiencies would approximately vary as the figures 18, 23, 26, and 32. The commercial efficiencies (the consumption of feed-water) varied as 19, 20, $17\frac{1}{2}$, and 15. Taking the Cornish engine, working with 40 lbs. absolute pressure, with a consumption of 20 lbs. of feed-water (which he was convinced that many Cornish engines did with such pressure), and Mr. Davey.

Mr. Davey. contrasting with it the efficiency of the Author's engine as a heat-engine, he found that the latter was superior by 5 lbs. of feed-water per hour, which was remarkable. The Cornish engine, compared with the compound as a heat-engine, was practically the same; but contrasting the Author's condensing engine with the Cornish or the compound as a heat-engine, it was inferior to them by $\frac{1}{2}$ lb. of feed-water per hour. But the most remarkable feature of all was that, comparing the result obtained with the Author's condensing engine with his non-condensing engine, he found that, to put it on a par as a heat-engine, he must bring the consumption down to 11 lbs. instead of 15 lbs. If he brought it to 11 lbs., it would be a most remarkable result indeed. The Author had suggested that the results obtained might very largely arise from the non-retention of water in the cylinder. The construction of the engine was such that the exhaust port was immediately on the top of the piston, and the steam acted only on the top surface of the piston, so that, if any water lodged on the piston, it was during the exhaust in an excellent condition for being swept away; and it could be readily supposed that very little water indeed would lodge in an engine constructed in that way. He was strongly convinced that to a very large extent want of economy in ordinary steam-engines, especially in horizontal engines, arose from the lodgment of water in the cylinder; so much so, that he had been in the habit of applying automatic drain-cocks to insure the water being got rid of from the cylinder; but, not being able to obtain very satisfactory results, he had in later engines put in mechanically-moved valves, small slide-valves, to act as drain-cocks. In the case of compound engines, the pipes from the two ends of the cylinder were brought to a slide-valve, worked by mechanical means from the engine, having the motion of the exhaust valve and discharging into a steam-trap. A similar arrangement was provided on the low-pressure cylinder directly into the discharging condenser.

Capt. Sankey. Captain H. R. SANKEY, R.E., said that in reference to the statement in the Paper, that he had tested the Author's planimeter by running it round a square scored on a sheet of copper, he had since tested it to ascertain the errors of eccentricity and graduation of the rolling-wheel, and had found them to be quite negligible. He had made an experiment to ascertain the rate of cooling of the mass of water used in the calorimetric trials. Steam was shut off at about 3.10 P.M., and the first readings were taken about 3.20 P.M.; another set at 3.35 P.M., and so on at intervals. The readings were taken at the bottom, at the top, and at the middle of the water, and at first they did not agree. As the time increased, however, the readings became more and more equal, showing that the temperature of the mass of water had become more uniform. The results obtained, which were plotted in Plate 4, Fig. 3, pointed to two things: first, that the rate of cooling was very slow, and within the limits of the experiment varied sensibly as the time; and secondly, that for such calorimetric measurements it was advisable not to commence taking the temperature for some little time after the steam had been blown into the water. With regard to the comparison of Mr. Gray's thermometers referred to in the Paper, he might mention that the Ordnance Standard thermometer was used for this purpose, a thermometer which was calibrated, and the errors determined by Colonel Clarke, R.E., F.R.S., in 1865. It was only necessary to state that those determinations were made by Colonel Clarke, to convince every one that they were correct. Captain Sankey had, however, re-determined the error of the freezing-point, and found that it had increased from 0°·42 in 1865, to 0°·70 at the present time, making a variation of 0°·28 Fahrenheit in twenty-three years. The standard was laid horizontally in a trough made for the purpose, provided with paddles to keep the water in motion. The two more delicate of Mr. Gray's thermometers were placed at an angle of about 70°—the angle at which they were held during the calorimetric trials. This was an important precaution as would be seen on referring to the comparisons marked * and 1 in Tables A and B. The results of the comparison were as followed (see Tables A, B, C, pp. 196, 197, 198). From the dimensions on p. 140 it would be seen that the volume of the low-pressure cylinder was twice that of the high-pressure cylinder; therefore for a ratio of expansion of 2 there would be no expansion of steam in the high-pressure cylinder; hence there would be no range of temperature and no initial condensation in that cylinder. Moreover, the Author had stated that he thought the amount of leakage was very small; therefore the missing quantity of water for a ratio of 2 ought to be sensibly 0. Taking Plate 3, Fig. 7, in which the ratios of expansion were plotted as abscissas, if the fair line drawn through the experimental points for the missing quantity were produced, it would be found that it intersected the horizontal axis exactly at the point 2. Again in the compound series, in which the expansions were made according to the law $\frac{P}{25}$ = ratio, the pressure would have to be 50 to give a ratio of 2; and it would be found, on referring to Plate 3, Fig. 4, that the fair line passed through "50." In Plate 3, Fig. 5, Capt. Sankey.

Capt. Sankey.

TABLE A.—COMPARISON of MR. GRAY'S THERMOMETER (HICKS, NO. 359,753), against the ORDNANCE STANDARD (CASELLA, NO. 3,142).

—	Reading of Standard.	Error of Standard.	Corrected temperature.	Reduced to Centigrade.	Reading of Mr. Gray's Thermometer.	Error of Mr. Gray's Thermometer.
February 29th, 1888. Afternoon.	43°·60	0°·75	42°·85	**6°·03**	6°·08	+0°·05
	44·80	0·76	44·04	**6·69**	6·70	+0·01
	45·88	0·77	45·11	**7·28**	7·30	+0·02
	46·90	0·77	46·13	**7·85**	7·90	+0·05
	48·34	0·77	47·57	**8·65**	8·73	+0·08
	49·20	0·77	48·43	**9·13**	9·22	+0·09
	49·20	0·77	48·43	**9·13**	9·21	+0·08
	50·18	0·78	49·40	**9·67**	9·78	+0·11
March 1st, 1888. Morning.	50·08	0·78	49·30	**9·61**	9·70	+0·09
	50·68	0·78	49·90	**9·94**	10·02	+0·08
	51·20	0·78	50·42	**10·23**	10·34	+0·11
	51·74	0·78	50·96	**10·53**	10·63	+0·10
	52·22	0·78	51·44	**10·80**	10·91	+0·11
	52·65	0·78	51·87	**11·04**	11·16	+0·12
	53·12	0·78	52·34	**11·30**	11·415	+0·115
	53·60	0·78	52·82	**11·57**	11·68	+0·11
	54·10	0·78	53·32	**11·84**	11·95	+0·11
	54·52	0·78	53·74	**12·07**	12·21	+0·13
	54·50	0·78	53·72	**12·06**	12·18	+0·12
March 1st, 1888. Afternoon.	54·80	0·78	54·02	**12·23**	12·36	+0·13
	56·00	0·79	55·21	**12·89**	13·00	+0·11
	57·62	0·79	56·83	**13·79**	13·91	+0·12
	59·14	0·80	58·34	**14·63**	14·72	+0·09
	60·50	0·80	59·70	**15·39**	15·435	+0·045
	62·50	0·80	61·70	**16·50**	16·56	+0·06
	64·42	0·80	63·62	**17·56**	17·62	+0·06
	65·00	0·80	64·20	**17·89**	17·93	+0·04
	65·56	0·80	64·76	**18·20**	18·26	+0·06
	65·55	0·80	64·75	**18·19**	18·345*	+0·155

For position of thermometers during comparison, see Table B.

* Horizontal.

TABLE B.—COMPARISON of MR. GRAY'S THERMOMETER (HICKS, No. 359,754), against the ORDNANCE STANDARD (CASELLA, No. 3,142). Capt. Sankey.

—	Reading of Standard.	Error of Standard.	Corrected Temperature.	Reduced to Centigrade.	Reading of Mr. Gray's Thermometer.	Error of Mr. Gray's Thermometer.
Afternoon, March 2, 1888.	70°·70	0°·82	69°·88	**21°·04**	21°·00	−0°·04
	71·75	0·83	70·92	**21·62**	21·58	−0·04
	72·20	0·83	71·37	**21·87**	21·85	−0·02
	72·67	0·84	71·83	**22·13**	22·11	−0·02
	73·11	0·85	72·26	**22·36**	22·39	+0·03
	73·54	0·85	72·69	**22·60**	22·60	0·00
	73·91	0·86	73·05	**22·81**	22·815	+0·005
	74·27	0·86	73·41	**23·01**	23·00	−0·01
	74·73	0·86	73·87	**23·26**	23·26	−0·00
	75·26	0·86	74·42	**23·57**	23·59	+0·02
	75·80	0·86	74·94	**23·86**	23·91	+0·05
	76·31	0·86	75·45	**24·14**	24·20	+0·06
	76·31	0·86	75·45	**24·14**	24·19	+0·05
	76·80	0·86	75·94	**24·41**	24·46	+0·05
	77·30	0·86	76·44	**24·69**	24·74	+0·05
	78·30	0·86	77·44	**25·25**	25·30	+0·05
	79·20	0·85	78·35	**25·75**	25·78	+0·03
	80·11	0·85	79·26	**26·26**	26·30	+0·04
	81·11	0·85	80·26	**26·81**	26·89	+0·08
	81·12	0·85	80·27	**26·81**	26·87	+0·06
	81·98	0·84	81·14	**27·30**	27·35	+0·05
	83·14	0·84	82·30	**27·94**	28·03	+0·09
	83·11	0·84	82·27	**27·92**	28·01	+0·09
	84·08	0·83	83·35	**28·47**	28·55	+0·08
	86·34	0·81	85·53	**29·74**	29·83	+0·09
	88·00	0·80	87·20	**30·67**	30·73	+0·06
	88·85	0·80	88·05	**31·14**	31·19	+0·05
	88·84	0·80	88·04	**31·13**	31·27[1]	+0·14
	88·50	0·80	87·70	**30·94**	31·10[1]	+0·16

Standard placed horizontally and read by microscope.

Mr. Gray's thermometer held at an angle of about 70°, being the angle at which it was read during calorimetric trials.

[1] Mr. Gray's thermometer placed horizontally.

Capt. Sankey.

TABLE C.—COMPARISON of Mr. GRAY'S THERMOMETER (HICKS, No. 35,259), against the ORDNANCE STANDARD (CASELLA, No. 3,142).

—	Reading of Standard.	Error of Standard.	Corrected Temperature.	Reduced to Centigrade.	Reading of Mr. Gray's Thermometer.	Error of Mr. Gray's Thermometer.
	36°·90	0°·70	36°·20	**3°·33**	2°·50	+0°·17
	38·83	0·72	38·11	**3·39**	3·50	+0·11
	42·12	0·74	41·38	**5·21**	5·3	+0·09
	41·23	0·74	40·49	**4·72**	4·8	+0·08
	46·90	0·77	46·13	**7·85**	8·0	+0·15
	50·18	0·78	49·40	**9·67**	9·8	+0·13
	49·50	0·77	48·73	**9·29**	9·48	+0·19
	52·22	0·78	51·44	**10·80**	11·0	+0·20
	54·52	0·78	53·74	**12·08**	12·3	+0·22
	53·80	0·78	53·02	**11·68**	11·85	+0·17
	60·67	0·80	59·87	**15·48**	15·71	+0·23
	65·00	0·80	64·20	**17·89**	18·05	+0·16
	66·83	0·82	66 01	**18·89**	19·1	+0·21
	73·91	0·86	73·05	**22·80**	23·0	+0·20
	81·11	0·85	80·26	**26·81**	27·0	+0·19
	88·50	0·80	87·70	**30·95**	31·2	+0·25

Both thermometers placed horizontally in water-tank.
Read by microscope with diagonally divided eye-piece.
Freezing-point test of Mr. Gray's thermometer, error – 0°·1 Centigrade.

where the law was $\frac{P - 10}{25}$, P would be equal to 60 absolute, for a ratio of expansion of 2; and the fair line passed through "60." These results were a strong confirmation of the accuracy of the experiments that the Author had made, considering that the missing quantity of water was determined from the main measurements in the trials. In conclusion, he wished to ask the Author if he could supplement the results given at the end of the Paper with regard to the efficiency trials with the Siemens dynamo, by stating what was the smallest consumption of water per electrical HP. he had yet attained?

