

Mr. B. SAMUELSON said he had endeavoured to describe the plant of a modern blast furnace rather from the point of view of an engineer than from that of a chemist. In the drawings (Plates 16 and 17) such details were given as, together with the dimensions and quantities stated in the Appendix, would enable any engineer to erect a blast-furnace plant.

He had stated the rapid steps by which the present large dimensions had been arrived at; and he doubted whether even now the utmost attainable perfection had been attained. But at any rate the Paper would afford a standard of comparison by which future progress might be judged.

Mr. I. L. BELL called in question some of the conclusions the Author had arrived at with regard to the operation of the blast furnace. In the first place, Mr. Samuelson spoke of hot air introduced into blast furnaces causing a very high temperature in the zone of fusion. He believed that was a fallacy; if the Author had been contented with saying, taking the quantity of fuel burnt, there was a greater quantity of heat evolved, in that opinion he might have agreed. There was undoubtedly a greater quantity of heat produced when the blast was heated, because that conveyed by the hot air was added to that produced by the combustion of the coke; but inasmuch as a greater amount of work to be done was imposed upon this extra heat by the larger weight of matter to be fused, practically, he imagined, the actual temperature of the hearth of a hot-blast furnace was reduced to that of furnaces blown with cold air.

The Author of the Paper said that the ore used contained 40 per cent. of iron in its calcined state. That being the case, he was at a loss to understand how it happened that, upon some of the occasions mentioned, the calcined ironstone yielded as much as 45 to 46 per cent. He was now smelting half a million tons of similar ironstone in a year, and he had never met with any which afforded above 1 or 2 per cent. beyond the percentage first stated, viz., 40. It appeared to him, taking 18 cwt. of coke per ton of iron, and ironstone yielding 46 per cent., that both were understated, and in about the same proportions; hence he conceived there would be danger in accepting 18 cwt. of coke as being able to produce 1 ton of foundry iron from the ordinary run of Cleveland stone. He felt he was on his trial in making such an assertion, but he would endeavour to explain the grounds upon which he questioned the accuracy of the Author's figures, so far as the power of any furnace to make 1 ton of foundry iron from Cleveland stone with 18 cwt. of coke was concerned, by examining shortly the operation of iron smelting.

The Author had alluded to the introduction of air at the bottom of the blast furnace. This gave rise to the generation of an immense volume of carbonic oxide. The carbonic oxide of course was in an intensely heated state, and during its ascent in the furnace it first communicated heat to the descending materials, and then it served to remove the oxygen contained in the ore. If it were merely a question of depriving this ascending volume of carbonic oxide gas and its associated nitrogen of all their heat, by contact with substances of a lower temperature but inert in a chemical point of view, the subject for inquiry would present little difficulty; and similarly if the carbonic oxide could be saturated with oxygen before it left the furnace, it could then be determined whether the fuel had effected the maximum amount of duty it was capable of performing. But the question was a more complicated one, and he would give his reasons for thinking that it was impossible by any alteration of dimensions of the blast furnace to retain the escaping gases until their temperature was lowered to that of the atmosphere. The cold material in its descent in the furnace met a column of ascending heated gases. The heat generated by the action of the carbonic oxide on the peroxide of iron contained in the ore amounted to about 40 per cent. of the whole evolved in the furnace itself. This took place chiefly in the upper part of the furnace, so that practically in every blast furnace there were two fires; one at the lower part, where the carbon was burnt to its first stage of oxidation, and a second at the top, in which the carbon received its second dose of oxygen, and this determined the temperature of the escaping gases. Under these circumstances it was immaterial to what altitude the blast furnace were carried; a height of 100 feet or more would make no matter. That this was no mere speculation he had proved over and over again: he had taken the temperatures of blast furnaces varying in capacity from 12,000 to 35,000 cubic feet, and he had examined gases leaving furnaces of 40 feet up to 103 feet in height, and in no case beyond 80 feet had he found any marked reduction in their temperature.

A word or two with regard to the power of the carbonic oxide to take up a certain quantity of oxygen. The formation of carbonic oxide depended upon the union of an equivalent of oxygen with an equivalent of carbon; carbonic acid depended upon the union of the same quantity of carbon with two equivalents of oxygen. And, as he had said, if the carbonic oxide, in its work in the blast furnace, could be made to take up the second equivalent of oxygen, the total amount of heat to be derived from the union of these two substances would be obtained. But this was not so; and for the simple reason that the very gas which resulted from the reduction

of the oxide of iron was of itself again capable of oxidizing iron. Conceive a vessel filled with carbonic oxide into which a small quantity of oxide of iron was introduced, and that it was heated to a certain point; almost the whole of that oxide of iron might be converted into spongy metallic iron. If now a higher temperature was applied the reverse of this action would take place. The newly-formed iron then commenced to rob the carbonic acid of a portion of the oxygen, and the iron passed again into a state of oxide, probably magnetic oxide of iron. In the blast furnace there were therefore two antagonistic powers at work. There was carbonic oxide first endeavouring to wrench from the oxide of iron its oxygen; and there was carbonic acid, resulting from that operation, endeavouring on its side to reoxidize the iron; and the question to be determined was, what was the condition of the relation of these two gases which constituted the neutral point? To that question he had addressed himself for the last two or three years, and his experiments justified him in believing that when to 100 volumes of carbonic oxide, 40 or 45 volumes of carbonic acid were added, the mixture at the temperature at which the gases left the furnace ceased to have any action on calcined Cleveland ironstone. The quantity of carbonic acid in the gases of a blast furnace was of course in direct proportion to the quantity of oxide of iron introduced; therefore the question to be determined was, what quantity of carbonic oxide must be present to preserve a reducing tendency and avoid one of an oxidizing nature. That quantity he had determined approximately in the laboratory, by an immense number of experiments, to be something like 40 or 45 volumes of carbonic acid to 100 volumes of carbonic oxide. Not satisfied with this experimental proof, he had recourse to a furnace emitting its gases with the carbonic oxide and carbonic acid in these proportions, and in them he exposed specimens of calcined Cleveland ironstone from periods of one hour to forty-eight hours. He apprehended that if there had been any actual reducing power still resident in those gases it would have made itself known upon ironstone exposed to their influence; but during forty-eight hours he never succeeded in expelling 6 per cent. of the original oxygen contained in the ore, a proof that practically these gases had been retained in contact with the iron ore long enough to exhaust their powers of reduction.

