Discussion.

Mr. Jones.

Mr. H. E. Jones wished to add that he had lately been struck by an analogy which many members, especially those connected with London gas companies, would follow between the lowest minimum of sulphur which they were able at any time to attain, and a feature in connection with Mr. A. Vernon-Harcourt's lamp tests which were being exhibited on the table. In examining the records of metropolitan gas tests generally, he had noticed that none of them appeared to get below 7 or 8 grains in 100 cubic feet as a minimum, and he believed that circumstance had influenced the gentlemen who prescribed limits for them in not making the absolute removal of sulphur a necessity, but giving them a margin. The Harcourt apparatus recorded an amount of sulphur which must be always added to by a prescribed quantity of 7 or 8 grains of sulphur. Until lately he was under the impression that those things had no connection with each other, and that it was merely a feature of the lamp test that it was always necessary to add 7 or 8 grains. But he had been led, in the course of the trial of a new kind of coal, to the impression that both the lamp test and the purifying material were ineffective upon the particular 7 or 8 grains of sulphur which appeared as a phenomenon in both cases. It was a phenomenon in the purifier, because the amount was never reduced, except in the course of a few accidental cases showing 5 or 6 grains; and in the lamp, because, no matter how pure the gas looked, it was always necessary to add 7 or 8 grains. Recently he had found a discrepancy between the lamp test and the amount of sulphur revealed by the standard test of the Referees, varying to an extent which he had never before observed; and at the same time the purifiers (which, according to all prescription, should have been effective, and had been effective previously and since), like the lamp, did not act or show the sulphur. He should like to ascertain from some of the members who had had longer experience than himself in sulphur purification, whether anything similar had been noticed by them. The subject was one upon which gas companies were a little tenacious, exposed as they were to penalties; but he should be glad if the engineers of other gas companies would state their experience. His object in writing the Paper had been to place something in the hands of those who might be seeking the same result as himself, which would save them the necessity of making a number of fruitless and disappointing experiments.

Mr. Vernon-Harcourt.

Mr. A. Vernon-Harcourt said he owed an apology to the

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Mr. Vernon-Harcourt.

members who had come to discuss the Paper, for venturing to interpose for a short time with a subject which, though kindred to that discussed in the Paper, inasmuch as it dealt with the same problem, proposed to deal with it in a different manner. He had, however, been invited to use that opportunity of giving some account of an experiment which was made five years ago to test a process for purifying gas; and diagrams representing the apparatus then used had been placed upon the wall (Plate 2). He would accordingly for a few minutes direct attention to that process. The object of the colour test, which had been already described by the Author, was to ascertain the amount of bisulphide of carbon present in gas. He had upon the table an apparatus of similar construction, which he had set up in order to illustrate the mode in which, on the same principle, sulphur compounds might be removed from gas on a large scale. Within a glass bulb were placed a number of little balls made of fire-clay and oxide of iron, which had been strongly heated in a current of gas so as to reduce the oxide to metallic iron. The current of gas was passed down a tube into the bulb to the bottom of the layer of the small balls of clay and iron. Through these it rose, and after passing over the heated surface, it was sent through a layer of oxide of iron, which was used for its ordinary purpose of purifying gas from sulphuretted hydrogen. Then it passed on to the colour test, which would serve to show the purification thus effected.

When coal-gas, containing bisulphide of carbon, was brought into contact with any substance heated to a temperature of not less than 250° centigrade, the sulphur which was combined with the carbon in the gas united with some of the hydrogen present, and passed from being bisulphide of carbon to being sulphide of hydrogen or sulphuretted hydrogen. In the form of bisulphide of carbon it was not arrested by oxide of iron, but only by that use of lime which the Author had described. There were various reasons for preferring oxide of iron as a means of purification to lime, and it had occurred to him, as it occurred before to Mr. Bowditch, that heating the gas or exposing it to heated surfaces, and passing it then through oxide of iron, might furnish the means of purifying it. He would attempt to show the effect produced upon the gas by that process. He would show also at the same time what the Author had described—the test which served as an approximate and ready means of ascertaining the amount of sulphur present in gas. He had before him a little bulb containing some fragments of pumice which had been moistened with a solution of platinum and heated, known as

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Mr. Vernon-Harcourt.

platinised pumice. The bulb was heated to a moderate temperature by a lamp burning as low as possible, and the gas was transmitted through it. The action he had described took place; the bisulphide of carbon was converted into sulphuretted hydrogen, and the test was made by observing the colour given to a solution of lead by the passage of the sulphuretted hydrogen through it. The test was made quantitative by observing what volume of gas had to be passed through the solution of lead in order to produce the standard colour. He had before him a tube containing a liquid having the depth of colour which was produced by the action on a lead solution of a known quantity of sulphuretted hydrogen. The gas was drawn through the apparatus by means of an aspirator. On turning on the aspirator a slender stream of water dropped into the measuring vessel below, and thus a stream of small bubbles of gas was drawn through the solution which contained the lead. The alkaline solution of lead was mixed with a certain quantity of sugar, in order that the effect of the sulphuretted hydrogen on the lead might not be to produce a precipitate, but only a colouration. He could not make the experiment with quantitative accuracy, but it would be observed that there was a gradual approximation in the solution to the colour of the standard placed by its side. It was easy with a little practice to hit very precisely the moment at which the two colours most resembled one another. When that point had been reached the tap was turned off, and the volume of water collected in the measuring cylinder below was read. By means of a table calculated beforehand one could find directly that such a volume of water corresponded to so many grains of sulphur in 100 cubic feet of gas. He would now test the gas which was passing through the heated clay and iron, and thence through a layer of oxide, to a bat's-wing burner. The capacity of the bulb was rather less than 2 cubic inches; the burner was consuming about 5 cubic feet of gas per hour. The result appeared in a few minutes, when it was observed that a very much larger quantity of water would need to be run into the cylinder before the same brown tint was produced after the gas had been purified in that way. He had conducted an experiment of that kind for several months in succession, without change of material, leading the gas through continuously, day and night, and testing from time to time the quality of the gas which passed out; and he had satisfied himself that for so long a period, it was possible continually to reduce the quantity of sulphur in gas from between 30 and 40 grains per 100 cubic feet to between 8 and 12 grains.

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Mr. Vernon-Harcourt.

The chemical and physical properties of substances were independent of their mass. If the contact of gas with a certain proportion of porous iron at a temperature of between 300° and 400° centigrade, for one-third of a second would change its bisulphide of carbon into sulphuretted hydrogen, the quantity operated on was immaterial. Now these conditions, which were those of his experiment, would also be realised by passing a large volume of gas through a correspondingly large mass of material, provided the required temperature could be maintained throughout the mass. Beyond the question how heat was to be applied, nothing was needed to pass from one scale to another but a sum in proportion; and he found that the purification of 5 cubic feet an hour by 2 cubic inches of material, corresponded to the purification of more than 5½ million cubic feet a day by 2 cubic yards. The practical problem was how to heat and cool again this great volume of gas in an apparatus, and with an expenditure of fuel, which should not add much to the cost of purification. He might be permitted for a few minutes to refer to the diagrams, Plate 2, which represented an apparatus, designed by Dr. C. William Siemens, and set up by the Gaslight and Coke Company five years ago, in order to try the experiment on a large scale. It was one of Dr. Siemens' regenerative furnaces applied to the purpose which had been described. The apparatus consisted of two brick chambers. The bricks were laid in a chequer, so that there were numerous similar channels for the distribution of gas through the brickwork. Above the brickwork was placed a layer of pebbles, and above these a series of tubes, through which the heated gases from a furnace passed to a chimney. The gas entering at the bottom rose through the brickwork and through the column of pebbles on one side of the partition wall; thence passed across the heated tubes into a similar adjoining chamber, where there was another set of heated tubes, which it must also traverse, and thence descended through a similar layer of pebbles and chequer-work of bricks. By means of the valve shown in the diagram it was possible to direct the current of gas either way; so that, when the gas had been passed for a time in the direction shown by the arrows, and after becoming heated, had carried its heat downward, and given it up to the material which it passed in its descent, by turning the valve the current was reversed, and the heat so transferred was not lost, but was received by fresh gas as it rose. The gas thus arrived, already in a certain degree heated, at the portion of the apparatus in which the chief heat was given to it; and so at each turn of the valve heat was accumulated. By this means there was

