Dr. SIEMENS said it was exactly four years since a Paper on the subject of pneumatic propulsion had been brought before the Institution by Mr. Carl Siemens.[1] The object of that Paper was to describe a system of propulsions in tubes which had been matured by his brothers and himself in the course of years, it being a system of continuous flow, or a circuit system. This had been established in Berlin in 1864, in London in 1869–70, and in a modified form in Paris in 1871–72. The scheme had been submitted to the Postmaster-General several years previous to its application in London. The object was to despatch letters throughout the metropolis by a system of circuits, uniting in one or two common centres or pumping stations, whence parcels would be sent out every five minutes to a number of receiving and transmitting stations lying in a circle (similar in appearance to that shown on the diagram representing the Paris system). The current flowed round always in the same direction, conveying with it a succession of carriers passing from any one station to any of the others. The system differed materially from the former method, by which one carrier was sent through a tube in one direction, and went back by vacuum in the opposite direction. Sir Rowland Hill looked favourably upon the scheme, and he was indebted to Mr. E. A. Cowper, M. Inst. C.E., who was at that time frequently consulted on engineering matters by the Post Office, and had previously worked on a similar subject, for his support in recommending its adoption. The present Paper discredited, to some extent, the circuit system, for which it proposed to substitute a "radial system." He was not inclined, however, to accept the verdict of the Authors of the Paper, who, he believed, had not stated all the elements upon which this question should be judged. The circuit system, when first established between Telegraph Street, the General Post Office, Fleet Street, and Charing Cross, was considered a complete success; the postal authorities asked several scientific men and gentlemen connected with the Press to observe its results, and they were extremely pleased with them, but since that time there had been a disposition on the part of their engineers to substitute the radial system. The first objection raised against the circuit system was, that no advantage was derived from it between Telegraph Street and Charing Cross, and that consequently the circuit had been broken up. Fig. 28 represented the circuit as originally established. Pressure

[1] *Vide* Minutes of Proceedings Inst. C.E., vol. xxxiii., p. 1.

was maintained in one reservoir, and vacuum in another, and the flow of air was always in one direction, carriers being introduced at the points indicated on the diagram through switches of a simple construction. It would be observed that the circuit was a very oblong one, the intermediate stations on both halves being locally united for the convenience of the traffic. The alteration since made consisted in the removal of the arc connecting the two branches ending at Charing Cross, so that the air flowing from the pressure reservoir was discharged into the atmosphere, and the atmosphere introduced at Charing Cross flowed to the vacuum reservoir. It so happened, however, that the pressures marked at each station remained at every point of the circuit the same, Charing Cross being just half-way on each branch of the circuit; and, although he quite agreed that it might be convenient to take away the connecting link, and to work each half with the atmosphere inserted in circuit, it made no dif-

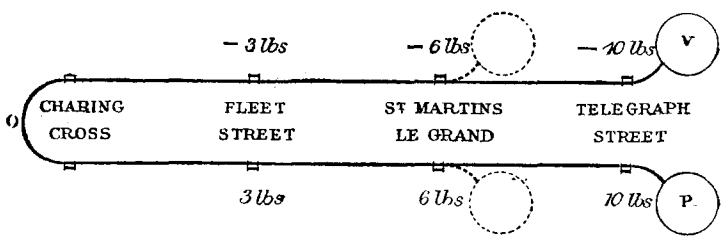



Fig. 28.

ference whatever in the principle of working. It had been intended originally to extend the circuit to Westminster; and if that intention had been carried out, the intermediate instruments at Charing Cross would have been indispensable. Although the present system was not worked as a circuit, it was worked on the same continuous method, and it would be observed that the postal authorities had adopted another similar open circuit between the General Post Office, Cannon Street, and Thames Street, which he had no doubt worked equally well, and went far to prove the advantages of the system. Another complaint was, that the iron pipes employed by him (Dr. Siemens) in laying down the Charing Cross circuit were apt to rust in consequence of the use of injection water in the air-pump which had since been discontinued; it was also stated that injecting water into the air-pump was accompanied by a waste of power. He entirely dissented from the latter proposition. He had prepared a diagram (Fig. 29) showing the curve of compression: if a piston travelled in the cylinder, the pressure

would rise, in the manner indicated by the dynamical curve, which compression was accompanied by a rise of temperature from 60° to 170° Fahr., in bringing up the pressure to double that of the atmosphere. By injecting cold water, not only was the cylinder lubricated as stated in the Paper, but the heat was absorbed by the water, the result being that the increase of pressure would not take place in the ratio indicated by the dynamical line, but in that indicated by the other line, which represented the ratio of isothermal compression. Injecting water therefore was not a source of loss of power, but of gain of power. Probably the quantity of water injected had been too small, and in that case no doubt vapour would be carried over into the reservoir. The postal

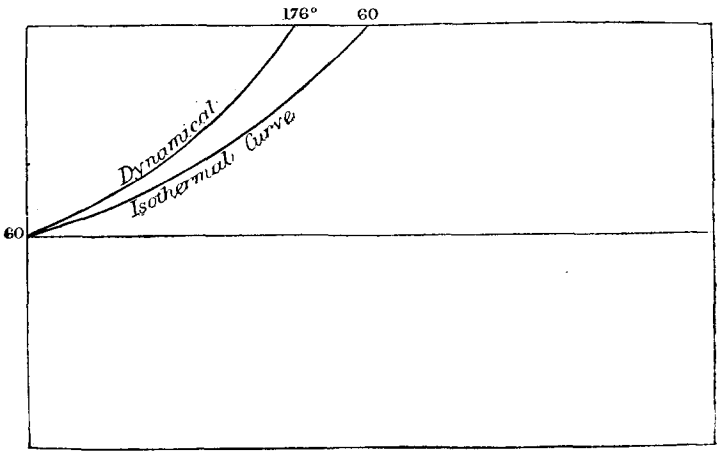



Fig. 29.

authorities had done away with the reservoir altogether, which, he thought, was a mistake, because it was necessary to allow the water to settle and the air to become dry and cooled down to the point at which it was fit to enter the pipes. As regarded rusting, the Authors themselves stated that in Paris, where the air was compressed by water, no inconvenience had been observed on that score, nor had any such effects been experienced in Berlin. If rust had given trouble in the circuit in question, he considered it was entirely due to the mode of working. No doubt a lead pipe was better in some respects for small diameters, but when he designed the circuit, cost was a very important element. He could not afford to have lead pipes inside cast-iron, the cost of which might suit the Post Office, now that the authorities were accustomed to spend their millions somewhat freely;

but at the time to which he alluded they were in the habit of going closely into estimates. No doubt it would have been better to line the inside of the pipes with softer metal, such as tin or lead, in order to obtain a smooth working; and he had proposed to tin the inside of the pipes, but that had to be negatived because it would have been too costly. He should have been glad of the opportunity of comparing the estimates of the two descriptions of pipe, believing, as he did, that one would cost several times as much as the other. Another objection raised in the Paper against the circuit system had been that time was lost at the intermediate stations. He did not, however, see the force of that objection. It was very important that the time of transit from the central station to the extreme end of the system should be as short as possible; but there could be no practical object in shortening the times of transit to the intermediate stations. He would take the case of the second continuous circuit established in London. It had been stated that, in working the circuit from the central station to Cannon Street and Thames Street continuously, the time of transit from the central station to Cannon Street was sixteen seconds more than when the latter was worked as a terminal station, the times of transit being seventy-two and fifty-six seconds respectively. That might be so; but he thought it was rather an advantage than otherwise to retard the flow in so short a tube, and the intermediate station in Cannon Street did not in any way diminish the speed of the flow from the central station to Thames Street. If the two were worked as separate continuous circuits, more than double the air would be consumed as compared with that required to work the three stations on the circuit system. If it were so desirable to diminish the time of transit, it would be much better to increase the diameter of the pipe. In that case there would be an advantage for both stations in point of speed and working capacity, and engine power would at the same time be saved. He thought, therefore, that the objection raised against the intermediate station did not hold good. Another objection was that the iron pipe caused more friction than the lead tubes. No doubt there was a little more friction to the carrier; but, seeing that this constituted a very slight amount in the total friction of the transit of air through the pipe, it was not a serious matter, and could have been avoided if the inside of the iron tube were simply covered with a soft metal.

He had certain objections to make to the theoretical part of the Paper. The Authors started with Zeuner's formulæ, expressing the dynamical effect produced in allowing air to expand from one

pressure to another. They presumed that if the air flowed into a long tube, it expanded in the same manner as if it were allowed to push a working piston forward, which, however, was not the case. If compressed air were allowed to re-expand behind a working piston the temperature would fall in precisely the same ratio in which it would rise in compression, the heat lost being the equivalent of the force communicated to the piston. But was it the same if air expanded into a long pneumatic pipe? Certainly not. There was in that case no working piston with resistance behind it, the carrier piston consisting only of a piece of hose containing some slips of paper, which offered practically no obstacle. All the resistance that had practically to be dealt with in the pneumatic pipe was that of the air itself. Suppose air of 2 atmospheres pressure were admitted at one end of the pipe (which might be 1 mile or 3 miles long), the pressure would taper down to atmospheric pressure at the opposite end. No work was accomplished here, except that exerted upon the air itself in being pushed through the tube, which, therefore, became the recipient, in the shape of heat, of all the force which had been exerted, and the result was that the expansion of the air from 2 atmospheres to atmospheric pressure would not be accompanied by any decrease of temperature. Therefore the dynamical formulæ regarding the force and volume of air expanding behind a working piston did not apply to the case of a pneumatic pipe. Assuming that the pipe itself was a non-conductor of heat, and that the temperature of the air on entering the pipe was the same as the temperature of the pipe itself, he maintained that the air would flow out of the other end of the pipe at exactly the same temperature as that at which it entered. Taking the case of a pipe of conducting material, and assuming that the air entered the pipe at 2 atmospheres pressure and at the temperature of the pipe itself, the temperature at which the air left the pipe must be in excess of that of the compressed air when it entered, inasmuch as the latter had work to perform; it had to push forward the air and overcome its friction against the side of the tube; and, inasmuch as work was performed in the early part of the operation, the temperature of the air would diminish. Heat would be communicated from the tube to the expanded and cooled air; but towards the end of the transit no work, excepting friction, had to be performed, and all the heat that had been picked up by the air in the early part of its transit would appear in the form of additional free heat at the end. After this explanation, he hoped that the Authors would

agree with him that the co-efficients in their formulæ, taken from the dynamical action of expanding air, were not applicable. It might be mentioned that the experiments given at the end of the Paper exactly confirmed his view. In other respects the theoretical considerations involved in this subject had been put forward in a complete and elegant manner, and some of the experimental results were extremely valuable.

Regarding a comparison of the radial with the circuit system, he believed that the advantage was with the latter. The radial system implied a greater number of tubes; and it was, therefore, wasteful in point of cost. It implied, if the radii were worked on the continuous system—which was almost necessary where there was so large a traffic as in London—a greatly increased consumption of compressed or rarefied air, as the case might be. Moreover if there were, say, twenty or thirty stations round the central station it would be practically impossible to lay as many tubes radiating from one centre, each tube consisting of a leaden pipe surrounded by an iron one. The streets would not be sufficient to contain such a number of tubes. Although the radial system might do for collecting messages from offices in the immediate neighbourhood of the central station, he felt sure that whenever the time came for the establishment of the pneumatic despatch system on a large scale, requiring larger diameters and a combination of hundreds of stations (so that a parcel could be sent from any one station to any other), it would be impossible to carry out such an object by the radial system, and a return to the circuit system would be absolutely necessary.

Mr. PREECE observed that, as the representative of Mr. Culley, he had to explain, as far as he could, the illustrations of the working of the pneumatic system. Dr. Siemens had said that the Post Office was a department in the habit of spending millions. That might be so; nevertheless it possessed individuals who were in the habit of exercising all their talent and ingenuity in producing economy and efficiency in working. It was for that reason that he wished to direct the attention of the Institution to one portion of the apparatus which, he thought, exhibited great ingenuity. He referred to the valve. The tubes used in the radial system were worked, when the business was slight, intermittently, first by vacuum, and then by pressure; and where the business was great they were worked continuously both with pressure and with vacuum. It therefore became a question of considerable importance to devise a valve which should, in the hands of a messenger boy, enable them to work under these four conditions without

difficulty. The valve represented in Plate 5, Figs. 1, 2, and 3, had been in incessant work for twenty-one months. To obtain the four different objects required, with one simple manipulation, was an exceedingly difficult mechanical operation. A piece of lead pipe, that had been in constant use for twenty-one years between Telegraph Street and the Stock Exchange, had been worn to a beautiful smoothness, but there appeared to be no diminution in its thickness.

He now proposed to speak on his own behalf. It was twenty-one years since, as assistant to Mr. Latimer Clark, M. Inst. C.E., he first became connected with pneumatic telegraphy; and if experience justified the expression of opinion, an experience of twenty-one years would be a sufficient justification for his addressing the Institution. In the course of last autumn he had visited Brussels, Berlin, Vienna, and Paris, with the consent of the Postmaster-General, to inspect the telegraphic, especially the pneumatic, arrangements of those towns, and he would briefly state the results of that inspection. At Brussels, the pneumatic telegraph was simply employed in blowing messages from the counter to the instrument room at the top of the building. The same process was adopted to a considerable extent in London, there being no fewer than twelve post-offices where a simple blower was employed to drive the messages from the counter to the instrument room. At Berlin, pneumatic telegraphy had received a somewhat wider extension. Dr. Siemens had, on several occasions, described the working of that system. There were two such telegraphs in Berlin —one connecting the central telegraph station with the Bourse, 800 mètres in length, and the other with the Potsdam and Brandenburg Gates, 2,200 mètres long. Both were originally worked on Messrs. Siemens' continuous, or circuit system, consisting of two pipes, through which the air was driven in a continuous stream. He was not, however, much surprised to find that that method had been abandoned.

In Paris, the central station was situated on the "other side" of the Seine, in a position somewhat similar to that occupied by the Waterloo Station in London. There were altogether seventeen stations in Paris, connected by a length of 14 miles of pipes. There were three circuits, as shown in Fig. 4, p. 117. The system was worked by steam and by water. At the central station there were two steam-engines, one being in reserve. At the Rue de Boissy there was a water-compressing engine. The water from the mains was used to compress the air in a cylinder, and by a very ingenious contrivance also to produce a vacuum. The same thing

obtained at the Grand Hotel. At the Bourse and at the Great Northern Railway station, there was steam power. There were thus, in Paris, three separate and distinct steam-engines, and three separate and distinct water-engines. Trains were made up from the central station every quarter of an hour. There were seven stations to serve. Seven distinct carriers, with the names of the stations on them, were inserted in the tube. From the Rue de Boissy to the Bourse they were sucked by a vacuum, and from thence they were driven by pressure round the rest of their journey. There was thus a regular circulation of carriers throughout the whole city. The daily average of messages during the month of August was 7,404. He had not the exact statistics for Berlin; but he believed the number there was about 3,000 daily.