The Author stated that in a furnace with a capacity of 16,000 cubic feet he employed 15 per cent. more coke than he did in one of a capacity of 30,000 cubic feet. Without questioning Mr. Samuelson's figures, he could not accept this as generally true.

The size of a furnace capable of doing perfect work was determined by the time required for a stream of carbonic oxide and its associated nitrogen traversing its contents to deliver to the materials as much heat as it could communicate, and to abstract as much oxygen from the calcined ore as they were capable of doing. He had satisfied himself that for this a furnace of 15,000 cubic feet sufficed; but he did not deny that the results obtained by Mr. Samuelson might be correct, although he believed the explanation to be that the lesser furnace of the two possibly had not been doing maximum duty. It might be said there was another way of looking at this question. What matter whether the furnace were large or small, provided the duty performed by the one was rateably the same as that of the other? As a matter of cost the ironmaster was served in the one case, so far as cost of plant was concerned, as well as in the other. This, however, was far from being the case, as might be seen on comparing the performance of Mr. Samuelson's furnace of 5,079 cubic feet, making 160 tons of iron per week, with one of 30,000 cubic feet, and running 500 tons of metal in the same time, *i.e.*, one furnace six times the size of another did only three times the work. The same held good when furnaces of larger dimensions were compared with each other, for he never found that a furnace of 25,000 cubic feet did twice the work as well as one of half the size. Making blast furnaces with so many thousand cubic feet more space would, without obtaining a corresponding increase of work, mean expending capital needlessly, *i.e.*, after sufficient capacity was reached to insure proper economy in fuel.

With regard to the question of superheated blast, until recently iron-smelters were satisfied if they heated the blast to a little above 600°. He believed the smelters in Cleveland were the first to point out, that the air could be very beneficially heated to more than that, and now the blast used was capable of fusing zinc, which was a temperature of about 780°, a change which had lessened appreciably the consumption of fuel. Encouraged by this success the same reasoning had been applied to the blast as to the blast furnaces. It was supposed that a rateable increase of economy would be derived from a further elevation of its temperature, just as it was considered that if fuel was saved by increasing the capacity of a furnace from 6,000 to 15,000 cubic feet, a further saving would result by adopting 20,000 and 40,000 cubic feet. He believed there was also a limit to the economy resulting from the use of superheated air; and notwithstanding the opinion of the Author of the Paper that perfection had not yet been reached, he believed

that it had very nearly been obtained with the blast furnace and the mode of reduction in the present form. Looking at the properties of the Cleveland ironstone, he thought perfection had been as nearly as possible attained, because he found that after certain dimensions were reached, say 15,000 cubic feet, the escaping gases were as cool and as rich in carbonic acid as from furnaces twice this capacity in which longer contact had been maintained between the ore and the carbonic oxide. This language was equally applicable to the use of superheated air. Of 1,000 units of heat evolved in a blast furnace using air about 850° their derivation would be as follows: in round numbers about 480 of those units would be evolved by the conversion of carbon into carbonic oxide at the tuyere; 130 units would be contributed by the blast, and 390 of the 1,000 units would be derived from the conversion of the newly-generated carbonic oxide into carbonic acid. Now it was manifest that the expensive source of heat in these three items was that due to the combustion of coke; and it was therefore obvious that the more blast heat could be substituted for coke heat the better; but this substitution had its limit, because the reducing power of the gases must be preserved. A withdrawal of coke heat was equivalent to a withdrawal of carbonic oxide, and this was equivalent to a disturbance in the relation this gas bore to carbonic acid, which he maintained must be preserved. Mr. Charles Cochrane, however, had erected furnaces of 43,000 cubic feet capacity, which he intended blowing with air at a temperature of $1,400^{\circ}$, so that shortly there would be additional experience to bring forward. In the meantime he predicted that no great economy of fuel would result from these extraordinary dimensions or high temperature of blast. This assertion, based at first on theoretical grounds, had recently been confirmed at the Consett Ironworks, where two blast furnaces had been erected blown with superheated air. In one furnace of moderate dimensions, but perhaps too small for the amount of work it was performing, the results were fair with blast at about $1,200^{\circ}$; but even here the proportion of carbonic acid was below that due to perfect working. Hitherto, he might observe, all temperatures given by experimenters were probably too high; but recently Mr. Siemens had placed in their hands a pyrometer by which the temperature of the gases or blast could be more accurately observed, by ascertaining the amount of resistance caused by temperature to an electric current passing through a platinum wire; the previous errors, however, being common to all observations, were not very material. The Consett Iron Company, not satisfied with what they had accomplished, built

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a second furnace one-third larger, along with heating stoves capable of raising the temperature still higher than in that just mentioned, but the result was not what was expected, and appeared paradoxical to the manager, who stated that he was burning more coke, with blast of a higher temperature, at this furnace than at the other, and that the gases were leaving both equally cool, and he inquired what came of the heat? This paradox was explained by the laboratory, which proved that there was less heat generated in the second furnace than in the first, owing to there being less carbonic acid in the gases. He recommended a reduction in the temperature of the blast, and according to the last account this reduction of the temperature of the blast was being attended by some reduction in the quantity of coke required to produce a ton of iron.

The ASTRONOMER ROYAL remarked that the Paper gave the present stand-point in the history of the practical science of the reduction of iron ores; but he thought it might be interesting if he endeavoured to supply a more distant stand-point of the state of things at a former period. In the year 1824 he examined the various operations in the Staffordshire iron district; his head-quarters were at Birmingham, and he made excursions to Dudley and other places. He went into pits in the Staffordshire Main, and saw a good deal of the district generally. The simple results which he wished to communicate were these. At the time he was speaking of, it was accepted as a rule, established by experience, that to produce 1 ton of pig-iron 4 tons of coal were required, and to convert that quantity of pig-iron into malleable iron 4 tons of coal were also required. Those two numbers might be accepted as facts established at that time, and which might serve as a stand-point in the history of this practical science; also that the blast furnaces such as were used in those days turned out about 50 tons of iron per week.

