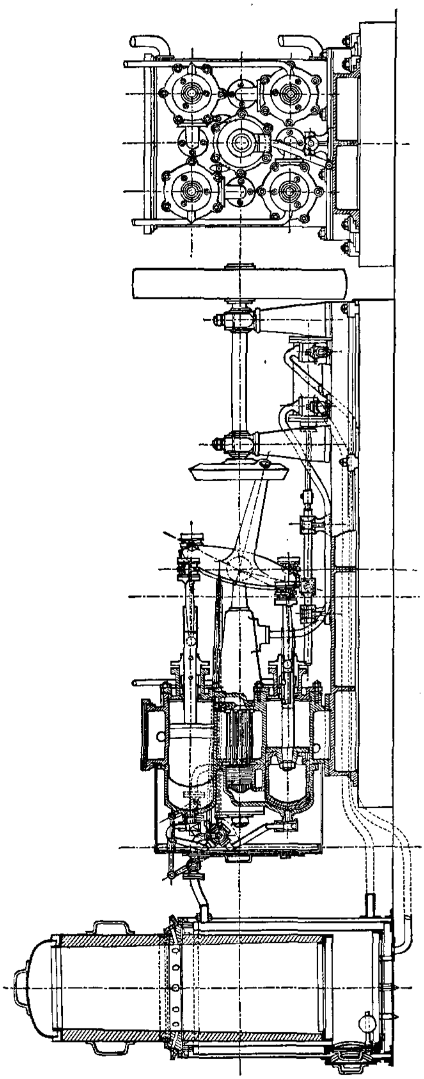
Discussion.

Mr. D. Clerk mentioned that Dr. Siemens had worked out the method of compression used in engine type 2 in 1860 in so complete a manner that no advance had since been made on it by any one. Dr. Siemens was again working at this type of engine, which, from the fact of it using hot-cylinder and regenerator, Mr. Clerk was certain was the best type for the very large gas-engines to be developed in the future. With respect to the cold-cylinder engine, of which alone he had treated in the Paper, he wished again to insist on this: that the theory which sought to explain the so-called sustained pressure on the indicated diagram by the hypothesis of slow inflammation (erroneously termed slow combustion) was a false one. That when maximum pressure was attained in the gas-engine cylinder it was certain that the whole mass was completely inflamed, and that no system of stratification producing slow inflammation could do good, but was quite opposed to the conditions of economy. Mr. Clerk.

Dr. Siemens said that one part of the Paper dealt with matters regarding the mechanical arrangement of gas-engines, and the other with a theoretical question, that of the law of combustion. He would refer to the theoretical part first, because the Author appeared to attach great importance to it, and as Dr. Siemens had from time to time given a great amount of consideration to the action of negative combustion or dissociation, it might be of some interest to the members to see how far his views fell in with those set forth by the Author. It was well known that by combustion no unlimited degree of temperature could be attained. Thus, in a furnace worked at very high temperature the fuel was not completely burned when it came in contact with the oxygen of the heated or non-heated air. The moment a certain comparatively high temperature was reached the carbon refused to take up oxygen, or the hydrogen refused to take oxygen, and what had been called by Bunsen and, shortly after him, by St. Claire Deville, dissociation arose. The point of dissociation was not a fixed one; partial dissociation came into play at a comparatively low temperature, and went on increasing at a higher temperature in very much the same ratio as vapour density increased with temperature. Thus, if aqueous vapour were passed through a tube at a sufficient temperature the whole of the vapour would be dissociated, and the oxygen and the hydrogen would be separated. It was true if these gases were left to themselves they would, Dr. Siemens.

Dr. Siemens. the moment the temperature lowered, again associate or burn; but if precautions were taken to cool them rapidly after they had attained that high temperature they would be found as a mixture of oxygen and hydrogen simply. The Author had stated that the law which governed these actions was not well known and required research, but Dr. Siemens would like to know whether he was aware of the researches of St. Claire Deville on the subject. It might be that the determinations of St. Claire Deville were not quite correct, but in the meantime they might be regarded as being so. He found that at atmospheric pressure the point of half dissociation of aqueous vapour arose at a temperature of 2,800° Centigrade, and that of complete dissociation at a much higher temperature. Taking that law as determined by the French philosopher, it did seem reasonable to suppose that when a mixture of hydrogen and oxygen, with or without a mixture of nitrogen exploded, the point was reached beyond which the temperature did not increase, and, according to the Author, that point was 1,500° Centigrade. If such a temperature was reached in the working cylinder complete combustion would not take place immediately, but only partial combustion would occur, which would go on as the temperature diminished by absorption into the cylinder or by expansion, and that combustion would be completed only in the course of the stroke. In that way the action which had been described with reference to the diagrams was reasonable enough. With regard to the mechanical arrangement of gas-engines, the Author distinguished between three types. In the first, the mixture of gas and air drawn in at atmospheric pressure was exploded. In the second, with which the Author had connected Dr. Siemens's name as that of the first proposer, the combustion was produced gradually; the gases were ignited as they flowed into the heating cylinder. In the third type, the gases, after being compressed and mixed, were admitted into the working cylinder, and suddenly exploded. With reference to the early engine which Dr. Siemens constructed in 1860, the Author had stated that it combined other elements, which were entirely wanting in the gas-engines of the present day. The gas-engine of the present day, taking either of the three types, was, in his opinion, in the condition of the steam-engine at the time of Newcomen. The fuel was burnt in a cylinder which it was attempted to keep cold by passing water over it, and it was easy to conceive that the heat so generated, was only partly utilised for maintaining the state of expansion of the heated gases, the cold sides of the cylinder taking a good half of it away at once, thus causing a great loss. Then there was another palpable loss in these engines. After expansion had taken place, after half the heat had been wasted in heating a cylinder which was intended Dr. Siemens.

Fig. 13.



Siemens Gas-Engine, A.D. 1860.

to be kept cool in order to allow the piston to move, the gases were discharged at a temperature of 1,000°, or in the best types about 700°. That amount of heat, representing in one case one-half and

Dr. Siemens. in the other two-thirds of the total heat generated, was thrown away. This was heat which could be saved and made useful. Instead of commencing the combustion at a temperature of 60°, if the heat of the outgoing gases were transferred to the incoming gases, combustion might commence at a temperature of nearly 1,000°, and the result would be a very great economy. In the engine which he constructed in 1860 (Fig. 13) all those points were fully taken into account. The combustion of the gases took place in a cylinder without working a piston, and in a cylinder that could be maintained hot, and the gases, after having complete expansive action, communicated their heat by means of a regenerator to the incoming gases before explosion took place. Although the engine was not worked with ordinary gas used for illumination, but by a cheaper kind made in a gas producer, he then thought that a gas-engine constructed on that principle would prove to be the nearest approach to the theoretical limits which could never be exceeded, but which might exceed the limits of the steam-engine four or five fold. The engine promised to give very good results, but about the same time he began to give his attention to the production of intense heat in furnaces, and having to make his choice between the two subjects, he selected the furnace and the metallurgic process leading out of it; and that was why the engine had remained where it was for so long a time. But now the time had come when there was a greater demand for engines of a smaller kind to do their best in houses and in small works, and when marine engineers especially had become fully alive to the importance of more economical arrangements. He therefore looked upon the question before the Institution as one of first importance to engineers, and he hoped that it would be well discussed.

Prof. Rücker. Professor RÜCKER said that in his work on Thermodynamics, Mr. Verdet had published a calculation of the theoretical efficiency of an ideal gas-engine. He assumed that no heat was lost through the sides of the cylinder, and that the explosion was so sudden that the whole of the gas was inflamed before the piston had appreciably moved; and under those circumstances he found that if the gases used were carbonic oxide, and a sufficient quantity of air to burn it completely, and if the whole of the carbonic oxide was burnt, the temperature to which the gases would rise, on the assumption that their specific heats remained constant, was 4,388° Centigrade. He found that the pressure would rise from 15 lbs. per square inch to 215 lbs., and that the efficiency of the engine would be 41 per cent.—that was, that 41 per cent.

Prof. Rücker.

of the total amount of heat produced by combustion of the gas would be converted into useful work. It was evident from the conditions of Mr. Verdet's problem that that was a purely theoretical calculation. The condition, for instance, that no heat was lost was one which could not be realised in practice. About four years ago, however, in the course of a series of lectures given by some of his colleagues and himself on coal, he pointed out that Mr. Verdet's calculation was not even theoretically correct; that Bunsen had proved that it was impossible that a mixture of carbonic oxide and air could reach such a temperature as 4,388° Centigrade, which was something like 2,800° above the highest temperature, which Berthelot had shown was consistent with Bunsen's experiments on the subject. The question then arose what the effect of dissociation would be upon the gas-engine, and Professor Rücker attempted to make a rough calculation to show how important it might be. In the first place, he assumed that the highest temperature which could be reached was that given by Bunsen's experiments, and in the next that the specific heats were constant and the inflammation instantaneous. With those conditions only about one-half of the carbonic oxide would be burned when the highest temperature was reached; then, as the piston began to move forward and the temperature fell, more would be consumed. But then there was the very important question as to how the temperature would fall, and in order to calculate that the law of cooling of a body heated to that extremely elevated point must be known. That, of course, he was ignorant of, and he was therefore obliged to make a rough assumption. Assuming that, as the piston moved forward, the gas burned so as to keep the temperature constant, he found that at the end of the stroke, when the pressure had fallen to that of the atmosphere, a part of the gas was left still unconsumed. Therefore in the half of the gas left unburned to begin with, there was sufficient to do all work that was done while the piston was moving forward. The only assumption he could make was that the temperature remained constant; any other, though that certainly was not true, would have involved some still more arbitrary hypothesis as to the law of cooling. Making, then, that rough assumption, he found that instead of a temperature of 4,000° Centigrade the highest reached would be about 2,000°; that the pressure, instead of rising to 215 lbs., would rise only to 103 lbs.; and that the efficiency of the engine would be only 25 instead of 41 per cent. That, though a very rough calculation, showed at once what the enormous importance of the phenomenon of dissociation might be. It served the purpose for

Prof. Rücker. which it was put forward, and showed that in any theory of the gas-engine physicists must make up their minds as to what part dissociation played in it. Passing from the theoretical problem that Mr. Verdet and himself discussed, namely, the case in which there was only enough air to burn the carbonic oxide completely, to the practical problem in which there was a much larger quantity of air present, a case arose in which dissociation was less important. The larger the quantity of air present the lower the highest temperature would be, and therefore, probably, the smaller the amount of dissociation. St. Claire Deville had shown that carbonic acid was dissociated at temperatures between 1,000° and 1,200°, and water at temperatures between 1,000° and 1,100° Centigrade. Inasmuch, therefore, as in the Author's engines, the highest temperature reached was about 1,500° (or 400° or 500° above the limits put by St. Claire Deville), it followed that if his measurement of the temperature was correct, which there was every reason to believe it was, and if St. Claire Deville's experiments were trustworthy, there was a certain amount of dissociation at the temperatures reached in his gas-engine. Passing, however, to the next question, namely, how much dissociation there was, the problem was much more difficult. With regard to that subject a series of Papers had recently appeared in the "Comptes Rendus de l'Académie des Sciences,"[1] which were so much to the point that he might be excused for giving a short account of one or two of the leading results at which the experimenters had arrived. The two gentlemen in question were Mr. Mallard (whose experiments on the rate of propagation of inflammation in gas had been mentioned by the Author) and a colleague, Mr. Le Chatelier. They had been making a number of experiments such as those that the Author had advocated in his Paper. They had made, indeed, what appeared to be one of the first serious attempts to investigate what was going on in gas heated between 1,000° and 1,500° Centigrade. The plan they adopted was as follows:—They exploded gases in an iron cylinder, attached to which was a Bourdon manometer; to that was attached a needle, which registered the pressure on a revolving cylinder. By reading off the curve so obtained, they got information as to the pressure in the cylinder at different times. He could not altogether accept their results without further confirmation. Some of the conclusions at which they had arrived were so striking that he thought they must certainly be supple-

