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. Mr. Willans.

Correspondence.

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 Mr. Bodmer.

Mr. Bodmer. he would take the data from Table I, under the trial letter and numbers S $\frac{110}{4 \cdot 4}$ (p. 164). Here there was a mean admission pressure of 106·34 lbs. per square inch, a ratio of expansion of 4·57, which might be taken as corresponding, with sufficient accuracy for the purpose of illustration, to a terminal pressure of 21 lbs. absolute. Assuming the steam to be initially dry, and calculating the quantity condensed by the time the pressure was reduced by expansion to 21 lbs., he found this quantity to amount to about 9 per cent. of the total weight of steam used. The percentage of feed-water missing at the end of the stroke was given as 21·53, but should be, in his opinion, strictly speaking, only 21·53 − 9 = 11·53, as otherwise 9 per cent. of feed-water would, by the Author's method, appear to be missing even with a non-conducting cylinder; whereas this proportion of condensation was the inevitable result of the adiabatic expansion of initially dry steam. The formula, as given by Clausius and Rankine for calculating the proportion of steam condensed (or re-evaporated) during adiabatic expansion, was—

$$x \frac{r}{T} = \frac{x_1 r_1}{T_1} + C \text{ hyp log } \frac{T_1}{T},$$

where r_1 and r denoted respectively the heat of evaporation at the initial and final pressure, T_1 and T the corresponding absolute temperatures, x_1 and x the proportions of steam to water in the working mixture, and C the specific heat of water, for which Clausius and Rankine adopted a mean value. Zeuner used the more accurate expression by which C varied with T, since the fluid heat—

$$q = t + 0 \cdot 00002\, t^2 + 0 \cdot 0000003\, t^3$$

where t denoted the temperature Centigrade. In making the preceding remarks he did not wish it to be supposed that he was finding fault with the Author's method, but merely with the application of the term missing to the difference between the observed quantity of feed-water and that indicated by the diagram after expansion had commenced. It might be well to call to mind, while dealing with the subject of cylinder-condensation, that the weight of water condensed during expansion in a non-conducting cylinder was no measure of the work done, since under certain conditions, when a given proportion of water was present at the moment of cut-off, neither condensation nor re-evaporation might be the net result of adiabatic expansion; that was, the proportion of water present was the same at the end as at the commencement of the expansion, although, of course, work had been performed. With regard to the conclusion arrived at by the Author, that the quantity of steam condensed was not affected by changes in the area of the exposed surface, it seemed to him that the trials, admirable as they were in every particular, were not adapted for definitely testing this point. The experiments carried out by Major English tended to show that the initial, or as he termed it, the clearance surface and not the total surface exposed, was chiefly instrumental in causing condensation; and this clearance surface the Author was not able to vary while leaving all other conditions unaltered, since it could be changed only when the simple was replaced by the compound or triple expansion system. Some portion of the greater economy of a compound, as compared with a simple, engine was not improbably due to the relatively smaller initial or clearance surface of the former. It might be objected that the engine tested by Major English was so different to that employed by the Author, that conclusions drawn from results in the one case were inapplicable to the other; but although the one engine ran at a very low, and the other at a high speed, it was noticeable that in the simple-engine trials the Author's engine consumed about the same quantity of steam per indicated HP. per hour at the lower ratios of expansion as that of Major English (42·76 and 35·96 lbs., Table I, line 20); and it must be remembered that at the moment when the steam was first admitted to the cylinder, and the greater portion of the initial condensation took place, the piston speed was nil or very low, so that differences in the mean velocity of the engines tested would not have much effect during that part of the stroke, as even with a high speed the velocity of the steam relatively to that of the piston must at the time in question be very great. A feature of great interest in the results obtained by the Author was the fact that, while with the simple engine a very considerable amount of re-evaporation occurred during expansion, in the low-pressure cylinder of the compound-engine there was very little, and in some cases no re-evaporation, but even a slight condensation. This indicated a nearer approach to adiabatic expansion in the latter than in the former instance, although still pointing to a communication during expansion of heat previously abstracted by the cylinder and piston metal. Even if the whole of the heat lost by initial condensation were subsequently restored during expansion, there would yet be a loss as compared with adiabatic expansion, as the increased work due to the restoration was not sufficient to compensate for the Mr. Bodmer.

Mr. Bodmer. larger quantity of heat absorbed. This was obvious when it was considered that any deviation from the adiabatic curve involved a departure from the best cycle, that of the perfect engine.

Mr. Clark. Mr. D. KINNEAR CLARK observed that the results of the experiments were corroborative of the long-known phenomena of the alternate condensation and re-evaporation of steam in cylinders worked with saturated steam, and of the increase of such condensation with the ratio of expansion in single cylinders, which he was the first to demonstrate[1] in 1851–52. The evidence also showed clearly what had usually been allowed, that the percentage of condensation decreased as the speed increased. The area of exposed surface, as calculated by the Author, did not in all instances express the whole area of surface instrumental in condensing the steam, for, at the higher rates of expansion, condensation was continued into the period of expansion, when of course the additional surface uncovered by the receding piston, so long as the steam was hotter than it, condensed the expanding steam. That such additional surface was instrumental in condensing the steam, was demonstrated by the evidence of indicator diagrams taken from a locomotive with exposed cylinders,[2] in which the expansion line fell nearly vertically from the point of cut-off. It was very doubtful, therefore, whether, as suggested by the Author, water present in the cylinder was a primary element of any influence on the process of alternate condensation and re-evaporation. The suggestion was a revival of Zeuner's hypothesis, that there was a quantity of water constantly in the cylinder which received and evolved alternately the heat of the steam admitted. This hypothesis had been thoroughly exploded by Hirn. Quoting from the Paper already referred to in Vol. lxxii., "Nothing is more common than a rush of water when the outside cylinder is tapped for the indicator, whilst the steam is worked with a considerable degree of expansion. With the goods engine, No. 125 C. R., of which the cylinders are" very much exposed, "the cylinders during one experiment were never free from a stream of water through the indicator, even for long-continued runs; and during temporary stoppages large accumulations of water were usually formed, which apparently could never be entirely dissipated, even through the open cylinder-cocks." In view of such a continuous flood of water, it was difficult to suppose that it was other than the result of condensation by surface. Whence otherwise, it might be

[1] Minutes of Proceedings Inst. C.E. vol. lxxii. p. 275.
[2] *Ibid.*, Fig. 4, p. 279.

asked, did it arise? Such water was scarcely perceptible when steam was admitted for the greater part of the stroke; and the gradually increasing quantity of water precipitated at the indicator, as the expansion-ratio was increased, evidently pointed to the greater range of temperature, and the comparatively greater area of condensing surface. The character of the expansive working with superheated steam in the cylinders of the "Great Britain" locomotive in 1850, might be referred to for the sake of contrast. He had shown [1] that there was less sensible steam, calculated as saturated, at the end of the period of expansion than at the beginning. This was an unreal deduction, and, in the light of the ascertained fact of alternate condensation and re-evaporation of steam in the cylinder, it was evident that the steam was in fact in a superheated state during admission, and that it behaved as a permanent gas, if not for the whole period of expansion, at least for the greater portion of it. The cylinder, suspended in the smoke-box, having been kept hot, at least as hot as the steam, and the steam having been rendered dry and gaseous by the heat of the smoke-box, there was, under the circumstances, no material degree of transference of heat between the steam and the cylinder; and the steam-gas was permitted to expand adiabatically, as was demonstrated by means of the indicator. Mr. Clark.

Professor J. H. Cotterill considered that the trials described in this Paper were of exceptional interest, as being the only experiments yet made at such high speeds and pressures; and probably, with the exception of those made by Messrs. Gately and Kletzsch,[2] the only experiments in which the effect of separately varying speed, expansion, and pressure had been systematically examined. Moreover, the remarkable consistency of the results showed that the Author had been unusually successful in eliminating experimental errors. The Author concluded that a small quantity of water, not spread over the whole exposed surface, but concentrated in its angles, was the main cause of initial condensation. No doubt certain facts were difficult to explain on any other hypothesis, such as: (1) the absence of any evidence that initial condensation was altered by change of condition of the exposed surface; (2) the apparently capricious differences between one engine and another; and (3) the small difference of economy between small engines and large ones. But there were other facts, which could hardly be explained without supposing that the surface had considerable Professor Cotterill.

[1] Minutes of Proceedings Inst. C.E. vol. lxxii. p. 283.
[2] The Journal of the Franklin Institute, third series, vol. xc. p. 326, 1885.

Professor Cotterill. influence, especially the marked economy undoubtedly realized in many cases by the use of a steam-jacket. And it must be remembered that the influence of surface might be expected to vary as $\frac{S}{\sqrt{N}}$, where N was the speed and S the surface; so that if it was imperceptible with the large surface of the Author's engine, it ought also to be imperceptible at a much lower speed when the surface was smaller. Again, on the Author's view it was not easy to explain the way in which the equality in quantity and in heat between condensation and re-evaporation, which was necessary for a permanent regime, could be obtained. It appeared to be necessary to suppose, a thing which was not *à priori* improbable, that the small quantity of water given by the Author in Table IV and elsewhere as about 0·05 lb., was the fictitious representative of a much larger quantity actually existing in the cylinder, which penetrated to different depths at different speeds by the changes of temperature in the course of a revolution. Perhaps the equality just mentioned might be realized by the penetration during evaporation being greater than that during condensation. On any possible view the range of temperature must be a principal factor, and it was satisfactory to find that the Author so fully endorsed this conclusion. The proportion in which the total range was divided by the temperature at the end of the expansion ought also to be important, but how far this conclusion was consistent with the Author's results was not at once apparent. A careful investigation would probably lead to the formation of a definite opinion on the vexed question of initial condensation; but the additional results of the condensing trials would be looked for with much interest, and it might be suggested that experiments at speeds and pressures, such as had been employed by other experimentalists, would be valuable as showing the influence of the special type of engine. The interesting experiments on the total heat of evaporation described in the Paper, like the recent determination by Mr. Anderson of the mechanical equivalent of heat, showed the advantage of conducting calorimetric experiments on a large scale. It could, however, hardly be supposed that the metal of the tank and the whole mass of water it contained really acquired a perfectly uniform temperature. Minute corrections appeared therefore of little importance; but it might be worth while to point out that, of the two values of x given in the Paper, the one described as not taking into account the variation in the specific heat of water was to be preferred, because Regnault experimented on the total heat, not the latent; the value $606 \cdot 5 - 0 \cdot 695\, t$, used on p. 152, as representing Regnault's value of the latent heat, being only obtained by supposing that the specific heat of water was unity. If corrections of this order were introduced, one was required to allow for the work of an ideal feed-pump necessary to complete the cycle of operations. It was satisfactory to find that the Author confirmed the conclusion that, in an ordinary boiler under favourable conditions of working, the amount of priming was small. In estimating thermodynamic efficiency, the Author considered as perfect, not an ideally perfect heat-engine for the limits of temperature employed, but an engine working under conditions more nearly approaching what was possible in practice. Much might be said in favour of this method; but if it was adopted it should be remembered that the standard of perfection was an arbitrary one, employed mainly for the sake of simplicity. It seemed, therefore, hardly worth while to employ the adiabatic curve as the standard expansion curve in place of the hyperbola, which was not only more simple, but also more nearly represented the actual expansion curve under the most favourable conditions of working. It would have been well if the Author had given some reason for the adoption of $\frac{7}{6}$ as the index of the adiabatic curve. He had never met with so large a value; at moderate pressures it varied little, being much more nearly $\frac{9}{8}$, and there was no reason to suppose it very different when the pressure was high.

Mr. B. Donkin, jun., remarked that the results and facts of the experiments had been carefully arranged, and the opinions given were few. The maximum number of carefully recorded facts, and the minimum number of opinions was just what was needed in many branches of mechanical engineering. The results had been admirably put on to curves, according to the "graphic method," which conveyed much to the eye, and required so few figures. A visit to the Author's testing-shed showed him that every arrangement had been made to ensure the most accurate results. He trusted that the Author would see his way to make a few experiments with increased piston-speed, say 500 to 600 feet per minute. It would be interesting to know the leakage, if any, of the steam passing the piston, while running, and this could probably be ascertained without difficulty. The Author had only tested for leakage with the engine not at work. He would further suggest that one or two trials be made with the steam-pipe between the boiler and the engine, eight or ten times as long as the one used, to see its effects on the results, and the missing quantity, particularly as the one employed was exceptionally short, probably shorter than usual

Mr. Donkin, Jun. in practice. The smallness of the indicator-diagrams taken with Crosby's indicator, was much to be regretted, and there was still a field for future improvement. Since the days of Watt, diagrams had been decreasing in size with increase of speed. The problem was, to get larger diagrams with higher speeds, and to be able to read them more accurately they should be larger.

