Discussion.

Sir John Coode. Sir JOHN COODE, President, said the Author had presented the description of, and the results obtained by, these remarkable engines in every possible point of view, and whether in regard to the conditions laid down by the Committee of consultation, or to the admirable manner in which those conditions had been carried out by Messrs. Mather and Platt, the results were highly creditable to all concerned. The Author stated that the makers had spared neither trouble nor expense in carrying out the work, and those members who had been fortunate enough to see the engine, as he had, would certainly agree with him in that statement. With regard to the expense, the sum paid to the makers represented only one-fourth of the actual cost, the other three-fourths being regarded as a present to the University College. That fact redounded so very much to the credit of Messrs. Mather and Platt that it ought to be widely known.

Professor Reynolds. Professor OSBORNE REYNOLDS said he only wished to direct attention to one fact which he thought had not been sufficiently put forward. In discussing the Paper, it should be borne in mind that the object in making the engine in that form was to render every part of it accessible to observation. That was the main object; it was not to get an economical engine, though it was fairly economical; the object was really to measure everything that could be measured. Every one who saw them must acknowledge that they were very handsome, but still they were a strange looking set of engines. They answered the purpose, however, and the design was simply for the sake of being able to get at all the parts of the engine in order to be able to measure everything about them. There were many complications, particularly in the jackets and in the shafts. The low mechanical efficiency of the engine, which was fairly low, as shown in Table I, was partly due to the great expansion in these totals, and also to the fact that for the sake of getting a good figure in the diagrams the passages had been made abnormally large, the area of the ports being 13 per cent. of the area of the piston. There was also a length of 35 feet of crank-shafting, which diminished the mechanical efficiency of the engine to some extent.

Mr. Cowper. Mr. E. A. COWPER felt that the Author should be thanked for trying to arrange the excellent engine that he had described, so as to get an accurate result of the performance of the steam;

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the object being not to make the most economical engine, but to endeavour by experiments to find how the steam operated in every possible way. It would have been highly interesting to have seen the original indicator diagrams drawn by the engines themselves, as also the friction diagrams. Had he been trying the experiments himself, he should have preferred to make the engines run at one speed, and then the indicator diagrams would have been comparable without any alterations. The diagrams shown, he understood, were average diagrams from a large number of experiments, but altered in proportion to the speeds. Of course, in making such experiments the engine must be in good order; he did not say perfect order, because that was not to be attained, but it must be really good. Unfortunately, up to experiment No. 55, it seemed that the expansion-valve was off the face, viz., that the slide-rod sprung it off the face, and it was not until the steam-pressure was sufficiently low in the cylinder that the valve came up to the face. It was stated in the Paper that there were leaks of steam into the receivers; he should therefore have preferred putting aside those fifty-five experiments and to have gone on to the fifty-sixth, and to have taken a number of others afterwards when the engine was in good order. There seemed to have been an escape varying from 5 to 10 per cent. of feed-water, which should have been stopped before calculating the exact quantity of water that went into the engine. The reverse had been done by the Author, who had taken into account the water coming from the engine, and the calculation of fuel, he understood, was upon that. The fuel for all the steam evaporated from the boiler had to be paid for, and, therefore, engineers were in the habit of estimating the feed-water; but as neither the weight of fuel burnt, nor the quantity of feed-water used, was given in this case, it was impossible to follow out the calculation. He must take exception to the statement that the steam was put into the cylinder as dry as possible, because part of the steam supplied to the engine went first through the jackets. Now the jackets acted to a certain extent in condensing steam, and thereby did very useful duty, keeping the cylinders warm. The steam coming into the engine passed from the boiler dry, but what passed through the jackets became damped. The Author stated that the jackets were made so that the water should drain back to the boilers. The engines were arranged in such manner that the water-line of the boiler was considerably below the water in the separator, the object being that any water that went into the separator should be allowed to return to the boilers by gravity, Mr. Cowper.

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Mr. Cowper. and there was a head of 6 feet to effect this. But, as Mr. Cowper had pointed out on former occasions, if the jackets were connected with the boilers to take steam from the boiler, and the bottom of the jackets also connected to the boiler again and the water was allowed to run back, all the air that came over with the steam was lodged in the jackets. In this case the damp steam from the jackets would probably bring most of the air out with it; but the fact that "the jackets were liable to fall off in efficiency" rather pointed to their not having pure high-pressure steam in them. This was not a fanciful objection, but was one practically of great importance, and he always made a point of letting out the air occasionally, or else continuously, from the jackets, so that no air should be lodged in them. Air, of course, would render them inoperative to a certain extent. He approved of the steam-jacketed receivers between each pair of cylinders, having found out the advantage of that arrangement in 1856, when he first introduced them. Cutting off the steam before the half-stroke in each cylinder prevented any second gush of steam into the cylinder whilst the piston was making its stroke, and before the cut-off, and was also very good. The engine, however, not being coupled, interfered somewhat with this, inasmuch as with that arrangement the gush of steam might come from the engine at different times, or just before the moment of cutting off steam. The indicator diagrams of course must vary a little, because of the varying time of one cylinder taking steam from the other; not to a great extent in this case since the steam-jacketed receivers were so very large, but it should be avoided, because the steam should not enter without doing its full duty. He could not but think that there must be some considerable friction in the engine, or else the duty would be greater with 185 lbs. steam and triple expansion. Higher duty had been attained with 55 lbs. steam in a compound engine with only two cylinders, but of large size. He thought, on the whole, the attempt to obtain exact experiments as to the action of steam was praiseworthy, and he should be glad to hear, that in future experiments with those steam-engines, some of the little points had been set right, so that more precise results might be obtained. He did not like to have a suspicion of a valve being on or off the face for part of the stroke.

Professor Unwin. Professor W. C. Unwin agreed with the Author that in building a college engine, a totally different object to that of building a commercial engine had to be kept in view. The most foolish criticism that could be made on a college engine was, that it was not a good type of commercial engine. Not many of the former

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engines had been built: but the Author had two advantages; he had come last in building an engine, and he had larger means than others, either directly, or through the liberality of the makers of the engine; and, as he had bestowed much attention and care in the design of the engine, it was not surprising that he had built an extremely good one. It was perhaps fair to recall the fact that the very first college engine built was one arranged by Professor Dwelshauvers Dery, of Liége. That first engine was in many respects the best experimental engine hitherto constructed. It was a simple engine with very remarkable valve-gear especially designed for experimental purposes. There were four valves with port areas one-twelfth the area of the piston. Then in Professor Dwelshauvers Dery's engine the valve-gear was so arranged that not only could the cut-off be varied through the whole length of the stroke, but the lead could be varied to a very considerable amount, and also the compression through the whole range of the stroke. The valve-gear of Professor Dwelshauvers Dery's engine was quite a typical valve-gear for an experimental engine. That engine had separate jackets both to the body of the cylinder and the covers. It had one other remarkable feature. The Author had tried to get the clearance spaces of his engine small, and very rightly so; but apparently not with much success. In his own engine at the Central Institution the clearance in the low-pressure cylinder was less and in the high-pressure cylinder slightly more without any special proportions. Before this engine was built, he designed an engine with Corliss valves, which would have given a much smaller clearance, and it was only want of funds which prevented his adopting Corliss valves; but in Professor Dwelshauvers Dery's engine the minimum clearance was only one-thirtieth of the cylinder volume. He had, three or four years ago, arranged the design of the engine built for the Central Institution.[1] It was a compound engine, differing in some respects from

[1] The experimental engine at the Central Institution had two cylinders $8\frac{1}{2}$ and 13 inches in diameter and 22 inches stroke, normal speed 100 revolutions per minute; the cylinders were jacketed on the body and back covers. Both cylinders had slide-valves and expansion-valves, and both engines had governors. The engine could be used simple or compound, with or without a condenser, and all the passages not in use at any given time could be blank flanged, so that there should be no leakage. The low-pressure expansion-valve was a Meyer valve. The high-pressure, a Hartnell valve, worked from the governor. But the governor could be put out of gear and the engine worked without it. Practically with a fixed load the diagrams were so similar, even when the engine was under control of the governor, that no object was gained

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the Author's engine, and it would be perhaps interesting for a moment to look at the differences. The Owens College engine was a vertical one, but the Central Institution engine was horizontal. A good deal had been made of the point of accessibility, it being said that a vertical engine was the most accessible. He could not understand how an engine in which there were three staircases was more accessible than an engine every part of which could be reached from the floor. The only thing necessary to make a horizontal engine accessible was to separate the cylinders, so that there should be a passage between them. He did not think there was much in that, but still the advantage of accessibility seemed to be on the side of the horizontal engine. In the next place, the most marked difference between the Author's engine and his own was the adoption of the extremely nice expedient of separating the three engines and letting them all go as they pleased. Mr. Thornycroft, when examining his engine three or four years ago, said: "Why did not you make your engines work separately?" He wished that that suggestion had been made twelve months earlier. The Author had the advantage of adopting that suggestion. If he were building a new engine, he should probably adopt the same expedient, but still too much might be made of it. First, it placed the action of the engine on a different plane to that of any engine which could be used for ordinary purposes.[1] It was not a very great objection, but still it had to be recognized. He did not see that it afforded much greater advantage than could be acquired by having a cut-off valve to both cylinders; or that much could be gained by building the engines separately, over what could be obtained perfectly well by varying the cut-off in the

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by putting it out of gear. When working compound, the engine was ordinarily used as a receiver-engine, but the crank angles could be varied. There was a surface-condenser of the Perkins type, and circulating water and condensed steam were separately measured. The clearance could be altered on each cylinder. The jackets drained to a separate measuring tank, and there was no steam-trap. There was a band friction-brake on a wheel, cooled inside by water on Mr. Halpin's plan. The engine was built for a steam-pressure of 150 lbs. per square inch, but had only been used at present with 70 lbs. pressure. The engine was designed in detail and very carefully constructed by Messrs. Marshall and Sons, of Gainsborough. The longest stroke and most moderate number of revolutions had been deliberately adopted, as best suited for students' purposes.

[1] The cylinder condensation being proportional (other things being equal) to the duration of a stroke, the action of the cylinder sides would be comparable with that in ordinary engines, only when all the cylinders had the same number of strokes per minute.

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low-pressure cylinders. The next important difference was the adoption by the Author of the fluid brake. Professor Unwin had considered the fluid brake before he made his engine, but he did not see his way at the moment to make a fluid brake which would run with a constant load. The Author had accomplished that end, and no doubt the brake he had constructed was an extremely good brake to use on an engine. At the same time, he must a little protest against the comparison of fluid brakes with other brakes. The Author said he had had a good deal of experience with other brakes, and he proceeded to give a comparative account, which almost made it appear as if he had never seen a Prony brake reasonably well constructed. In the first place, he said that the Prony brakes required "constant observation and watching." Professor Unwin had a brake on a wheel 10 feet in diameter, which worked all through the day without attention or watching. It was said that a single engine could not be started without relieving the brake of its load; but this engine started with half the load, and he thought it would start with a full load. The Author said that such brakes were cumbersome, and were not easily adapted to measure greatly different powers. He did not know what cumbersomeness of the brake meant. It was like a fly-wheel on a shaft; it did not cumber him that he knew of, nor the engine. No doubt the fluid brake had a greater range of power than the solid brake; that was, it might be worked to a smaller fraction of the maximum load. That was an advantage, but not an enormous one. He could get his own brake down to a small fraction of the maximum load, and by having a couple of brake straps could reduce the load as he pleased. It was a little more troublesome, in the few cases where a small power had to be measured, but he did not recognize that any better result could be obtained from the fluid brake or a more accurate one than could be had, with a little trouble, from the solid brake. The Author had shown an indicator gear as a new gear. Professor Unwin remembered having the pleasure, about two or three years ago, of showing to the Author on the Guilds engine a gear which was identical with the one represented, except that there was a slight difference of construction, which gave it a little advantage in neatness. Instead of the long spring adopted by the Author, keeping the cord from the engine tight, he had a spiral spring inside a small 3-inch pulley. A second pulley alongside took the cord from the indicator. By a touch, one pulley could be put in or out of gear with the other pulley; and, on the whole, that made an arrangement more easily used by the student, and in which he was least

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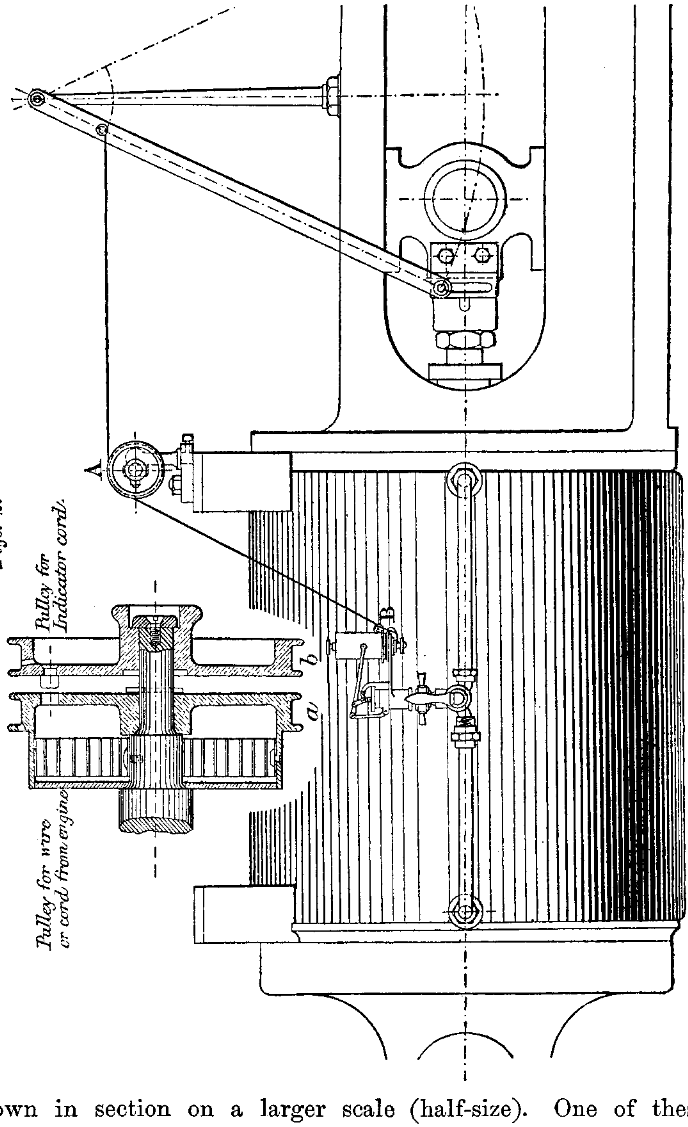
Professor Unwin. likely to put any straining action on the indicator. *Figs.* 2 showed the indicator gear mentioned. At A were two pulleys,