[1] Vol. xci., p. 825; vol. xciii., pp. 145, 962, 1014, 1076.

mented by other experiments before they could be accepted. Prof. Rücker. But for the moment he would put aside all difficulties connected with the experiments, and simply state the conclusions. It was found, dealing with gases at very different temperatures, that the curves obtained upon the revolving cylinder showed a point of discontinuity. At the very highest temperatures the curves were somewhat different from what they were at low temperatures, and the assumption they made was that at the high temperatures dissociation had set in, whereas at the lower temperatures there was no dissociation; therefore the law of cooling would be different in the two cases. If, however, that interpretation of the experiments was accepted, it would be found that the temperatures at which dissociation took place to any considerable extent were higher than those he had mentioned. Thus the Authors stated that carbonic acid did not dissociate appreciably below 1,800° Centigrade, and that steam-gas did not dissociate appreciably below 2,000°. Here, then, there were temperatures considerably above those obtained in the gas-engine; if, therefore, the results in question were to be accepted, dissociation could not play a very important part in the matter. But although at first sight the experiments told against dissociation taking place to any large extent, in order to account for the phenomena they observed, Messrs. Mallard and Le Chatelier had had to introduce another hypothesis which practically came to very much the same thing. In all the earlier calculations upon the subject the assumption had been made that the specific heats of the gases were the same at high as at very low temperatures, but within the last few years two or three experimentalists of note had brought forward results tending to show that the specific heat of the gases increased as the temperature rose. The two most important researches made upon the subject were those by Professor E. Wiedemann and Professor Wüllner, the latter of whom showed that at temperatures between zero and 100° Centigrade there was an appreciable rise in the specific heat of gases at a constant volume. Messrs. Mallard and Le Chatelier had taken that hint, and they found that in order to explain the facts observed by them on the assumption that there was no dissociation, they must assume an enormous increase in the specific heats of the gases at high temperatures. But there were one or two points which appeared to present difficulties in their way. Wüllner showed that at the temperatures at which he worked, as might be *primâ facie* expected, the increase was much greater in a compound gas like water or carbonic acid than in an elementary gas such as oxygen or nitrogen. But Messrs. Mallard and Le Chatelier

Prof. Rücker. completely reversed that, and found that the increase was much greater in the elementary gases than in the compound ones; and they went so far as to show that oxygen would at a temperature of 1,000° have a specific heat no less than one hundred and sixty-five times greater than that which it had at zero. That result was so astonishing that it could not be accepted without much more proof than had at present been offered. But putting aside for the moment Messrs. Mallard and Le Chatelier's interpretation of the experiments, he wished to consider what they meant from a wider point of view, viz., that those gentlemen had come across a phenomenon which pointed to the fact that a vast quantity of heat was rendered latent. If specific heat at constant volume increased, the meaning of it must be that the work done by the heat was done within the molecules of the gas, that the heat was spent in separating or preparing for separation the atoms of those molecules, which were gradually being forced asunder; whether they were actually forced asunder or not might be a question, but a large amount of work was spent in separating them, or preparing to separate them, by loosening the bonds between them; and Messrs. Mallard and Le Chatelier's experiments served as much as anything previously brought forward to illustrate that point. He thought it must be assumed with almost certainty that a large quantity of heat was rendered latent in gases at temperatures between 1,000° and 1,500° Centigrade. All would agree that a certain amount of that heat was spent in dissociation (for Messrs. Mallard and Le Chatelier stated that they harmonised their results with those of St. Claire Deville by supposing that his experiments were more sensitive than their own), and the remainder of the heat would be spent, if not actually in dissociation, in preparing for dissociation. There was one other point in the Paper which he thought of interest. The Author had pointed out how different the rate of propagation of an explosion would be in the case of gaseous mixture which was confined to that in an unenclosed space. Messrs. Mallard and Le Chatelier had made experiments on that point; they had inflamed gas and air mixture in a tube closed at one end, and they found that when it was inflamed at the closed end the rate of propagation was much greater than when it was inflamed at the open end. In the one case the gas was merely burning backwards through the tube, in the other the expansion of the gases would spread the inflammation. So enormous was the difference, that in some cases they found that the rate of propagation was one hundred times greater when the gas was lighted at the closed end of the tube than when it was lighted at the open end. That was a point which strongly confirmed the Author's view—that inflammation spread through the gas almost instantaneously. Although, therefore one could not but feel that on those points there was a great lack of experimental data, all the facts that were brought together might, at present, be best explained by the hypothesis that the inflammation spread very rapidly through the gas, and that at high temperatures, say of over 1,000°, a very large amount of heat was rendered latent, either in actual dissociation or in incipient dissociation. Here, then, was an explanation of the curious maintaining of the temperature to which the Author had referred. As the gas cooled, the latent heat was given up and the curve was thus kept up to a high temperature by the heat previously absorbed in the molecules of the gas. Prof. Rücker.

Mr. JOHN IMRAY observed that the patents for gas-motor engines were at present under litigation, and that was a reason for not entering into details on the subject. He considered the Paper a misleading one. The Author had taken up a line of argument which tended to divert the mind from certain peculiarities of his engine and the Otto engine, peculiarities which were the only ones worthy of being discussed, but which had been passed over *sub silentio.* Hypotheses had been brought forward to account for certain facts where no hypotheses were wanted, the facts being plain and straightforward. He might be permitted to say a few words as to the history of the gas-engine—which he would challenge the Author to controvert. For the last forty years attempts had been made to produce a practical gas-motor engine, but almost all of them had failed. Some of them, no doubt, had come out before their time, others were never fully worked out, but more or less they had all failed. The only ones that came into much use before 1863, were a few made by Lenoir and Hugon, but they turned out wasteful and inconvenient, and in other respects so bad, that they were soon given up, and a large number of the engines made on stock, after ruining the makers, had been converted into steam-engines. In 1863, Messrs. Langen and Otto designed a plan of gas-engine known as their atmospheric engine. He did not say that they had the first idea of it, but they certainly worked it out into a practical form; and many thousands of those engines had been made and were still at work. Their only defect was that they were a little inconvenient on account of the noise, and that their power was limited to 2 HP. or 3 HP. Mr. Otto then set his mind to work at the subject, tried many plans and took out many patents; at last, in 1876, he succeeded in producing the engine which bore his name— Mr. Imray.

Mr. Imray. an engine which had made such an enormous change in the whole business of gas-motors, that, whereas the machine was previously a mere mechanical toy, it had become a great commercial success. Thousands of those engines were at work to the extent of several hundred thousand HP., and they not only compared well with, but they exceeded in many respects, the steam-engine in point of economy as much as in point of convenience. Those were the facts of the history of the engine. The change which Mr. Otto had introduced, and which rendered the engine a success, was this—that instead of burning in the cylinder an explosive mixture of gas and air, he burned it in company with, and arranged in a certain way in respect of, a large volume of incombustible gas which was heated by it, and which diminished the speed of combustion. He would only refer to Figure 9. If the theory of dissociation were true, it would follow that the lower the temperature the more dissociation would take place, which was undoubtedly altogether wrong. He would ask the members to accept with reserve the history and the theory that had been laid before them, until events showed how entirely different the things really were.

Mr. Bousfield. Mr. W. R. Bousfield did not propose to quarrel with the greater part of the facts stated, which were for the most part indisputable, but he thought neither the interpretation which the Author had put upon them could be upheld, nor the new and, to most of them, rather startling theory of the action of the gas-engine which had been submitted in the Paper. He did not say that the phenomena of dissociation played no part in the action of the gas-engine; he did not say that when the explosion took place, there might not be a certain quantity of ammonia and a certain quantity of nitric acid formed, and that the phenomena of dissociation might not take place to a certain extent; but what he did say was that neither the formation of nitric acid nor the formation of ammonia nor any of the phenomena connected with dissociation could account for the facts mentioned. He would only refer to two of those facts, namely, that notwithstanding the enormous loss of heat through the walls of the cylinder of a gas-engine, amounting to 50 per cent. of the total amount of heat put into the cylinder, the curve of the indicator diagram still kept up the theoretical adiabatic line which it should follow, supposing the whole of the gas were burned at the beginning of the stroke, and the walls of the cylinder were non-conducting. That was a startling fact which had to be dealt with in one way or another, but the interpretation of the fact seemed to him to be very

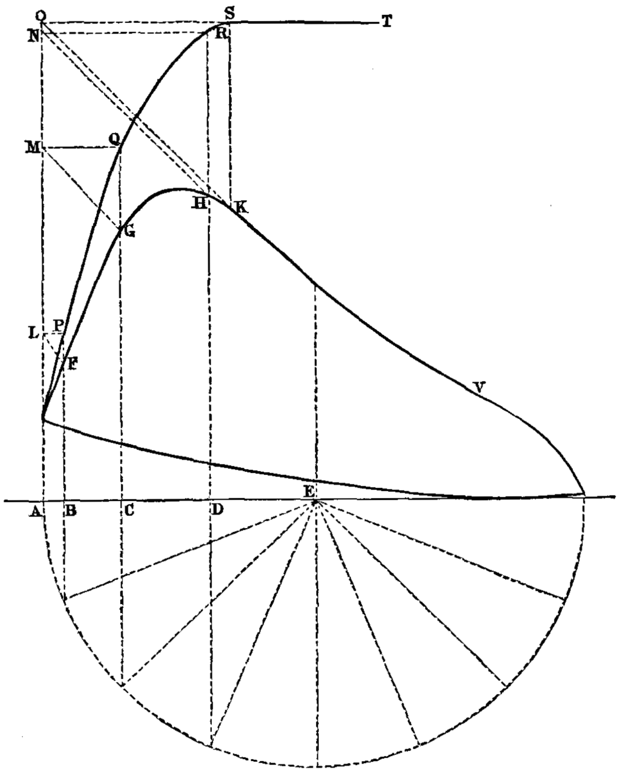
Mr. Bousfield.

simple, and even in the Paper there were materials for arriving at a conclusion upon it. The Author had stated that a mixture of gas and air took a certain time to ignite, that if ignition was set up at one point it took a certain time before it was communicated to another. There was also the further fact that the rate of communication of the ignition from one point of the dilute mixture to another varied directly with the amount of dilution of the mixture. Supposing for instance there was a mixture of gas and air in the right proportions for explosion, the ignition would take place at a certain speed; if more air was put in, the rate would be less; the greater the quantity, the less the rate at which the ignition travelled. That simple fact he thought sufficient to account for all the phenomena. The diagram which the Author had given (Fig. 9) seemed to him, taken in conjunction with the fact to which he had referred, to support the theory which had been put forward by Mr. Otto and by the scientific world in general. In the Otto gas-engine the charge varied from a charge which was an explosive mixture at the point of ignition to a charge which was merely an inert fluid near the piston. When ignition took place, there was an explosion close to the point of ignition that was gradually communicated throughout the mass of the cylinder. As the ignition got further away from the primary point of ignition the rate of transmission became slower, and if the engine were not worked too fast the ignition should gradually catch up the piston during its travel, all the combustible gas being thus consumed. When the engine was worked properly the rate of ignition and the speed of the engine ought to be so timed that the whole of the gaseous contents of the cylinder should have been burned out and have done their work some little time before the exhaust took place, so that their full effect could be seen in the working of the engine. This was the theory of the Otto engine. What was the theory which the Author had put forward? He had stated that when gases combined a high temperature was set up; that a high temperature prevented combination of the gases beyond a certain point; and therefore, at the moment of ignition, there existed in the cylinder a body of gases heated to a temperature beyond the point of dissociation. A part of those gases being in a state of combination, and having therefore given out a heat which was doing the work of pushing the piston; a part of the gases, not being in a state of combination, being ready to combine as soon as the temperature was lowered to such a point that they could combine and give out work. Looking at that theory, it

