Discussion.

The President. The PRESIDENT moved a vote of thanks to the Author for his Paper.

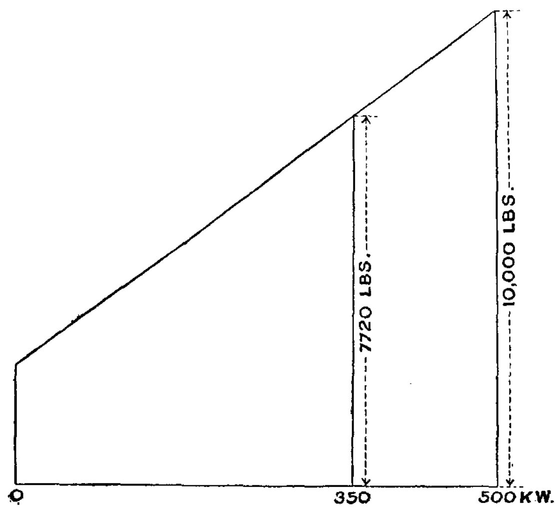
The Author. The AUTHOR desired to add an explanation of two points. In *Fig. 1* the piping was shown as arranged on the ring system, and on p. 2 the abolition of the ring system was mentioned. The piping was actually as shown in *Fig. 1*, but was shut off between the points X, X. The second point was that the effect of the employment of feed-heaters, with a view to reduce the temperature, had been tested. The tests had been somewhat rough, being based on the quantity of coal, the quantity of water evaporated, and the number of units generated. The conditions of working had been averaged. The boilers had been worked week after week, alternately with and without feed-heaters; the duration of each test had been fixed by the combustion of a definite quantity of coal, and the results obtained had shown that no advantage was to be found on either side. The actual figures obtained were 6·05 and 6·07 lbs. of coal per unit with the heaters, and 5·95 and 6·09 lbs. of coal without the heaters. The figures were higher than those indicated in the week of test, due to the fact that the heater-tests had been carried out in the summer months under reduced loads, and consequently impaired economy.

Captain Sankey. Captain H. RIALL SANKEY considered that the Paper was full of information of a very valuable kind. He much admired the care and trouble the Author had taken, not only in making the measurements but also in checking them. There were many points on which remarks could be made, but as time was short he would confine himself to two matters. On p. 8 the Author referred to the selection of fuel according to its calorific value. It was probably true that it was difficult to obtain contracts based on the calorific value of the coal; he had not much personal experience in the matter, but he could quite imagine that in smaller undertakings the collieries would have the whip-hand and would not care to guarantee the calorific value. That the qualities of coal for producing steam in a boiler were not proportionate to its calorific value was, he thought, not surprising; in fact, it would be surprising if they were, because a great deal of the value of coal in a furnace depended on its physical nature, on the amount and quality of the ash it contained—which, however, also affected the calorific value—on its capability of forming clinker, and on the kind of clinker it formed. Hence, notwithstanding the importance of obtaining the calorific value, it was necessary to make a test in a boiler, as the Author said. The Author seemed to think that sampling the coal was a difficult matter, and that it was not likely to lead to accurate results. It was not an easy matter, and required a great deal of care; but when it was done with care the results were really satisfactory, and he thought the inaccuracy of the determination would be less than the inaccuracies of many of the engineering measurements recorded in the Paper. But having obtained the sample, the Author still seemed to think that the chemical determination of the calorific value was doubtful because of the smallness of the sample. He was not a chemist, and was not there to defend them, but the fact was that engineers did not quite realize the extreme accuracy that had to be secured in making chemical analyses. He and many other engineers knew what such work meant, and if the necessary care were taken, results were obtained which were reliable. The Author referred to a moisture-test made on a very large scale. Captain Sankey would like to ask whether moisture-tests had also been made on small samples, and, if so, how the results agreed with the large-scale test. Referring to the Author's method of determining the consumption of the main engines, namely, by determining the amount of heat rejected by the engine, in order to get this amount of heat, the assumption was made that the steam was dry at the exhaust; but evidently the Author had great doubts as to the validity of that assumption, because he remarked (p. 22) that the gauges might not have been telling the truth—which was often the case—and also that the temperature might not have been measured accurately. The moisture in the steam issuing from the exhaust would be in the form of a mist, and it was quite easy to imagine that close to the walls, where the thermometer was, the moisture might have been evaporated and superheated. He noticed that the walls were 20° higher in temperature than the saturated steam, so that the apparent superheat there was not difficult to explain. The total heat of the steam going into the engine was 1,245 B.Th.U. per pound, assuming a specific heat of 0·545 as taken by the Author. The steam at exhaust had an absolute pressure of 1·43 lb. per square inch, and it would be found that dry steam at that pressure had a total heat of 1,117 B.Th.U. The difference between those two quantities, 128 B.Th.U., would be the heat that was being removed from each pound of feed according to the Author's assumption. The greater portion of

Captain Sankey.

Captain Sankey. that—almost the whole of it—had been converted into indicated horse-power; a certain quantity had been radiated, but with the particular engines used a certain quantity had been added by conduction. For the present purpose he thought it might be assumed that those two quantities of heat were equal, and that in reality the 128 heat-units per pound had been converted into work. Inasmuch as a horse-power-hour was equal to 2,545 B.Th.U., if that figure was divided by 128 it gave the feed, which came out to 19·8 lbs.: for engines working on a condenser and with a fairly high superheat, that was obviously a very high consumption. Further, referring to p. 37 it would be found, by a short calculation, that the feed per electrical

Fig. 11.