The next opportunity he had of visiting the iron districts was in the year 1837, when he went into South Wales and saw what was done at the Merthyr and the Dowlais Works. This was then the general condition of things. The iron was for the most part puddled and reduced to a malleable state. The consumption of coal at Dowlais was considerably over 1,000 tons per day, or about 9,000 tons per week. Of that he was assured by Sir John Guest. There were then in blast at Dowlais, he was pretty sure, eighteen furnaces. The blast furnaces were scarcely bigger than those in 1824, and the hot blast was not introduced to any great extent. It was known at that time, for he recollected that there

were some questions at issue about patent rights, but he did not see it in operation at Merthyr or Dowlais, so that the quantity of coal he had stated might be taken as representing the amount of fuel consumed in working these furnaces with the cold blast, viz. about 500 tons per week for each.

Making all allowances for the amount of coal consumed in refining and puddling, which applied to the greater part of that iron, and also for steam power for rolling, it would seem that the consumption was not much, if at all, reduced; and it appeared to him that the gain which had been made in regard to the saving of coal and the increased yield of the furnaces had taken place almost entirely since that time. His impression was, too, that no great improvement in the yield of the furnaces had been made between 1824 and 1837.

It was considered a point of some importance, introduced by Sir J. Guest, that after the operation of smelting he did not run the iron into pigs, but that he ran it in a hot state into the refinery, and in that way saved one heating. The causes of the great improvements that had taken place should be separated as far as possible, and therefore he was glad to see on one of the tables accompanying the Paper a clear account of how much was due to the temperature of the hot blast; but how far the general improvement might be due to other causes he could not say. Further information on this point was desirable. Both the size and the form of the furnace required careful consideration. Furnaces at the present time were less conical and more cylindrical than formerly. He would only add that it would be matter of great value if a competent ironmaster would undertake the history of the operations of refining and puddling. He was not himself aware what change had been made in them till the Bessemer period; a good account of those operations would complete the history of the manufacture of iron in England.

Mr. BRAMWELL observed, that he understood Mr. Bell to make what seemed to him the extraordinary statement, that the increase of the temperature of the blast did not increase the intensity of the heat in the zone of fusion, though it might increase the quantity of heat. Now, as a test, compare Mr. Bell's views with the theory and facts of the Siemens regenerative furnace, where the incoming gases and air were heated by the outgoing gases, so that each time they took up a higher temperature, and developed their heat upon the higher platform. In these furnaces, undoubtedly, the heating of the air and of the gas added to the intensity of the heat produced by combustion; and it seemed to him that this must be

equally true in the case of the heated blast of blast furnaces, and that the theory of Mr. Bell must be incorrect.

Again, he understood Mr. Bell to say that the gases could not be removed from the blast furnace at a low temperature, because the carbonic oxide in deoxidizing the iron ore became carbonic acid, and that would evolve the heat due to the difference between the heat of combustion of carbon into carbonic oxide and into carbonic acid; and as that deoxidization took place in the upper part of the furnace, there must inevitably be an increase of temperature in the gases just as they were about to leave the furnace. He doubted the correctness of this view in respect to the development of heat by the deoxidizing of the iron ore. If carbonic oxide were changed into carbonic acid by combination with the oxygen of the atmosphere, or with other oxygen in a gaseous state, then heat would be no doubt evolved; but if the change from carbonic oxide to carbonic acid were made by combination with oxygen, which had to be obtained by the resolution of another combination in which it existed in a solid state, as in the case of the ore of iron where the oxygen existed in combination with the iron; then he thought the result of the process would not produce heat, as that evolved by the conversion of the carbonic oxide into carbonic acid would not be of sufficient intensity to deoxidize the iron ore.

Although he did not feel warranted in supporting this view from his chemical knowledge, he thought he might do so from observation of some experiments, or rather actual workings, in iron which he had investigated several years ago. It was probably twenty years since the well-known French chemist Chenot devised a means of producing steel by a process which he called '*cémentation à froid*,' of which he would give a brief description, as perhaps the process was not generally known. In this process pure ore of iron—as hematite—was put into an air-tight chamber of the nature of a kiln, along with charcoal, or other matter containing pure carbon, and exposed to the action of external heat. Under the influence of the heat the carbonaceous matter united with the oxygen of the ore, and reduced the ore to what was called sponge of iron—a material which could be cut with a knife, being in fact iron in minute particles, held together by its mixture with earthy matter. This sponge of iron was then passed under edge-runners, or other mechanical appliances, and was crushed into powder. Part of this powder was mixed with resin, or fish-oil, or some other pure source of carbon. There were then mingled together part of the plain crushed ore and part of that which had been mixed

with the resin or fish-oil, the proportion of the latter being greater as the steel was required to be more hard. The preparation was next compressed in moulds of the size of a bottle-cork, and the compressed material was put into an ordinary steel smelting crucible. Some of the very best steel he ever saw was thus made. He brought this instance forward to show that when carbonic oxide was converted into carbonic acid by union with oxygen previously in a solid state, it had not the power of deoxidizing the iron ore, and producing a balance of heat in the operation, although carbonic acid was formed; but that the application of external heat was required to enable the operation to commence, and that after that operation was begun there was not sufficient heat evolved to keep it going without the continuance of external heat. This indicated that the conversion of the oxygen from the solid condition, in which it was when combined with the iron, into a gaseous condition, required such an amount of heat, that all which was produced by carbonic oxide combustion was absorbed by the deoxidizing operation.

With these remarks, he would now pass to a non-chemical reason why it was impossible, by infinitely increasing the size of blast furnaces, to infinitely decrease the heat of the outgoing gases, and with this decrease to lessen the consumption of fuel.