The Vienna system was almost identical with that adopted in Paris. At the central station there were two engines of 26 HP. each, one of which was kept in reserve. The engine worked pumps which compressed and exhausted air in four cylinders marked G, G, H, H (Plate 7). The pumps and cylinders were connected by a thick pipe with similar reservoirs at the Fleischmarkt Station, No. 2; so that the engine which exhausted and compressed the air at one station did the same thing at the other. There were also two engines in the Gumpendorf Station, marked C D, C D, which exhausted and compressed air in the cylinders G G, H H. Trains of seven carriers each were driven at regular intervals. They were forced in one direction by pressure, and drawn in the same direction by vacuum. At certain points—the Bourse, the Landstrasse Station, and another—there were single pipes, and the carriers were forced in one direction and drawn in the other. The system adopted at Vienna and Paris differed from that employed in London in two essential particulars. In the first place, the former system was the circuit, and the latter the radial. In the next place, throughout Paris and Vienna iron pipes were used. In deciding upon any pneumatic or telegraphic system, the first question the engineer had placed before him was that of speed of transmission. Without speed, telegraphy was almost useless. If it took as long to send a message from Great George Street to the City, as it would to send a commissionnaire, very little business would be taken to the wires. The Postal Telegraph department in London had decided that no message should occupy more than five minutes in transmission from one point to another. They had endeavoured to secure that rate of speed throughout the whole country, and had succeeded; and it was mainly owing to that fact that the number of messages had risen in five years

from 6,000,000 to upwards of 20,000,000. Whatever benefit there might be in the circuit system compared with the radial, it was also necessary to consider the adaptability of the two systems to particular places. The circuit system might be specially adapted to Paris, but it was altogether impracticable and inapplicable to London. In Paris, the chief telegraph station was not central at all, but was at the outskirts of the city. If it had been at the Bourse instead of at the Rue de Grenelle, several sides of a polygon might have been dispensed with, a length of no less than 10,622 mètres of pipes might have been saved, and the engine power could have been concentrated in one building. In London the stations, instead of being scattered as in Paris, were concentrated round the central station, and they were all distinct and independent of each other, requiring only communication with the central station. The average daily number of messages dealt with was 17,704; and considering the limit of speed adopted, the work could only be accomplished on the radial system. In Paris, a person might arrive at a station just after a train had left, and would have to wait a quarter of an hour before another was despatched, and then the transmission of his message to a short distance might occupy ten minutes more, and thus delay it twenty-five minutes. In Vienna it was still worse; there was often a delay of thirty-three minutes. People on the Continent did not appear to realise the desirability of cultivating great swiftness of transmission. In establishing the Parisian system, the problem solved was to insure that no message should exceed at the most an hour and a half in transmission. At Vienna, the messages were so much delayed that the people at the Bourse employed velocipedes; whereupon the authorities were compelled to lay down a distinct pipe, on the London system, to serve the Bourse. In fact, wherever the business on the Continent required it they were forced to adopt the radial system. To adopt the circuit system in London would be like taking a passenger, who wished to go from Great George Street to the City, round by Willesden Junction; or serving Dover, Hastings, Brighton, Southampton, and Bristol by a single line working through Reading.

In introducing lead pipes in London the authorities had not committed so great an offence as Dr. Siemens appeared to impute to them. No doubt the Post Office had spent millions for the benefit of the public; but many persons in connection with that establishment exercised their ingenuity in providing economical arrangements, and in introducing lead pipes instead of iron that policy had not been departed from. Dr. Siemens

had said that he could not employ iron casings to lead pipes in consequence of the extreme cost. What, however, were the facts of the case? Dr. Siemens' firm had laid iron pipes between the General Post Office and the Strand Station at a cost of 15*s.* a yard. The expense of the iron pipes laid in Paris had been 13*s.* 8*d.* per yard. At the time when the Messrs. Siemens were laying iron pipes between the City and the Strand, Messrs. Reid Brothers were laying lead pipes, cased with iron, at a cost of 13*s.* 3*d.* per yard. The average cost in London had been 12*s.* 8*d.* for a $2\frac{1}{4}$-inch lead pipe in a 3-inch iron casing. They had also 3-inch lead pipes in 4-inch iron casing, larger than any that the Messrs. Siemens had laid down, and the cost was 17*s.* 4*d.* per yard; so that the cost of the pneumatic system was not, as Dr. Siemens had stated, "several times" as much as the other. On the contrary, the London system was cheaper than the Continental. He might be permitted to state that the authorities had determined to put lead pipes inside those laid down by the Messrs. Siemens.

When the subject was first brought before the Institution, he had ventured to propound a theory as to the motion of air through tubes. He had on that occasion endeavoured to show that the motion of carriers through tubes was a function of the mass of air, rather than of its volume, that it was a case of uniform motion, and that it was in reality due to a difference of pressure. Mr. Culley and Mr. Sabine had endeavoured to prove that the motion of air was due to the expansion from the higher pressure to the lower. What, however, was expansion but simple motion? and to what was the motion due, if not to the difference of pressure? According to the molecular theory of gas, the molecules were in ceaseless motion, and it was their impact against the sides of the containing vessel, or their effort to get away from the position in which they were held, that constituted pressure. In fact the word "pressure" was indissolubly connected with the idea of motion imparted, or motion prevented; so that in saying that the motion of air was due to a difference of pressure, he endeavoured to "hark back," as it were, to the very foundation of the science, and to take up that branch of the motion of air which rendered it analogous to the motion of electricity and of heat. The motion of heat was due simply to a difference of temperature; the motion of electricity was due to a difference of potential; and in the same way, the motion of air was due simply to a difference of pressure; these terms being in reality analogous. He had pointed out that in a long pipe the

fall of the pressure was uniform. In Fig. 3, p. 81, it was shown that, in a certain way, that was not always the case; but that exception really proved the rule. The diagram rather exaggerated the difference between the real and the calculated pressure. The actual difference was due to the fact that, at the central station at Fenchurch Street the loop and the valves acted as a contraction of the pipe, and so heaped up, as it were, the pressure on one side and diminished the vacuum on the other. On the previous occasion Mr. Bramwell, not satisfied with his own knowledge of mathematics, had brought in the assistance of a senior wrangler, who had evolved a curve not agreeing with his (Mr. Preece's). The fact, however, was undeniable that the pressure on the tube, from beginning to end, fell inversely as the length of the tube, and was represented by a straight line. M. Bontemps had fully confirmed that view, which was also supported by the experiments of Messrs. Culley and Sabine. The reason why his theory had not been received was, that volume had been confounded with mass, and that the law of dry gases had been taken as applicable to damp gases. The gas in the pneumatic tubes was damp air, while the formulæ that had been given were applicable to dry gases. These differences were indications of how little was known of the true theory of the subject, and of how much there remained yet to be done to withdraw the subject from the region of speculation to the solid foundation of scientific truth.

Mr. SAUVEE agreed in thinking that the pneumatic system adopted in Paris was better than the London one. The latter might, more justly, be called the City system, as it had been restricted (except in the Charing Cross line) to the City. Though a considerable saving might have been realised by connecting several of the offices nearest each other, thereby dispensing with double lines for each of those offices, the City system had been worked to the complete satisfaction of the business world, and did great credit to the engineers who designed it.

In speed pneumatic transmission could not of course compete with electricity, except where the number of messages was too great for the number of wires. Telegrams had in that case to wait their turn, and there was considerable delay in their transmission. But if used only for the speedy delivery of letters pneumatic transmission was invaluable. London, of all cities in the world, on account of its immense area and population, was the one where the pneumatic system might be worked to the greatest advantage. A net-work of five or six groups of circular lines, with head offices at Charing Cross or at the central office, might be sufficient. The

problem was how to deliver a letter from one end of the diameter of a circle of 4 miles radius to the other end, within half an hour at the utmost from the time it was posted. Dr. Siemens had, he believed, first proposed to the post-office authorities this scheme a few years ago. Since then, the researches of Messrs. Varley, Latimer Clark, Sabine, and others in England, and of Messrs. Bontemps and Crespin on the Continent, had fully demonstrated the practicability of establishing, for the transmission of letters, pneumatic lines combining rapidity in working with cheapness of first establishment.

According to his view the comparison by Mr. Preece between the Paris and Vienna systems and the City system was not correct either in principle or in fact. The principle of working the lines in Paris and in Vienna was different from that in London. In the former cities the trains collected and distributed the messages as they went their round, while in London the messages were all collected at one point and afterwards sent to their various destinations. This latter method must of course entail a great waste of power. Besides, in Vienna, there was no restriction as to the number of words in each telegram; and in this lay the great advantage of pneumatic transmission over electricity. In Paris it was intended to work the net-work as soon as completed on the principle of allowing for each message as many words as the form would hold.

When the Austrian Government desired to establish a pneumatic system in Vienna their Engineer visited Paris, London, and Berlin, and it was only after long and careful consideration that the circuit system had been adopted. Plate 7, Fig. 1, showed a plan of the Vienna system as it had been laid in 1874. There were in Vienna ten telegraph offices:—1, Central Office; 2, Post Office; 3, Taborstrasse; 4, Landstrasse; 5, Kärnthnerring; 6, Neumanngasse; 7, Gumpendorfer Strasse; 8, Mariahilf; 9, Mariatreu; 10, Stock Exchange. The offices Nos. 1 and 7, each placed about halfway on the main line, were the only two stations at which the machines were erected for working the whole of the system. The line had been laid underground in trenches 3 feet 4 inches deep, so as to be protected from the frost. The pipes were wrought-iron lap-welded tubes $2\frac{9}{16}$ inches interior diameter. The line proceeded from the Central Office No. 1 to No. 2, where it led to an apparatus constructed for the arrival and forwarding of the trains. From No. 2 the line started from an apparatus, similar to the other, and placed by the side of it, and led to the office No. 5, where it was similarly connected as at No. 2. From No. 5 it went to

No. 6, and from there to No. 7, which differed only from the preceding stations in the machinery for working the line, which was similar to that at No. 1; but the apparatus for receiving and for forwarding was the same; from No. 7 the line led to No. 8, then to No. 9, and arrived again at No. 1: it was, therefore, a kind of circular main line, which connected the offices Nos. 1, 2, 5, 6, 7, 8, and 9. In this line the trains always travelled in the same direction at regular intervals. The branch stations 3, 4, and 10, were in communication with the offices Nos. 1 and 2 on the main line, through branch lines. At the office No. 2 there were only pressure and vacuum reservoirs, which were in constant communication with those at the central office through two special lines of pipe.

The traffic was worked by trains at intervals of fifteen minutes, carrying away from each office the whole of the messages accumulated in the interval, and leaving at the same time all the messages addressed to the office which had been posted at the other offices up to the departure of the train, irrespective of their number. This method of working the pneumatic telegraph was entirely different to the one in use in London or Berlin. It could be seen at a glance how much simpler and cheaper the system adopted in Vienna was for laying the lines; the mode of working the traffic by trains at regular intervals was also considerably cheaper, and was very often more speedy than the electric telegraph, especially when there was a large increase in the number of messages within a very short time. The Vienna system, though very much resembling that adopted in Paris, was worked on a different plan in regard to pressure. The working pressure of the air in the reservoirs at the central station, and at No. 7, was 28 lbs.; the pressure in Paris being only $9\frac{1}{2}$ lbs. Two-thirds of the distance between each station was worked with full pressure, and the last third by expansion, the mean working pressure being $22\frac{1}{2}$ lbs. per square inch.

The various speeds obtained in pipes with an initial speed due to a mean pressure of $22\frac{1}{2}$ lbs. to the square inch were shown in Plate 7, Fig. 3, which also recorded the time occupied by a train in travelling through, either by pressure or by vacuum, the full lines representing vacuum. The train starting from the central office at the hour would leave station No. 2 at $H + 1^m$; it would reach Kärnthnerring at $H + 2^m$, leave there at $H + 2\frac{1}{2}^m$, to be at Neumanngasse at $H + 3\frac{1}{2}^m$; and start from there at $H + 4^m$ by the Gumpendorf line, which was 1,739 yards long. This line would have been previously emptied of air by the vacuum pumps

at No. 7, and therefore the train would travel through it at a speed equal to that obtained in a line half that length, and it would arrive at office No. 7 in $1^m$, or at $H + 5^m$, to start from it at $H + 5\frac{1}{2}^m$ for office No. 8, which it would reach at $H + 6\frac{1}{2}^m$; it would leave this station at $H + 7^m$, No. 9 at $H + 9^m$, and would return to the central office at $H + 10^m$; having thus completed the circuit in ten minutes. The train had therefore five minutes to spare before the next journey at $H + 15^m$. During that time the machine at the central office and the reservoirs at the Post Office (No. 2) would have worked the branch lines at offices Nos. 10, 3, and 4, by pressure or vacuum, and the journeys to and fro would have been completed between the passages of two trains on the main circuit. The work was done by a 15-HP. engine at the central station, and by a 9-HP. engine at No. 7, or 24 HP. for the whole system. The engines and pumps were in duplicate at each of these offices. The branch line from No. 1 to No. 10 was worked by vacuum. It was 450 yards long, and the distance was traversed in twelve seconds; about 50 yards a second. The carriers were similar to those used in Paris, viz., iron boxes covered with leather.

Of the schemes for working pneumatic lines between towns, one proposed between Paris and Versailles by M. Crespin, the well-known contractor for the pneumatic lines in Paris and Vienna, had received the approbation of the authorities. The line was double, 18 kilomètres long, and was divided into sixteen sections of 1,125 mètres. At A, B, and C (Plate 7, Fig. 4) were the engines and pumps to work the line; at station A, halfway between Paris and Versailles, were two 50-HP. engines, capable of exhausting in ten minutes 211 cubic mètres, and of compressing at the same time 141 cubic mètres of air at 14 lbs. pressure. This volume of compressed air corresponded to the whole length of 18 kilomètres. The engines at the head stations, B and C, were two of 25 HP. They worked pumps exhausting in ten minutes a length of 4,500 mètres of line, and compressing in the same time 900 cubic mètres of air, at 14 lbs. pressure. At these two stations the forwarding and the receiving apparatus were also erected. The large pressure reservoirs at A, B, and C were connected together by a cast-iron pipe $4\frac{1}{2}$ inches in diameter, which was laid in the same trench as the pneumatic tube. A short link put into communication this pipe and the small pressure reservoirs holding 15 cubic mètres, erected close to the pressure relay at the end of each section. The line was exhausted by the three vacuum reservoirs at A, B, and C, and by two intermediary reservoirs at V and $V_1$, connected with

the large reservoir at A. The total capacity of the vacuum reservoirs was 280 cubic mètres. Each vacuum reservoir worked a vacuum relay. On Fig. 4, sixteen pressure relays and five vacuum relays were represented. To explain the working of the line, and the action of the pressure and vacuum relays, it must be supposed that the carrier or train of carriers was just on the point of starting from C. There was vacuum in front, and pressure at 14 lbs. per square inch was turned on behind; the carrier started under an effective pressure of about 28 lbs., with a mean speed of 40 mètres per second, and it soon reached pressure relay No. 1 (Figs. 5). Passing through it struck against the tappet, whose rod, *t*, knocked off the hook C. This released piston P and the slide-valve T attached to the piston-rod; the fall of piston P put the second link of the line in communication with the small pressure reservoir at No. 1, and the fresh supply of compressed air gave a new impetus to the carrier. The introduction of air at the back of the carrier was regulated by the other piston; the time it took to reach the top of its stroke being fixed, by opening the small cock *r* on the cover. As soon as the carrier had passed through the pressure relay No. 1 this piston began its up-stroke, and attained the top when the carrier arrived at No. 2 relay: the valve S attached to the rod of the piston followed it up and closed the communication between the line and the pressure relay. As the pressure decreased the piston P began to ascend, and the slide-valve T closed the communication between the pressure relay and the small pressure reservoir. At the top of the stroke piston P hooked itself up again, and the relay was ready for the passage of the next carrier.