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Mr. Vernon-Harcourt.

a very small expenditure of fuel compared with the quantity of gas to be dealt with. It was possible to bring each portion of gas in its turn to the temperature necessary for the production of the chemical change. There was also an arrangement by means of which, with the aid of the chimney, air could be drawn through the apparatus, in place of driving coal-gas through. The reason for that provision was, that it was thought possible that there might be a deposition of carbon upon the surfaces which were heated unless the temperature could be readily moderated. Such a deposition had happened, and to some extent, no doubt, it must always happen. Gas was never so clean that it could pass through any system of pipes, or any material, for a length of time, even when cold and at a distance from the works, without some carbonaceous deposit accruing; therefore a provision was made by which the apparatus could at any time be restored to its original condition by shutting it off for a few hours, and drawing air through it instead of coal-gas. All the carbon could thus be burnt off from the bricks and pebbles.

The other diagrams represented subordinate parts of the apparatus which he had himself devised. One of them was an arrangement for turning the reversing valve of a regenerative furnace mechanically at regular intervals. It consisted of a water-tumbler attached to the spindle which turned the valve. A slender stream of water fell into one or other of two buckets, according to the position of the tumbler at the time. He would direct attention to two modifications which he had found it necessary to adopt in order to determine that the apparatus should turn over in spite of the adhesion which occurred through the deposition of tar from the gas upon the surfaces of the valve, and in order that when it turned over it might not fall with too much force, and so endanger the valve. When a valve stuck at all, the tumbler did not reverse true to time if its action depended on the accumulated weight of water, and then it fell over heavily. In order to remedy this, the buckets themselves were hinged so that they tumbled independently. While they were receiving the water, owing to their being set slantwise, the centre of gravity of the bucket and the water shifted outwards. By means of a screw the slope of the bucket could be set at will. When rightly adjusted, by the time the bucket was nearly full, or sufficiently full, the centre of gravity had moved enough for the bucket to tumble over. When the bucket had fallen over a little way, its edge jerked against the end of the arm of the principal tumbler, and the jerk was sufficient to start the valve from its seat. The

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Mr. Vernon-Harcourt.

difficulty he had at first experienced in the irregular turning of the tumbler was successfully got over by that device of making each of the buckets also tumble separately. To prevent violent tumbling, two pieces of iron tube were placed below the apparatus filled with water, and in each was slung, by a cord from the arm of the tumbler above it, a can about 4 inches in diameter, rather smaller than would exactly fit the pipe. By adjusting the relation between the size of the can and the size of the pipe filled with water, in which it sank and rose, any amount of drag could be put upon the movement of the tumbler. Thus, as soon as the tumbler had begun to turn, there was at once a pull upon the cord, and in consequence of the atmospheric pressure upon the water, the bucket moved slowly and the tumbler fell gently over, and caused no jar upon the valve.

The remaining diagram illustrated an arrangement for observing continuously the temperature of the furnace. It was a registering air-thermometer, showing the deviations on each side from the desired temperature. It consisted of a siphon barometer which was connected air-tight with a vessel placed within the heated chamber, and measured the tension at varying temperatures of the air inclosed within the vessel. He had tried different materials for the vessel. Copper was not perfectly air-tight. Silver was not sufficiently rigid. He had no doubt that with a cylinder of glass or china those defects would be avoided. At each end of the mercury in the siphon-barometer was an index—a piece of wood pressing with two light springs against the side of the glass, which floated up when the mercury reached it. Supposing the two indexes had been adjusted before the apparatus was left for the evening, if during the night the temperature had risen much, the index in the long limb, which moved with sufficient friction to keep its place, would have risen by a corresponding amount; and supposing the fire had been let down and the temperature had fallen, the mercury would then have risen in the short limb, and the index in that limb would have been displaced. After the required temperature had been produced, then, and not till then, the junction between the air-vessel and the barometer had to be made. This was done, as shown in the diagram, by slipping a piece of india-rubber tubing over the glass tube at the end of the barometer, and over that from the air-vessel; then the whole was surrounded by a short piece of wider tube which was filled with glycerine, and in that way a sound joint was readily made and maintained.

He observed that the Author had followed a common practice in

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Mr. Vernon-Harcourt.

expressing the relation between the quantity of gas to be purified and the means used for purifying it by stating the area of material used in purification. He could not believe that that was a correct mode of stating the relation. A certain amount of gas had to be acted upon by a certain amount of solid material. How much action took place must depend upon the time of contact between the gas and the whole of that material, not merely the surface. In providing the purifying apparatus the engineer naturally dealt with area, providing so many boxes, of such a size, with the means of fixing in them so many tiers of sieves, leaving undetermined the depths of the layers of purifying material. But in estimating the action of a porous solid traversed by a gas, its mass, and not its area, must be taken into account. He might give an example of the relations which were obtained by considering the bulk of the material used in the purification of gas, instead of the area, by reading a few figures from a Table in which he had put together some results that were communicated to him three years ago by several of the managers of the principal London gasworks. He would not give the names, lest it might be considered that the result obtained in one case was better than that obtained in another. The Table was as follows:—

Station.	Cubic Feet of Purifying Material in Action per 1000 Cubic Feet of Gas per Hour.			Cubic Feet of Gas Purified per Cubic Foot of Slaked Lime used.	Grains of Sulphur in 100 Cubic Feet of Gas. Average for December and January, 1876-77.
	Lime.	Oxide.	Total.		
A	20·3	71·0	91·3	3,127	13·5
B	29·5	32·2	61·7	4,274	15·7
C	53·1	17·8	70·9	..	18·4
D	54·1	29·0	83·1	4,040	14·4
E	81·1	50·3	131·4	4,260	13·9
F	84·4	42·2	126·6	5,400	16·1
G	91·1	64·0	155·1	5,000	15·7
H	94·7	110·0	204·7	..	15·5
I	110·0	35·4	145·4	..	17·9

He was glad to observe the statement made by the Author, that, using his method, the difficulty of the nuisance which was supposed to be inseparable from lime purification, was now entirely got rid of. The foul lime was brought into a condition which, though giving evidence to a near observer of its containing sul-

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phur compounds, was quite inoffensive at a distance of 50 yards. Nothing more satisfactory than that could have resulted from the process which the Author commended to their attention. Another interesting observation in the Paper related to the effect of exposing to air the foul lime which had been used to remove bisulphide of carbon. A question had occurred to him, which perhaps the Author might be able to answer, namely, whether that effect was due to the action of the air or to the stirring and breaking up of the material, which was the concomitant result of removing the material from the purifier and replacing it. It occurred to him as possible that it was not a chemical action of the air, but that it depended on the breaking up of the little nodules into which the lime was formed, so as to expose fresh surfaces which had not before been traversed by the gas. The disproportion between the mass of foul lime used and the amount of bisulphide of carbon removed showed how incompletely the undisturbed lime was acted upon. A point which was to him, as a chemist, one of great interest was the observation as to the effect of cold on the material in question. The Author had observed that when lime at a low temperature was acted upon by sulphuretted hydrogen, there no longer was produced the coloured offensive material known to all who were familiar with gasworks, but an inoffensive white substance; and this was supplemented by the further observation that nevertheless the substance was a compound of lime with sulphuretted hydrogen, for that, simply by its temperature rising and by exposure to air, it again acquired all the properties of the ordinary foul lime. It did not appear whether the low temperature spoken of by the Author was actually the temperature of freezing. If it were so, it would be natural that a difference should be caused. The moist material was dried up by being frozen, and the presence of moisture might be necessary for the formation of the active compound. The additional cost of purification was stated by the Author at the moderate figure of one penny per 1,000 cubic feet, in order to produce gas containing the comparatively small amount of 10 or 12 grains of sulphur per 100 cubic feet, instead of between 30 and 40 grains. A point of great importance in the Author's process was the use of oxide. He employed oxide, and not lime, for the purpose of removing the sulphur which existed in the gas as sulphuretted hydrogen. Another observation which to him was novel, and appeared to be of great interest, as well as practical importance, was, as to the difference in the permeability of the lime through which the gas had passed, according to whether it was first acted upon by car- Mr. Vernon-Harcourt.