The question to be considered was, whether by continuing to increase the height of the blast furnace the outgoing gases could not be caused to give up more of their heat to the incoming materials, and thus to economize fuel. It was argued, by those who advocated with this view the further heightening of furnaces, that as benefit had already resulted from the furnaces being made taller than before, there might be still greater advantage from continuing to increase the height. He thought those who thus argued overlooked the fact, that for one body to give up its excess of heat to another there was required not only a sufficient time during which the two bodies should be in contact, but also a sufficient capacity in the heat-receiving body to absorb the excess of heat that the other body had to give out. Now in blast furnaces, as originally constructed, there was not sufficient time for the transmission of the heat; but by heightening the furnace the time had been increased, and the gases had parted with more of their heat, and economy had been effected. But it did not follow of necessity that an extension of time must enable the gases to part with a further portion of their heat; for, as he before said, that would depend upon whether or not the capacity for receiving heat in the material which was introduced, in a given time, was sufficient

to absorb the heat stored in the gases which went out in the same time. This seemed to him a very simple proposition; but in order to make himself clear, he had prepared some diagrams (Plate 18, Figs. 1—5), by the aid of which he hoped to make himself intelligible.

For the sake of illustration, he would assume that the problem was how to take out of a quantity of flowing hot water the whole of its heat above the temperature of a quantity of flowing cold water, and to transmit the heat to that cold water. This might be looked upon as a mere illustration, or as a case in actual practice, viz., where the continuous surface blow-off of a marine boiler was made to give up to the incoming feed-water the whole of its heat above the temperature of that water.

Let Fig. 1 represent a jacketed pipe, through the interior of which there should pass upwards in a minute 100 parts of water at a temperature of 200° , containing a total of 20,000 units of heat, and let there pass through the jacket round this pipe, and in a downward direction, in the same time, 100 parts of water at a temperature of 60° , containing 6,000 units of heat, and let the area and length of the pipe and the jacket be such that it would take two minutes for any particle of water to traverse the pipe and for any particle to traverse the jacket; and let it be assumed that the two minutes represented the exact time required for the transference of the heat. Then it would be seen that the complete transference would take place, the hot water would have parted with 14,000 units of heat to the cold water, so that the cold water would now have become as hot as the hot water was, and the hot water would have become as cold as the cold water was.

Next assume, as in Fig. 2, the same condition of things, with the exception that the pipe and the jacket being only half the length they were in Fig. 1, the time during which the two fluids would be passing through the apparatus would be one minute instead of two, such time could only suffice for the transference of 7,000 units of heat, and therefore the temperature of the outgoing water would only be cooled down to 130° , and the incoming water would only be raised to 130° . This would be an instance of want of effect due to want of time, as in the very short blast furnaces, and the defect could be cured by simply increasing the height of the apparatus to that shown in Fig. 1, as it was cured in a blast furnace by increasing its height, because there was sufficient quantity of cold water to take up the heat of the hot water if due time were given.

But assume, Fig. 3, that the quantity of incoming cold water

be reduced so that 80 parts only came in during a minute instead of 100 ; the effect would still be the same as in Fig. 2 as regarded the outgoing water ; viz., it would leave at a temperature of 130° , having parted with 7,000 units of heat ; but the effect upon the incoming water would be different ; for although it could only have absorbed the 7,000 units of heat, yet, being less in quantity than in Fig. 2, its temperature would be raised to $147\frac{1}{2}^{\circ}$, instead of to 130° , in the act of taking out with it 7,000 units of heat. Manifestly so long as the outgoing water was not raised to the same temperature as that of the incoming water, a better effect could be obtained by giving more time.

Let the apparatus be constructed as in Fig. 4, of such a height as to give one minute thirty-six seconds for the flow of the water : this time would be sufficient, at the rate of 7,000 units for one minute, for the transference of 11,200 units of heat ; and 11,200 units of heat added to the 4,800 units representing the incoming 80 parts of water at 60° would raise those 80 parts to 200° , thus making them as hot as the incoming water ; but that water would escape, after a deprivation of 11,200 units, with 8,800 units in it, representing 88° , or 28° more than that of the incoming water. A person observing this difference of 28° , and knowing that the difference of 70° in Fig. 3 had been lowered to the 28° of Fig. 4 by simply lengthening the apparatus, and thus increasing the time of contact, might imagine that he could get rid of the difference of 28° by a still further increase in the length of apparatus and corresponding time of contact, and he might therefore make it like Fig. 5, which was the same as Fig. 1, and gave two minutes of contact ; but if he did this he would obtain no advantage whatever over the minute and thirty-six seconds of contact of Fig. 4, because that time of contact was sufficient to enable the 80 parts of incoming cold water to attain the same heat, viz., 200° , as the heat of the outgoing hot water ; and as manifestly the cold water could never become hotter than the hot water that heated it, all increase in the length of the apparatus, so long as the incoming cold water was restricted to 80 parts in a minute, must be useless, and the only way of effecting a further reduction in the temperature of the outgoing hot water must be by increasing the number of parts of the incoming cold water, and if it were desired to obtain absolute uniformity of temperature in the outgoing hot and the incoming cold water, the quantity of the incoming water must be raised to at least 100 parts as in Fig. 1.

From the difficulty of making the proposition of the difference between time and quantity in this blast furnace problem clear,

many persons interested in blast furnaces were directing their principal attention to increasing the height of those furnaces, knowing that such increase had been attended up to a certain point by the lowering of the temperature of the outgoing gases, and by a corresponding saving of fuel. But he believed they were being misled, because they did not bear in mind the fact, that the incoming materials in a given time remained a constant quantity in relation to that of the outgoing gases, so that when a height was attained sufficient to give time for the incoming material to take up from the outgoing gases the whole of the heat which that incoming material was competent to absorb, any increase in time must be entirely useless.