Mr. Bousfield. seemed as if the point involved was a mere question of words, so far as regarded any question of infringement. In either case, what had to be dealt with was this. The adiabatic line represented the line which was traced out upon the indicator diagram when no heat escaped through the walls of the cylinder, and when the whole heat which the gases lost was converted into work done by the piston; so that, taking an indicator diagram, and finding the work done as represented by the area included by the curve, the ordinates and the atmospheric line, this work ought to be equal to the quantity of heat, represented in foot-lbs., which had been given out by the gas, as shown by the difference of temperatures and specific heat of the gas. Of course, when heat was escaping through the cylinder, and when the adiabatic line was still kept up to, a considerable amount of energy must be developed somewhere, in order to make up for the energy which went through the walls of the cylinder. The only source of energy in the gas-engine was the union of combustible gases and oxygen, and it followed that that constant supply of energy must come from the combustion of the gases within the cylinder. It was therefore a mere question of words, because, whether the energy was developed by the combustion of the gases which took place through the lowering of the temperature below the point of dissociation, or whether that energy was given out through the combustion of the gases which took place from the communication through the mass of an ignition which travelled slowly through it, in either case it was a gradual combustion. It was therefore a mere question of theory, and he did not see in what way it could affect the question of infringement. If Messrs. Crossley and Mr. Otto had overlooked the theory of dissociation, and had attributed the gradual combustion to something which they ought not to have attributed it to, he did not see how it could affect their position. The real point of difference, however, in a scientific point of view, between the Author and himself was this. The Author assumed that the ignition was quickly transmitted through the cylinder, and took place almost at once near the beginning of the stroke, and that the ultimate combustion was due to dissociation; whereas Mr. Bousfield thought with Mr. Otto and many others that the cause of the supply of energy was the gradual communication of ignition through the contents of the cylinder. The Author assumed gratuitously that when the point of maximum pressure was reached, that point marked the communication of ignition throughout the whole of the cylinder. That there was absolutely no ground for that assumption could be very readily shown.

Neglecting for the moment the loss of heat through the walls of the cylinder, the curve representing the increase of pressure due to the combustion of the gas, supposing the gases to combine at the same rate as they actually did, but not to be allowed to expand by the motion of the piston, could be ascertained thus:—Divide the atmospheric line (Fig. 14) into spaces A B, B C, C D, D E, &c., representing equal small spaces of time, or equal parts of a revolution. Mr. Bousfield.

FIG. 14.

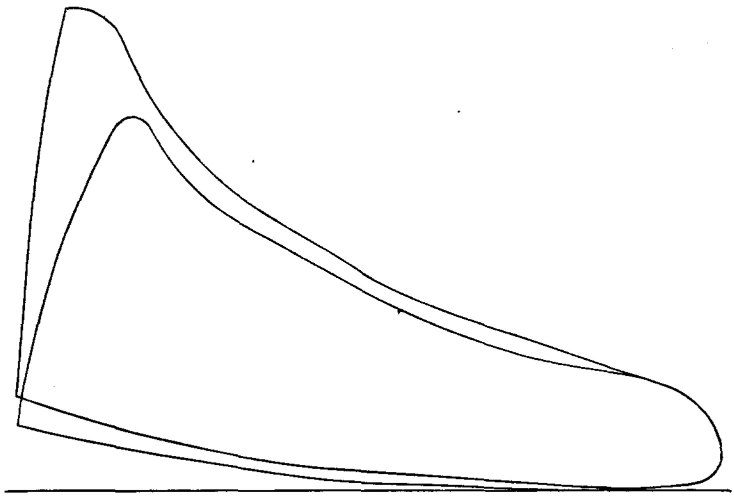


From each of the points A, B, C, &c., raise ordinates A L, B F, C G, &c., to meet the indicator curve in the points F, G, H, &c., and from the points F, G, H, &c., draw adiabatics to meet A L in L, M, N, &c. From L, M, N, &c., draw lines parallel to A B to meet their corresponding ordinates in P, Q, R, &c. Then the curve P, Q, R, &c., drawn through these points, would be a curve, the ordinates of which were proportional to the pressure at any time

Mr. Bousfield. of the contents of the cylinder, supposing these contents to remain confined in the space at the end of the cylinder, and not allowed to expand, and supposing the rate of combustion of these contents to be exactly the same as actually occurred. This curve, therefore, showed the actual progress of the combustion deduced from the working diagram. Even neglecting the loss of heat through the walls of the cylinder, it would be seen that this curve ascended to a point past the point of maximum pressure, viz. till the point K, at the commencement of the part K V (which was supposed to be exactly adiabatic) was reached. From the point S this curve became in the actual diagram a straight line parallel to A B. If, however, the theoretical diagram, allowing for loss by conduction, were taken, the curve P Q R S would ascend throughout the stroke. Hence the maximum point on the diagram was simply the point where the increase of pressure due to combustion was balanced by the decrease of pressure due to the forward motion of the piston, and there was no reason for saying that this maximum point corresponded to complete ignition. He had had an opportunity of taking diagrams from the Otto gas-engine, which Professor Ayrton had at the City Guilds Technical School, Cowper Street. The engine was designed for the electric light, and the cam, controlled by the governor, was made in a series of steps. He therefore had the governor taken off, and the cam and the roller on which it acted so arranged that it should work independently of the velocity of the engine on a given step, so that the charge might be, as nearly as possible, the same at all speeds. And he varied the load by braking the fly-wheel. The two sets of diagrams were taken, one at a speed of one hundred revolutions, and the other at two hundred; thus might be seen the effect which must be due to the phenomenon he had spoken of—the ignition travelling gradually; it could not be due to dissociation, for the reason which Mr. Imray had pointed out. In the diagrams the phenomena of dissociation ought to be exaggerated at the higher temperature, but instead of that, it would be seen that the effects attributed to dissociation were less at the higher temperature where dissociation should be most active, and greatest at temperature below the point of dissociation; he therefore did not see why the results should be attributed to the phenomena of dissociation, when they could be perfectly explained by the rate of progress of ignition through the cylinder. With the full charge at one hundred and at two hundred revolutions the effect of difference of speed was small, as shown by the two diagrams in Fig. 15. In that case, the rate at which the ignition went through

the cylinder was so great that it only made a very little difference Mr. Bousfield.
in the curve when the rate got up to two hundred revolutions. He then fixed the roller on the third step, when there was a less charge

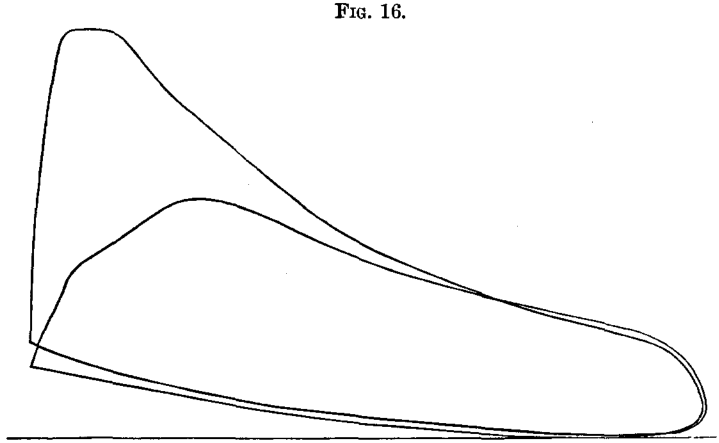
Fig. 15.



4th step. 100 to 200 revolutions.

of gas. The diagram, Fig. 16, showed the hundred-revolution curve, in which the gas had time to explode, and to carry the pencil indi-

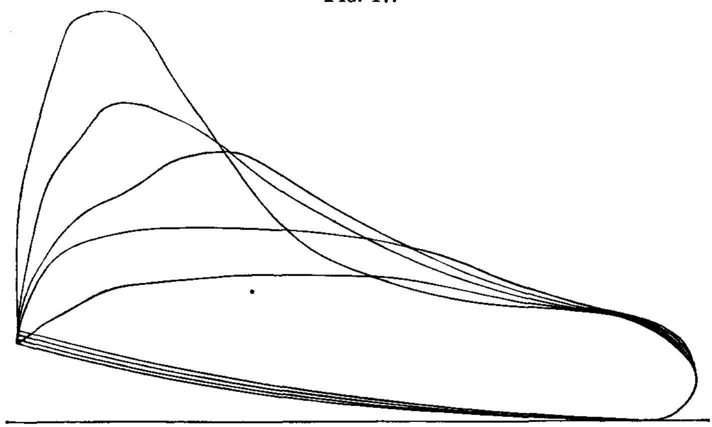
Fig. 16.



3rd step. 100 to 200 revolutions.

cator up to the maximum point, and then down to the adiabatic line. Going to two hundred revolutions with the more dilute mixture, Mr. Bousfield. the rate of propagation of ignition was slower; therefore at that speed, although the temperature was less, dissociation would have much more to do. The effect was much more marked, simply from the dilution of the mixture; there was therefore a less rate of propagation of ignition, and the curve took the form shown in the diagram. Fig. 17 showed the same effects on the diagram when

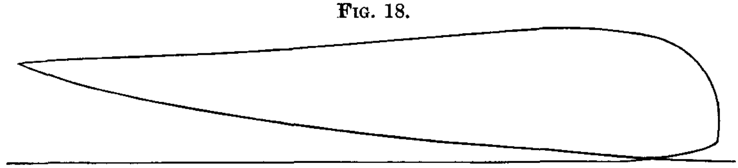
FIG. 17.



2nd step. 100 to 200 revolutions.

the curve roller was on the second step, and consequently still less gas was admitted. The five superposed diagrams were taken at speeds between one hundred and two hundred revolutions per minute. It would be observed that the curve at the higher speed generally went outside the other. There was less work done at the beginning, and more gas to be combined at the end, and therefore a greater amount of work done at the end of the stroke. He did not wish to carry the comparison all the way through, but he would leave it to the Author to show how he explained the diagrams under the dissociation theory. In Fig. 18 there was

FIG. 18.