Mr. Beaumont. Mr. W. W. BEAUMONT might be permitted to say, not in a spirit of contradiction to Mr. Anderson's statement that the Paper was a theoretical one, that he thought the Paper highly practical, and that if there was any part in which it was a little unsatisfactory it

Mr. Beaumont

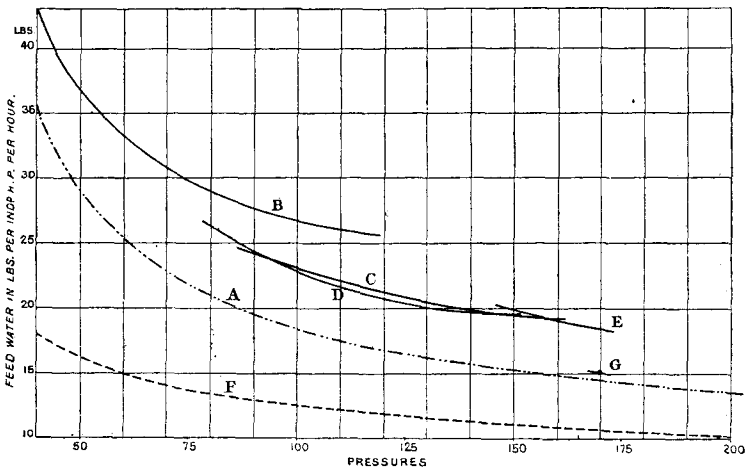
was in the theoretical. It dealt with what had been for from thirty to one hundred years the main difficulty with constructors of steam-engines, as to the reasons for the great difference between the quantities of steam used and the steam that theoretically an engine ought to use. The Author had made such a large number of experiments, and made them with such great care and accuracy, giving all the detailed results, that the Paper was of great value. It was commonplace to say that Watt understood that the missing quantity was one that might be lessened by certain means; and also that the theoretical deductions of some investigators much later than Watt, did not teach as much as they were supposed to do, when it was said that the chief object for engine constructors should be to secure the greatest possible range of expansion, or rather to carry expansion to the greatest possible limit. Mr. D. K. Clark, M. Inst. C.E., amongst others particularly directed attention, as the result of numerous experiments with high-pressure locomotive engines, to the large quantity of steam condensed, or got rid of in some way, without giving useful effect when the steam was expanded through too great a ratio. Rankine, perhaps, more than any one else had elucidated the thermo-dynamics of the steam-engine. At the same time, it might perhaps be said that a little too much adherence to $\frac{T - T_1}{T}$ had prevented such progress being made as might otherwise have followed. It had been for several years understood as a practical fact, though to some extent contradicted by many persons most interested in the science of using steam, that the most economical results had been obtained with engines when the range of temperature in one or in each of several cylinders was reduced to a small one. It was clearly shown in the Paper that the range of temperature in a cylinder was one of the most important things to consider. The Paper gave every figure in connection with about one hundred and twenty trial runs; and, apart from the curves showing the quantity of steam that an engine should use if it did what it ought to do according to thermo-dynamic laws, and if it were not made of the material that it was made of, there were curves showing at a glance the exact quantity that the engine had used under different conditions. The Author, in the Tables, had given first the experiments relating to a simple engine; he had given the quantity of steam used under the best condition with a simple engine with a certain range of expansion; and he had shown what the efficiency was with that range, continuing the various ranges of expansion up to 4½, and then stating the efficiency. He had shown

Mr. Beaumont. that with a very small range of expansion the efficiency of the engine was something like 80 per cent. With that large efficiency the steam used by the engine was 40 lbs. per indicated HP. With a larger range of expansion, but a much higher pressure, he used instead of 40 lbs. of feed-water only 26 lbs.; but the efficiency of the engine was only 70 instead of 80 per cent. In other words, the engine, although giving 1 HP. for every 26 lbs. of feed-water, did not give as good an account of the quantity of heat supplied to it with the steam as when it was using 40 lbs. of water. The efficiency, therefore, was not exactly a satisfactory index of the value of the engine and of the use of steam under those conditions. It simply showed that the steam at the higher temperatures was, from a thermo-dynamic point of view, used much less economically than at the lower temperature. He was referring to that point because he gathered that the subject under discussion was not the construction of the steam-engine, except perhaps as far as concerned the high speed at which the engine ran and the advantages so obtained, but rather the performance of the steam in the engine. Turning to the compound engine used in that way, it appeared that the efficiencies and the quantities of water varied very much in the same manner. One point had been much discussed, which was still occupying the attention of steam-engine users, and especially the builders of marine engines, who were fighting the battle of compound engines against triple-cylinder engines and quadruple engines, namely, the question of cylinder condensation, which was much wrapped up in all those points. With the simple engine the quantity of water that remained in the cylinder and was not re-evaporated, the missing water at the end of the stroke, was roughly 18 per cent.; in other words, 18 per cent. of the water, supplied to the engine in the form of steam, had to be re-evaporated during the exhaust stroke of the engine, and passed away as wet steam. This led to a point on which, perhaps, the Author could throw some light; he found that in the compound engine, although the efficiency increased greatly and the quantity of feed-water was very much reduced, the efficiency being increased with the reduction of feed-water, the missing quantity in the low-pressure cylinder at the end of the stroke was about the same as at the end of the stroke in the simple engine. So that it would appear that the total condensation was much the same in compound and in simple engines. The same thing might be said concerning the triple engine. It would seem that the advantage gained by the compound and the triple engine was due to the smaller quantity of

Mr. Beaumont. condensation per cylinder, owing to the smaller range of temperature in each, and to the efficient use in the second and third cylinders of the water re-evaporated in the first and second cylinders during their exhaust strokes. He might perhaps also venture to say that the greater range of pressure, with given range of temperature at the higher pressures, had a good deal to do with this economy. It was clear that the quantity of steam that could be condensed in the cylinders of an engine could only be about the same as that which was re-evaporated, minus, of course, the quantity represented by the heat turned into work, and by the losses due to radiation and conduction. If that was the case, it led to the question where was the balance to be found between the number of cylinders that must be used in an engine, and the economical value to be obtained by reducing the range of temperature in any one cylinder, so as to get the best results to which the experiments pointed? For illustration of his remarks he had calculated the curve for the theoretical quantity of water necessary for condensing-engines, and he had repeated the Author's curve, Plate 3, Fig. 1. Curve F (Fig. 11) was the quantity of water that it might be expected the condensing-engine would use, on the same assumptions as those upon which it might be expected a non-condensing engine would use water, in proportion to the curve A. The Author had only made one experiment with his engine as a condensing engine; but that experiment had been carefully carried out. The quantity of water used by the condensing engine was represented by the point G, namely, 15·1 lbs. per indicated HP. Theoretically, that engine, he thought, should use 10·6 lbs. per indicated HP., that was, the engine using steam at 170 lbs. on the square inch, at which the triple engine showed an efficiency of 77 per cent., and a very small consumption of steam, had an efficiency of 70 per cent., which was somewhat lower than the others. It was commonly said that the reason for getting so much smaller effect from the adoption of a condenser than would be expected by reference simply to the number of lbs., or the proportion of atmospheric pressure brought into use, was that the influence of the condenser was brought to bear. Taking the whole gist of the experiments, that was seen to be true. During both strokes of the low-pressure cylinder of a triple engine or a compound engine, or any engine connected with a condenser, the evaporation might be large, and during the exhaust stroke of the engine with a condenser the range of temperature was very great. With the engine used in the experiments, taking a back pressure of 5 lbs. the temperature was only 162°·4, so that the range in

Mr. Beaumont. the low-pressure cylinder with a condenser was much larger than that in either of the other cylinders according to the Author's experiments. The influence of the condenser simply meant that during the exhaust stroke of the engine the evaporation was very rapid indeed, and the loss of heat by the cylinder correspondingly great, and it might be the cause of a very large proportion of all the loss represented in the diagram. Taking any of the Tables, the water missing at the end of the stroke was, as he had said,

Fig. 11.



Non-Condensing Engine.

A = Feed theoretically required according to $W = \dfrac{\frac{33,000}{770}}{\left(\frac{1,438 - \cdot 7\,T}{T} + \frac{T - t}{T + t}\right) T - t}$

B = Feed-water used by Willans' simple engine.

C = Actual results. Feed used with C $\frac{P - 10}{25} = R$.

D = Feed used with C $\frac{P}{25} = R$.

E = Feed used by triple engine series T′ $\frac{P - 10}{25} = R$.

Condensing Engine.

F = Feed theoretically required, according to $W = \dfrac{\frac{33,000}{770}}{\left(\frac{1,438 - \cdot 7\,T}{T} + \frac{T - t}{T + t}\right) T - t}$

G = Feed actually used by engine working with condenser.

18 or 19 per cent. When then the new steam came into the cylinder that had been so lowered in temperature by effecting that large amount of evaporation, it could be easily understood that there was a great deal to be done in heating up the cylinder again.

Mr. J. G. Mair said that, unfortunately, the engine was not under discussion, or he should have desired to say a few words about it. However, a great point was gained in having the fact brought before the Institution, that an engine used steam and not coal. He wished to ask why the Author had adopted 770 as the foot-pounds of work equivalent to an English thermal-unit. Joule's value was about 772·5, and in America, Professor Marks, with a 45-HP. engine, churned about 2 tons of water per hour, and he found that it came to 772·8. He believed that Mr. Cowper had made experiments on a very large scale, and it was to be hoped that he would give the meeting the benefit of his experience. The first point in the Paper was the authenticity of the trials. He would first refer to the question of the priming in the steam, and would ask how much water was evaporated from the boiler per square foot of surface, so that he might have an idea as to whether the boiler was worked hard or not? The priming as stated was certainly small; but he did not think it was less than it ought to be in a boiler slowly worked. The experiments made with the large calorimeter were most valuable. Notwithstanding the exceeding difficulty of ascertaining the amount of priming in the steam, it had been measured by Hirn, and many others; he had tried to measure it with a calorimeter containing only 50 or 60 lbs. of water, and the results were most anomalous. Sometimes he had saturated steam, and sometimes superheated, so that the experiments were futile. It appeared to him that trying to measure priming in a calorimeter on a small scale was like taking a 2-foot rule and measuring the length of a standard mile to ascertain if the 2-foot rule was correct. In America, engineers were working on the opposite tack. They took the amount of moisture in the steam as it came over, and measured the amount of heat required to dry that steam, and by so doing they were able to ascertain how much short the steam was of having the total number of heat-units that it ought to have if it were all dry steam. That system, he thought, ought to be tried in this country, as it was certainly going the right way to work. It was a constant calorimeter, and the readings would show how much moisture was coming over at any time during a trial. The Germans had used Glauber salts in the boilers, and had tested the steam for chloride of barium, and also for sodium, with a spectroscope, but that was not a very practicable method for engineers. It would, he thought, be a great boon if somebody could discover a simple form of calorimeter by which to ascertain the amount of priming in the steam. With regard to the

Mr. Mair.

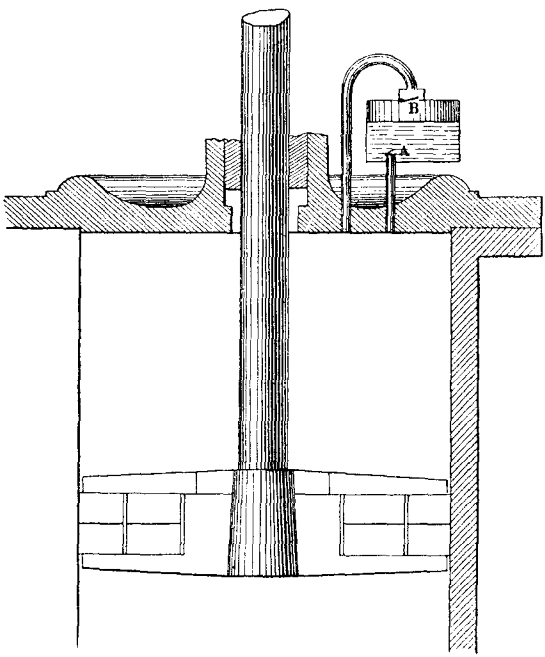
Mr. Mair. question of the accuracy of the indicator diagrams, he was glad the Author had used the American Crosby indicator, which was undoubtedly the best form of indicator for high speeds. It was an indicator that had an increasing amount of tension on the string, in order to make up for the inertia of the drum and moving parts. Although, owing to the small scale, he should rather hesitate to accept the ordinates, as being perfectly correct to a very small percentage, he did not think that the total integration of the diagrams could be far wrong when taken out as they had been. If they were at all wrong, it was in all probability in the Author's favour, and possibly the engine had done better than appeared from the Tables. The Author's experiments formed an exceedingly valuable series, and he believed that they were the most complete on record. The efficiencies, as stated, were rather misleading; for instance, taking the simple trial S $\frac{40}{1 \cdot 57}$ and S $\frac{100}{4}$, the former efficiency was given as 81·08 per cent. and the latter as 67·1 per cent., while the engine used 42·76 lbs. of feed-water with the higher efficiency at 27·8 lbs. with the lower. The efficiencies could also be calculated according to Carnot's well-known law $\frac{\tau_1 - \tau_2}{\tau_1}$; but it must be borne in mind that the smaller the range of temperature through which the engine worked the more easy was it to approach this theoretical efficiency, so that comparisons on this basis were also misleading. Instead of comparing the feed used with that required theoretically, he thought it was better to use an efficiency obtained by comparing the heat-units expended by the engine per HP. with the thermal equivalent of a HP. It was a curious fact that the engine when working compound should give the best result with only 5·6 expansions, and when working triple 6·4 expansions, 130 lbs. of steam being used in the first and 170 lbs. in the latter. As a manufacturer of steam-engines, he dared not have made an engine running with so few expansions, because he did not think that he could have got a good result; he would have wanted to make more expansions, and the only reason why a small number was good in this case must be attributed to the very large number of reciprocations which the engine made. He would like, however, to have seen the engine compared by heat-units in place of lbs. of feed, as 1 lb. of feed required more heat to convert it into steam the higher the boiler pressure. The Author had stated in the Tables the amount of heat missing per stroke at the point of cut-off. He should have been glad if the Author had given the square feet of surface per lb. of steam that

Mr. Mair.