horse-power-hour was 22 lbs. If the two figures he had mentioned were compared, the combined efficiency of those engines was obtained, namely 90 per cent. He had never heard of a combined efficiency of that amount; even 85 per cent. would be very good. He next wished to show the very marked effect of assuming the presence of a very slight amount of water. Presuming that the steam, instead of being dry, had 3 per cent. of moisture in it, i.e., was steam of 97 per cent. dryness, the heat rejected in the exhaust would be found to be 1,083 B.Th.U.; the difference between that and the total heat at admission, namely, 1,245 B.Th.U., was 161 B.Th.U., the feed became 15·7 lbs. and the combined efficiency 69 per cent.; so that the very small amount of 3 per cent. of wetness in the steam altered the calculated steam-consumption from 19·8 lbs. to 15·7 lbs., and the combined efficiency from 90 per cent. to 69 per cent. He thought that showed that the method described was one that could not be relied on. The Author said there was no other way of obtaining the steam-consumption except by deducing from the total feed that required by the auxiliaries, and he then found there was a balance unaccounted for. If he had the test-sheets of the engines and also knew the average load on the engines during the number of hours they had been at work, Captain Sankey thought a much nearer approximation to the consumption could be obtained. To explain this method, *Fig. 11* gave the Willans law for an average 500-kilowatt set. The total feed at full load was 10,000 lbs. per hour, and if the average load was 350 kilowatts, the feed per hour would be about 7,720 lbs.: the test-sheets of the engine, however, would give the figure accurately. If that were multiplied by the number of hours that the particular engine had run, and in a similar manner a total were made for all the engines during the whole of the test, the figures for the feed-water would be given much more accurately than in the manner the Author had adopted. On the other hand, it might mean a large amount of labour, and possibly that had prevented the Author from adopting this method. The Author also referred to the fact that he had found a difference in the analysis of the gas in the curve of the flue, which he ascribed to centrifugal force. The idea seemed perhaps a little far-fetched, but as a matter of fact, an Italian had invented a centrifugal separator for gases, and Captain Sankey believed he had been able to get 5 per cent. more oxygen as the result. The idea was to use the enriched air in boiler-furnaces and blast-furnaces; but he believed the power required for the fan rather outweighed the gain. Captain Sankey.

Mr. T. Rooke remarked that the Author drew attention to the published returns of the costs of working electricity supply-stations, and every engineer who had had control of such undertakings had doubtless observed the discrepancies to which he referred. They were, Mr. Rooke thought, accounted for—at all events to some extent—by differences in the load-factor. The Author did not state what the load-factor had been while the tests were being taken. He also noticed in the same returns that some of the stations operated with gas-engines showed very high fuel-costs. It would be interesting to know how the advocates of gas-engines reconciled those high costs with their claims for fuel-economy on the part of gas-engines. The Author mentioned that he used his blow-off discharge for heating the feed-water. Mr. Rooke did not know whether he mixed it with his feed-water, but it seemed to him Mr. Rooke.

Mr. Rooke. rather questionable practice to do so. With regard to the determination of the value of fuel by means of the calorimeter, that was a common practice in a station of which he was in control for some time; in fact, not only were calorimetric tests of the fuel made daily, but also rough analyses to determine as far as possible the amount of moisture, volatile hydrocarbons, and fixed carbon present. In that way it was found possible to determine very closely the value of the fuel for heating, and to secure entire freedom from the operations of financial experts in the stokehold and prejudices on the part of firemen. A bomb calorimeter was used for the purpose, and check results were obtained by analysing the contents of the bomb after obtaining the reading to determine the calorific value of the fuel. An endeavour was made to purchase coal by paying for it according to the heat-units it contained, but, like the Author, he found that the coal-contractor scarcely understood the process, and certainly did not fall in with his views on the subject. As the contractor justly said, he was scarcely responsible for the calorific value of the coal in a particular seam; he did the best he could for the supply-company, and they must take what they could get. The works in question eventually succeeded in getting a fairly satisfactory supply by keeping a large bulk sample of the coal, and also by making calorimetric tests. The Author referred to the use of CO_2 apparatus, and to the variation in the results according to the position in the flue from which the gases tested were taken. Many stations had permanently installed a CO_2 apparatus which gave a continuous record of the CO_2 in the flue-gases. Mr. Rooke found, however, that exceedingly good results could be obtained by making the tests with the Orsat apparatus, and by training the stokers to fire the boilers in the most economical manner. The CO_2 apparatus was used not so much as a check on the firemen but as a means of training them to burn under the best conditions a particular class of fuel. In that connection he thought that if a fuel could be purchased containing a certain quantity of heat, and it was found that that fuel was the best that could be obtained for the money available, it would be better to adapt the boiler-furnaces to consume that fuel than to purchase a fuel which happened to suit the particular furnace. The Author also referred to the condensation of steam in his pipe system, and seemed to think that the discrepancies due to the fact that saturated steam was used during the test would not be very great. Mr. Rooke believed that some years ago Messrs. Willans and Robinson made some interesting tests on the heat-losses in the air-buffer cylinders of their engines, and found that when dry air was in the cylinder the heat-losses were practically negligible, but that the presence of a small quantity of moisture increased the losses enormously. There was very good reason to believe that steam-pipe losses with steam saturated when it left the boiler, but damp at quite a short distance from the boiler, were very considerable indeed, and that the advantages derived by the use of superheated steam in electricity supply-stations were very largely accounted for by the much smaller steam-pipe losses which occurred under those conditions. He would conclude by saying that although such observations as had been made by the Author were highly instructive, yet in obtaining economy in electric-light stations, a well-organized system of returns would enable the manager to detect daily any unnecessary loss, would promote interest amongst the members of the staff, and would produce the best possible results. Mr. Rooke