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Mr. Vernon-Harcourt.

bonic acid and directly carbonated, or acted upon in the first instance by sulphuretted hydrogen, and then by carbonic acid. In the first case, the Author found that the material became dense and that the gas would not readily pass through; and in the latter case, that the material became porous. In conclusion he wished to express his opinion that the Paper was a most important contribution to the science, as well as a serviceable guide to the practice, of the purification of gas.

Mr. Patterson.

Mr. R. H. PATTERSON desired to express his high appreciation of the Paper. He thought the most interesting part was that which related to the appearance of ammonia at the outlet of the lime purifiers, when no ammonia was detectable at the inlet. It was quite natural that under such circumstances theories should be started, and it was also useful; but the first point was, to ascertain whether the ammonia had not really been contained in the gas as it entered, and had been masked by existing in some strong compound or other, which was broken up by the lime, so that the ammonia was set free. Ten years ago some similar puzzling phenomena were brought to light in gas purification, and they were alluded to in the Paper, though with considerable inadvertence. In quoting from the Gas Referees' First Report on Sulphur Purification, the Author stated that it contained some assertions which subsequent experience had proved to be fallacious. The first of these was the statement that washers and scrubbers occasionally put more sulphur, in the form of bisulphide of carbon, into the gas than the gas had before—somewhat parallel to what the Author found with regard to ammonia. Now, that was not an opinion or mere assertion; it was the result of experiments carried on in seven or eight gasworks, instituted by the Referees, co-operated in by the managers of those works, and the results were incontestable. It certainly was very puzzling that more bisulphide of carbon appeared at the outlet of the washers and scrubbers than was contained at the inlet; but when the explanation was given—and it was given by himself—the whole thing became very simple. The carbonic acid in the foul gas coming in contact with the liquor in the scrubbers, displaced some of the sulphur, both sulphuretted hydrogen and bisulphide of carbon, which had been previously absorbed, and consequently, whenever that happened, a larger amount of sulphur went forward in the gas than before. The other alleged fallacy, as the Author called it, was the statement in the Report that oxide-of-iron purifiers showed a similar phenomenon, though certainly from a different cause. The experiments were conducted in the same manner, and undoubtedly

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more bisulphide of carbon was ever and anon found at the outlet of those vessels than at the inlet. Mr. Patterson's explanation of the phenomenon was this. Eight or nine years ago he found that solid sulphur, in a state of fine division, commingled with an inert substance like sawdust, took out a considerable amount of bisulphide of carbon. It was said at the time that it must be of no importance whether the substance with which the sulphur was mixed was chemically inert or not, and that his statement was all nonsense, because oxide of iron had not that effect. Now, what took place in oxide-of-iron purifiers? Suppose that the last of the series, No. 3 or 4, contained revivified oxide of iron, with of course a quantity of powdered sulphur in a very fine state of subdivision; in that purifier the gas entered almost entirely free from sulphuretted hydrogen, consequently no chemical action took place; the temperature remained unmoved; and the sulphur powder took up a portion of the bisulphide of carbon. The next thing was that No. 3 or 4 was moved forward and became No. 2 and ultimately No. 1; in which position the entering gas was as foul as could be with sulphuretted hydrogen. A lively chemical action took place; the temperature rose, and the bisulphide of carbon, being very volatile, was driven off. When the oxide in all of the purifiers had been revivified, the absorption of the bisulphide in the last of the purifiers was generally balanced by the giving off of the bisulphide in the first purifiers of the set; but when fresh oxide was used in one part and revivified oxide in another, corresponding changes took place in the result. With regard to the general subject of sulphur purification, Mr. Vernon-Harcourt's remarks brought to his mind a period that he was not likely to forget, and the state of the sulphur question at that time. Exactly eight years ago, at a lecture at the Royal Institution, Mr. Vernon-Harcourt brought forward the process which he had described anew before this Institution. Mr. Vernon-Harcourt then stated, what was the universally acknowledged fact, that at that time no one knew anything about the sulphur question, that everything had been tried, and that nobody could take out bisulphide of carbon. He specially stated that not even by lime could the bisulphide be removed, and that nothing would do it but the process which he then recommended, and which, although it had been unsuccessfully tried, he now recommended anew. And yet, fully a month previously, by his Patent of March 1872, Mr. Patterson had solved the sulphur difficulty by several different processes and materials. The Author had stated that after Patterson's process had been used for several years, it was found Mr. Patterson.

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Mr. Patterson. that when sulphuretted hydrogen largely entered the sulphide-of-calcium purifier, more sulphur was contained in the purified gas than before. Well, what else was to be expected? The sulphide of calcium was being supersulphuretted, the purifying material was being fouled and was losing its power. After that was noticed, the Author stated it was found better to take out the sulphuretted hydrogen as well as the carbonic acid, and he regarded that as a development of Patterson's process, and an improvement upon it. But what was called by the Author Patterson's process happened to be only one out of five of the processes in his patent, and only one out of the two which he devoted to lime. If anyone looked at the first process in his invention, he would see that it was exactly what the Author now desired. After producing the sulphide of calcium by his process, Mr. Patterson said in his patent, "Pass the gas into it after you have taken out all the sulphuretted hydrogen as well as the carbonic acid." And the reason he then gave was exactly what was now said by the Author, viz., that if that was done, the sulphide of calcium would remain efficient for an indefinitely long time, for there was no fouling material entering except the small portion of bisulphide of carbon.

Mr. Gandon. Mr. C. Gandon said that the process described by the Author appeared to him to resemble closely the plan which was adopted about three years ago at the Crystal Palace District Gasworks; but the Author passed the gas first into a lime purifier, whereas at the Crystal Palace District Gasworks the oxide of iron for removing the sulphuretted hydrogen was used first. He could bear testimony to the benefits derived from the method adopted, and it appeared to him that there was very little difference in the two plans. Perhaps the Author could state if any advantage was gained by the order in which he proceeded? Mr. Gandon could not see any. At the Crystal Palace District Gasworks no difficulty whatever had been experienced in keeping the amount of sulphur compounds in the purified gas below the standard required by the Act of Parliament, and very little difficulty in removing the spent lime, although, with the two oxide purifiers in use, it was not possible entirely to remove the sulphuretted hydrogen from the gas before it entered the lime; therefore, the lime was not altogether in a state of carbonate when it was removed, but it was so far harmless that no nuisance arose from it.