1st step. 130 revolutions.

the least amount of gas with which the engine would work, and the speed was one hundred and thirty revolutions. The compression

Mr. Bousfield.

was 30 lbs.; the compression-line was the same as the others. The working-line was a line nearly parallel with the atmospheric-line, but slightly rising, and at the end the ignition was not finished, indeed, in this case, if a light was applied to the exhaust the contents would explode. According to the Author's theory, that maximum point near the end of the stroke in the last diagram was a point where the ignition was complete, and therefore all the gas should have combined at that low temperature where no dissociation could take place. Those were points which the Author would have to meet in order to support his theory. Many of the facts mentioned by the Author were incontestable, and his chief dispute with him was as to the interpretation he had put upon them. The Author had said nothing against the theory to which he had referred except that it was new, no argument whatever being advanced against it. The Author stated—"From the considerations advanced in the course of this Paper, it will be seen that the cause of the comparative efficiency of the modern type of gas engines over the old Lenoir and Hugon engine is to be summed up in one word, 'compression.'" He had not had time to go carefully through the diagrams; but he did not think that they were fair comparisons, and he thought that other elements ought to have been taken into account. The Author had given the old Lenoir, and had stated that the temperature was the same, that the mixture of gas was the same, and that the great advantage over the Lenoir was compression. Mr. Bousfield might be permitted to point out that, in the Lenoir engine, the adiabatic line was much above the actual line. It would be fairer to substitute the word "dilution" for "compression," so that the sentence would read: "The cause of the comparative efficiency of the modern type of gas-engines over the old Lenoir and Hugon engine is to be summed up in one word, 'dilution.'" The fact, however, was that it could not be summed up in one word; the two should be taken together, compression and dilution. The Author further stated: "The proportion of gas to air is the same in the modern gas-engine as was formerly used in the Lenoir." He did not think so. He believed that the Lenoir worked up to 13 to 1, and could not get further. He did not know what proportion Otto used, but it was considerably more than that. It was also stated that the time taken to ignite the mixture was the same; but that was a gratuitous assumption. The Author said: "The cause of the sustained pressure shown by the diagrams is not slow inflammation (or slow combustion as it has been called), but the dissociation of the products of combustion, and their gradual combination as the temperature falls, and com-

Mr. Bousfield. bination becomes possible. This takes place in any gas-engine, whether using a dilute mixture or not, whether using pressure before ignition or not, and indeed it takes place to a greater extent in a strong explosive mixture than in a weak one." Dissociation took place far more at high temperatures than at low; and if the Author's application of the theory were correct the phenomena of dissociation ought to play a much greater part at high than at low temperatures. He had pointed out that this was not so in the diagrams, and that it was not so with Lenoir's explosive engines where the curve fell far below the adiabatic line. The Paper contained other matters which he had not time to dwell upon; but he thought he had said enough to challenge the Author to show how he got rid of the old theory, and explained the facts to which Mr. Bousfield had referred.

Dr. John Hopkinson. Dr. JOHN HOPKINSON said a very interesting question had been discussed by Professor Rücker and Mr. Bousfield, to which he desired to refer. The Author maintained that the ignition of the mixture of gases had extended throughout the whole space at a time approximately represented by the point of maximum pressure. Others, on the contrary, maintained that the ignition had not extended through that space by that time, but that it took a time lasting into the descending part of the indicator diagram before the disturbance had extended throughout the whole of that space. The Author attributed the maintenance of the temperature during the latter part of the curve, and its approximation to an adiabatic curve, to the gradual combination of the gas through the mass, that combination not occurring completely in the first instance owing to the temperature being so high that a certain measure of dissociation occurred, or at all events so high that complete combination could not occur. He thought that the question might be submitted to a crucial test. Suppose the opponents of the Author were right, if a given mixture of air and gas were exploded in a gas-engine revolving at a low rate of speed or in an entirely closed space, it would be expected that the maximum pressure would approximate to that calculated from the heat due to the combustion of the gas present and the temperature resulting therefrom. If the engine were running slowly, or if the explosion were made in a completely confined space, the pressure would be expected to rise to a point very greatly in excess of that observed in the gas-engine running at its normal speed. Whether that were so he did not know. The experiment might be objected to on the ground that when the engine was running slowly there was a great loss of heat through the walls of the cylinder. That would give rise to a second crucial experiment. If the Author was right the maximum pressure in large and small engines would be about the same; if those who differed from him were right, in a large engine the maximum pressure would probably be greatly in excess of that in a small engine, there being less loss of heat through the walls of the cylinder. What the answer might be he did not know, but it appeared to him that there were there the elements of settling the question. The Author divided gas-engines into three classes, and had made a comparison of their theoretical efficiency. In the second the mixtures were admitted into the cylinder, and, without increase of pressure, the heat produced was devoted to increase of volume. In the third the mixtures were introduced into the cylinder, and then burned with an increase of pressure without immediate increase of volume; and in those two cases he took, for the purpose of comparison, different maximum pressures. In the second type he took a pressure of 76 lbs., and in the third over 200 lbs. *Primâ facie* it would seem natural, in order to make a fair comparison, that the same maximum pressure should be taken in the two cases. Probably the Author had a good reason to justify his making a comparison on that basis, and, perhaps, in his reply he would point it out. He agreed with those who had so often spoken on the subject of the gas-engine that in that engine lay the future of the production of power from heat of combustion. It was quite in its infancy, and it had already beaten the best steam-engines in economy of fuel, for the obvious reason that it was practicable to use with it much higher temperatures. The steam-engine tolerably approximated to the theoretical efficiency that might be expected from it, having regard to the temperatures between which it was practicable to work it. That was not the case with the gas-engine, there being still a very large margin for practical improvement. Having regard to the very short time during which gas-engines had been used, he thought that practical improvements would take place, and that, when such difficulties as that of starting a large engine as conveniently as steam-engines could be started had been overcome, the gas-engine would supersede the steam-engine. Dr. John Hopkinson.

Mr. F. H. WENHAM observed that he had but little to say with regard to the present theory of the gas-engine, but he wished to make some practical references to one of the three types of engine referred to in the Paper. He had tried many experiments with heated air or gas-engines, and had come to the conclusion that for economy and efficiency they surpassed steam-engines for small powers. In engines made by himself some years ago the air was Mr. Wenham.

Mr. Wenham. pumped into a closed chamber which contained a coal fire, or in which petroleum was ignited by being injected on to an incandescent surface, and the expanded air was then conveyed through a passage and valves into the working cylinder, which was single-acting. A great many of these engines were made at the time, but there were practical difficulties in the way of construction. It was found that the engine would not endure for any length of time if the initial pressure in the cylinder exceeded 15 lbs. per square inch. That, after deducting the power absorbed by the air-pump, left a mean pressure of only 6½ lbs. during the stroke. Under these conditions the air entered the cylinder at 1,100° Fahrenheit, equivalent to iron at a dull red heat, and left at the exhaust at 466°, or a few degrees above the melting point of tin. The reason why only a low pressure could be obtained in that form of engine was the difficulty of keeping the valves and joints of the communicating passages air-tight at temperatures above the dull red heat. At its first introduction there was a considerable demand for the engine; but it had since been superseded by the modern gas-engine. In the gas-engine the cylinder itself was the heat generator or furnace, and there were thus no valves or passages for the intensely ignited air to pass through, and that source of destruction was avoided; consequently very high temperatures could be utilised and corresponding high pressures obtained. The high temperature of the air at the time of the combustion of its self-contained fuel in the form of gas had given pressures in the cylinder at the commencement of the stroke of 200 lbs. per square inch at a temperature of 3,000° Fahrenheit. During the stroke there had been a mean pressure of 70 lbs., with the exhaust air leaving the cylinder at a temperature of 1,300° Fahrenheit, a higher degree than that at which it could be allowed to enter in the air-engine to which he had referred. It appeared to him that this must be a source of great loss. In future improvements of the gas-engine he believed that such waste heat would be utilised as a motive power, as this could be effected by a simple arrangement. Of course it would be desirable to make the gas-engine double-acting, and he had no doubt that this result would also be accomplished.

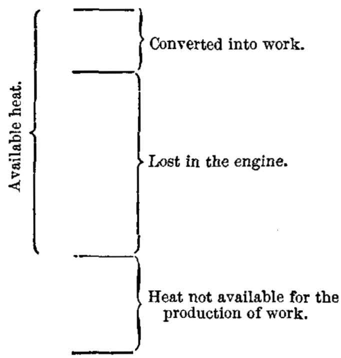
Mr. Davey. Mr. H. DAVEY remarked that the Author had thrown new light on the chemical part of the subject, by explaining the action due to the dissociation of gases, a branch of the subject probably but little understood by inventors and gas-engine manufacturers generally. The Author started with the assertion that the steam-engine only gave an efficiency of 0·1, and then proceeded to show that the efficiency of a gas-engine, using 22 cubic feet per IHP. per hour, was by experiment 0·178. Mr. Davey.

In round numbers, 1 lb. of coal yielded $4\frac{1}{2}$ cubic feet of gas, and a good steam-engine consumed 2 lbs. of coal per IHP. per hour. It therefore followed that neglecting the by-products, the steam-engine used an amount of coal per IHP. per hour which would yield 9 cubic feet of gas, whilst the gas-engine used 22 cubic feet, or an equivalent of nearly 5 lbs. of coal. That was the practical statement of the case; but looking at it theoretically, it would be seen that the facts were still against the Author's deductions, and that the sweeping last paragraph of his Paper was not quite so deserved as it would appear. For small powers the gas-engine was superior to the steam-engine, for many practical reasons.

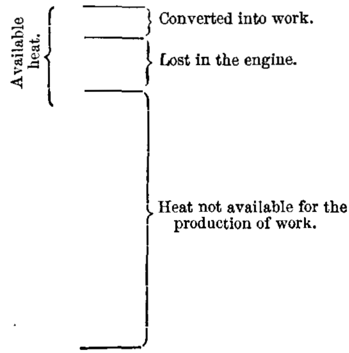
The efficiency of a heat-engine was not the actual work done divided by the mechanical equivalent of the total heat, but the work done divided by the mechanical equivalent of the available heat. The Author had recognised that fact in dealing with the gas-engine, but in stating that the efficiency of the steam-engine was only 0·1, he had forgotten that that value was obtained from the total and not the available heat. The possible efficiency in a steam-engine working between the limits T and t, = say 760° and 560°, was $\frac{760 - 560}{760} = 0{\cdot}26$. The complete combustion of 1 lb. of coal = 16,000 units, and therefore the available heat was 16,000 × 0·26 = 4,160 units, equivalent to 3,211,520 foot-lbs.; 1 lb. of coal produced $\frac{1}{2}$ an indicated HP., or 990,000 foot-lbs. The efficiency therefore was $\frac{990{,}000}{3{,}211{,}520} = 0{\cdot}3$, or three times that assigned to it by the Author. Calculating the efficiency of the Author's gas-engine on the same basis, the result was $\frac{3{,}258 - 762}{3{,}258} = 0{\cdot}76$ as the possible efficiency. The complete combustion of 2 cubic feet of gas produced heat equivalent to about 1,100,000 foot-lbs. The available heat in a perfect gas-engine, expressed in foot-lbs., was therefore 1,100,000 multiplied by 0·76, which was equal to 836,000 foot-lbs.; but the actual work done by 2 cubic feet of gas in the Author's engine was only 180,000 foot-lbs. The efficiency was therefore $\frac{180{,}000}{836{,}000} = 0{\cdot}21$.

The actual efficiencies were therefore—the steam-engine 0·30; the gas-engine 0·21.

Mr. Davey. The following diagrams of the distribution of losses would make this fact clear :—



GAS-ENGINE.



STEAM-ENGINE.

The gas-engine was a long way behind it, except for small powers. The cost of working was the crucial test. Let coal cost 18*s.* 8*d.* per ton, and gas 3*s.* 9*d.* per 1,000 cubic feet; then 10 lbs. of coal would cost 1*d.*, and 22 cubic feet of gas, 1*d.*; 10 lbs. of coal = 5 IHP. in the steam-engine, and 22 cubic feet of gas 1 IHP. in the gas-engine; therefore the cost of fuel was 5 to 1 in favour of the much-abused steam-engine. With Dowson gas, probably the gas-engine was as cheap in working as the steam-engine; but that was due to economy in the working fluid and not in superior efficiency of the engine itself. Increased economy might be obtained in the steam-engine when the practical difficulties of using high temperatures were surmounted.