Professor Dwelshauvers Dery. Professor V. DWELSHAUVERS DERY, of Liége, observed, that when live steam from the boiler entered a cylinder at the commencement of the stroke, it encountered in the clearance steam of very low tension, or already slightly compressed, and cold metal with which it came in contact. Its first work would not be shown by the indicator diagram, namely, that which was necessary to compress the steam in the clearance to bring it to its own tension. In this work it would lose a portion of its energy, as well as by leakage, radiation, and priming. It would also lose energy in reheating the metal which enveloped it, the base of the cylinder, the face of the piston, the walls of the steam-ports, and a small portion of the cylinder-wall. For it was impossible that saturated steam in contact with a colder body—the metal—should not warm it, and in warming it be partially condensed. In proportion as the piston advanced, fresh cold metal was exposed to the steam, and probably a fresh partial condensation was brought about. It was not probable that the body of vapour in motion in the cylinder and dead space during admission, was homogeneous. He was led to believe that the water of condensation on the heated cylinder cover, was swept forward; that thus, near the cover, the steam was relatively dry, while near the piston and the metallic surfaces recently exposed, there was more or less abundant precipitation, diminished to a certain degree by the metal of the piston which, at first briskly heated, partially vaporized the dew which covered it. Thus, even during admission, the double phenomenon was seen of the heat given up by the metal to the steam and by the steam to the metal. The final result, the difference of these two quantities of heat exchanged during admission, increased by the heat lost by the live steam to compress that which was in the dead space, he called R_a, and he was able to calculate it thus:—To the internal energy U_3 of the steam which was in the dead space, was added the heat of the steam admitted during the time of admission Q; and the internal energy of the steam in the cylinder at the end of the admission U_o, was equal to $U_3 + Q$, diminished by the external work during admission T_a, and by the heat R_a communicated to the metal or otherwise lost.

$$U_o = U_3 + Q - T_a - R_a.$$

The energy U_o of the steam at the end of admission was calculated by the pressure indicated at the same instant, as if the whole body of steam present was homogeneous and equally damp. If there remained a weight M_c of vapour in the dead space, and if there was added to it a weight M_a from the boiler, the total weight of damp steam in action would be $(M_a + M_c)$. Let V_o be the space occupied by the steam at the end of admission, and δ_o the weight of unit volume of saturated steam at the pressure P_o indicated on the diagram at the end of admission, the weight of pure steam at this instant would be $m_o = V_o \delta_o$, and the weight of moisture $(M_a + M_c) - m_o$; the percentage of steam in the mixture would be $x_o = \frac{m_o}{M_a + M_c}$; the internal heat $U_o = (M_a + M_c)$ $(q_o + x_o \rho_o)$, an expression in which q_o was the heat of the water,

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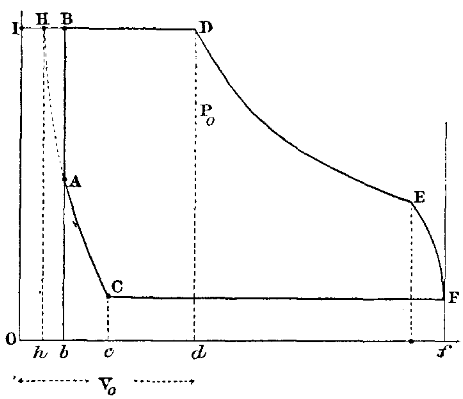
Fig. 17.



and ρ_o the internal latent heat of the steam. He could not conceive that the cushion steam should remain isolated from the incoming steam, and act separately without mixing; that it did not form an integral part of the total mass of steam in action. Fig. 17 nearly represented his view. The piston had commenced the compression of the steam in the clearance in c up to the end of the stroke b, following the line C A; then the steam continued the compression following a line such as A H. The volume of steam admitted was proportional to $\overline{HD}$ at the pressure P_o; the volume of steam which was in the clearance was proportional to IH; the volume of pure working steam was proportional to $\overline{ID} = V_o$. It was the volume of a mass probably not homogeneous, but concerning the composition of which there was no information.

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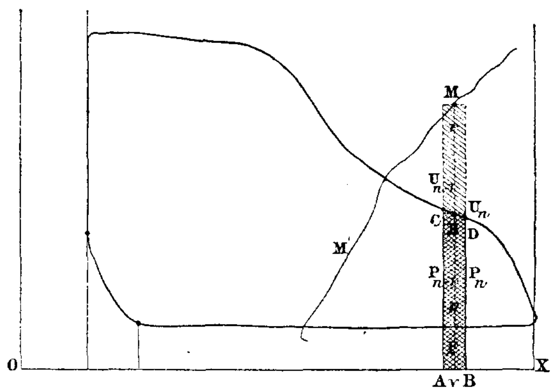
The hypothesis that it was composed of two masses acting separately, each in its own way, was not justified by any appearance. It was necessary, then, to treat it as homogeneous, any other hypothesis being arbitrary. If this theory was sound, he could not admit that the theoretical process of expansion was adiabatic. This property, in fact, would assume two things: first, that the mixture of water and steam in action was homogeneous, and afterwards that two bodies, steam and metal, at different temperatures, could be in contact without exchanging heat. But this was a theoretical impossibility, whilst homogeneity was ideal. The lack of exchange of heat was not, on the contrary, ideal, and not even desirable for the economical use that man made of the machine. In fact, during expansion, the metal could usefully restore all, or a portion of the heat that it had absorbed from the

FIG. 18.



steam during admission; and it always did so. For during expansion, at one time it was the steam that gave up heat to the metal, at another, the metal that gave up heat to the steam. He was of opinion that this fact could be clearly shown by the careful experiments of the Author; he would speak here only of the trials made with a single cylinder; and here he wished to apply the diagrammatic method that he would explain. In Fig. 18 (an indicator diagram) he considered the movement of a piston from A to B, generating the volume $v = \overline{AB}$. At A, the steam driving the piston possessed the internal energy U_{n-1}; at B, it had only the internal energy U_n because it had lost heat at first, to do external work $T^n_{n-1} = A \frac{P_{n-1} + P_n}{2} v = A p v$; afterwards to heat the metal, $R''_{n-1} = A r v$. In these expressions, A represented the inverse $\frac{1}{E}$ of the mechanical equivalent of a calorie. With the symbols above might be written

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$$U_{n-1} = U_n + T''_{n-1} + R''_{n-1},$$

hence

$$R''_{n-1} = U_{n-1} - U_n - T''_{n-1}.$$

Given the total weight of working mixture during expansion $(M_a + M_c)$, and the pressure of the steam considered homogeneous at the points A and B by the ordinates of the indicator diagram, the values of U_{n-1} and U_n were deduced therefrom. By means of the indicator diagram, or taking the approximate formula $A \frac{P_{n-1} + P_n}{2} v$, the external work T''_{n-1} was deduced. The preceding equation would show whether there was or not an exchange of heat between the steam and metal. If $R''_{n-1} = 0$, there was no exchange. If $R''_{n-1} > 0$, the steam heated the metal; finally if $R''_{n-1} < 0$, the metal gave up heat to the steam. The heat thus exchanged between fluid and metal could be represented by a continuous diagram which he called a *diagram of exchanges*, and which he constructed by the same method as the diagram of pressures. For this latter T''_{n-1} was proportional to $v \times \overline{EF}$. He calculated an ordinate $\overline{FM} = r$, so that on the same scale R''_{n-1} should be equal to $v \times \overline{FM}$. In this way he obtained a point M from the diagram of exchanges, and, repeating the same operation, the series of points M from which this diagram was constructed. By an arrangement which seemed rational, for the direct stroke he carried the ordinates representing positive exchanges above the axis O X, that was to say, when the steam heated the metal; and below it those which represent negative exchanges, when the metal gave up heat to the steam. For the return-stroke, he carried above the axis O X the ordinates which represented the negative exchanges, that was to say, from the metal to the steam; and below it, those which represented positive exchanges, or from the steam to the metal. Hitherto he had only applied his method to eight trials, made at Logelbach in 1873 and 1875, on an engine with a single cylinder, unjacketed, under the direction of Mr. G. A. Hirn, on which the late Mr. Hallauer had made a detailed report.[1] If it were permissible to draw conclusions from so small a number of trials made

[1] Bulletin de la Société Industrielle de Mulhouse, 1877, vol. xlvii. p. 144.

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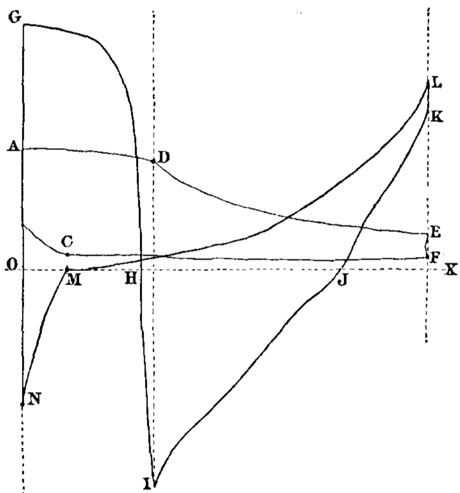
under various conditions, they would be generally as followed:—1. At the commencement of admission there was energetic positive exchange from the steam to the metal. 2. Before the end of admission, except in the experiment without condensation, and at the commencement of expansion, the exchange was negative, that was to say, from the metal to the steam, part of the water of saturation evaporated, and the value of x increased. With steam superheated before admission, but which was partially precipitated, it might happen that the steam, damp at the commencement of the expansion, might not only be dried but even superheated for the new pressure during part of the exhaust, and even to the end of the exhaust, as had happened in the trial without condensation. 3. Towards the end of the expansion, the steam again heated the metal, and was condensed on the walls of the cylinder, except in the non-condensing trial, when the steam was still superheated at the end. 4. From the commencement of exhaust, if there was water of condensation on the metal, the water evaporated, and the metal cooled in producing this evaporation. The more water of saturation present, the more would the metal lose heat in evaporating it. This was what Mr. Hirn called "Refroidissement au condenseur." When no water was present, and the steam was superheated at the end of the expansion, the metal did not give up heat, or only a little during exhaust. This happened in the trial without condensation when the metal received heat instead of communicating it. But here there was perhaps an error, because the result was very near zero. These average results were explained in the diagram, Fig. 19, where ADEFCA was the diagram of indicated pressures; and GHIJKLHMNG was the diagram of exchanges, decomposed thus:—GHI during admission; IJK during expansion; LHM during emission; MN during compression. There might well be objections to this sketch; and he thought he had shown all the weak points of it in his memoir to the 'Société Industrielle de Mulhouse.'[1] It would be too long to repeat them here. The Author had demonstrated experimentally that the extent of the active surface of metal had little influence on that of the exchanges of heat. But Mr. Dwelshauvers Dery could not admit the conclusion drawn therefrom, that the metal was passive. If surface was wanting, it was by the depth of the bed of metal affected that it would be supplied, from the moment that the difference of temperature became large. If the steam was super-

[1] Bulletin de la Société Industrielle de Mulhouse, 1888, p. 93.

heated the exchange would be slow, as shown by the diagrams; but if the steam was damp, the rapidity of the exchange would be extreme, because the water of saturation was without doubt in a state of unstable equilibrium. This water of saturation was the special vehicle of the heat, as Messrs. Hirn and Hallauer had pointed out. The appearances of the exchange of heat between damp steam and metal could be explained without recourse to the hypothesis of a permanent provision of water in the cylinder. All his diagrams of the exchange of heat had been prepared in accordance with the assertion of Hirn, that there was no such provision of water, and that at the end of condensation there only remained behind the Professor Dwelshauvers Dery.

FIG. 19.



piston pure steam at the indicated pressure. He did not think anything objectionable, or even difficult of explanation, would be met with in the results that the diagrams of exchanges presented. The interesting experiments of the Author had been made under conditions of extreme speed. It would be perhaps worth while to examine them by his method, applicable to a single cylinder, and to deduce therefrom the influence of the speed of piston on that of exchanges, and the difference of results obtained under this respect with and without condensation.

Professor J. A. EWING considered the experiments especially valuable on account of the systematic way in which the conditions of working had been varied, and the large number of different conditions under which one and the same engine had been worked. Professor Ewing.