Dr. Tidy. Dr. Meymott Tidy said he acted as an assistant of Dr. Letheby during the time he was conducting his first experiments on the presence of sulphur in gas, and as he had been experimenting

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upon sulphur impurity in gas ever since, he had perhaps had better opportunities of watching the progress of gas purification than had occurred to most engineers. He thought that the Paper was a valuable contribution to the discussion on the removal of sulphur, but there were some things in it that he regretted. Knowing how honourable, how honest, how cautious and painstaking in all his experiments Dr. Letheby was, he felt it to be his duty, as his pupil, assistant, colleague, and now his successor, to say that he regretted the objections raised by the Author to the action of Dr. Letheby in this matter. He emphatically stated that no step was ever taken by Dr. Letheby, and no experiment was ever put forward by him, but with the most absolute faith in the truth of what he asserted. He believed that Dr. Letheby was one of the first, if not the first, to assert that the removal of the sulphur compounds other than sulphuretted hydrogen in gas, was effected by lime when it assumed the state of a sulphide, but he never asserted that every condition of sulphide of lime would remove those sulphur compounds; for there was a state to which the sulphide of lime might be brought when the sulphur compounds were not removed by it. That was Dr. Letheby's view. It was undoubtedly the fact that pure lime would remove nearly the whole of the sulphur if it were properly used, and used in sufficient quantity—that was, the lime that would remove not merely the sulphur existing as sulphuretted hydrogen, but the other sulphur compounds too. But there was another aspect of the question, which as a medical man he ventured to put before the Institution, viz., the purely sanitary aspect. Gasworks must be in the midst of the people; and it was the duty of Medical Officers of Health to examine their sanitary condition. He would say nothing about the means to be adopted for preventing gasworks from becoming a nuisance. He admitted that there were enormous difficulties to be encountered, but he was convinced that, if lime was properly used, the sulphur could be reduced so as to be brought within the requirements of the Gas Referees without the lime being converted into a sulphide. But space would scarcely allow this in London. Thus the gas companies were placed in a difficult position. Considering the value of ground, they must use sulphide. If they used sulphide they ran the chance of being prosecuted by the sanitary authority for a nuisance. If they did not use sulphide they ran the chance of being prosecuted by the Gas Referees for an excess of sulphur in the gas. Hence the position of gas companies was a most trying one. He desired to repeat, recalling the many days and months Dr. Tidy.

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Dr. Tidy. that he had spent with Dr. Letheby, that no fact had ever been put forward by him with reference to the purification of gas and the removal of sulphur, but with the single desire of doing his duty to the profession as a professor of chemistry, and to serve the public as a sanitary authority.

Mr. Keates. Mr. T. W. KEATES could not concur in the view put forward by Dr. Tidy as to the efficiency of lime as a means of removing bisulphide of carbon. He did not think there had been any reliable experiments showing that lime was capable of producing that effect. He had a strong impression that the effect produced in the removal of bisulphide of carbon was due exclusively to one particular sulphide of calcium, and it was in that direction that gas engineers should look for a solution of the question. He did not think that any distinct experiments had been made with reference to that point, but the results of all practice seemed to show that only one sulphide of calcium was capable of producing the effect in question. It was shown, he thought, by the circumstance that the mere fouling of lime was not sufficient to render a lime purifier a means of removing the bisulphide of carbon. So far from that being the case the effect was quite the other way. If a lime purifier, rendered foul to a certain extent by the absorption of sulphur, was in a condition to take out the bisulphide of carbon, and was rendered still further foul by the absorption of an additional amount of sulphuretted hydrogen, he believed that its efficiency might be destroyed. He knew of certain experiments which fully bore out that view. He remembered Mr. G. T. Livesey telling him that once, expecting to render a lime purifier more efficient, he wetted the lime, and continued to pass into the purifier a quantity of foul gas containing sulphuretted hydrogen. He succeeded in rendering the purifier excessively foul, charging it with a large additional quantity of sulphur, but, to his astonishment, he found that the purifier was almost inert in taking out the bisulphide of carbon. Mr. Patterson also had stated, that, if a purifier was in an efficient condition, one could, on passing through it foul gas containing sulphuretted hydrogen, destroy its efficiency. He observed that the Author had given an account of an experiment in which he found that an inefficient foul purifier was rendered efficient with an admixture of an additional quantity of lime by passing air through it. All those experiments seemed to bear upon the question, and he was strongly of opinion that if gas engineers would make some more definite experiments in the direction of applying one particular sulphide of calcium to foul gas, very useful results might be

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obtained. The theory and the practice were now widely distant, and he could not but feel that there was something essentially wrong in the whole matter. It required an enormous amount of lime to produce the necessary effect, and the purifiers soon seemed to lose their efficiency; whereas, as a matter of chemical fact, the quantity of sulphide of calcium theoretically required to take out the bisulphide of carbon in gas was comparatively small, bearing no relation whatever to the quantity actually used. Mr. Keates.

Mr. W. Airy said he should like to say a few words on the subject of gas which needed little or no purification, such as that manufactured from naphtha. A few years ago he had occasion to go for Mr. Price Williams to the town of Kasan, on the Volga, to report upon the gasworks there. Naphtha was used at those works in its raw state as it came out of the ground, being brought from Baku, on the Caspian. It was distilled in small cast-iron retorts at a cherry-red heat, and it was introduced through a tube furnished with an ordinary hydraulic seal. A small quantity of water was also introduced into the retorts in the same manner. It took about 1 lb. of naphtha to make 10 cubic feet of gas. The gas was exceedingly pure, and as a matter of fact needed very little purification, but the works were furnished with the ordinary purifying apparatus of condensers, scrubber, and purifiers. The agent in the scrubber was wet hay, that in the purifiers was dry lime. The chemical tests indicated scarcely a trace either of carbonic acid, ammonia, or sulphuretted hydrogen, and the photometer test showed that the gas was thirty-seven-candle quality, estimated according to the English method. It was white and pure, and was burned in fine thin jets. He brought home with him a certain number of the sperm candles which were used for testing in Russia, and it was found that they gave grain for grain of sperm 10 per cent. more light than those used in England. That was a matter of considerable importance, because it was commonly thought that a grain of sperm gave a constant amount of light—at any rate legislation in England proceeded on that assumption. He should be glad to know if it was commonly recognised that there were different qualities of sperm. He thought that the kind of gas to which he had referred was worthy of consideration, for certain situations, as in country mansions, where the quantity used was not great, and the cost was not a matter of importance. The bad effect produced by semi-purified gas, such as that used in London, upon books, wall-papers, pictures and the like, was well known. He should be glad if the Author would give some information as to the precise operation of foul gas Mr. Airy.

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Mr. Airy. upon the binding of books. His own idea was that the sulphur in the gas formed with the dampness of the air dilute sulphuric acid which had a great affinity for water; the dilute acid attacked the water of combination in the leather of the books and in the paste and the glue of the bindings, and when these were thoroughly desiccated the books fell to pieces.

Mr. Clarke. Mr. JOHN CLARKE observed that several experiments were made with Mr. Vernon-Harcourt's apparatus some years ago at Horseferry Road, but the apparatus did not succeed. Though he did not know the cause of the failure, he thought it arose from the difficulty of getting the vessels to hold the gas tight under the pressure, on account of the expansion and contraction taking place from changes of temperature. With a view of overcoming that difficulty, he had himself tried some experiments, and had succeeded in arranging the furnaces and pipes so as to raise the temperature of 50,000 or 60,000 cubic feet of gas, from 60° to about 600°. The average result of his experiments was to reduce the quantity of sulphur in the gas only to the extent of 25 per cent. Taking into consideration the small reduction in the quantity of sulphur, the loss of gas, the loss of illuminating power, the great expense of heating up to the required temperature, and the great danger of dealing with large quantities of gas under that temperature, he had been led to abandon the experiment as impracticable. He thought it was not likely that anything would answer the purpose at present better than the lime now used. Attention should be directed to the removal of the nuisance as much as possible, and with that view he was trying some experiments by washing gas with solutions, and he had succeeded sufficiently to warrant him in continuing them.