The following graphic illustration of the theoretical economy of high temperatures was useful in conveying a clear idea of possible efficiency of any heat-engine :—

Degrees absolute.

	520°	1040°	1560°	2080°	2600°	3120°	3640°
Absolute zero.	0	$\frac{1}{2}$	$\frac{2}{3}$	$\frac{3}{4}$	$\frac{4}{5}$	$\frac{5}{6}$	$\frac{6}{7}$
	60°	580°	1100°	1680°	2140°	2660°	3180°

Possible efficiencies.

Degrees Fahrenheit.

Summary.

Mr. Davey.

——	Steam.	Gas.
Efficiencies as heat-engines	0·30	0·21
Consumption of coal per IHP. per hour neglecting by-products	2 lbs.	5 lbs.
Cost of fuel per IHP. per hour including by-products, the sale of which enabled the gas to be sold at the price taken	0·2*d*.	1*d*.
Cost of fuel, the gas-engine using Dowson gas, probably	0·2*d*.	0*s*. 2*d*.

Sir Frederick Bramwell.

Sir FREDERICK BRAMWELL, Vice-President, said he had not intended to speak on the subject, but was now induced to do so as he thought the last speaker's remarks were misleading; as he (treating the residuals as not worth consideration) had put forward that the true mode of comparing the economy of a gas-engine with that of a steam-engine was to charge against the gas used the whole of the coal carbonised to produce that gas, thus ignoring the heating value of the coke, and the money value of the other residuals.

Mr. Bamber.

Mr. E. F. BAMBER wished the Author had commenced his Paper with that portion which treated of the analysis of the gas, and had given the mechanical equivalent of a unit of the same both in the pure and diluted state. If the explanation had then followed, that the mechanical equivalent of the latent heat of expansion per unit of the gaseous mixture per degree of temperature was nearly the same as for atmospheric air, the reason why the gas-engine might be considered in theory as an air-engine would have been clearer, namely, that the adiabatic curve, or curve of no transmission of heat, was nearly the same for both. The Author commenced by an attack upon the steam-engine. Much heat was required in evaporating water whose specific heat was high, and hence the efficiency of the steam-engine was low, and something better was needed; whereas it was clearly proved by Rankine, a quarter of a century ago, that the maximum efficiency of a theoretically perfect heat-engine, working between given limits of temperature, was equal to the ratio of the range of temperature to the higher absolute limit of temperature, and quite independent of the fluid employed. Raising the temperature entirely by compression or using regenerators were the two means by which the actual efficiency might be made to approach the maximum limit. The Author believed in compression, but his

Mr. Bamber. method of defence of it and his illustrations of its advantages did not appear to be quite correct. He took three types of engine: the first and third were explosive gas-engines; the second was worked at constant pressure, and these he treated as air-engines. The first and second were worked between the same limits of temperature, but in the second compression was employed. What the Author wished to prove by the theoretical diagrams of these types was that the constant-pressure engine using compression was more theoretically perfect than an explosive-engine using none, whilst an explosive-engine using compression was the best of the three. But he had shown by type No. 2, that by the use of compression an efficiency could be attained higher than the maximum efficiency of a perfect heat-engine, which seemed to require some explanation. The maximum was equal to $\frac{\tau_1 - \tau_2}{\tau_1}$ in absolute degrees of temperature, and was for 1,537° Centigrade and 1,089° Centigrade equal to 0·247 for both types; whereas the Author made it 0·21 for the first and 0·36 for the second. The Author allowed that type No. 1 would be improved by further expansion, but that that would require a vacuum-pump and condenser; yet surely it made no difference, so long as they both consumed the same quantity of heat, whether a compression-pump was used at the beginning or a vacuum-pump at the end of the stroke, whilst indeed there might be theoretical reasons in favour of the latter. Types 1 and 3 were respectively worked without and with compression; they were both explosive-engines, and the efficiency of the latter was made double that of the former, but the latter was made to discharge at 648° Centigrade, and the former at 1,089° Centigrade. If these figures had been reversed, so would have been the efficiencies. Had the Author explained that there was a certain maximum efficiency for heat-engines, and that by means of compression a larger percentage of that maximum could be attained than without it, there would have been no reason for objection; but that was a very different thing from trying to show that it was possible to obtain more than the maximum efficiency of a theoretically perfect heat-engine.

The real value of the gas-engine was, that it contained the furnace and engine in one; thus the necessary heat lost in the furnace to make a draught, and the unnecessary loss of heat by radiation from a large steam boiler were both avoided in the gas-engine, and, finally, the gas-engine could be used safely at a maximum limit of temperature, which could not be employed in the steam-engine. There was no doubt a great future for this class of motor. Mr. Bamber.

Mr. E. A. Cowper had been rather startled by the assertions of some of the speakers, that it was worth while to mix air and gas in such an imperfect way, that combustion should not take place in the best manner. If, instead of mixing air and gas at the commencement of the stroke, a portion of the gas was let in at a later period of the stroke, it reminded him of an indicator figure he had taken off a steam-engine many years ago, where the admission was so bad that, after the steam had been admitted and was partly expanded and wire-drawn down, a second volume of steam was admitted to the cylinder, which simply filled it up to a higher pressure without doing any work, though the expansion of that second quantity of steam became useful for the small portion of the remainder of the stroke. If the gas and air were not properly mixed, and the whole let in and exploded at once, or as quickly as possible, a portion of the work that ought to be done was lost, because it was not burned at the highest temperature at the commencement of the stroke; there was not the first motion due to the high pressure, and then there was not the use of it for expansion. As to the second point, working with atmospheric pressure, and allowing the gas to be drawn into the cylinder, he agreed with the Author that gas and air, the explosive mixture, ought to be forced into the cylinder with some reasonable pressure, he did not say what, perhaps 20, 30, or 40 lbs. per square inch. When he was an apprentice to the late Mr. J. Braithwaite, Ericsson was working at his caloric-engines, and he found that by pumping air into the engine to begin with, so as to have two or three atmospheres, he could get much more work out of the same coal, and make the engine in every way more efficient, besides enabling a small engine to do the work of a large one. It was likewise decidedly advantageous to press the gas and air into the cylinder in the first instance. An indicator figure like the Author's (Fig. 6) was the best he had seen from a gas-engine. It gave the highest reasonable pressure at the commencement of the stroke, 200 or 220 lbs., and a very fair expansion curve. He thought that whatever theory might be adopted, an indicator figure would have to be taken, to see what was actually done in the cylinder, and the mechanical arrangements be varied so as to obtain the best results. He did not think that any theoretical calculations would give a better curve than could be obtained by actual experiment. He wished to ask the Author the cost per HP. 1 HP. could now be got with a good steam-engine for $\frac{1}{10}d.$ per HP. Mr. Cowper.

Mr. Cowper. per hour for fuel, or 103 HP. per hour for 1*s.*, and he should like to know the actual results obtained by the Author with a gas-engine.

Mr. Gray. Mr. J. MACFARLANE GRAY said that the deficiency of combustion at the commencement of the stroke had been calculated from the temperature 1537° Centigrade by assuming the specific heat constant as for low temperatures. The isochronous vibrations of the atoms in the glowing state was a set of motions additional to those which were taken into account in the specific heat constant at low temperatures, and the energy of these vibrations must be also additional to that of constant specific heat. The Author's estimate of the extent to which ignition took place before the maximum temperature would therefore be below what actually occurred. The glowing state or inflammation preceded and also followed ignition. The great difference in the forms of the diagrams taken from the Crossley engine at different speeds was, he thought, in accordance with the *rationale* of retarded ignition attributed to Mr. Crossley. He thought that even the Author's theory also remarkably corroborated this view of the subject of retarded ignition. Mr. Crossley had shown that his arrangement was one which ensured that ignition would be slow, and the Author had now brought in another argument to show that ignition could not possibly take place faster than Mr. Crossley had stated. With regard to the practical economy of the gas-engine, he knew of one instance where gas-engines had been adopted for a printing works up to 80 actual HP. An engine of 8 HP. nominal was taken first, then another engine of the same size, and these gave so much satisfaction that a 16 nominal HP. engine was ordered, and that had since been duplicated, working each to 30 HP. actual. Mr. Crossley in this case himself advised a steam-engine after the two small engines, but the user of the power had from his own experience formed a higher estimate of the economy of the gas-engine than its maker would advocate for it. All these engines had worked well, the cost of repairs had been practically nil, and the firm were still satisfied that they got power economically, compared with a steam-engine which they had had at former works, although coal was not high-priced in that district.

Mr. Severn. Mr. H. A. SEVERN said that in his own experiments the explosion of the gas in open and closed tubes varied with the temperatures and pressures. He was inclined to believe that the true curve would be obtained by finding out the best temperature and pressure which were together conducive to the best explosion. He did not think that the maximum explosion was due to temperature or to pressure separately.

Mr. J. E. Dowson observed that much had been said on the question of dissociation. He ventured to point out that in the experiments of St. Claire Deville the gases acted upon were very small in volume and were not confined under pressure, whereas the gases in the cylinder of a gas-engine, after the first ignition had taken place, were under very considerable pressure in a closed chamber. There was doubtless a range of temperature within which two bodies having a chemical affinity for one another would combine, and when that temperature was raised or lowered the combination would not take place. But surely the tendency of gases to be dissociated or to remain uncombined was affected by a pressure which was exerted on those gases, and which directly tended to facilitate their combination. Seeing that in his experiments St. Claire Deville required a temperature of 1,200° Centigrade to dissociate a very limited amount of oxygen from hydrogen,. when experimenting on a small scale, and that the maximum temperature obtained by the Author was only about 300° above that of St. Claire Deville; seeing also that the latter's experiments were made at atmospheric pressure, whereas in the Paper no reference was made to the counteracting effect of increased pressure on dissociation, he should be glad if the Author would state if he had taken that into account, and if so what were his reasons for supposing that it should not modify the theory which he had advanced. He should also be glad if the Author would state if he had tested any samples of gases taken from the exhaust or from the cylinder at one or more points. The results of such tests would bear directly upon the question of complete or partial combustion, and would be of great interest. The question raised as to dissociation and its supposed effects was certainly one of importance, and should be set at rest in the most convincing manner possible. It was, however, so difficult to determine the reactions which occurred in the cylinder of a compression-engine when worked with coal-gases, owing to the presence of so many kinds of gases, and owing to the complication of conditions which obtained, that it would assist the solution of the problem, and be instructive in other respects, if a few experiments were made with a compression-engine worked with a simple gas such as hydrogen or carbonic oxide mixed with air. Mr. Dowson.

Sir William Armstrong, C.B., President, said the Paper was a very important one, and he was glad that it had been brought before the Institution. The subject also involved a great deal of controversial matter, and some valuable and interesting remarks had been made on the various controverted points. But setting Sir William Armstrong.

Sir William Armstrong. aside all matters of controversy, he might say that the gas-engine was undoubtedly, theoretically speaking, a very superior heat-engine to the steam-engine. In the first place, there was in the gas-engine internal combustion, and therefore all that loss was avoided which was incident to the transmission of heat from the exterior of the apparatus to the interior. There was also the fact that heat communicated to air was in a much more available form for the production of mechanical effect than heat communicated to water. But there was very much yet to be done to put the gas-engine in a position to supersede the steam-engine. It appeared to him that the difficulties in the way applied more to the generation of gas than to its application in the engine. It was very difficult, for example, to see how to generate gas for motive power in a locomotive with the same facility that steam could be generated. In the case also of ships it would apparently be very difficult to substitute gas for steam. At the same time it should be considered that the gas-engine was only in its infancy, whereas the steam-engine was in a great measure played out. Comparing the gas-engine with the steam-engine at a period when it was no older than the gas-engine, the superiority of the gas-engine would be much more marked than when comparing it with the mature steam-engine of the present day. And it was highly probable that after some experience had been had of the new form of engine it would appear in a very superior light. It was at present very heavily handicapped by having to use gas which was not specially prepared for it; it was burdened with the expense of rendering gas available for illuminating purposes. No doubt the time would soon come when gas would be made for the special purpose of producing motive power, and when that was the case the advantages of the gas-engine would be greatly enhanced.