Mr. DAVID FORBES said that he was now, and had been since 1847, professionally and practically connected with iron smelting. He might also state, that in 1853 Mr. Samuelson consulted him before erecting his first blast furnaces in the Cleveland district. The Author had fully succeeded in describing and embodying in the Paper the main features of a first-class blast furnace plant of the present day. If the furnaces described in the Paper were compared with those which obtained ten years ago, it would be seen that independent of more perfect mechanical arrangements, the two main points of difference consisted in the tendency to augment the temperature of the blast, and in the tendency to increase the internal capacity of the furnace. On both these points he differed from Mr. Bell, inasmuch as he believed these were both steps in the right direction. He thought Mr. Bramwell had forgotten in his calculations the effect of the chemical action that took place in a blast furnace. It was not a simple case where air was sent in and out, but one where air was sent in, but compound gases sent out, owing to the chemical action brought into play; so that purely mechanical considerations were, to a great extent, out of the calculation. He believed, and he knew several eminent foreign metallurgists agreed with him, that the effect of superheated blast was not merely to increase the quantity of heat, but to increase the intensity of the heat at the focus of fusion. The object was not to increase the temperature of the upper part of the furnace, but to make it more intense at that part where the actual melting took place, where the greatest amount of heat was required to bring the whole into perfect fusion. For this reason he considered the limit to heating blast was merely the practical limit, which could only be proved by experiment, as to whether the blast could be more cheaply heated outside the furnace than inside.

The next point was as to the dimensions of the furnace. It had

Fig. 1.

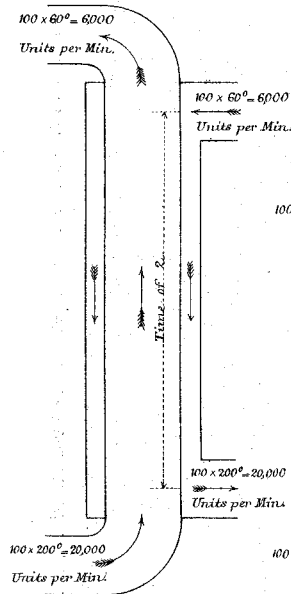


Fig. 2.

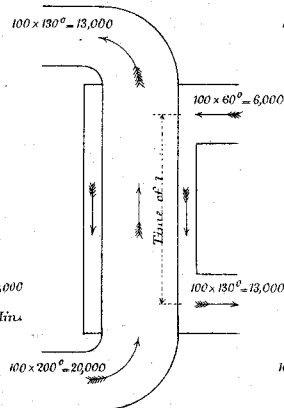


Fig. 3.

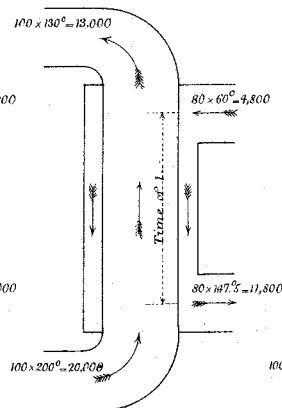


Fig. 4.

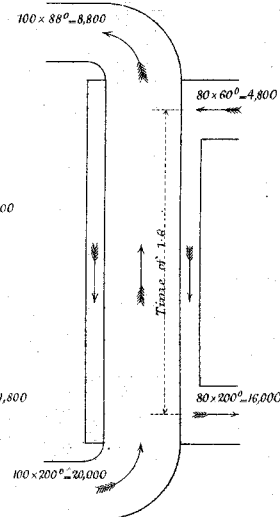
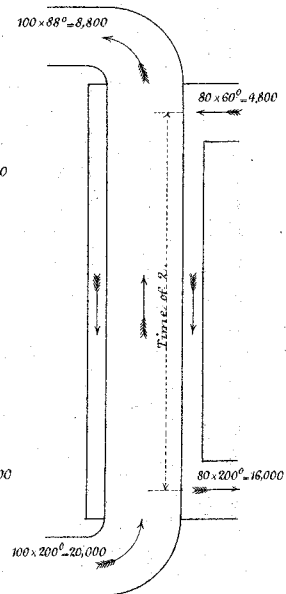


Fig. 5.



been stated as a proof that the proper dimensions for a furnace had been exceeded, that the larger furnaces did not produce the same increased yield of iron in the ratio of their increased capacity; that was to say, a furnace of 30,000 cubic feet internal capacity would not produce six times the amount of iron which a furnace with a capacity of 5,000 cubic feet would. Now he thought it would be quite as unreasonable to expect that, as to expect that the ironmaster who made 5s. per ton on his pig from small furnaces should make 30s. per ton on the yield from a furnace six times as large. Increased capacity was only one element in the calculation; but there were others which might altogether counteract its advantages, and which must be taken into account. The capacity of the furnaces of the present day was mainly increased in one direction only. Later experiments made in the Raschette furnaces of Russia and Germany had shown that there was reason to believe that an increased production of iron would follow the adoption of a very different form of furnace. It would be seen that the greater amount of the increased capacity of the furnaces described by the Author was in the shaft of the furnace, and not in the real working part—the focus of the furnace. He thought, therefore, that when the subject was studied more carefully, far better results would hereafter be got from the large furnaces than at present. It took many years, and great numbers of experiments, before the proper dimensions of the parts of an ordinary small furnace were obtained so as to get the conditions of most economical working; whereas, at present, there were but few examples of large furnaces, and these were made without any further guidance than what experience had taught in the small ones. A more intimate acquaintance with the larger furnaces would, he thought, lead to better results.