Mr. W. H. PATCHELL did not know whether the Author was responsible for the Blackburn works, but as an old station-engineer he knew the great difficulties there were in carrying out any researches of the kind described. The employers were apt to say that the engineer was there to sell units and not to run a laboratory; and even if they did not express themselves as bluntly as that, the engineer had various distractions in the course of his business, which made it very difficult to carry out tests. He therefore thought that all who had been in a position similar to that occupied by the Author thoroughly appreciated the great care he had taken in obtaining the figures set forth in the Paper. Unless engineers set ideals before them, no progress was made. It was not enough to know that they were using 10 lbs. of coal per electrical unit; they ought to know what should be used under the best conditions. The Author seemed to have set out with a sort of satisfaction in 28 lbs. of steam per kilowatt-hour. That seemed to Mr. Patchell much too high as an ideal. He had made lately a practical test on a plant consisting of three 1,600-kilowatt sets, with a 30 per cent. overload-capacity. The test was for 8 hours on two sets, the average load was 2,040 kilowatts, and the steam-consumption for the two was only 19·3 lbs. per kilowatt-hour. He thought the Author ought not to be content with 28 lbs. He had often marvelled at the way engineers were almost hypercritical with the steam-guarantees given by engine-builders. They would squabble over a fraction of a pound in the steam-consumption guaranteed, and would give a bonus when the guarantee was beaten; yet they would lay down a plant very similar to the one described by the Author—as if the maximum amount of condensing surface had to be incorporated. Personally he thought it did not matter what the engine-guarantees were if Mr. Patchell.

Mr. Patchell. such large steam-pipes as those described by the Author were used. Mention was made in one place of a 14-inch pipe for 16,000 lbs. of steam per hour. If a 7-inch pipe, or at the outside an 8-inch pipe, were substituted, the steam would get through quite comfortably, and a very much better effect would be obtained. No reference was made in the Paper to the cleanliness of the boilers, but a remark on p. 5 made him feel rather sorry for them, namely: "The feed-water is supplied from the condenser-discharge, heated by feed-pump exhaust-steam, and supplemented by drainage from steam-traps, blow-off valves, etc." He thought a mixture of that sort ought not to be put into boilers. The Author might say that he used some kind of feed-purifier. Mr. Patchell did not know the nature of the water obtained at Blackburn, but he thought the Author was almost certain to get some oil through into the boilers, and in that way obtain an objectionable scale, which, if not absolutely bad for the boiler-plates, was bad for heat transmission. Mention was made of water-meters: but if water-meters were used for hot water, and were also working under a head, it was astonishing what records were obtained from them. With regard to the question of fuel-analyses, and to Mr. Rooke's statement that it was difficult to get fuel-contractors to guarantee their coal, a great deal depended on whether the fuel-contractor was the colliery-owner or a middleman. The middleman had to pass on anything that the colliery-owner put on him. Mr. Patchell had tried, not altogether without success, to get colliery-owners who sold their own coal to sell it on an approximate analysis; and good results were obtained. At the present time he thought there was a ray of hope in the sky in that direction. The hours of labour of miners were to be shortened; if that process were continued, the hours of work would become so short, and coal would become so high in price, that it would eventually be sold under the Food and Drugs Act! Buyers of coal might then really get what they paid for. He suggested that the Author had made a little slip on p. 15 in dealing with the gas-analysis by referring to an air-ejector; he thought the Author meant an air-inducer. In *Fig. 6* there was a curious lag between the readings of the two instruments. He suggested that that was due to an actual lag in the flow of gas through the pipe. Possibly one instrument was supplied with gas from one part of the pipe, and the other instrument from another part: the gases were identical, but not taken at the same moment. The remarks on steam-temperature were very interesting, and he hoped it would not be considered outside the scope of the Paper if the Author in his reply gave some information about the cylinder-lubrication. It was stated (p. 18) that the temperature varied from 450° to 650° F. To begin with, he took exception to such a variation, because in his experience it was very bad. He thought alterations in temperature of that kind were worse than a fairly high superheat. He believed the engines were Belliss and Willans engines, and he would not care to run either a Belliss or a Willans engine, or any other engine, with a piston-, slide-, or other form of valve, at anything approaching 600° F. The Author referred to the specific heat of steam as 0·48 in one portion of the Paper, and 0·54 in another. An exceedingly good Paper had recently been read by Professor Carl C. Thomas before the American Society of Mechanical Engineers,[1] in which he stated that the value was 0·57 for 126° F. With regard to the steam-consumption of the auxiliaries, he thought the amount of steam used by the jets under the fire-bars was unusually low. Perhaps it had only been put through to cool the bars and not to form a draught, but 2 per cent. for jets under the bars seemed to be much lower than usual; 12 to 15 per cent. was much nearer the average. He would like to know what evidence the Author had to support his statement that the steam was really superheated at the exhaust. He could hardly believe that, because he knew from experience the very rapid way in which the superheat disappeared in the cylinder. One of the engines used by the Author was a triple-expansion engine, but the majority of them were compound. The Author might be using reheaters, but, if not, Mr. Patchell certainly would not expect to find any superheat at the exhaust. On p. 24 reference was made to the necessity for maintaining the steam-pressure in boilers for 22 or 23 hours. He would like to know what was the necessity for keeping pressure in a boiler for that time. Such a boiler would have to be set almost abnormally if it did not pay better to let the fire right out and fire up again, instead of banking it for 23 hours out of the day. He also thought the diagram given of the load showed that electrical engineers ought to come to the rescue of the mechanical engineer, because part of the load could be much better carried by batteries. The load-factor mentioned in the technical journals in their analyses of accounts, which had been referred to by Mr. Rooke, was the load-factor on the whole station; but the load-factor on the engines was frequently overlooked. It was quite lost in the plant load-factor—the load-factor generally published—which simply meant the ratio of the

Mr. Patchell.

[1] Transactions, vol. 29, p. 1021. See also *Engineering*, vol. lxxxv, p. 415.