Mr. Greville. Mr. H. LEICESTER GREVILLE said that allusion had been made to the particular sulphide of calcium which was supposed to be efficient in the removal of bisulphide of carbon. At the Commercial gasworks it had been ascertained that lime, when at a particularly low temperature, did not produce the active sulphide. He had investigated the question to a certain extent, and had obtained some rather important results. In the first place, sulphide of calcium, as it was generally understood by chemists, viz., the mono-sulphide, was, he believed, never produced in the purifiers. He found that the compound produced, when gas containing sulphuretted hydrogen acted upon lime, was calcium sulphydrate, which had no action on bisulphide of carbon. It was comparatively white and odourless; when exposed to the air it heated very rapidly, the temperature rising to 180°; it became

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Mr. Greville.

dark green, and foul, and it then contained a totally different compound, which he was now investigating. As far as he could at present make out, the compound appeared to be calcium bisulphide, a yellow soluble salt. If the oxidation process was allowed to go on, the compound became white and was again inactive, becoming mainly converted into hyposulphite. He believed that green calcium sulphide, generally known as foul lime, was only produced in purifiers by reason of the accidental quantities of air, which got in with the gas, acting on the first compound which was produced. But for that accidental introduction of air, such a thing as the activity of a sulphide-of-calcium purifier on bisulphide of carbon would not be known. Mr. Keates had alluded to the discrepancy between the theoretical and the practical efficiency of sulphide-of-lime purifiers. At the Commercial gasworks they had made the lime take up a large quantity of sulphuretted hydrogen, and the amount of sulphur duty which it had afterwards performed had been equal to half the quantity of sulphuretted hydrogen originally taken up. When the vessel was discharged it was no longer calcium sulphide; it was, as the Author had stated, comparatively inoffensive, and he believed it was due to the fact that calcium sulphide, or the corresponding compounds, had entirely disappeared, and been replaced by the calcium-sulpho-carbonate, which was not offensive on exposure to the air, and did not give off sulphuretted hydrogen in the same way as ordinary foul lime did. With regard to the amount of sulphur in the gas not shown by Mr. Vernon-Harcourt's colour tests, he might say that that quantity was generally put down as about 6 or 8 grains. On a recent occasion at the Wapping works of the Commercial Gas Company, Mr. Vernon-Harcourt's colour tests only showed 8 grains, but the amount shown by the Referees' test was 24 grains; the official report corroborated the latter, and the question arose where that peculiar form of sulphur could come from, because there was the curious coincidence that the sulphur which was not indicated by the lamp was also not removed by the purifiers, although there were exceedingly active vessels on at the time. He examined the crude gas, and found that the amount of sulphur, as shown by Mr. Vernon-Harcourt's test, and by the Referees' test, was practically identical. Eventually it was found that that peculiar form of sulphur was being thrown out of the carbonic-acid series of vessels, and its origin could only be traced to the use of a fresh description of coal. The abnormal state of things gradually faded away, and at present there was the usual concord between the Referees' test and the lamp.

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Mr. Stevenson. Mr. G. W. Stevenson had not for some years been engaged in the management of gasworks, so that although he had acquired experience by his connection with gas companies, he could not set it against the careful observations of the Author. His criticisms, in regard to one or two matters, would be rather with reference to his practice as an engineer than to his present knowledge of the management of gasworks. He thought that many of the difficulties alluded to by the Author might be overcome by an enlargement of the purifiers. Thus the Author spoke of 0·30 square foot of area in each purifier in a set of four for every 1,000 cubic feet of gas per diem to be purified, and stated that in working he got occasionally stains of sulphuretted hydrogen on the outlet of the last purifier of the series. Mr. Stevenson made his purifiers double that capacity, namely, 0·60 square foot for every 1,000 cubic feet of gas per diem to be purified, and there was no stain of sulphuretted hydrogen on the outlet of the four purifiers. He worked all four purifiers in rotation, and all at the same time, and he found that the second and third purifiers, containing sulphide of calcium, did take out largely the sulphur compounds. He could not give figures as to the extent of the purification from sulphur compounds. In the provinces, where his practice chiefly lay, there were no very stringent regulations with regard to those compounds. He dissented from the Author's statement that there was no force in the objection, sometimes raised, that the cooling of gas should not be too sudden. He could state from experience that naphthaline was produced more by a sudden cooling than from any other cause, and that was a nuisance which all gas engineers and managers strove to avoid. In his own practice he made the gas travel for ten minutes (there was no magic in the ten minutes, but in the area which had to be provided) before it reached the purifiers from the retorts. In that way it was cooled gradually, and by the time it reached the condensers out of doors it deposited very little tar. He took a light wrought-iron boiler-plate foul-main all round the walls inside the retort house, separating the tar from the gas immediately the two left the hydraulic main. There was thus a very gradual cooling of the gas, and when it was thoroughly washed, the purifiers had comparatively little to do. He did not agree with a remark that occurred in a Paper lately read before the North of England Gas Managers Association to the effect that with a properly-sized main from the hydraulic, together with sufficient condensing and scrubbing power, the only need of the oxide or the lime purifiers was to give the gas

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what might be termed "just a finishing touch." The purifiers did a great deal more than give a "finishing touch" to gas. They did the bulk of the work of purification; but the gas should be properly prepared before it was permitted to enter them—properly condensed and washed. The purification of gas began the moment it left the retort. By the cooling process the tar and ammoniacal liquor were deposited, and by the washing process a large portion of the carbonic acid was got rid of, as well as some of the sulphuretted hydrogen, and perhaps some of the sulphur compounds. Mr. Stevenson.

Dr. C. William Siemens said about eight years ago Mr. Vernon-Harcourt spoke to him on the subject of his process, and inquired whether an apparatus could be designed by which gas could be cheaply brought up to a certain temperature, passed over a given amount of surface, heated to a certain point, and then cooled down again. After consideration he saw his way to a solution of that problem, and on receiving a letter from the Board of Trade asking him to co-operate with Mr. Vernon-Harcourt, he designed the apparatus to which reference had been made. He had nothing to do with the chemical part of the subject, which was left entirely to Mr. Vernon-Harcourt; but he had no doubt as to the practicability of the apparatus. It had been put up only for experiment, and not with the care and expense that would be bestowed on an instrument for regular use. The gas was heated to a certain point in passing up one regenerator, and it was passed over an extended pebble surface, to which he had added, in a central compartment, a considerable surface of iron turnings; and then, having been passed over that heated surface, it was brought back gradually to an ordinary temperature. The apparatus seemed to answer its purpose perfectly; some of the tests showed a low degree of sulphur; but in course of time it seemed to diminish in its power to eliminate the sulphur, and, on subsequently opening the apparatus, it appeared to him that the favourable result in the first instance had been due chiefly to the iron turnings put in at the hottest portion. The conclusion appeared to be that if iron in a divided condition was presented to gas in a heated condition, the bisulphide of carbon was decomposed and the object attained. It would be a question for gas engineers to decide whether, with such a condition attached to the process, the consumption of a certain amount of iron, would still be an economical one. If so, he believed there would be no difficulty in overcoming some of the mechanical drawbacks inseparable from a mere experimental apparatus. But Dr. Siemens.

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Dr. Siemens. he had no further interest in the question than the love of contributing to a scientific experiment. He had no practical experience of the process of purification by lime, and therefore could not say to what degree sulphur might be thus eliminated; but the question was one of such great importance that he should be sorry to see gas companies discourage experiments in the direction proposed by Mr. Vernon-Harcourt, or indeed in any other direction. The cost of an experiment of that description, though it might amount to some hundreds of pounds, was as nothing when compared with the enormous interests involved in the destructive effects produced by sulphur in gas. There was one way of getting over the difficulty, which had been already alluded to by Mr. Airy, viz., that of making gas of materials free from sulphur, or (which was the next best method) using materials, rich in carbon and free from sulphur, for enriching ordinary gas. He believed that a good and cheap gas could be produced by enriching gas of a poor description, such as could be made in one of the gas producers ordinarily employed by him for furnace work. This gas had a low illuminating power, but if enriched by volatile hydro-carbons, its illuminating power might be made high; and with a little competition the gas companies might find the means of still further improving the quality of the gas in regard to its freedom from sulphur.