Mr. C. W. SIEMENS observed that the information contained in the Paper and elicited by the discussion went far to show that, by carrying out a chemical operation on a large scale, the utmost results which theory indicated could be almost realised. Two years ago, when the question of large furnaces was first mooted, he made a calculation of the work done in a blast furnace, and of the heat that was developed in that blast furnace. By taking into account, on the one hand, the ore to be heated and reduced, the pig to be melted, the slag to be melted, and the heat and composition of the escaping gases at the temperature at which they were carried away; and, on the other hand, the temperature of the blast and the heat due to its combustion with coke, in producing the gaseous mixture escaping from the top of the blast furnace, he found that 20 cwt. of ordinary coke should suffice

theoretically to smelt a ton of pig iron from Cleveland ironstone containing 40 per cent. of metallic iron, and that 21 cwt. per ton of iron was a result that might be practically realised. Assuming the temperature of the blast at 1000° , the best results at that time were about 26 cwt., but it struck him that Mr. Samuelson had obtained the calculated minimum result almost completely; his consumption being $20\frac{1}{2}$ cwt., and the temperature of blast about 1100° . Mr. Bell arrived at nearly the same figures by another mode of reasoning, and had followed up this subject very minutely, by examining into the particular stages of the chemical operation carried on in the blast furnace, deducing very valuable data for the metallurgist.

Nevertheless, he would not recommend others to follow Mr. Samuelson under all circumstances in the construction of a blast furnace. He was at that moment constructing some blast furnaces of very different dimensions; they would only have a cubical capacity of 10,000 feet instead of 30,000 feet; for he believed it would be inexpedient in this case to adopt such extreme dimensions. It would require the peculiar conditions of the Cleveland iron district in order to do so with advantage; namely, an excellent hard coke and an ironstone rather strong and comparatively poor. For richer ore and tender coke he believed the size of blast furnace such as Mr. Samuelson had described would be inapplicable.

With regard to the effect of extreme capacity, he agreed with Mr. Bramwell's views, while at the same time he admitted that Mr. Forbes' remarks were perfectly correct. But Mr. Bramwell spoke of the mechanical absorption of heat—the regenerative action within the blast furnace;—on the other hand Mr. Forbes spoke of the chemical action between the gases of the blast furnace and the ores; and both these actions were subject very much to the same laws. To get from the fuel a maximum of effect in a steam boiler plenty of heating surface was given, as in the case of the Cornish boilers; and it was reasonable to suppose that more surface would give superior results, simply because the difference of temperature between the heat-transmitting current and the heat-receiving surfaces was brought down to a minimum. Where a current of gas flamed rapidly through a mass of solid matter, there must be a considerable difference of temperature between each particle of the fluid and the solid receiving that heat: whereas, if the mass was greater, and the current relatively slower, there must be a more perfect adjustment between the two temperatures, giving rise to a more economical result. In the same way the

chemical action would be more perfect if a gas containing a certain proportion of carbonic oxide (mixed with carbonic acid and nitrogen) had plenty of time to communicate its carbon to the piece of ore which was ready to take it in yielding up its oxygen. If the quantity of material present were infinite a perfect adjustment between these conditions would arise, depending upon the temperature and the proportional amount of carbonic oxide present; but such a condition of equilibrium could never be obtained. It was therefore necessary in practice to consider how much would suffice for the purpose, because other considerations entered into the subject. If a blast furnace was very large in proportion to the quantity of work that was to be performed in it, a greater economy would be attained by a relative increase of first cost. This was the case in Mr. Samuelson's blast furnaces, where 30,000 cubic feet capacity produced only 500 tons of pig metal per week; whereas a furnace with a capacity of 10,000 or 11,000 cubic feet would produce 300 tons. It was therefore clear that these large furnaces produced approximately only about one-half the amount of work per cubic foot of capacity that the smaller furnace did. The question arose how far was it judicious to expend more capital to obtain additional saving, if other means might be resorted to of arriving at the same, or even a greater increase of economy. This point had not been touched upon in the course of the discussion. Mr. Bramwell had shown that where there was a greater outgoing current than a heat-absorbing material to meet it, an unstable condition or waste of heat must ensue; and Mr. Forbes had argued in the same way on chemical grounds. In the blast furnace the object was to waste as little heat as possible, and to bring out the gases at the top as cool and as thoroughly deprived of their chemical or reducing power as was practicable. Both these objects were obtained by diminishing the relative quantity of gases. If, for instance, the relative amount of gases for a given amount of incoming material could be reduced by one-half, and if the chemical conditions were the same, it was clear that the smaller quantity would be more thoroughly deprived of its heat; but the smaller quantity would have had time to divest itself more thoroughly of its chemical action also: there would be a larger relative amount of carbonic acid gas produced, and the coke would be consumed in the furnace to greater advantage. This could be accomplished by a blast of high temperature; and in this respect his opinion differed very much from the theory of Mr. Bell, to the effect that there was a theoretical as well as a practical limit

to the beneficial heat of hot blast. He believed, on the contrary, the blast could not be made too hot for economical purposes, and that real progress in iron smelting must henceforth be looked for chiefly in that direction.

Mr. E. WILLIAMS said he had been unable with blast furnaces as now constructed to obtain any appreciable advantage by increasing the heat of the blast above 900° or 1000° ; and in this respect his practical experience confirmed the belief expressed by Mr. Bell. At a large ironworks in the North of England, it had been proved by experience that an excessive temperature of blast was prejudicial. At one of the furnaces of the Consett Iron Company the blast was heated to 1400° or 1500° , and there was a greater expenditure of fuel per ton of pig iron made than at an adjoining furnace where the heat was 1200° . This, he was convinced, was the case; though, personally, he had no experience of blast of a temperature much beyond 1000° .

The shape of the Middlesbrough furnaces had been objected to. On this point he would only say, that, if Mr. Forbes would give a sketch of a better furnace, the iron smelters of that district, who were always anxious to improve their furnaces, would be happy to give it a trial, provided it was not too costly, and it was thought there was a fair chance of success.