Figs. 2.

shown in section on a larger scale (half-size). One of these pulleys, *a*, was permanently attached to the rocking-lever on the engine by a wire or cord kept tight by a spiral clock-spring

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inside the pulley. The other pulley, *b*, was attached by a cord to the indicator B. The pulleys were on a universal joint, so that they were easily placed in the plane of the two free parts of the cords. A steel pin on pulley *b* fitted a hole in pulley *a*. Hence pulley *b* could be put into gear with pulley *a*, or pulled out of gear in a moment. When out of gear the indicator barrel was at rest. The arrangement shown was for one indicator. The engine was also fitted with cocks for using two indicators simultaneously, one indicator at each end of the cylinder. But with a sufficiently large pipe no difference was found in the diagrams whether one or two indicators were used. The results had, during the past four years, been systematically reduced in almost the same way as at the Owens College. The diagrams had always been drawn out in that way, with one very simple difference, namely, that a mean diagram was obtained with the clearance spaces shown. The diagrams were distant from the vertical axis of the co-ordinates by a distance which represented the clearance space; and then separate saturation curves were drawn, one for each diagram, leaving the diagram in that shape. The Author drew an ideal compression curve, and then moved back the diagrams until the compression curve came to the vertical axis of the co-ordinates. It was a matter of small importance. He thought that when the clearance spaces were left in the diagram rather more was shown, and rather fewer assumptions were made, and it lent itself better to adding to the diagram the rectangles, showing the heat exchange with the cylinder walls. He mentioned that, partly because he had been working in the same direction as the Author for some time, and for the credit of his Institution he did not like to think that it had been altogether outdone. He might add that the diagrams he obtained were almost identical with the high-pressure and the intermediate cylinder diagrams. There was no third cylinder in the Guilds engine, and therefore not quite as great re-evaporation; but when the jacket was in full action, a low-pressure diagram was obtained just touching the saturation curve in the best cases, or slightly off it in the others. He had for some time noticed how very large the jacket condensation was. In trying the Worthington engine at the Hampton Water-Works in conjunction with Mr. Mair-Rumley, from 13 to 16 per cent. of the whole of the steam used was condensed in the jackets. In the Guilds engine the amount of condensation was often 12 per cent., the difference in the two cases being that there was no jacket on the receiver between the cylinders, whereas in the Worthington engine, at Hampton, there was a jacket on the intermediate receiver. The

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Professor Unwin. Author got still larger condensation from the jackets of the Owens College engine. It was rather remarkable that it was worth while throwing away from one-fourth to one-fifth of the steam, in order to keep the cylinder warm. He had looked with interest at the theoretical considerations on the action of the jackets on the cylinder submitted by the Author, who had taken a new line in considering merely what amount of heat supplied from the jackets was necessary to re-evaporate all the water produced by adiabatic expansion. It rather seemed that, in the numerical figures, the Author overlooked the fact that the whole heat must be given to the steam during a half of the revolution. He might be wrong, but from the figures in the Paper it appeared that it was so, and that the difference of temperature which the Author had given required to be somewhat greater. The Author neglected entirely what he believed in many engines was an important thing, namely, the percentage of priming water. He did not think it was ever absolutely absent, and suspected that, in a good many cases, it was an important factor in cylinder condensation. Then the mean temperatures, and the total quantity of heat transmitted to the steam were dealt with. It must be remembered that in order to prevent condensation on the cylinder walls, their temperature must be at every point of the stroke higher than the temperature of the steam, and that again would introduce a correction into the numerical result. The extraordinary cumulative effect due to the presence of water on the cylinder walls could not be more strikingly shown than by the very large black patches on the indicator diagrams for the case of no jackets.

Mr. Beaumont. Mr. W. W. Beaumont considered the paragraph relating to "trials without jackets" (p. 176) somewhat obscure. He understood, however, that it meant trials without jackets round the cylinders and the cylinder covers; but still with the jackets at work round the steam-pipes or the receivers. The question that occurred to him was this: the Author stated that the steam was admitted into the low-pressure cylinder as dry saturated steam, carrying into the cylinder the total heat of evaporation from the temperature of the condenser at the temperature of admission, and that it carried this heat to the condenser, less the heat-equivalent of the indicated work done in the cylinder. Was it right in that case to assume that the whole of the heat-equivalent of the work done should be deducted, or only that part of the work-equivalent which represented the expansion part of the stroke? It appeared that the receiver was really acting as a boiler, and that, instead of deducting from the heat carried away

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Mr. Beaumont.

into the condenser the whole of the work represented by the whole of the stroke, only that part represented by the expansion part of the stroke should be deducted, because steam was actually evaporated so as to suit the assumption that perfectly dry steam entered the cylinder—evaporated in that vessel just the same as if it were a boiler. He thought, therefore, that the heat-equivalent of the work done during the admission part of the stroke should not be taken into consideration as a deduction from the heat passing into the condenser; in other words, that the $p\,v$ part of the stroke should be deducted. That would slightly alter the expression given on p. 176 of the Paper, which would become

$$\frac{H_1 - h_3}{772} - \left(\frac{(\text{I.HP.} \times 42 \cdot 7) - \frac{p\,v}{772}}{\text{lbs. per minute from the hot-well}}\right).$$

If that was not deducted he found that the equivalent in heat-units of that part was very nearly the heat-equivalent which the Author said in Table II was in excess, namely, 60 units. It made rather more than the 60 units, and less heat should be accounted for in the observed than in the calculated figures. The next point was, that the Author, in the arrangement shown (he mentioned this altogether apart from the propriety of his design), used high-pressure steam to evaporate water from which he got low-pressure steam. It was obvious that there must be a loss in doing that. If the paragraph, p. 176, was taken to mean that the trials really were without jackets altogether, without jackets round the pipe as well as round the cylinder, then, of course, the same correction would be true, inasmuch as it would not be the total heat of evaporation that would have to be taken into consideration, but the heat of evaporation under constant volume only. Leaving that part of the subject, though much might be said upon it, he wished to refer to p. 167, in which the Author spoke of a different kind of brake. He gave categorically his reasons for adopting the new instead of the older form of brake. Firstly, he said that such brakes required constant observation and watching; secondly, that a single engine could not be started without relieving the load; and thirdly, that brakes were cumbersome and not easily adapted to measure greatly different powers. With reference to these three points, it was fairly shown in the Paper that Mr. Beaumont had the honour of presenting to the Institution last session, that friction-brakes not only could be, but had been used, for trials of engines extending over greater periods than any of the trials mentioned

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Mr. Beaumont. in connection with this engine; that engines could be started without relieving the load, and that the variation in the position of the suspended load need not be very great. Taking the last point first, he might recall attention to the brake known as the Balk brake, used for many years by Messrs. Ransome, and for engines working at very different powers. That brake could be set so that it should be run at least an hour at a time with the weight in the scale-pan, not moving more than $\frac{3}{4}$ inch, less the diameter of the pin that went into a slot of that width. With that brake the work upon the engine, or that the engine was doing, could be read off direct at any time, and the brake was run without water. Another and a better brake for varying loads, and needing perhaps even less attention, was one used by Mr. Halpin and others, in which there was a channelled fly-wheel with water running into it. The water simply ran into it and evaporated. If the wheel was kept sufficiently cool, that brake might run for almost any length of time without more attention than the one man driving and stoking the engine could give it. The variation in the level of the load was very little indeed. With regard to that variation, he noticed that the Author had shown in the illustration something very much like a dash-pot, and above that again, a spring, and he said the danger of such an arrangement had been carefully considered. This was with reference to the water-brake. He did not say what the precautions were, but he supposed they took the form of a dash-pot. If a dash-pot were used with either the Balk brake, or the water-channelled wheel-brake, or the rope-brake on the water-channelled wheel, all the results could be as easily, as economically and as accurately obtained as with this water-brake, and with a smaller number of pipes, water-connections and so on. Pipes and water-connections, however, did not seem to frighten any one concerned with the engine shown, so that perhaps they would not be looked upon as an objection. He thought that those three paragraphs might be answered therefore with at least a qualified denial. In conclusion, he wished to say that this arrangement of engines might have some advantages for experimental purposes; but the question was, whether those advantages were not obtained at the cost of great disadvantages.

Mr. Bodmer. Mr. G. R. Bodmer said it was superfluous for him to congratulate the Author on his very instructive Paper, since its value was obvious to all interested in the theory of the steam-engine. It seemed, however, rather a pity that, while making a new departure with regard to the design of the engine, the Author did not go a step further and jacket the piston. It was attended by some

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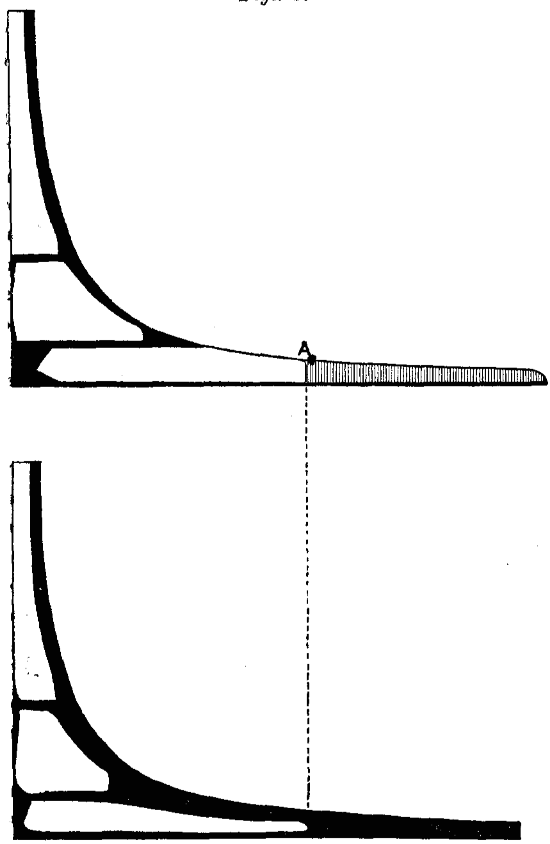
practical inconvenience, but there was no very great difficulty about it, and it would be an interesting experiment. With respect to the trials and their results, it was rather to be regretted that the engines were allowed to run at different speeds, because it prevented the conclusions drawn from the experiments being directly applied to ordinary triple-expansion engines. For Mr. Bodmer.

Figs. 3.



instance, when the low-pressure engine ran at a higher speed than the high-pressure one, it was equivalent to having a larger low-pressure cylinder, viz., to having a greater amount of expansion than was indicated by the cylinder-volume. It was obvious that in an ordinary engine it was impossible to vary the final cylinder-volume, and, with a given cut-off, the expansion ratio

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Mr. Bodmer. was fixed. On that account it would be wrong to infer that the same advantage could be secured by jacketing an ordinary triple-expansion engine or compound engine as the Author had been able to obtain by jacketing his engine. To explain his remarks he had prepared *Figs. 3*, from the Author's illustrations. The lower diagram represented the experiments with the cylinders unjacketed; the upper diagram represented the corresponding experiments with cylinders jacketed. Taking the lower *Fig.* to represent the diagram of an ordinary unjacketed triple-expansion engine (the length of the diagram corresponding to the stroke of the low-pressure cylinder), then, if that steam-engine were jacketed, the volume of the low-pressure cylinder could not be increased or treated in a way that was equivalent to increasing it, by allowing it to run at a greater speed than the high-pressure part of the engine; therefore, the advantage obtained from jacketing that engine would be represented, not by the whole of the upper diagram, but only by the white portion; and, as compared with the Author's results, the whole advantage represented by the shaded portion of the diagram was lost, being the equivalent of the increased speed of the low-pressure, as compared with the high-pressure engine. With this portion of the diagram cut off, it was evident the advantage obtained by jacketing was much diminished. Nevertheless, the Author's experiments were extremely interesting, and tended to show that, in order to obtain the full advantage from jacketing an engine, the volume of the low-pressure cylinder must be made greater than if the engine were unjacketed, assuming a given cut-off in the high-pressure cylinder. Coming to the question of the action of the jackets, the Author assumed that heat actually passed through the walls of the cylinder from the steam in the jacket to the steam expanding in the cylinder, and attributed, at any rate, part of the beneficial action of the jacket to the passage of such heat. The figures appeared to a great extent to contradict this view, and Mr. Bodmer had prepared a "Supplementary Table," recapitulating a few of the Author's figures, and also giving some ratios calculated from them. Before referring to that Table he might mention that recent experiments had shown, notably those of Mr. Willans, and also the Author's trials with the unjacketed engine recorded in Table IV, that it was not essential to re-evaporation that the cylinder should be jacketed; but that re-evaporation took place in an unjacketed cylinder to a certain extent. The necessary heat, of course, came from that previously absorbed by the metal during the admission period. Line III of the Supplementary Table gave

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Mr. Bolmer.

Supplementary Table.