Professor Ewing. These conditions, moreover, extended to higher speeds and pressures than had been examined in any earlier series of systematic tests. Particular interest attached to the trials in which the speed was varied until it reached 400 revolutions per minute, as showing, by the reduction which was found to take place in the amount of steam condensed on admission to the cylinder, that the process which went on within the cylinder became more and more nearly adiabatic as the speed was increased. The results exhibited in a striking way the advantage of high speeds, of moderate ratios of expansion, of high pressures, and of compound working. The efficiency actually realised was remarkable for the performance of a non-condensing engine of small size. He had difficulty in following the Author's conclusion, that there was but a small effect on the initial condensation when the surface exposed to the entering steam was varied. The Author appeared to expect more initial condensation when the boiler-pressure was high than when it was low, on the ground that "the steam being denser in one case than in the other, a greater number of steam molecules should be able to impart their heat to each square inch of surface." On the contrary, he should expect, *cæteris paribus*, that with dense steam, the portion condensed would be a smaller fraction of the whole, for the greater the density the less was the surface of metal exposed to the action of each lb. of steam. In point of fact, the results in Table VII did show less initial condensation for high-pressure than for low-pressure steam. The distinction which the Author drew between water contained in the clearance space and on the cylinder walls themselves, as factors in the production of initial condensation, was very possibly a valid and important distinction, but he wished the grounds of it had been more specifically stated. Students of thermo-dynamics might be somewhat puzzled by the Author's using certain thermo-dynamic terms in other than their commonly accepted conventional senses. The phrase "thermo-dynamic efficiency," for instance, had a meaning assigned to it on p. 162 and in the Tables very different from its stereotyped meaning. Again, when the Author spoke (p. 132) of "the most perfect engine possible," working under "ideal conditions," he was not speaking of a thermo-dynamically reversible engine following Carnot's cycle. The formula quoted from Clausius referred to a steam-engine receiving steam at the higher limit of the range of temperature, expanding it adiabatically to the bottom of the range, and rejecting it at that temperature. The cycle in such an engine was not reversible, on account of the absence of adiabatic compression; its want of reversibility, and the consequent falling short in efficiency from the efficiency of a perfect heat-engine, had been fully stated by Professor Cotterill in the eighth chapter of his well-known Treatise. In another point of theoretical interest, the Author was not quite at one with accepted authorities. His formula for the adiabatic expansion of steam, $p^6 v^7$, or $pv^{1 \cdot 167}$ = constant, used a higher index than Zeuner's, in which the index was 1·135 when the steam was initially dry, and still smaller when the steam was wet. Professor Ewing.

Mr. Isherwood. Mr. B. F. ISHERWOOD, Chief Engineer U.S. Navy, observed that the careful experiments made by the Author on his non-condensing steam-engine, were worthy of the closest attention. The practical result of these experiments was, among others, the ascertaining qualitatively and quantitatively, under the experimental conditions, the direction and extent to which cylinder condensation influenced the use of steam in the production of power; and the showing that this condensation was the cause of the enormous difference between the economic effects actually obtained in practical steam-engines, and those that should have been obtained according to purely theoretical considerations. Isolated experiments on steam-engines, giving the performance under one set of conditions only, or, at the most, with a very few variations, might be found in considerable numbers; but, owing to the fact that the economic results were greatly affected by the type and dimensions of the engine, and the circumstances under which the experiments were made, the results from different engines could not, with any safety, be combined, so that general laws might be confidently drawn from them, and a clear view obtained of the behaviour of the steam during its passage through an engine. What was imperatively needed for this purpose was an exhaustive series of experiments made with the same engine, the same methods, the same instruments, and the same observers, and this requirement had been largely supplied by the experiments under consideration. Objection might be made to an unreserved acceptance of the results, because of the very small dimensions of the experimental engine, the cylinder condensation depending quantitatively, other things being equal, on the area of the interior surfaces of the cylinder proportionally to its capacity; so that the smaller the cylinder the greater would be the steam condensation relatively to the weight of steam entering the cylinder per unit of time; but this did not affect the determination of the cylinder condensation qualitatively, and had, further, the advantage of causing the qualitative results to be more strongly marked and certain from their very exaggeration. The objection to the smallness of the cylinder

Mr. Isherwood. applied with greater force when the reciprocating speed of the piston, or the number of double strokes made by it in a given time, was about the same with a small cylinder as with a large one; but in the Author's experiments this objection lost the principal part of its force, from the fact that the reciprocating speed of his small piston was far greater than that which was usually employed in the case of large cylinders; and, as the cylinder condensation was much affected inversely by the reciprocating speed of the piston, the disparity of piston-speed in the two cases operated largely to lessen any difference in the quantitative results that might be due to excessive disparity in the size of cylinders. Quite hopeless would be the desire of having an exhaustive series of experiments, like these made by the Author, if they had to be made on medium-sized or large engines. The cost in money, time, and labour would be so enormous that they would probably never be undertaken, unless by order of some Government which did not regard expense; meanwhile, the best use should be made of the valuable knowledge so freely given by private effort. The Author's experiments were doubtless as varied as his apparatus would permit, and were unusually complete. Their accuracy was beyond question, and there only remained to inquire into the propriety of his methods, and to discover the causes that had produced the given results. The steam being used without condensation, the back-pressure in the cylinder would be somewhat above atmospheric pressure, and its temperature would be correspondingly higher than 212° Fahrenheit, or about 100° higher than the temperature of the back-pressure in the cylinder when steam was used with condensation. As cylinder condensation was diminished by lessening the difference between the temperatures of the steam and back-pressure, the cylinder condensation, with other things equal, would be less when the steam was used without than when it was used with condensation. Hence, the cylinder condensations in the Author's experiments were less than they would be in the case of a comparable condensing engine. The selection of a uniform speed of piston for all the experiments was most judicious. With small engines this equality was absolutely necessary in experimenting; but it became of less and less importance in proportion as the engine was increased in size, or as the cylinder condensations became smaller from any cause. Were there no cylinder condensation, differences of piston-speed would have no effect on the economic production of the power. The experiments showed that, under the experimental conditions, a marked increase of economy followed each increase in the reciprocating speed of the piston, other things remaining the same. But the economy due to this cause was greatly less in the experimental compound engine than in the simple engine, owing doubtless to the less cylinder condensation of the latter. With all other things the same, except the reciprocating speed of the piston, the absolute number of lbs. of steam condensed in a given time in a given cylinder would be the same, provided the transfer of heat between metal and steam were instantaneous; for during one-half the time the same steam-pressure was in the cylinder, and during the other half the same exhaust or back-pressure, so that as far as difference of temperature in the cylinder was concerned and time of action of this difference, there was nothing to produce inequality of result. As, however, the metal took time to absorb and to give out heat, less heat would be absorbed and given out as the time was less; consequently, the absolute number of lbs. of steam condensed in the same cylinder in the same time with all things the same except piston-speed, would be less with greater speeds of piston, but in what ratio only experiment could determine. In the experiments, this condensation was very irregular—even to contradiction; but the mean of all the trials, with both simple and compound engines, showed that the above inference was correct, and that the absolute cylinder condensation in lbs. per unit of time slightly decreased as the piston-speed increased. Of course, the percentage of condensation, referred to the feed-water consumed, would lessen in rather more than the inverse ratio of the piston-speed. Notwithstanding the evidence of considerable economy which the experiments showed in favour of high reciprocating speed of piston, not lineal speed or feet per minute, a word of caution was needed as regarded its practical adoption under other than the experimental conditions, which were that the speed of the same piston was to be increased, the pressure upon it remaining the same, so that doubling the piston-speed would double the power developed by the engine. If, however, a fixed power was required, and the choice, as regarded economy of fuel, was between a larger cylinder working with a given pressure and a given speed of piston, and a smaller cylinder working with the same pressure, but with a speed of piston greater in the ratio of the space displacements per stroke of the pistons of the two cylinders, the experimental results would not apply; for the smaller cylinder, other things being equal, would have a greater cylinder condensation than the larger one, so that the economy of fuel in the two cases might even be reversed from what the experiments indicated, and the larger cylinder with the slower

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Mr. Isherwood. reciprocating speed of piston might have less cylinder condensation than the smaller cylinder with faster speed of piston. Also, the comparison would be much influenced by whether the cylinders were steam-jacketed or not, by whether the steam was superheated or not, and by whether it was used with or without condensation, so entirely did the results depend on the limitations. The experiments, among other important results, showed the controlling influence exerted upon the cylinder condensation by the measure of expansion with which the steam was used. They showed, as all other similar experiments had shown, that the cylinder condensation, other things being equal, increased with the measure of expansion with which the steam was used. Mr. Isherwood regretted that experiments had not been made with steam without expansion, since, by the very small cylinder condensation that would doubtless have been found in such cases, they would have illustrated the fact that cylinder condensation was due to difference of temperature of the interior surfaces of the cylinder, only as a secondary cause, the primary one being the expansion of the steam. When steam was used without expansion, cylinder condensation was extremely small; but just in proportion as it was used more and more expansively, the cylinder condensation increased more and more, until, with excessive measures of expansion, this condensation might rise to over 50 per cent. of the weight of steam entering the cylinder. Now, in all these progressive cases, from no expansion to very great expansion, the difference of temperature in the cylinder remained exactly the same, being the difference between the temperature due to the initial steam-pressure and that due to the back-pressure. Hence, if cylinder condensation were caused by only this difference of temperature, it ought in the same cylinder, with everything the same except the measure of expansion with which the steam was used, to be constant, instead of which it increased in some ratio with increase of expansion. The expansion of the steam in the cylinder was then the originating cause of cylinder condensation, the heat-conducting metallic interior surfaces of the cylinder acting only in giving effect to this cause, by transferring heat from the cylinder to the atmosphere in the case of non-condensing engines, and to the refrigerating water of the condenser in the case of a condensing engine. With steam used expansively, the heat transmuted into the power developed by the expanding steam, that was to say, the power developed due to the mean pressure of this steam between the point in the piston-stroke at which the cut-off valve closed and the end of the stroke of the piston, was taken from the heat of the expanding steam, which

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latter suffered a corresponding condensation in every molecule; the result was that the expanding steam, instead of being transparent like non-expanded steam, appeared like a mist, fog, or cloud, the opacity of which was greater the greater the expansion of the steam; or, in other words, the greater the power developed by the expanding steam. The expanding steam was wet steam, and wetter in proportion to the expansion. Saturated steam, like other gaseous bodies, absorbed and gave out heat with difficulty; but when intimately mixed with watery particles, the latter received and emitted heat with facility and to a large degree, because of the great specific heat of water; and because, by virtue of their intimate mixture with the steam molecules, they acted on the latter so as effectively to cause them to become heat-carriers also. Thus the expanding wet steam, in proportion to its wetness, absorbed heat from the metallic surfaces of the cylinder, which heat was conveyed, on the opening of the exhaust, to either the atmosphere or the condenser, as the engine might be non-condensing or condensing, and was lost as regarded the economy of the power, having to be replaced in the metal by heat taken from the entering steam at the commencement of the next stroke of the piston. In this manner wet steam was the carrier of heat out of the cylinder, the metal of the cylinder acting like a sponge, to alternately take up heat from the steam entering, and to deliver it to the wet steam leaving the cylinder. A complete transfer of heat was thus made from the metal of the cylinder to the atmosphere or condenser, during each exhaust stroke of the piston, and a replacement of the same was effected from the entering boiler-steam at the next stroke. In restoring the heat which had been abstracted, the steam from the boiler necessarily underwent a corresponding liquefaction, the resulting water of condensation deposited upon the cylinder-surfaces being again vaporized by its contained heat, and by the heat in the metal of those surfaces, partly during the expansion portion of the stroke of the piston, and partly during the exhaust stroke, owing to the lessening pressure in the cylinder due to the expansion, and due to the cylinder being put in communication with the atmosphere or condenser. The cylinder was in this way, during a double stroke of the piston, alternately a condenser and a boiler. In fact a large portion of the pressure shown by an indicator diagram, between the closing of the cut-off valve and the end of the stroke of the piston, was supplied by the vaporization of the water of condensation, and this portion could scarcely be called expanded steam. As the piston moved onward, after the closing of the cut-off valve, Mr. Isherwood. there was a continuous succession of condensations and vaporizations, under the continuous lessening of the pressure up to the end of the stroke; but during the exhaust stroke there was vaporization alone. There was excessive heat reaction between the metal of the cylinder and expanded steam, which was wet steam. The expansive use of steam thus became the originating cause of cylinder condensation, the metallic surfaces furnishing the material through and by which it acted. The refrigeration produced in the cylinder, by the transmutation of heat into the work done by the expanding steam, was the primary cause of cylinder condensation, the quantity of the latter being in some proportion to the quantity of the former. This, he believed, was the first time these important facts had been stated; and a correct appreciation of them and of their consequences must form the basis of any sound theory of the steam-engine. They, and only they, accounted rationally for the enormous disparity found in the actual steam-engine between the theoretical weight of steam required per hour for the development of 1 H.P. under given limitations, and the actual weight of steam as found by experiment. Until the discovery was made that work was obtainable from heat only by the expenditure of an equivalent quantity of heat; or, in other words, that sensible or mass *vis viva* produced by a steam-engine could only be obtained by the transmutation into it of an equal quantity of the molecular *vis viva* of the steam, the experimental results from the steam-engine were inexplicable. When, however, the proper consequences of that discovery were considered, in connection with the facts of the heat-absorbing and heat-communicating properties of the metal of the cylinder, the differences of temperature in the cylinder, the production of water, though ever so little, by the refrigeration of saturated steam, and the nearly complete expulsion of the contents of the cylinder at the end of each stroke of its piston, each step of the process could be traced; the links of causes and of effects could be connected into a rational chain, and the resulting economic losses could be ascertained not only qualitatively but quantitatively. By thus arriving at a correct understanding of the causes of the loss of useful effect in steam-engines, engineers were enabled to apply such remedies as were in their possession, to comprehend the limitations imposed by nature and by art, and to know when all had been obtained that was obtainable, ceasing to waste effort against the insurmountable. James Watt stated that his engines lost much heat by the condensation of steam in their cylinders, due to the action of the metal of which they were made; but he gave no explanation of the how or why, nor was it possible for him or any person to have given it at the epoch when he lived. Mr. Isherwood. The science of thermo-dynamics had no existence until long after; and, until it was firmly established, the action of steam in the cylinder of a steam-engine had to remain a mystery. Watt did indeed offer an explanation, but it was both inadequate and erroneous. After him, from time to time, others rediscovered the fact called with convenient vagueness cylinder condensation, and recommended for its lessening the jacketing of the cylinder, and the superheating of the steam, as he had done; both efficacious means; yet these were only expressions of experimental results, unaccompanied by their rationale. They had got no further than Watt. So far was not difficult. All that was necessary was to compare the weight of steam evaporated in the boiler, with the weight of steam found as such in the cylinder at the end of the stroke, or some portion of the stroke, of the piston. To ascertain a fact, however, was one thing; to correctly account for it was quite another thing. Mr. Isherwood was the first person—and still remained the only one—who had calculated the weight of steam condensed in a cylinder by the transmutation of so much of the steam-heat as was equivalent to the power developed by the expanding steam, and had included this governing and primarily causing quantity in the explanation of the cylinder-condensation results, which without it could not be explained. No theory of the behaviour of steam in a steam-engine that did not include this transmutation of heat into the power of the expanding steam, and was not based upon it *ab origine*, could be true; that principle and its obvious corollaries, as above set forth, exclusively solved the problem. In the Author's experiments, as in those of others on the same subject, with the exception of his own, no recognition had been made of the effects due to the transmutation of heat into the power developed by the expanding steam, which was one of the deficiencies in that valuable series of trials. To return to cylinder condensation. When the steam was used without expansion, there was very little difference of temperature in the cylinder metal during a double stroke of the piston, let the difference between the temperature of the steam of initial pressure and that of the exhaust, or back-pressure, be as great as it might, because of the small heat reaction between the metallic surfaces and the contained dry steam. As a consequence, when the piston reached the end of its steam-stroke, there had been no water of condensation deposited upon the cylinder surfaces to be evaporated during the exhaust-stroke, and the resulting steam passed into the atmosphere or condenser carrying with it the total heat of vaporization. There would be only the nearly dry back-