The carrier passed successively through pressure relays 2, 3, and 4; but just in front of pressure relay No. 4 it passes through vacuum relay No. 1 (Figs. 6). In transit it struck tappet V, which unhooked the valve, and so closed the line behind, as shown in the diagram. The compressed air in the section just traversed by the carrier now escaped through the perforated part, M, of the tube in the relay and the escape valve N. The piston P descended and the sluice S closed, but as soon as the carrier had passed through, the valve was closed, the piston P was raised up, under the influence of the exhaust going on at the top of the cylinder, by means of the small tube *t*, and also under the influence of atmospheric pressure underneath. As it ascended it opened the sluice S, and thereby established communication between the vacuum reservoir and that part of the line on the left which was now filled with air at atmospheric pressure. The escape-valve N kept closed, and as

soon as the air was exhausted once more the piston P fell down and opened the valve. It took eight minutes to exhaust the first vacuum section, and the carrier reached vacuum relay No. 2 in two minutes only; so that when valve D was opened again exhaust had been going on in the second vacuum section during six minutes. To promote the down stroke of the piston P the difference in the degree of exhaust between two successive vacuum sections was regulated at about $\frac{1}{2}$ lb. This was obtained by means of a vacuum regulator (Fig. 7). C C′ represented a short length of cast-iron pipe on the branch pipe connecting the vacuum reservoir with the vacuum relay; a slide-valve, T, was attached to the piston N, atmospheric pressure pushed up the piston and closed the valve; but the weight P kept the piston down as long as the degree of exhaust required had not been obtained.

Fig. 8 showed the apparatus for receiving or forwarding the carrier. When used for forwarding carriers the valve C was closed and the small door P open; the carrier or train of carriers was dropped into the tube, the door P closed, the tube in front of the carrier put into communication with the vacuum reservoirs, and pressure was turned on behind it. To help the carrier to pass the large cock R pressure might be introduced at the back of the carrier in the apparatus itself through the small cock *r*. When the apparatus was used for receiving the carriers at the end of their journey the valve C was open, and the train, on arriving in the receiving-box, P′, knocked, at the farther end of the box, against a suitable buffer, so as to deaden the shock; the valve C was then closed again, the large door opened, and the carrier or train of carriers removed.

Fig. 9 represented a carrier and a piston: they were both made of wrought iron: the carriers were provided at both ends with two collars of anti-friction metal. These rings were slightly rifled outside, like a gun, so as to impart a rotary motion to the carriers. This motion insured equal wearing of the rings, and removed any grit or dust that might impede the travelling of the carrier. The piston was hollow, with openings to catch the dust or rust that might be in the tube. Each train was composed of one piston and six carriers; and the weight of messages was from 12 lbs. to 14 lbs. Forty thousand messages could easily be forwarded in a day with trains every fifteen minutes.

Mr. J. W. Barry said when Mr. Carl Siemens' Paper was read four years ago, he had ventured to express a doubt as to the advisability and economy of transmitting messages by the pneumatic process from the City to Charing Cross, the dis-

tance being $1\frac{3}{4}$ mile, and the time occupied in transmission nine and a half minutes. He thought that to send a message (which had come from the Continent, Scotland, or any other distant place, at the extreme speed of telegraphy) through London at a speed of 20 or 30 miles an hour was not a system that would commend itself. It was now stated, however, that the pneumatic tubes were only to be applied in instances in which the distance between the stations in question and the central station was traversed in five minutes. It was obvious that there must be a point at which the pneumatic system ceased to be economical in point of either time or money; and he should have been glad if some better estimate had been given on that subject. With distances of $1\frac{1}{2}$ mile and $1\frac{3}{4}$ mile, he thought there could not be much economy, even of money, in sending messages by the pneumatic process. He was not alluding to letters, but to telegraphic despatches. The money question had been dealt with rather cursorily in the Paper. It had been said that, on the whole, the pneumatic system in London was more economical than transmission by wire. But the system dealt with in the Paper included both long and short circuits; it would be interesting to know the details of the estimate, and to separate the cost of the long circuit from that of the short circuit. In the case of a message from St. Martin's-le-Grand to Charing Cross, he could not help thinking that the pneumatic process would contrast unfavourably both in time and money with the wire. Mr. Culley had stated that the rate of sending telegraphic messages was one message per wire per minute, at an extreme speed, or one message per wire in two minutes for ordinary working throughout the day. If that was a correct statement it still appeared to him a mistake to throw away the three or four additional minutes required to traverse the five minutes' distance in question. It had been stated in the Paper that the public was very exacting in regard to the Post Office arrangements, but he must demur to that statement. It should be remembered that London was now worse off in regard to price than it was before the Post Office took to the telegraphs. Formerly it had the advantage of a sixpenny tariff; but now no telegram was sent for less than a shilling. He thought the sixpenny rate might be with advantage restored, or else some method adopted of transmitting a special class of expedited letters by the pneumatic process. He threw out these observations because the remarks he had made four years ago appeared, to some extent, to have been borne out by subsequent experience.

Professor UNWIN desired to thank Mr. Sabine for the useful

information conveyed in the Paper. He had recorded a large number of experiments, which must have required industry and perseverance, and the results were, from a scientific point of view, exceedingly valuable. When the discussion on Pneumatic Tubes took place a few years ago, he pointed out that the theory of the motion of carriers in tubes, when worked continuously, resolved itself into this—that the work produced by the expansion of the air in the tube required to be equated to the frictional resistance of the air, and that the other resistances were so small that they might, at least for an approximation, be neglected. He had since learned that Mr. Sabine had anticipated him by twelve months or more in that view of the question, and that as early as 1870 he had published formulæ, based upon that mode of proceeding, which were substantially identical with those given by him on the present occasion. There was, however, this difference between his treatment of the problem and Mr. Sabine's. He stated his belief that the expansion of the air in the tubes would be isothermal, while Mr. Sabine assumed it was adiabatic. Four years ago he had given the same reason for assuming the expansion to be isothermal, which Dr. Siemens recently stated so very clearly. After Mr. Sabine's own experiments, and those of M. Bontemps, he thought there could be no doubt that the expansion of the air in the tubes was nearly isothermal. He did not think it made any great difference in determining the transit time of the carrier, whether it was assumed to be isothermal or adiabatic, but it would make more difference in estimating the power required to propel the carriers through the tube. If the adiabatic law led to simplicity of formulæ, there might be some reason for adopting it; but the reverse was the case, for the formulæ were more simple, assuming the expansion to be isothermal, than in assuming it to be adiabatic. There was another point which he thought of still more importance. The whole of the work, obtained by the expansion of the air in the tube, was expended in overcoming the friction of the air against the tube, with a very small exception. To obtain an estimate of the friction of the air in the tube, it was needful to find an expression for the friction of a short length of the tube in terms of the velocity, and to integrate that expression for the whole length. Mr. Sabine had proceeded in a different way; he had practically assumed that the air had, at every point of the tube, exactly the same velocity, namely, the mean velocity for the whole length. If there were no great difference of velocity at different parts of the tube, this proceeding would lead to a

sensibly correct result; but he found on examination—and it was confirmed by what Mr. Carl Siemens had said in his Paper—that there was a great difference in the velocity. The carrier started at a comparatively low speed, and attained the highest velocity at that part of the tube where the pressure was least (Fig. 30), and in that case it was not accurate to assume that a constant mean velocity might be substituted for the integration of velocity through the length of the tube. It had appeared to him that it would be worth while to try the result, first, of

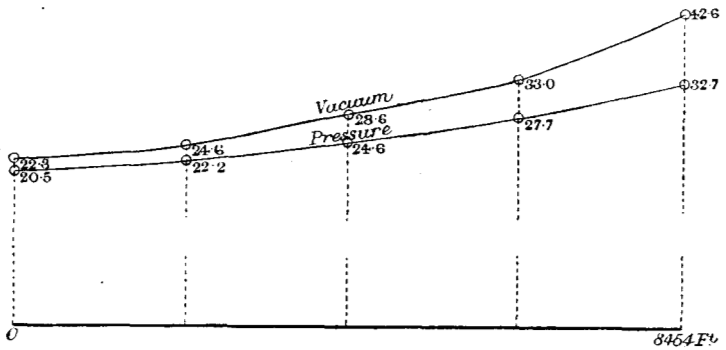



Fig. 30.—Curves of Velocity in feet per second.[1]

assuming that the law of expansion was isothermal; and, secondly, of attempting the integration of the friction corresponding to the different velocities in different parts of the tube, and he had obtained, in that way, some expressions which he should be happy to place at the service of the Institution. Mr. Sabine had tested his formulæ only in one way. He had ascertained from the experiments the values of the constants in his formulæ, and then using those constants he had deduced the transit time for the whole length of tube. The agreement in this respect between the formulæ and experiment was certainly very good. Professor Unwin had this and two other tests of his formulæ. In the first place, he found that they agreed as well as Mr. Sabine's with the experiments in giving the transit time for the whole length of the tube; and in the next place, he had been able, by their aid, to draw curves of pressure for the whole length of the tube. The diagram (Fig. 31) showed the curves of pressure which he had obtained for the two experiments narrated in the Paper. It would

[1] These curves are for the same data as those given by Mr. Sabine, p. 80, and may be compared with Fig. 3.

be seen that at every point the theoretical pressure in the tube rose above the straight line between the two terminal points, and that agreed entirely with Mr. Sabine's results. Mr. Preece had previously stated that the fall of pressure followed such a law, that it could be represented by a straight line, and he appealed to the diagram in proof of that statement. No doubt if air were incompressible, the fall of pressure would occur in a straight line; but, as it was not, it was impossible that it should do so, and it was rather extraordinary that a diagram exhibited by Mr. Sabine to prove that the fall was not in a straight line, should be appealed to as a confirmation of the

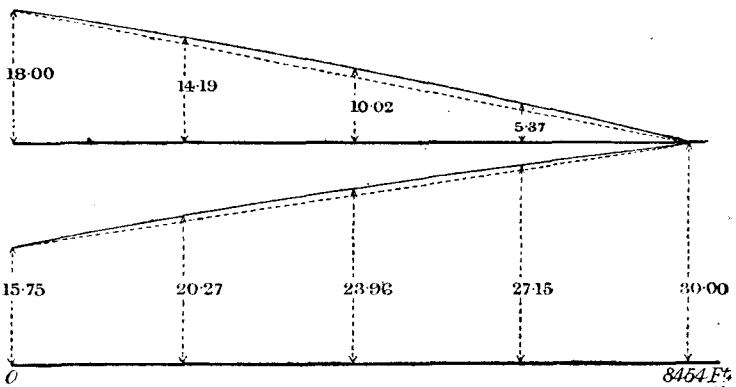



Fig. 31.—Curves of Pressure in inches of Mercury.[1]

contrary assumption. In the third place, he had obtained curves of velocity in different parts of the tube, which showed precisely, as Dr. Siemens had formerly stated, that the velocity increased very much towards the terminal end of the tube. Mr. Sabine would, he thought, see that in another point those results were beyond what he had obtained. He did not think that Mr. Sabine's formulæ would give the time of transit over an intermediate portion of the tube. He next came to a point of even greater importance. An estimate had been given of the work expended in driving the carriers through the tubes—first the work expended when the tube was under pressure, and according to a perfectly correct method; but in the examination into the work expended when the tube was under vacuum, a very different method had been adopted. It was stated that the working of the pump

[1] These curves are for the same data as those given by Mr. Sabine, p. 80, and may be compared with Fig. 3.

in the vacuum tube was much more simple than when the carrier was driven by pressure. He was entirely unable to see any difference between taking air out of the atmosphere at 15 lbs. pressure, compressing it to 30 and then forcing it into a tube, and taking the air out of the tube, say at 9 lbs. pressure, compressing it, and then forcing it into the atmosphere. The two ways of dealing with the air were identical, and the expression which gave the work required in one case, would be identical with the expression for the work required in the other case. Mr. Sabine, however, obtained for the vacuum tube a totally different expression from that which he used for the case of the pressure tube. He thought that the error had arisen from finding the work done in one stroke of the vacuum pump and neglecting the return stroke. He had calculated rightly enough the work done by the piston of the vacuum pump while it was drawing air from the pneumatic tube; but he had apparently forgotten that, in the return stroke of the pump, the atmosphere did work upon the piston above what was necessary to force the air in the cylinder into the atmosphere, and that that surplus work, in any properly-constructed pump, was stored up. The result of what appeared to him to be a very considerable miscalculation was, that Mr. Sabine had very much overestimated the work expended in the case of a vacuum pump. He arrived at the conclusion that it was equally economical to work the pump by pressure and by vacuum. If, as Professor Unwin believed, he had overstated the work of the vacuum pump, that conclusion was erroneous, and it was more economical to work the tubes by vacuum than by pressure. It might be remembered that Mr. Bramwell suggested that it would be economical to employ a lighter gas than atmospheric air—that much less work would be required if the tubes were filled with hydrogen than would be required if they were filled with ordinary air. The same principle would show that, when the tubes were filled with air of less density than the common air, they would work more economically than with air which, on an average, was of greater density than the common air. In London, of course, the prime necessity was speed of transit, the power consumed being a matter of secondary importance; but if the pneumatic tubes came to be used, not merely for sending messages to short distances in very small tubes, but on a larger scale, it would become a question whether, if hydrogen could not be used, at least air of less density than the common air might be. He had been too much occupied to make any exact calculation, and he only threw out the sug-

gestion that an improvement might be effected in the working of a tube, like that to Charing Cross, in the way he had mentioned. As originally constructed, it was a single tube which went to Charing Cross and back again. At the end of it there was a pump which took the air out at one end of the tube and pumped it into the other end. If, at starting, the whole of the tube contained air of ordinary atmospheric pressure, the effect of starting the pump was that the pressure at one end of the tube became greater than atmospheric pressure, while that at the other end became less than atmospheric pressure. But if, before starting the pump, the whole of the air in the circuit had been reduced to a tension of one-half, or one-quarter, of atmospheric tension, the work of the whole of the tubes would be done with a very much less weight of air; and in that case it would be found that there was an economy of power. He did not know that there would be any sufficient gain, because a somewhat more complicated apparatus would be required; but, so far as concerned economy of power, a saving would be effected. Of course it might be replied that, if, initially, some of the air in the tubes were abstracted, the leakage at the points where the messages were introduced would soon bring the whole of the air in the tubes up to the density of ordinary atmospheric air. That, no doubt, would be so if no provision were made to prevent it. If tubes were to be worked with air of less than atmospheric density, in addition to the ordinary pump, which took the air from one end of the tube and drove it into the other, a special pump would be needed, whose function would be to maintain the proper density of the air in the tubes; but he did not think that that presented any particular difficulty.[1]