—	—	44	41	33	35	56	40
I [44]	T. U. received as dry steam per minute . .	6,710	8,874	10,700	12,812	14,154	19,303
II [47]	T. U. received from the jackets, &c., per minute	2,337	1,390	3,072	1,643	3,325	2,225
III	Ratio of heat received from jackets to heat received as dry steam per cent.	34·8	15·7	28·7	12·8	23·5	11·5
IV	Excess of heat received by jacketed as compared with unjacketed engine, per cent. . .	16·5	..	14·0	..	10·8	
V [36]	Thermal efficiency . .	0·185	0·141	0·192	0·153	0·194	0·155
VI	Relative increase of efficiency due to cylinder jacket, per cent.	31·2	..	25·5	..	25·1	
VII	Ratio of heat lost by radiation to heat received as dry steam, per cent.	18·3	..	13·0	..	8·3	

the ratio of heat received from the jackets to that received as dry steam, and he had placed the results of corresponding experiments with engines jacketed and unjacketed side by side. For instance, column 44 referred to a series of trials with jacketed cylinders; 41 to the corresponding series with unjacketed cylinders. Taking the first two columns, for the jacketed engine the percentage was 34·8, and for the unjacketed 15·7. The 15·7 represented heat received only from the receiver jackets, and, assuming the ratio of the heat lost in the receiver jacket to be the same in both sets of trials, the proportion of heat given off by the cylinder jackets would be represented by the difference between 34·8 and 15·7. Line IV gave in percentage the excess of heat received by the jacketed as compared with unjacketed engines. That was for the first two series of trials (44 and 41), 16·5, for the second (33 and 35), 14·0, and for the third (56 and 40), 10·8 per cent. This was the heat actually given off in one way or another by the cylinder jackets as distinct from the receiver jackets, and owing, presumably, to the expenditure of that heat the increased efficiency represented by line VI was due. From this it appeared that for an expenditure of heat equivalent to 16·5 an increase of efficiency equal to 31·2 per cent. was obtained. That of itself was sufficient to show that only a portion of the apparent advantage secured came from the heat passing through the walls. To arrive at the proportion of heat actually transferred from the steam in the

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Mr. Bodmer. cylinder jackets to the working steam, the heat lost by radiation must be deducted from the total expenditure in the cylinder jackets. The Author had not separated the loss by radiation in the receiver jackets from that in the cylinder jackets, and consequently Mr. Bodmer was unable to arrive with accuracy at the latter quantity. So far as could be judged from the illustrations accompanying the Paper, the ratio of the surface from which radiation took place for the cylinder jackets to the total radiating surface was as 1 to 3, and assuming this to be correct, an estimate could easily be formed of the quantity passing through the walls, which would be less by from 25 to 30 per cent. than the values given in line IV. As a matter of fact it might not be superfluous to point out, that, on thermo-dynamic principles, it was impossible any improvement in efficiency could result from an increase in the work done, due directly to the transfer of heat from the steam in the cylinder jackets to the steam working in the cylinder, because heat communicated in such a manner was utilized even less effectively than the heat introduced with the steam admitted to the cylinder. From this it was obvious that the whole of the advantage gained by the use of the cylinder jackets was a consequence of the restoration of heat absorbed, previous to cut-off, by the cylinder walls; this restoration took place partially in an unjacketed engine, but was rendered more complete by the action of the jacket in preventing recondensation during the admission period in the intermediate and low-pressure cylinders. The best of all would be to prevent initial condensation in the high-pressure cylinder, where the steam was of most value. It was to be regretted that the Author had not stated the proportion of steam condensed at various points of the stroke in the trials with jackets, in the same way as this had been done in Table IV for the trials without jackets. On p. 184 the Author said: "If the walls of the cylinders are maintained at the temperature of the initial steam, the expanding steam will absorb heat. This heat must pass through the walls; and as heat only flows through metal down the gradient of temperature, the temperature on the outside must be greater than that on the inside." Now this statement, he thought, was at variance with known facts. The heat absorbed by the expanding steam did not necessarily pass through the walls. In a cylinder without a jacket, the temperature of the walls after cut-off was lower than the temperature of the initial steam, and yet experiments had clearly shown that the expanding steam absorbed heat, and re-evaporation took place. It was in the main not a question of the passage of heat through the walls, but, as already indicated, of a

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restoration of heat to the steam from which it had been taken during the admission period. The Author's calculation, of the amount of heat per lb. of steam necessary to prevent supersaturation, was not applicable to the conditions obtaining in his engine; it would only be correct if the whole of the steam, at the moment when expansion commenced, were quite dry and were maintained in this condition throughout. The quantity of heat required to prevent the condensation otherwise accompanying work performed (in a non-conducting cylinder), depended on the proportion of water initially present. If about 50 per cent. of water were present, no heat would be required to prevent further condensation, the saturation curve for the dry portion of the working mixture (steam and water) corresponding with the adiabatic curve for the whole of the mixture. The only correct method was to calculate, from the total measured work done during expansion on one side of the piston and the increase (or decrease) in the internal heat, the quantity of heat communicated during expansion, and compare this with the heat given up to the walls during admission. The difference between these two quantities would give the heat which must pass through the walls from the jacket-steam. Mr. Bodmer.

Mr. P. W. Willans said the subject was one which had long been of interest to him. He would refer in the first place to a paragraph in the Paper (p. 152), and then to two paragraphs in the abstract of the Paper (p. 195). "A systematic and experimental investigation of the steam-engine involves two sets of conditions, which, unless it be in a laboratory, can hardly exist together, namely, the time and attention of the scientific investigator, and the assistance of a considerable number of trained observers. In the engineering laboratory, these conditions should exist: the first being supplied by the permanent staff, and the second by the students as their training advances." "For this purpose, ordinary steam-engines, doing ordinary work, were about as good as ordinary animals in their ordinary occupation, were for subjects of physiological experiment. In certain particulars both could be accurately observed, as, for instance, how much coal or food was necessary during the performance of a particular operation; but to ascertain how all the separate organs were performing their several functions was impossible with an ordinary steam-engine, without subjecting it to a species of mutilation which left it about as fit to continue ordinary work as an animal that had been the subject of physiological study." "It was evident that the engines and apparatus for operating upon should be such, that each organ, Mr. Willans.

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Mr. Willans. while similar to, and performing the part of, the corresponding organ in an ordinary engine, should be so arranged with respect to the other organs, as to admit of the manner in which it performed its part being completely gauged; and that this should be simultaneously with all the organs, both those corresponding to the ordinary engine and such special organs as should be introduced to admit of access to the others, so that the action of these latter might be accurately discounted." He had asked himself the question whether, what was practically stated in these paragraphs—that experimental investigations in connection with steam-engines could only be properly carried out in the laboratory, and on engines of special construction—was or was not true, and what advantages those working in a laboratory had over others? He had come to the conclusion that a man in the Author's position had one great advantage, namely, that he ought to be quite unbiassed in his work by any feeling as to whether it would come out well or not; and that was a position in which it was exceedingly difficult for men like himself to place themselves, because they were influenced every day by the feeling of competition, and by a desire to do better than others. The Author, however, was working under great disadvantages in other respects; he had only one engine, and although he might give it at the start as many organs as he pleased, yet he could not give it the organs which were not yet invented. He compared experiments on steam-engines with physiological experiments on animals, but there was this difference between animals and engines: that in their time at any rate, an animal was pretty much the same animal, for if the Author took a rabbit and went on experimenting for the next hundred years, he would still be able to get another rabbit with which to continue his experiments; but if he took an engine, the worst of it was, there was soon somebody else who had made a better engine, and evolution in the case of steam-engines being so rapid, it was impossible to make a laboratory engine which should for any long time together remain a representative one. However efficient such an engine might be now, it would soon be superseded, and the scientific investigator, with only one engine, would then be in about as good a position to settle points relating to steam-engines as the physiological experimenter would be to determine the speed of the next Derby winner, if the only animal available for experiment was a specimen of the ancient Eohippus. Then the maker of steam-engines had another advantage, although it might sometimes be thought a disadvantage, and that was, that when he had made an engine and said that it did so and so, somebody else frequently

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Mr. Willans.

said it did not. He did not say that this would always seem an advantage at the time; but in the long run it certainly was an advantage to the experimenter to have to prove his case and his figures to the satisfaction of an outsider. He had read the Paper carefully, checking it figure by figure, so far as the data given would allow, because, first of all, he wished to understand the methods on which the Author had been working. Being about to start similar trials, he wished to get the benefit of his ideas, and therefore had gone more carefully into the matter than he would have done at any other time. He regretted, however, to say that he had not been favourably impressed with many of the Author's methods, and he also totally disagreed with his choice of the standard with which to compare the work of his engine. He proposed, therefore, to follow the Author through his various methods and calculations, and the data which he would make use of were the following:—

Table I, line 56	.	lbs. per hour of mixed feed to boiler, neglecting leakage.
,, ,, 37	.	lbs. of water discharged from hot-well per minute.
,, ,, 46	.	lbs. per minute of water from the jackets, &c.
,, ,, 27	.	Total I.HP.
,, ,, 5	.	Initial temperature calculated from pressure in 1st receiver.
,, ,, 6	.	Final temperature calculated from condenser-pressure.

Now with respect to these data, and the methods by which they had been obtained, he wished to offer a few criticisms. The mixed feed per hour (line 56) was the sum of water discharged from the hot-well, and of that discharged from the jackets. Neither of these quantities had been weighed as a whole, and the only reason he could see for the method adopted—namely, weighing 100 lbs. at a time in the case of the hot-well water, and noting the rate at which the water rose in the separator for a portion of the time in the case of the jacket-water—was that an engine trial and boiler trial were made together, and that the Author wished to get the best possible results in both in order to beat the record. This, in the abstract of the Paper, he claimed to have done, and he must not be surprised, if on that account his figures were scrutinized closely, and his methods criticised where there was occasion. The Author had stated that there was a difference in most cases of from 5 per cent. to 10 per cent. between the water supplied to the boiler and that discharged from the hot-well. He did not think that any experimenter was

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Mr. Willans. justified, when he knew of such a discrepancy, and knew also that the boiler was "practically tight," in taking the discharge from the hot-well for the purpose of calculating his results; he could not believe that there was any law of nature by which differences of 10 per cent. between the two could be accounted for. Only recently, in a Paper by Mr. Thornycroft,[1] an account was given of several trials of a torpedo-boat, and in these the difference between feed-water and hot-well discharge was always less than 1 per cent., after allowing for steam used by a steering engine. Why then should there be such a difference in this case? And why, if there was such a difference, was it taken as proved that none of this steam passed through the engine? The Author took no account of what was usually present, namely, a little vapour from the hot-well, and the same from the piston-rods and glands (there were nine rods in all, including the valve-rods); but he jumped to the conclusion that the engine had nothing to do with this 5 per cent. or 10 per cent. Mr. Willans contended that this was not right, and he felt sure that if any other engineer gave such figures about his own engines, they would be most severely criticised. Mr. Willans had given, in all cases, in his Paper,[2] the total feed-water pumped into the boiler during the period of the experiment, the water being drawn from a large tank mounted on a weighing-machine (entailing one reading only for the whole time), and he had based all his figures upon the total so ascertained. Would the Author think it fair of him if he deducted 5 to 10 per cent. from the total, because the Professor found that such a difference generally existed? He also thought that the figures for feed-water for each trial might have been supplied, seeing that there was a doubt in the matter. Next, with respect to the figures for jacket-water, Table I, line 46. The Author explained that these were arrived at by "measuring the rate of discharge from the jackets every half-hour." The cock at the bottom of the separator was, as he understood it, shut by the student every half-hour, and the rate at which the water rose in the gauge-glass on the separator was noted. Now the dimensions of the separator were such that not more than 20 or 25 lbs. of water could be allowed to collect in it, so that, assuming absolute accuracy in the observations taken every half-hour, about 40 or 50 lbs. of water per hour would be measured; while 237 lbs. in the case of trial 56 was assumed to be the quantity passing through per hour; if therefore he understood

[1] Minutes of Proceedings Inst. C.E., vol. xcix. p. 41.
[2] *Ibid.*, vol. xciii. p. 128.

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Mr. Willans. the Author correctly, only one-fifth of the jacket-water had in this trial gone through even a form of measurement. Surely it would have been a more certain plan to have made the engine or boiler trials independently, and to have weighed accurately the jacket-water for the entire trials. Next, as to the total indicated HP., an elementary difficulty was that the dimensions necessary for re-calculating the indicated power were not given. On p. 153 the dimensions of the engine were stated to be "somewhat as follow." He did not consider that "somewhat as follow" was quite near enough. A dimension was also missing, for, in calculating the power, however interesting the diameter of crank-shaft might be, it was not so necessary as the diameter of the piston-rod, in getting at the net area of the cylinders, and he trusted it had not been omitted from the calculations as well as from the table of dimensions. Then, if he understood the Author correctly, the mean pressure had been calculated by measuring the pressure on the diagrams at ten breadths, and taking the mean of these as the mean pressure. "These results have been several times checked by a planimeter without establishing any sensible difference." He wished to point out that the measurement could only be accurately done by means of a planimeter, for if measurements by ordinates were taken in the middle of ten equal breadths, in the case of the jacketed low-pressure diagrams, Plate 4, Fig. 14, there would be, for trial 33, a result at least 5 per cent. in excess of that given by the planimeter; this was on account of the late admission and the position which the first measurement happened to take with reference to the corners missing from the diagram. The same remark applied in a varying extent to all the diagrams; he thought it was impossible to impress the fact too strongly on the minds of students, that the measurement of indicated power by ordinates was an inexact and uncertain method.

In order to ascertain the thermo-dynamic efficiency of the engine, it was necessary to ascertain the higher and the lower limits between which the engine worked. Here again he was met by a difficulty, for the temperatures given in Table I, line 54, as the temperatures of the boiler, were the same throughout all the six trials, namely, 383° Fahrenheit, whereas the mean absolute pressure in the boiler varied from 200 lbs. in trial 44 to 207 lbs. in trial 56. This would not make much difference to the efficiency, but it had made it a little difficult to follow the Author's calculations, the temperature being the starting-point. In going through the Paper casually, his attention had been drawn to the very high thermal efficiency which the Author gave for one of the trials,

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Mr. Willans.