Mr. Patchell. units that the works turned out to what they could have turned out if they had worked all hours in the year at the maximum load. He thought that the engine load-factor was the more important of the two. He had been lately looking through some American reports of various plants run by a syndicate. A great deal had been heard for the last 5 years in London of the question of antiquation of plant. English engineers had been told that they did not antiquate their plant half fast enough; but the facts with regard to the plants controlled by the syndicate he had mentioned showed that the best plant, as regarded the coal per kilowatt-hour, was one consisting of a large number of small belt-driven units; and the difference was due solely to the great care exercised by the engineer in running the engines. He therefore advised engineers not to scrap their plants if they were at all effective, but to see what could be done with them, instead of putting their faith in very large units. He did not say there was not a proper use for large units. He had put up the largest reciprocating engines in the country, so that he did not want to be taken as blowing hot and cold; but the unit must have a proper relation to the size of the load to be dealt with. On p. 32 the temperature of the gases from the boiler was given as 610°, and the temperature of the feed to the boiler as 303°. That seemed rather a large difference. The feed certainly ought to be better with such hot escaping gases. The Author reprinted a Table from Mr. Stott's excellent Paper, and invited the members to compare it with his own. Comparison was a little difficult, because the two were got out on different lines For instance, Mr. Stott's Table gave the loss to the stack as 22·7 per cent., whereas the Author did not give the loss to the stack at all in his Table. On p. 35, line No. 79, the heat carried off in the chimney-gases was 67·6 per cent. He did not mean to say that the bases of comparison were the same; possibly they were not; but at any rate the stack-losses at Blackburn must be very much greater than those in Mr. Stott's station in New York. Then he wished to ask why the Author used 185 lbs. per square inch steam-pressure for a compound engine, which he did not think made for economy. He thought that to a certain extent the heat-losses at Blackburn were put in such a way that "the wood could not be seen for the trees." The description of the Blackburn plant showed that Willans and Belliss engines, Lancashire boilers, and condensers of three different types were used. The output was 4,142,000 units, and the load-factor was 19·7 per cent. The coal-cost was 0·3*d.*, and the works-cost 0·69*d.*, per unit. In a town not very far off, Bury, Belliss engines, Lancashire boilers, and jet- and ejector-condensers were used. The output was 3 million units instead of 4 millions at Blackburn, and the load-factor was 20·48 per cent., which was very near to 19·7. The coal-cost was 0·22*d.* instead of 0·3*d.*, and the works-cost 0·44*d.* instead of 0·69*d.* He thought that showed there was something in his statement that some other figure than the load-factor must be looked at. The power of the plant installed at Blackburn was 4,518 kilowatts against 2,260 kilowatts at Bury; the maximum load at Blackburn was 2,400 kilowatts and at Bury 1,675 kilowatts. The spares at Blackburn were 88 per cent. of the maximum load, against 35 per cent. at Bury. He thought that showed that the latter plant had been put together with great care and with less condensation-loss, because the plant must be more compact when there was less of it to deal with a given load; while great differences in the results were seen, although the load-factor was much the same. Mr. Patchell.

Mr. G. WATSON agreed with Mr. Patchell that the chief trouble with the boilers installed at Blackburn was probably due to the scale. The very low evaporation per pound of fuel was very likely due largely to that cause. It might be worth while, in order to get better results in the boilers, to try heating the air before combustion. According to the Author the draught in the chimney was so good already that that might be done without the addition of fans; at least it might be tried with some of the boilers. In that way the temperature of the gases going into the chimney would be reduced, and a considerable economy would be effected. If only 2 per cent. of steam was used in the jets, the figure was not a large one; but he thought it would be better to moisten the grate-bars than to use live steam. He also thought that the loss from the clinkers obtained with mechanical stokers was often underrated. He believed that in many cases that loss was equal to 7 per cent., or even 10 per cent., of the fuel. The clinkers should be sorted, the fused portions which had been thoroughly burned being screened out, while the remaining cinders should be burned over again in a special firebrick-lined furnace. In that way he thought considerable economy might be effected. He suggested that the air-leakages, which had attracted the Author's attention, might be bigger than he ,allowed for. In the setting of Lancashire boilers particularly there was apt to be a great deal of air-leakage through the brickwork of the side flues. He was not sure whether water-softeners were used, but it seemed to him the purity of the water aud the cleanliness of the boilers might make a great difference to the efficiency. Mr. Watson.

Mr. W. H. PATCHELL apologized to the Author for making a misstatement in his previous remarks, owing to reading the Paper Mr. Patchell.

Mr. Patchell. hurriedly. It had been pointed out to him that the Author did give the heat rejected to the chimney in the second line on p. 26, namely 13·36 per cent. He thought the reason he overlooked it was that he ran his finger down the percentages, and he did not find any figure heavy enough.