Mr. Isherwood. pressure, which would, when forced out by the piston, carry off only the quantity of heat represented by the weight of the back-pressure multiplied by its specific heat, and by the number of degrees of heat it might have received by contact with the cylinder surfaces; and this quantity of heat was but small, especially in the case of condensing engines. In non-condensing engines, with their high back-pressure, more heat would thus be passed off. The lowering of the temperature of the cylinder surfaces, by the heat abstracted from them by the back-pressure, was the only reduction of temperature they would suffer from difference of interior temperature with non-expanded steam, and this lowering would be but slight, the surfaces remaining constantly at nearly the temperature of the entering steam. The steam of the back-pressure might be somewhat wet, however, because the exhaust being made for the most part by the elastic pressure of the steam itself when the exhaust-port opened, this steam would do work in expelling itself, and to that extent would suffer equivalent condensation; at the same time it was also more or less pushed out by the piston, which, to the extent that it increased the back-pressure, did work upon the latter, and superheated it to a corresponding degree. Probably these effects in opposite directions nearly neutralized each other. There remained another, and entirely independent, cause of cylinder condensation when the steam was used expansively, which he had noticed in one of his publications over a quarter of a century ago. It was the condensation due to the expansion *per se* of the steam. The condensation of expanding steam, thus far referred to, was caused by the work it did in expanding against an equal external resistance. The condensation due to expansion *per se* was not, however, due to that cause, but to the work done by the expanding steam in overcoming the cohesive resistance of its own molecules. This latter condensation would take place if the steam expanded without overcoming any external resistance. It was due wholly to overcoming internal resistance, and must exist if the steam molecules cohered. Now the experiments of Thomson and Joule on the passage of gases through porous plugs, whereby the gases underwent expansion without doing external work, showed that there was a sensible loss of temperature as the result of the expansion, which loss was greater as the gases were nearer their point of condensation, and was wholly due to the overcoming, in expansion, of the molecular cohesion. The fall of temperature was considerable with carbonic acid gas, which was the nearest approach to a vapour experimented with. Probably, with a vapour like saturated steam, the molecular cohesion

Mr. Isherwood. was strong, and there must be a correspondingly large transmutation of the heat of the steam into the work of overcoming this cohesion when the steam was expanding. The relations between the temperature and pressure of saturated steam, as compared with the similar relations of an ideal gas, showed how great was the departure of that vapour from Boyle's law, or, in other words, how great was its molecular attraction. Although data were wanting for a numerical determination of the effect of this cohesion on cylinder condensation, he was of opinion it would be found sufficiently great to require that it should be included in an exhaustive theory of the steam-engine. Important data, which should always be given in the case of steam-engine experiments, but which had never been given except in the accounts of the experiments made by him, were the mean pressure of the expanding steam (measured down to the zero line), and the fraction of the stroke of the piston during which the steam was expanded, as they were required for calculating the weight of steam condensed in the cylinder to furnish the heat transmuted into the power developed by the expanding steam alone. This was a very important quantity, as the condensation due to the interior surfaces of the cylinder was primarily caused by it and was in some proportion to it. This transmutation, of part of the heat of the expanding steam into the power developed by that steam, made the expanding steam wet steam, the condensation being effected upon every molecule of the expanding steam by the taking from each molecule the portion of its *vis viva* converted into the work done upon the piston by the expanding steam. The resulting water of this condensation was never deposited upon the surfaces of the cylinder, but remained intimately mixed in the steam, and was exhausted with it on the opening of the exhaust port of the cylinder. During the expansion portion of the stroke of the piston, the expanding steam was always in the saturated state; its pressure continuously fell and its temperature fell normally with its pressure, consequently, as the water of condensation just referred to must necessarily have the temperature of the enveloping steam, part of that water would vaporize when the pressure fell. Hence, from the commencement to the end of the expansion portion of the stroke of the piston, there would be continuous condensation of steam due to the work continuously being done, and there would be continuously re-vaporization of part of the resulting water due to the continuously decreasing pressure, thus rendering the calculation of the adiabatic curve of expanding steam an impossibility. The condensation referred to, due to the development of power by the

Mr. Isherwood. expanding steam, was not in the slightest degree influenced by the cylinder surfaces, nor by their changes of temperature; but would *occur just the same in a cylinder impervious to heat.* When steam was used expansively in a cylinder, the cylinder condensation was the aggregate of two entirely distinct quantities; one, the condensation due to the work done by the expanding steam, the other the condensation due to the metallic surfaces of the cylinder including those of the piston and steam passages, and to the changes of their temperature. The last quantity, however, was almost wholly caused by and bore a direct relation to the first, without which it could have existed to but a very trifling degree. Hence, of the difference between the weight of steam evaporated in the boiler according to tank measurement, and the weight existing in the cylinder at any point of the expansion portion of the stroke of the piston according to indicator measurement, only the part which remained, after deducting the condensation due to the power developed by the expanding steam, should be credited to the effect produced by the surfaces of the cylinder and their changes of temperature. Wet steam—that was, steam from every molecule of which some *vis viva* had been taken; or, in other words, steam that had been subjected to partial molecular condensation as opposed to complete mass condensation—had the property of conducting heat in a marked degree, while dry steam, on the contrary, was practically almost destitute of this property. This kind of wet steam could only be produced by using steam expansively, and the more expansively it was used the wetter it was, and the wetter it was the greater was its power of conducting heat. From the foregoing it would be perceived how indispensable, in the analysis of steam-engine performance, was the knowledge of the weight of steam condensed in the cylinder to furnish the heat transmuted into the work of the expanding steam. It was the key to the solution of the problem of steam-condensation in working cylinders, no rationale of which could be given by any hypothesis that ignored it. Unexpanded steam was dry and transparent in a working cylinder throughout the whole stroke of the piston, suffering scarcely any condensation, no matter how great the difference between the temperature of the entering steam and the temperature of the exhaust or back-pressure. Expanded steam was wet and opaque, and wet and opaque in proportion to the measure of its expansion. The surfaces of the cylinder, and their changes of temperature, were involved in the problem of cylinder-condensation only by means of the wetness of the expanding steam, the finely divided water of condensation forming a connection between those

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surfaces and the interior of the body or mass of the steam in the cylinder, and enabling this mass in function of its capacity as well as of its external surface to abstract heat from the metal surfaces. This action was entirely different from that of steam not doing work condensed on the surface of a cooler mass of metal. In such a case the steam was dry, and only the layer in direct contact with the surface was affected, the condensation being wholly superficial and proportional to the area of the surface and the difference of the temperatures. But in the case of cylinder-condensation when the steam was used expansively, the condensation was not only at the superficies of the mass of steam but was throughout the entire mass, every molecule of which, so to speak, was simultaneously acted upon by the metallic surfaces of the cylinder. Hence, the enormous quantity of condensation which comparatively small surfaces of cylinder were enabled to effect. If any cylinder not doing work, the exhaust port being kept closed and the piston stationary, was put in communication with the steam-room of a boiler, the metal of the cylinder being maintained at the temperature of the exhaust steam, by the external application of cold, the condensation of the steam would be entirely superficial, and the water of condensation would be deposited upon the metallic surfaces, and would be insignificant in quantity compared with that which would be obtained from the same cylinder in operation, using the steam expansively between the same temperatures. This great difference of effect denoted an equally great difference of cause in the two cases, which depended on the fact that in the one case the condensation was of dry steam at the surface of contact, while, in the other case, the condensation was of wet steam molecularly throughout the mass. When unexpanded steam was used, the metal of the cylinder remained during a cycle or double stroke of the piston at nearly the temperature of the steam. Suppose that, at the commencement of the stroke of the piston, the metal of the cylinder had the temperature of the entering steam, evidently the metal would retain that temperature to the end of the stroke of the piston. Now, when the exhaust port opened, the steam would expand into the atmosphere or condenser against whatever elastic resistance existed in those places, and would thus do work, losing heat and temperature proportionally; but it would suffer only a little molecular condensation in consequence, because the heat converted into this work would not differ much from the heat due to the difference between the steam-pressure and the exhaust-pressure, and which by the lowering of the pressure had become available as an addition to the heat of the back-pressure.

Mr. Isherwood. Thus, during the exhaust stroke of the piston the back-pressure would be nearly dry; and, being but little in weight, would abstract only a small quantity of heat from the metal. If the steam was exhausted into an absolute vacuum, the back-pressure would be superheated. When expanded steam was used in the cylinder, the refrigeration of the metal of the latter took place during the expansion portion of the stroke of the piston by the absorption of heat from the metal by the wet steam, so that when the piston commenced its stroke the entering steam flowed in upon a metal surface considerably colder than itself, and consequently underwent a condensation of the quantity required for furnishing the heat necessary for raising the temperature of the metal to that of the steam. This amount was ascertained experimentally, in any given case, by subtracting the weight of steam, as shown by the indicator at the point of cutting off the steam, from the weight of steam evaporated in the boiler. The heat transmuted into the power developed by the steam before expansion, that was, between the commencement of the stroke of the piston and the point of cutting off, had already been furnished in the boiler during the evaporation there. The back-pressure from expanded steam was wet steam, and assisted during its expulsion in the refrigeration of the metal of the cylinder. But there was still another cause, beside cylinder condensation, which greatly affected the economic performance of a steam-engine. It was the relation which the pressures on the opposite sides of the piston bore to each other, the one being the steam-pressure and the other the back-pressure. The economic performance of an engine should always mean the quantity of heat expended in a unit of time to cause it to produce a given useful dynamic result, say, the development of 1 HP. during that time employed in overcoming the external commercial load, or doing the work for which the engine had been constructed. Now, the total work done by the steam was represented by the power developed, calculated for the mean pressure on the piston taken down to zero or the line of no pressure. This was the total HP.; it was the entire dynamic effect produced by the entire steam; and, for the purpose of ascertaining the true or complete performance of the steam, this total HP. must be considered in proportion to the total steam expended. The determination of the cost of the total HP., in lbs. of steam consumed per hour, was essential to a clear understanding of the theory of the steam-engine, and should always be included in the investigation of experimental results; otherwise, not only was an important part of the work of the steam left unaccounted for, but

