Mr. W. H. BARLOW said, having determined to construct the two bridges mentioned in the Paper of cast-iron, and having observed, in many large works of that class, that there appeared to be considerable waste of metal, the strain at the centre frequently not exceeding 2 tons per square inch of sectional area, he conceived that material could be economised by a better arrangement. These experiments were instituted to prove, practically, how this might be accomplished. In these trials, it was evident that the iron yielded from the effects of distortion, and not from actual pressure. The results had been rather disappointing. With a load of 9 tons, or 10 tons per square inch of section, uniformly distributed over the whole surface of the structure, safety might be insured; but when the weight was unevenly distributed a larger margin was required.

Mr. FAIRBAIRN gathered from the description given in the Paper, and from the drawings, that the form of structure adopted, at Standish, combined the properties of the girder and of the arch. The span was 83 feet 4 inches, and the castings of each half-arch were 43 feet 2 inches in length, the joint being in the centre. A great part of the strain would be borne by the abutments, and if they were perfectly secure, he believed that four half-arches would carry a greater load than two plain horizontal girders; although he was not quite sure that the arch was so good in other respects, for a straight girder was a very simple kind of bridge. He believed, however, as a general rule, that wrought-iron was a preferable material for large railway-bridges, and that it was both cheaper and safer.

In the preparation of cast-iron, much depended upon the time the metal was kept in fusion. It had been proved by experiments, that some kinds of iron, when melted in an air-furnace, or cupola, and kept in a state of fusion from two to three hours, would be increased in tenacity from one-third to one-half. It ought also to be known, that if the metal was allowed to cool gradually, a more perfect crystallization would be the result.

Mr. RENNIE thought, although the experiments were valuable in themselves, it would be difficult to draw conclusions from them. It was clear, that lateral oscillation having been caused by the mode in which the load was applied on the outside, both regularly and irregularly, no satisfactory result could be deduced from these trials. The spandrils might have been omitted in the model arches, because the experiments related to the arch and not to the spandrils. He should have been glad, if the Author had been in a condition to show, what a straight girder of equal depth, span, and thickness, would have borne, in order that a comparison might be made between the arch form and the girder. He had made some experiments on that subject in 1847, when determining the ultimate strength of bars of Blaenavon cast-iron, of different depths and thickness, the sectional area, the length, the distance between the supports, the weight, and the quality of the iron being in all cases the same. His object was to show the amount of work that could be done by a given weight of iron, and the best mode of disposing the metal. The bars were 1 square inch in section, the length was 4 feet 6 inches, the span was 4 feet, and the weight of each between 16 lbs. and 17 lbs. He found that,

a rectangular bar,	1 inch square,		bore an average load of	511 lbs.	at the centre.
„	2 inches deep and	$\frac{1}{2}$-inch thick	„	1,121	„
„	3 „	$\frac{1}{3}$ „	„	1,568	„
„	4 „	$\frac{1}{4}$ „	„	2,352	„

It thus appeared, that a bar 4 inches deep by $\frac{1}{4}$ of an inch thick was more than four and a half times as strong as a bar of precisely the same weight and length, but 1 inch square. In the same way he had ascertained, that a bar of similar length, but which, instead of being parallel, was tapered towards the ends on the upper side, the curve being that of a parabola, and the weight being thus reduced to 11 lbs., would

when 2 inches deep	at the centre, and	$\frac{1}{2}$-inch thick,	bear	953 lbs.
„ 3 „	„	$\frac{1}{3}$ „		1,429 „
„ 4 „	„	$\frac{1}{4}$ „		2,262 „

If the upper side was elliptical in form, the weight was increased to 12 lbs., and there was a slight addition in the strength; thus,

when 2 inches deep	at the centre, and	$\frac{1}{2}$-inch thick,	it would bear	950 lbs.
„ 3 „	„	$\frac{1}{3}$ „		1,450 „
„ 4 „	„	$\frac{1}{4}$ „		2,360 „