Dr. SIEMENS desired to congratulate the Institution upon the very lucid explanation and scientific exposé of Professor Unwin, with every word of which he agreed. He had already discussed Mr. Sabine's Paper, but now proposed to offer a few remarks on the theoretical principle involved in M. Bontemps' communication. An interesting account had been given of experiments to determine the velocity of carriers in pneumatic tubes by electrical markers, with records of the observations on a chronograph. The results thus obtained must, he thought, be accepted as indisputable; but he was inclined to doubt some of the generalisations attempted in the Paper. It was perfectly true that when two carriers followed one another in a tube worked by

[1] *Vide* p. 263, *et seq.*

a continuous current, the time occupied by each carrier in traversing the same section of the tube from one marker to another must be the same, because the current flowing through the tube was always the same; but it did not follow that the absolute speed, the number of feet traversed per second, should be the same in each portion of the tube. M. Bontemps appeared to have found that that was substantially the case—that after a short period of acceleration, the speed of the carrier fell into a uniform rate until almost the very end of the journey, when it again increased, and he stated that these results seemed to verify Fournier's theorem, according to which "equal impulses given throughout the journey of an accelerated body must produce the same velocity." These results did not coincide with the common-sense view of an elastic fluid expanding behind a light working piston, but he thought that an explanation of the experimental results was nevertheless possible. The air, say of 2 atmospheres pressure at one end expanded down gradually to atmospheric pressure, and the same index of air between the two carriers must elongate as the carriers went along; and expansion must take place throughout the course because working power was required at every point. But in taking the case of a carrier not fitting the tube entirely, and yet causing some friction against the sides, he should expect the results which were stated in the Paper. In that case the impulse given to the carrier in the tube would be carried by the rush of air past it, and this would be the same throughout, and there would practically be the same power active to overcome friction at every step of the course. The result would be a uniform speed for the chief part of the course, till the very end, when the rush of air past the piston would greatly increase. It was to be hoped that the Author would continue his observations with the appliances he had made in order to obtain further information on the interesting subject of gaseous friction in long tubes. An explanation had been attempted by Mr. Preece, of the apparent sluggishness of the air to expand throughout its course, by the fact that the medium was not pure air, but air mixed with vapour of water, which mixture would follow another law of expansion than that of either fluid taken separately. He dissented entirely from that view of the case. He had shown, and Professor Unwin had quite confirmed that view, that the air expanded isothermally—that both air and vapour would pass through a tube without altering in temperature; therefore no condensation of the vapour would take place; and as vapour and air both followed the law of Mariotte in precisely the same manner,

there could be no difference whether dry air was used or air containing a slight proportion of vapour.

In advocating the use of the radial system in preference to the continuous or circuit system, Mr. Preece said that he had travelled over the continent of Europe with a view of ascertaining the working of those systems elsewhere; and that, while he found the radial system established in Brussels, he ascertained that at Berlin the circuit system, which had been adopted in 1863, had failed. This was startling news to him; because, although he had never described the system as established at Berlin, he had referred to it, and his brother also had referred to it, in his Paper, as an historical step towards the accomplishment of the circuit or continuous system as established by them in London. He accordingly wrote to Berlin for information, and he had ascertained that, so far from the system having failed there, it had been during the last twelve years in uninterrupted operation, and that the only thing that could be construed into a partial failure was the circumstance that after the one circuit from the telegraph office to the Bourse had been established, a second circuit from the telegraph office to the Brandenburg Thor was added, and it had been found that the boiler power was not sufficient to work both systems continuously together. For a time, therefore, and probably at the very time when Mr. Preece paid his visit to Berlin, the one system was shut off when the other was worked between the telegraph station and the Exchange during the busy part of the day. With that exception, which he understood had since been set right by the addition of boiler power, the system had been working precisely in the same manner as it had been established twelve years ago, and it had given no cause of complaint nor inconvenience in the working. Mr. Preece further stated that the cost of the iron pipes, in connection with the circuit system as established in London, was at any rate higher than the cost of the system of tubes advocated by the Engineers at the Post Office, and that his (Dr. Siemens') firm charged for the iron pipe at the rate of 15*s.* per yard, whereas another contractor had laid lead pipes at a rate of 13*s.* 8*d.* He would not dispute those figures, but Mr. Preece had fallen into the error of making, no doubt unintentionally, a very unfair comparison. In the first place, he compared a 3-inch tube with a tube of much less diameter; he was not quite certain whether it was a $1\frac{1}{2}$-inch or a $2\frac{1}{4}$-inch tube that he referred to as having been laid for 13*s.* 8*d.* He also compared a mere tube which had been laid in connection with an established apparatus, with the system of tubes

and instruments, carriers and other matters, required to constitute a complete circuit system. In the one case the instruments, carriers, and station fittings were not included in the estimate, and in the other they were included. There were also to be added in the case of the circuit system the engineering and general expenses which fell upon his firm in designing, making, and laying down the new system in London. He was employed as Engineer of the Post Office in designing not only the tube, but also the engines, boilers, reservoirs, and pumping machinery to work the system, and the contracts were let to three firms:—Messrs. Easton and Amos, who made the engines and pumping apparatus; Messrs. Aird, who laid the tubes and completed the earthworks; and Messrs. Siemens Brothers, who made the other mechanical arrangements. It should also be stated that as the system had been matured by his firm at great expense, and patented, they had a perfect right to superadd to their cost a reasonable amount for patent right. Including all the charges the Post Office paid for the first circuit the sum of £5,212, which was at the rate of 15*s.* per yard; but of this sum £2,900 were paid for the tube and the earthwork, including Mr. Aird's profit on the latter, all the rest being taken up by other work. Thus the figures for comparison were 8*s.* 4*d.* per yard for a 3-inch iron pipe, as against 13*s.* 8*d.* per yard for a lead tube of about half that area, which figures fully justified, he thought, his former argument. Mr. Preece likewise stated, that although the continuous or circuit system of working might be suited for such places as Paris, Vienna, and Berlin, it would never do for London, where speed was a principal object. He should be very sorry to have put forward for London a system that was not capable of the greatest development of speed, knowing as he did the value of time. But Mr. Preece, in describing the advantages of the radial system, seemed to forget that the two principal distances worked by the Post Office at the present time were worked on the continuous system, in exact accordance with the principles laid down by himself. All that had been done in the first circuit laid down by him was to take out about 3 yards of pipe at the neutral point at Charing Cross. One branch was worked by pressure, the other by a corresponding vacuum, and at the extreme point the pressure was neutral, so that the connecting link between the two sides might be taken out with impunity without altering the system in the least. The only difference would be that instead of bringing the same air back to Telegraph Street or to the General Post Office, there would be air which had travelled through the instrument room at Charing Cross and which had taken up a good

deal of vapour from the numerous persons engaged there, giving rise, probably, in a measure to the inconvenience of rust in the iron tubes; an inconvenience which had not made itself felt in Paris, Vienna, or Berlin, where iron tubes were used. He thought that with proper care that might be completely prevented in London. He admitted that it would have been expedient—indeed, he proposed it at the time—to have the inside of the iron tubes tinned, which would have given all the advantages of the lead tube coupled with the comparative cheapness of iron tubes. Mr. Preece seemed to imply that a circuit system of iron tubes was a roundabout system by which, in order to get from Charing Cross to Telegraph Street, it would be necessary to go round by Islington. That was not the case, nor had he proposed any such thing in laying down the first circuit between those places. The continuous system, if worked in circuits, could be so arranged that the distances between the two principal points on the circuit would be minimum distances, even though the intermediate stations might be a considerable distance apart. If a tube were established on the circuit system between Great George Street and the City, one branch might pass by the Strand, or the Embankment, and the other over the bridges through Southwark: both would be equally near, and the intermediate stations upon the two branches would be a considerable distance from each other, and be thus accommodated by pneumatic communication without increasing the time of transit between the principal stations, and without involving an extra consumption of air or power. On the whole, he thought that the radial system was well adapted for very short distances, and for very light carriers. If the object was to collect telegraphic messages from the streets immediately adjoining St. Martin's-le-Grand, it would be absurd to speak of establishing a circuit system, and Messrs. Clark and Varley had established that communication in a very efficient way. But whenever it was desired to carry pneumatic communication beyond those limits, to extend it over considerable spaces, so that not only a few offices in the City, but the whole of the metropolis might derive benefit from it, it would be absolutely necessary to resort to some such system as he had advocated.

Mr. BRAMWELL remarked that he wished to say a few words upon the question of the laws which govern the motion of elastic fluids, such as air, through pipes. He thought it might be taken that for the purpose of transmitting telegraph messages through pipes by the pressure of air, or by the exhaustion of air, the power required for the friction of the pistons, and even for setting the air

in motion, was so little that the resistance might be regarded as practically a simple question of skin resistance, or friction of the air against the interior of the pipes. If any corroboration of that view were wanted, it would be found in this Paper, where there was a statement of the relative speeds of transit through a 3-inch tube and through a $1\frac{1}{2}$-inch tube under equal pressures. Through the 3-inch tube the time occupied was one hundred seconds, and through the $1\frac{1}{2}$-inch tube, one hundred and forty-one seconds. Taking equal pressures upon the tubes, the proportionate velocities ought (if the resistance was a skin resistance only) to be as the square roots of the diameters of the pipes. The speed in the small tube was $\frac{10}{14}$ of that in the large tube; and if that were squared, it would give $\frac{100}{196}$; or, in round numbers, one to two.

There was a statement in the Paper upon which certain propositions had been founded, which statement and propositions he feared were likely to mislead. It was said that the power required to work a tube of 3-inches diameter would be 100, while the power required for a tube of $1\frac{1}{2}$-inch diameter would be only 18. He thought it would be found that that was not so; for although these figures might truly represent the proportion between the horse-power of the two engines required under the two different circumstances, nevertheless, as looking at the respective velocities in the $1\frac{1}{2}$-inch and 3-inch tubes already stated, the smaller sized engine would have to work to transmit a message for a given distance during a time represented by 141, while the larger power to transmit a message to the same distance would only have to work during a time equal to 100; the comparative final results in power expended would be as $100 \times 100 = 10{,}000$ for the 3-inch tube, to $18 \times 141 = 2{,}538$, or in the proportion of 100 to 25, as might have been expected. Therefore any calculation based upon the saving of power in the proportion of 100 to 18 was erroneous. It should be in the proportion of 100 to 25. Another statement, by its incompleteness, was also misleading: viz., that the water was used in the air-pump for lubrication. No doubt it did lubricate; but it was also used for a different and for a much more beneficial purpose—that of diminishing the extra resistance caused by the heat given forth in the pump before the opening of the discharge-valve, which heat enlarged the volume of the air, and (if not removed) would involve the expenditure of extra power to discharge the augmented bulk; but if that heat were put into water, the air was reduced to its true bulk, and the demand for power was diminished. He had been apprenticed to a member of the Institution named Hague, who, nearly half a century ago, was the inventor of a mode

of transmitting power to distances of $\frac{1}{2}$ mile and $\frac{3}{4}$ mile, by the exhaustion of air. At the end of the exhausting pipe he put an engine, something like a steam-engine. Atmospheric air drove the engine, and thus did the required work at a distance from the motive power. The pumps used were exhausting, not compressing pumps, but the air, which on expanding after passing through the engine became cooled, was brought back again to the atmospheric temperature in its passage (in an attenuated condition) along the pipes, and then, on being compressed in the pump to atmospheric density so as to be capable of opening the discharge-valve, gave forth the heat in a sensible form which it had absorbed while in transit, and thus augmented the volume of the air, and added to the power required to work the pump. He believed it was himself who first suggested that it would be desirable to put into those pumps (for the purpose of getting rid of the heat) a small injection of water. That was done forty years ago. They did not use many indicators in those days; but this rough result was obtained, that whereas the steam-engine working the air-pump would only make twenty-seven revolutions when working without the injection, it would make thirty revolutions with it. Looking at the utility of injection as a means of saving power, it was a pity that a statement should be made, leading to the inference that the only object of the water in the pump was for lubrication. It had been suggested, when a similar subject was under discussion four years ago, that the fall of pressure in a pipe through which there was a flow of air, for such purposes as were then under consideration, might, in a diagram, be represented by a straight line. Mr. Bramwell ventured at that time to say that, though he had not sufficient mathematical power to determine what the line should be, he felt certain it could not be a straight one, and he gave his reason for that opinion. The speaker alluded to, however, now reiterated his queries, and had the hardihood, in order to prove his straight-line theory, to refer to a diagram put forward by the Authors of the Paper, which diagram showed conclusively that the fall of pressure was not represented by a straight line, but by a curve. The reason that he gave was rather astonishing. It would have been, he said, a straight line except for certain circumstances, which were as follows: "The difference was due to the fact that at the central station at Fenchurch Street the loop and the valves acted as a contraction of the pipe, and so heaped up, as it were, the pressure on one side, and diminished the vacuum on the other." But what did the Authors of the Paper say? That there was not any obstruction at the turning point, but that they had put in at

Fenchurch Street a carefully-prepared curve-piece. Moreover, if there had been obstruction, he did not agree that the effect of it would have been to heap up pressure. Those who had heard Mr. Froude's address to Section G of the British Association, at Bristol, would remember that he not only stated, but proved conclusively by experiment, that the place where there was the least pressure in a pipe through which water was flowing, was where the pipe was smallest. He had ventured to exhibit a diagram (Fig. 32), showing, as he thought, the true curve of pressure, and he was glad to see that the curve drawn by Professor Unwin justified that diagram. He was speaking, as on a former occasion, not of any intermittent motion of air, but of uninterrupted motion, and of the fall of pressure that would take place in tubes through which there was a continuous current, as in the case of the circuit system representing that fall. Under these circumstances it appeared