—	Number of trial . . . .
A	Lbs. of mixed feed per hour (neglecting leak)—Table I, line 56 . .
B	Total indicated HP.—Table I, line 27 . . . .
C	Water per indicated HP. per hour, neglecting leak, but otherwise as usually reckoned, *i.e.* hot-well and jackets . . . .
D	Water per indicated HP.—Table I, line 70 . . . .
E	Measured efficiency—Table III, line 9 . . . .
F	Thermal efficiency as usually reckoned—Table I, $\frac{\text{line 34}}{\text{line 48}}$ . . . .
G	Initial and final temperatures, Fahrenheit—$\frac{T^1 \text{ steam-chest}}{T^2 \text{ condenser}}$ . . .
H	Cost of 1 lb. of steam in thermal-units, water raised from condenser temperature to steam-chest temperature and evaporated at steam-chest temperature . . . .
I	Lbs. per minute feed from hot-well—Table I, line 37 . . . . .
J	Thermal units received per minute by engine in weight of steam answering to hot-well discharge . . . .
K	Thermal units given up per lb. of steam condensed in jackets at boiler temperature . . . .
L	Lbs. of jacket-water per minute—Table I, line 46 . . . . . .
M	Thermal units received by engine per minute from jacket-steam . .
N	Total heat per minute (feed and jackets) . . . .
O	Heat utilized—Table I, line 34 . . . .
P	Absolute efficiency—$\frac{\text{line O}}{\text{line N}}$ . . . .

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Mr. Willans.

44	33	56	41	35	40
516·2	793·2	977·4	562·8	805·2	1,213·2
33·23	55·76	72·1	30·92	48·51	73·87
15·53	14·23	13·56	18·20	16·60	16·42
14·1	13·2	12·68	17·3	16·0	15·9
18·5	19·2	19·4	13·8	15·3	15·5
15·71	17·31	17·65	12·91	14·35	14·67
$\frac{381·15}{115·9}$	$\frac{380·7}{120·35}$	$\frac{382·8}{129·8}$	$\frac{383·2}{110·8}$	$\frac{383·6}{122·4}$	$\frac{382·0}{134·6}$
1,108·9	1,104·3	1,095·5	1,114·6	1,103·0	1,090·5
5·83	9·58	12·34	7·73	11·47	17·58
6,464·8	10,579·2	13,518·5	8,615·8	12,651·4	19,171
843·43	843·1	841·29	841·89	841·59	842·5
2·77	3·64	3·95	1·65	1·95	2·64
2,336	3,068	3,323	1,389	1,641	2,224
8,801	13,648	16,840	10,005	14,292	21,395
1,421·0	2,384·0	3,085·0	1,323·0	2,074·0	3,158·0
16·14	**17·46**	**18·31**	**13·22**	**14·51**	**14·76**

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Mr. Willans.

—	Number of trial . . . .
Q	Theoretical efficiency of Carnot cycle: heat utilized per lb. of steam = latent heat at A $\times \frac{A-B}{A}$ . . . .
R	Theoretical efficiency of Clausius perfect engine: heat utilized per lb. of steam $= (1{,}438 - 0{\cdot}7\,A)\frac{A-B}{A} + (A-B) - B\log_\epsilon\frac{A}{B}$ . .
S	Theoretical efficiency of jacketed engine: heat utilized per lb. of steam $= 1{,}438\log_\epsilon\frac{A}{B} - 0{\cdot}7\,(A-B)$ . . . .
T	Author's theoretical efficiency—Table III, line 8 . . . .
U	Percentage of theoretical efficiency of Carnot cycle—$\frac{\text{line P}}{\text{line Q}}$ . . .
V	Percentage of theoretical efficiency of Clausius perfect steam-engine —$\frac{\text{line P}}{\text{line R}}$ . . . .
W	Percentage of theoretical efficiency of Rankine's jacketed engine— $\frac{\text{line P}}{\text{line S}}$ . . . .
X	Author's percentage of theoretical efficiency—Table III, line 10 . .
Y	Feed-water per indicated HP. hour required by Clausius perfect engine . . . .
Z	Feed-water per indicated HP. hour which would be raised from T_2 to T_1 and evaporated at T_1 by total heat per minute, line N $= \frac{\text{line N} \times 60}{\text{line H} \times \text{line B}}$ . . . .
..	Efficiency $\frac{\text{line Y}}{\text{line Z}}$ . . . .

In the above Table T_1 and T_2 are temperature Fahrenheit scale,

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Mr. Willans.

44	**33**	**56**	**41**	**35**	**40**
31·49	30·93	29·98	32·26	30·92	29·34
28·33	**27·86**	**27·06**	**28·96**	**27·8**	**26·52**
25·8	25·4	24·7	26·3	25·4	24·3
23·3	23·2	22·7	23·3	23·2	22·4
51·2	56·4	61·0	40·9	46·9	50·3
56·9	**62·67**	**67·6**	**45·6**	**52·1**	**55·66**
62·5	68·7	74·1	50·2	57·1	60·7
79·4	**82·6**	**85·4**	**59·2**	**65·9**	**69·4**
8·17	8·33	8·65	7·94	8·35	8·86
14·33	13·29	12·79	17·41	16·02	15·93
56·9	62·67	67·6	45·6	52·1	55·66

and A and B the equivalent temperature Fahrenheit absolute.

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Mr. Willans 19·4 per cent. in one case (in the body of the Paper 0·20 was named, p. 174). The percentage of theoretical efficiency actually obtained in this case (trial 56) was given as 85·4. In non-condensing engine trials he had been accustomed to get as much as 82 per cent. himself; but in such condensing engine trials as he had made it was very much below that, and he had not hitherto seen, in any recorded experiments, figures at all approaching 85 per cent.; he had, therefore, from the Author's data named above, re-calculated all his figures as to efficiency in the standard methods, and the results of these calculations were given in the preceding Table, pp. 218–221.

In this Table, lines A, B, D, E, I, L, O, T, and X were simply transferred from the Author's Table for comparison. The figures in line C were obtained by dividing those for mixed feed in line A by those for total indicated HP. in line B. The figures in line A were those which took no account of the leak to which reference had already been made, and the calculations based on these figures, therefore, gave the Author the benefit of the doubt, as he did himself. The figures in line C were directly comparable with many of the figures given for water per indicated HP. in other cases of jacketed engines, in Mr. Mair-Rumley's among others. It was not everybody who had the advantage of being able to drain the water back into the boilers in the admirable manner adopted by the Author, and frequently the jacket-water was allowed to drain into the hot-well, and was pumped back into the boiler as part of the feed. The Author's figures, "lbs. per indicated HP. of feed-water to supply as dry steam the total heat for engines," were given in line D for comparison, and it should be understood that these figures did not represent the total steam passing from the boiler in the shape of steam and returning as water, but such an amount of steam as would be evaporated by the heat abstracted from the total steam passing through the engine and through the jackets. Undoubtedly the Professor was justified, in his case, in looking at the matter in this manner; but it was evident that certain conditions were necessary as to the relative levels of engines and boiler in order to permit the jacket-water so to drain back. In line E were stated the Author's figures for thermal efficiency, the only figures given in the Paper for the absolute efficiency of the engine. The wording of that line was rather curious, "thermal efficiency as given by heat discharged in condensing water"; but he was surprised to find it stated, in the abstract of the Paper, though not distinctly in the Paper itself, that it did not include radiation; and no doubt many of the members had not realized

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that radiation was excluded. The heat-units lost by radiation in some cases amounted to a very large proportion of the heat-equivalent of the indicated power. In the case of trial 44, for instance, the loss by radiation was given as 1,227 thermal units, while the indicated power was equivalent to 1,421 thermal units. If radiation was excluded from such trials, the work done might almost as well be neglected. He had re-calculated that line by dividing the figures in line 34, which gave the thermal units discharged as indicated HP., by the figures in line 48, which gave the total heat supplied to the engine. The results were supplied in line F, and were those which were usually taken as representing the absolute efficiency of a heat-engine, namely, $\frac{\text{heat utilized}}{\text{heat supplied}}$. The Author was trying jacketed against unjacketed engines, and it was obvious that the radiation in the former must be greater than in the latter. In excluding the radiation, he omitted one of the most important factors in the comparison between the two engines; the effect of the omission being greater in the case of the jacketed than in the unjacketed engine, it gave the jacketed engine an unfair advantage. Mr. Willans was not in any way concerned to defend unjacketed against jacketed engines; he only said that in this case it was not a fair trial. Mr. Willans.

What the Author called thermal efficiency was not, therefore, what engineers were in the habit of calling absolute efficiency, and he thought he should show also, that the standard with which the Author had compared his results, was not the one upon which engineers ought to look as representing the highest theoretical efficiency. He had recalculated the efficiency of the engine in the various trials, taking as the initial temperature that answering to the pressure in what the Author called the first receiver, the steam-chest of the high-pressure engine, and as the final temperature, that answering to the condenser back-pressure. These temperatures were, in the case of trial 56, 382°·8 and 129°·8 Fahrenheit. The feed-water from the hot-well might be assumed to be raised in the boiler from 129°·8 to 382°·8, and to be evaporated at the latter temperature; if it happened to leave the hot-well at a lower temperature than 129°·8 Fahrenheit (the temperature answering to the condenser back-pressure), that was a circumstance which ought not to tell against the engine as an engine, for the feed-water could, theoretically at least, be raised by the exhaust-steam to the temperature of the latter, but that was the highest temperature to which the feed-water could be raised by waste heat from the engine

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Mr. Willans. itself. He therefore assumed that each lb. of water, leaving the hot-well, represented an expenditure of heat equivalent to that required to raise it from the temperature answering to the condenser back-pressure, to that answering to the pressure in the first receiver, plus the latent heat of evaporation at the latter temperature. In the case of trial 56, the expenditure of heat per lb. of hot-well discharge (line H) was 1,095·5 thermal units, and the lbs. discharged per minute (line L) were 12·34; thus the thermal units debited to the engine per minute on account of steam answering to the hot-well discharge were 13,518·5 thermal units (line J). In the case of the jacket-water only the latent heat was given up, and he therefore debited the engine with the latent heat of each lb. of steam condensed at boiler temperature; in the case of trial 50, the jacket-water (line L) amounted to 3·95 lbs. per minute, and the latent heat being 841·29 thermal units per lb., the heat debited to the engine on account of jacket-steam was 3,323 thermal units (line M). The total heat debited to the engine per minute, lines J and M, amounted to 16,841 thermal units (line N) = heat supplied. The heat utilized was the heat equivalent of the work done = 3,085 thermal units (line O). Line P gave the absolute efficiency of the engine, as calculated from the above figures, and it was, for trial 56, 18·31 per cent. All these calculations were based on the quantity of water proved to have passed through the engine, and did not include the leakage already mentioned.

The next question was as to the engine which should be taken as the standard of perfection. It had been a common thing to take the Carnot cycle, working between the same limits of temperature, as the standard; in this cycle all the heat was assumed to be received at the higher temperature, and the heat necessary to raise the condensed steam, from the lower to the higher temperature, was assumed to be supplied by the conversion of a part of the power generated into heat, by the process of compressing a portion of steam remaining in the cylinder. Not long ago he went very fully into this question,[1] and gave his reasons for not using the Carnot cycle as a standard. These reasons were mainly theoretical, but he thought there was one very strong practical reason why the Carnot cycle would never be carried out in practice, and this was that it would always be cheaper to heat the feed-water by the waste gases from the furnace. In order to heat the feed-water dynamically, some part, however small, of the power of the engine must be used, but, as the Author had shown in this case, the furnace

[1] Minutes of Proceedings Inst. C.E., vol. xcvi. p. 239.

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Mr. Willans. gases were always at hand for the purpose, and as their heat would otherwise be wasted, it was better to use it than any part of the power of the engine, however small that part might be. If, however, it was fitting in any case to compare the absolute efficiency of an engine with the absolute efficiency of the Carnot cycle, it would be so in this case, where in some trials almost 33 per cent. of the steam passing through the engines and jackets passed through the latter. All the heat supplied to this jacket-steam was supplied at the higher temperature, and consequently the percentage of work due from it was greater than that due from the heat supplied to the hot-well water. Unfortunately, however, although the heat was supplied in the boiler at the higher temperature to the hot jacket-water, it was transmitted through the walls of the cylinder to the working fluid under very different conditions. Still, he thought it would be interesting to give in line Q the absolute efficiency of the Carnot cycle working between the same limits of temperature (line G). In the case of trial 56 this efficiency was 29·9 per cent. as compared with 18·31, the absolute efficiency of the actual engine. Line U gave the percentage of the theoretical efficiency of the Carnot cycle reached by the actual engine in trial 56, which was $\frac{18 \cdot 31}{29 \cdot 9}$, or 61·3 per cent. of the theoretical possibilities, taking that cycle as the standard.