Mr. Chester. Mr. W. R. Chester said that during the last twelve months he had encountered somewhat the same experiences and difficulties at Greenwich that the Author had had to contend with at the Commercial and Ratcliffe works. The great thing to do in order to comply with the requirements of the Gas Referees had been to form a sulphide of calcium, or at least a compound of lime that would pick up and hold fast the sulphur compounds in the gas. The Author seemed to have accomplished this result in a manner similar to that in vogue at several other metropolitan works, viz., by working one or a series of oxide purifiers first, to eliminate to a large extent the sulphuretted hydrogen, which he believed to be detrimental to a sulphide purifier when once formed. By interposing these oxide purifiers, he could not only form a sulphide purifier, but keep it in an active condition to arrest the sulphur compounds for almost any length of time. This he could do with a purifying area per purifier of 0·30 superficial foot per 1,000 feet maximum make of gas per day, and with a scrubbing capacity of $3\frac{3}{4}$ cubic feet per 1,000 cubic feet per day, with a make of 28 gallons of 8-ounce liquor per ton. With almost exactly the same conditions at Greenwich it was impossible to form

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directly a sulphide purifier that would eliminate more than from 3 to 6 grains of sulphur compounds. With a purifier having an area of 0·34 superficial foot per 1,000 cubic feet maximum make, and a scrubbing capacity of 8 cubic feet per 1,000 feet, and in addition a Kirkham's washer, with a total make of 27·5 gallons of 8-ounce liquor, a compound was still left in the gas that prevented the formation of a sulphide of calcium, or indeed any other form of calcium that would lay hold of the sulphur compounds. After a purifier had been on for some time, and sulphuretted hydrogen had passed freely into it to the point of saturation, CO_2 having previously been scrupulously taken out, there was next to no effect in the diminution of the sulphur compounds, and not until the purifier-box had been emptied and the lime well watered, thrown into a heap and exposed to the air, did it become an active power in reducing the sulphur compounds. When the lime was taken out to be watered and turned, it was a pale pink colour, as delicate as the internal colour of a blush rose, rather wet and nearly odourless, and would not pick up more than 3 grains of sulphur. But after being exposed to the air it rapidly changed to a dark green colour, evolved much heat and steam, and became an active agent for arresting sulphur, sometimes picking up as much as 25 grains immediately after it was replaced in the box. It then continued to take up sulphur compounds, gradually getting less and less active till it became inert, and began to give up sulphur. The inability to form an effective sulphide purifier, except by indirect means, was not due, he believed, either to carbonic acid, ammonia, or to too much sulphuretted hydrogen, for he had been able experimentally to form an effective sulphide of calcium when all three impurities had been present in quantity, but it was due to the baneful presence of another compound, the constitution of which was at present unknown, but some of the peculiarities of which, however, had been discovered during the last twelve months—(1.) It was broken up in passing lime, a portion forming ammonia, and a portion combining with the lime, and making it inert on the sulphur compounds. (2.) It was nearly all soluble in water when the water was present in large quantities. (3.) And present in such quantity in the gas that 10 cubic feet of water would wash it out of 2,000 cubic feet of gas by pumping it round and round through a scrubber at a rate half as fast as that at which the gas was passing. That compound once out, a sulphide purifier active in reducing the sulphur compounds could be readily made in the presence of carbonic acid and ammonia. He had a sample of the water containing that com- Mr. Chester.

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Mr. Chester. pound, and it had some peculiar characteristics; it was neutral to litmus and turmeric paper, and apparently free from sulphuretted hydrogen, as it would not discolour lead paper. Although 2,000 cubic feet of gas, containing both sulphuretted hydrogen and ammonia, had passed through it, it was nearly odourless; it contained a dark grey precipitate and traces of cyanogen and hydrocyanic acid. The lime used was 0·9 bushel per ton of coal carbonised, or about 1·3 bushel per 15,000 feet of gas, and cost 0·47*d.* per ton.

Mr. Jones. Mr. H. E. JONES, in reply, said he had been interested in the remark made by Mr. Chester to the effect that he had found as much as 6 grains of ammonia developed where it had not been previously observed at the outlet of the lime purifiers. He had called the attention of the Referees to the fact two years ago, and some alteration was made in the limit of ammonia in consequence of his making the fact clear to their minds. With regard to the observations of Dr. Siemens and Mr. Airy, he thought too much stress was sometimes laid upon the impurities left in the gas. The weight of 100 cubic feet of gas (which was more than a 5-feet burner in an ordinary room would consume in twenty hours) was 23,000 grains, and supposing that the sulphur contained in it amounted to 23 grains, it only formed $\frac{1}{1000}$ part of the whole. In three or four hours the amount developed would be only $\frac{1}{50}$ per cent., and if the room was decently ventilated the chances were that it would contain only $\frac{1}{50}$ of that $\frac{1}{50}$ per cent. to affect the books and the lungs; so that he thought people need not trouble themselves very much about it. But those who declaimed against gas did not hesitate to burn it in a most barbarous and unsuitable manner, despising the improved burners of Mr. Sugg and Mr. Wright, and burning the gas in the most unwholesome and extravagant way, filling the room with smoke and soot, and with unnecessary products of combustion, principally carbonic acid, and then turning round to the unhappy gas manager and saying, "This is what you have done for us." With reference to the observations of Mr. Stevenson, he desired to say that he by no means fixed 0·30 superficial foot area per 1,000 feet of maximum daily manufacture as his choice of the size of purifiers, but that happened to be the size of some purifiers which he had found to be operative, and on comparing the figures of the principal gas companies he found it to be the minimum upon which the ordinary rotative process of lime purification was effected. Below that the lime would be useless. He was the last man to quarrel with anyone who pointed out the significance of sulphur impurity. It was

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Mr. Jones.

the duty of gas managers more than any others to remove anything from the gas, no matter how small in quantity, which might be objectionable; and for himself he should like to see gas employed in the conservatory and in the boudoir of the most refined lady. But there was a common impression that all that was necessary was to fill the purifiers with lime, and success would immediately follow. He confessed that he had not been successful. If he had the acreage of some of the rural gas companies instead of being crowded by buildings, and having to pay so much for land, he should advise a much larger proportion. Only lately when he, with Mr. Robert Jones, was laying out some large works in the east end of London, they had been struck by the fact that, owing to the present most approved systems the purifiers were becoming almost the largest plant in the works, occupying nearly as much cubical space, and being nearly as costly, as the carbonising apparatus which produced the gas in the first instance. He had been interested in listening to the remarks of Mr. Clarke, than whom no one was more competent to speak on the question of sulphur. He had manufactured a large amount of gas daily for many years, and he was untainted by the reproach of ever having once failed to bring the amount of sulphur within the limit required by the Referees. With regard to the purifying process of Mr. Vernon-Harcourt, referred to by Mr. Clarke, he did not think that it ought to be condemned, or put on one side too readily. Gasworks were not built in a day, and purifiers were not brought to their present system of working in a day. The experiment made at Horseferry Road station could not have been full and complete. The test-lamp, to which he attached so much value, illustrated the possibility of converting a form of sulphur, which did not otherwise exhibit itself upon lead, into another form which was easily shown upon lead, and easily removed by oxide of iron. That being so, he was sure, if sustained effort were spent upon that plan, that something might be done. He did not say it was the cheapest or the best way, but there were elements of success in it. He agreed with Mr. Keates, that it was only a small portion of the lime that was fouled with the sulphuretted hydrogen, and which afterwards became such a nuisance to their neighbours, which was really effective. He had not the slightest idea of casting any disrespect on Dr. Letheby's name. Dr. Letheby, however, had certain short and ready methods of coming to conclusions on the matters in question, and when they were conveyed to Committees of the House of Commons and of