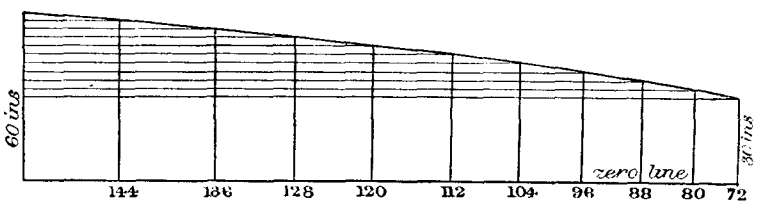



Fig. 32.

to him that the curve must be a parabola. The resistance, he believed, might be taken to be practically a skin resistance, which varied as the square of the speed, and inversely as the density. If, therefore, in any portion of the pipe, there was a density of 1, and a speed of 1 in a given time, there would be a resistance of 1; but if, in any other part of the pipe, the air had so expanded that there was only a density of $\frac{1}{2}$, there must be a speed of 2. But a speed of 2, with equal density, would give a resistance of 4; but being only half the density, it would give a resistance of 2; therefore the resistances increased directly with the diminution of pressure. That condition of things, he thought, could only be satisfied by a parabolic curve. He had found difficulty in determining what would be the fall in pressure at equal distances along the pipe; but he soon found that a very simple calculation sufficed to ascertain what would be the varied distances from pressure-gauge to pressure-gauge which would give uniform falls of pressure, and this mode of treatment showed that the curve was a parabola. The diagram he had placed upon the wall showed a supposititious pipe 1,080 yards

long, with, at one end, a pressure of 60 inches above zero, and at the other a pressure of 30 inches above zero; and if pressure-guages were inserted at distances increasing in an arithmetical ratio from the one end to the other, there would be a uniform drop of pressure at each gauge. Upon these principles the curve would be a parabola, and that would fulfil the conditions he had given. Another necessary condition was this: if the total difference between the pressure at one end and the pressure at the other above zero were ascertained, then the two terminal distances should bear the same relation to each other as the two terminal pressures. It would be seen by the diagram that, with 60 inches pressure at one end of the pipe, and 30 at the other, the two pressure-gauges, the first and the last, were 72 feet, and 144 feet from their respective ends of the pipes. He believed that a parabolic curve having its vertex on the zero line would fulfil every condition arising under a steady flow, but would not do so where there was an intermittent flow. Where there was such a flow and a sort of popgun action, the circumstances were very complex and difficult to investigate. For instance, M. Bontemps, in his Paper, had pointed out that, on the opening of the cock, the pressure-gauge fell, and afterwards rose a little, but never to its full extent. One could well understand why that should be. The air issued out of the vessel S under great pressure, and there being only atmospheric pressure to resist it, at first came out with great velocity, but it speedily found further resistances, viz., the inertia, and the friction of the atmospheric air in the tube, which resistances, as it were, heaped the air up. The air was elastic, and after the first shock it recoiled. This, he thought, might be the solution of the fact observed by M. Bontemps, that there was a period during which the speed was less than either at the beginning or at the end of the motion. To show the difficulties experienced in dealing with the question of the intermittent flow of elastic fluids, he (Mr. Bramwell) might be permitted again to direct attention to an experiment made by him, some years ago, on the South-Western railway, when trying Le Chatelier's Contre-vapeur system, which showed that with an aeriform fluid, there might be the same ram-action as with a water-ram; and that it was possible to get a higher pressure from the injection of a stream of elastic fluid under pressure into a space than the pressure in the vessel from which that fluid proceeded. In the instance in question a pressure had been obtained in the cylinders of a locomotive of 160 lbs. on the inch (and it might have been higher, but the indicator would not register it), while the pressure in the boiler was only 140 lbs. Many in-

stances had come under his notice where the steam was reversed, as in the case of the colliery winding engines, in order to bring them to rest when the cage was approaching the top, and where the indicator diagrams showed a similar effect of a pressure in the cylinder above that in the boiler from which the steam proceeded.

He would now call attention to an error into which M. Bontemps appeared to have fallen. He understood him to say that because when piston No. 2 was put into the pipe six seconds after No. 1 the pistons traversed past the indicating point at intervals of six seconds, this uniformity of time in passing proved that the air contained between those pistons did not alter its volume, and thus (M. Bontemps argued) it was shown that there was no expansion, and that the air behaved like a non-elastic fluid. He, however, thought the conclusion drawn by M. Bontemps was entirely erroneous, as he would endeavour to show by an illustration. If he dropped off the edge of the table six marbles, with a second interval between each, they would arrive at the ground with a second interval between each marble, but they would go at much greater speed when they did arrive than when they started; and if the distances between any two of those marbles were taken any part of the way down, it would be found that they were much farther apart near the ground than they were just after leaving the table. Therefore the fact that the pistons passed given points at equal intervals of time, whether those points were near the beginning of the journey or near the ending, was entirely consistent, and was just what should happen with the expansion of air between the two pistons.

He was glad to hear Professor Unwin make the suggestion, that in lieu of using pipes charged with hydrogen, it would be better to have them worked below the full pressure of the atmosphere. It was clear to his mind that as resistance was due to the velocity and to the density, it was a most desirable thing that the density should be as low as possible; and probably a ready mode of reducing resistance would be to keep the stream at half atmospheric pressure at one end, and at a quarter at the other, rather than to keep it at full atmospheric pressure at one end and at half at the other, because the reduction in density would so diminish the power required to draw the air through, that there would not be needed anything like the difference of pressure at the lower density that would be needed at the higher. Probably there would be in-draughts of air, and leakages; but he doubted whether the power required for a constant expulsion of air from

the pipes to maintain a general vacuous condition would not be below that which would be saved by having the tubes in that partially vacuous condition throughout. The circuit system of Dr. Siemens was peculiarly adapted to such a system, and also to the use of a lighter gas—such as hydrogen. He was glad that the haze which had been thrown upon the question, by those who took part in the discussion previously, had been cleared up by Dr. Siemens. Many must have believed that the circuit system of Dr. Siemens was such a one as that used in Paris, where a train of carriers was made up and despatched once in a quarter of an hour, the train being stopped at the different stations, and despatched again when the carrier for that particular station had been taken out. Those who remembered the Paper of Mr. Carl Siemens would know that no such clumsy expedient was contemplated. The very essence of the system was that there should be an air rope, as it were, always running through the pipes, and always running in one direction, that the carrier could be "hooked" on to that air rope whenever it was necessary, and that it could be "unhooked" at any station. Therefore to compare the time occupied in the circuit system adopted at Paris, with the time occupied with the system explained to the members of the Institution four years ago, was misleading. He believed it was not understood, until it had been explained that evening, that although there was in the case of the pipes from the Central Station to Charing Cross and back a break of a few yards at a point where there was uniformity of pressure within and without the pipe, nevertheless there was a constant current going in one direction, and a constant current coming back in the other; so that, in truth, the essence of Dr. Siemens' system was adopted. The only thing wanting was the connecting link at the place where there was no strain—no pressure either outwards or inwards.

Mr. COWPER observed that he was much pleased to hear the remarks of Professor Unwin with regard to the expansion of the air, showing how the distance between the carriers increased as they came out at the end of the pipe. It would be a great pity if the idea should prevail that the expansion of air in tubes was not understood. There might be slight differences as to the precise formulæ for calculating the exact speed or resistance; but that the air did expand, and issue much faster than it entered, was so clear, that he could not comprehend how M. Bontemps could reiterate the statement that two despatches kept their distance throughout the whole length, or speak of their attaining a uniform velocity, and keeping it up to the end. He himself said in another part of

his Paper: "The conclusion from these results is, that the slight augmentation of speed towards the end of the journey is due," &c., admitting that there was a slight augmentation; yet the whole Paper was based upon the argument that the carrier went at a uniform speed. Mr. Sabine had given a curve of true expansion, or true compression, and he stated that the same curve would represent nearly the compression or the expansion of the air. That again merely showed that the air did expand, and go faster at the exit than at the entrance. How any one could regard it as a proof of uniform velocity he could not imagine. As far back as 1855 he was called upon by Sir Rowland Hill, jointly with Mr. Gregory, to see what could be done to accelerate the mails. The matter was discussed, and it was hoped that 100 miles an hour could be accomplished. He had arranged for tubes with carriers packed with leather that would go 90 miles an hour, but it would have been at an extreme cost. It would have required an 18-inch tube, filled with a light moving power, viz., a partial vacuum. He agreed with what had been said as to the advantage of a light propelling medium. Mr. Gregory found that a much higher velocity could be attained with air in a more rarefied state than the ordinary atmosphere. Dr. Siemens, in one of his earliest descriptions of his invention before 1866, referred to hydrogen, or any other light gas, as being useful in the pipe; and he believed it was mentioned in his patent. The great friction of air in the tube was no doubt that which had to be overcome. It was not chiefly the piston, even if it weighed, as in Paris, 19 ounces; and with a light piston, the power required to drive it was as nothing. If greater velocity was needed, the tubes must be enlarged, and the air rarefied. He might mention an experiment made by himself, through the kindness of Mr. Latimer Clark, at Lothbury. The experiment was first tried with a tube as ordinarily used, with vacuum reservoirs and pumps at one end, and open at the other. When the despatch was put in, the reservoir was opened to the pipe, and exhaustion commenced. When this had gone on a little while, the piston soon started, and went through the tube. The utmost speed that could be obtained was 36 miles an hour. The pipe was then partially exhausted. There being no valve to the pipe, he put the piston in first, and plugged the end, exhausted the pipe, and pulled out the plug suddenly. In that way a speed of 51¼ miles an hour had been obtained with only a 1½-inch tube. The length of the pipe was ⅓ mile. That showed that any light medium, such as hydrogen or rarefied air, was more effective for propulsion than ordinary

air. The speed of 90 miles an hour, with large tubes and long lengths, could only be obtained by vacuum, not by pressure. Relays of reservoirs would be required every ½ mile, and engines every 4 miles. Sir Rowland Hill thought that the expense would be far too great, either for London or for Crewe and Holyhead. He accordingly advised him to accelerate the mail very considerably, and hence the "Wild Irishman." He then suggested an extra charge for express despatches in London, but Sir Rowland Hill was so fond of the penny post that he would not think of a 4*d.* or 6*d.* tariff. With ten pipes, or a total of about 39 miles in length, the whole of London could be fairly served, so that no part would be more than ½ mile from one of the stations. All the populous part of London, with about eighty-two despatch offices, would be well served, and there would be a more speedy delivery of letters than at the present time. There had been a good deal of discussion about half-hour deliveries, but that would give only a fractional saving over the two and a half or three hours during which letters now remained in the hands of the Post Office. If, however, tubular lines were laid and an extra price charged, the letters might be delivered by boys as soon as they were received. There was one point in the tables to which he took exception. He thought it was unwise to discard the reservoir for cooling the air. He would put a cooler, if cold water was accessible, for the condensing engine, and would first make it do duty by cooling the air below dew-point, causing it to deposit as much moisture as possible. He believed it was an error to take so narrow a view of the transit of despatches through the tubes, as though the question related only to the sending of one despatch, it being of no consequence whether there was a continuous supply of air, or a rope, as Mr. Bramwell called it, so as to take a large number of despatches. It was important to make the pipes do as much duty as possible, as they were expensive implements. One pipe was quite enough for one street, and it should take as many despatches as possible. If a pipe could be made to go one way and to come back another, serving two districts instead of only one, and at good speed, a great advantage would be gained. A despatch from a distant station might not reach its destination by the shortest way; but it would always go quickly, and the pipe would do much more duty than if it went straight from one point to another. He thought, therefore, that the advantages of the continuous over the radial system were very great. The Authors of the Paper had stated: "Where despatch is not of paramount importance, the circular system has advantages, by giving com-

munication between each station on the route." That was really required; and if the public only knew how it could be done, and what profit would be yielded by it, they would be a little more exacting in their demands than they were at present. As it was, only a few merchants in the City got the benefit of the radial system. The footpath in Cheapside was at present full of pipes, and some pipes were obliged to be taken through Gresham Street. There were twenty-four pipes, and a length of only 17 miles and a very few centres. With ten circuits eighty-two stations might be supplied, and the whole of London well served. He hoped to see further extensions of the Tubular Despatch system, believing it would be for the benefit of London, and of the Post Office exchequer.

Mr. IMRAY observed that he considered M. Bontemps had conceived erroneous views of the results of his experiments. He had given it to be understood that air behaved differently from any other elastic fluid—indeed, almost as an inelastic fluid; and he attempted to account for it by the presence of vapour, and otherwise. But it would be seen that air in the tube behaved precisely as an elastic fluid ought to behave. He would direct attention to a diagram (Fig. 33), showing the velocities according to M. Bontemps'

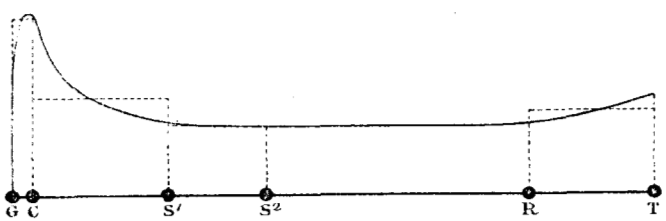
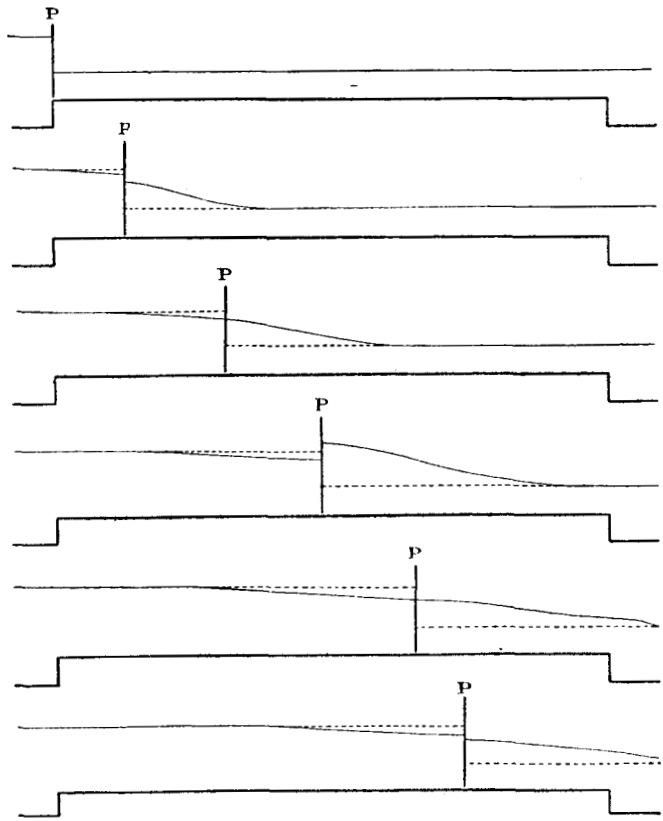