Prof. Smith. Professor R. H. Smith observed that his remarks would be directed not to the results of the tests recorded in the Paper but to the methods of testing adopted. There was, he thought, no special value or interest in the results, which throughout the whole Paper the Author admitted to be bad. The great merit of the Paper was the Author's dogged refusal to take anything whatever for granted, and his persistent scepticism as to the validity of the more common methods of measurement. Those, he thought, were great merits in an experimenter. Incidentally, with regard to the results, he wished to refer to what Mr. Patchell had said about the exhaust steam not being superheated. In the expansion of exhaust steam from engines, the expansion carried the steam down to a temperature far below that of the metal of the cylinder of the exhaust-passages, and it might very well be largely heated during its passage through those outlets, by conduction from the metal. In the list the Author gave of seven different methods of waste (p. 2), the Author mentioned the loss of heat by leakage, radiation, and condensation; and Professor Smith thought that conduction should be added, because it sometimes led to important losses. The Author seemed to take a very unusual view with regard to the drop of temperature to be expected in the case of the flow of steam through a pipe. The flow that he tested, using a 14-inch pipe, had a velocity of 11 feet per second, or 7½ miles per hour; mention was also made of a 10-inch pipe which gave double that velocity, 22 feet per second, which was a very considerable velocity, and would develop much friction. Very possibly that accounted for the observed low drop of temperature. The Author expressed surprise that that should happen, but Professor Smith did not think that any surprise need be felt. On the other hand, no mention whatever was made of the fall in pressure along the pipe, which was the important thing, and which at so high a velocity might be expected to be appreciable. The Author referred to the great difficulty of rightly and thoroughly valuing the moisture in fuel supplied in large quantities to furnaces, and referred to the very unsatisfactory evidence given by drying small samples. Another method of dealing with this matter would be to take one fairly large volume of fuel in bulk (it need not be more than 1 cubic foot for the small coal used for boiler-furnaces), which should be measured carefully as to the volume in bulk, crushed, and, if necessary, sampled. This should be weighed, then dried, and weighed again, giving the weight of dry fuel corresponding with the carefully measured volume of the coal in bulk. The quantity of moisture in the coal taken into the furnace-room varied very largely with the weather—with the dampness or dryness of the atmosphere; but the volume of the coal in bulk did not vary much in consequence of those variations in moisture. He suggested that, after finding the weight of dry coal corresponding with the fairly large unit of bulk, the fuel should, for any large test, be measured by volume and not by weight. Secondly, with regard to the measurement of water, he desired to point out that in most tests the water was measured by volume, and often no notice was taken of the variation of specific gravity on account of difference of temperature, although it was important; at any rate, it affected the quantity of the water by a considerable fraction of 1 per cent. He thought it would be much better to measure the water by weight and not by volume. Many methods were available, but tipping a double tank, with a calibrated glass-tube gauge to each half, was the simplest. Thirdly, the Author referred to the very unsatisfactory evidence of the calorific value of fuel given by ordinary tests, such as sampling one part in seven million of the fuel used. Professor Smith had used very successfully a simple form of fuel calorimeter, which, instead of burning 1 or 2 grammes, burned 15 grammes. With some kinds of fuel he had burned over 20 grammes, or more than $\frac{3}{4}$ oz., which was much more satisfactory. The calorimeter was exceedingly simple in its construction, it used large quantities of water instead of small, and it never gave a rise of temperature of the water of more than 10° C., the usual figure being about 6° or 7°. With that small rise of temperature, especially if care were taken to have the water initially a little below the atmospheric temperature, there was practically no cooling-correction, such as was necessary in every other form of calorimeter with which he was acquainted. The instrument was simply formed of a large glass cylinder, about 8 inches in diameter and 2 feet 6 inches deep, surrounded by a wooden box, with an ample air-space between them. The air-space was carefully partitioned and filled quite lightly, not tightly, with cotton-wool or asbestos-wool, so as to prevent any air-currents. The results obtained proved that there was practically no measurable loss of heat to the outside with such a calorimeter, for without any deduction for loss of heat from the outside by radiation or other cooling, calorific values were always obtained well up to and sometimes even slightly exceeding those given by the most careful tests of the same qualities of fuel Prof. Smith.

Prof. Smith. by the Favre-Silbermann calorimeter. An error in such an instrument was always in defect, and the result of getting sometimes more than was obtained with the Favre-Silbermann calorimeter with the correction for outside cooling, proved that the instrument had practically no outside cooling. All experimenters with the kind of plant described in the Paper knew what a very large quantity of heat was lost by radiation, and he thought it was unfortunate that no attempt had been made to measure by a direct method the heat so lost. He had lately used an instrument, made by Messrs. J. J. Griffin and Company, which measured in a very simple manner and with great accuracy and sensitiveness the heat radiated from a surface upon a large scale.[1]

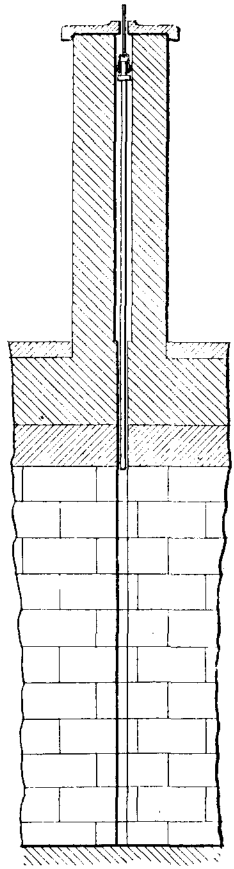
Mr. Halpin. Mr. DRUITT HALPIN agreed with Captain Sankey that calorimeter tests alone could not be relied on for the practical use of fuel in boilers. A very important point in such work was, as Captain Sankey had stated, to observe carefully to what extent the coal clinkered; but there was also another matter which greatly influenced the question under some conditions, namely, the rapidity with which coal would burn. Some coal burned much more slowly than other, and it was frequently a matter of great importance that a reasonable amount of coal should be burned. In the Author's list of the causes of the low efficiency in his station, the first was failure to obtain the most economical class of fuel. The Author was to be sympathized with in that connection, but this shortcoming was a matter for the Author's Committee to deal with; probably they would not give him what he wanted, and he was not responsible for that. Several speakers had referred to the second failure mentioned by the Author, namely, failure to obtain the prescribed calorific value in the fuel. Colliery-owners would not sell coal according to its calorific contents, but one must be thankful for small mercies. Civilization had advanced so far that colliery-proprietors were

[1] The radiated heat is received upon a surface of 1 square foot formed of a zigzag coil of very thin copper tube of small square section. This surface is blackened, and is carefully protected from air-currents and other atmospheric cooling, as also from cooling by conduction to other parts of the instrument. The heat is carried away by water-circulation through the copper coil. The quantity of water per minute and its rise of temperature are measured with exactitude, the latter never being more than 3° to 4° C. It is important to proportion the water-flow to its rise of temperature so as to minimize the resultant error of observation in their product. In one form the instrument indicates continuously the instantaneous flow of water. It is believed that the whole heat radiated is measured in this way within a maximum error in defect of 1 or 2 per cent. The instrument is very sensitive to slight changes in the radiating surface.—R. H. S.