Sir William Thomson. Sir WILLIAM THOMSON said that he had recently seen a very interesting experiment made by the Author with a gas-engine at Glasgow, which he thought had a most important bearing on the mode of action of the gas in the cylinder. The experiment was made in the presence of his brother Professor James Thomson and Professors Jack and Ferguson (of Mathematics and Chemistry in the University of Glasgow), who were all much interested in the enquiry. The object was to test the nature of the mixture in close proximity to the piston, so as to be able to form some idea as to whether or not the explosion took place through the whole space; to be judged by finding whether, right up to contact with the piston, gas and air were present in proportions suitable for combustion. He need not enter into details as to the way

Sir William Thomson.

in which the experiment was made, but he might say, in a general way, that while the piston was being pressed in to condense the mixture at a definite point of the stroke, communication was made with the cylinder. The small experimental cylinder and piston were placed in proper position, in communication with an aperture bored for the purpose in the main cylinder. The Author of the Paper would be able to explain the details better than Sir William Thomson could. It was sufficient to say that by an automatic arrangement, worked mechanically from the cross-head, the communication was made exactly at one definite point of the stroke, and the experimental piston was pressed up in the cylinder so as to let it fill. At any time afterwards the stop-cock could be opened by hand, and the nature of the contents tested. In every case the contents were found to be explosive—an explosive mixture of gas and air —proving that up to the very point, which he understood was within about an inch from the piston, coal gas was present in suitable proportions for producing an explosion. There was one other matter to which he wished to refer, which had been noticed in the discussion. There appeared to be some difference of opinion upon it, but to his mind it scarcely appeared open to doubt—that the diagram, which showed an exceedingly sudden rise and a gradual fall, proved that combustion was practically complete at a point corresponding to the summit of the curve. Literally and precisely the instant of the maximum of the curve was that at which the rate of loss of pressure by expansion, the much smaller rate of loss of pressure by loss of heat carried by convection of the fluid to the solid boundary, and out by conduction through the metal, were exactly counterbalanced by the rate of combustion still going on. It seemed certain that the rate of loss by the two causes he had indicated was exceedingly small in comparison with the rate of rise by the initial progress of the explosion; therefore, practically speaking, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation at the highest temperature attained allowed it to be.

Mr. Clerk.

Mr. D. Clerk, in reply upon the discussion, said that two of the speakers seemed to think that the question at issue was one of infringement of patent, but he desired to arrive at the truth, apart from mere questions of personal interest. The question of infringement was to him one of complete indifference.

The question he was anxious about was the purely scientific one. Was his theory of the action of the gas-engine the true one, or was

Mr. Clerk. Mr. Otto's? This matter might appear to some persons a small one, but he considered it of vital interest, being convinced that not many years hence the gas-engine would have a science of its own, and scientific names connected with it as much honoured as any ever linked with the steam-engine. Dr. Siemens had fully corroborated his view of dissociation, and in the effect it had on the gas-engine diagram, in preventing the more rapid fall, which must otherwise occur; but he did not agree with him in the necessity for further research on dissociation, believing that St. Claire Deville's work was sufficient. Dr. Siemens would observe that St. Claire Deville's researches were referred to in the Paper; but what he asked for had never to his knowledge been published, that was a complete curve of the dissociation of water and carbonic acid. St. Claire Deville's results were more of a qualitative than of a quantitative nature. He feared that the method used was not capable of the necessary accuracy.

He thoroughly believed that the engine for the very large powers to be constructed in future must be of one type 2, with hot chamber or cylinder, and regenerative contrivance in some form; indeed, about two years ago he constructed and experimented with such an engine, and he was continuing his experiments.

The mechanical difficulties were much greater than in the cold-cylinder, type 3. It must be remembered that the cold-cylinder gas-engine was the engine of the present, and it was most satisfactory that even with small sizes so high a duty should be obtained. It proved that when larger engines were made a much higher duty might be expected. The theory of the cold-cylinder engine did not allow of the application of any regenerative contrivance, and consequently arrangements must be made to get the greatest possible fall of temperature due to work done. A very interesting account had been given by Professor Rücker of his view of the problem, and the necessity of correcting the calculations of previous observers in the light of present knowledge of the laws of combustion had been demonstrated. It was satisfactory that Professor Rücker so thoroughly agreed with him on the necessity for considering dissociation in any theory of the gas engine, and had independently arrived at similar conclusions. The experiments of Messrs. Mallard and Le Chatelier corroborated those of Professor Bunsen in this, that at the high temperature of combustion a large amount of heat was rendered latent. So striking a fact could hardly have escaped the notice of many other experimenters who might not have published their results. He had noticed it about five years ago, while making experiments on the maximum pressure obtainable from a pure explosive mixture of gas and air. A cylinder 9 inches in diameter and 9 inches long, was filled with a mixture of gas and air in the proportions for maximum explosive effect, and ignited the mixture by means of a hollow stop-cock, after Barnett's style of igniting arrangement. With the temperature of the mixture before ignition at 12° Centigrade, the highest pressure attained was 97 lbs. per square inch above the atmosphere. The pressure was measured by a loaded valve of known area, as in Bunsen's experiments. The absolute pressure attained was only about 7½ atmospheres; if complete combination had taken place, and no heat kept back by dissociation or absorbed by change in specific heat, then the pressure should have been, at the lowest estimate, 11 atmospheres. He concluded that Professor Bunsen's explanation of this fact was a true one. The effect was equally visible in the large cylinder used by him and in the small tube used by Professor Bunsen. These experiments, and the recent experiments of Messrs. Mallard and Le Chatelier, make it certain that in a uniformly ignited gaseous mixture the temperature was limited, and the apparent loss of heat was very slow, and that this effect was due to dissociation, either complete or incipient. Such a mixture in expanding during work would give rise to all the phenomena described in the Paper. He was pleased that his conclusions on the relation between rate of inflammation at constant pressure and constant volume had been experimentally proved by these gentlemen. He had been challenged by Mr. Imray to controvert his statement on the history of the introduction of the gas-engine. This he did not do, because he considered Mr. Imray's account fairly correct. Mr. Clerk.

The only remark of Mr. Imray on his theory was:—"He would only refer to Fig. 9. If the theory of dissociation were true, it would follow that the lower the temperature the more dissociation would take place, which was undoubtedly altogether wrong." It was difficult to understand this statement, it was so exceedingly irrelevant. He could hardly believe the speaker had ever studied the pressure, volume, and temperature relations of gases. On the indicated diagram low pressure had been mistaken for low temperature, neglecting the increased volume due to the travel of the piston. Mr. Imray had supposed that the maximum pressure on line *d* (Fig. 9), being lower than on line *a*, therefore the temperature was also lower. He failed to see the bearing on the theory under discussion of Mr. Bousfield's statement:—"He did not say that when the explosion took place, there might not be a certain quantity of ammonia and a certain quantity of

Mr. Clerk. nitric acid formed." The question why, when maximum pressure was reached at the beginning of the stroke, he assumed that the flame had spread throughout the mass in the cylinder was much more to the point. From the original of the diagram, Fig. 6, he had taken the two extreme lines, shown at diagram Fig. 19, *a* and *b* were the points of maximum pressure. In the Paper he had not detailed the method used to calculate the temperature attained at the point of maximum pressure; it was necessary to do so before proceeding further. First he determined the exact volume of the space at the end of the cylinder into which the mixture was compressed, then on the diagram he had drawn the adiabatic line of compression, it was the dotted line shown at Fig. 6; the lower black line was the actual compression line drawn by the indicator. It would be seen that the two were as nearly as possible coincident. The cause of this had been pointed out. The temperature at the point *c* was known to be 150°·5 Centigrade, and the pressure 41 lbs. above atmosphere, and assuming the volume to remain constant, the temperature at *a* was calculated from the pressure 220 lbs. above atmosphere.

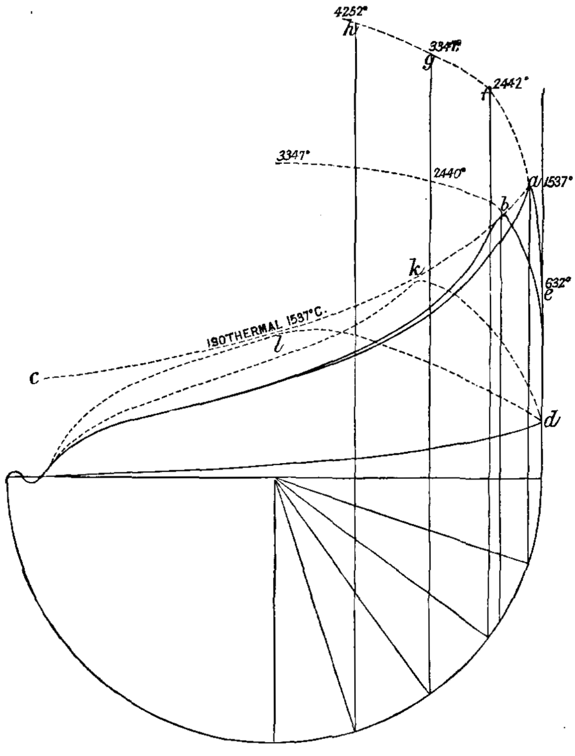
Let P = pressure before ignition, and P′ pressure after ignition, T = temperature before ignition, and T′ temperature after ignition, then—

$$T' = \frac{P' \, T}{P}$$

both pressures and temperature absolute. In diagram Fig. 1 it was shown that the temperature of compression, corresponding to 40 lbs. above the atmosphere, was 150°·5 Centigrade, and from these figures the temperature 1,537° was obtained. This was the minimum possible temperature, as would be observed from certain considerations developed at p. 237. Whether the flame had spread throughout the mass of the mixture or not, this was the average temperature. From *a*, Fig. 19, was drawn an isothermal line, *a b c*, dotted; at the point *a* the temperature had commenced to fall, up to that point it had been rising at a very rapid rate. The semicircle drawn below the atmospheric line showed the path of the crank-pin, and each division represented in time one-fiftieth of a second; the engine was running at one hundred and fifty revolutions per minute when the diagram was taken. Comparing the condition of the gaseous mixture in one-fiftieth of a second before maximum pressure, and one-fiftieth of a second after maximum pressure, in the first one-fiftieth of a second the average temperature had increased 905° Centigrade, while in the second hundredth it had diminished about 189° Centigrade. Within a limit of one twenty-fifth of a second there was a point where the increase of temperature ceased, and where a fall of temperature began. What did this mean? Why did the increase of temperature cease in so sudden a manner and a fall of temperature set in? Mr. Clerk.

From the point d to a the temperature had been increasing, this increase being due to the progress of the flame; at the point a the increase ceased, and a fall set in. Take the point e, then the average temperature was 632° Centigrade; from e to a the time

Fig. 19.