Fig. 33.

experiments. He had marked on the base line the different stations, drawn to scale, at which M. Bontemps' instruments were placed. The velocities of the pistons were also drawn to scale (vertical), and the effect was that in a short distance between the first stations the velocity after rising to a high point—91 feet—rapidly dropped through the middle part of the pipe, going on nearly uniformly, and then increasing at the end. It was all very well to disown the results that M. Bontemps attempted to derive from his experiments; but a reason ought to be given. He had therefore, in another diagram (Figs. 34), attempted to show graphically how air should behave in such a pipe. He had taken water, so as to show by its waves the pressure of the air, and

assumed it to be inclosed in a trough, with a large cistern at each end, one being considerably higher than the other. The piston P, which was supposed to be guided so as to move vertically, was subjected to the pressure of the high cistern, and had on its other side a low pressure. It was immediately sent forward; the wave gathered in front from the displacing of the water in front, while




Figs. 34.

the water behind dropped a little into a hollow, or a negative wave. This went on accelerating the piston until it came to the condition, where the wave piled up in front of the piston was exactly the height of the wave behind it. There was then no further acceleration, but the whole of the water having got a momentum forward, the pressure behind kept on falling a little,

while the pressure in front kept on rising. Then the piston was retarded for some time, until again the wave in front and the wave behind came to a state of uniform motion which continued as long as that state of things lasted. Gradually, as it got to the end of the pipe, the wave had freedom to flow into the larger reservoir, and there was an acceleration, as shown in the diagram of velocities. Thus converting the height of a wave, which was a mere graphic representation, into the pressure of an elastic fluid, there were obtained, as it appeared to him, the various phases of pressure which the air underwent in the pipe, and the various velocities produced in the piston through those variations of pressure, and they appeared exactly to accord with the diagram which resulted from M. Bontemps' experiments. There was another point in M. Bontemps' Paper, about the successive pistons following each other at equal distances, to which he desired to refer. On Fig. 35

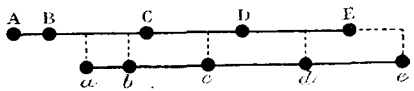



Fig. 35.

he had marked a body to be moved forward with varying velocities through a certain distance, marking the varying velocities by varying lengths of base line; and underneath he had drawn another line in which the same variations of velocity occurred, but at a later period. The distances were all identical. Although the velocities varied at every point, yet as the phases of variation which each body underwent were the same, the distances remained the same, which accorded with M. Bontemps' results. Fig. 36

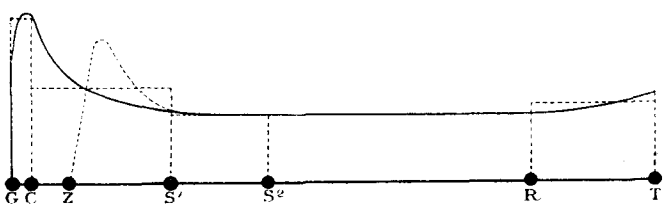



Fig. 36.

indicated exactly what M. Bontemps' experiments showed as to the motion of the two pistons. That of the first piston was represented by a dotted curve running from Z, and then going to the same line as the later piston, shown by the curve beginning at the central station S. Thus, he thought it might be satisfactorily

proved that the air, in the pipe which M. Bontemps had experimented with, behaved precisely as elastic air ought to behave, subject to Mariotte's law, the pressure being proportionate to the density; and there was no reason to suppose that the presence of aqueous vapour, or any other cause, affected the results in such a way as to make the air behave differently.

Mr. H. EATON said that, as the greater part of the pneumatic tubes in London had been laid under his superintendence, he might give a few facts in connection with the practical working of the system. The extension of the system, on the removal of the central station from Telegraph Street to St. Martin's-le-Grand, involved an additional length of $13\frac{3}{4}$ miles of new tubes. Of this length more than 1 mile had been laid with 3-inch lead in 4-inch cast-iron pipe, in order to utilise the 3-inch wrought-iron tube laid by Messrs. Siemens. The remainder was $2\frac{1}{4}$-inch lead in 3-inch iron. The 3-inch lead in 4-inch iron cost about 2*s.* 6*d.* a yard more than the other; but it was laid in 1873, when iron was very dear. The advantage of lead over iron was, he thought, apparent. The lead was protected by cast-iron pipes, which were not so subject to mechanical damage as wrought-iron ones not so protected. The streets of London were often taken up, and he had known several cases where a wrought-iron tube, unprotected by cast iron, had been pierced by a pickaxe. It was a common practice amongst the workmen of gas and water companies to tap a pipe, and, when the contents rushed out, to insert a plug. That might do in the case of gas and water, but in pneumatic tubes it stopped the first carrier that came. Many experiments had been tried to ascertain which was the best and cheapest material for the carriers, and it had been found that those made of gutta-percha, covered with felt, travelled fastest and were by far the best as regarded first cost and maintenance. Carriers in wrought-iron tubes would travel 100 miles before needing repair. The tube from the Central Station to the Stock Exchange was partly lead and partly iron, and there the carriers lasted for a distance of 250 miles. In lead tubes most carriers would run continuously for four months without requiring repair. It had been stated that the maintenance of carriers in the two wrought-iron tubes to the West Strand cost more than that of all the carriers in the rest of the tubes. The whole system of the Electric and International Company was first of lead. The first iron tube was that to the West Strand. Before that no trouble had been experienced from water, although the air was delivered precisely as at present; but in the engine used for the West Strand system water was injected into the pump, and the

pump itself was surrounded by a water jacket: that injection caused water to pass into the tube, so that at two points on the route boxes were inserted to trap the water, and it was necessary to remove it once or twice daily by means of a siphon. It had been suggested that the air should be cooled before being used. But it had been calculated that to cool 2,000 cubic feet of air per minute, corresponding to the quantity passing at present from the pumps, would cost £150 per annum to bring the temperature down from 120° to 70°, supposing the water could always be obtained at a temperature of 50°. Dr. Siemens had stated that the system laid by his firm included the switches, &c. The price quoted by Mr. Preece did not include the valves, but he had made a calculation from which he found that the valves used, divided into the mileage, increased the price from 12*s.* 8*d.* to about 13*s.* per yard. He believed that the carrying capacity of the tubes had not been stated. A 3-inch carrier would hold thirty-six message forms; a 2½-inch carrier would hold fourteen. As business increased so could the carrying power by using forms of thinner paper, or by coupling two or more carriers together. It appeared to him that the radial system as used by the Post Office was best adapted to the purpose for which it was intended. The ideal of perfection in the matter of telegraphs was absolute annihilation of time and space, and the system that made the nearest approach to that appeared to him to be the best. The amount of local traffic between station and station in London was so trifling as not to require the laying of a single tube. All the tubes were used to collect messages from outlying stations to the central station to be transmitted thence by wire, or to convey the messages telegraphed to the central station from the provinces to the nearest point from which they could be delivered by hand. The pneumatic system, as developed by the Post Office, had been utilised in a manner that perhaps might be available for large factories or offices supplied with engine power. The Central Station instrument gallery had an area of 20,000 square feet; the distance from one corner to the other was in some cases 90 yards or 100 yards, and in order to facilitate the transmission of messages rapidly from one part of the gallery to the other he had introduced a very simple system. A message tube was fitted into one side of a box, which was both a forwarding and a receiving box, and connections were made to two cocks, one for pressure and one for vacuum. A boy stood in front of it, and when messages were brought to him he simply inserted them in the carrier, which conveyed them from one part of the room to the other in about

five seconds. There were ten such lines, the longest being 90 yards, and the shortest 60 yards.

Mr. PREECE, in explanation of Dr. Siemens' correction of his statement as to the abandonment of the circuit system in Berlin, said that there were two systems of pipes in Berlin, both of which had been originally worked on Messrs. Siemens' principle. Now only one pipe was worked at a time. The system had not been abandoned in regard to the present working, but it was intended to abandon it in any future extensions. In comparing the circuit system as employed in Paris with the radial system adopted in England, he had no intention of drawing a comparison between the continuous or 'air-rope' system and the mode of working adopted in London. He spoke of the circuit system extended along the streets of a city to meet the requirements of intermediate stations, without reference to the mode of working adopted in the transmission of air.

Mr. CULLEY observed, through the Secretary, that it was not the object of the Authors to show what systems would best apply to conditions totally unlike those which obtained in London; but to describe the system which an experience of many years had proved to be necessary to perform the work to be done at home. The Paper was, in truth, somewhat of an apology for the adoption of the more costly radial system in place of that in use in Paris and elsewhere; and the Authors had endeavoured to show that the latter could not meet the demands of the service, and that the extra expense was unavoidable. Mr. Cowper had stated that, with ten circuits of eight stations each, London would be well served. Mr. Culley felt sure the public would not for a day tolerate the delay to messages which would arise from the substitution of tube circles of eight stations for the existing wire circuits, on all of which the delay on the line was now under five minutes. The Paper referred to telegrams alone, not to the delivery of letters. The messages from the City stations must be brought to the Central Office for transmission into the country within two or three minutes. To accomplish this, the communication, whether by tube or by wire, must, in cases where the traffic was large, be direct. It would be as impracticable to group the Central Station, the Stock Exchange, Leadenhall Street, and Fenchurch Street in one continuous tube as to place London, Liverpool, Manchester, and Birmingham on one wire. As it was desirable to be very definite in the terms employed to denote or describe each different system, he would venture to ask if a mere "up" and "down" line of parallel tubes could be correctly called a "circuit" system? The analogy

of the railway would seem to decide this question in favour of the term "double line." The choice between lead and iron tubes was a point which deserved great consideration. It was not quite fair to compare the cost of Dr. Siemens' 3-inch with the Authors' $2\frac{1}{4}$-inch tube, and perhaps the best available comparison might be that between the Paris $2\frac{1}{2}$-inch iron tube, and the lead $2\frac{1}{4}$-inch tube at 13*s.* 8*d.* and 13*s.* 3*d.* respectively. Although it might be said that the circumstances were not exactly alike, yet it was clear that the lead with its iron cover was not much dearer than iron; possibly because in the first case the articles were those in ordinary use, while the particular iron pipe was a special manufacture. But he submitted that nothing less than a large saving would have been a sufficient justification for abandoning the system which had given such excellent results for so many years, and which bade fair to last for an indefinite period, especially when the results obtained in the iron tube were not entirely satisfactory. The rusting of the iron would probably not occur to so great an extent in a tube worked by the engines now employed at the General Post Office. As regarded the suppression of the air reservoir or container, which had been spoken of as an ill-considered measure, he would remark that, in respect to the power of condensing, or rather of collecting, the moisture of the air, the large and very long air-mains employed (each with a capacity of 400 cubic feet), and which were exposed to the air from the bottom to the top of the lofty building, were at least as effective as any other form of container it would have been practicable to employ. However this might be, as a matter of fact, no sensible amount of moisture had ever been found in the lead tubes. No doubt lighter air, or hydrogen, would give better results; but the practical difficulty of employing either, and the cost of hydrogen would seem to be prohibitory. Mr. Cowper had narrated the result of an experiment in which the tube was exhausted before the insertion of the carrier, with a great increase of speed. This was known as the "closed tube" method, and might, in some cases, be used with great advantage. But where the traffic was heavy and continuous, the time gained by the increase of velocity would be lost on account of the interval which must be allowed for exhaustion between each transit. Some allowance must also be made for the extra trouble in working the traffic.

Mr. SABINE, in reply upon the discussion, stated his regret that Mr. Culley had not been present to give some personal explanations, his practical experience being probably greater than that of any engineer in the world in pneumatic matters. With regard to

the discredit which he had been supposed to throw on Dr. Siemens' system, it appeared to him that no one in the course of the discussion had really stated what that system was. Mr. Bramwell had stated that it was not the circular Paris system. Then what was it? Was it the circuit that Dr. Siemens represented as an ellipse, drawn in the form of two straight lines joined by a communication at the end? That could not be, for Dr. Siemens himself disowned the piece at the end, and said that the tubes would work better, or as well, without it. The mere fact of two straight lines being laid together could not form a new system. That had been patented, in 1810, by George Medhurst, who described it in a few plain words: "If there are two tubes of the same dimensions leading from one place to another, packets of letters may be conveyed each way at the same time without the possibility of clashing against each other, and many packets may be conveyed in the same tube, which can never approach each other, but will proceed with a uniform motion and equal rapidity to their destination. Where the tubes enter an air-tight chamber the packets will be deposited, and may be delivered, or forwarded to the next stage through their proper tubes commencing in the same room, and their progress can never be impeded by the seasons or the elements." The up and down line was no more a new system than the London and Brighton railway was a new system. The only thing that the communication at the end did was to connect the two, so that one could do nothing without the other. If a carrier had to be sent from the central to the end station, it would be necessary to expend an equal amount of engine power in drawing a useless amount of air from the end to the central station in the other direction. That part of the invention was not Messrs. Siemens'. The latter in its entirety was much more perfect. It was a system of two concentric circles having air passing through them in opposite directions, so that each station on the line had two methods of arriving at the central station. What had been called Messrs. Siemens' system was only a compromise, which had been adopted perhaps because circumstances did not allow the system to be carried out in its entirety. Dr. Siemens had mentioned that the $2\frac{1}{4}$-inch lead tubes had cost several times as much as iron tubes. That question had been gone into before the present system was adopted, and it was found that if the iron tubes were of the special manufacture required for a pneumatic line, they were as dear as the ordinary articles of manufacture —the lead tube and the cast-iron tube which had been put down previously. Besides, it was perfectly well known, from the working

of the Berlin system, that water was introduced into the tubes. It might be from the pumps, but possibly it was from leakage through the joints. When the tubes were first introduced in Berlin they were supplied at regular intervals with jackets pierced with holes, leading down to wells in the streets, and the water leaking into the wells was periodically pumped out. The behaviour of the tubes in London was similar, but there were no jackets from which to collect the water. With regard to the cost, neither Dr. Siemens nor Mr. Preece were quite accurate. Mr. Preece omitted to state that he had included the valves and other machinery in the 15*s.*; and Dr. Siemens, in stating the price at 8*s.* 4*d.*, forgot to say that neither profit nor superintendence had been included in that sum. The truth probably lay about midway, which would come very near the Paris price of 13*s.* 8*d.*, as compared with 13*s.* 3*d.*, the price of the London lead and iron tubes. The opinion of Dr. Siemens and Professor Unwin, that the expansion of air in a tube was necessarily isothermal, was, he thought, untenable. Experimental facts certainly did not justify that view of the expansion of air in tubes or in any other position. Air set in motion from a container must be set in motion at the cost of some work; and the only work that air could obtain was that which it got from the reduction of the heat in it; and as long as any motion lasted in the air, the temperature could never come back, unless by conduction, to that at which it started. He had conducted the experiments for Messrs. Siemens from the first, when they were made with glass tubes, and afterwards with the actual tubes in Berlin. These, soon after they were laid, had been worked on the continuous system, the air being pumped from one station through the middle station and back again. The loss of heat was so great that on passing over the canal bridge, towards the winter, it would be found that one tube was warm, while the other was covered with ice or snow. In the cellar below the station, one would be found to be quite warm and the other cold. He had recently gone into the central station in London and felt the temperature of the air of the vacuum tubes and of the pressure tubes; and he found what Mr. Wilmot (the practical engineer of pneumatic telegraphs) told him was always the case —that there was not the slightest difficulty in recognising which was the vacuum and which the pressure tube, by the hand, and observing which was warmer and which was colder than the atmosphere. In opening the streets Mr. Wilmot said he could recognise the two tubes in the same way. That did not look like isothermal expansion. If air were inclosed in a confined space,