Being now desirous of arriving at the relative values of the arch and the girder, a bar 1 inch square was bent into the form of an arch, the versed sine of which was one-fifteenth of the span, when it was found that it bore a weight of 1,267 lbs., being nearly two and a half times as much as a horizontal bar of the same proportions and weight. When the versed sine was nearly one-eighth of the span, a similar bar bore a weight of 1,729 lbs., or nearly three and a half times as much as the straight bar. The suggestion having been made, that considerable increase of strength would be obtained by an admixture of wrought-iron with the cast-iron, as proposed by the late Mr. Morries Stirling, Mr. Rennie had continued the experiments in that direction, and he found that by an addition of 10 per cent. of wrought-iron the strength was increased $22\frac{1}{2}$ per cent.; with 20 per cent. of wrought-iron the strength was increased $31\frac{1}{2}$ per cent.; and with 30 per cent. of wrought-iron it was increased 60 per cent. That seemed to be the limit, or maximum, for with 40 per cent. of wrought-iron the strength began to decrease, and showed only an increase of 33 per cent. beyond the strength of cast-iron. Similar results were obtained with bars

2 inches, 3 inches, and 4 inches deep respectively. If the greater strength resulting from the arched form was added to the previous results, it would be found, that a bar 4 inches deep and $\frac{1}{4}$ of an inch thick would sustain 6,900 lbs., and with the alloys from 8,418 lbs. to 11,040 lbs., being an increase of 22 to 60 per cent. This was upwards of twenty-two times the strength of the original bar of 1 inch square (without alloy), when tried horizontally, and showed what might be effected by an improved form, and by the addition of an alloy of wrought-iron. The number of castings experimented upon in each case was three.

Mr. Allen Ransome had considerable experience in testing the strength of cast-iron, as in accordance with the requirements of certain contracts, which had extended over several years, his firm were required to cast bars, for the purpose of testing the strength of the iron used, and to record the breaking weight and the amount of deflection, upon each day's melting. The bars were 2 inches deep by 1 inch thick, and were supported, edgeways, upon bearings 3 feet apart. The breaking weights were applied at the centre. If the test bars broke under a less weight than 25 cwt., the castings made from such meltings were rejected. It was not difficult to make sure of the iron bearing this test; indeed, the daily records showed, that the bars bore a strain varying from 25 cwt. to 28 cwt., and in some cases even 33 cwt. But it was difficult to account for the variations exhibited. The bars were always cast simultaneously in pairs, and from the same metal, so that every condition, in the case of a pair of bars, was perfectly similar. The same metal was poured in at the same temperature, and the bars were cooled side by side alike. Yet it not unfrequently occurred, that of these twin bars one would bear a strain, before breaking, of 30 cwt., while the other would break at 25 cwt., without any appearance of difference being exhibited in the metal, or any other cause that could indicate a reason for this difference. Generally speaking, in testing a dozen bars, a variation in the results would be shown to the extent of 8 or 10 per cent. This uncertainty, as to the strength of cast-iron, was borne out by the results of the experiments detailed in the Paper. It induced him to agree entirely in the opinion, that unless a great excess of strength, over ordinary calculations of the breaking strain of cast iron, was insisted on, in all cases where there was risk of accident, and where, as on railways, such accidents involved serious consequences, wrought iron was by far the best material for bridges and girders. Mr. Ransome added that the experiments had been made with different kinds of iron; but although a better average result was obtained from the superior descriptions of iron, the same uncertainty existed in all cases.

Mr. Bramwell said, it would appear from the table, that there were instances in which cast-iron had failed under a pressure of 2·93 tons per square inch; he doubted, whether any iron was known, of so poor a quality as that would indicate. The fact was, the rails which tied the standards together having cambered, caused the arches to act as cantilevers, so that the fractures really arose from tension, and not from compression. Under these circumstances he was at a loss to know, what was the practical utility of the table, as it did not give the strain per square inch under which the arch failed.