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its great effect upon the economy of the remaining part was left unexplained. In the Author's account of his experiments, as well as in the accounts of the experiments made by all investigators except his own, the total HP. was not given nor even referred to, but the indicated HP. was taken as the measure of the effect produced by the entire weight of steam consumed per hour. Now the indicated HP. was only a fraction, and a very variable one, of the total HP. It was what remained of the total HP. after deducting the power required to overcome the resistance of the back-pressure; and as the latter, with a given type of engine, might be taken as constant, different steam-pressures would give with the same back-pressure very different economic results, measured by the cost of the indicated HP. in lbs. of steam consumed per hour from the boiler, which, without a knowledge of the total HP., would be inexplicable. With a constant back-pressure and a very low steam-pressure, each increase of the latter would produce great economy, which had led to a wide-spread belief of some wonderful efficacy in high pressure *per se*; whereas the economy was due almost wholly to the increased fraction of the total pressure utilized as indicated pressure. Of course, this increased fraction thus utilized of the total pressure became less with each increase of the steam-pressure, until, as the cylinder condensation increased with the difference between the steam-pressure and the back-pressure, the loss by this increased condensation exactly neutralized the gain by the increased fraction utilized of the total pressure by the increased steam-pressure, after which an economic loss instead of gain would follow further increase of pressure. The indicated HP. was not, however, the proper dynamic unit for measuring the commercial economy of steam-engine performance. That HP. included, in addition to overcoming the external load, the power required to work the engine *per se* or unloaded; and, as the piston-pressure needed to do this was constant for a given engine, let the piston speed and the load be what they might, it must be deducted from the indicated pressure, as only the remainder or net pressure was applied to the crank-pin and utilized in doing external or commercially valuable work. Now the pressure on the piston required to work the engine *per se*, bearing no relation to the steam-pressure and being entirely independent of it, might be any fraction, large or small, of the indicated pressure, and could, therefore, enormously influence the economic result as measured by the commercially valuable work done, if the indicated pressure was small, and but little if it was large. Hence, in all steam-engine investigation for the purpose of

Mr. Isherwood. ascertaining the true economic result following the use of steam under different conditions, the dynamic unit should be the net HP., and neither the indicated nor the total HP.; indeed, the indicated HP. was not directly needed in the determination. The total HP. gave the philosophic result for the total steam expended, and the net HP. gave the commercial result for the same quantity of steam; yet these two essentially necessary determinations had always been ignored, and only the indicated determination given, which expressed nothing clearly, and confused two entirely dissimilar and independent quantities. When the economic comparison was made for the net HP., the effect of increased steam-pressure became still more marked, as the fraction of the total pressure utilized for external work was then less than when the indicated HP. was employed for that purpose. Thus, it appeared that the economic result of using steam under different conditions of pressure, expansion, cushioning, back-pressure, speed of piston, &c., was so greatly affected by the conditions, that it was important the proper units of comparison should be employed, or erroneous determinations would be announced. Further, in reporting the results of experiments, care must be taken to give every possible experimental quantity separately, and not those only which appeared necessary to the investigator, as regarded the support of his own views. Others might be able to draw from the complete quantities very different and, perhaps, much sounder conclusions. In all things relating to the use of steam, for the production of power, so much depended on the limitations, on the type and proportions of the engine, &c., that unless the whole evidence was submitted the case could not be properly judged, and very erroneous opinions might be formed. The value of the Author's experiments would have been much increased had he given for each the pressure at the point of cutting off the steam, the pressure at the end of the stroke of the piston, the back-pressure against the piston both including and excluding the cushioning, the point of the stroke at which the cushioning of the back-pressure commenced, and the back-pressure itself at that point, also the mean pressure of the expanding steam measured down to the zero line. The pressure required to work the engine, *per se*, could be easily ascertained from indicator diagrams taken with the engine operated without its load, and with any speed of piston. The determination of the measure of expansion with which the steam was used was quite impossible, as would easily be understood from the foregoing, because the weight of steam as such in the cylinder at different points of the expansion part of the stroke

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of its piston varied at every point. There were continuous condensations and vaporizations; sometimes the former exceeded the latter and sometimes the latter exceeded the former. The best that could be done was also the simplest; it was to divide the space-displacement of the piston per stroke, plus the space in the clearance and steam-passage at one end of the cylinder, by the space-displacement of the piston per stroke up to the point of cutting off the steam plus the space in the clearance and steam-passage at one end of the cylinder. The quotient would be the ratio of these bulks, which were the spaces occupied by the steam; and, it would correctly give the measure of expansion, were the weight of steam as such constant throughout the expansion portion of the stroke of the piston. The Author had not followed this method, and he did not think his variation an improvement; but it strongly illustrated the necessity of an experimenter furnishing all the experimental quantities separately, so that others might make their own calculations from them. Various authorities, and among them Rankine and Zeuner, had given formulas for the calculation of the adiabatic curve of expanding steam doing work, and the Author had used one of them. The calculation, however, could not be made; it was beyond the reach of mathematics, owing to the continuous condensations and evaporations of expanding steam when doing work, both external and molecular, and even supposing the expansion to take place in a cylinder impervious to heat. There was no use attempting the impossible; and, in this case, the result only misled. Many of the formulas regarding the behaviour of steam, so confidently set forth by professors, were just as erroneous as the one in question. Among them was one employed by the Author to ascertain whether any, and if any, how much, water was carried over to the cylinder from the boiler by the steam. This formula originated with Mr. Hirn, and had been extensively employed without question all over the industrial world; nevertheless, it gave erroneous results, as would easily be perceived from the following:—The principal quantity in it was the total heat of the boiler-steam taken from Regnault's table of the same. This total heat could be obtained from the steam only on the condition that the latter was condensed under the same pressure as it was generated under. In other words, that during condensation it should have as much work done upon it as was done by it during its generation. Now when the steam was drawn from the boiler and condensed in the water of a tank beneath the atmospheric pressure, it was condensed under less pressure than it was generated under, and would have correspondingly less total

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Mr. Isherwood. pipe; because the difference of pressure at the two ends of the pipe being proportional, approximately, to the boiler-pressure above zero, more power would be used with the higher-pressure steam to transport it to the cylinder, and correspondingly more heat would be taken from the expanding steam in the pipe to produce this power. Hence, in all cases, the shorter the steam-pipe, other things equal, the greater would be the economic result, and this independently of any losses of heat by external radiation. For maximum economy, the boiler and cylinder should be brought together as closely as practicable, and there could be no greater mistake, from the fuel economy point of view, than to have in any large manufacturing establishment the boilers concentrated in one place, while the engines were in different places receiving the steam through long pipes. The universal opinion was that if these pipes were sufficiently protected from loss of heat by radiation, there would be no other loss. This erroneous opinion was based on utter ignorance of the law in accordance with which the steam was transported, as just shown. The loss of useful effect, due to long steam-pipes, was of considerable practical importance, the difference being between the fuel cost of the power when using comparatively wet or dry steam in the cylinder. And further, for equal initial cylinder-pressure, the boiler-pressure would have to be higher the longer the pipe, involving greater cost of fuel in the vaporization of the water, and greater loss of heat by radiation. A description of any set of experiments ought to include the length and diameter of the steam-pipes, for they entered sensibly as factors in the economy of the performance. In this connection the remark had frequently been made that steam superheated at the boiler, say from 15° to 30° Fahrenheit, lost about all of this superheating before it reached the cylinder, when it passed through pipes of medium length. This loss of the superheating, as well as the considerable reduction of pressure between the boiler and the cylinder, had always been erroneously attributed to loss of heat by radiation. But both phenomena were due to the same cause, namely, the refrigeration of the expanding steam in the pipe by the heat abstraction, in order to produce the power developed by the expansion, to transport the steam from one end of the pipe to the other. If the steam in the pipe did not effect its transportation by its own expansion, there would be no sensible loss of either pressure or superheating in a well-protected pipe. The same facts applied to the length of the exhaust pipe between the cylinder and the condenser; the longer this pipe the greater would be the difference between the back-pressure in the cylinder and the

Mr. Isherwood. pressure in the condenser. If the foregoing principles were intelligently applied to the Author's experiments, the various results obtained, due to his various modifications of the conditions, would be explained and easily understood. There would be no anomalies and no incongruities. The economic use of steam in a steam-engine depended conjointly on the following conditions:—1st. The relation of the steam-pressure on one side of the piston, measured down to zero, to the sum of the back-pressure on the other side, also measured down to zero, and of the pressure required to work the engine *per se*, because the piston moved only in virtue of their differential, that was, did external work; and because the steam expended was that due to the power developed calculated for the steam-pressure down to zero, while the differential was only a variable fraction of this pressure. 2nd. The absolute boiler-pressure, because a greater percentage of the heat of evaporation was utilized dynamically at higher than at lower pressures, subject to greater loss by greater radiation, and by the greater power required to transport the steam from the boiler to the cylinder, and by the greater loss due to the escape of the gases of combustion from the boiler at a higher temperature with the higher pressure than with the lower pressure. This last quantity was a very important one, as the furnace temperature was constant. Also, the gain by higher absolute boiler-pressure was limited by the proportionally greater cylinder condensation attending it, when using the steam with the same measure of expansion. The greater the initial cylinder-pressure, other things being equal, the greater would be the fraction utilized of the total mean pressure on the piston, and herein lay the principal gain attending the use of higher pressures; but this gain became rapidly less as the pressure became greater, the back-pressure remaining constant, until, within practical limits, increase of pressure ceased to be advantageous. The limit of initial cylinder-pressure was, of course, higher with non-condensing than with condensing engines, because the back-pressure was greater with the former than with the latter. 3rd. The measure of expansion with which the steam was used. The gain from this source was limited by the existence of back-pressure and pressure necessary to work the engine *per se*, because the more expansively steam of a given pressure was used the less was its mean total pressure on the piston, and the less was the differential of pressure producing motion. Also, by the fact that the cylinder condensation was primarily due to, and was in some proportion to, the expansion of the steam; and by the fact that the spaces in the clearances and steam-passages at the ends of the cylinder enormously affected the

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gain by expansion, diminishing it in a far higher ratio for the greater than for the less measures of expansion. And, finally, by the fact that the more expansively steam was used, the larger must be the cylinder to develop with it a given power, thereby furnishing more interior surface for cylinder condensation, and more exterior surface for heat radiation. 4th. By the reciprocating speed of the piston. When the reciprocating speed of the piston was increased, other things being equal, the power developed by the engine was proportionally increased. Now the commercial problem was, how was the economic performance affected by the speed of the piston when the development of a constant power was required. This admitted of two solutions, one of which was lowering the net piston-pressure in the same ratio that the piston speed was raised: the other was retaining the same net pressure, but using it in a cylinder of proportionally less capacity as the speed of the piston was greater. Experiment alone could solve these problems; but the method adopted by the Author of merely increasing the speed of the piston of the same cylinder, without change of piston-pressure, though giving interesting and valuable results for the conditions, and commercially useful ones for the case where additional power was required to be developed with the same net pressure on the piston, had no value where constancy of power governed the problem. In a commercial point of view the solution really needed was for equal net piston-pressure and equal net power developed, the practical conditions being obtained by lessening the capacity of the cylinder in the ratio of the increase of its piston-speed. 5th. By the amount of the cushioning or compression of the back-pressure. The back-pressure, or pressure remaining in the cylinder during the time the exhaust port was open to the atmosphere or condenser, could be compressed up to the initial cylinder-pressure with decided economic gain. This was not because it thus filled the clearance and steam-passage spaces at the cylinder ends, thereby lessening the quantity of steam drawn from the boiler per stroke of piston; for the power expended in the compression, with the measures of expansion practically used for steam, was about equivalent to the steam saved by the compression; but because during the compression the steam was superheated, or had its temperature raised by the work of compression done upon it, and being in contact with the relatively cold surfaces of the interior of the cylinder, imparted heat to them, all the heat so imparted being a clear economic gain, as it proportionally lessened the cylinder condensation during the succeeding stroke of the piston. If the steam-valve of a cylinder were so set that it began to open