With regard to size of furnace, economy of fuel was due to increased capacity in so far as the height was increased, but widening the furnace would not produce economy of fuel, though it produced larger makes of iron, and, therefore, a larger factor by which to divide the general charges. No economy of coke resulted from increasing the capacity of a furnace in any direction other than in its height. He had under his management at Middlesbrough and elsewhere furnaces of different heights, with an internal capacity varying from 11,000 to 30,000 cubic feet, and he found that the taller furnaces, 95 feet high, used least coke; and of these, two furnaces with the bosh 16 feet in diameter used rather less per ton of pig iron made than furnaces of the same height with wider boshes. This difference, he thought, was explained by the fact that the narrow-bosh furnaces had a thicker lining of brickwork than the furnaces of larger internal dimensions. The height should be fixed so that the weight of the column of materials should be rather below the crushing point of the most friable material charged. For finely-divided ore, that was crushed already, the very tall furnaces would not answer; but where the furnace was charged with material in moderately-sized lumps, sufficiently large to allow the blast to pass up through the interstices (and

when it ceased to do that the furnace would not work at all), the higher the furnace the better, so long as the crushing point was not reached. As a fact, the richer ores of this country were in a fine state of division, but it was the mechanical condition of the ore which made it unsuitable for tall furnaces, not the richness.

Mr. C. W. SIEMENS observed that tall furnaces were not suitable for rich iron ore; for if it was finely divided, such as hæmatite, the minuteness of the subdivision was prejudicial to the free operation of the blast; but if it was in large lumps the ore would be liable to fuse before it was reduced, and 'gag' the furnace.

Mr. JOHN LANCASTER, M.P., remarked that he was much in the same position as Mr. Williams, in so far as what little knowledge he had of the subject had been acquired by practical experience; he, too, frequently had to change his opinions as to certain results obtained. In a commercial point of view there was much to admire in the representative furnaces of 1871; but he thought it would be a mistake to consider them as representing the various districts of iron manufacture. They were, no doubt, adapted for particular ores and fuel, both of which were of a very special character. The fuel of the district in which these furnaces were situated was perhaps mechanically the hardest fuel in Great Britain, and hence would withstand greater pressure and a higher furnace than any other. In 1852 he erected some furnaces in North Staffordshire 60 feet high, the old standard being 48 feet, but that height was more than the mechanical structure of the fuel could endure, and he was obliged to mix it with imported fuel to support the height of the furnace. He would therefore caution iron smelters, in districts where the fuel was not mechanically strong, against resorting to such lofty furnaces as these. Under any circumstances he questioned whether, taking the cost of the structure into account and looking at the quantity of iron produced, it was wise to construct furnaces of such large dimensions. He had himself erected furnaces 80 feet high with a 23-feet bosh, but he had reverted to what he considered the more economical height of 65 feet for a mixture of hæmatite and other ores. He therefore thought that furnaces of half the capacity of those described in the Paper would yield a better commercial result upon the capital expended.

Mr. J. KITSON inquired whether the increase of temperature in the furnace had not a tendency to make the iron tender? He had himself a predilection for cold-blast iron.

Mr. I. LOWTHIAN BELL said he still retained his opinions respecting the cause of a certain temperature being maintained in

the escaping gases of a blast furnace; he also differed entirely from Mr. Siemens and Mr. Forbes when they said that there was no limit to the heat of the blast producing a saving of fuel in the smelting of iron. In answer to Mr. Kitson's inquiry, a record had been kept for some years at the Clarence Works of the breaking weights of the metal produced at that establishment. No appreciable difference had been observed since the blast of a higher temperature than formerly had been used, but what the effect might be when the air was raised to 1400° he was unable to say.

Mr. E. A. COWPER observed, on the subject of the temperature of the blast and the advantage that was found to result from raising it higher than usual, that he might state from an experience extending over more than twelve years, that it was found in raising the temperature every 100° so much was gained in the saving of coke, and, with Cleveland ironstone, the advantage continued with every rise in temperature up to the present, as it was natural to suppose it would, and a temperature of considerably upwards of 1400° had been maintained for months together. Good results had been obtained under very high temperatures of blast when hematite was mixed with the Cleveland ironstone. Ironmasters were becoming much more impressed with the saving that could be effected, by greatly increasing the temperature of the blast, and many both at home and abroad were adopting the regenerative high temperature hot-blast stove. With a furnace of 20,000 cubic feet capacity a saving of 4 cwt. of coke per ton of iron made had been obtained by simply increasing the temperature of the blast from 1000° to 1442° .¹ He had received the exact returns of the consumption of coke per ton of iron made at Consett, with different temperatures of blast, and found the saving was as follows:—No. IV. furnace, a saving of 1 cwt. 2 qrs. 27 lbs. of coke by raising the temperature of the blast from 1200° to 1400° , and with No. V. furnace a saving of 1 cwt. 3 qrs. 8 lbs. by raising the temperature of the blast from 1250° to 1500° . Mr. Samuelson had no doubt arrived, as nearly as possible, at the greatest economy of coke that could be obtained with the temperature of blast he used, and the materials he dealt with, but he had got it at the cost of 10,000 cubic feet of capacity in the furnace, the furnace having 30,000 cubic feet capacity in place of 20,000 cubic feet, which was generally considered large; and that, he apprehended, was obtained at a

¹ *Vide* Minutes of Proceedings Inst. C.E., vol. xxx., p. 319.

cost of probably £5,000, besides the additional working expenses consequent on the larger scale of the works. He would merely add, that a considerable saving could be obtained with a smaller outlay, if the temperature of the blast, instead of the size of the furnace, were increased.

Mr. B. SAMUELSON, in reply upon the discussion, said that Mr. Bell had stated his belief that the intensity of the heat in the zone of fusion was not augmented by increasing the heat of the blast; whereas Mr. Forbes and Mr. Siemens appeared to be of opinion that it was increased. Those gentlemen were able to form an opinion as chemists whether that was the case or not, and they differed; but he, not being a chemist, had no right to make any statement on the subject because he did not know, nor did he think that any practical iron smelter had ascertained the heat in the zone of fusion of a blast furnace. He must apologise for having stated, definitely as a fact, what he did not know. Mr. Bell seemed to think that he was mistaken as to the quantity of fuel used in these new furnaces, and that he had failed to compare a furnace of large capacity in its best state with a furnace of small capacity in its best working condition. Now he was satisfied he had made a fair comparison; but inasmuch as he might have been deceived, he had taken pains to verify the statement, and he found, from an examination of his books, that the statement of the saving effected was quite within the mark. It could only be possible that an error had occurred in this statement, either if somebody had charged for a large amount of coke that had not been received at the small furnaces, or that some one had made him a present of a large quantity of coke for the large ones which had not been paid for. In reference to the one contingency he had the fullest confidence in his agents; in reference to the other contingency he should be sorry to think so ill of the management of coke and coal owners of South Durham as to suppose that it could arise. The figures he had given had been carefully verified; they had been taken under fair and proper circumstances for comparison; and were the result, not of a week or a fortnight's working, but upon a statement of accounts of fuel received and paid for over successive periods of six months.