Mr. Halpin.

compelled by Act of Parliament to sell coal by weight, and possibly they might be made to go a little farther in the future. The third cause of failure mentioned by the Author, namely, faulty combustion, comprising excessive or deficient air-supply, was one of the most important matters engineers had to deal with in connection with boilers. The results of the most elaborate series of tests on that point of which any record had, to his knowledge, been published, were those made by the Frankfort Commission of 1891. A number of boilers were tested, including Cornish boilers, water-tube boilers, vertical boilers and boilers of the horizontal Field type.[1] The analyses made showed in the first place the quantity of air required theoretically to burn the fuel. An analysis was then made of the amount of air actually used at the back of the bridge, showing the excess that had come through the grate. A further analysis was made at the exit from the boiler, showing the increase of air which had actually leaked through the brickwork and which of course did a great deal of mischief. The whole of the losses were shown in a diagram,[2] including the radiation from the brickwork; and the total efficiency of the boiler for the heat put into it was also shown. The efficiency-curve[3] rose continuously as the excess of air diminished: where the air was 169 per cent. in excess, the efficiency of the boiler was down to 62 per cent., and at the other end of the scale, where the excess of air was only 47 per cent., the efficiency was 79 per cent. That showed what an important part the air-supply played in connection with combustion and the production of steam in boilers. Leakage through dampers would occur in the way they were ordinarily made. The damper shown in *Fig. 12* was designed by the late Mr. W. Schönheyder. It was exceed-

Fig. 12.



[1] *Zeitschrift des Vereines deutscher Ingenieure*, vol. xxxviii, p. 733.
[2] Proceedings of the Institution of Mechanical Engineers, 1908, p. 149.
[3] *Ibid*, p. 150.

Mr. Halpin. ingly simple, cheap, and absolutely effective. It was made of ordinary stock bricks laid in cement, the precaution being taken of putting them in water for a couple of days beforehand. Under those circumstances the bricks would stand, if they had been made wet enough. The chief point to remember was that, instead of having a very large opening right across the whole flue, a cast-iron cap should be placed at the top through which the rope went. With $\frac{1}{2}$-inch rope and a $\frac{5}{8}$-inch or $\frac{3}{4}$-inch hole there was infinitely less leakage than in the ordinary way. The Author stated that his steam-jets used only 2 per cent. of the steam. Mr. Halpin hoped that was so, but personally he had never known such a thing to happen. The tests he had made convinced him that 15 or 16 per cent. was much nearer the truth. He knew it would be considered rank heresy by those interested in mechanical stokers, but that was his experience. He desired to suggest one remedy, which he thought could be carried out very simply. Many engineers had probably used for years nickel-seated valves which did not cut when exposed to high pressures and high temperatures; and he thought if makers of stokers could cut a very small hole with a diamond drill through nickel-steel, so that it would keep at that diameter and not get larger, some benefit would be obtained. Very likely the quantity used at starting was 2 per cent., but it was something quite different after a short time, owing to the cutting action of the steam on the orifices. On p. 18 the Author gave a remarkable figure in dealing with the question of pipe-radiation, saying that the condensation in the pipe-system under the trials amounted to an average of 953 lbs. of water per hour, equivalent to about 810,000 B.Th.U. per hour, which figure divided by the total radiating pipe-surface gave a heat-loss of 473 B.Th.U. per square foot per hour. As far as Mr. Halpin could gather from the Paper, the test was made when one station was shut down and the other station was running, so that the question was not complicated by velocity. He supposed the pipes were clothed. He asked that question in all seriousness, because even the ordinary radiators put in for heating buildings and rooms could not deliver as much heat. They transmitted about $2\frac{1}{2}$ units per hour per square foot per degree of difference of temperature, and if exposed to draughts they would do more. He could not understand how in any experiment of the kind such high results were obtained if the pipes were covered with even the least efficient non-conducting composition. It was his duty about 25 years ago to make a number of radiation-experiments in Germany, and he then found it possible, within commercial limits, to reduce the radiation-losses by 92 to 95 per cent. Those results were obtained with cork, which, to his mind, was infinitely the best non-conducting material in existence, owing to the fact that in it the air-imprisonment was so complete, and the cells so small and uniform. The Author also referred to the fact that measurements of the water were made with counters and pumps. Mr. Halpin thought that counters and pumps should not be used in a test of that kind. The tanks used would, however, give a certain check upon the process. The statement was made on p. 21 that gauges were put across the water-nozzles. He hoped the Author would give a little further explanation on that point, as he would like to know how the velocities were measured and how the apparatus was arranged. The Author spoke of his electrical measurements being accurate within 2 per cent. He thought it showed very strong faith to trust in electrical instruments to that extent, and he hoped the Author had taken means to see that he was getting such a result. The leakage through the blow-off cocks must be serious, and he could not understand why steps were not taken to stop it. There were in existence arrangements of double plugs in cocks, one inside the other, and he had himself very often used for cases of the kind two distinct cocks in series, so as to be sure that one was absolutely tight and cold, and so that it could easily be dealt with. The final efficiency of 5 per cent. shown in the Table was exceptionally low, but one must take the figure as one found it, and thank the Author for relating everything so frankly, and showing how such heavy losses had been brought about. Mr. Halpin.