Mr. CHAPPÉ explained, that none of the failures occurred from the crushing of the iron, but from distortion alone, which produced extension. These experiments were instituted, to ascertain what weight of metal it was necessary to put into the haunches, to bring the ultimate strain upon the arch. It was shown, in every case, that the arches failed in consequence of the haunches breaking from extension, before the cast iron had arrived at the ultimate crushing strain, proving that the weight of metal in the arch was greater than the weight of metal in the haunches would support. These experiments afforded data for calculating the strength of metal required in the haunches. The object of casting each half in one piece was to facilitate the erection. If cast in two, or three pieces, a considerable quantity of scaffolding would have been necessary.

Mr. HENRY MAUDSLAY thought these experiments ought not to have been brought before the Institution, as they were evidently imperfect and incomplete. The results recorded in the table, given in the Paper, did not convey a correct idea of what iron was capable of bearing, and if taken as the basis for calculations, must lead to error. In the second and following experiments, the arches were not supported on solid abutments, as would be the case in practice; but the springing-pieces were connected together by wrought-iron rails, after the manner of tie-bars, thus transforming the arch into a kind of girder. As the load was applied, the rail tie-bars would naturally be brought into tension; and he had no doubt, in these experiments they had been so elongated, as to cause an additional strain to be thrown on the cast-iron arch, and thus to vitiate the results.

In the course of the discussion, it had been said, that wrought-iron was mixed with cast-iron, to produce Stirling's metal, by stirring them together in a pot, or ladle. Some experiments at the works of Messrs. Maudslay, Sons, and Field, seemed to show, that the best method of mixing the metals was, to form a hollow in the sand, in the form of a short 'pig,' into which were laid strips or pieces of wrought-iron, cut up from boiler plate scrap, of about the bulk of an ordinary pig, or in different proportions according to the experiment. Cast-iron was then run in, and when cold a solid mass was formed. This was then placed in the furnace, and re-melted, when the amalgamation was found to be more complete, than if the metals had been simply stirred together; for wrought-iron could not be brought to the same state, by melting, as cast-iron, as the former was of a fibrous nature, whilst the latter was granulated.

Mr. BRAMWELL had adopted a similar plan, in making castings with Stirling's metal. The cast-iron was congealed around the wrought-iron, and he found, by the re-melting, the amalgamation became complete. He provided two cupolas, one for producing the ordinary cast-iron, and the other for receiving the mass of mixed metal, while still hot, to be re-melted.

Mr. R. STEPHENSON, M.P., said, the chief object of the experiments seemed to be, to ascertain the value of a cast-iron arch, as an arch simply. He contended that no advantage could be gained, by employing cast-iron, or even wrought-iron, in that form, for bridge purposes; and he thought the idea of so using either material had been long ago abandoned. The Paper might be valuable as containing a record of facts, but it appeared to him that the experiments were conducted upon exceedingly bad girders; for nothing could be worse than the mode of forming the arch. It was difficult to cast pieces of the size and of the shape shown, and in cooling, a fracture was almost certain to take place somewhere. Afterwards, when unequal strains were brought upon the arch, the direction and amount of any one strain could not be readily determined. If twenty experiments were made, upon the same description of girder, and under precisely similar conditions, he much doubted whether fracture would ever occur twice in the same place. Cast-iron arches must not be trusted as if they were arches of equilibrium. Nothing was more absurd than to suppose, that in an arch which was only one-twentieth part of the weight it was intended to carry, the vis inertia could come into play. Where a stone arch was twenty times the weight it had to carry, the forces could then be dealt with as if in equilibrium with each other; and all arches of stone might be treated mathematically in that way. It had been stated, that additional strength woud be obtained by making a certain bridge in the form of an arch, because it had been proved, experimentally, that a bar of iron bore a greater strain when bent into an arch form than when straight. No doubt the results had been accurately recorded, but al the contingent circumstances had not been given. For instance, no mention was made of the greatly-increased strength necessary to be given to the abutments, to render them immovable and capable of resisting the horizontal thrust, beyond what would be required with a flat girder, in which the weight was insistent. He was satisfied, that whatever economy of metal might result, from the arch form, would be more than compensated by the greater cost of the abutments.