Now putting on one side the Carnot cycle as an unattainable ideal, and for the practical reasons already given an undesirable ideal, he would next compare the efficiency of the actual engine, in trial 56, with the efficiency of the perfect steam-engine of Clausius; that was to say, he would compare the return from heat expended, with the return from 1 lb. of steam supplied to an engine in which there were no losses from passages, leaks, or cylinder-walls; in other words, an engine which took 1 lb. of steam into the cylinder at the higher temperature, expanded it adiabatically to the lower temperature, and expelled it at the lower temperature. It might be said that this was also an unattainable ideal, and in the sense that no engine would ever be made which absolutely came up to it, it was so. Still it represented the highest aim of the engineer, and it was being steadily approached. In certain classes of engines, certain classes of loss had been practically eliminated; for instance, in low-speed engines there was no appreciable loss from passages, and he himself had shown that, in a high-speed engine, it was possible to approach very closely indeed to the adiabatic curve. Under certain conditions this could be approached, for instance, within 5 per cent.; therefore he always took as his standard

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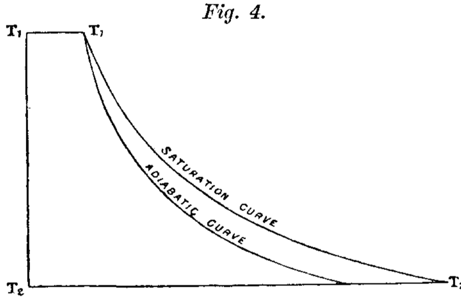
Mr. Willans. the result from the practical but perfect engine of Clausius, and compared his results with it. Line R gave the theoretical return from such an engine always working between the limits of temperature given in line G. This return was, in the case of trial 56, 27·06 per cent. of the heat supplied, and comparing this return with that from the actual engine, which had been already shown to be 18·31 per cent. of the heat supplied, he found that the relative efficiency of the Author's engine, as compared with the perfect steam-engine of Clausius, was 67·6 per cent. The Author, however, took as his standard an engine which gave a return of only 22·7 per cent. of the heat supplied to it, in the case of trial 56. He compared what he called "thermal efficiency" (which was arrived at by neglecting radiation, one of the most important factors in the case), with something that did not, to Mr. Willans, represent the "thermal" efficiency of any engine or cycle whatever.

It would be easier to follow his argument by reference to *Fig. 4*, which showed the work possible from 1 lb. of steam treated in different ways.

Fig. 4.



T_1 and T_2 were the temperatures between which the engine worked. The inner curve of expansion was the adiabatic expansion-curve for steam expanding from T_1 to T_2; not "a special adiabatic curve," as the Author said, but the adiabatic curve, for there was only one, if dry steam was supplied. The outer curve was the saturation-curve for 1 lb. of steam between T_1 and T_2. The inner line was the line which the indicator would trace if no heat was given off to the sides of the cylinder and none received from them. The outer line was what the indicator would trace if, by adding heat during expansion, the water necessarily formed was re-evaporated by heat received from the steam-jacket. The cost of the inner, or adiabatic diagram for a theoretically perfect steam-engine, was the cost of raising 1 lb. of water from T_2 to T_1, and converting it into steam at T_1. In the case of an engine

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Mr. Willans.

working between the same limits of temperature as those in trial 56, the cost was 1,055·5 thermal units (see line H). The area of the diagram, namely, the work done, was 297·4 thermal units, representing an efficiency of 27·06 per cent. as given in line R. The cost of the outer diagram was the cost of 1 lb. of steam as above, plus the cost of keeping it during expansion in the state of saturation. This latter added heat was given by the formula $1{,}438 \log_{\epsilon} \frac{A}{B} - (A - B)$, and was in the case of an engine working between the same limits of temperature as those in trial 56, 259·58 thermal units. The area of the diagram was represented by 335·5 thermal units, so that the return for heat expended was only 24·7 per cent. Thus, between the thermal limits in question the return from steam used in an engine expanding its steam adiabatically was 27·06 per cent. as compared with 24·7 per cent. in the case of the best possible jacketed engine. The cost of the saturation diagram was to the adiabatic as 1·23 to 1, while the return from it was only as 1·13 to 1; so that when the Author stated, that both in the case of the jacketed and unjacketed trials, he had compared the actual efficiencies with the highest theoretical efficiency between the same limits, and that he had calculated the theoretical efficiency as for saturated steam, it seemed to Mr. Willans to be a contradiction in terms. The work shown by the saturation diagram, compared with the cost of such a diagram, by no means represented the highest theoretical efficiency. The Author, however, appeared to think that it did, for in the abstract of the Paper he congratulated himself on the result in the following words: "Thus, in these trials, with steam at boiler-pressure in the jackets, low-pressure diagrams had been obtained, apparently for the first time, in which the curve of expansion coincided exactly with the curve for saturated steam." The fact was that jacketing was simply a compromise. It was effective in promoting economy in certain cases, because more was gained by preventing condensation than was lost by re-boiling the water formed during expansion, and using high-pressure to make low-pressure steam. But for comparison with the work performed, the standard should be the adiabatic diagram. In many diseases it was necessary to prescribe poisons; but after a doctor had prescribed, say arsenic, it would hardly be satisfactory to the patient's friends to be told that he was now a fair sample of a man poisoned by arsenic, even if the original disease had vanished. The question was how the patient's state compared with that of a healthy man. So with the Author's engine. The question was how far did it

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Mr. Willans. come short of the ideal perfection possible in a steam-engine; not how near was it to the comparatively sickly jacketed standard. Looking at the matter broadly, it seemed to Mr. Willans that the Author had increased the efficiencies in the various trials by omitting factors always of great importance, and in this case of especial importance, and that he had then compared these inflated results with a new standard for which there was no sound reason. The efficiency which the Author gave as 85·4 per cent. was, he considered, 67·6 per cent. These were the figures to which he wished to draw the attention of the meeting. It would be seen that the Author's figures for water per indicated HP., line D, agreed very closely with line Z calculated by him; and he presumed that these slight differences could be accounted for by differences in the temperatures taken as the higher and lower temperatures for ascertaining the cost of steam; but he thought the figures given by himself were those which would be taken by most engineers. He had given, in line S, the theoretical efficiency of the best jacketed engine working between the limits of temperature in the various trials; but even these efficiencies did not exactly correspond with the efficiencies which the Author had taken as a standard, which were given in line T. Evidently the area of the saturation diagram had not been taken in its fulness as the measure of work obtainable through the agency of 1 lb. of steam, and the explanation was apparently found in a sentence which appeared in this connection, p. 181: "The area enclosed between the limits of pressure and volume . . . expresses in foot-lbs. the greatest possible amount of heat that can be converted into work, through the agency of 1 lb. of steam maintained in a state of saturation between these limits." In fact the Author had compared his results, not with the whole area of the diagram for 1 lb. of steam maintained in a state of saturation between the limits T_1 and T_2, but with that diagram shortened so as to correspond with the terminal pressure in his own experiments, that was, with a still lower standard than the jacketed engine; this was an interesting comparison for some purposes, but had no connection with what was now usually understood by the words thermal efficiency. If the standard was to be altered with the point of cut-off, a fire-engine or a donkey-pump would be the most efficient engine in existence.

With respect to the general results of the trials, a point which had struck him very much was the small effect of the jackets on the missing quantity in the high-pressure cylinder. He should be glad to know whether the Author could give the figures for the con-

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densation in the various cylinders in the case of the jacketed-engine trials, in the same way as he had given them for the unjacketed ones (Table IV). So far as could be judged from the diagrams, the effect of jacketing on the amount of the missing quantity in the high-pressure cylinder was very small. Was it possible that there was considerable leakage past the low-pressure and intermediate jackets? The mean temperature of the liners of the cylinders would probably be, in these cases, considerably below the temperature of the main casting of the cylinder when the engine was at work. Consequently the liners would be relatively less in diameter and shorter than when the engine was standing, and such leakage would be exceedingly difficult to detect, because the only way apparently of observing it would be when the covers were off and the engine standing, at which time the liners would be as hot as the cylinder. Mr. Bryan Donkin's recent experiments [1] showed that the cylinder wall varied in temperature with the steam inside, and in the case of an engine in which it did not so vary there could hardly be condensation as in this case. The Author had spoken of the mechanical efficiency of the engine as being rather low, and he had explained it by pointing to the large valves. These large valves no doubt took a good deal of driving, but on the other hand they were made large so as to have as small losses as possible from passage friction, and therefore there was probably a corresponding gain in the cost of the indicated power. If it were tried to obtain the last ounce of indicated work, it would very likely be got with a loss of real efficiency. The power lost in moving the valves, however, was not wholly lost, as it probably, in great part, reappeared as heat imparted to the steam. The Author had, he thought, lost sight of this in arriving at his heat-balance. In the case of trial 56 only 76·5 per cent. of the indicated power was converted into work, so that 23·5 per cent. was lost in friction. It was not unreasonable to suppose, he thought, that with these large valves the internal friction from them and from the glands and piston-rings accounted for a large portion of the total loss. Assuming that 15 per cent. of the work was consumed in internal friction in the case of trial 56, about 462 thermal units would have to be added to the radiation account, because the condensing water would include that amount of heat received from the indicated HP. wasted in friction, which heat would in Table I, line 35, be reckoned twice over unless 464 heat-units were transferred to the radiation account. Would the Author state why he was so anxious for the indicator Mr. Willans.

[1] Minutes of Proceedings Inst. C.E., vol. xcviii. p. 250.

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Mr. Willans. to have a vertical position? He had added an otherwise unnecessary elbow in order to permit it to stand in this position; Mr. Willans preferred to place the indicator in a horizontal position on a vertical engine, and was not acquainted with any reason against such a position. With regard to all the methods of making the trials, they appeared to him to err on the side of complexity. The list of fittings, p. 160, was a truly formidable one, and was so different to the state of things in his own trials, where the boiler had only five fittings—a blow-off cock, a water gauge, a stop-valve, a safety-valve, and a feed check-valve—and only one steam-pipe with a joint at each end, that he should not be surprised at even greater discrepancies than those which had been found to exist. From the fact, however, mentioned on p. 174, that several trials could be made under similar conditions, the results not varying by more than 1 per cent., he concluded that the leak was a constant one, and this being so, it appeared extraordinary that it had not been located. Had the Author considered how serious a matter it was to send out fourteen students to make similar trials, all prepared to accept such discrepancies as inevitable and unaccountable?

He thought, in conclusion, that it might be interesting to compare the Author's results with a condensing trial of one of his own engines.[1] In this trial, which was not conducted under conditions favourable to the highest economy, but only for comparison with a trial made with the same number of expansions exhausting into the atmosphere, the consumption of steam calculated as feed-water pumped into the boiler was 15·2 lbs. per indicated HP.-hour. The mean admission pressure was 168 lbs. absolute, and the back pressure in the condenser 4·4 lbs.; he was sorry that he had not the steam pressure in the high-pressure steam-chest for exact comparison with the Author's trials, but he would take it at 5 lbs. higher than the mean admission pressure in the cylinder. The higher and lower temperatures were then 369°·5 Fahrenheit and 156°·7. The water theoretically required per indicated HP.-hour between these limits was 10·26 lbs., and the water actually used being 15·2 lbs., the thermal efficiency was 67·5, or almost exactly the same figure as that found in the Author's best trial, if the calculation was made in the same manner. It would be observed, therefore, that the thermal efficiency of the Author's engine could hardly be said to be unprecedented, as even an unjacketed engine worked with an eight-fold expansion had given the same thermal efficiency, when

[1] Minutes of Proceedings Inst. C.E., vol. xcvi. p. 255.

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calculated in exactly the same way, except that the Author's figures were based on water coming out of the hot-well, and on jacket-water estimated rather than measured, while his own were for water pumped into the boiler. He regretted that he was as yet unable to give figures for condensing-engines, using steam of as high a pressure as that used in the Owens College trials, but he hoped to do so shortly. Mr. Willans.

The Author's figures for unjacketed trials were absolutely worse than his own, although the pressure used was greater and the vacuum better, being 17·4 lbs., 16·0 lbs., and 15·9 lbs., for steam of 200 lbs. pressure and calculated from the hot-well discharge, against 15·2 lbs. for steam of 168 lbs. pressure and calculated from the feed-water. The Owens College engine had been worked, so far as he could ascertain from the diagrams, with a ratio of expansion in the jacketed trials of 20 to 25, and in the unjacketed of something like 15. In his own trial, quoted above, the ratio of expansion was only a little over 8, which would give some idea of the relative sizes of the engine. In his own case, too, the power developed, 37·66 indicated HP., was much smaller than in the case of the Author's best trials, 72·1 indicated HP.

Mr. Thornycroft.

Mr. J. I. THORNYCROFT said he would confine his remarks particularly to the subject of the engine being divided. His name had been mentioned in connection with this. He did not take much credit to himself, because the idea had cost him little trouble; but he thought it was an important thing in the engine, and he must say, though not in defence of it, that the engine being divided had not been sufficiently appreciated. It appeared to him that for the purpose of the engine under discussion it was a most suitable arrangement. Had the engine been constructed as some speakers recommended, it would have been made with a certain ratio of cylinders which could not be altered, and when it had been once tried and its performance ascertained, with the point of cut-off properly adjusted in the several cylinders, there would be little more to learn from the engine. But a compound engine, where the different parts could be run at different speeds, was one in which, if the engine were quite tight and perfect, the ratio of the cylinders could be altered, as far as possible; in fact, within almost inconceivable limits. He considered that in dividing the engine a great field for inquiry had been opened, because it could be made to represent any particular ratio of cylinders which it was desired to examine. Although the brake had been criticised, he thought it was an object of great interest. The Author had taken Mr. Froude's brake; but finding a defect

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Mr. Thornycroft. in it, which at the time was not quite understood, he had turned that defect into a very important and valuable feature. The vortex in that brake was liable to become hollow, and by allowing the water to run out he could regulate the power of the brake within quite sufficient limits for all that was required. When the Author asserted that the engine could be run at from any speed down to 20 revolutions per minute with the load on the brake unchanged, so as to be independent of the speed of the engine, he thought it showed a great success, and the ability to make experiments with three independent engines running at the same time must be, to a certain extent, attributable to the success with which he encountered that difficult problem, and made a brake which would answer his purpose. The trials under discussion included not only a trial of the engine, but a trial of the boiler, made simultaneously. If he differed at all with the Author it was in this, that the work was not all profitable. If the engine was taken alone, and the boiler was made large enough to supply an excess of steam, so that the pressure could be maintained at an exact and constant amount, it would be better than being always hampered with a boiler trial. He seemed to have done it with considerable success, for the results of the different trials were within 1 per cent. The great utility of the engine being divided was shown in the diagrams where the jacketed and unjacketed engines were compared. It was shown that if the engine was jacketed, to get the best results there must be a different ratio of cylinders. Mr. Bodmer had put forward a diagram (*Figs.* 3) showing that in the unjacketed engine he used a different ratio of cylinders, and that the jacketed engine was credited with more economy than it would be if it had a smaller ratio of cylinders. That argument might be turned the other way. It meant that had an unjacketed engine been obliged to run with as large a cylinder as a jacketed engine, the work in the unjacketed engine would have been worse than the Author had shown. He had examined the Author's Tables with great interest, and was certainly astonished to find that with practically the same load on the brakes he was able to alter all the conditions of running, the engines being adjusted so as to give good results in the distribution. He would ask the Author to be kind enough to give not only the good results, but the bad results he might get by using an improper ratio of cylinders for the particular cut-off, because it was not true that a number of useful experiments could be tried with a given ratio of cylinders by varying the cut-off. If the cut-off in the engines was varied, and it was then tried to put a quart of steam

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into a pint of space, or the other way about, it would not fit. He thought the Author had hardly done justice to the arrangement in what he had claimed, but he must thank him for the great pains he had taken to bring the matter to that state of perfection. Mr. Thornycroft.