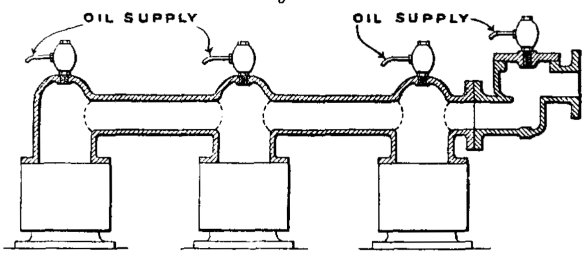
The Author, in reply, observed that he was glad of the assurance that the sampling of coal, if performed with care, constituted a reliable index to the value of a bulk quantity. It would seem impossible to devote to the sampling of so large a quantity as 230 tons the degree of attention necessary to justify absolute reliance on the accuracy of the process as carried out. The error might or might not be overshadowed by others occurring in the prosecution of such a series of tests. The moisture-trials had been first made on a small scale, and the results had shown a much greater range of variation than was encountered in the larger tests subsequently adopted, the mean being, however, very much the same in both cases. Regarding the question of dryness of the engine exhaust, Captain Sankey's clear explanation confirmed the opinion expressed in the Paper as to the improbability of the truth of the indications. All the evidence obtainable appeared to point to the reality of a dry exhaust, and the only course possible on this evidence was to accept its existence tentatively. The The Author.

The Author. suggested plan of preparing log-sheets of the operation of individual engines was almost impracticable, as nearly half the output was generated for tramway purposes on a continuously and irregularly varying load. The Author did not think the allusion to the centrifugal separation of oxygen from air was quite relevant in connection with the similar action observed in flue-gases, oxygen being only about 10 per cent. heavier than air, while CO_2 was 50 per cent. heavier than its volume of flue-gas. Mr. Rooke's suggestion that differences of load-factor were largely responsible for divergence in economical performance was true only to a limited extent, as inspection of the published records would show. The Author could not agree that "rough analyses" of the fuel used, as a supplement to calorimetric investigation, were at all likely to be as satisfactory as working trials. He had little confidence in the most careful coal-analysis unless conducted by a chemist accustomed to such work. His experience with the CO_2 recorder had shown the great superiority of a continuous record as against occasional or periodic testing by means of a portable apparatus so liable to temperature-error as the Orsat. Referring to the radiation-loss in pipes, the Author did not suggest that there was little difference in the quantity, whether superheated or saturated steam was employed, but rather that its determination in practice with the former was difficult and unreliable. Mr. Rooke justly attached importance to a well-organized system of returns as to the performance of the plant. Assuming the continued accuracy of the measuring-apparatus, the information so obtained was, however, only comparative, and little knowledge was derived of the possibilities of the plant, these being rendered apparent only by making tests of an absolute character. Replying to Mr. Patchell, the Author did not consider 28 lbs. of steam per average unit a high figure to take as ideal, having regard to the smallness of the generating-sets and to the character of the load. The 19·3 lbs. of steam mentioned by Mr. Patchell was quite unattainable where three-fourths of the machines were 10 years old and of less than 300 kilowatts capacity, and where three distinct classes of supply were given. He endorsed Mr. Patchell's opinion as to the absurdity of stipulating for fractional steam-consumption guarantees on the load-factors usually obtaining in central-station supply, and also of installing abnormally large steam-pipes, particularly where high-speed engines were employed. With regard to the cleanliness of the boilers, a somewhat ambiguous remark on p. 5 had apparently conveyed a false impression. The leakage from blow-off valves referred to the discharge from economizer and other relief-valves, and not to boiler

The Author.

sludge-valves, the discharge from which was stated on p. 25 to be unsuitable. The condensing water was obtained from a disused town reservoir, and the scale found in the boilers was only slight. The variation in steam-temperature between 450° and 650° F. was regrettable not so much in itself as because the existence of the lower figure implied a large radiation-loss, occasioning a considerable fall in temperature when small volumes of steam were in circulation through the pipes. The variation was not to be interpreted as fluctuation in temperature, the objection to which was obvious. Mr. Patchell's objection to the employment of slide-valve engines at a temperature approaching 600° F. was met by the facts that the engines referred to in the Paper had been so worked for about 5 years with very satisfactory results, and that similar engines were now in use under such conditions in many electrical power-stations. Difficulties in cylinder-lubrication had been surmounted after a careful investigation involving modification of the lubricating-

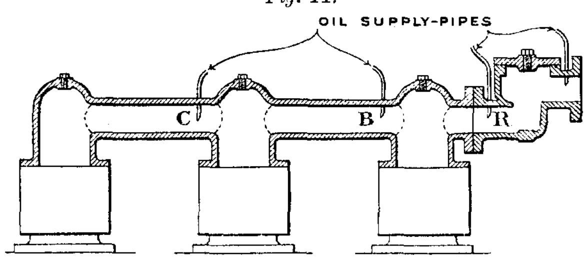
Fig. 13.



arrangements. The fact that the 1,200-HP., nine-cylinder, triple-expansion Willans engines were run continuously on three to five drops of oil per minute, except for a few moments before shutting down, showed that some degree of success had been obtained. The case of these engines presented the greatest difficulty, and *Fig. 13* showed the steam-chest with the original provision for lubricating. This arrangement involved the introduction of the oil in the form of drops, implying its subjection to the action of the steam during the formation of the drop, and meanwhile the passage of a large quantity of dry steam into the cylinders. It was also extremely doubtful whether any large amount of oil would reach the cylinders and valves, a considerable portion being probably deposited on the walls of the steam-chest and the connecting pipes. Attempts to obviate these objections culminated in the arrangement shown in *Fig. 14*. The projection of a pipe with a sharply bevelled extremity well into the steam-pipe ensured the constant removal of the oil in

The Author. minute quantities by the rapid pulsations of the steam. The steam was thus charged with oil which was carried into the cylinders without previous exposure to a high temperature for any long period. The removal of covers each night after shutting down permitted the results of each modification to be seen, and it was found that pipe C need not be used at all and pipe B only very slightly, the bulk of the oil being injected through pipe A, with a small quantity at the throttle lubricator. This resulted in reduction of the amount of oil used, and hence in diminution of the quantity present in the condenser-discharge. During the test which was the subject of the Paper, the cylinder-oil used amounted to 5 gallons. The volume of water passed through the condensers was 107 million gallons, the proportion of oil to water being thus 1 to 20 millions, even assuming that the whole of the oil supplied was discharged to the condensers. There was little reason, therefore, for the anxiety expressed by some speakers as to the presence of oil in the boilers.