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Mr. Jones. the House of Lords, they often resulted in the issue of a Bill with limitations and penalties upon gasmakers, which had proved very irksome. It was most intolerable to him that he dared not make experiments. Not many months ago he wanted to change the sequence of two sets of purifiers, but he was afraid to do so because he thought it might lead to a breach of the regulations and to consequent penalties. He went to Dr. Pole, one of the Gas Referees, and asked if he could relieve him. Dr. Pole replied, "No, you must go to Dr. Williamson, the Chief Gas Examiner;" and when he saw Dr. Williamson, he said, "I cannot help you; I cannot authorise you to break the law." It was Dr. Letheby's view that all the sulphur could be washed out of gas with ammoniacal liquor and scrubbers, and he enunciated it in 1875, when Mr. Jones's former Paper was read; but that view was completely exploded, and he did not believe that any one would now assert that by washing with ammoniacal liquor the slightest reduction could be effected. Dr. Letheby quoted the case of the Staffordshire Gas Company which never suffered from naphthaline, because they washed the gas sufficiently. Mr. Jones's father, who had worked the Staffordshire district coals for many years, never knew of naphthaline either, but he did not wash the gas. It was not there because it was not in the coal. He had been asked by Mr. Gandon his opinion as to the desirability of removing the sulphuretted hydrogen first, instead of the carbonic acid. He had found that when he removed the sulphuretted hydrogen first, the lime produced was quite inoffensive, but there resulted an enormous pressure. He had known a single purifier with primarily not more than 1 inch head of water-resistance increasing, at the point of complete saturation, to 16 inches head of water. He had adopted the plan he had mentioned on account of the great pressure from the purifiers when the lime was carbonated without being first treated with sulphuretted hydrogen. Mr. Patterson had complained of what he called his inadvertency in pointing out certain "fallacies." In spite of what had been said to the contrary, he maintained that they were fallacies. If oxide-of-iron purifiers could remove sulphur and give it up at other times, it could at least be only a very casual thing; it must take it up at one time and give it up at another, the balance remaining the same. But the conclusion drawn in one of the Referees' reports was that oxide of iron was continually giving up sulphur. It was impossible; the sulphur must come from somewhere. He was working with a methodical and regular system, and he had never yet seen an instance of the kind. He was not speaking of a single

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experiment, but of two daily at three works for nearly three years each—experiments that he could never have made if it had not been for the short and expeditious method of testing of Mr. Vernon-Harcourt. The scrubbers were an instance of the same; he had never found any reduction of sulphur from the scrubber, and on the other hand he had never found any contribution. The particulars given by Mr. Vernon-Harcourt, as to the relative duty of a cubic foot of lime per cubic foot of gas were very valuable, and he thought they would all clear the ground if they took the same form of expression. There was a good deal of obscurity in the way in which returns had been made, where superficial dimensions were given in contrast with cubic dimensions. Unless, as in purifiers, the depth followed a general rule of proportion to area, such returns were almost valueless. With regard to the exposure of lime to air, he was uncertain whether it was a chemical action or a breaking-up action such as Mr. Vernon-Harcourt had suggested, but the result was beneficial. He was rather disposed to think that it was chemical, because when, instead of drawing the air through, the lime was thrown out of the purifier and put back again, it had undergone changes which to the eye and to the nose indicated a change in constitution, and not merely in mechanical arrangement. With reference to the sulphiding at low temperature, he had reason to believe that the temperature was not as low as the freezing point of water. He did not believe that water in combination with lime was crystallised. As a matter of fact, at ordinary temperatures in experimental glass purifiers the lime was not active. It was stated in the Paper that the cost of the purification was something less than an additional penny. He wished he could say that the question remained there. By those, however, who had the direction of gas companies, both in London and in the provinces, the cost was always put on one side, and he had never been asked to economise on any such subject. If that were the only cause of anxiety he would cheerfully forego any question or discussion about it, but it was an anxious matter, and a satisfactory result could not be accomplished without the greatest vigilance on the part of the manager and his staff. The consternation amongst his own foremen when anything was wrong in that respect, or when a penalty was imminent, could only be compared to what might take place if the premises were on fire. He should like to see some inducement held out, and some latitude given. Gas engineers were forbidden by the circumstances to make experiments on a practical scale, and could not take the slightest step beyond the process they had empirically

Mr. Jones.

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Mr. Jones. arrived at, for fear of the conditions being disturbed. They dared not move, and the result was that they did not advance; nor would they advance until there was some change in the law.

Correspondence.

Mr. Anderson. Mr. G. ANDERSON considered the Paper a valuable contribution on an interesting subject, hitherto but imperfectly understood. Since Mr. Patterson, the late Gas Referee, pointed out that the difficulty of removing the sulphur compounds, other than sulphuretted hydrogen, was in consequence of the presence of carbonic acid, it had been easy to comply with the requirements of the law, but there was still the difficulty of doing so without creating a nuisance. That difficulty, however, he believed had arisen, and still continued, from the want of apparatus of adequate capacity. Although his provincial works were not under the law that applied to London, he was much interested in the embarrassment in which the London gas companies were placed. Some years ago, he caused gas to be tested, first with the Referees' test, and subsequently with Mr. Vernon-Harcourt's colour tests, while working with lime in four purifiers. The tests varied from 12 to 16 grains per 100 cubic feet. The washer and scrubber, however, were very effective, and removed a large quantity of carbonic acid and sulphuretted hydrogen. He also made strong liquor, of 14- to 16-oz. strength, and found from experiments that such liquor contained double the quantity of impurities per ounce of strength that weak liquor did. Hence he considered it not only advisable thoroughly to remove those impurities before the gas reached the purifiers, but to do so in a certain manner, namely by producing strong as against weak liquor. As the strong liquor was also more valuable to a chemical manufacturer, he saw no reason for the existing custom of producing weak liquor of only 9- to 10-oz. strength. As to the purifiers, it was a tradition that the lime should be spread on the trays in layers about 3 inches thick, and of such dampness that it would squeeze into a solid mass in the hand. Both of these he considered most inadequate conditions. For many years he had increased the thickness gradually, and for the last eight or ten years he had put in layers of lime 10 to 14 inches thick. Some people thought that such thick layers must give great resistance, but he did not find it so, because he ignored the theory as to dampness, and made the lime as wet as it could be, consistent with its not falling into paste. In such a state the lime existed in nodules from the size of a walnut down-