Mr. W. B. BRYAN regretted that the Author had not measured the actual amount of feed-water at the trials, so as to compare it with the water discharged from the engines. In a triple-expansion engine which he was about to describe, and which was built by Richardson and Sons, of Hartlepool, in 1887, he had found the missing quantity of water to which the Author referred to vary approximately from 5 to 10 per cent. In endeavouring to compare the work of a triple-expansion pumping-engine belonging to the East London Water-Works Company with that at the Owens College, a few words of explanation were necessary. In each case he had taken the net HP.; in the Owens College engine the brake HP., in the pumping-engine the work actually done in the pumps. The pumping-engine was of the inverted marine type, and the cylinders were 18 inches, $30\frac{1}{2}$ inches, and 51 inches in diameter, with a 3 feet stroke. The high-pressure cylinder jacket was in circulation with the boiler; the intermediate pressure and low-pressure cylinder jackets discharged their condensed water through steam-traps to waste. The normal speed was 23 to 24 revolutions per minute. The plunger-pumps, three in number, were worked direct off the cross-heads. The cranks were set at 120°, and the crank-shaft was prolonged so as to actuate two 18-inch deep well pumps 190 feet below the surface, through two bell-cranks. There were eight bearings. The engine had a surface-condenser, the circulation water being that raised from the well, passing to the plunger-pumps. It would thus be seen that the engine had five pumps to actuate, besides a large number of working parts causing friction. The main steam-pipe was 75 feet long, and the steam-pressure 130 lbs. per square inch. The conditions, as regarded the amount of friction, were in favour of the Owens College engine. A number of trials had been made during the past two years, and the example given showed only average results. The feed-water, discharge from hot-well and jackets, were all from measurements in tanks, the tanks having been graduated from exact weights of water placed therein. All trials were made during the ordinary working of the engine. The power exerted was much less than the engine was designed for, on account of the water in the well having risen to such a height as to drown the pumps to the extent of 130 feet. All the trials were of eight hours' duration. In the ordinary working of the pumping-engine, Mr. Bryan.

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Mr. Bryan. just described, the feed-water and the water required to make up the missing quantity were recorded daily. The pump HP. was taken from the water-pressure resistance in the plunger-pumps and lift in the well-pumps, there being no deduction or

PUMPING-ENGINE.

	Lbs.
Feed-water, per hour	2,225·0
Water discharged from the hot-well, per hour . .	1,937·5
„ „ „ ip. cylinder jacket . . .	77·1
„ „ „ lp. „ „	41·1
Total water discharged, per hour	2,055·7
Difference between feed-water and water discharged per hour	169·3
Water condensed in hp. jacket returned to boiler. .	110·0
Pump HP.	140·0
Water per pump HP. discharged from hot-well per hour	13·84
„ „ passing through engine, including all jacket-water, per hour	15·47
Indicated HP.	160·0
Water per indicated HP. passing through engine, including all jacket-water, per hour	13·53

THE OWENS COLLEGE ENGINE.

Trials with cylinder jackets at boiler-pressure.

—	No. 44.	No. 33.	No. 56.
	Lbs.	Lbs.	Lbs.
Water discharged from hot-well and jacket-water per hour	516·00	793·20	977·40
Brake HP..	26·32	45·32	55·15
Water per brake HP. per hour	19·60	17·50	17·72
„ per indicated HP. per hour . . .	15·53	14·23	13·56

addition for any purpose. He might add that the efficiency of the engine in dividing the pump HP. by the indicated HP. for the whole of the trials worked out to between 87½ and 88 per cent. All these were calculated from diagrams taken from a day's running, the diagrams being recorded every quarter of an hour. He thought those results would show that the Author's engine must take the second place.

Mr. Charles E. Cowper. Mr. CHARLES E. COWPER thought the Author must be congratulated upon his excellent arrangement for making a variety of trials. Those who were engaged frequently on engine trials as a matter of professional business would envy the facilities which he had; but they would not envy him the large number of joints and

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Mr. Charles E. Cowper.

chances of leakage. With regard to the combined diagrams, Plate 4, Figs. 14, the members would agree it was very much to be wished that engineers should adopt some standard system of comparison. At the present time some half dozen different ways of combining diagrams were adopted, and it was very difficult to compare the results given in one Paper with those in another. The British Association had adopted a standard system of electrical measurement, and he thought that engineers might have a standard system for combining diagrams. He was much disappointed on reading the Paper to find that known leakages and unexplained losses interfered with the value of the results. The Author had some time ago contributed a Paper on the theory of the indicator,[1] in which he discussed very minutely the errors from inertia and momentum of the piston, and other slight imperfections of the instrument. Mr. Cowper accordingly expected, above all things, to find accuracy in this Paper, but he regretted that this quality appeared to be wanting in some of the results. After the first fifty-five trials it was found that some valve was leaking; he looked to the fifty-sixth trial and hoped it would be better, but found, p. 182, it stated that even in this case steam was leaking into the receiver at the rate of $\frac{1}{2}$ lb. per minute, which was rather serious on a total consumption of 12 lbs. Such a result might afford a very excellent text for a professor to lecture his students upon, but he thought that experiments in which leaks had not taken such an important part would have been more valuable as well as more suitable to place before that Institution. The Author spoke of the completeness of the system for checking results. Now what did he find in the Paper? In No. 41 he found the radiation was 421 units; the Author said that was 500 units too small. There was an "error of observation somewhere." He thought it would have been better had the Author thrown aside the results of that trial. But there was a more serious objection, to which other speakers had alluded, namely, measuring the quantity from the hot-well, and not from the feed in the usual way. The Author stated that the feed was carefully measured, with the result that it was from 5 to 10 per cent. greater than the discharge from the hot-well. That 5 to 10 per cent. unexplained loss seriously interfered with the results, and why the Author should charge that on the boiler he did not know. He thought that the boiler, tested before and after the trials, was more likely to be tight than the engine, which had twelve valves, nine piston and valve-rod stuffing boxes, seventy

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

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Mr. Charles E. Cowper. flanges and unions, and one hundred elbows, tees, &c.; altogether there were probably two or three hundred joints where leakage might occur. He would ask the Author whether he made independent boiler trials, and whether, in that case, he measured the water in the same way as in the engine trials, or whether he measured it from the feed-water, and omitted altogether the 5 or 10 per cent? Mr. Cowper had found by experiment that still water at the temperature of the hot-well would evaporate at the rate of 1 lb. per hour for each 5 square feet of surface. Of course, when there were waves, the evaporation would be more, and in the present case there were two or three tanks, but he did not know what the surface amounted to. It had already been mentioned that the hot air would come out saturated with moisture from the air-pump, and would so carry away some of the water, as well as some of the heat. With regard to the difference in temperature between the hot-well and the feed-tank, he found that the Author had given it as in some cases zero, and in some between 20° and 30°, and he could only imagine that there was here again some error of observation. Mr. Cowper had selected from the Tables in the Paper some of the most important figures (from a pure science point of view), and had drawn up a balance-sheet,

BALANCE-SHEET—TRIAL No. 56.

Dr. | *Cr.*

Line.	—	T. U.	Per cent.	Line.	—	T. U.	Per cent.
(44)	Steam in cylinders	14,154	81	(34)	Indicated work .	3,085	17·7
(47)	Jackets . . .	3,325	19	(33)	Rejected in condenser . . .	12,862	73·6
				(41)	Radiation. . .	1,176	6·7
				(40)	Loss from hot-well	356	2·0
(48)		17,479	100·0	(42)		17,479	100·0

placing on the debtor side all the heat in thermal units supplied to the engine, and showing on the creditor side the disposal of that heat, and had calculated the percentage in each case. He might say that he had taken those figures from Table I "without prejudice," that was to say, under protest, because he objected to the first basis of measurement from the hot-well. The result was 17·7 per cent. of indicated work, which was to be discounted by the 5 or 10 per cent. for error, which made 17 per cent.

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or 16. It was still a very good result. He might mention that some large pumping-engines by Messrs. Simpson and Co., which were compound, not triple-expansion, gave a little over 15 per cent. indicated work. He ventured to suggest that the adoption of a balance-sheet, in papers of that nature, would effect some saving of time on the part of members who perhaps would not care to read through three or four pages of tables, in the same way as a balance-sheet saved the time of those who would not read through three or four pages of accounts. Mr. Charles E. Cowper.

Professor OSBORNE REYNOLDS, in reply, said he should like in the first place to make an acknowledgment of a fact of which he had only become aware within the last two days. He had given the history of the brakes as far as it was within his knowledge. The first brake he made was one from a drawing of the late Mr. William Froude. In the copy of the Paper published by Mr. Froude, there was nothing about any arrangement for admitting water to the centre of the brakes through the veins. He had to thank Mr. J. H. F. Froude for informing him within the last few days that, although his father did not make any statement in his Paper of the necessity, in order to make his brakes work regularly, he had pointed out this necessity of supplying them through the central veins, in the discussion that followed his Paper, and had subsequently employed this method of supplying the water. At the same time, his father had considered the question, which had been suggested in the discussion, of regulating the power of the brakes by regulating the supply of water in them, and had come to the conclusion not to recommend it. These facts were entirely unknown to him when he wrote the history of the brakes, and he was glad to have the opportunity of acknowledging it before the Institution. Coming back to the immediate discussion, he would first beg leave to repeat what was stated in the Paper, and yet more fully stated in the abstract, and again in the few words he said after the Paper was read, namely, that the purpose of the investigation was to determine the exact manner in which the separate parts or organs of the steam-engine performed their several functions. With this view each principal organ had been designed so that it might perform its own special function as perfectly as possible, and at the same time allow of its performance being measured irrespective of the action of the other organs. This necessitated the serious hampering of the engines, taken as a whole, in any economic competitions; but this had been done without the smallest hesitation, any such competition being entirely outside the purpose for which they were intended. So, in making the trials, and re- Professor Reynolds.

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Professor Reynolds. ducing the results, every effort had been made to obtain and express the results of each organ separately. He felt it necessary to re-state this, because, by the greater part of those who had taken part in the discussion, this purpose had been either discredited or ignored. Professor Unwin, however, not only acknowledged the purpose, and expressed his approval of it generally, but paid him the great compliment of saying that in several of the more important particulars he had been working in the same direction. It was particularly gratifying to hear him say that he had been working on the same lines in reducing the diagrams. It was the purpose of a reduced diagram to show in detail how far the primary organs of the engine, pistons, cylinders and valves, &c., had performed their part in rendering effective such steam-pressure as was possible under the conditions of working, and further, to separate as much as possible the sources of inefficiency in these organs. Until some such system as that adopted in this research became general, engine-trials would fail to serve what should be their most important purpose, that of showing the immediate sources of inefficiency. These diagrams did not seem to have been well understood by some of those who took part in the discussion, particularly by Mr. Willans, who attributed to a late cut-off what was really due to the setting back of the diagram, so that the ideal compression-line came to the point of zero volume, and appeared to think that such was the shape of the actual diagrams. How far this was the case would be seen by the fac-simile diagrams appended to his reply. He was surprised that Mr. Willans, who had made investigations with the special object of determining the amount of cylinder-condensation, should have had nothing to say about these diagrams, and the manner in which they represented, for every part of the stroke, what Mr. Willans had only given at one or two points. As an illustration of the importance of such diagrams, in which the actual diagrams were set back by the ideal compression-curve, and then compared with the saturated curve, he might point out that had Mr. Willans adopted this plan in his search for the missing quantity, he could not have fallen into the mistake which he had made in estimating the steam accounted for in the cylinder. By taking the apparent steam in the cylinder, less that in the clearance at the point of admission, as the steam accounted for, instead of the apparent steam, less that in the cylinder and clearance at the point of compression, he had taken all the steam condensed during compression as accounted for, and thereby consistently under-estimated the missing quantity, which was the main object of his search. In the high-pressure cylinders,