and expanded, say in a cylinder, and the piston suddenly lifted, the temperature of the air inside the cylinder fell. If air were inclosed in a vessel, and a hole made in the side to let it out, a thermometer held in front would show that the air was chilled in coming out. M. Mignot, a French engineer, had, upon that principle, constructed a beautiful apparatus for producing large blocks of ice. He allowed the air to expand in the passage between the spaces of vessels containing water, which as it passed through it chilled and produced the ice. There was no isothermal effect there. The only difference between M. Mignot's passage spaces and the pneumatic tube was, that while the former were 10 feet long, the latter might be 1,000 feet. In a pneumatic tube of short length there could be no question that the air did not expand isothermally, for it became quickly chilled, and the chilling might be carried to such an extent as to produce ice. If the length were greater the effect produced would be less, because it would be masked by the heat developed by friction and conduction. With regard to the Berlin tube the case was peculiar, for the same air was absolutely pumped round and round the system, and as it passed round became alternately hot and cold. If the system were open at the middle, it might be said that there was some error of observation; but as the same air alternated between hot and cold, it was evident that it did not pass round the system isothermally. He thought it would be time enough to challenge the correctness of the formula when some proof had been put forward to show that expansion in pneumatic tubes was necessarily isothermal. At present it remained simply a matter of speculation, and any error from the unknown rate of expansion was practically eliminated in the formula itself. With regard to vacuum-working, a suggestion had been made based upon a clause in Dr. Siemens' patent specification, that hydrogen would be a valuable substitute for vacuum air. He could scarcely imagine that the suggestion had been made seriously. With every carrier admitted into the tube there would enter atmospheric air, and the hydrogen would soon produce explosive gas, and the lives of the operators would certainly be endangered. There was a very great mistake made in supposing that in practice vacuum-working was much cheaper than pressure-working. As a fact, at the Post Office a 50-HP. engine was used for pressure, and one of the same power for vacuum—both worked up to their maximum; practically, the same number of tubes were used; and no difference had been found in the cost of working. Professor Unwin had discovered a misstatement of the vacuum work with the

steam-engine: that mistake was entirely his (Mr. Sabine's), not Mr. Culley's.

Herr ZELLI, Director of Telegraphs in Vienna, observed, through the Secretary, that the aim of the system had been to forward telegrams arriving at the central station to the addressees in the several districts, to forward from the district offices those intended for country addressees to the central office, and to transmit letters from one district to the others, all with the utmost possible despatch. The letters were not restricted in number of words, but their weight must not exceed 10 grammes. They might be open or closed, but must be handed in without being fastened by sealing-wax.

For carrying out this design there were ten stations, connected by a line of tube, which at both ends entered into an apparatus serving for the reception and the despatch of telegrams and letters. The various stations were:—Central Telegraph Station, No. 1; Meat Market, No. 2; "Kärnthnerring," No. 3; "Wieden," No. 4; "Gumpendorf," No. 5; "Neubau," No. 6; "Josephstadt," No. 7; "Exchange," No. 8; "Leopoldstadt," No. 9; and "Landstrasse," No. 10. The stations Nos. 1 to 7 were in a circular closed line, so connected that No. 1 formed the commencement and end of the line, while the remaining three stations were branches from this closed circuit.

Compression and rarefaction of the air were accomplished by eight air-pumps, of which four served for compression and four for rarefaction. The former had a diameter of 450 millimètres and a height of 860 millimètres, so that each had a capacity of 0·136 cubic mètre, and they made forty to fifty strokes per minute. The same number of strokes per minute was also made by the latter; but these had a diameter of only 350 millimètres, a height of 660 millimètres, and a capacity of 0·063 cubic mètre.

For working the air-pumps, two horizontal steam-engines without condensers, but with variable expansion gear which could be regulated at will, were used as follows in stations 1 and 5, the first with 20 lbs., the last with only 11 lbs. pressure. In the same stations were kept, as reserves, two extra steam-engines of similar make, so that during the occasional repairs of the whole machine, the working might still proceed.

There were twelve reservoirs of strong iron plate, of equal capacity with the whole pneumatic tube system, six of which were for compression, with a capacity of 116 cubic mètres, and six were vacuum reservoirs, with a capacity of 90 cubic mètres. Two reservoirs for compressed air, and two reservoirs for vacuum, were

placed at each of the stations 1, 2, and 5. These reservoirs were connected with the air-pumps and the pneumatic apparatus of the station in which they were situated.

A manometer was used, each degree of which showed the pressure equal to the weight of 1 centimètre of quicksilver.

Each station was fitted with a commencing, an intermediate, and a terminal apparatus.

In general, the pneumatic apparatus consisted of a conical tube with a small locking cover to introduce the carrier with the telegrams and letters, of a large locking cover to receive the carrier arriving with letters and telegrams, and of a tube-closing valve interposed between the small and large locking covers in a cylindrical box, which served either to open or to close communication with the tube.

Besides the above-mentioned constituent parts, the pneumatic apparatus possessed those described in the following category :—

In the commencing apparatus, a regulating valve fixed at the back, and used for connecting the apparatus with the reservoir tube; for connecting the apparatus with the discharge air-tube or free atmospheric air; and for shutting off both these connections. And a forwarding valve inserted in the discharge tubes of both sorts of reservoirs in the station and in the tubes coming from the regulating valve of the pneumatic apparatus, and so arranged that the apparatus and the line-tube could be connected either with the compression or vacuum reservoir, or with both kinds of reservoirs shut off.

In the intermediate apparatus, which was not in direct communication with the air reservoirs, and could therefore be only put in connection with them by the tube, and was provided with an air-outlet tube instead of the regulating valve; a valve-lid on the end of the pneumatic apparatus, which could, by a lever, be either opened to establish communication with the outer air, or shut to keep up through communication. Each station provided with this sort of apparatus was connected with at least two line-tubes, the ends of which led into the apparatus. Between both parallel entering line-tubes was a large connecting valve, and before this in each separate line, a locking contrivance was inserted, fitted with a light hand-lever. The large connecting valve served to put in communication the line-tubes discharging into the station without interruping the passage of air through the apparatus. The vacuum locks prevented the escape of the compressed or rarefied air from the tube when the apparatus was opened. By the admission of air by a small lifting valve, the train

going out of the station could be brought up behind the large connecting valve.

The terminal apparatus possessed the same constituent parts as the intermediate apparatus, but had only one light vacuum lock. Each pneumatic apparatus was also fitted with a manometer, and all were firmly set up on strong iron tables.

Twenty pieces of pneumatic apparatus were introduced in the pneumatic system in Vienna, viz. :—Nine pieces in three stations at the commencement of the line; eight pieces in four stations in the central portion of the tube; and three pieces in three stations at the end of the line.

The air reservoirs in stations 1, 2, and 5 were brought on one side into connection with the air-pumps worked by the steam-engines, and on the other side with the commencing apparatus by its own tube. The length of this connecting tube was 2,457·8 mètres, of which 251·8 mètres were of cast iron, with an internal diameter of 105·4 millimètres, or 2·18 cubic mètres capacity, and 2,206 mètres of rolled iron, with an internal diameter of 6·5 millimètres, or 7·26 cubic mètres capacity.

The tube connected the pneumatic stations, and formed the actual channel through which the telegrams and letters were forwarded.

The length of the tubes was 11,852·45 mètres. The single lengths of tube, made of rolled iron, were 5 mètres long and 65 millimètres in diameter, and had an internal capacity of 38·81 cubic mètres each.

According to the position and purpose of the several stations, they came under three categories :—

1. Head, or chief stations, in which pneumatic commencement apparatus and air reservoirs were placed. (Stations 1, 2, and 5.)

2. Intermediate stations, where two or more tubes entered, and so had two or more pieces of intermediate apparatus. (Stations, 3, 4, 6, and 7.)

3. And terminal stations, where one or more ends of the tubes entered, supplied with terminal apparatus. (Stations 8, 9, and 10.)

To the appliances for working belonged :

(*a*) Telegram boxes, for the reception of the telegrams and letters, of hammered steel, 50 millimètres in width and 110 millimètres in length, over which a leather cover was slipped, to prevent the contents of the box from falling out during its passage.

(*b*) Piston boxes, similar to the last, but with a leather ring near one end, which fitted hermetically to the inner surface of the

tube. The piston boxes served to drive the telegram boxes, chiefly by the hermetical closing of the tube, and so preventing the loss of too much air near the telegram boxes, between which and the tube there was a play of 14 millimètres. Numbers were put on the telegram boxes corresponding to the numbers of the stations.

Two neighbouring pneumatic stations were also connected by a telegraph wire, and supplied with the necessary electric apparatus. The telegraphic correspondence was confined to the necessary advice and signalling of the departure or arrival of a carrier, or the required supply of air. The telegrams and letters, if the addressee did not live within the delivering radius of the station to which the message was directed, were forwarded to the pneumatic station concerned for delivery, and were sent by the pneumatic train in the telegram box marked with the number of the station in question. In each pneumatic station messengers were in readiness to deliver the telegrams or letters. There were also five branch offices, appointed for the reception of telegrams and letters, to be collected for forwarding from the pneumatic station.

The forwarding of the carriers was arranged according to their several orders of handing in.

Through the stations 1, 2, 3, 4, 5, 6, and 7, back to station 1, a train of six telegram boxes and one piston box was in constant circulation, and was stopped in turn at each station, and the arriving letters and telegrams destined for that station were taken out, and others despatched.

The manipulator in No. 1, from which the train of carriers started, filled the boxes with the telegrams and letters waiting to be forwarded to their respective stations. He then telegraphed to the next station, and put the boxes in the small locking cover of the apparatus in the connecting stations 1 and 2. He next, by means of the cock at the other end of the apparatus, opened communication with the compressed-air chamber, and the air acting upon the piston box, introduced last of all, the boxes were forced to station 2. The atmospheric air in the tube was thus driven out through the apparatus in station 2.

By the noise produced by the movement of the carriers in the tube, the manipulator at station 2 heard when the train of carriers was near; he partially stopped the free exit of the escaping air, and thus the train was checked, and only proceeded at a slow rate into the apparatus.

As soon as the train had arrived, he advised the operator at station 1, who on his part immediately shut off the entrance of the compressed air. The manipulator in No. 2 opened the large closing

cover of the apparatus there, and removed the boxes destined for station 2, emptied them of their contents, and forwarded in a similar manner the collection of letters and telegrams to station 3. When the train had arrived there, the manipulator at station 3 shut off the apparatus from the tube which was now filled with compressed air, and drew the train forward into the closing cover of the apparatus, exchanged the outgoing for the incoming telegrams, telegraphed to station 4, and put the train into the tube leading there. Station 3 would then, by means of the lifting valve, put the two tubes, 2 to 3 and 3 to 4, in communication, and thus the compressed air out of the reservoir of station 2 by tube 2 to 3 passed into tube 3 to 4, and forced the train up to station 4. On the train arriving there, the manipulator at station 2 shut off the compressed air, station 4 called station 5, and laid the train leading to station 5. The official there now exhausted the air out of the main tube, and thus drew the train from station 4 to his own, which the manipulator effected by connecting the apparatus with the reservoir of rarefied air (vacuum reservoir).

From station 5 to 6, and from that to 7, the train was again driven by the compressed air of the reservoir of station 5, and, finally, from station 7 to station 1, whence it started, the manipulation being a repetition of that from station 2 to 5.

In the direct lines, as stations 1 and 8, 2 and 9, and 2 and 10, the manipulation was similar to what had been described, namely, the train was pushed out by compressed air and drawn back by rarefaction.

Each station, as a rule, was provided with only one box; still, in case of necessity, two boxes could be forwarded from the same station, though the number of boxes by one train must never exceed eight at the most.

By the application of different kinds of cocks, valves, slides, &c., precautions were taken, in the event of interruptions in the working between the several stations, to render it possible to carry on the working otherwise than as described. The length of the line extending from No. 1 through 7 back to 1 was 8,832 mètres, and the train occupied fifteen minutes in transit, of which seven minutes were taken up with the stoppages at the stations and eight minutes for the actual passage of the train. In the event of any interruption in the tube, the place was ascertained by a simple instrument connected with a chronograph, within 0·02 per cent. of the length of the tube, in the longest line in Vienna within 1 mètre. In order to increase the traffic in the local correspondence of Vienna, negotiations were in progress for lowering the present

tariff of 20 kreuzers to 10 kreuzers for letters, and to 6 kreuzers for express correspondence cards.