Fig. 14.



Regarding the maintenance of steam-pressure in boilers out of use for the greater part of the day, the Author had not adopted the suggestion of Mr. Patchell to let the fires out and relight them when required, as he was of opinion that the leakage of seam and gusset rivets resulting from such a course would produce inconveniences more serious than the expenditure incurred by the existing method. Comparison of the temperature of the flue-gases and of the boiler feed-water had been made inaccurately, due perhaps to hasty examination of the Paper. As would be seen on p. 34 and in *Fig. 9* (p. 29), the gas-temperature at the economizer-inlets was shown as 565° F., and the rise in temperature of the water was somewhat higher in regard to this than was usually obtained within the Author's knowledge. The statement that Tables II and III were constructed on different lines was similarly unfortunate, as beyond the fact that Mr. Stott's figures had been reduced to a basis of 1 lb. of coal, and that Table II had not been so reduced, there would seem to be no difference. The percentage losses in the two cases were strictly comparable. The Author was in agreement with Mr. Watson as to the desirability of cooling grate-bars without the use of live steam, and had made some experiments in this direction, but he had not been able so far to devise a satisfactory arrangement. The sorting of clinker and ash was not commercially practicable unless they were in a condition in which they should certainly not be removed from the ash-pits. It was so simple a matter to burn the ash thoroughly, in a Lancashire boiler-furnace, by slightly opening the ash-pit doors before cleaning out, that no need for sorting should arise. Professor Smith's remark on conduction as leading to important losses might be true, but no practical means was available for its evaluation. With regard to the experiment relating to the fall of temperature of superheated steam, Professor Smith would seem to hold the opinion that friction between steam and pipe-surface increased the temperature of the former. There was, on the contrary, strong reason to believe that the increased scouring-action due to higher velocity rendered the abstraction of heat by the pipe much more rapid, and so resulted in a greater diminution of steam-temperature. The fall of pressure along the pipes was of course greater in the smaller pipe, but was only appreciable by the most sensitive laboratory instruments. From Mr. Carter's experiments, a pressure-drop of roughly 1 lb. on 200 lbs. absolute per 100 feet of 12-inch piping, accompanied a velocity of about 80 feet per second, whereas the velocities in the trial were 11 feet and 22 feet per second respectively. The Author considered Professor Smith's method of measurement of coal by volume as unlikely to lead to improved results, and also that the suggested estimation of radiation by direct test was impracticable in dealing with long steam-pipes exposed to varying external temperatures. Mr. Druitt Halpin, with other speakers, was sceptical as to the steam-consumption of the grate-jets. The figure stated, namely, 2 per cent., was approximately accurate, the steam being used for cooling only and not for the creation of draught. The jets were supplied through a $\frac{1}{4}$-inch copper tube with controlling cocks barely open, the whole apparatus being under constant supervision. It was within the Author's experience that where this agency was employed for the production of draught a steam-consumption approximating to 15 per cent. was of frequent occurrence. The radiation-loss of 2·15 per cent. of the total heat had attracted Mr. Halpin's attention, and would certainly seem to be of a high order. Taking his figure of 2·5 units per square foot per degree of difference of temperature, the performance of radiators under the conditions obtaining would be represented

The Author.

The Author. by 900 units per square foot—roughly double, and not as Mr. Halpin suggested less than, the heat-delivery of the steam-pipes. The Author ventured with diffidence to suggest that Mr. Halpin's figure might be too low for a large temperature-difference. The late Sir William Anderson had shown[1] that with only 220° F. difference of temperature, the rate of heat-transmission was 3·5 B.Th.U. per square foot per hour, and that the heat-delivery increased in greater ratio than the difference of temperature producing it. There was every reason to consider that the total quantity of heat lost by leakage and condensation was fairly accurately determined by the tests, which were made repeatedly and with uniform results. Its reduction to the basis of heat-transmission per square foot might be open to some question, owing to the difficulty of estimating the surface of a large system of piping varying between 3 inches and 14 inches in diameter, together with its drains. The condensation in the superheaters under the conditions of banked fires would also in all probability be considerable. With regard to the measurement of velocity of water through the condenser-nozzles, the gauge used was a simple U tube containing mercury, one end being coupled to the large water-supply pipe, 2 feet above the nozzle, and the other to the condenser immediately below it. A reading comparatively unaffected by the velocity of the water in the pipe was thus obtained. The reliability of electrical measuring-apparatus was also called into question by Mr. Halpin, who appeared to view it with unnecessary suspicion. There was no difficulty in obtaining such a degree of accuracy as was mentioned in the Paper, closer limits being frequently specified and complied with in good instruments. The leakage from blow-down valves did not present cause for great anxiety, the whole of the water rejected by them only resulting in a loss of 0·13 per cent. of the heat of the coal. The larger portion of this loss was of course incurred in the regular blowing down to get rid of sludge. The provision of duplicate blow-down valves would entail complication and risks outweighing the small loss involved by their leakage. The final efficiency of 5·4 per cent. was referred to as exceptionally low. The performance was, of course, bad in an absolute sense, but was quite typical, and perhaps above the average, of electric central-station practice. It should be remembered that the generating-station was worked for considerable periods at about 2 per cent. of its rated capacity.

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[1] Minutes of Proceedings Inst. C.E., vol. xlviii, p. 263.