passed through, because the value of the surface had direct reference to the number of lbs. of steam that passed over it. The larger the number of lbs. of steam that passed over it, or, what was equivalent, the smaller the number of expansions made, the less was the condensation. One of the main points of the question was, he thought, that no one could determine *à priori* what delivery there would be; or practically how much steam an engine would use; and the question was, why was this so? The reply was, because an unknown quantity of steam was condensed during admission. It was condensed during admission and up to cut-off, and possibly longer. The Author agreed with Dr. Zeuner that the reason why initial condensation took place was that water remained after exhaust and absorbed heat from the incoming steam, thus causing condensation. He totally differed from that view, for he did not believe that in the Author's engine any water remained after exhaust. The engine stood vertical, the whole of the exhaust passages were at the bottom of the cylinders, and the tendency was for the water to be swept away; so that he did not think it was possible for water to remain after exhaust. The Author had given the quantities of water that would have to remain in the cylinder after exhaust to act in the same way upon the steam during admission as the iron cylinder walls would do. Those quantities were certainly small, but it should be borne in mind that the engine was small. He wished to compare those quantities with the amount passing through the cylinder in the shape of feed. Taking two of the experiments haphazard, and beginning with Table VI, C $\frac{90}{3 \cdot 2}$, the quantity of water supposed to remain in the cylinder was absolutely six times the amount of the feed-water that passed through per stroke, and taking the quantity of water shown by the indicator at the end of expansion, the quantity that the Author supposed to remain in his engine was sixty times its weight. Again, in Table VII, C $\frac{60}{4}$, the quantity of water supposed to remain in the cylinder per stroke was four times the amount of the feed-water, and thirty times the amount of the water remaining in the cylinder at the toe of the diagram. He did not think that such a proportion could remain in the cylinder, and he might be permitted to refer to an experiment shown by Mr. Hirn on that subject. He had placed on the cylinder-cover a small vessel which could be partially filled with water, and led from the cylinder two pipes, one pipe through the bottom of the vessel, and one to its top as shown in Fig. 12. At the ends of the pipes inside the vessel non-return

Mr. Mair. valves were placed, so that the steam on admission to the cylinder opened valve A and passed into the water, valve B being then closed; as expansion proceeded valve B opened and valve A closed, and the water was found to evaporate and escape from the vessel. That, he thought, was proof that water could not remain in any passage or pocket in the steam-engine after the turn of one stroke. But it was argued by the Author that the water could remain in the passages on the surface of the cylinder and on the surface of the piston. It was stated in the Paper that taking the figures

FIG. 12.



there given, and dividing them by the specific heat of iron, a rough indication would be given of the depth to which the action might penetrate the metal. Now the depth to which the action could penetrate the metal relative to the depth it could penetrate water was dependent upon something besides the specific heat; it was dependent on the conductivity. Mr. Hirn had stated that iron had forty times the conductivity of water, and Dr. Hopkinson had said that the conductivity of iron was one hundred times that of water. Was it probable, then, that water would remain on those surfaces? There was no doubt that the figures respecting the conductivity of iron showed that iron could and did very quickly absorb and part with heat, and he thought, therefore, that it was due to the iron and not to water that condensation was produced. Mr. Hirn had not only given experiments based upon absolute trials of steam-engines, but he and the late Mr. Hallauer had shown that during admission a certain amount of heat passed from the steam into the cylinder walls, that during expansion a certain portion was absorbed by the steam from the walls, and that during exhaust a certain amount was also absorbed from the walls. Those three quantities formed a balance. Taking for example, a certain percentage of the water coming through the cylinder, the heat absorbed from the walls during exhaust would evaporate a large part of it; and the result was that, at the point of compression, instead of having the same percentage of moisture there was always far less; in fact when the exhaust valve was opened the pressure and temperature suddenly fell, and if there was any water on the cylinder walls it would have a temperature far above that of the exhaust, and it must flash into steam; in doing so heat was taken from the cylinder walls, their temperature fell, and the fresh steam entering from the boiler was condensed; therefore at the end of exhaust there was, practically, no water in the cylinder. Mr. Hirn had also made experiments with the lubrication of cylinders. He had put into the grease that went into the cylinders a quantity of fuchsine or aniline colouring matter, which was passed at once into the condenser. It was well known that if a large quantity of oil was put into a cylinder it did no more good than a small quantity, because it went straight through to the condenser. If grease, which was more adhesive than water, could not stay on the walls, he did not believe that water could stay here. With every respect, therefore, for the Author's opinion, he totally disagreed with him on the point that water could remain in the cylinders of his engine after exhaust. He should like to discuss the action of the wall in very low-speed engines, but that point was not the subject of the Paper. He thought that if the Author would read the Papers written by Mr. Hirn and the late Mr. Hallauer on that subject, in refutation of Dr. Zeuner's statements that water remained, he would alter his views on the question of initial condensation. Mr. Mair.

Professor W. C. Unwin observed that the Paper was a remarkable one, for the extreme care with which every detail that could affect the results of the experiments had been examined and brought into account. He did not think that in any previous Professor Unwin.

Professor Unwin. experiments the feed-water had been accurately weighed. In another respect also, for theoretical purposes, the experiments were extremely valuable, because the resistance was almost absolutely a constant one, a dynamo being used for the purpose. There was, however, one respect in which he felt somewhat discontented with the apparatus used in the experiments. The Author had spoken, not too highly, in praise of the Crosby indicator; and for all practical purposes it was as good an instrument as could be desired; but he was inclined to think that, for a purely scientific purpose, it was desirable to have an indicator giving diagrams five times the area of the Crosby diagrams. He should like also to have an instrument that would not admit of the just possible error arising out of the use of slack and elastic strings. But it was not because of the care taken with the details of the experiments that they were chiefly remarkable, but because the Author had throughout put perfectly definite questions to be answered, and had obtained perfectly definite answers. He had made experiments as to the ratio of expansion, speed, pressure and the like, and he had an absolutely definite series of answers to the questions he had put. The Author had been good enough to allow him to see the carrying out in all its details of one of the trials. He had plotted on the diagrams for that trial the saturation-curves corresponding to the amount of steam in each cylinder (Plate 4, Fig. 6). The saturation-curve was not quite a continuous one, because a little water was taken out of each of the reservoirs, and therefore the quantity of steam present in each cylinder diminished from high pressure to intermediate, and from intermediate pressure to low. The saturation-curve was extremely close to the indicated expansion curve; indeed he had never seen any so close to the expansion-curves of steam. But the special point on which he desired to speak was this: The broad result of the Author's experiments was that the cylinder-surface had very little effect on the amount of initial condensation. That appeared to be in very strong opposition to Mr. Hirn's view, that the initial condensation was directly due to the walls of the cylinder; and it had driven the Author to the theory that water in the cylinder played a much more important part than was ordinarily assigned to it. He had expressed himself very cautiously and correctly on the subject, and Professor Unwin only wished to protest against a possible extension of his meaning, which he thought would be erroneous. With regard to cylinder-surfaces, many rules had been promulgated in which the initial condensation had been made proportional to the area of the

Professor Unwin.

surface exposed during admission; but it ought to be borne in mind that cylinder-surfaces were not all of the same kind. There were bright surfaces and black surfaces, vertical and horizontal surfaces; surfaces which were heated up at the beginning of the stroke, and surfaces which were only exposed late in the stroke; surfaces enclosed in steam, and surfaces exposed on one side to radiation; and it was quite impossible that all those different kinds of exposed surfaces could act in the same way, and that there could be any rule for the amount of initial condensation by taking the mere total cylinder-surface in the reckoning. It was quite possible that the surfaces of the admission ports, and so on, which were exposed *initially to the steam*, might be evaporating steam at the same time that the portions of the cylinder exposed later were condensing steam; and it followed from that, that there might be some surfaces in the cylinder which had become dry before the release began, while others were still wet, and those surfaces which had become dry would be practically, as far as any transmission of heat went, out of the cylinder; they would have no effect upon the transmission of heat. He was disposed to think that the Author's experiments showed that a portion of the cylinder-surface became dry early in the stroke under the conditions in which he had been working, rather than that the metal of the cylinder was of so little consequence as he seemed to imply. On that point he should like to read a single sentence from the Paper: "These facts seem to suggest that the action of the cylinder-walls is not seriously felt at high rotative speeds, the cause of the condensation being rather to be looked for in the alternate heating and cooling of a small body of water retained in the cylinder." He wished to point out what apparently had not occurred to the Author, that a body of water in the cylinder taken by itself would not account for the initial condensation and subsequent re-evaporation. If, in the Willans engine, there was a convenient pocket with the necessary quantity of water, that water would be heated up by condensing the steam at the beginning of the stroke, and it re-evaporated the steam at the end of the stroke. Supposing 1 lb. of water to be in the pocket at the beginning of the stroke, that it condensed 1 oz. of incoming steam, and that during the exhaust that 1 oz. of steam was re-evaporated, he wanted to point out that steam was condensed during admission at a pressure which was nearly the pressure of the boiler, and re-evaporated during the exhaust at a pressure which was nearly the pressure of the condenser; the total heat of the steam at the higher pressure was greater than the total heat of the

Professor Unwin. steam at the lower pressure; and consequently the 1 oz. of steam condensed at the beginning of the stroke, and re-evaporated during the exhaust, would require less heat during re-evaporation than it gave up in condensation; consequently it would leave behind it a small quantity of heat in the 1 lb. of water in the cylinder; not a large quantity, 2 or 3 per cent., perhaps, of the heat which it gave up at first. Still, if the process of condensing steam at high pressure and re-evaporating at low pressure went on, the whole of the 1 lb. of water initially present would gradually be evaporated, in a not very large number of strokes. Thus the water, apart from anything else, would not account for initial condensation, and subsequent re-evaporation. The very interesting experiment of Mr. Hirn, to which Mr. Mair had alluded, was no doubt to be explained in that way. The water, which he found to evaporate gradually, was heated by steam condensed during admission and during the exhaust this was re-evaporated at a lower temperature. But though water in the cylinder could not of itself account for the initial condensation and subsequent re-evaporation, a very little change in the conditions would make it sufficient to have a very powerful effect. It was only necessary to replace the small amount of water in the original 1 lb. which was lost during each stroke, being evaporated gradually by the small fraction of heat left after evaporating the steam condensed. This might be done by priming water brought over from the boiler. Or, if there was no priming, the original 1 lb. of water would remain, if there was taken from it, each stroke, the 2 or 3 per cent. additional heat which it had retained from the steam that it had first condensed and re-evaporated. Both those things were important; a very small amount of priming coming over would keep up the quantity of water in the cylinder, and would make that water as active as the metal of the cylinder, in serving as a medium for the transfer of heat from the initial steam to the exhaust steam, without doing any work in the cylinder; or a small transfer of heat from the water to the cylinder would enable the water to continue in the cylinder and act in the same way. That was important in two ways. It was pretty certain that some anomalies in experiments had arisen from priming water coming over. It might be seen how powerful was its effect, if it was deposited among the water which was on the cylinder-walls; also how it was that a small amount of heat transferred from the jacket to the cylinder prevented the absorption from the water on the cylinder of the small excess of heat which it received from the steam. Thus a small amount of heat transferred from the jacket might be powerful in preventing the water accumulating in the cylinder, and causing its re-evaporation into the condenser, rendering the cylinder dry. He did not know whether that was the exact explanation. It had only occurred to him during the past week, and he had not thought it out carefully enough to put it forward as a belief; but he did not see any fallacy in it, and he was inclined to think that it was the explanation of why the water on the cylinder-wall either might or might not be a very powerful cause of initial condensation. The Author had shown one way by which the noxious action of condensation in the cylinder might be very much reduced. He had shown that by higher pressures, by faster speeds, by dividing the stages of expansion, it might be reduced to an exceedingly small amount. Unfortunately, those high speeds and pressures could not in all cases be adopted. What was to be done? He would ask whether another path by which the same result might be attained had not been too soon abandoned. At one time superheating the steam had been tried, which was a very powerful means of reducing initial condensation. In the celebrated experiments of Mr. Hirn in 1877, experiments conducted under identical conditions with a beam-engine, the initial condensation was 35 per cent. with saturated steam, and that was reduced to 6 per cent. when superheated steam was used at about 200° of superheating. Of course, when superheated steam was used it gave rise to trouble. The superheaters were things which, with steam on one side and fire on the other, burnt out very fast, and they were almost incapable of proper regulation, so that it was not surprising that in those trials superheating proved too troublesome to be continued. On looking at the fact that a very small amount of heat was necessary to superheat steam, he was not sure that some other way might not be found of giving heat to the steam in a perfectly regulable way, so that in cases where high pressures and speeds could not be adopted initial condensation might still be done away with to a large extent. At any rate, he thought that the question of superheating steam had not been finally settled. Broadly, he was inclined to think with the Author that water in the cylinder might be a very powerful agent in the condensation; but it was on the condition that in some way or other the water was enabled to remain in the cylinder, either by taking heat from it by the cylinder-wall, or by continually re-supplying it by priming. Professor Unwin.

Mr. E. A. Cowper said that the members ought to be much obliged to the Author for the great care he had taken in making his experiments, and particularly to the six gentlemen who had Mr. Cowper.