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where the compression reached nearly to the pressure of admission, the condensation during compression would be very considerable compared with the quantity which had been found missing at cut-off. Again, had Mr. Willans used the mean diagram to show the missing quantity, instead of trying to determine it for one point, and that the point of cut-off, it would have appeared that, in the cases of the higher speed, at this point the missing quantity was apparently less on account of the inertia of the indicator-piston. He hoped Mr. Willans would now be induced to make another reduction of his indicator-diagrams by means of the mean diagram, after the method described on p. 179. In this case he had no doubt Mr. Willans would *find the missing quantity at cut-off in* his high-pressure cylinder increased some 6 or 7 per cent. at the higher speeds, and that the cylinder-condensation varied with the square roots of the speed. The trials of Mr. Willans appeared to have been so carefully made, and the engines themselves so admirably adapted to test the effect of speed on cylinder-condensation, that it would be a great pity if the results, instead of furthering knowledge, were allowed to remain a stumbling-block for want of accurate reduction. Amongst others, Mr. Willans had asked how he explained the small effect of the jacket on the condensation in the high-pressure engine. In spite of the fact that Mr. Willans stated that he had carefully read the Paper, he must refer him to pp. 184 and 185, where he had carefully discussed this point, and given an explanation. The importance of this explanation was acknowledged by Professor Unwin, who, however, pointed out what he thought was an error in the calculations as to the gradient of temperature necessary to allow the heat to pass through the thickness of the liner in not taking account of the periodic character of the absorption in the cylinder. This, however, was not so, as Professor Unwin would readily see, for the periodic effect extended to such a small distance into the liner, that it had no more effect on the gradient of temperature in the thickness of the liner, than the diurnal demand for water on a service-reservoir had upon the gradient of mean flow from the storage-reservoir. Mr. Bodmer also discussed this point, commencing by questioning the necessity for a flow of heat, and ending, after correspondence, by recognizing such necessity. The examination of this flow of heat through the walls of the cylinder, and the consequent influences on the efficiency of the steam-jackets of the thickness of the walls, and the state of steam as to excess of temperature and purity from admixture with air, was itself one of the objects he had primarily in view during the design and construction of the engine, and

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Professor Reynolds. afforded a fair illustration of the character of the others. The accomplishment of this object, so far as the clear demonstration of the importance and character of these influences, was a result which, even if the research were carried no further, would constitute a sufficient scientific reward for the very considerable undertaking in instituting and working these engines. He hoped, however, to go further, both in extending the knowledge of these influences, and in clearing up other doubtful questions. The excess of heat in the discharged steam over that it possessed at release, was another important matter that had been examined and established in these trials. This point, however, had not been entered upon in the discussion. Mr. Beaumont's comments were merely an attempt to show that a discrepancy between the measured and theoretical heat discharged in one of the trials was owing to an error in estimating the heat theoretically discharged in all the trials without jackets. Mr. Beaumont overlooked the fact that by altering the formula to suit the one anomaly, he would bring an equal anomaly into both the others. In his formula, Mr. Beaumont put a wrong construction on the total heat of evaporation, overlooking the fact that this included the external work done during evaporation against the pressure of the surrounding steam. Coming to the criticisms on the engines, and method of conducting the trials, he would first thank Mr. Thornycroft for his appreciatory remarks generally, and particularly for so clearly pointing out the advantage to be derived from the separate brakes, which remarks relieved him from the necessity of replying to such criticisms as had been made by other speakers, except to acknowledge Professor Unwin's appreciation of these advantages. Respecting the indicating-gear, he acknowledged with pleasure Professor Unwin's kindness in showing him his laboratory, and although he had no recollection of the indicating-gear doubtless saw it, as he remembered being struck by the single indicator with alternative pipes. He willingly admitted that if there were any points of resemblance in the two gears that were not common in other gears, he might unconsciously have adopted them. In respect, however, to the specialities in the gear he had described, and which alone seemed to make it worth describing, they, as far as he could see from *Fig. 2*, were entirely absent from Professor Unwin's gear. He had not—(a) the stiff steel wire, held straight and tight by a spiral spring, in such a position that both indicators could take motion from it with equal, and the shortest possible, lengths of cord; (b) the buttons on the wire, and the claw-hooks, which constituted the principal feature of the gear, allowing of hooking up at any speed with equal facility, and

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this without adding sensibly to the weight of the moving parts; (*c*) the simple method of attaching the gear to the indicator-cocks without other fixing. These features rendered the gear the simplest gear to work with two indicators that he knew, and specially adapted it for high speed, it being as easy to work at 600 revolutions per minute as at 100. It was no object of his to disparage Professor Unwin's gear, which seemed excellent as regarded facility for a single indicator at low speeds. It did not, however, seem well adapted for two indicators, and if run at a high speed the weight of the spring and double pulleys, in the middle of a long cord, would seriously disturb the motion of the drum; while if wire were used, it would frequently break from bending, or if thin from overloading. The defect in the rider-valve mentioned, p. 183, in connection with the discussion of any possible leakage that might explain the missing steam shown in the diagrams, was only revealed by a most rigorous examination of the diagrams, and its effect on the distribution of steam and the efficiency of the diagrams was insensible, being only a slight wire-drawing for a little way from the cut-off. The leak through the steam-valve into the receiver No. II, in trial 56, was an incident of the trial, and only discovered in virtue of the exceptional facilities offered in these engines for observing the condition of each part separately. That these two slight defects should have been the only defects found in the six trials, notwithstanding the closeness of the watch kept on every part, was the very strongest assurance of not only the good, but the very high condition of the engines. Where no defects had been recorded, it might be that things had been perfect, but it was infinitely more probable that defects had not been looked for. In careful scientific work, it was a rule to record the largest errors, as affording definite information as to the degree of accuracy. This rule he had followed. As to discarding any trial on account of these defects, which were revealed after or during the trials, this would seem very much like picking out favourable trials, to prevent which it was made a rule in this investigation to record every trial with its defects. The loss of steam or water was a defect common to all steam-engines and boilers, though the extent of this loss was seldom known, even approximately. Indeed, it was only in the case of surface-condensing engines that it could be measured at all, and then the apparent loss was liable to be less than the actual loss on account of water leaking back into the condenser. It was almost impossible to determine the leakage of a surface condenser working under ordinary conditions. Like a boiler it might be

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Professor Reynolds. tested cold and found tight, yet leak badly when hot. The Owens College engines afforded exceptional opportunities for testing the condenser. No water but what passed through the engines was admitted to the condenser; while, not only was this water always measured, but also the heat given up by this water; and hence the heat showed how far the water entered as steam or leaked in as water. This check might not be within 3 or 4 per cent. of the water passing through the engines, so that with 20 lbs. a minute it would still be possible to overlook something like $\frac{1}{2}$ lb. per minute. It was, however, possible to keep the condenser under the same conditions as regarded pressure and temperature when passing 1 lb. per minute as when passing 20 lbs., and then the heat checked the water to $\frac{1}{20}$ lb. per minute. Several trials of this kind had been made, and had shown no determinable leak whatever. In this way the actual difference in the feed and the discharge from the hot-well was shown to represent, not only lost water, but all the water lost. As regarded the question how far 5 or 10 per cent. was a usual loss of water, it might be noticed that the statement of loss given in the Paper referred only to triple-expansion trials with 200 lbs. steam-pressure, conditions on the extreme limit of practice, so that there was every probability that the loss of water in these engines would be beyond the ordinary. He had heard from many engineers experienced in working marine engines that 5 per cent. was the minimum loss at high-pressure. Mr. Bryan stated that he had found the lost water from 5 to 10 per cent., and gave an instance in which the loss was 7·7 per cent. with about 130 lbs. of steam. At the time of writing the Paper the loss occurring at lower pressures had not been determined; it had since been done, and it was found that up to a pressure of 90 lbs. the loss did not exceed 10 lbs. an hour, and hence the loss of from 40 to 50 lbs. an hour, which occurred at about 200 lbs. pressure, was to be attributed to the pressure. In the matter of liability to leakage from the engines and boiler, these engines were under much the same conditions as marine engines; but in respect of the water-heater and of the complicated system of pipes for supplying the jackets and under full pressure of steam, they were at a great disadvantage. On the other hand, very exceptional efforts had been made to render these engines tight. Every leak was stopped as soon as it was seen, and the boiler, economizer and the entire jacket system were tested under cold-water pressure. On the last occasion when this was done, pressure was kept up at 200 lbs. per square inch for two hours with 20 lbs. of water. Nevertheless, during the engine-

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trial on the next day, 200 lbs. of water were lost in four hours. That the bulk of the lost water escaped as steam or water, through apertures in the envelope subject to high pressure and high temperature, seemed certain; but where these apertures were, whether numerous minute leaks or one larger leak, had not been ascertained. That no sensible quantity of water left the jacket-pipes was certain, these pipes being all well above the floor. If the pipes, therefore, leaked water, this water was evaporated by the heat of the pipes, the heat being replaced by the condensation of steam in the pipes, &c.; in this case, the quantity of water which returned from the jackets was not affected. The water that escaped from the glands of the high-pressure and the intermediate engines had been definitely shown to be very small. The glands had always been worked tight, namely, screwed up on the least suspicion of a leak. In some of the trials special precautions had been taken. In the trial already mentioned as following the cold-water test the glands were treated as usual, but the packing was rather old, so that when the 200 lbs. of water were lost, to be certain of the glands they were repacked, and another trial was made with the glands screwed and the rods kept cool by continuous tallowing. The glands were several times tested with cold polished steel, and no dew was found. The loss of water was 218 lbs. in the four hours. It had been suggested that the lost water had been discharged as vapour from the air-pump, or had been evaporated from the tank. If this were so there would be the same loss in low-pressure trials as in high, for the condenser and tanks were under the same conditions. The only tank exposed was the feed-tank and the tip-can, in which the temperature of the water was within 2° of that in the hot-well. While considering that the feed-water could not contain more than one-hundredth of its volume of air at atmospheric pressure, the air discharged from the air-pump must be so small that it could not carry away any sensible moisture. He had, however, made arrangements to measure the air and vapour from the air-pump; these arrangements were not quite complete, but they enabled him to say that the volume of air and vapour did not appreciably exceed that of the one-twentieth part of the water discharged. He had replied at length to the remarks on this loss of water, because, for one reason, it seemed to have been the opinion that it had been rather slurred over. He hoped what he had now said would be sufficient to show that, so far from such being the case, it had been pursued by every means at his disposal. Considering that such a loss of water was a matter of general occurrence with high pressures and

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Professor Reynolds. surface-condensers, he considered its investigation to be of very great interest and importance. If the water was lost as steam, such a loss, if a necessary accompaniment of high pressures, must largely diminish the economical advantage, which would otherwise be realized. While the revelation, by the measurement of the feed- and hot-well discharge, of a loss of more than 5 per cent. of the feed-water, which so far defied, not only all usual means of detection, but every means that he had been able to apply, must, until it was cleared up, cause grave doubts in accepting any conclusions as to the behaviour of steam in the engine, based upon trials in which the feed alone had been taken as showing the quantity of steam passing through the engines.

The severe criticisms of Mr. Willans seemed to demand some notice, which, however, had been made difficult by the attitude which he had taken. He began by disputing the Author's motives for these trials, and stated that "the Author wished to beat the record," and perverted a passage in the Abstract, in which any such motive was disclaimed, into a claim to have beaten the record. The manner in which these engines had been hampered with excessive radiating-surface and shafting, should have shown that general economy had been a secondary consideration. But this did not suit Mr. Willans; he was determined to compare these trials with his own, and so placed Professor Reynolds in the position of a competitor, in order that he might ignore the otherwise very obvious reason for the methods of reduction which had been adopted, and imply that they had only been adopted to place the economic results in a favourable view. This seemed neither courteous nor fair, and he must entirely decline to take the position of a competitor for economic precedence, although, were economy any object, it would be somewhat flattering to find Mr. Willans, who clearly considered himself the champion of the record, so anxious for his position. It was perfectly open to make any economic comparison so long as the results were dealt with accurately, and no misleading impression of them was conveyed. To begin with, Mr. Willans re-stated the Author's results, discounting the economic advantage (about 5 per cent.) which was obtained by means of the water-separator returning the water condensed in the jackets to the boiler, measuring it on its way, which arrangement Mr. Willans in another place described as admirable. Not content with this, he attacked the method of estimating the jacket-water by measuring it accurately over an interval of five minutes every half-hour, because "*only one-fifth* of the jacket-water had

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in this trial gone through even a form of measurement." "A form of measurement!" The water was collected in a carefully calibrated vessel, under full pressure of steam; while in place of this Mr. Willans proposed to take it out into the open air and weigh it, regardless of the fact that one-fifth part of the water would immediately pass off as steam. The measurement of jacket-water had always been a difficulty, and those having experience of this would, he felt sure, appreciate the advantage of this separator. There was no necessity to limit the intervals of measurement to five out of every thirty minutes; it had sometimes been measured for twenty-five out of the thirty minutes, yielding similar results for similar trials. In some trials, all the water had been drawn off from the blow-off cock, keeping the water in the gauge at a certain level, so as not to allow steam to pass, the water being caught in buckets and weighed, a weighed quantity of cold water having first been put in the buckets to condense the steam. This was an accurate, though laborious method, which could not be used without the water-gauge. In this way both the gauge and the method had been severely checked. If the working of engines was so regular that indications for a period of less than half a second every half hour were deemed sufficient, surely the definite measurement of the jacket-water for periods of five minutes every half hour might pass without question. Mr. Willans attempted to discredit the usual method of reducing diagrams by means of ten breadths. After asserting that "the measurement could only be accurately done by means of a planimeter," Mr. Willans proceeded to say that if the area of the low-pressure diagrams (Plate 4, Fig. 14, trial 33), were taken by ten breadths, it would be at least 5 per cent. greater than if taken by the planimeter. It was difficult to believe that this was merely a guess; yet, if Mr. Willans had actually compared the measurements, he must have effected observation errors of at least 3·8 per cent. The figure was a sufficiently definite one, and as the essential error of the method of ten breadths depended solely on the figure, and not on the scale, and the area of a similar figure on a sufficiently large scale could be definitely and accurately measured, say to 0·02 per cent., and also by ten breadths to the same degree of accuracy, the error of the method of ten breadths, as applied to this figure, admitted of definite determination. Having in his possession the original mean diagram, of which the scales were 10 lbs. to an inch and 2 cubic feet to an inch, from which the diagram in question was a copy, reduced first by the pentagraph and then by the engraver, he had the large diagram measured, so as to obtain its mean breadths to less than

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Professor Reynolds. one-thousandth part, as well as by the method of ten breadths.[1] He thus found that the excess by the method of ten breadths was 1·2 per cent. It might seem that these were very small errors to dispute about, but they were very important. If the ten-breadths method gave ordinary diagrams 5 or even 3 per cent. too large, it would be unfit for use, and all the results hitherto obtained by its aid would be subject to this error. Its standing error was, however, nothing approaching 5 per cent., not even 1 per cent. The diagram selected by Mr. Willans was not an indicator diagram, and had a figure which entailed a much larger error by the ten breadths than actual diagrams, such, for instance, as those in the Appendix to this reply, would entail. The planimeter was an extremely useful instrument, but still it had errors of its own, particularly when used in certain positions, positions in which it was very apt to be used. But these standing errors were not the only ones in measuring diagrams; the errors of operation, which increased as the size of the diagrams diminished, were apt to take a particular direction. To make a free-hand tracing with a vertical pencil of a diagram, not more than $\frac{1}{4}$ inch broad, and with a line something like 0·01 inch thick, and to maintain the true area to at least 2 per cent., required conditions of light and an

[1] To measure the diagram, its length was divided into 20 equal parts, and the breadth at each of the points of division was measured, also the breadths at the ends. These measurements commencing, from the left end, which point was indicated as zero, were given in order under the figures 0, 1, 2, 3, &c.