The next question was—What risk had he run by increasing the capacity of these blast furnaces very much beyond those which he had previously constructed? In two ways he did not run any great risk. He did not jump suddenly from a furnace with a capacity of 16,000 cubic feet to a furnace of 30,000 cubic feet, but he watched what had been going on in his neighbourhood; and he

found, somewhat in contradiction of the experience of Mr. Bell, that economy had, in every case, been derived from increased capacity.

But there was another view in which he satisfied himself that he was not running any great risk; he referred to the opinion of Mr. Cowper, that increasing the capacity from 15,000 cubic feet to 30,000 cubic feet involved an additional cost of about £5,000 in the construction of the furnace. Now, the actual total cost of the large blast furnace *per se*, as shown in the abstract, was £5,000; consequently it was impossible there could be an increase of £5,000. No doubt there was an increase perhaps of £1,500 or £2,000; but there were savings in other ways. There was saving in the simplification of parts. If in one vessel 500 tons of iron were made instead of 300 tons of iron, there was a saving on the ton of iron in the tuyere arrangements and communications from the heating stoves and in other details; so that, in some measure, the increase of cost in the actual dimensions of furnace was partly made up in other ways.

He was now erecting a third furnace, as nearly as possible of the same dimensions, for less than one-third the cost for two, say for £14,000, so that for £70,000 he should have a plant of three furnaces; for he had already sufficient engine power, sufficient kiln power, and the other arrangements provided for two were sufficient for the three. He had observed this, and it had become a rule in the Cleveland district, that the cost of a good blast furnace plant, within the limits of proper economy, was £1 per ton per annum of the production. If the plant cost £40,000 the produce was about 40,000 tons of iron; if the produce was 70,000 tons of iron the expenditure would be £70,000; and this would be carried out in the present instance.

Apart from the chemical points raised by Mr. Bell, there were many mechanical considerations with reference to blast furnace plant which had been overlooked in the discussion. He had stated in the Paper, that with a pressure of $4\frac{1}{2}$ lbs. per square inch at the blowing cylinder he only obtained a pressure of blast of $3\frac{3}{4}$ lbs. at the tuyere. That was a bad result, and it was a point which he hoped would have been examined, so that a better result than he had stated might be obtained in future. There were allowances to be made for expansion and friction, but the loss of pressure was too great and ought to be diminished.

In reference to the percentage of iron contained in the ore, when describing the ironstone of Cleveland, he said that, in a calcined state, it contained from 37 to 40 per cent. of iron; and that state-

ment was fair as a general description of the ironstone of the district; but he quite agreed with Mr. Bell that, on an average, less than 40 per cent. of iron was obtained. The minimum yield of between 18 and 19 cwt. of coke, which Mr. Bell considered impossible, applied to one week only, and might be attributable to the weighing up of some of the iron produced having been carried forward from one week to the next. This was, of course, corrected in the averages. As to these latter, Mr. Siemens was of opinion that he had arrived at a result which very nearly corresponded with the theoretical result; while Mr. Bell thought he had not obtained any such results, because being the best obtainable theoretical result he could not arrive at it in practice. But what he contended was, that nobody knew what was the most perfect theoretical result. The number of units of heat to produce a ton of iron had been estimated, it was true, in the laboratory; but he did not believe there was one chemist who had ascertained for himself approximately in the laboratory the theoretical quantity of heat required for all the operations of the blast furnace. Tables had been constructed combining all these as ascertained; some by one authority, others by another, and the sum of them had been taken as correct. He believed it would be a marvellous thing that the theoretical result should be correct; for he was not aware that in any other manufacturing or chemical process theoretical perfection had been attained within 15 or 25 per cent.; and he was fully persuaded that, in the present instance, there was a margin for a further saving.

That there was a further margin for saving he could prove in this way:—He had taken off the gases from the top of the large blast furnace at intervals of half an hour, the interval being the time between the introduction of one charge and the introduction of another. He found that between those two periods there was a difference of more than 300° in the temperature of the gases; for whereas immediately after a charge of cold materials had been introduced the temperature was about 300° , at the end of half an hour the temperature was upwards of 600° . He thought it was desirable to approximate more closely to an average temperature of the materials than this. There was no theoretical reason why there should be a rise of temperature of 300° between those two periods. If the volume of materials charged into the furnace in a given time bore a smaller ratio to the total quantity of material in the upper part of the furnace, the reduction of temperature by the introduction of the cold material would be smaller, or, what meant the same thing, the actual temperature, before the introduction of the material, would not be so high.

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Capacity as distinguished from mere increase of height had been referred to by Mr. Williams; but what followed an increase of height of furnace? Why the mass of materials which was to absorb the waste heat of the gases was also increased:—in fact, the regenerative principle of Mr. Siemens was carried out. But the same occurred if the diameter was increased; and in practice increase of height had this disadvantage—it had a tendency to crush the fuel which was charged into the furnace, and thereby to increase the difficulty of the passage of the air through it. These were distinct considerations—the one practical as to facility of smelting, the other as to economy of fuel.

He had only to add, in confirmation of what fell from Mr. Bramwell, that there was an element in the economy of fuel in the time allowed; that whilst it took between sixty and seventy hours for a charge to descend in the larger furnace, in the smaller one this took place in from thirty to forty hours.

May 9, 1871.

CHARLES B. VIGNOLES, F.R.S., President,
in the Chair.

THE discussion upon the Paper No. 1,299—"Description of Two Blast Furnaces, &c.," by Mr. B. SAMUELSON, was continued throughout the evening.