Engine speed 150 revolutions per minute.
One division of circle = $\frac{1}{50}$ part of a second at above speed.

taken one-fiftieth of a second, and the temperature rose to 1,537° Centigrade; in that time it had increased by 905°; suppose the same rate of increase to continue for another one-fiftieth of a second, the pressure would rise to the point f, and the temperature would be 2,442° Centigrade, the points g and h showed the effect of further increase. But the increase had abruptly ceased at the point a; from a to f the volume had changed so slightly that the rate of

Mr. Clerk. cooling could not have increased appreciably. The amount of work done in that movement was also relatively insignificant, and yet from some cause the increase of temperature going on with such rapidity, 905° in one-fiftieth of a second, had not only diminished, but an opposite effect had set in. It could not be supposed for a moment that the progress of the flame had been abruptly stopped by any cause other than completed inflammation of the whole mass. The flame which in one instant of time had been flashing through the explosion mixture had reached the enclosing walls, it had uniformly heated the whole combustible mass, and in the next instant the temperature began to fall; the law of cooling took effect. The very rapid rate of rise, and the abrupt change from rapid rise to slow fall in temperature, at a given point, showed that at that point completed inflammation had been attained. The cooling which was so slow as to be unable to put an appreciable check on the rate of rise up to the point of maximum temperature, could not be supposed to suddenly increase to such an enormous extent as to completely absorb and overpower at that instant the effect of continual spread of flame. There could be no doubt that, as Sir William Thomson had pointed out, on diagram Fig. 6, the maximum of the curve indicated truly the instant when the combustion was as complete as dissociation allowed it to be. It was certain that at this point of the diagram the flame had spread completely through the whole volume of inflammable mixture, and that in whatever way the sustaining of the pressure to nearly the adiabatic line was to be explained, it could not be accounted for on the hypothesis of a continued spread of flame.

A little consideration of the conditions of the indicated diagram would show that the slower the rate of inflammation, relatively to the movement of the piston, the less distinct would the point of maximum pressure become, and the more rounded would the apex of the diagram appear. Nevertheless the point of completed inflammation was easily determined from the point of maximum temperature, when near the end of the stroke this point might not be the point of maximum pressure. He had been careful to make this distinction, and had said, with reference to slow inflammation, pp. 241, 242:—"This supposed phenomenon has been erroneously called slow combustion; if it has any existence it should be called slow inflammation. It has a real existence in the Otto engine only when it is working badly; but even then maximum temperature is attained, and very distinctly marks the point of completed inflammation." On diagram Fig. 19 was shown the effect of increasing the speed of the engine while preserving a constant rate of inflammation. If the speed were increased from one hundred and fifty revolutions per minute three times, or to four hundred and fifty revolutions per minute, it would be found that the point *a* would be moved forward to *k* and *b* to *l*. In both cases the temperature attained would be nearly 1,537° Centigrade, a slight fall would be observed due to increased cooling surface and to a part of the work being done before maximum temperature was attained. But in all cases the maximum temperature marked the point of completed inflammation and the temperature began to fall so soon as it was attained. For ignitions attaining their maximum very late in the stroke, maximum pressure need not coincide with maximum temperatures; but a reference to the isothermal line showed the point of highest temperature. Using an inflammable mixture of constant composition, and varying the speed of the engine, it was always found that ignitions attaining maximum temperature later and later in the stroke always came very near the isothermal line drawn from the point of highest pressure at the beginning of the stroke. The lines never ran over this isothermal. This meant that, whether inflammation was completed early or late in the stroke, nearly the same maximum temperature was attained. It followed from the relations between isothermal and adiabatic lines, that the lines drawn by the indicator from late ignitions always crossed those from early ignitions. This was shown by the diagrams taken from an Otto engine by Mr. Bousfield, for which he must thank that gentleman. In these diagrams, however, it was evident that the mixture used had not been of constant composition at all speeds. This would be evident by examining Fig. 15. When the speed had been changed from one hundred revolutions per minute in the larger diagram to two hundred in the smaller, the increased speed of the engine had caused it to take in a smaller weight of gaseous mixture, as was shown by the compression line leaving the atmospheric line later, and that the pressure on completion of the in-stroke only rose to 22 lbs. per square inch instead of 30 lbs., as in the other. If the mixture had been the same the point of maximum pressure would have crossed in the first diagram at this point, and the pressure line would have run into the first lower down, as was shown in his diagram at *b*, Fig. 19. In the Otto engine the hot exhaust remaining in the space when each cycle was completed still further complicated the comparison between different speeds. At the higher speeds the walls of the cylinder had less time to cool the exhaust, and consequently the average temperature of the Mr. Clerk.

Mr. Clerk. mixture before compression must be greater at high speeds. In his own gas-engine this complication had no existence, because the whole charge was replaced at every stroke. In Mr. Bousfield's diagram, Fig. 16, the same change of mixture was evident, but here the change of speed of the engine was relatively greater, and consequently the lower diagram crossed the upper one somewhat earlier. In Fig. 17 this was more and more evident; still no two of the compression lines coincided, showing the proportion of exhaust to inflammable mixture to be continually increasing, and the maximum temperature attainable by the ignition consequently becoming less and less. Even in diagram, Fig. 18, maximum temperature was attained, and could easily be discovered by calculating the average temperature at each point along the line of increasing volume. Mr. Bousfield stated that a light applied to the exhaust of an engine, giving diagram, Fig. 16, caused explosion, and from that inferred that combustion was not completed at the end of the stroke. He would find that when this happened the engine was missing ignition altogether and discharging the unburned contents into the exhaust. He might observe that the horizontal line in that diagram did not mean constant temperature, but indicated constantly increasing temperature. Mr. Bousfield had evidently fallen into the same error as Mr. Imray, and confounded low pressure with low temperature without considering the change of volume. It was a characteristic of the inflammation of a gaseous mixture in mass, that so long as inflammation continued to spread, so long did the average temperature increase. Dissociation did not begin to sustain temperature until the temperature fell. In the construction of the theoretical diagram Mr. Bousfield had fallen into error. He drew from the points F G H, Fig. 14, to A L produced, lines which he described as adiabatics, and then said that the curve drawn through P Q R "represented the pressure at any time in the contents of the cylinder, supposing these contents remain confined in the space at the end of the cylinder, and not allowed to expand." Now the lines F G H should not be adiabatics but isothermals, as Mr. Bousfield's object in constructing the diagram was to get the time taken in a closed space to attain the temperature existing in the engine at the points F G H. The points L M N should show the pressure at constant volume at these temperatures. If Mr. Bousfield calculated the temperature from an actual diagram, he would find that maximum temperature coincided with maximum pressure when at the beginning of the stroke. He thought from his remaining criticisms that Mr. Bousfield had not understood the nature of the proof advanced in the

Paper, and that when he had studied the subject and appreciated the nature of the considerations advanced, he would admit the truth of the theory set forth in the Paper. Mr. Clerk.

It had been asked by Dr. Hopkinson whether the pressure rose higher when an engine was running slowly than when it was running fast? Whether the pressure attained on exploding a gaseous mixture in a closed space and in an engine was the same? Given the same proportion of gas to air and the same temperature and pressure of mixture before ignition, then the pressure attained after ignition was the same in all stages where the maximum pressure was attained at the beginning of the stroke; it was the same whether in a closed space or in an engine. But the ignition must be rapid enough at the higher rate of speed to give maximum pressure at the beginning of the stroke. As he had already pointed out, if an engine was to run fast enough it might overrun the rate of inflammation, and the maximum temperature would not be attained till towards the end of the stroke. If an engine was run at two hundred revolutions per minute and maximum pressure was attained at the beginning of the stroke, then however slowly that engine ran using the same mixture, the maximum pressure would always be the same, it would not increase. Dr. Hopkinson then asked, Was the maximum pressure the same in large and in small engines? When using a similar mixture, the same pressure and temperature before ignition, it was the same. In small engines the temperature fell more rapidly than in large ones because of the greater proportion of cooling surface to volume of gases, but the maximum pressure attained was nevertheless the same because of the rapid rate of ignition. The results obtained in the large cylinder to which he had alluded, and those obtained by Professor Bunsen in a small tube, each showing a limit to the rise of temperature which could not be referred to cooling, and each showing complete spread of flame, proved that the maximum pressure to be obtained from an explosive mixture was independent of the dimensions of the vessel used. Dr. Hopkinson had asked why, in comparing types 2 and 3 of engine, he used different maximum pressures; why in the second type he used 76 lbs. per square inch above the atmosphere, and in the third over 200 lbs. per square inch. His reason was this: the three types were taken under conditions which have been found in practice to be the most favourable for each. He had compared the theory of these types of engine as nearly as possible under conditions used in practice. It was quite true that type 2 should be compared with type 3 under similar conditions of pressure from a purely

Mr. Clerk. theoretic standpoint; but the object of the Paper had been to inquire into the cause of the greater efficiency of the third type as in use against the two first also in use. It would be seen that to attain a pressure of 200 lbs. per square inch in type 2 it was necessary to compress the mixture to that pressure before ignition, the temperature of compression being nearly 365° Centigrade. This involved considerable loss of heat in the reservoir, and increased the chances of leakage while compressing; in type 3 a pressure of 40 lbs. per square inch before ignition was all that was required to attain 200 lbs. after ignition. He believed that type 2 could work advantageously at a much higher pressure than 76 lbs. per square inch, but he questions whether it could do so at so high a pressure as 200 lbs. The advantage of type 3 in this respect was a comparatively low pressure before ignition. With careful workmanship doubtless it would be possible to use an engine of type 2, the theoretical efficiency of which would be quite as much as type 3, as given in the Paper.

The description by Mr. F. H. Wenham of his work on hot-air engines was interesting, and his distinction of the cylinder itself as the heat generator or furnace was the essential one between gas- and hot-air engines, and was indeed the great cause of success in these engines. Mr. H. Davey had objected to his comparison of the efficiency of gas- and steam-engines, and considered the basis of comparison of efficiency used by him as an unfair one. In comparing engines of the same system it was right, as Mr. Davey stated, to use as the standard the mechanical equivalent of the total available heat; but in engines of totally different nature the only basis of comparison was the number of heat-units given to the engine, and the number of these heat-units converted into mechanical work. If one system was necessarily limited in range of temperature, as the steam-engine was, then the inquiry must not be how near it approached perfection within that range, but how much heat could another system convert into work as compared with it. In comparing steam-engines with steam-engines Mr. Davey was perfectly right; in comparing them with gas-engines the general basis must be taken. He agreed that the speedy downfall of the steam-engine was not to be anticipated; he only held that the gas-engine was now in its infancy, that it contained greater possibilities than the steam-engine, and that in the future it was certain to be in every way a great advance on the steam-engine, and likely to supersede it.

The propriety of treating the gas-engine as an air-engine had been called in question, and he had been asked whether the specific

Mr. Clerk.

heats of air and the gaseous mixture used were in any way comparable. The specific heat of air at constant volume was 0·169, and the specific heat of a mixture of 1 volume of coal-gas and 12 volumes of air could not exceed 0·200, so that for the purpose of approximate comparison their adiabatic curves might be considered as nearly identical. So little was known of the specific heat of gases at high temperature that Mr. Clerk considered it simply an affectation of accuracy to endeavour to make the comparison closer. He was aware that the efficiency of a heat-engine was independent of the nature of the fluid employed, provided the temperatures between which the engines worked were the same—that was provided there was the same difference between source and refrigerator. But this was just where the steam-engine failed. Given equal amounts of heat from the same source, in the steam-engine the high temperatures could not be utilised, because, first, a certain quantity of heat had to be expended to change the physical state of the water; and as the steam produced was rejected as steam all the heat so expended was lost for the purpose of procuring high temperature. With air, on the other hand, the same quantity of heat from the same source, a much higher temperature was attained, and consequently a greater range of temperature due to work performed. The use of steam necessitated a limited range of temperature, and the discharge of all the heat used in converting water from a liquid to a gas. It had been argued that in engine type 2 he had over-estimated the efficiency, and made it greater than was possible from a perfect heat-engine working between the limits of temperature used. Mr. Bamber had fallen into error by mistaking the limits, and in this he was not alone. This type of engine presented very interesting peculiarities in theory, which, so far as he was aware, had hitherto been missed by writers on thermo-dynamics. Although 1,537° Centigrade was the maximum temperature, and 1,089° Centigrade the temperature of discharge with the exhaust, yet these temperatures were not the limits within which the engine was working; the refrigerator, which was at atmosphere temperature 17° Centigrade, was being used to a certain extent without being apparent.