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retained in the cylinder, was not precisely equal in value to the power expended in compressing it; the balance was much affected by the measure of expansion with which the steam was used. For all cut-offs later than two-thirds of the stroke of the piston there was a loss in this respect by cushioning, but not a great one unless the expansion was very small: for all cut-offs earlier than one-third of the stroke of the piston there was a gain in this respect by cushioning, but not a great one unless the expansion was excessive: and for all cut-offs between one-third and two-thirds of the stroke of the piston there was substantial equality; but for all measures of expansion there was a gain by cushioning when the thermal effect was included in the comparison. The gain by cushioning, all effects being included, might vary with the measures of expansion habitually employed from 4 to 8 per cent., a very important quantity, showing that in any experiments with the steam-engine the fact of the cushioning or non-cushioning should be distinctly stated, and the amount of cushioning described. The effect of the foregoing various and variable conditions would be extensively modified, other things being equal, by superheating the steam, or by enveloping the cylinder in steam-jackets. In these cases the losses due to cylinder condensation would become of less importance, and the gains due to pressures would become of more importance. The Author had used the number of lbs. of feed-water, consumed per hour per indicated HP. developed by the engine, as the measure of the economic effect of the steam. This was only accurate for the purpose of comparison when in all the experiments the boiler-pressure was the same; but when that pressure varied, and greatly too, as in his experiments, another quantity should be taken for ascertaining the economic effect, namely, the number of units of heat consumed per hour per HP.; and this quantity should be computed for a uniform temperature of feed-water, as all the experiments were made with a non-condensing engine. With this kind of engine the temperature of 200° Fahrenheit could be commanded practically for the feed-water, and that temperature should be assumed in calculation. Of course, the Author's results could be easily corrected in accordance with this suggestion, by multiplying the lbs. of water by the difference between the total heat of the boiler-steam in Fahrenheit units and the heat in the feed-water in the same units. It was bootless to make experiments as close as 0·5 per cent. as regarded the measured quantities, and then to calculate the results in a manner that might give an error, comparatively, of several percentages. Unless the economic results were expressed in units of heat per

Mr. Isherwood. dynamic unit, and for the practical temperature of feed-water with each kind of engine, there could be no comparison between the heat economy of condensing and non-condensing engines, the feed-water in the case of the former having a proper temperature of 100° Fahrenheit less than the latter. Now, as more heat was required to vaporize 1 lb. of water under a higher than under a lower pressure, the substitution of units of heat consumed in place of lbs. of water influenced the economy against the higher pressures. There was also another correction necessary where the boiler-pressures were different; but its value would have to be estimated in the absence of data, namely, the correction due to the higher temperature with which the gases of combustion must leave the boiler in the case of higher steam-pressures. This correction was also against the economy of steam at the higher pressure, and must be included in any correct determination of economic results referred to fuel consumed. That it was worthy of attention would appear from the facts, that the temperature of the gases of combustion in the furnace was constant and about 1,750° Fahrenheit, that the heat escaping from the boiler in these gases was measured directly by their temperature in the chimney, and that this chimney temperature must be as much greater with higher-pressure steam as the temperature of such steam was greater. For example, a chimney temperature of 525° Fahrenheit caused a loss of 30 per cent. of the heat of combustion, and a chimney temperature of 560° caused a loss of 32 per cent., a difference of 35°, being equivalent to a loss of 2 per cent.; and this was what would result from a difference of 35° in the temperature of the boiler-steam. Steam of 75 lbs. pressure per square inch above zero had a temperature of 307°, while that of 150 lbs. pressure had a temperature of 358°, a difference of 51°, and equivalent to a loss of heat of nearly 3 per cent. The object of experiments, made under varying circumstances, was to ascertain in each case the consequent gains and losses of useful effect referred to quantity of heat expended. The amount of these gains and losses was true for the particular engine and the particular conditions only, the experiments determining the how much. The differences under different circumstances might be large; and although experiments on any steam-engine would show all the causes of loss or gain qualitatively, yet direct measurements were needed to ascertain their value quantitatively, for each engine and for each group of circumstances. Hence, experiments could never be too many, for only after accurate results had been obtained from a very great number of engines of different types, dimensions, valves and valve-gear, and

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using the steam under different conditions of steam-pressure, expansion, back-pressure, *cushioning*, *piston-speed*, *superheating*, steam-jacketing, &c., could a correct theory of the steam-engine be formed, and data obtained from which in any given case the economic performance could be predicted. To this collection the Author had made an important contribution, for which he was entitled to the thanks of not only engineers but the world, for all the world was interested in the improvement of the steam-engine, its great motor and most useful servant, without which the present civilization would be impossible.

Professor Perry.

Professor J. PERRY observed that Rankine, in his memoir on John Elder, published about twenty years ago, expressed views on steam-engine phenomena which coincided with those of the Author of the Paper. The Author had shown that 0·072 lb. of water in the cylinder would account for the observed effects. This water, spread over the admission surface, would only cover it to a thickness of $\frac{1}{130}$ inch, and its existence was therefore quite possible. Considering that the average temperature of the gases in a gas-engine cylinder was much above that of melted cast-iron, and that there was cold water always circulating in the jacket, and that in spite of all this only about half the total heat of combustion was given up to the cylinder by the gases, the possibility of the dry iron of a steam-engine cylinder acting so well as a carrier of heat as was usually supposed, might well be doubted. But if there was a film of water over the surface, or a pocket of water, the surface behaved in a very different way. It was not sufficiently recognized that water attained with extreme rapidity the temperature of steam in contact with it in a space free from air. It was not by conduction nor radiation of heat that the action took place; it was by evaporation and condensation. It must be remembered, also, that with this film of water upon it, the metal of the cylinder became operative. Experienced men never talked of the conductivity of the iron as of importance in the cooling of dry steam in a cylinder. Some people had impugned Mr. D. M'Farlane's experiments with a copper ball,[1] because he neglected the conductivity of the metal; but it could be shown quantitatively that if the ball had been made of a very non-conducting material, it would have cooled with much the same rapidity. It was the surface emissivity which was of importance in the heating and cooling of the cylinder walls. With no film

[1] Report of the British Association for the Advancement of Science, 1871. Notices, p. 44.

Professor Perry. of water, this would be so small that the cylinder walls would absorb or give out very little heat; but, with such a film, a new sort of phenomenon took place, the emissivity became enormously increased, and even if the water itself did not perform its evil function, it would enable the cylinder walls to do so. He accepted Rankine's conclusion, that if water could be prevented from forming in the cylinder, the range of temperature would not produce much evil. Rankine had said that high speeds, superheating, steam-jacketing, and compounding were the remedies, and he thought that the Paper had given an accurate proof of Rankine's propositions. He felt sure that the Author was wrong in concluding that in a perfectly non-conducting cylinder the water must increase, there being no priming. The problem was very simply put as followed, t_1 being the initial and t_3 the final temperatures:

1st. When ω lb. of water at t_3 was suddenly immersed in an atmosphere of steam maintained at t_1, it received heat $\omega\ (t_1 - t_3)$ by the condensation of ω^1 lb. of steam, where $\omega^1\ \lambda_1 = \omega\ (t_1 - t_3)$ λ_1 being the latent heat of steam at t_1.

2nd. When $\omega + \omega^1$ lb. of water at t_1 was suddenly immersed in an atmosphere of steam maintained at t_3, part of it ω^{11} was evaporated, the remainder being water at t_3

$$(\omega + \omega^1)\ (t_1 - t_3) = \omega^{11}\ \lambda_3.$$

Hence
$$\omega^{11}\ \lambda_3 = \left(\frac{\omega^1\ \lambda_1}{t_1 - t_3} + \omega^1\right)(t_1 - t_3);$$

or,
$$\omega^{11} = \omega^1\left(\frac{\lambda_1 + t_1 - t_3}{\lambda_3}\right);$$

or,
$$\omega^{11} > \omega^1.$$

That was, more water must be evaporated at the lower temperature than was condensed at the higher temperature, and therefore, without priming, with a perfectly non-conducting cylinder, or if the water was all in the shape of spray, not touching the walls, the water present must diminish.[1] But although he thought Mr. Gray's calculation to be wrong, he did not con-

[1] Professor Perry would have more fear in putting forward this adverse calculation were it not that Mr. MacFarlane Gray had used his graphical method to prove another wrong proposition, namely, that the exhaust steam from Mr. Parsons' turbine could not be superheated. Now the only defect of the Parsons, or any other, steam-turbine lay in the loss of energy of the steam by eddying motion everywhere, in fact the exhaust steam was throttled steam, and it was exceedingly easy to prove that throttled steam must be superheated.

sider the presence of priming necessary for the continued presence of water in the cylinder. However well a cylinder might be lagged, it did radiate heat to the engine-room, and its radiation was quite sufficient to account either for accumulating water or the constant quantity of the Author, without priming. It seemed to him that it was in this connection that the usefulness of the steam-jacket became evident. With a steam-jacket, and no priming, it was obvious from his calculation that the cylinder-walls must be dry, and, in his opinion, it was of importance to gain this dryness, even at the expense of heat given to the steam by the jacket towards the end of the stroke, especially as, with no priming, this giving of heat would be through dry walls, and therefore small. He should like, for the sake of students, to refer to a few minor points. Professor Perry.

1st. He was interested in finding out the assumption on which Mr. MacFarlane Gray's formula for U was founded. He had difficulty in understanding why the more correct formula

$$U = \left\{\frac{1{,}438 + 0\cdot 7\ A}{A} + 1\right\}(A - B) - B \log \frac{A}{B}$$

should have been discarded, as it was just as easy to work with. After assuring himself that the formula gave approximately correct answers when applied to numerical examples, he had at length found what Mr. Gray's assumption was; and students would be interested to know that he virtually assumed that when 1 lb. of water at temperature B was raised in temperature to A, as water, it received all its heat A − B at the average temperature $\frac{A + B}{2}$. As to the use of U, he knew how convenient it was to compare a steam-engine with the most perfect heat-engine working between the same limits of temperature, and he congratulated the Author on the great success he had met with in working such a small engine. But it was not altogether wise to hide the fact that the Author's 80 per cent. efficiency really meant less than 10 per cent. efficiency. Surely there was no fact which ought more constantly to force itself on the attention of engineers, than that they were spendthrifts who were wasting their fortune in coal laid by for them during millions of years.

2nd. He was interested to know how the Author arrived at his rule for cut-off. The proper ratio of cut-off was given by dividing the initial pressure by 25. The Author called this an approximation to the "theoretically best" value. But Professor Perry understood that until more was known about condensation in

Professor Perry. the cylinder, no theory was available, and that the Author was helping to give materials to make a theory. Assuming that there was no loss by condensed water, and calculating the indicated work done by 1 cubic foot of steam, and finding r, that this might be a maximum, a very easy problem, the answer was, if r was the ratio of expansion, p_1 the initial, and p_3 the back-pressure, and if F was the pressure necessary, according to the Author, to overcome the friction of the engine, the law of expansion, $p^m v^{m+1}$ constant being taken—

$$r = \left(\frac{p_1}{p_3}\right)^{\frac{m}{m+1}}, \qquad \text{and not } \left(\frac{p_1}{p_3 + \mathrm{F}}\right)^{\frac{m}{m+1}}$$

because F could only come in if brake-energy or power was concerned, whereas the Author considered only indicated energy or power all through the work. Taking simply $p\,v$ constant as the expansion law, then, for the best value, assuming no condensation,

$$r = \frac{p_1}{p_3} \quad \text{or} \quad \frac{p_1}{14 \cdot 73},$$

whereas the Author took $r = \frac{p^1}{25}$! It was evidently a very valuable empirical formula, as was shown by his experiments, the results of which were given in Plate 3, Fig. 7. That figure showed it was indicated power which was made a maximum by this value of r, and it was evident that for maximum brake-power the cut-off should be later in the stroke than the rule affirmed. He could have wished further experiments had been made of the same kind, as without them the comparisons in the Paper must be considered as not between engines working at their best cut-off, but between engines working with the above law of cut-off. In whatever way the Author might have arrived at this important empirical rule, it was worth while to notice what it implied mathematically. He found that it implied that the loss of indicated energy per lb. of steam was greater in proportion to the increase in r. He had much inclination, but no permission, to dwell on this important matter, and compare this assumption with the results shown in the lower curve (Plate 3, Fig. 7), where it was evident that the missing steam at cut-off was greater in proportion to the increase in r.

3rd. He would only refer to one other of the numerous interesting points in the Paper. Why did the Author use the law $p\,v^{\frac{7}{6}}$ constant, as the adiabatic for steam? No adiabatic for steam followed exactly a law of the shape $p\,v^k$ constant; but when k was

Professor Perry.

1·130 this law was more nearly correct than when any other value of k was used. It was not sufficiently recognized that if the adiabatic was put in this shape, the best value of k depended on the initial and final pressures, and also on the excess quantity of water present. The value which he had just given was based on the assumption that the range of pressure was, as usual in steam-engines, 100 lbs. to 8 lbs. per square inch, and that all the water present was in the shape of steam at the beginning of the expansion.[1] Now, according to his calculations with this range of pressures, the Author's value of k, namely $\frac{7}{6}$, presumed that there was just twice as much water present in the cylinder at the beginning of the expansion as there was steam. Or, to put the matter in another form, if the law of expansion was $k = \frac{7}{6}$, and there was no water present at the beginning of the expansion, it would be found that the water lost heat-energy during the expan-

[1] Professor Ayrton and he had for several years used the following easy method of calculation. First, they found that as the expansion part of the indicator diagram always followed approximately the law $p v^k$ constant, it was always worth while to obtain the value of k if the diagram was to be studied. To do this it was absurd to use only two points on the curve. They measured p and v (to any scales whatsoever) at many points, and they plotted log p and log v as the co-ordinates of points on squared paper, using the straight line which lay most evenly among these points to obtain their value of k.