Mr. Cowper. assisted in the trials. He preferred trials of a longer duration, say twenty-four hours, especially if the boiler had a large water-surface. He had been glad to refer to the Minutes of Proceedings for the description of the engine, which was practically the same as that exhibited.[1] The air-cushion was probably required in order to prevent the connecting-rod, with its long piston-rod and several pistons, from being thrown up off the crank at the top-stroke, and thus causing a blow; there was no waste of power here, except the friction of the air-piston, as the air was simply compressed and expanded again, without having time to lose heat when compressed; the piston also acted as a guide for the top end of the connecting-rod. He was anxious to find out the aim of the machine. He imagined that, in the first place, a high-speed engine was to be made, that being indispensable. If so, it must have a short stroke; it must have a good pressure on the piston, or there would not be sufficient power. Secondly, it must work quietly, and to that end the top brass of the connecting-rod was made to keep in close contact with the crank, by having the engine single-acting, as it was in Brotherhood's engine. That, he considered, must be the reason for its being made single-acting. Thirdly, if there were such high speeds as 300 or 400 revolutions per minute, the pistons might possibly be cut if they were not lubricated by a constant supply of water. If that were so, it was a reason for having no steam-jackets, so that the steam as it went in should be considerably chilled or cooled, a quantity being condensed into water, and so lubricating the cylinder at every stroke. That was a very expensive way of sacrificing power to lubricating the cylinder. If steam-jackets were applied to the engine, they might be kept at any given temperature, say, only a few degrees below the temperature of the incoming steam; the cylinder would then be moist, and there would not be the quantity of water that there now was passing through the engine continually. Fourthly, in order to prevent an excessive variation in pressure at the beginning and at the end of the stroke, he presumed, was a good reason for making it compound, or even triple expansion; another reason, as stated in the Paper, was that the variation in the temperature in each cylinder should be less. That had been done long ago by Mr. Loftus Perkins in his three-cylinder engine, and also by Mr. Adamson, who put the pistons on one rod. Mr. Loftus Perkins had introduced separate cranks. With regard to the percentage

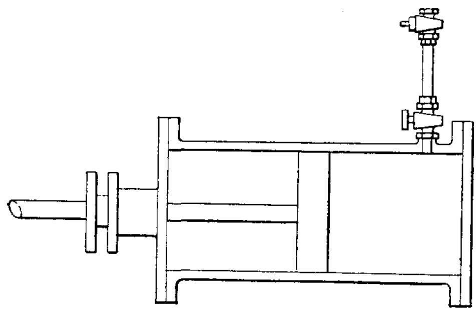
[1] Minutes of Proceedings Inst. C.E. vol. lxxxiii. p. 182.

efficiency, he could not quite agree to exclude all consideration of the steam that had been made in the boiler, excluding from the quantity of water in the form of steam the "missing quantity" in calculating the efficiency. The statement in the Paper was: "As to the percentage of work due from the steam present at the cut-off, which was obtained in the trials, the Author has shown that this varied from about 85 per cent. in the triple, and 87 per cent. in the low-pressure compound trials, to considerably over 100 per cent. in the slow-speed simple ones, the increase being mainly caused by the evaporation of the water initially condensed." As to the action of condensation and re-evaporation, the subject was a complicated one, and he could not then go fully into it; but he maintained that the difference between the action of condensing steam, and re-evaporating water which had been steam, was very great indeed. If air was heated or cooled in a vessel, it became heated or cooled gradually, as the particles circulated and came in contact with the sides; but if steam was put into a cylinder at all colder than itself, it suddenly condensed, and, in fact, disappeared. It was almost impossible to say how quickly this took place. If the pressure in the cylinder was lowered, so that the temperature due to the steam at the lower pressure was less than the temperature of the cylinder, that cylinder would evaporate the water by boiling it off. Therefore the cylinder acted first as a condenser and then as a boiler, and he need hardly say that it had a most ruinous effect in regard to economy. He accordingly maintained, as he had done for a long time, that steam-jackets were useful things. He made a pair of 35-HP. engines more than forty years ago, and the proprietor did not choose to go to the expense of steam-jackets. But at different degrees of expansion he had 19 per cent. loss, 22½ per cent., 27 per cent., and, with a still greater expansion, 40 per cent. loss; in other words, the steam at the end of the stroke, *i.e.*, at the toe of the indicator figure, ought to have given 40 per cent. more power. This made him feel still more strongly in favour of steam-jackets, and whenever he could he introduced them, and had found the greatest advantage from so doing. Mr. Hodgson's pulsometer was an example of the difference between heating and cooling air and steam; for the pulsometer would not work well, or be satisfactory, unless a little air was admitted at every stroke; when that went in, mixed with the steam, and the steam condensed on the surface of the water, the air remained on the surface as a film, and separated the steam to a certain extent from the water, so that there was not so much steam condensed as there otherwise would be. Steam-jackets

Mr. Cowper.

Mr. Cowper. were sometimes misused by having a steam-pipe from the boiler and a drain-pipe back to the boiler, without any means being taken to free the jacket of air. As the steam carried over air, and was condensed, it left the air in the steam-jacket, and there it remained. He had known a steam-jacket quite cool although connected with a boiler, simply because it was filled with air. In order to get the best effort out of steam it should be kept alive. He did not see any other way of doing this but that of keeping the cylinders in which it was placed as warm as the steam, so that it should not condense, as in fact James Watt had shown. If steam-jackets were put to an engine it did not follow that they should be kept as hot as the boiler. They could be kept just below the temperature of the steam coming in. If it was wished to have the cylinder covered with moisture, as in the present case, he presumed, was intended, then the moist surface of the cylinder would be

Fig. 13.



quite sufficient for lubrication; but it would not condense that large quantity of water that would be condensed if there were no steam-jackets. He had himself tried a little experiment, which was not of his own invention, but that of the late Mr. Appold. He put a glass tube closed at the top on to a high-pressure cylinder, working expansively, as shown in Fig. 13, and every time the steam came in the glass tube became dull and white with moisture, just as in breathing upon a piece of glass, which before the end of the stroke all re-evaporated, and the glass tube became bright. He took a shovel full of coals and warmed it up, and then there was no condensation; and when the heat was removed the tube began to show a little spot of moisture, and then more and more, until it became white each time, proving that condensation and re-evaporation did go on in the cylinder when it was working expansively. There was never any moisture left on the glass during the whole time of the return stroke, as the pressure then was at the very lowest, and the heat of the glass tube was ample to evaporate any moisture off it. With regard to the experiments for ascertaining the quantity of water in steam, he was strongly of opinion that steam coming from a boiler made in a commercial way was always damp, that there was always water in it. If proof of that was wanted, it was only necessary to evaporate some salt water quickly, and some salt went over to a certainty. Mr. Mair had referred to an experiment for ascertaining the "mechanical equivalent of heat." Mr. Anderson and Mr. Cowper had tried an experiment some time ago at Erith, and he had read a paper at the British Association on the subject. They employed about 5½ HP. continuously all day long, and they weighed the water coming out from the apparatus. They had two thermometers graduated to ½ inch to a degree, and they took the temperature going in and coming out, and weighed the water heated. The engine was provided with a counter to take the exact number of revolutions, and the brake to measure the power was the one that Mr. Froude used for trying a pair of marine engines, so that the heating instrument itself was the gauge of power. He brought out a constant of 769; but he believed there was in the experiment a small gain of heat, and that it would really come out if tried again at 770, which was very near Joule's 772. The extent of the experiment was about eight hundred times greater than that of Joule's. With regard to the last experiment named in the Paper, and in which he felt a great interest, it was stated, p. 162:—" Only one condensing-engine trial has so far been made; in this 170 lbs. absolute steam-pressure was used, and the consumption of steam was 15·1 lbs. per HP.-hour; but it is expected that better results than this will be obtained with cylinders of more suitable proportions." It was not stated whether this engine was steam-jacketed or not, but certainly the result was a fair one. He considered, taking all the circumstances into account, that the high-pressure engine had performed remarkably well for a quick-running high-pressure expansive engine. Mr. Cowper.

Mr. A. C. Kirk said he cordially endorsed everything that had been said with regard to the value of the experiments. Such a series of experiments had never been previously undertaken, and perhaps they would be impracticable with large engines. The engine employed was of the single-acting type, and probably the results obtained from it were better than would be got with double-acting engines. The old Cornish single-acting engine was a very Mr. Kirk.

Mr. Kirk. admirable one in its day, much better in regard to the utilization of heat than the double-acting engines that followed it, at least until they were intelligently compounded. The simple double-acting engine (he spoke especially of marine work) held its own for some time after Randolph and Elder introduced compound engines. But they were only very partially compound. Two pistons worked opposite ways, and the steam first expanded in one cylinder, and then expanded simultaneously in two. There was no real compounding until the receiver-engine, for which engineers were indebted to Mr. Cowper, was introduced, with a proper heat-trap interposed between the two cylinders; then half the expansion with the corresponding range of temperature was completed, the first half in one cylinder and the second half in the next. In observing the action of those—not by careful measurements, for in commercial business such things were impossible—he came to the conclusion roughly that jackets or no jackets, there was nothing to be had by expanding in one cylinder more than from 2 to $2\frac{1}{2}$ times with 60 lbs. of steam per square inch. Expansion-valves were fitted upon high-pressure cylinders by some specifications; but as far as he could obtain data, either from short trials while the ships were in his hands, or from the best logs that he could get, they were no better than the plain throttle-valve. About that time it fell to him to have to provide an engine for Mr. Rowan's water-tube boiler in the "Propontis." It was arranged by Mr. Dixon, the shipowner, that this boiler should be fitted in the ship, but with the engine he concerned himself little. Having observed that from 2 to $2\frac{1}{2}$ times was as much expansion as could be used in one cylinder, the obvious thing was with a pressure of 150 lbs. steam per square inch to take it in three steps, and this was done. The engine gave a very marked economy. That was in 1871, and the engine was working at the present day with the full pressure of 150 lbs. That showed there was no particular wear and tear of engine by using high-pressure steam. The failure of the water-tube boiler, after about two years' work, led to the idea of using high-pressure steam (above 60 or 70 lbs.) on board ship being abandoned, until, in 1881, Mr. Thompson, by his persuasion, consented to have the same thing repeated, but with ordinary boilers, in his new ship the "Aberdeen," and the ship was running at the present day, having given no trouble in the interval. The step gained in dividing expansion was clearly the limiting of the temperature through which the steam worked in each cylinder; and the reason why that was a gain was that there was less condensation and less evaporation in the cylinders. He thought there was no doubt about that fact; though Mr. Mair seemed to question it somewhat. When, however, the steam was dry, when it was superheated even moderately, and no lubrication was used, the cylinders became worn; but when the steam was not superheated the cylinders and packing-rings were worn much less. There must therefore have been a difference in the condition of the surfaces of the metal, and the only difference that could well be, was brought about by the surfaces never being absolutely dry. He had no doubt that it was the limited range of temperature of each cylinder that caused the triple-expansion engine to be so economical. As to the value of steam-jackets, he felt that in speaking on that subject he was on delicate ground. No doubt in the earlier non-compound engines, when the steam was worked through a large range of temperature, jackets were a very valuable addition; but as far as he had observed, with the ranges of temperature in the best compound and in the modern triple-expansion engine, he could not trace any advantage in the jacket. The ideal function of a steam-jacket was a neutral one; simply to prevent condensation. Unfortunately, it also acted as an evaporator. When so acting, it was in fact a boiler in which higher-pressure steam was employed to generate steam of a lower pressure. Without going into the ultimate value of its action in the simple double-acting, or still more perhaps in the single-acting Cornish engine, it was clear that, to expend boiler steam to generate lower-pressure steam in the low-pressure cylinder (or even in the intermediate) was not an economical way of using it, the more so as the steam, generated in the low-pressure cylinder, had little opportunity of doing any work, but went immediately into the condenser. Better it should go in as water. Jacketing the high-pressure cylinder seemed to add nothing perceptible to the heat economy, but contributed sensibly to the wear and tear. The very large volume of water that came from a steam-jacket, although the range of temperature in its cylinder was small, led him to think that the steam thus condensed would be better employed if put into the cylinder itself. He had found intelligent marine sea-going engineers use the jacket to heat up the engine, and then after a little time shut it off. In fact, the subdivision of expansion into successive steps was a much more efficient method of economizing steam than the steam-jacket had ever been, and had rendered the latter useless. Mr. Kirk.

Mr. MacFarlane Gray observed that on numerous occasions, commencing the 3rd of October, 1887, he had watched trials and conducted trials of the experimental central-valve engine at the Mr. MacFarlane Gray.