RETURN of the ROYAL PNEUMATIC STATIONS from the 1st of May, 1875, to the 30th of November, 1875.

| Stations. | Letters sent out. | Telegrams forwarded to the Central Station. | Telegrams. | | | | Remarks. |
|---|---|---|---|---|---|---|---|
| | | | Coming into Central Station. | Forwarded thence. | Delivered locally. | Total. | |
| I. . | 636 | 176,083 | 643,836 | .. | 308,278 | .. | The stations were opened for pneumatic working on the 1st of May, 1875. |
| II. . | 908 | 29,981 | .. | 54,819 | .. | .. | |
| III. . | 741 | 51,099 | .. | 80,165 | .. | .. | |
| IV. . | 358 | 6,514 | .. | 29,981 | .. | .. | |
| V. . | 745 | 1,442 | .. | 16,198 | .. | .. | |
| VI. . | 297 | 4,184 | .. | 19,783 | .. | .. | |
| VII. . | 210 | 2,560 | .. | 11,087 | .. | .. | |
| VIII. . | 168 | 35,783 | .. | 26,525 | .. | .. | |
| IX. . | 322 | 9,567 | .. | 82,504 | .. | .. | |
| X. . | 277 | 4,357 | .. | 14,496 | .. | .. | |
| Total | 4,662 | 321,570 | 643,836 | 335,558 | 308,278 | 643,836 | |

Dr. STEPHAN observed, through the Secretary, that in Berlin there were at the present time two pneumatic systems, which had been laid down at different periods. The older connected the Chief Telegraph Station with the Exchange Station; the other system was used for transmitting telegrams between the Chief Station and the stations at the Brandenburg Gate and the Potsdam Gate. The distance between them was as follows:—

| | | Mètres. | Yards. |
|---|---|---|---|
| I. | From the Chief Telegraph Bureau to the Exchange | 890 | 973·33 |
| II. | „ „ „ „ Brandenburger Gate | 1,442 | 1,577 |
| III. | „ Brandenburger to Potsdamer Gate . . . . | 831 | 908·8 |

The first system had been fully described[1] in a Paper by Dr. Brix on "The Pneumatic Despatch Tube between the Central Telegraph Station and the Exchange Building in Berlin," of which paper the following was an abstract:—

As the success of forwarding messages by pneumatic despatch depends upon a knowledge of the laws of motion of air in tubes, it is advisable to investigate first the principle of this system of transmission, under the limitations necessary in employing tubes of comparatively small diameter and with low working pressure. With a view to establish these principles, the following experiments were made with tubes of various diameters and lengths. By means of a pump with fly-wheel and winch, which could be used either as vacuum or pressure

[1] *Vide* "Zeitschrift des Deutsch-Oesterreichischen Telegraphen-Vereins." Jahrgang xiii. (1866), p. 90, &c.

pump, or as both pressure and exhaust pump, the air was condensed or rarefied in a reservoir. The reservoir communicated with the atmosphere by means of the tube, in which was measured the velocity of the air. The pressure in the reservoir was measured by a mercurial manometer. The quantity of air passing was measured by a gasometric arrangement. The measured quantity of air, divided by the section of the tube, gave the velocity. By Mariotte's law the velocity at opposite ends of the conducting tube can be readily calculated. Suppose, for instance, the air in the reservoir diminished to and maintained at half atmospheric pressure, and the velocity of the air of atmospheric tension entering the tube to be 50 feet, then the quantity of air leaving the reservoir being double the volume, the velocity must be 100 feet.

Let $v_1$ be the final velocity of the air in the tube; $v_{11}$ the initial velocity, and $v$ the velocity at a point distant $x$ from the beginning of the tube; the mean velocity will be $v^1 = \frac{v_1 + v_{11}}{2}$. Let $l$ be the length of the tube, and $d$ the diameter (both in feet), $h$ the pressure of the entering air, $h_1$ the pressure of the issuing air, $h - h_1$ the effective pressure, $\alpha$ a constant: then—

I. The final velocity,

$$v_1 = \alpha \cdot \frac{h - h_1}{h} \cdot \sqrt{\frac{d}{l}};$$

II. The initial velocity,

$$v_{11} = \alpha \cdot h \cdot \frac{(h - h_1)}{h^2} \cdot \sqrt{\frac{d}{l}};$$

III. The velocity at the point $x$,

$$v = \alpha \cdot \frac{(l - x)h_1 + xh}{l} \cdot \frac{h - h_1}{h^2} \cdot \sqrt{\frac{d}{l}};$$

IV. The mean velocity,

$$v^1 = \alpha \cdot \frac{h^2 - h_1^2}{2h^2} \cdot \sqrt{\frac{d}{l}}.$$

These formulæ are only approximative. The constant $\alpha$ is dependent on the inner surface of the tube, but was experimentally shown to be 15,950. Required, for instance, the mean velocity of the air in a tube of 13,000-feet length, and 3 inches diameter, for a pressure difference of 1 atmosphere, there is obtained:—

(1). At 1 atmosphere pressure (in excess),
$h = 2$ atms., $h_1 = 1$ atm.,
a mean velocity of 26·2 feet per second;

(2). At 1 atmosphere under pressure (vacuum),
$h = 1$ atm., $h_1 = 0$ atm.,
a mean velocity of 35 feet per second.

(3). At ½ atmosphere pressure and ½ atmosphere vacuum $h = 1\frac{1}{2}$ atm., $h_1 = \frac{1}{2}$ atm., a mean velocity of 31·1 feet per second.

The small moment of inertia of the mass of the reservoir, and the momentum of the air itself, disappear in comparison, as quantities, with the friction of the air in the tube. By the circuit system important advantages are gained. As appears from formula IV., the mean velocity of the air depends upon the factor $\frac{h^2 - h_1^2}{h^2}$, which remains unchanged when $h$ and $h_1$, and also their difference, are proportionally diminished. The work expended in the pump is directly

proportional to the density of the compressed air, which is carried off proportionally as $h_1$ increases. As by the circuit system the mean density in the tube can be reduced, so can also reduction be made in the power expended. The following short tables of experimental results show close agreement between the calculated and observed quantities, and serve to indicate how nearly the preceding formulæ are correct.

RELATION BETWEEN FINAL VELOCITY AND PRESSURE.

At one end super-pressure, at the other atmospheric pressure.

| $h - h_1$ in Centimètres. | $\frac{h - h_1}{h}$ | Quantity in Cubic Feet. | Velocity, in Feet per Second. | |
|---|---|---|---|---|
| | | | Observed. | Calculated. |
| 16 | 0·174 | 0·47 | 22·6 | 22·0 |
| 18 | 0·192 | 0·51 | 24·6 | 24·3 |
| 20 | 0·208 | 0·55 | 26·6 | 26·2 |
| 22 | 0·225 | 0·59 | 28·6 | 28·4 |
| 24 | 0·240 | 0·64 | 30·5 | 30·2 |
| 26 | 0·255 | 0·67 | 32·2 | 32·1 |
| 28 | 0·270 | 0·71 | 34·0 | 34·0 |

The pressure was measured by a mercurial manometer, the quantity of air by a gas-meter; the length of the experimental tube was 348 (Prussian) feet, and the diameter ¼ inch. The barometer stood at 760 millimètres during the experiments.

RELATION OF VELOCITY TO PRESSURE.

At one end super-pressure, at the other vacuum.

| Pressure in Centimètres. | Quantity of Air-Current, in Cubic Feet. | | |
|---|---|---|---|
| | In Middle. | At End. | Calculated. |
| ± 7 | 0·186 | 0·205 | 0·201 |
| ± 10 | 0·240 | 0·277 | 0·270 |
| ± 12 | 0·267 | 0·317 | 0·311 |
| ± 16 | 0·313 | 0·396 | 0·396 |

RELATION OF VELOCITY TO LENGTH OF TUBE.

| $h - h_1$ in Inches. | Diameter in Inches. | Length in Feet. | Quantity in Cubic Feet. | Velocity, in Feet per Second. | |
|---|---|---|---|---|---|
| | | | | Observed. | Calculated. |
| 6 | 0·25 | 112 | 0·7 | 34·3 | 34·3 |
| — | — | 84 | 0·8 | 39·2 | 39·6 |
| — | — | 56 | 1·0 | 49·0 | 48·7 |
| — | — | 28 | 1·4 | 68·6 | 68·6 |

The calculated values are based on the assumption that the velocity varies inversely as the square root of the length.

RELATION OF VELOCITY TO LENGTH OF TUBE.

| $h - h_1$ in Inches. | Length in Feet. | Diameter in Inches. | Quantity in Cubic Feet. | Velocity, in Feet per Second. | |
|---|---|---|---|---|---|
| | | | | Observed. | Calculated. |
| 12 | 100 | 6·75 | 0·860 | 42·1 | 42·1 |
| — | — | 5·20 | 0·450 | 36·4 | 36·9 |
| — | — | 3·25 | 0·185 | 27·0 | 26·0 |
| 10 | — | 6·75 | 0·810 | 39·6 | 39·6 |
| — | — | 5·20 | 0·401 | 32·2 | 34·6 |
| — | — | 3·25 | 0·161 | 23·4 | 24·4 |

These calculated values are based upon the law that the velocity varies as the square root of the diameter of the tube.

The following table gives the mean velocities of the air in tubes of 13,000 feet in length, with various diameters, and with the application of

(*a*) 1 atmosphere sur-pressure;
(*b*) 1 atmosphere under-pressure;
(*c*) ½ atmosphere sur- and ½ atmosphere under-pressure, as follows:—

| Diameter in Inches. | Mean Velocity. | | |
|---|---|---|---|
| | 1 Atmosphere Sur-pressure. | 1 Atmosphere Under-pressure. | ½ Atm. Sur-pressure. ½ Atm. Under-pressure. |
| 2½ | 23·9 | 32·0 | 28·4 |
| 3 | 26·2 | 35·0 | 31·1 |
| 3½ | 28·3 | 37·8 | 33·6 |
| 2 | 30·3 | 40·4 | 35·9 |

The scheme of the line of pneumatic tubes is as follows:—Two parallel tubes of wrought iron, 2¼ inches in diameter, are laid under the pavement at a depth of 3 feet. These tubes are connected about 1 foot from the end by a cross tube in the Exchange building; the parallel tubes terminate with cocks or stop-valves. In the Telegraph Station one of the tubes, about 5 feet from the end, communicates through a cock with a reservoir of compressed air; the other tube at 1 foot from the end is also in connection through a cock, with the reservoir of air at reduced pressure. A continuous stream of air is maintained in the tubes by an air-pump.

The carriers (*Depeschenwagen*) consist of a central despatch-carrying compartment, provided at each end with two wheels, the planes of revolution of which are at right angles, so arranged that a line passing along the centre or axis of the carrier would also pass along the diameter of each of the four wheels. The wheels or runners are massive, and of hard steel, running on polished axles. The despatch chamber is of brass tube 7⅝ inches long, 1¾ inch outer diameter, and $\frac{1}{32}$ inch thick. It is closed with two iron caps, which carry projecting pieces for the axles of the runners. The runners are about 1½ millimètre less in diameter than the tube.

The end of one tube serves as a departure station; the end of the other as an arrival station. On the departure tube are two cocks; that near the receiving station being closed, the cock near the end is opened and the carrier introduced. The terminal cock is then closed and the other opened, when (the tube being inclined downwards) the carrier rolls into the main tube. The other tube at each station has a receiving chamber, with proper apparatus to catch the carrier. Both stations are underground. At one portion of the line the tubes pass over an iron bridge, rising from the level by a mean gradient of 1 in 6, and falling to the level on the other side of the bridge. The bridge is a swing-bridge over a river, and the tubes have to be thus raised to admit of ships passing under them at high tides. There are four difficult bends in the course, two being of 40-feet radius.

The total length of the two tubes between these stations is 2,835 feet, and with 9-inches mercury pressure and vacuum the passage is accomplished in ninety-five seconds to the Exchange, and in seventy seconds from the Exchange. The foregoing formulæ of Dr. Siemens' show a calculated mean velocity of 95·69. About eight hundred despatches a day have been forwarded through the tubes.

The subsequent system differed from the first only in the following particulars:—

1. The internal diameter of the pipes was 3 English inches.

2. The apparatus for despatching and receiving had been altered and made into one.

The mode of working in both pipe-systems was the same, and had not been altered since described in the before-named "Zeitschrift."

The results of the works laid down by Messrs. Siemens and Halske, of Berlin, on the principles worked out by Dr. Siemens, had been entirely satisfactory.

In the extension of the present pneumatic system about to be undertaken, the principle upon which that system had been worked could be abandoned, inasmuch as the traffic between the telegraph station intended to be connected with the existing network would not be so considerable as to warrant the additional and higher working expenses which would be incurred if the permanent air-current were retained. For the present conditions, on the contrary, it seemed perfectly sufficient to allow the despatch of telegrams between the individual stations to take place at fixed intervals of time—every fifteen minutes—as with the pneumatic systems in Paris, and lately in Vienna. In the time in which no despatch took place, the work done by the engine—compression or exhaustion of the air required for the sending of messages—could be stored up in air-reservoirs of greater dimensions.

Thus the entire work of the engine was utilised, and the working expenses would be lower, than with the system of permanent air-circulation in the pipes. The latter system, indeed, had the great advantage, in the case of a very brisk traffic, of enabling telegrams to be sent at any moment; but, as stated above, such was not the case in Berlin. On this account alone, Siemens' system would not be retained in the new pneumatic network.

In the extension about to be undertaken, altogether fifteen telegraph stations would be connected with each other (Fig. 38). The aggregate length of pipes necessary for this amounted to 20,000 mètres (12 miles 752 yards), and their breadth would be 65 millimètres (2·559 inches). The air employed as propeller would be compressed or exhausted in large reservoirs by four engines, to be erected at different points of the system. The reservoirs for compressed air would only be connected with the pipes when a despatch of telegrams was about to take place; whereas the reservoirs for rarefied air would be permanently connected with the pipes. These pipes would be open only during the despatch of the carriers. In order to lessen as much as possible the time of despatch, the net-

work had been so planned that the telegraph bureaux, as indicated in the sketch below, were divided into two circuits, each with two engines. Both circuits were connected with the Chief Telegraph Bureau, numbered 1. As the two ends of one current, which met in the chief office, had been also connected with the office in the Exchange, numbered 9 in the sketch, it was possible to carry on the traffic taking place between these two offices during Exchange time without delay, by sending out carriers every five minutes.

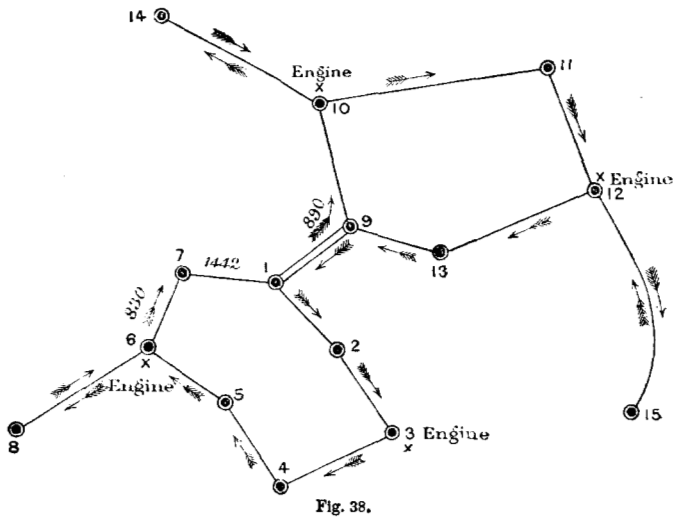



Fig. 38.

The three offices really situated outside the pipe circuit (Nos. 8, 14 and 15) would be connected with the nearest office by branch pipes.

In the closed circuits the transmission of telegrams always took place in the same direction, and therefore either through compressed air only, or through exhausted air only. In the branch pipes, however, the service would be conducted in both directions; in one by using compressed air, and in the other by exhausting the pipes of air.

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November 30, 1875.
THOS. E. HARRISON, President,
in the Chair.

The discussion upon the Papers, No. 1,439, "The Pneumatic Transmission of Telegrams," by Messrs. CULLEY and SABINE, and No. 1,445, "Experiments on the Movement of Air in Pneumatic Tubes," by M. BONTEMPS, occupied the whole evening.