The diagram was not a simple one; the efficiency 0·36 was the result of the united action within two different limits. The diagram from 1,537° Centigrade to 1,089° Centigrade was the same both in types 1 and 2, and working between these limits the maximum possible efficiency was 0·247; but in type 1 this efficiency was not attained, because at 1,089° Centigrade the air had not the same density as before expansion, and some work had

Mr. Clerk. been expended in changing the volume to twice its original amount. If before heating the air had been compressed slightly, then heated to 1,537° and expanded to its original volume, and lowered in temperature due to work done to 1,089°, the duty would be 0·247. If in type 1 a condenser were used, and the temperature reduced to 17° Centigrade, the additional work obtained, would raise its duty to 0·247, without this it remained at 0·21. In both types the efficiency between the limits 1,537° Centigrade and 1,089° Centigrade was the same; but in type 2 a considerable amount of work was obtained in the earlier part of the diagram, a certain amount of work was done on increasing temperature from 217°·5 Centigrade to 1,537°, and a considerable proportion of heat could be converted into work on an increasing temperature, still conforming to the law $\frac{T_1 - T_2}{T_1}$ as the maximum possible between the limits.

In type 2, to a certain extent, the refrigerator at atmosphere temperature was made available in a portion of the action, and consequently a portion of work done on increasing temperature, while the latter half of the stroke was accomplished on falling temperature. This was the reason why a greater efficiency was got than the apparent limits would allow. Mr. Bamber then argued that it made no difference whether it was necessary to use an air-pump or not, if only the same quantity of heat were consumed and the same theoretic efficiency obtained. In practice it made all the difference; the great cause of failure with hot-air engines was not imperfect theory but very low available pressures combined with high maximum pressures. Nearly all the power indicated was used up in friction; in the earlier gas-engines the average pressures were very low also. The advantages of compression were a high available pressure, small cooling surfaces, and small loss by friction. There the efficiencies depended on the range of source and refrigeration; but compression allowed all this to be attained under practical conditions. It was hardly necessary to explain that there was a certain maximum efficiency for heat-engines. What he had shown in this Paper was that a greater proportion of this was possible under working conditions with compression than without.

The parallel by Mr. Cowper between slow inflammation and imperfect admission of steam in a cylinder was very just, and illustrated the great loss of power and heat involved by imperfect mixing of gas and air, or by failing to attain maximum pressure as soon after firing as practicable. It was only by a constant application of theory to practice, and a constant testing of results obtained by varying conditions, that he had been able to produce the diagram which Mr. Cowper approved. The amount of gas consumed by his 6-HP. engine was 22 cubic feet per 1 HP. per hour. Of course in cost this did not stand comparison with the coal used by a large modern steam-engine; the steam-engine had greatly the advantage; but compared with a small steam-engine it was economical. When gas was manufactured expressly for gas engines it need cost but little more than the coal used to produce it, and as the gas need not be illuminating all the carbon might be converted into gas. The gas might be in fact a mixture of carbonic oxide and hydrogen. Mr. Clerk.

The accuracy of his determination of temperature had been questioned by Mr. J. MacFarlane Gray, who considered it vitiated by change in specific heat of glowing gas as against a cold gas. That determination was independent of specific heat and was in fact deduced from the law of Charles. As Professor Rücker had remarked, increase in specific heat must be due either to dissociation or incipient dissociation, and the recent experiments of Messrs. Mallard and Le Chatelier thoroughly supported his conclusions on the phenomena of cooling a uniformly ignited gaseous mixture. Mr. Gray had misunderstood his argument in stating that he supported Mr. Crossley's views on slow ignition. Mr. Crossley and Mr. Otto's supporters had always stated substantially if a mixture of gas and air was exploded the pressure would rise instantaneously and fall almost instantaneously. Indeed Mr. Imray had defined this as the difference between explosion and slow combustion, and in support of this statement he had referred to the diagram, Fig. 12. This diagram Mr. Clerk had shown to be completely wrong, and an impossible one. He had also shown Mr. Crossley's statement of maximum temperature attained on that diagram to be without foundation; indeed to be contradicted by the diagram itself. Holding, as the advocates of slow ignition did, that cooling was so rapid, it was consistent that they should believe that it was necessary to keep up the flame by spreading to sustain the pressure. But this necessity was exactly what he had always denied. He had shown that the flame went completely through the whole mass at the beginning of the stroke, and that the cooling of the gases was comparatively a slow process. In this he was confirmed experimentally by all scientists who had worked on the subject, and was supported by the conclusions of Sir William Thomson. It would not be easy to imagine two theories more widely differing than his own and Mr. Otto's.

The remarks by Mr. MacFarlane Gray on the Otto engine and

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Mr. Clerk. the expenditure in repairs seem hardly within the scope of a discussion on theory. Mr. J. E. Dowson had said that in all experiments on dissociation the gases were not confined under pressure, and that this pressure would have some effect in reducing the amount of dissociation. He would point out that Professor Bunsen's experiments resulted in a pressure of 11 atmospheres, that Messrs. Mallard and Le Chatelier's more recent experiments were also made under pressure, and that they both proved that either dissociation, or incipient dissociation, as shown by change in specific heat, took place in every ignited gaseous mixture. It would be difficult to make the tests suggested, and when made they would not really bear on the question, as they would only show that the cooling made it impossible for a portion of the gases to combine; the actual amount of dissociation existing at the high temperature could not in that way be determined. He agreed on the necessity for further experiment on dissociation with simple gases. Sir William Armstrong considered the gas-engine superior to the steam-engine as a heat-engine, but thought that the great difficulty consisted not so much in the engine as in the convenient generation of gas for it. This was certainly the great difficulty at present in the way of making the gas-engine do all that the steam-engine could do; but he had no doubt that a gas-generator could be made to use all the fuel, and to give gas as safely, and with as much ease, as a steam-boiler gave steam. He believed that in this matter Mr. Dowson had made a step in the right direction, and he was sure that it was only a matter of time to produce a generator as manageable as might be necessary for every condition of use.

The experiment which he had the pleasure of showing to Sir William Thomson and to his friends from Glasgow University, at the Crown Iron Works, was to determine the nature of the gases in the motive-cylinder of his engine before compression, at a point near the piston. The apparatus was shown in position on the engine in Plate 1, Fig. 1. Figs. 2 and 3 were elevations of the exterior; Fig. 4 was a sectional elevation, and Fig. 5 was a sectional plan, giving details of the principal parts. The general action of the engine was this: in the cylinder A moved the piston *a* which served simply to take in the gaseous charge, and at the proper time to force this charge into the motor-cylinder, displacing at the same time the products of the previous combustion. This cylinder—which he called the displacer-cylinder—took into it for two-thirds of its forward stroke an explosive mixture of gas and air; the gas was then cut off, and air alone admitted for the

Mr. Clerk. remaining third. The cranks of the displacer- and motor-cylinders were so arranged, that when the motor-piston began to pass over annular parts at the out-end of its stroke, the displacer-piston was moving in. As soon as the pressure in the motor cylinder had fallen to atmosphere, the automatic lift-valve C rose, and the charge from A flowed into B at D; the air which entered last leaving first, and entering the cylinder in advance of the inflammable mixture. The effect of this arrangement was to prevent premature ignition of the charge by the hot gases which were being expelled through the exhaust. The capacity of the displacer-cylinder was greater than the united volume of space at the end of the cylinder, and the volume swept by the motor-piston from the moment of closing the exhaust; the consequence was that a portion of the contents of the displacer cylinder passed through the motor-cylinder, and away in the exhaust pipe. It was intended that the portion which passed away should be nearly pure air, and that what remained should be inflammable throughout. The motor-piston thus compressed the cool fresh charge, which was ignited when the crank was crossing the in-centre. He wished to determine whether the mixture next the piston was inflammable or not. On Fig. 1, Plate 1, was shown a small cylinder communicating with the motor-cylinder at a point 4 inches from the exhaust port; it contained a piston, kept in position with a catch. As soon as the catch was let go, the piston moved rapidly up, propelled by a spring; at the same time the stopcock F opened. When the motor-cylinder received its charge, and the piston was on its return stroke, wherever it had closed the exhaust port 1 inch, and was within 3 inches of the aperture G, it let go the trigger, and the small piston moved up. The engine running at one hundred and fifty revolutions per minute, the motor-piston occupied one-twentieth of a second to move the 3 inches, and in the same time the smaller piston had reached the top of its cylinder, taking in a charge, which was an average sample of the charge in the cylinder from 3 inches to the piston itself. The stopcock F then had closed, and the charge could be examined at leisure. On opening the trial cock, and applying a light, the mixture invariably exploded. This proved that the charge in the cylinder of his engine was inflammable up to the piston. He found by experiment that the small piston took about one-twentieth of a second to rise, because if the spring were let go any later than when the aperture was 3 inches from the piston, it did not reach the top of its stroke before the motor piston had crossed the aperture. This was easily seen by the movement being suddenly checked near the top of

Mr. Clerk. the stroke, and the piston hanging without striking the rubber buffer placed there. To those who had followed the proof in the Paper and in this reply, it was unnecessary to show that air in front of gas and air was not required to produce a diagram of rapid rise and slow fall. To those who had not done so, this experiment at once appealed, and proved that this diagram had been produced with an explosive mixture throughout. No more convincing proof of the fallacy of the Otto theory could be given ; according to Mr. Imray, such a mixture should give a rapid rise, and as rapid a fall. It should give a diagram similar to Fig. 12.

The average composition of the mixture when diagram Fig. 6 was taken was 1 part of gas and 12 parts of air—a mixture identical in composition with that used in the Lenoir engine.

While giving every credit to Mr. Otto as the first to make compression commercially successful, he dissented from the belief that it was necessary to prevent fall by the expedient of slow inflammation. That success was attributed to a thing which had no existence except when the Otto engine was working badly, and was then attended with great loss of power and heat. By great knowledge of gas-engine detail, Mr. Otto had been enabled to be the successful adaptor of compression, but not the inventor of a new system. It was evident that Mr. Otto completely misunderstood the very conditions of his own success. Compression—and compression only—was the cause of the great superiority of the modern gas-engine; the composition of gaseous mixture used was capable of great variation without substantial change in duty, and it was unnecessary to dilute a mixture below the point of easy ignition at atmospheric pressure.

Correspondence.

Dr. Adams. Dr. J. ADAMS found difficulty in verifying the figures in Mr. Clerk's Paper owing to the absence of the Table referred to in p. 223[1], which was said to contain the data used throughout the calculations. At the same time he had satisfied himself of the ability and practical value of the communication, and of the clearness with which it demonstrated principles for which the Author contended. Although he meant to direct attention to some points of detail that might be amended, they did not affect the Author's general argument. He had looked carefully into the thermal values assigned by the Author for coal-gas, and he thought it would have been well that any assumption of fixed values should have been accompanied with an emphatic recognition of the fact

[1] Since supplied, p. 250.—SEC. INST. C.E.