Second, it was to be remarked that the weight of water present at every point of the expansion was proportional to

$$\frac{1 - \frac{16}{17} k}{v};$$

so that if $k < \frac{17}{16}$ evaporation was going on,

$k > \frac{17}{16}$ condensation was going on.

Third, the intrinsic energy of 1 lb. of water of which the steam formed v cubic feet was

$$E = \text{constant} + e f^m + g f^n v,$$

f being the pressure in lbs. per square foot, $e = 53{,}115$, $g = 29{\cdot}67$, $m = 0{\cdot}182$, $n = 0{\cdot}8857$, and if h was the rate at which the water received heat during expansion, in the same units as f

$$\frac{dE}{d\,v} + f = h.$$

The above expression for E enabled h to be readily calculated for any value of k. Without giving details he might say that they had found the following calculated table to be very useful. It gave values of $\frac{h}{f}$ which they called s. Given any diagram, first find k, then multiply each pressure by the corresponding number in the Table, to obtain the diagram showing the rate at which the stuff received heat from the cylinder during expansion. It was assumed that the

Professor Perry. sion, at a rate which was very nearly 0·36 time the absolute pressure everywhere. These two results were not much affected by taking other ranges of pressure than that which he had chosen.

Mr. Salter. Mr. F. SALTER remarked that in the determination of the dryness of the steam, the agreement between the figures arrived at seemed not so close as might have been hoped from the exceptional scale on which the experiments had been conducted, and the delicacy of the instruments used. Perhaps the reason of this was that by the method adopted, the calorimetric one, the quantity determined was the amount of steam in the mixture of steam and water that left the boiler. By the chemical process, the smaller quantity, namely, the water primed over, was measured; hence, with equal care and delicacy of apparatus, the error should be proportionately smaller. The chemical system would, perhaps, be more used in this country if its simplicity and accuracy were better known. One easy method was the following:—A little common salt was put into the boiler; enough to render the water brackish to the taste was usually sufficient. Any priming which took place carried with it a proportionate amount of salt. Such primed water,

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whole quantity of water present was in the shape of steam either at the beginning or at the end of the expansion.

VALUES of s.

Pressure lbs. per Square Inch.	$k = 0·9$.	$k = 1·0$.	$k = 1·111$.	$k = 1·2$.	$k = 1·3$.
8·4	3·09	1·86	0·41	−0·85	−2·29
14·7	2·82	1·69	0·36	−0·81	−2·14
39·3	2·32	1·40	0·27	−0·74	−1·88
69·2	2·01	1·23	0·21	−0·71	−1·74
101·9	1·79	1·10	0·18	−0·68	−1·65

Other numbers might readily be found by interpolation.

These values of s satisfied, with just such a rough approximation to accuracy as need not make it worthless, the law—

$$h = 8\,(1·130 - k)\,(p + 6),$$

h being in the same units as p the pressure, that was in lbs. per square inch, so that the heat diagram was at once obtainable from the indicator diagram. The use which he had made of the above expression for E in finding exactly what the Author assumed in taking $k = \frac{7}{6}$ as the adiabatic was readily understood.

Mr. Salter.

together with any resulting from condensation after the steam left the boiler, was drawn off from a separator, or any other place at which the steam came to comparative rest. The quantity per hour of this separator-water was measured as well, of course, as the quantity of water evaporated and primed by the boiler. Its saltness, as compared with that of the boiler-water, indicated how much of it was primed, and the proportion of that amount to the whole quantity of steam and water leaving the boiler could then be determined. The saltness of the water from the separator and of that in the boiler was found by taking a sample of each, and treating it in the following manner: A measured amount, say 100 cubic centimetres, was coloured to a canary yellow by the addition of a drop or two of a solution of bichromate of potash. Into this was dropped, with continual stirring, a standard solution of nitrate of silver from a graduated burette. A white precipitate of chloride of silver was formed as long as there was salt left in solution; but the moment this ceased to be the case, a pink colour, due to the formation of chromate of silver, indicated the fact, and the original strength of the salt solution was shown by the amount of silver solution used. The delicacy of the test lay in the change of colour which occurred immediately the silver was in excess of that required to precipitate the chloride. An objection sometimes made to this method was that it assumed that all primed water was caught by the separator, and that none of it was carried as fog along with the steam. This, however, could easily be verified by condensing a sample of the steam after it left the separator, and testing it with silver solution. But in any case the objection did not apply where the whole of the steam was condensed and collected, as in the calorimetric method of determining the dryness of steam; and the chemical method would therefore form a doubly valuable check upon the other, if used in conjunction with it, because it gave the ratio of water primed to absolutely gaseous steam, instead of the ratio of the steam under trial to "Regnault Steam," on the dryness of which the Author seemed to cast some doubt.

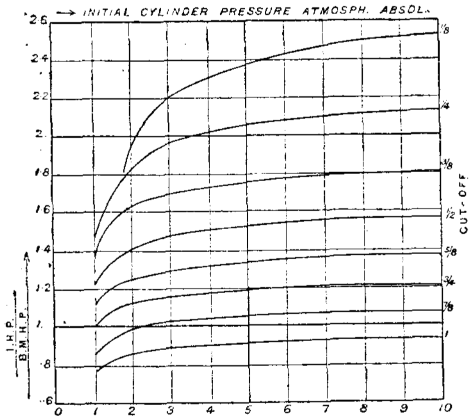
Professor Smith.

Professor R. H. SMITH observed that he had been accustomed to take the English heat-units per hour equivalent to 1 HP as 2,565, corresponding with 772, the usually adopted mechanical equivalent. The lowest curve on Plate 3, Fig. 1, was found from the equation on p. 131, which involved B, the exhaust temperature; but it was not stated what B had been taken in working out the results. He could not reconcile this formula, given on the authority of Mr. J. MacFarlane Gray, with the ordinarily accepted equation for the same thing, except for small ranges of temperature; but the

Professor Smith.

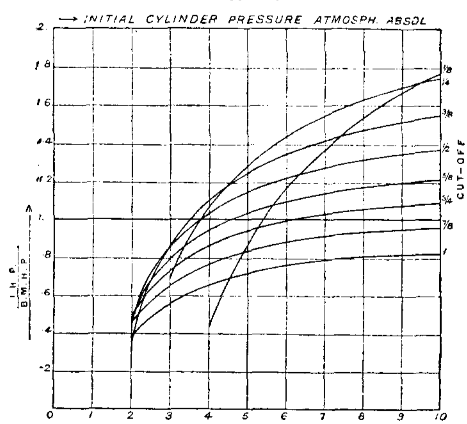
values of $\frac{A - B}{A}$ occurring in steam-engines ran up to $\frac{1}{4}$. In Plate 3, Fig. 2, was given a series of curves for different pressures,

FIG. 20.



the highest point of each of which curves showed the thermo-dynamically best ratio of expansion. This diagram was for non-condensing engines, and the effects of both back-pressure in excess

FIG. 21.

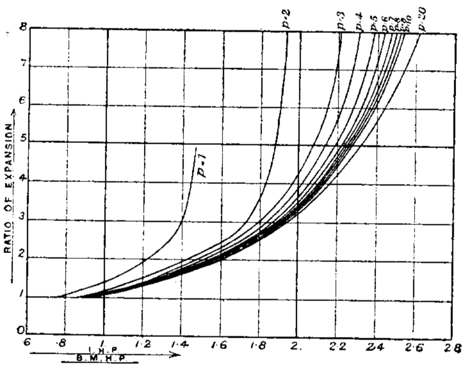


of atmospheric pressure, and of clearance space, were left out of the calculation. Both of these very considerably affected the true results, the losses from each increasing with the ratio of expansion; so that the best ratios calculated with regard to these losses were

Professor Smith.

less than those given by the Author in Plate 3, Fig. 2. He had plotted out four diagrams, Figs. 20, 21, 22, and 23. Figs. 21 and 23 were for non-condensing engines with $1\frac{1}{5}$ atmosphere as back-

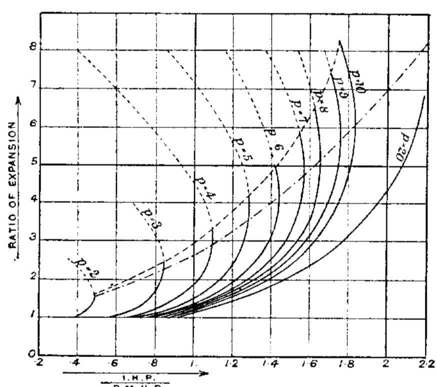
FIG. 22.



CONDENSING ENGINES, FOR DIFFERENT INITIAL PRESSURES, 1 ATMOSPHERE TO 20 ATMOSPHERES ABSOLUTE.

pressure; Figs. 20 and 22 for condensing with $\frac{1}{5}$ atmosphere as back-pressure. The clearance space was in each case taken as $\frac{1}{20}$ the volume swept through by the piston in one stroke, and the

FIG. 23.



work corresponding to this volume was taken as wholly lost. The curves gave the values of $\frac{\text{I. HP.}}{\text{B.M.HP.}}$, or the ratio of "engine indicated power" to "boiler mechanical HP.," the latter corresponding to the actual mechanical work done by the expansion of the water into steam in the boiler. The ratio was the same as that of the

Professor Smith. area of the indicator diagram per lb. of steam to $p\ v$, where p and v were the pressure and volume of 1 lb. of steam. The dotted curve passed through the summits of the curves in Fig. 23, and gave the most economical ratios of expansion for non-condensing engines. The curves in the Paper had been found from the equation $p^6 v^7 =$ constant, while those in his diagrams were founded on hyperbolic expansion $p\,v =$ constant. The corresponding curves, Fig. 22, for condensing engines, did not reach their highest points within the limits of the diagram, thereby showing that the thermo-dynamically best cut-off was earlier than could be adopted for other practical reasons (among them the condensing action of the cylinder walls). What might be termed the "commercial" best ratio of expansion was still lower; in calculating this many other items, such as interest on first cost, cost of lubrication, and generally of engine attendance, having to be taken into account. This question had recently attracted a good deal of attention in America. Table X showed the results of tests for priming. He had used the method explained very frequently, but never got reliable results so long as only a small quantity (20 lbs.) of cold water was used. With a large quantity (about $\frac{1}{2}$ ton) he had obtained results which he considered trustworthy. But he had always used a vessel of as non-conducting a material as possible, and assumed that the heat going into the sides of the vessel was negligibly small. To make this assumption as true as possible, he blew in steam for a very short time, and raised the temperature of the water through a very small range, this range being measured of course accurately (to $\frac{1}{20}$° Centigrade). After ceasing to blow in steam the thermometer slowly rose for a short time, while the additional heat was being uniformly diffused; it then slowly fell, the fall being due, of course, to the cooling partly from the surface, and partly from conduction to the solid sides of the vessel. At its highest point, however, it moved very slowly, remaining appreciably constant for several minutes, and the subsequent fall was very slow as compared with the previous rise. This showed that the cooling was extremely slow; and, therefore, the highest reading of the thermometer could be taken without appreciable error as the correct one. In the calculations of Table X, on the contrary, it was assumed that the whole iron of the tank rose in temperature along with the contained water. The weight of water taken as equivalent to the iron was 779 lbs., this weight and that of the actual water together being 4,416 lbs. in No. 1 experiment. The calculation gave 99·8 per cent. of steam. But if it had been assumed that the iron took none of the heat the percentage would have been only about 77. Of course the iron did absorb some of the heat; but it seemed legitimate to question in what ratio its rise of temperature stood to that of the water. If this was to be estimated, the above range from practically 100 down to 77 per cent. showed what a very large source of doubt and possible error existed in this question. If the iron were to share the full rise of the water temperature, evidently the iron in its turn would transmit some of its access of heat to the surrounding air by conduction and radiation; so that in any case the assumption proceeded upon could not be taken as correct. A certain element in the calculation depended on estimate or guess-work, and unfortunately this element affected the result very largely. These were the considerations which led him to prefer using wooden tubs and reducing the rise of temperature and the duration of the experiment to a minimum. No doubt the neglect of the heat going into the wood occasioned a small error; but it was probably a great deal smaller than the opposite error involved in using a conducting material for the sides, and assuming it to be heated along with the water to the full extent. There was another objection to this same calculation of priming. It assumed that the heat given out by the steam was its latent heat at the boiler-pressure plus the specific heat multiplied by the fall of temperature. Now, the actual heat it lost by conduction was this quantity less the area of the whole indicator diagram, obtained by its evaporation and expansion down to atmospheric temperature. Similarly, the heat given up by conduction by the water, mixed with the steam, was less than that assumed in the formula by the work this water did in passing from boiler to tank (during this passage it burst into steam and afterwards condensed). These amounts of work bore by no means inconsiderable ratios to the whole heat dealt with. This work was no doubt spent partly in reproducing heat in the tank, the kinetic energy of the eddies being rubbed down into this form. But all of it was certainly not reproduced thus, and the proportion recovered as heat in the water was incapable of being even guessed with any approach to accuracy. He doubted if any boiler, unprovided with a special superheater, produced really dry steam. For these reasons he still felt at liberty to doubt that the large initial quantity of water, found in these and other experiments, was wholly due to cooling influences that operated after the steam had entered the cylinder. It was generally assumed that throttling the steam, and the sudden in-rush that took place on the opening of the slide-valve, had a tendency to superheat rather than to partially liquefy the steam. Professor Smith.