0	1	2	3	4	5	6	7	8	9	10
0·15,	1·44,	2·05,	2·00,	2·03,	2·05,	2·03,	1·95,	1·70,	1·44,	1·25,

11	12	13	14	15	16	17	18	19	20
1·02,	0·92,	0·82,	0·76,	0·70,	0·63,	0·565,	0·51,	0·475,	0·18.

Mean breadths were thus obtained—

(1) Mean of the alternate ten breadths . . . . . . = 1·2460
(2) „ half-end breadths and remaining breadths, Trapezoidal rule . . . . = 1·2045
(3) Mean by Simpson's rule . . . . = 1·2321

If (1) was too large (2) was too small, and the probable true mean was given according to Simpson's rule, by adding twice the first to the second and dividing by three. To check the accuracy of this estimation, as the end trapezoids were the only ones in which there was irregularity, the mean breadths of these were measured by the planimeter. These were for the first 1·326, and for the last 0·430. These added to the sum of the breadths from 2 to 18, taken out by Simpson's rule gave the mean breadth = 1·2312, which checked the other to 0·08 per cent.; then subtracting these from (1) the excess of the method of ten breadths = 0·0148 and 0·0139, or 1·2 per cent.

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amount of skill extremely difficult to attain, while the ten breadths could, by the aid of an engraver's glass, be measured to almost any degree of accuracy. Another respect, in which Mr. Willans' statements were calculated to cast discredit on the method of reducing the results of these trials, was that of the thermal and the theoretical efficiencies. In the Paper, thermal efficiency had been used in a somewhat restricted sense, compared with the somewhat loose manner in which it was often used. Its application was restricted to the special organs in which the steam operated to perform work; thus it was used to express the ratio which the heat converted into work bore to the heat received into the engine proper, instead of confusing this heat with that lost by radiation, or otherwise lost. This restriction was defined in the Paper and in Table II, "Thermal efficiency as given by heat discharged in condensing water." The purpose of so restricting it was obvious, namely, to keep the results depending on the action of these organs separate from other and quite independent organs. Mr. Willans objected to this step towards localizing the imperfections of the engine, apparently because it had not been previously taken. He insisted on again mixing up the radiation with the internal action of the engines, as if it were wrong to separate them. This, however, was not so much the point. The restricted thermal efficiency depended on two distinct actions, one the action of the steam under the conditions aimed at in the engines, the other the action of the engine in fulfilling these conditions. Thus the thermal efficiency was the product of two efficiencies, one called by Rankine the efficiency of the steam, and commonly "theoretical efficiency," the other as yet without a name, but determined by the ratio which the thermal efficiency bore to the efficiency of the steam, and expressed as "percentage of theoretical efficiency." It was, if strictly determined, the most important quantity of all the results of the trials, as it expressed the efficiency of the primary organs of the engine in performing their functions, while, if incorrectly determined, it was simply a source of confusion. This might be called the physical efficiency of the engine. To obtain correctly this physical efficiency, it was necessary to find the thermal efficiency, free from all confusion by extraneous circumstance, and also the efficiency of the steam according to the conditions under which it was used. In these trials with jackets the conditions aimed at were clear. Every effort had been made to keep the steam saturated while expanding from the pressure of admission to that at which it was released, to be discharged from the cylinder against the pressure

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Professor Reynolds. of the condenser, the release-pressure being controlled by the load put upon the engine. Now, the efficiency of dry saturated steam under such conditions had been known for more than thirty years, and there were no two opinions about it. Rankine and Clausius has given formulas which led to the same results. These theoretical efficiencies of the steam (Table III, p. 194), had been obtained directly by interpolation in Rankine's tables, according to Rankine's formula, given in Article 287 of his "Manual of the Steam-engine and other Prime Movers."

$$\text{Efficiency } \frac{U^1}{h} = \frac{U_1 - U_2 + v_2 (P_2 - P_3)}{U_1 - U_2 + H_2 - h_4}.$$

Mr. Willans, entirely ignoring the scientific importance of these terms, and observing only that, by taking the efficiency of steam under conditions which were not those aimed at in the trials, he could lower the percentage of theoretical efficiency, objected to the Author taking the correct efficiency for steam, and constructed a table in which he placed the theoretical efficiencies for the six trials as given in the Paper, and above these, three rows of theoretical efficiencies, all decidedly higher than the Author's, and differing from each other in an ascending scale, as if to prove, by mere weight of evidence, that the efficiencies given in Table III were too low.

The first two of these rows of efficiencies, "the theoretical efficiency of Carnot's cycle," and "the theoretical efficiency of Clausius' perfect engine," were clearly inapplicable to the trials in which the conditions did not approximate to Carnot's cycle any more than they did to those of Clausius' perfect engine. The third row of efficiencies was headed: Theoretical Efficiency of Jacketed Engines: Heat utilized per lb. of steam $= 1{,}438 \log_e \frac{A}{B} - 0 \cdot 7 (A - B)$. Standing thus directly over the results which the Author had given, obtained from Rankine's formula, there could have been but one intention, namely, to convey the idea that the Author had misused Rankine's formula and underestimated these theoretical efficiencies. What then was the fact? Mr. Willans had given a formula, emphatically not Rankine's, had headed the results calculated by this formula "Percentage of Theoretical Efficiency of Rankine's Jacketed Engine," and placed them immediately over the "Author's percentage of theoretical efficiency" (lines W and X). Rankine had given no rule for the theoretical efficiency of a jacketed engine, but he gave a formula for the

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efficiency of dry saturated steam, which was the same as was now commonly meant by the theoretical efficiency of the jacketed engine. He also had given a formula for the heat utilized per lb. of dry saturated steam, which was—

$$U' = a \log_e \frac{\tau_1}{\tau_2} - b\,(\tau_1 - \tau_2) + v\,(p_2 - p_3),$$

in which the suffix $_1$ referred to admission, $_2$ to release, and $_3$ to the condenser. It was this formula that Mr. Willans had used in concocting his own; he had put A for T_1, B for T_2, and left out the important final term. Nor was this all: in calculating the results Mr. Willans had, for B or T_a, the temperature of release, substituted the temperature of the condenser, which in these trials was some 60° Fahrenheit less than the temperature of release. The Author forbore all comment on this proceeding on the part of Mr. Willans, merely quoting some of Mr. Willans' statements as to his own methods of proceeding and those of the Author. "He," Mr. Willans, "had therefore, from the Author's data named above, re-calculated all his figures as to efficiency in the standard methods." "He (the Author) compared what he called 'thermal efficiency' with something which did not, to Mr. Willans, represent the 'thermal efficiency' of any engine or cycle whatever." "Looking at the matter broadly, it seemed to Mr. Willans that the Author had increased the efficiencies in the various trials by omitting factors always of great importance, and in this case of a special importance, and that he had then compared these inflated results with a new standard for which there was no sound reason."

Having now replied to all the more important criticisms, he would only say that he had offered the results to the Institution, believing them to be of interest and importance; that he regretted his inability to render the Paper more easily intelligible, and that he had every confidence its importance would be more and more realized as it became better understood. And in thanking the meeting for the kind attention paid to the Paper, he would express the hope that some good would be found to come out of it.

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Professor Reynolds.

TABLE.

Number of the trial	44	33	56	41	35	40
Lbs. of coal put upon the fire during the trials, including 14 lbs. of wood taken as 7 lbs. of coal	321	487	411	365	521	509
Lbs. of coal deducted for fuel left unconsumed at the end of the trials	21	21	21	21	21	21
Lbs. of coal consumed during the trials	300	466	390	344	500	488
Lbs. of water discharged from the hot-well supplied to the boiler at the temperature of the feed during the trials	2,095	3,595	3,000	2,790	4,266	4,308
Lbs. of water added at the temperature of the feed to make up the feed during the trials	..	288	..	..	278	273
Lbs. of jacket- and priming-water returned from the water separator by gravitation to the boiler at the temperature of the boiler during the trials, being the product of the rate per minute given in Table I, line 46, and the duration of the trial in minutes	944	1,365	960	595	725	646
Lbs. of total water supplied to the boiler during the trials, including water lost	..	5,248	..	..	5,269	5,227
Lbs. of water supplied to the boiler during the trials, excluding water lost	3,039	4,960	3,960	3,385	4,991	4,954

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WHITWORTH ENGINEERING LABORATORY, THE OWENS COLLEGE, MANCHESTER. Professor Reynolds.

December 4, 1888. Name, C. S. THOMSON. Engine No. II.
Indicator D. Spring 60.

No. of Diagram.	Time.	Boiler Pressure.	Receiver Pressure.		Greatest Indicated Pressure.	Least Indicated Pressure.	Release Pressure.	Mean Effective Indicated Pressure.	Revolutions per Minute.
			No. –.	No. –.					
	A. M.								
1	10.0	193	59·0	9·0	59·0	9·0	17·0	30·7	250
2	10.30	192	58·0	8·5	58·0	7·5	16·0	29·35	240
3	11.0	180	58·0	7·5	57·0	8·5	16·5	29·0	240
4	11.30	179	56·0	7·5	55·0	7·0	16·0	29·45	222
5	12.0	184	58·0	7·5	57·5	7·0	16·0	30·8	240
	P.M.								
6	12.30	185	61·0	8·0	58·0	7·0	12·0	30·6	230
7	1.0	180	58·0	8·0	58·0	9·0	17·0	28·35	260
8	1.30	190	56·0	8·0	57·0	9·0	17·0	28·8	260
9	2.0	185	60·0	8·0	57·0	5·0	13·0	29·3	248
10	2.30	184	58·0	7·0	56·0	8·0	16·0	28·6	214
11	3.0	185	59·0	7·5	59·0	8·5	17·5	29·7	230
12	3.30	181	58·0	7·0	60·0	7·5	16·0	31·1	200
		2,218	699·0	93·0	691·5	92·5	190·0	355·75	2,834
		185	58·25	7·7	57·6	7·7	15·8	29·64	236

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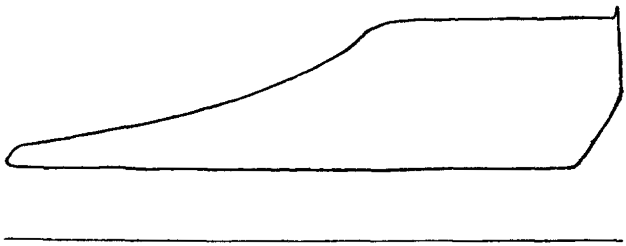
Professor Reynolds.

TRIAL 33.—FULL-SIZED DIAGRAM XII

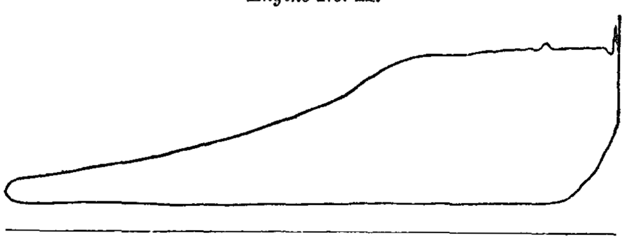
FRONT END.

Engine No. I.



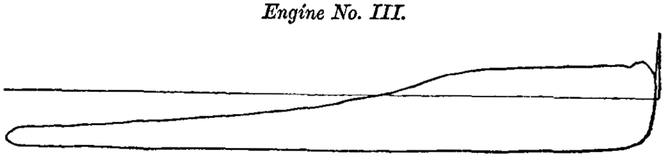
Revolutions 200. Spring 150 lbs.

Engine No. II.



Revolutions 230. Spring 60 lbs.

Engine No. III.



Revolutions 252. Spring 40 lbs.

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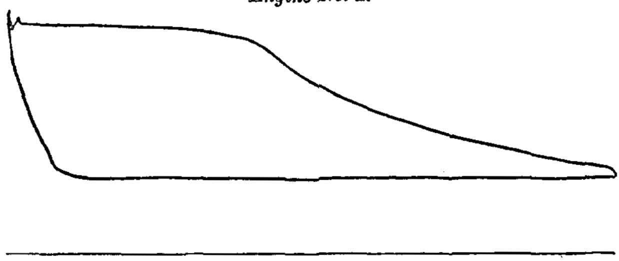
TAKEN at 3 P.M., DECEMBER 4, 1888. Professor Reynolds.

BACK END.

Engine No. I.

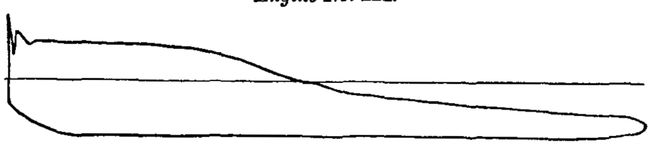


Revolutions 200. Spring 150 lbs.

Engine No. II.

Revolutions 230. Spring 60 lbs.

Engine No. III.



Revolutions 252. Spring 40 lbs.

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