Mr. MacFarlane Gray. works of Messrs. Willans and Robinson, Thames Ditton. The testing-house was a separate building away from the workmen, and was fitted with everything requisite to facilitate and to ensure accurate trials. On the 16th of January, 1888, he conducted a special trial with the engine arranged for triple-expansion. He was assisted by Mr. P. A. Low, Assoc. M. Inst. C.E., but every detail was under his own direction and personal observation the whole of the time. He had gauged the cylinders previously and had checked the dimensions on a measuring engine. He now reported upon that trial, and how it was conducted. At noon he commenced his inspection. The engine was then running with the feed-pump on. When preparations were complete the feed-pump was stopped, and the feed-water cistern disconnected from it and from the supply-pipe. The reading of the weigh-bridge lever was then, the cistern being balanced on the weigh-bridge, 52 cwt. 3 qrs. 3 lbs. The counter which was out of action read 360,000. The height of the water was accurately taken by nearly closing the water-cock, and observing when the then perfectly steady level fell to a fine mark on a fixed gauge stick. The height of this mark was also taken by rule. When accurately at the mark the time was noted, 12h. 26m. 50s., and at the same instant the counter was thrown into gear; the engine having all the time been running steady, driving a dynamo. The feed-pump was now again connected to the cistern, and the trial had commenced. During the trial indicator cards were taken every twenty minutes from each of the working chambers, viz., the high-high, the high-receiver, the high, the low-receiver, and the low-pressure cylinder, five cards in the set, and eleven sets were taken at nearly equal intervals. The engine worked perfectly steady at practically uniform speed, without the slightest hitch of any kind, during the whole of the trial. In anticipation of 4h. 6m. 50s., the pump was again stopped and disconnected from the cistern with the water above the gauge mark. At that moment the counter was thrown out of gear, and its reading was noted as 451,621. The gauge being again partly closed to steady the level, the water was observed to come to the mark exactly at 4h. 10m. 10s. The weigh-bridge reading was again taken, 28 cwt. 0 qr. 3 lbs. The barometer was 30·41 inches; and the engine-room temperature 60° Fahrenheit. A drain cock from the high-receiver gave 2 lbs. 2 oz. of water per hour, and one from the low-receiver gave 15 lbs. 7 oz. per hour. The eleven sets of cards had been carefully measured, and the HP. was calculated for the engine having a stroke equal to 6 inches and net piston areas, thus:—

Mr. MacFarlane Gray.

	Square inches.
h.h.	34·5
h.r.	31·416
h.	71·47
l.r.	65·97
l.	141·4

These areas corresponded to the gauge measurements taken. The calculated cards gave—

	Mean pressure. Lbs. per square inch.	Mean pressure referred to Low. Lbs. per square inch.
h.h.	43·908	10·714
h.r.	14·680	3·259
h.	22·728	11·490
l.r.	6·886	3·213
l.	16·455	16·455
Total		45·131
Correction for barometer		0·18
Total if barometer had been = 14·7 lbs.		45·311

		H.	M.	S.		
Counter,	time	12	26	50	reading	360,000
		4	6	50		451,621
		3	40	0	= 220m.)	91,621

Revolutions per minute . = 416·46
Total indicated HP. . . = 40·42

		H.	M.	S.		Cwt.	qrs.	lbs.
Feed,	time	12	26	50	weight	52	3	3
		4	10	10		28	0	3
		3	43	20		24	3	0
		= 223·3m.				= 2,772 lbs.		

$$\frac{2,772 \times 60}{223 \cdot 3} = 745 \text{ lbs. of water per hour;}$$

$$\frac{745}{40 \cdot 42} = 18 \cdot 42 \quad \text{,,} \quad \text{,,} \quad \text{per HP.}$$

In that result there was the gross weight of feed, including the water drained from the receivers. There was no steam-jacket on any part of the engine. The steam condensed in the passages and slide-valve chambers was all included in the weight of water mentioned, so that no addition had to be made to the result to get at the measure of the efficiency of the engine in lbs. of steam per hour HP. In the diagram, Plate 4, Fig. 5, he had shown the curve of adiabatic expansion for steam of 185 lbs. gross, or say 170 lbs. by the gauge, from 185 to 21 lbs. pressure. The diagram

Mr. MacFarlane Gray. was on a scale 1 inch = 10 lbs. pressure. That was the vertical ordinate. Horizontally it gave the volume of 1 lb. of the steam on such a scale that each square inch of the area represented 2,000 foot-lbs. As it was before him on sectional paper of $\frac{1}{10}$ inch, each square represented 20 foot-lbs. He had shown the cards obtained on the same scale, giving the work performed by 1 lb. of steam in the engine at the pressure at which it was done. The perfect engine would give the whole area which measured down to 15 lbs. pressure, 68·6 square inches or 137,200 foot-lbs. The sum of the areas of the cards was = 53·31 square inches; therefore the engine indicated $\frac{53 \cdot 31}{68 \cdot 6} = 77 \cdot 7$ per cent. of the perfection due to boiler-pressure, 170 lbs. by the gauge, or deducting the portions of card and diagram above the intended admission pressure, the result was $\frac{53 \cdot 00}{65 \cdot 9} = 80$ per cent. of the perfection due to steam at 170 lbs. total pressure of admission. On this diagram only the working steam was represented. He considered it was confusing to give the constant steam of the clearance spaces along with the working steam. This was especially the case in an engine having many chambers with different quantities of what might be called "play steam." There was more work in making the diagram without these than with them; but that work was repaid by the greater clearness of the diagram. He hoped that the pains he had taken to eliminate the misleading clearance expansions would be appreciated. He need not enlarge upon the excellence of those results; he believed they were the best on record, and made under conditions of rigorous accuracy such as had seldom, if ever, before been fulfilled. The indicator cards showed that the missing quantity, or initial condensation, in the Willans engine, was less than what had been found in any previously published trials of other engines with unjacketed cylinders. Notwithstanding, the Author had suggested that even in this engine it was probably principally a film of water, and not the metal, which absorbed and gave out, during each stroke, the missing quantity of heat by additional condensation and subsequent re-evaporation. It had been stated, as an objection to this hypothesis, that the heat of condensation at the higher temperature was more than sufficient for re-evaporation at the lower temperature, and that therefore such a water-film could not exist. If the film of water of initial condensation were so thin that its temperature was always practically the same as that of the steam in the cylinder, then the heat of condensation would be really insufficient for the re-evaporation of an equal weight. When the film was of greater thickness, so that its temperature did not keep pace with the temperature of the steam, the heat of condensation might be equal to that required for the re-evaporation of an equal weight of water, and thereby a limit to further increase of water-film would be established. Under the ideal condition of equal temperature of film and steam throughout the stroke, the following was the proportion of re-evaporation to initial condensation. If a film of water left in a steam-cylinder at the temperature B Fahrenheit absolute was heated to the temperature A Fahrenheit by the condensation of a portion of steam at that temperature, the weight of the augmented film would be, say, n times the weight of the steam condensed upon it. As the actual weight did not affect the problem, it would be convenient to regard the augmented weight as equal to 1 lb. The latent heat only of the condensed steam would be applied to heat the original film, $\frac{n-1}{n}$ of 1 lb. of water, from B to A; therefore Mr. MacFarlane Gray.

$$\frac{n-1}{n}(A - B) = \frac{1{,}438 - 0 \cdot 7\,A}{n};$$

therefore

$$\frac{1}{n} = \frac{A - B}{1{,}438 + 0 \cdot 3\,A - B};$$

and that would be the weight of condensed steam in the augmented film. At the lower temperature, the latent heat of the steam portion would be B ($\log_e$ A − $\log_e$ B), and its weight would therefore be—

$$\frac{B\,(\log_e A - \log_e B)}{1{,}438 - 0 \cdot 7\,B}.$$

The difference of the two weights was, therefore, using common logarithms—

$$\frac{A - B}{1{,}438 + 0 \cdot 3\,A - B} - \frac{B\,(\log A - \log B)}{624 \cdot 5 - 0 \cdot 304\,B}.$$

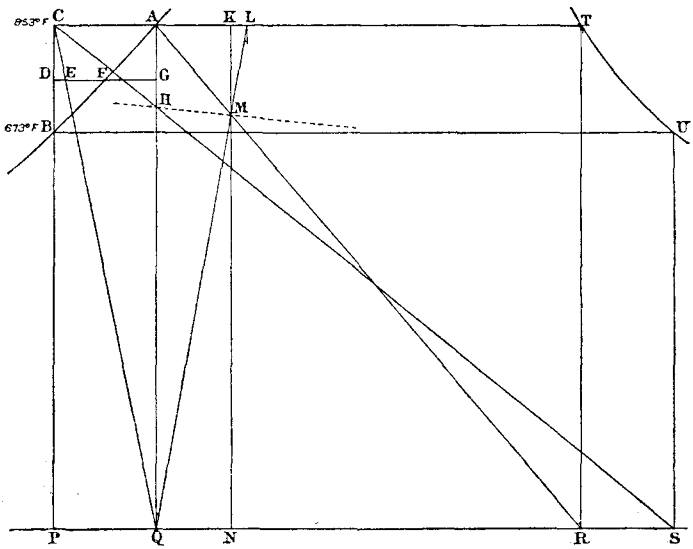
Writing A = 853 and B = 673, that became 0·176315 − 0·165356 = 0·010959. That was, the re-evaporation was less by so much than what was condensed. The film would therefore increase according to the ratio

$$1 + \frac{0 \cdot 010959}{1 - 0 \cdot 176315} = 1 \cdot 013305,$$

or $1\frac{1}{3}$ per cent. per stroke. On the diagram, Fig. 14, the lower temperature was = the ordinate P B, and the higher temperature

Mr. MacFarlane Gray. was = Q A. The heat added was the area P B A Q. The latent heat at the high temperature was the area Q A T R, and the latent heat at the lower temperature was the area P B U S per lb. weight. The range of temperature was, as before, from 673° to 853° Fahrenheit. The graphic solution of the problem was thus represented: Draw D G to make B D F = F A G, that was, draw D G a little below the centre of C B. Draw Q C cutting G D in E. A width = G E, the height of Q A would be = Q G × G D. Set off A L = G E, and join Q L. Draw T R, R A, and then N K through the intersection at M. The film at the high temperature consisted of N M of

Fig. 14.



original liquid, and M K added by condensation, representing the whole weight by N K. Draw U S, S C. At the lower temperature the weight of the film was Q H, and the weight of the steam evaporated from it was H A, Q A as before denoting the total weight. He had drawn a broken line through H and M to show better that K M was greater than A H. There was, however, no significance attached to any other point in the line. He had calculated the ratio of increase per stroke from the higher temperature at 836°, and for the lower temperature at small intervals down to 673°. He had calculated also the increase for each of the ranges of temperature into which the triple engine divided the total range of temperature. The following was a tabular statement of the results :— Mr. MacFarlane Gray.

°	°	°	°	°	°
836	0·040504	836	0·071832	836	0·101180
800	0·040107	770	0·070439	740	0·098116
d	0·000397	*d*	0·001393	*d*	0·003064
w	0·959496	*w*	0·928168	*w*	0·898820
i	0·000414	*i*	0·001501	*i*	0·003409
836	0·11983	836	0·137651	836	0·160464
720	0·11516	700	0·131122	673	0·150963
d	0·00467	*d*	0·006529	*d*	0·009536
w	0·88017	*w*	0·862349	*w*	0·839536
i	0·00531	*i*	0·007571	*i*	0·011317

°	°	°	°
793	0·021067	785	0·047185
774	0·020945	741	0·046501
d	0·000122	*d*	0·000684
w	0·978933	*w*	0·952815
i	0·000125	*i*	0·000718
741	0·019204	732	0·059923
723	0·019081	673	0·058494
d	0·000123	*d*	0·001429
w	0·980796	*w*	0·940799
i	0·000125	*i*	0·001519

Against the higher temperature was written the weight of steam condensed at that temperature, making the augmented weight = 1. Against the lower temperature was the weight of water evaporated. Against *d* was the difference of these. Against *w* was the weight of the original film at the lower temperature. Against *i* was the increase per stroke per unit weight of film. All these calculations referred to the ideal condition in which the temperature of the film of water was always identical with the temperature of the steam in the cylinder. Increase of water-film and increased rapidity of reciprocation would tend to make the temperature of the film lag behind the temperature of the steam, both in rising and in falling; and such difference of temperature would soon produce equality of re-evaporation and initial condensation, and so check any further increase of the film. Where the water-film was of greater thickness, evaporation would be in excess of condensation, until the quantity of liquid was reduced to that thickness of film for which the evaporation was just equal to the condensation. The experiment by Hirn, referred to by Mr. Mair, was therefore in accordance with this statement. There was no reliable

Mr. MacFarlane Gray. information regarding the rapidity of equalization of temperatures such as those now under discussion. It depended upon that alone whether the Author's hypothesis was sufficient to explain the results he had obtained. He had witnessed some of the engine trials in the Dexter, Rush-Dallas, and Bache series of experiments on compound and non-compound engines, conducted by Mr. Emery and Mr. Loring at Baltimore and Boston, in the United States, in 1874. He admired the system on which those trials were conducted, and at his suggestion the results were sent to this country for publication. The Author, having read that report, had unconsciously made his trials upon nearly the same lines, but improved in every way. In place of the crowded engine-room and exposed deck of a small steamer there was a specially built test-house at Thames Ditton, with a weigh-bridge, water-cistern, and electric warning-bell; and the engine and boiler were all under the eye and control of one person, who could himself, undisturbed, conduct the whole of the trial. The American trials were upon four different engines by three different makers. The Author's trials were all upon the same engine, which could be changed into simple, compound, or triple, and modified in respect to cut-offs just as desired. The results obtained were therefore, as scientific data, free from many not intended differences which must have affected the American results. The Willans engine, driving always the same dynamo but with its wires differently coupled, could be run at 100, 200, 300, or 400 revolutions per minute, and still with practically the same piston-pressures. This was easily done with the dynamo when the Author had thought out the plan; but it could not be done with an engine driving a steam-ship propeller, as in the American trials. The quality of the steam had not been ascertained in the American experiments, and being from four different boilers the proportion of priming water was probably not the same in them all. The test-house at Thames Ditton, in his opinion, in relation to steam-engine efficiency, occupied the same position as that held by the Admiralty Experimental Works in relation to the science of steam-ship resistance. He looked forward to the completion of the series on the condensing engine with great interest.