Whilst admitting the increased strength of Stirling's metal over ordinary cast-iron, weight for weight, for girders, he yet thought, that the expense of the process of mixing malleable and cast-iron was such as to render cast-iron alone the preferable material. The cost per ton was so much greater in the one case than in the other, as to allow of a larger quantity of material being used in the latter, and yet equal strength be obtained at less expense.

Mr. LOCKE, M.P., President, said, no doubt there would be a certain advantage, in point of strength, in doubling the depth of a bar of iron, the sectional area remaining the same. It had also been urged, with truth, that the strength of a flat girder would be increased, by the addition of straight struts on the under side, and that in that way it might be made quite as effective as an arch. But it must be borne in mind, that it was not always convenient to use struts. It had been a question, whether the fractures of the spandrils arose from compression, or from extension. He believed it was due to the latter strain, for though it had been argued, that it was due to the lateral distortion, he did not think there was room for distortion to take place. The arched portion of the girder might represent the experiment, totally independent of the spandril, and if such a girder was tested, both straight and curved, he believed, the same result would ensue, as mentioned by Mr. Rennie. In confirmation of the statement, that a girder of the form experimented upon was bad, because of the unequal contraction in casting, Mr. Locke remarked, that he had known girders to be brought upon the ground with such varying fractures in the spandril pieces, connecting the upper table with the arch, as to render it a question, whether they should be put up, or returned upon the hands of the contractor. He doubted the value of experiments to produce fractures, which so frequently occurred in the ordinary mode of manufacture. In some experiments that were made at Dublin, upon the ribs for the roof of the Lime-Street station, Liverpool, the ribs twisted, distorted, and failed, not from any defect in their section, which was sufficient to bear a much greater load than was actually applied, but from the want of the necessary lateral supports. When the roof was completed, the lateral struts, which were always intended to be used, being brought into action, completely prevented distortion from taking place. In the case before the Institution, he thought the fracture arose, principally, from the unequal cooling of the metal, which made a girder of that kind difficult to manufacture. He agreed, that it was better for tensile strains to use wrought-iron rather than cast-iron.

Mr. HAWKSHAW, *V.P.*, confessed he did not understand the principle aimed at in the Paper. It was evident, that girders, or arches might be constructed of many different forms, and the weights to break them might be applied in different places, so that to make each form equally strong in every place, the weight of metal might be continually increased at the weaker points. But he could not understand what the Paper proposed to lead to, as the girders shown, appeared to be combinations of metal without reference to what was under compression, and what was under extension. There would, therefore, be great difficulty in deducing useful results from the experiments. With reference to the question of iron girders, Mr. Eaton Hodgkinson's formula gave an excess of strength in girders of large size. Those who were acquainted with that gentleman would know, that before committing himself to a formula of general application, he would, if he erred at all, be sure to err on the right side. His formula gave from 20 to 25 per cent. of extra strength, arising in part from the fact that the middle web in practice was generally made thicker than in Mr. Hodgkinson's experimental girders.

Mr. HEMANS regretted that the experiments had not been carried out upon a greater number of arches. If there had been five, or six arches connected together, lateral distortion would hardly have been possible, and then a direct comparison of the effects of compression could have been made. From the slender character of the models, the object of the experiments appeared to be lost; for instead of giving a strain equal to 10 tons or 12 tons to the square inch, only 2 tons or $2\frac{1}{2}$ tons had resulted. The experiments could not therefore be regarded as of much value, or as a test of what could be done with cast-iron.

Mr. CHAPPÉ remarked, that in all the experiments, the fractures arose from the tensile strain destroying the haunches. Unless the haunches were made stronger than was the usual practice at the present time, a greater strain must not be calculated upon than was given in the Table in the Paper. No lateral distortion occurred at the upper part of the curve. The two arches were connected at the top by plates, as stated in the Paper, and wood was firmly wedged underneath, to prevent lateral motion.