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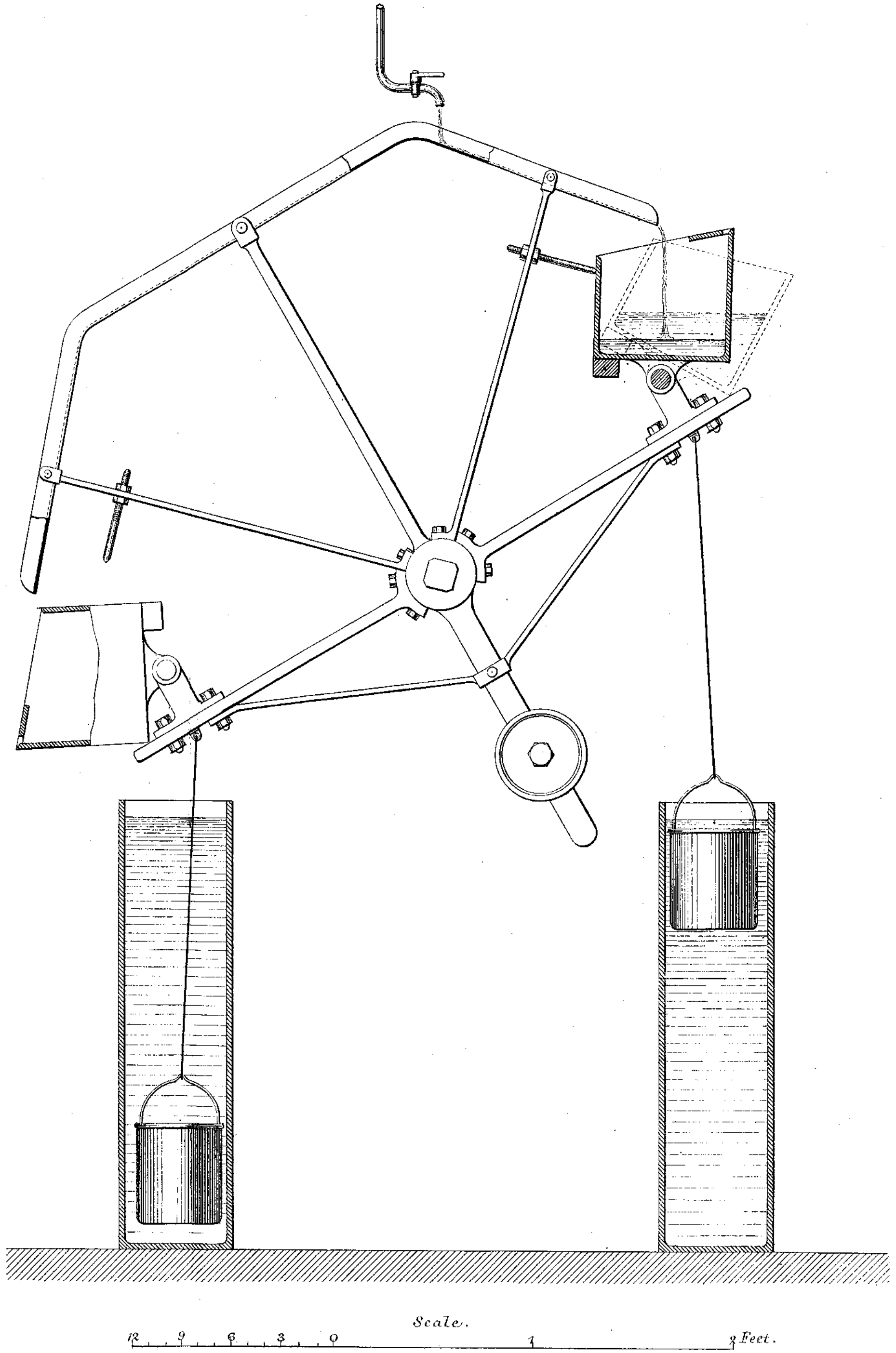
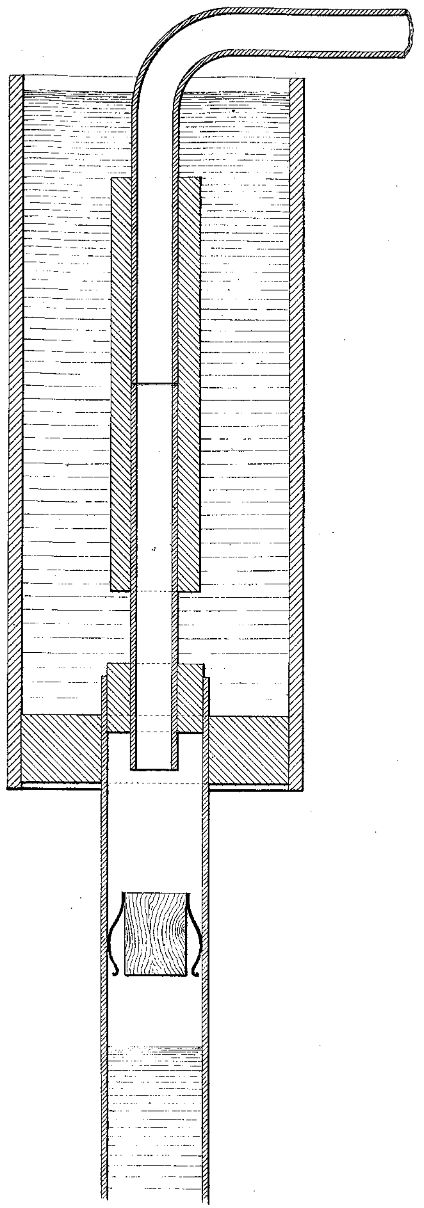
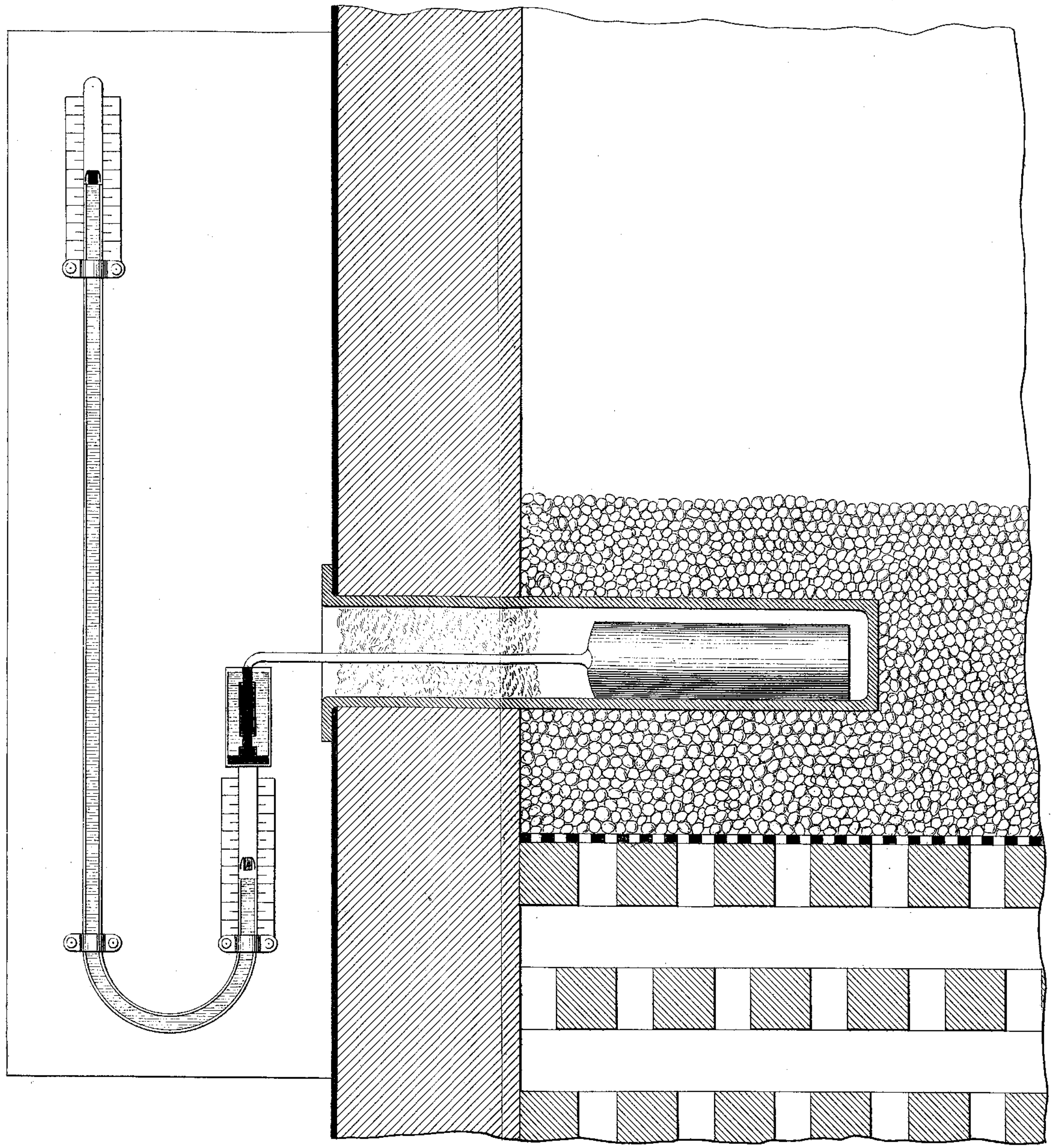
THE PURIFICATION OF GAS.

PLATE. 2.





SECTION OF GLYCERINE JOINT,
ACTUAL SIZE.



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