Professor Kennedy. Professor ALEX. B. W. KENNEDY cordially agreed in thinking that the Paper was the most important contribution to the experimental knowledge of steam-engine work that had yet been brought forward, and he was glad that it had been submitted to the Institution. There was only one important omission in it;

the Author, with great modesty, had not called so much attention to the absolute results attained as to the comparative results. Yet the absolute results were in themselves very remarkable. That a small non-condensing engine should have been worked over and over again by different people, and under all sorts of different conditions, at 19 lbs., and under 19 lbs. of water per indicated HP. per hour, was an extraordinary result in economy. The central point of the Paper was, of course, the question which had been several times dealt with, of the effect of initial condensation on the economy of the steam-engine. He had frequently had occasion to say that this initial condensation was probably the principal remediable cause of the want of economy in the steam-engine. He had welcomed Major English's Paper, read a few weeks ago before the Institution of Mechanical Engineers,[1] because the engine he had tested was so thoroughly bad as to give special point to his figures, and to show, in all its incongruity, how absurd engineers might sometimes be in the way of providing appliances for elaborately sending 50 per cent. of the steam direct from the boiler into the condenser, for that was really what it amounted to. He was glad to have trustworthy figures which showed exactly to what extent that happened. The Author now showed how, after all, engineers need not send away 50 per cent. of steam, but might be content with 15 or 10, or even 5 per cent. Of course the why of that difference was one point which would lead to enquiries by engineers. Everybody would wish, if possible, to get an engine which should have as little initial condensation as the engine tested by the Author. It was certainly not in the least degree less to the Author's credit, but it was, perhaps, discouraging to other people, that the engine with which he had attained such good results was an engine differing a great deal from the ordinary types, and it was possible that they might work a long time, with some types of engine, before they could manage to save from instantaneous transformation into water so large a proportion of steam as the Author had done. He did not wish to discuss the construction of the engine, but as comments had been made upon it, it was only right to point out the two or three special features in it which might have an important bearing upon the particular point under discussion. In the first place, although the engine was nominally non-jacketed, yet it would be seen from the model, and from an examination of the construction, that the top end Professor Kennedy.

[1] Institution of Mechanical Engineers. Proceedings 1887, p. 503.

Professor Kennedy. of each cylinder was jacketed by the cylinder above it. Then there were always the hot trunks continually passing down from a hotter into a cooler space, and continually wiped over by a very vigorous current of steam, and so not only kept hot by contact with hot steam, but by the eddying and rubbing contact of the whole quantity of steam that did the work of the engine. Then, of course, there was the continuous drainage of the water which was inherent to the particular arrangement adopted. There was also the fact that the engine was single-acting, and that the Author adopted the equilibrium-stroke principle to a certain extent, so as to "stage" the temperatures considerably more than would otherwise be possible. Again, the engine only attained its highest economy when the number of reciprocations—the number of breathings in and out of heat, so to speak—was extremely great. The Author had directed attention to the extraordinary difference that he had obtained with the same engine in otherwise similar conditions, by an alteration of speed; not an alteration of piston-speed so much, because that was slow or moderate under any circumstances; but an alteration in the number of breathings, or the number of times that the heat was taken in and thrown out again. He thought that those points, although they did not form, even when taken together, a proper explanation of the excellent results attained, would have to be borne in mind by those who wished to obtain if possible the same results, and to reduce initial condensation to anything like the same figures. The question whether the initial condensation was due to water in the cylinder or to the dry walls of the cylinder, or to both together, had been referred to at length by Mr. MacFarlane Gray, Professor Unwin, and others, and he did not know that he had anything to add to what had been said on the subject. It might be noted that the weight of water, which would be sufficient to do the whole of the condensation, would amount to a film of about $\frac{1}{100}$ inch thick upon the end of the cylinder and the surface of the piston. This amount of water would be sufficient, even if the iron had no action at all when dry. That was, of course, a much greater amount of water than corresponded to the weight of the feed per stroke, as Mr. Mair had pointed out; but it did not seem a large amount to remain as a film over the surface. He next wished to say a little about the calorimetric trials, the results of which were given in Table X. There were nine trials, but he was informed by the Author that three of them, to which notes were added in the Table, were not very satisfactory. Professor Kennedy had therefore taken the remaining six, and calculating

Professor Kennedy.

from them he had found that, if they were taken as affording measure of the dryness of the steam, the mixture contained 100·05 per cent. of steam—the mean of the six amounting to just over 100 per cent. The interpretation which he gave to that figure was, of course (0·05 being quite within experimental error), that the steam was perfectly dry, and that Regnault's value for the latent heat applied exactly to the conditions taken by the Author. But he wished to point out that the Author's experiments enforced, most strongly, the impossibility of using that particular method for the quantitative determination of priming. Most careful experiments were made with large weights of water; they were made at leisure by skilled operators—and still the best six of them gave results varying from − 0·5 per cent. to + 0·7 per cent. The priming was not likely to be more than 0·5 per cent., so that the experimental variations between those first-rate experiments were greater than the whole quantity of liquid. But this method had been frequently proposed and used for the purpose of direct quantitative determination of the priming. In a kindred institution in America, where the subject was discussed a short time ago,[1] these small variations were pointed out. It was said that the method was extremely good, and gave results within 0·5 per cent., but it did not appear to be noticed that what was called 0·5 per cent. was really 100 per cent. of the particular quantity that was looked for; it was 0·5 per cent. of the whole quantity of steam, but it was 100 per cent. of the quantity of priming! He only mentioned the matter because he thought that the Paper as it stood might be misinterpreted on that point, and that attempts might be made to determine priming by a method which, even with all the care and skill that had been brought to bear upon it, showed itself at once quite unadapted to that particular purpose—not the purpose, of course, for which the Author had used it. Another point in the Paper was the question as to what standard should be used for measuring the efficiency of an engine, or what particular engine should be taken to have an efficiency equal to unity. There were several standards, and sometimes it was rather perplexing to know which it was the most advisable to adopt. He would take as an illustration one of the trials in Table III (the one marked A), which had been made under his own eye. If the results of that trial were compared with the working of what was called a perfect engine, using the same weight of steam per stroke, and working between the same tem-

[1] Transactions of the American Society of Mechanical Engineers, vol. vi. p. 256.

Professor Kennedy. peratures, the engine would have an efficiency of 84·2 per cent. of its highest possible efficiency. Such an engine would give, per lb. of steam received, an amount of work expressed by

$$L_1 \left(\frac{T_1 - T_2}{T_1}\right),$$

where T_1 and T_2 were the temperatures (absolute) between which the engine worked, and L_1 was the latent heat of 1 lb. of steam at T_1. (L_1 was the quantity given in the Paper as 1,438 − 0·7 A.) But this was hardly fair, because a perfect engine using the same weight of steam would use so much less heat than the actual engine—the latent heat of the steam only, and not the heat necessary to raise the temperature of the water. The actual engine could utilize some part of the heat which it so received by virtue of being an imperfect engine, and this would unfairly increase its efficiency as based on this standard. If, again, the engine was compared with a perfect engine receiving the same amount of heat as the Author's, supposing all the heat to be received at the temperature of the steam, the efficiency would go down at once to 71·4 per cent. The formula for the work theoretically possible for an engine of this kind, per lb. of steam, was

$$(L_1 + h_1) \left(\frac{T_1 - T_2}{T_1}\right),$$

where h_1 was the heat necessary to raise 1 lb. of water from the temperature of discharge to the initial temperature of the steam, *i.e.* from T_2 to T_1. This, he thought, was a very convenient method of comparing efficiency; taking as a standard an engine which received the same amount of heat as the actual engine, but which received it at the temperature of the steam and rejected it at the temperature of the condensing water, or of the steam or the atmospheric pressure, as in the present case. As a third standard the engine might be compared with a perfect engine using what Zeuner called the same "heat-weight" as the actual engine, and working between the same limits of temperature. The amount of work done by such an engine, per lb. of steam, would be

$$\left(\frac{L_1}{T_1} + \log_e \frac{T_1}{T_2}\right) (T_1 - T_2),$$

and compared with this standard the efficiency in the particular trial under discussion would be 69 per cent., or nearly the same figure as in the last case. The Author had taken for his standard a non-perfect engine, receiving the same heat as the actual engine, and receiving it in the same fashion. It was a little more difficult to compare with that standard than with the standard he had mentioned, because the formulas necessary were a little longer; but as long as it was known exactly what the standard was he did not think that it would be objected to. He should like to mention one point with regard to the interpretation of the indicator cards in some of the trials, because it occurred frequently, and might be a matter of some importance. At some of the trials a card such as was shown by Fig. 10 was obtained. He thought that the point marked at *a* on that card, and taken by the Author as cut-off point, was not a real point of the card at all, but was the crest of a wave caused by oscillation of the pencil, and was therefore measurably higher than the true point. He thought that this difference was sufficient to account for the fact that he had calculated the initial condensation in this trial to be over 7 per cent., while in Table X it was given as only about 4½ per cent. Either quantity was extraordinarily small, and he only referred to the discrepancy as illustrating a point of some interest in the interpretation of indicator cards. Referring to the diagram, Plate 4, Fig. 4, for which he was himself responsible, the dotted lines there shown represented the expansion of the indicator cards by a method described by him at a recent meeting of the Institution of Mechanical Engineers.[1] The curves were here so arranged that their horizontal distances from the vertical line at any pressure represented accurately the amount of the net feed-water received per stroke which was actually present in any cylinder as steam at that pressure. Cushion-steam in clearance spaces was thus eliminated from consideration altogether, and the ordinates of the new curve could be directly compared with the ordinates of either an adiabatic curve or a saturation curve corresponding to a weight of steam equal to the net weight of feed-water used per stroke. In the triple-expansion trial, of which he had just discussed some figures, he had found that, after the first condensation of steam on entry to the cylinder, subsequent liquefaction of steam during expansion was almost exactly that corresponding to the conditions of adiabatic expansion. Professor Kennedy.

Mr. W. SCHÖNHEYDER said that in the Paper there were some useful Tables, showing the behaviour of the steam in passing through the cylinders. There were also some diagrams intended to show the same thing graphically, but as those diagrams did not Mr. Schönheyder.

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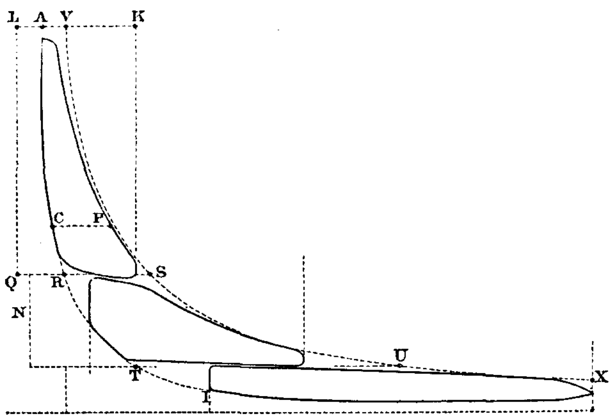
[1] Institution of Mechanical Engineers. Proceedings 1887, p. 70.

Mr. Schönheyder. appear to him to be true, he wished to say a few words as to what he considered the correct mode of preparing such diagrams, namely, placing three diagrams of three cylinders on one Paper so as to show at a glance the amount of condensation, re-evaporation, and so on. He had adopted that method as early as 1871. At that time triple engines were not known, and the plan, therefore, had reference only to compound engines. The diagrams he exhibited (Fig. 15) were not from actual practice, but were ideal diagrams. He believed, however, that they were sufficiently accurate. The small diagram, the intermediate cylinder, and the low-pressure cylinder diagrams were shown in the ordinary way, and the clearances were set off on the left side of the diagrams in the usual

FIG. 15.



Scale 1 inch = 80 lbs.

manner. It was well known, and it had been referred to in the Paper, that in drawing a horizontal line such as C P intersecting the compression curve and the expansion curve, the proportion which such line bore to the size of the cylinder A K was the proportion of steam which was in the cylinder at the time, and at that particular pressure. If, therefore, the compression curve of the high-pressure diagram was continued downwards to R, the line RS represented the amount of steam which was being exhausted from the high-pressure cylinder, and which must appear in the second cylinder. Taking the second cylinder diagram, with the compression curve continued upwards to R, and placing it in the position shown, the expansion curves of the two cylinders could then be compared; and it would be the same for the third

Mr. Schönheyder.

cylinder. The length of the line TU represented the amount of steam which was exhausted from the second cylinder, and it should also represent the amount of steam which appeared in the low-pressure cylinder. Altogether in that manner the vertical lines, which represented the boundary line of the clearance spaces, might or might not fall in one vertical line. In the design exhibited they did not fall in a vertical line. When, then, it was wished to compare the whole work of the cards with the theoretical curve, the theoretical curve must be set off, say V S; but it must be set off in such a manner that the curve was measured from the vertical line L Q, because it was the total volume in the cylinder plus volume in the clearance which was expanding in the cylinder during that time. For the second cylinder the expansion curve S U must be set off from the vertical line N, the boundary line of the clearance spaces, because it was the volume of the steam in the cylinder plus the volume of steam in the clearance which was expanding in the cylinder at that time, and that volume was not necessarily the same as that which was expanding in the first cylinder. It would readily be seen that the compression curves and the expansion curves C R T I and V S U X were not necessarily continuous, but might (as in the present example) have humps at R T, S, and U, or depressions at these points. Set off in that manner, it could be seen at once where condensation or where evaporation took place, and so on. Not only was the method useful for comparing diagrams of actual engines, but it was extremely useful in constructing engines; it gave with great facility the proportion of the cylinders according to the proportion of the work to be obtained from each, the points of cut-off, compression, and the like. He drew attention to the subject because he knew there was a difference of opinion as to the correct mode of putting the diagrams together. It appeared to him manifest that if the diagrams were put in such a way that the boundary lines of the clearances were in one vertical line, it would be impossible to compare them in a proper manner.