Professor Smith.

Professor Rankine supported this theory, but it seemed very doubtful. The immediate expansion of the steam past the throttle-valve and slide-valve could not be otherwise than nearly adiabatic, so that partial liquefaction was likely to be the first result. Rankine argued that the kinetic energy simultaneously generated was subsequently changed into heat, and more than counterbalanced the primitive liquefaction. But the transformation occurred, partly at any rate and probably chiefly, by rubbing and impact against the metal walls of the passages and cylinder; and it was a question whether more than half of the heat thus generated did not go into the metal instead of the steam. Certainly some of it was absorbed by the metal. It would be interesting to calculate what thickness of iron it would be necessary to suppose heated from exhaust-temperature to steam-temperature to account for the missing quantity. He had done this for Table II of the Paper, and gave the results below. He had taken the specific heat of iron as 0·122, this being its value at the mean temperature involved, and he had neglected the small heating of the metal during the period of compression. Of course, if this were the only cause of condensation a greater thickness would be affected, the temperature sloping downwards inwards from the surface, the total thickness appreciably affected being more than double that calculated. The results were :—

TABLE II.

Trial Letter . . .	C $\frac{80}{3\cdot2}$	C $\frac{90}{3\cdot2}$	C $\frac{90}{3\cdot6}$	C $\frac{100}{3\cdot6}$	C $\frac{100}{4}$	C $\frac{110}{4}$	C $\frac{110}{4\cdot4}$	C $\frac{120}{4\cdot4}$
Thickness of metal heated } inch	0·0026	0·0027	0·00307	0·00255	0·0036	0·0035	0·0036	0·00346

Trial Letter . . .	C $\frac{120}{4\cdot8}$	C $\frac{130}{4\cdot8}$	C $\frac{130}{5\cdot2}$	C $\frac{140}{5\cdot2}$	C $\frac{140}{5\cdot6}$	C $\frac{150}{5\cdot6}$	C $\frac{160}{6}$
Thickness of metal heated } inch	0·0038	0·0037	0·0040	0·0042	0·0041	0·0043	0·0047

These were calculated from the full surface exposed to the steam at cut-off. But the latter part of this surface was exposed for a short time only before the instant at which the missing quantity was taken. He had calculated an average surface exposed for the full time up to cut-off, supposing the heat conducted to each element of the surface to be proportional to the time it was exposed. It had been said that the depth to which the heat penetrated varied

Professor Smith.

as the square root of the time of exposure; but this seemed a misapplication of one of the Fourier results. It probably varied as a lower power of the time; but as he was unable to solve the problem accurately, he here put forward provisionally the results of the above supposition. The crank was taken as rotating uniformly, the obliquity of the connecting-rod was neglected, as was also the time of pre-admission.

Let S = stroke × cylinder circumference.

$c\,S$ = clearance, or initial surface, including passage, cylinder-cover and piston.

k = cut-off ratio, so that $k\,S$ was additional surface exposed during admission.

$(c + k)\,S$ = whole exposed surface at point of cut-off.

A = angle of crank rotation up to cut-off $= \cos^{-1}(1 - 2\,k)$.

N = number of revolutions per minute.

$\frac{A}{2\,\pi\,N}$ = time from dead-point to point of cut-off.

Then the average surface supposed to be exposed during the full time $\frac{A}{2\,\pi\,N}$ was

$$\left\{c + \tfrac{1}{2}\left(1 - \frac{\sin A}{A}\right)\right\} S.$$

The following were the values of the latter part of this function:—

TABLE of proportions of the CYLINDER-SURFACE UNCOVERED during ADMISSION to be taken as EXPOSED during full TIME of ADMISSION.

Cut-off k .	0·05	0·1	0·15	0·2	0·25	0·3	0·35	0·4	0·45	0·5
$\frac{1}{2}\left(1 - \frac{\sin A}{A}\right)$	0·017	0·034	0·051	0·0685	0·0865	0·1045	0·123	0·142	0·8615	0·182
$\frac{1}{2\,k}\left(1 - \frac{\sin A}{A}\right)$	0·34·	0·34	0·34	0·342	0·346	0·346	0·35	0·355	0·36	0·364
Cut-off k .	0·55	0·6	0·65	0·7	0·75	0·8	0·85	0·9	0·95	1·0
$\frac{1}{2}\left(1 - \frac{\sin A}{A}\right)$	0·202	0·223	0·246	0·269	0·293	0·3194	0·348	0·380	0·419	0·5
$\frac{1}{2\,k}\left(1 - \frac{\sin A}{A}\right)$	0·367	0·37	0·378	0·384	0·39	0·399	0·41	0·422	0·44	0·5

Professor Smith.

Only from one-third to one-half of the exposed cylinder-surface should, therefore, be added to the initial surface in the above calculation of thickness of metal heated. In accordance with this mode of reckoning, the previously stated thicknesses should be corrected to the following:—

Trial Letter . .	$C\frac{80}{3\cdot2}$	$C\frac{100}{4}$	$C\frac{120}{4\cdot8}$	$C\frac{140}{5\cdot6}$	$C\frac{160}{6}$
Thickness heated . inch	0·0035	0·0045	0·0046	0·0049	0·0055

These approximated more nearly than did those in the previous Table to the depths of heating in those initial parts of the surface which were exposed to the steam throughout the full time $\frac{A}{2\pi N}$.

Mr. Thurston.

Mr. ROBERT H. THURSTON, Director of Sibley College, U.S., believed that he was the first to institute a scientifically systematic investigation of the method of variation of losses by cylinder-condensation and internal wastes with variation of speed, of range of temperature, and with change of ratio of expansion. But the results of the most successful attempt made under his direction, by Messrs. Gately and Kletzsch, although, for the time, of interest and value, were by no means so complete or fruitful of new information, in many respects, as were those of the Author of this Paper. The earlier work of Clark, of Hirn, and of Isherwood and Emery, opened the subject very effectively, and their labours, and especially those of Professor Cotterill, later, had thrown much light on this branch of study of the theory, pure and applied, thermo-dynamic and physical, of the steam-engine. But much remained to be done before it could be determined to what extent these phenomena modified the older theories and the older practice, and how they affected the commercial efficiency of heat motors; which subject was, after all, that with which engineers were most concerned. The aim of the Author to fix, for the class of engine selected, the best ratio of expansion, to determine the efficiency of the machine, and to measure the thermo-dynamic and total efficiency, had been very successfully attained. It was singular that the consideration of the wastes occurring within the cylinder, their method of operation, and their influence upon the physical and commercial theory of the machine, although known from the time of James Watt, should have been recognized so rarely and so slightly by the constructors of the thermo-dynamic theory of the engine. Even now, a discussion had hardly ceased in

Mr. Thurston. which a distinguished authority on the thermo-dynamics of the steam-engine denied the propositions of a no less distinguished practitioner, whose experimental investigations had plainly exhibited the correctness of his position. Fortunately, however, it was to-day generally recognized that to the thermo-dynamic theory was to be added a physical theory of wastes, in order to complete the system of philosophical principles according well with the facts of experience. It was such investigations as these, permitting as they did a quantitative measure of deductions, that were now needed. It was satisfactory that such care had been taken to secure exact results of observation. The correction of the indicator-spring, hot, had, in his own experience, at least, proved to be one of the essentials to correct determinations of power. The exactness attained by the use of the planimeter was very gratifying. In his own laboratory work of this character he had come to the almost exclusive use of this system of measurement, and found it most satisfactory. As regarded the quality of the steam, experience, extending now over at least twenty years in the use of the calorimeter in such investigations, led him to assert that there was no excuse for the presence of wet steam in the steam-chest of any engine if proper forms of boiler were used, and they were not over-driven. He should consider 5 per cent. by weight a high proportion of water to be entrained by the steam, and should think 1 per cent. quite as much as should be admitted in good practice. Since this was very important, as bearing on the economy of the engine and on the amount of cylinder condensation, the dryness of the steam in this case was a matter of special congratulation. He was surprised to notice so small a proportion of condensation at cut-off in both the simple and the compounded engines. He had been accustomed to find 25 per cent. a small figure for the first and 10 per cent. for the second class. He presumed that the high speed and the dryness of the entering steam accounted for it; otherwise, he should suspect some error of measurement. The engine must be of an excellent design and of remarkably good construction. The fact reported, that the loss of pressure in the ports was proportional to the density of the steam, confirmed Clark's investigations of thirty years ago. The handling of the calorimeter in these experiments presented a novel method; although in the main and in principle similar to Hirn's. He found the weighing of the introduced steam, in the presence of the enormously greater weight of the condensing water in the calorimeter, one of the most delicate details of the work, and one leading, commonly, to liability to serious error. He should imagine that, if there were any sensible errors in the work

Mr. Thurston. reported, they were likely to be at this point. The impression received from reading the Paper, however, was wholly favourable to its accuracy. The apparently small effect of variation of area of internal surface exposed to steam seemed to him remarkable. In slower engines he had found this quantity to have great importance. The same remarks were, in less degree, true, in such comparisons, with regard to the effect of density of steam. The influence attributed to variations in amount of water present was most certainly in accordance with what was already known on this subject. The experiments upon the effect of air in preserving adiabatic changes reminded him of the work of Mr. Warsop on mixed air and steam in the locomotive, and indicated the possible value of this method, in some cases, in the reduction of cylinder wastes. He presumed that the gain observed, in some of these trials, due to throttling, resulted from the drying of the steam entering the engine. The deduction that the compound engine was less dependent upon the automatic system than the single cylinder supported the conclusions of Mr. See, the President of the American Society of Mechanical Engineers, who built two sets of similar marine compound engines, one of which was fitted with automatic gear, and the other with the old gear. There was practically no difference in the economy of their operation. The set of curves exhibiting graphically the results of these experiments were even more interesting to him than the Paper. He noted that the efficiency of the simple engine so varied that the maximum fell outside the range of work done, and somewhere about 200 lbs. pressure; that the curve for the compound engine fell some 20 per cent. lower, and the maximum efficiency was not far from 160 lbs.; and that the triple-expansion engine gave its maximum, the curve being mainly still lower at, probably, pressures considerably exceeding 200 lbs., perhaps as high as 250. He noted also the curious, though not at all doubtful, fact that the latter was less efficient than the compound, under the circumstances of these trials at least, at 150 lbs. He observed that the efficiency increased constantly, and evidently beyond the speed of engine limiting these trials. The ratio of expansion, for this engine, giving maximum duty was about 5·5; which led him to conclude that, in ordinary operation, the most work would be had for the money expended in its production at somewhere about $3\frac{1}{2}$ or 4 at most. The economy attained was excellent for so small an engine, and working without condensation. The efficiency of machine, as reduced by friction, was, he thought, remarkably good, when the small size and power of the engine were considered. He should imagine that a large and powerful engine would give reduced figures for fuel and steam used, would bring up the efficiency of the machine to nearer 90 per cent., would find the point of maximum efficiency as a function of steam-pressure at considerably lower points, and would have a ratio of expansion at maximum efficiency at least 10 per cent. higher for the non-condensing, and at double these figures for the condensing engine. Mr. Thurston.

*** The reply of the Author to the Correspondence will be given in a subsequent volume of the Minutes of Proceedings, when he proposes to enter fully into the question of the best method of calculating the thermo-dynamic efficiency. Mr. Willans also wishes to devote further consideration to the questions raised by Mr. Isherwood.

SEC. INST. C.E.

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20 March, 1888.

GEORGE BARCLAY BRUCE, President, in the Chair.

The discussion on the Paper by Mr. P. W. Willans, on "Non-Condensing Steam-Engine Trials," occupied the whole evening.

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27 March, 1888.

GEORGE BARCLAY BRUCE, President, in the Chair.

After the transaction of the routine business the President rose, an act that was followed at once by the Council, members, and visitors present, all of whom remained standing during his delivery of the following remarks :—

"It is my mournful duty to notice from this Chair the lamented death of our highly esteemed Past-President, Mr. Thomas Elliot Harrison. For fifty-four years the name of Mr. Harrison has appeared as a Member on the books of this Institution. To it he, with no stinted hand, gave of the early years of his professional life, of the strength of his full manhood, and of the mature wisdom of a ripe old age.

"The Institution of Civil Engineers knew Mr. Harrison well, and valued him alike for his qualities as an Engineer, which made him so safe an adviser and guide in the conduct of public works, and for that transparent uprightness and rectitude of thought