Mr. C. E. Cowper.

Mr. Charles E. Cowper remarked that allusion had been made to some experiments at Erith with regard to the mechanical equivalent of heat, by Mr. William Anderson and Mr. E. A. Cowper, upon which a Paper had been read by them at the British Association. As it had fallen to his lot to carry out these experiments, and as he was, perhaps, the only person in the room who had actually made such experiments, he might be permitted to say a few words respecting them. Attention had been drawn to the fact

Mr. C. E. Cowper. that the Author had used the figure 770, instead of the usually accepted figure 772. He believed it was the fact that many scientists were perfectly contented to use the figure put forward by Dr. Joule, without much inquiry as to how it was arrived at; but there were probably some practical men who doubted such figures, or at least were not very well satisfied with them. Many, however, who were concerned with the steam-engine, both practically and theoretically, were no doubt glad to have the figures confirmed on a large scale, Joule's experiments having been carried on on a very small scale. Without going into details, he might say roughly that in the Erith experiments cold water, as cold as could be got in a cistern in summer, was running through a ½-inch pipe, going through the machine, and coming out as hot as the hand could bear. The water was churned by the 5 HP. obtained from a steam-engine, and observations were made at one-minute intervals for hours consecutively. The great advantage of the form in which the experiments were made was that the whole was in a normal condition. There was no correction, such as Joule had to make in his experiments, for the specific heat of the various materials of which the apparatus was made, and he thought the results would be satisfactory to every practical mind. It had been mentioned that the value obtained was 769; but it was quite possible that there might be a difference of several units in repeating the experiments. Perhaps every one was not aware that Joule, after having published his result of 772, by some corrections of the thermometers (which had to measure very small amounts, only fractions of a degree), made the figure with water 773; with mercury he made it 774, and with friction of cast-iron in mercury nearly 775. Evidently it was not very important what exact figure was used; it did not matter whether it was 770 or 772; the latter, however, was perhaps the best, because most published Tables were calculated upon it. The question of steam-jackets had been a constant bone of contention. People on the one side spoke of jackets in use, and people on the other side, who said that there was no advantage in jackets, probably often made their observations with jackets not in use. There were many examples of the useful effect of jackets in slow-working engines, but it did not follow that that was the case with very high speeds and very high pressures. The circumstances altogether were different in the case of the Willans' engine, and it did not follow that the same advantage would be obtained in all cases by the jackets. He should be glad, however, to learn the results of the further experiments which he believed

Mr. C. E. Cowper.

the Author proposed to make with steam-jackets. The results that he had obtained were undoubtedly very good, the amount of water being brought down to 19 lbs. per indicated HP. per hour. Possibly many of those present were buyers and users of steam-engines, and many had to deal with pumping water out of pits, where the value of water was not great, but where it had to be got rid of. He would ask them to imagine for a moment that they were using a boiler in which 1 lb. of coal would evaporate 10 lbs. of water. The engine referred to by Professor Kennedy, upon which a Paper had been read at the Institution of Mechanical Engineers, used in the best trials 34 lbs. of water, requiring about $3\frac{1}{2}$ lbs. of coal. The average on several trials of good economical pumping-engines was 15, 16 and 17 lbs. of water, representing $1\frac{1}{2}$ and $1\frac{3}{4}$ lb. of coal. A difference of $1\frac{1}{2}$ lb. or 2 lbs. of coal might not appear very much, but, putting it at 100 per cent. increase in the total cost of fuel, it appeared much larger. With regard to the question of the missing quantity, he had taken out several cases of economical engines, and had found that the missing quantity, calculated in the same way as the Author had calculated it, at the cut-off in the first cylinder and the toe of the diagram in the last cylinder, presented a very striking difference between the slow-working economical engines (such as pumping engines with steam-jacketed cylinders) and the high-speed engines of the Author. In the Author's Tables it would be observed that the quantity of water was increasing; he began, for instance, with 40 per cent., and finished with 18; whereas in all the cases of slow-working engines the water decreased, there being less water at the end than at the beginning. With regard to the calculation of efficiencies, he could not agree with Professor Kennedy, and the only fault which he found with the Paper was as to the way in which the "efficiencies" were given. He agreed with Mr. Davey that the quantity of water used fairly represented the efficiency (of course, taken inversely, the greater the quantity of water the less the efficiency). He admitted with Mr. Mair that the engine used water and not coal; but from a thermo-dynamical point of view the consumption should be measured in heat. There was only one way of taking absolute efficiency, and that was to compare the power obtained with the quantity of heat used in thermal-units. With a good engine it only came out at about 15 per cent. It was no doubt disheartening to a manufacturer to hear that when he had made a good engine it only had about 15 per cent. efficiency; still, he did not think that the Author was justified in taking the effi-

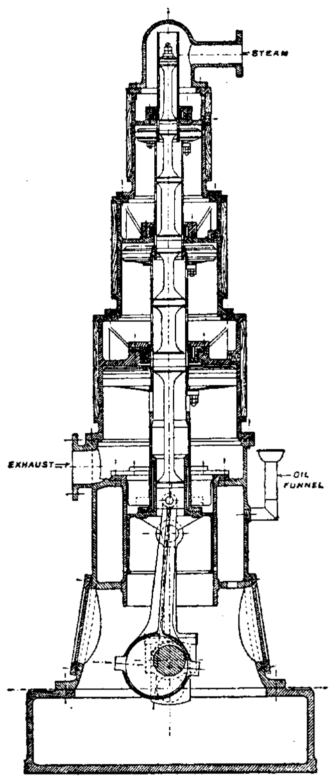
Mr. C. E. Cowper. ciency in the way he had done. He thought he could show from the figures in the Paper that it led to a very decided fallacy. In Table I, "simple series," 4 expansions, 27·8 lbs. of water, the "efficiency" was put down at 67·1 per cent. Working the same engine with less than 2 expansions, with 42·76 lbs. of water, the "efficiency" was 81·08 per cent.; so that the more the water was wasted the better was the "efficiency," so called, which was obviously an error. Although, however, he had found this one fault with the Paper, he thought that the results were exceedingly good.

Mr. Heenan. Mr. R. HAMMERSLEY HEENAN regretted that the Author had in all cases given indicated instead of brake HP. The indicated power of an engine, especially a high-speed engine, was by no means a very trustworthy method of testing its power under various loads. He had seen reciprocative high-speed engines pull up dead, under a full steam-pressure, from internal friction. An engine might have a great deal of internal friction and yet show a good diagram. He understood the Author to say that the engine drove a dynamo. Now, if he would give the electric power developed by the dynamo side by side with the indicated power throughout, he would make his series of experiments more complete. The percentage loss from internal friction, etc., would be interesting throughout all the results given. In testing his own engine, Mr. Heenan had made it a rule, for many years, to measure the brake HP. when testing for steam-consumption. For the exact measurement of brake HP. he had found the water dynamometer of the late Mr. Froude invaluable. The load might be adjusted with the greatest accuracy, and the machine ran for hours without attention. He should like to direct attention to the recent progress of the Tower spherical engine. The early engines were, of course, marked by many errors both in design and in construction. He might claim, however, that after seven years of manufacture most of the difficulties had been vanquished. The smooth silent running of the new type of engines, coupled with their remarkable economy of steam, had won for them such a good name that they had been specially selected by the Admiralty for the Queen's yachts "Osborne" and "Victoria and Albert," now being fitted up with electric light. The Admiralty had at present in use spherical engines capable of developing in the aggregate nearly 2,000 brake HP. With regard to the life of a high-speed engine, which was of as much importance as its economy, and concerning which the Author had given no information, the wearing powers of spherical engines might be judged from the following extract from a letter received from Messrs. Gray, Dawes, and Co., of the British India Steam Navigation Company: "We have much pleasure in testifying to the fact that two of your engines, 'Tower Spherical,' have been in use on board the 'Dacca' s.s. for sixteen months without ever being opened up or repaired: and when on her last homeward journey it was found necessary to open them up, it was merely for the purpose of putting in some new springs to the packing which took a few minutes." Mr. Heenan.

Mr. W. H. PREECE merely desired to state from a practical point of view that every engineer engaged in electric-light enterprise owed a debt of gratitude to the Author for the work he was doing. There was an enormous field for engines of that class, as well as of the class referred to by Mr. Heenan, in carrying out installations for electric-lighting purposes. The great question at present was the relative advantages of small compared with large engines. There was a strong feeling that a small engine applied during the whole of the twenty-four hours at its full efficiency would do economically a much better amount of work than a large engine, which could be applied economically only during the periods of maximum electrical pressure—usually the hours when people were dressing for dinner, or for the theatres. As a matter of fact, in any electric-lighting installation there were only two hours, or perhaps at most three, during the day when the whole establishment was called upon to do its best. If a small engine working economically, like the engines of Messrs. Willans and Robinson, would enable them to tide over those hours, there was not the slightest doubt that electric light might be supplied at a price that would compare most favourably with that of gas. The Author's experiments entirely removed the wide-spread prejudice that existed against high-speed engines as large steam consumers. There was another feature connected with the Author's engine which was of great importance, namely, its compactness and the small space it occupied. At the Post Office, where the expenditure on gas was £15,000 a year, it had been a matter of very great consequence to apply electric light; but that had not been done because of the space occupied by the usual class of engines. A room about 8 feet square became vacant, and he at once seized upon it, in order to put in it two of the Author's engines geared to two dynamos, and in that way five hundred lights had been obtained. For two years past the engines had worked with very great satisfaction, and had enabled a very important question to be solved. Mr. Preece.

Mr. Willans. Mr. P. W. Willans said it had been a great pleasure to him to lay the results of the trials before the Institution, and to see that they were accepted as reliable. He felt that this was mainly due to the kindness of the gentlemen who had assisted him, and he wished to take the opportunity of thanking them for all their care and trouble, for without their assistance and testimony it would have been useless to expect that confidence in the results which had been to him the most gratifying feature of the discussion. Fig. 16 represented the engine with which the trials had been made. Only one objection, he thought, had been taken to the method of making the trials, and that was Mr. Cowper's objection as to their duration. In a mass of work such as those trials had entailed, it was clearly undesirable to spend more time on each than was necessary, and the question which naturally arose at the outset, was the one as to the possible error in the water measurement. The load being almost absolutely constant, the only error was that due to the observation of the water-level. He had given the probable average error as $\frac{1}{4}$ per cent.; but even had it been $\frac{1}{2}$ per cent., he thought it would not have been worth while to make twenty-four hour instead of three-hour trials. He feared that the "personal error" in a twenty-four hour trial would be apt to become a rather large one towards the end.

Fig. 16.



Scale 3 inches = 1 foot.

He thought that Mr. Cowper had hardly realized the enormous amount of work involved in these trials and in the calculations of the results; they had, as it was, involved about six months' hard work for himself and his assistant Mr. Low, to say nothing of others who helped with the calculations. The number of indicator diagrams which he had himself measured had been over three thousand, and he must say that he should have

Mr. Willans.

hesitated to turn the three thousand into twenty-four thousand for the sake of reducing an error of $\frac{1}{4}$ per cent. or $\frac{1}{2}$ per cent. Another point was that in trials such as these, which were intended to bring out small differences under different conditions, the state of the engine ought not to vary much from time to time if accuracy was to be ensured. The gentlemen who had very kindly made trials for him, fixed the duration of their own trials themselves. None of them suggested twenty-four hours, and he feared if he had asked them they would very likely have had other engagements. He proposed to deal with two points only which had been raised in the course of the discussion; first, the amount of water shown to be present in the engine, and secondly, the effect of the retention of any of that water on initial condensation. Mr. Cowper had spoken very strongly about the amount of water passing through the engine, and rather hastily assumed that it was necessary for the lubrication of the piston. So far from the initial condensation being excessive, he believed it to be the least on record, and as the amount of steam in the cylinder at the termination of the stroke agreed very well with the true adiabatic-expansion curve, it appeared to him that very little remained for jackets to do. He quite agreed that, in the case of low-speed engines, jackets were necessary, in order to prevent excessive initial condensation; without them it seemed impossible that the gain from their use should be large. Mr. Cowper thought that there would be, even in that case, a large gain, and he was very glad to hear it, for it might bring his results up to those for large engines, which he alluded to, but did not specify. Mr. Anderson had referred to Mr. Parsons' high-speed engine, and called attention, in terms which he understood to be those of praise, to the fact that the steam came out of it dry. He had not got a word to say against Mr. Parsons' engine, and he was delighted to be able to congratulate Mr. Parsons on the success which he had achieved; but if the steam came out dry, it was not merely a successful engine, it was a miraculous one. In a perfect engine, only water would issue from the exhaust pipe, and therefore he did not wish to see dry steam but wet steam come out of the exhaust pipe of his engine. The wetness of the steam did not afford any clue to the initial condensation, for almost exactly the weight of the steam initially condensed was re-evaporated before the steam escaped. The water coming out of the exhaust pipe was the water which had been formed in doing useful work with, perhaps, a slight addition due to radiation and priming. He thought one of the most interesting

Mr. Willans. facts shown by the trials was, that, in many cases where the expansion was great, almost all the water initially condensed was re-evaporated as the expansion proceeded, so that, at the end of the stroke, nearly all the steam due to the feed-water was there; the toe of each diagram approaching very closely to the adiabatic-expansion curve. Then as to the curves. Mr. Anderson asked why various curves were used? $p\,v\,\frac{17}{16}$ was not a curve answering to the expansion of steam in a cylinder at all; it was Brownlee's admirable expression for the relation between the pressure and volume of saturated steam. The curve which he had used, $p\,v\,\frac{7}{6}$, was slightly within the true adiabatic-expansion curve. The error might, at some points in the expansion, be 2 per cent. perhaps. The true expansion curve could not be exactly expressed by a formula of that character, and it appeared to be necessary to point out that he had not used it for calculating the efficiency in any case, but only for determining approximately the best number of expansions, and roughly, for comparing the mean pressure obtained with that which ought to be obtained, if only the steam shown by the indicator were present, and there was no steam initially condensed. For the purpose for which he used the formula $p\,v\,\frac{7}{6}$, namely, fixing the ratio of expansion at which to make the trials, it was desirable to use a curve rather inside than outside the true expansion curve. The adiabatic curve, due to the steam, shown by the indicator to be present at cut-off, was sometimes used as the standard of efficiency, and he wished to direct attention to the grave errors that might arise from so using it. Mr. Cowper was startled by an efficiency of over 100 per cent. Mr. Willans had not called it an efficiency; but he had seen the efficiency calculated in that way, and it was most misleading, because the mean pressure was increased owing to the raising of the curve by the re-evaporation of the steam initially condensed. It would be observed from the Tables that wherever the percentage of the theoretical mean pressure due from the steam present at cut-off was high, the thermo-dynamic efficiency was low. Mr. Cowper spoke of keeping the steam "alive," and Mr. Willans gathered from that that he wished to keep it in the state of saturated steam all through the expansion. Now steam was not put into a cylinder to keep it alive, but to convert the life which was in it, or its heat, into the motion of the piston. During the process a portion of this steam was condensed; in a good engine a large portion, and in a bad engine a small portion. It was better to get quit of the dead steam as soon as possible, and return it into the boiler again, rather than to re-boil it by heat added during expansion. If that were done, part of the heat would be used in making low-pressure instead of high-pressure steam, and there would be a reduced efficiency for such heat. That was most beautifully shown by Mr. Gray's $\theta\ \phi$ diagram. In the diagram, Fig. 1, which dealt with 1 lb. of water or steam, the vertical ordinates were the absolute temperature, and the area energy in heat-units. The indefinite area to the left of the line B represented the heat assumed to be in the 1 lb. of water when it was put into the boiler, say 673 U at 212°, the lower limit in a non-condensing engine. The area of the vertical strip between the line A and the line B, *i.e.* the areas K + M, represented the heat necessary to raise the water from 673° to 829° = 156 U. The area L + N = 858·7 units = the heat of evaporation of steam at 829° absolute. Then the area K + L was the heat which ought to be converted into useful work if the performance of the engine was perfect, and the area M + N was the heat necessarily wasted. At the termination of the expansion the proportion of the lb. of steam which should theoretically have been condensed in doing work, and be present in the cylinder as water, was represented by the line C D, the proportion which was still steam being represented by the line B C, B D being unity. Supposing the steam could be kept always dry and saturated by heat from the jacket, which was what he understood Mr. Cowper to mean by keeping the steam alive, the areas X + Y would denote the heat which had to be added, and the diminutive area X represented the heat which could be utilized. Thus, beyond preventing initial condensation, jackets could do no good, and it was not economical to keep the steam "alive" by adding heat during expansion. There were two ways by which initial condensation might be reduced; one was by jackets, which was only to his mind a partial remedy, as there were parts which no jackets could touch; and the other was by high speed and efficient drainage. Mr. Cowper said he was "anxious to find out the aim of the machine," and he would at once reply that what was aimed at was an economical engine in a small compass; it was not that an engine had to be made to drive a particular piece of machinery at 400 revolutions; but the object was to get the same economy of steam and greater economy of first cost and maintenance than with larger and more elaborate mechanism. If, as Mr. Cowper thought, Mr. Willans found a great advantage from the use of jackets, he could get that advantage very easily, for jackets could be adapted in a more complete and simple manner than in any other type of engine. As to the question whether the initial condensation observed was caused by the water, or iron, or both, he purposely expressed him- Mr. Willans.

Mr. Willans. self very cautiously, because he wanted to hear what was said on the other side. There did not appear to be any doubt in the minds of those who had considered the matter, that if water was present it would have a similar effect to that of iron. For instance, Hirn, the great supporter of the view that it was iron, said in his latest Paper on the subject: "That one could algebraically substitute water for iron as a reservoir of heat, we understand at once, without going into analysis" . . . and again, "Is it the iron of the walls, or is it a hypothetical supply of water, variable with various engines which show such divergencies from our equations, and which, quite as serious a matter, can alter the output of an engine by 40 per cent." Professor Unwin agreed that the water would have the effect supposed if the supply could be replenished, and Mr. Mair, although he spoke very strongly against that view, based his argument almost entirely on what appeared to him to be an ill-founded belief—that water could not permanently remain in the cylinder. Mr. Mair mentioned an experiment by Mr. Hirn, which he said proved positively, that water could not permanently remain there; but if he understood Mr. Mair rightly, he thought that experiment by no means conclusive on the point; he believed that water, placed as Mr. Mair described, must boil away, whereas he thought that water would, under certain conditions, accumulate in the cylinder to an extent which depended on the details of the particular engine. It seemed to him that one of the strongest arguments against iron being the sole cause of that loss was to be found in the fact, that although the laws which governed the transmission of heat to the iron from the steam, as explained by Dr. Hopkinson, were well known, it was only in accidental cases that they could be reconciled with the facts. Hirn admitted that, when he said that it was not possible to determine by the help of mathematics the consumption of steam of any given engine within 40 per cent. He would first deal with Mr. Mair's illustration from Hirn's experiments, from Professor Unwin's point of view, although he did not entirely agree with him. Suppose that what Professor Unwin said was correct, and that water placed in a cylinder boiled away unless replenished, could any situation be found where the water would be more likely to boil away and less likely to be replenished, than that which Mr. Hirn had chosen? No priming could well find its way up to the little reservoir, nor could the water formed during expansion get there except in infinitesimal quantity. Professor Unwin said that unless replenished by priming, or encouraged to remain in the cylinder by the abstraction of heat by the walls, the water would soon evaporate; but he thought Professor Unwin forgot the water formed during expansion, which would usually be more than enough to make good the waste he named. Professor Unwin took a case in which a drop of water, assumed to be in the cylinder at the temperature of exhaust, was increased by the condensation of the admission steam while being raised to the higher temperature, and was then reduced by the evaporation of steam supposed to take place at the exhaust temperature only. Mr. Willans had disagreed with Professor Unwin as to the result of this action; but he had no doubt now that, for that particular case, Professor Unwin was perfectly right. This was that a drop of water so acted on would boil away if not increased in the manner named above; but in an expansive engine there was, he thought, every reason to suppose that such an increase would take place. Mr. Gray had also shown that the drop of water would increase if the liquefaction, during expansion of the steam given off from the drop itself as the temperature fell, was considered, and if the temperature of the drop of water was assumed to follow that of the expanding steam. It was impossible to say whether Professor Unwin's view or Mr. Gray's agreed best with what actually happened; and he therefore agreed with Professor Unwin entirely that the balance was a most delicate one, and likely to be disturbed by the slightest change in the conditions. In Hirn's experiments, not only did heat pass into the liquid by direct contact with the steam, but it was also conducted through the little tube in such a manner that the resulting liquefaction did not add to the volume of water, as in the case of water in the cylinder; and being placed in such a position that no accidental addition of priming or other water could take place, the balance was disturbed, and the water boiled away—it would be very extraordinary if it did not. He was much obliged to Mr. Mair for the way in which he had spoken of the trials described, and he only regretted that he could not agree with him on that one point. The whole argument turned on the question of whether the water could remain in the cylinder. If it could it would have to some extent at any rate the effect he had attributed to it. Mr. Mair said he did not believe it was there. He himself had seen it. He had placed on the table three covers for the pistons of the engine junk rings, if he might call them so. One was the ordinary piston with a flat top, the second was hollowed so that the water drained to the exhaust ports at the centre, and the third was corrugated. The initial condensation was least with the hollow cover, and greatest with the corrugated one, in Mr. Willans.

Mr. Willans. other words the condensation increased with the facilities for the retention of water. If the cylinder was lifted quickly, and after due precautions to prevent water draining into or out of the cylinder, the water could always be found in quantity varying with the facilities for retaining it. In the simple trials the quantity of water in the cylinder was very great, so great as to make it difficult sometimes to take diagrams. He had listened with great interest to what Mr. Mair had said, and he had studied carefully Hirn's latest remarks on the subject, but he was unconvinced. He by no means held that iron played no part in the matter. It played a part certainly, and under some conditions a large part, but he held that water also played a part, and one which was too important to be neglected. He could not quite agree with Professor Kennedy as to the interpretation of the diagrams. He did not think that the point measured and denoted by the line *a*, Fig. 10, could possibly be on the crest of a wave; because at that moment the pencil was not oscillating, and there had been nothing to cause waves. It had just begun to fall after a period of comparative rest, and he believed that the diagram might be trusted to give a true indication of the pressure in the cylinder at that point. In reply to Captain Sankey, the lowest water consumption per electrical HP. recorded by him so far had been 22 lbs., but he expected under favourable conditions to obtain 1 electrical HP. for 20 lbs. of feed-water. Allusion had been made by Mr. Kirk to the early receiver compound-engine of Mr. Cowper. He was not sure when the first of these was made, but he believed that one of the earlier engines, if not the earliest, of this class had been made by Mr. McNab of Greenock, who, in the year 1860, designed a receiver compound-engine with steam-jacketed receiver, and cranks the angle of which could be altered with relation to each other. With this engine he experimented, and came to the conclusion that an angle of 90° was the most suitable one for marine work. In reply to Mr. Heenan, he did not quite see how the information (to obtain which the trials were made) could have been prepared by studying the figures for water-consumption per brake HP. He might say, however, that with the compound-engine fully loaded the brake HP. was fully 90 per cent. of the indicated HP., and the electrical HP. might be taken at fully 80 per cent. of the indicated HP. with an efficient dynamo. He had given, Plate 4, Fig. 2, the results of one trial, in which the electrical HP. was 82·3 per cent. of the indicated HP., and recent trials gave promise of even better results. He had not thought it desirable to furnish particulars of the number of

Mr. Willans. engines supplied for use in the Navy or elsewhere, as he wished the Paper to contain only such matter as was of interest to all engineers. He would reply to the various criticisms of the method adopted of calculating the thermo-dynamic efficiency in his answer to the various correspondents.

Correspondence.

Mr. Bodmer. Mr. G. R. Bodmer observed, with reference to the formula $p^6 v^7 = \text{constant}$ employed by the Author for calculating the theoretical mean pressure of the steam during the trials, that a similar expression was first applied to the expansion of saturated steam by Rankine. It must, however, be remembered that all formulas of this class were merely approximations, based upon the results obtained from the accurate formula for the expansion of steam in a non-conducting cylinder, and although useful to some extent, took no account, in their ordinary shape, of the condition of the steam at the commencement of and during expansion, that was to say, of the proportion of water present at the moment of cut-off, and whether condensation or re-evaporation subsequently occurred. It was by no means difficult to apply the accurate method of calculation, and, as he would show, some of the results given by the Author would have been to a certain extent modified had he used it. Among the quantities given by the Author in the Tables were the following:—(1) Percentage of total feed-water missing at cut-off. (2) Percentage of total feed-water missing at 0·604 of stroke or at cut-off low-pressure cylinder. (3) Percentage of total feed-water missing at end of stroke. Now the first of these quantities, which was of most importance, would not be affected by the substitution of the correct for the approximate method; but it was otherwise with quantities (2) and (3), which would be modified, as the following considerations would show. Suppose the steam to be dry at the moment of cut-off and then to expand, as it should do in a perfect engine, without loss or communication of heat; evidently these would be the most favourable conditions possible, and the results obtained under them should form the standard of comparison with less perfect processes. Now with dry steam at the moment of cut-off, and adiabatic expansion, by the end of the stroke a certain amount of condensation must have taken place, and this amount should not be included in the missing quantity, since it would occur with a perfect engine, and could not therefore be fairly considered as a deficiency. As an example

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