

## DESCRIPTION OF TENSILE TESTS OF IRON AND STEEL BARS.

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In fulfilment of the promise made at the Spring Meeting of last year, in the discussion upon Professor Kennedy's Report on the experiments on Riveted Joints (Proceedings 1885, page 275), the writer has now the pleasure of communicating the results of the series of tests he was then making at the Horseley Iron Works, Tipton. His principal object in making these tests was to ascertain the relative effect produced on the tensile strength of a flat bar of iron or steel—

- 1st, by a hole drilled out of the bar to the required size ;
- 2nd, by a hole punched  $\frac{1}{8}$  inch smaller in diameter and then drilled out to the size of the first hole ;
- 3rd, by a hole punched in the bar to the size of the drilled hole.

The details of these tests are given in the appended Tables 1 and 2. The test pieces in each series were cut from separate and complete bars ; and each variety was taken from three different parts of the bars, as indicated by the test Nos. marked upon the diagram at the foot of each Table. The centre and end pieces were left unperforated, and were tested in order to supply the basis of comparison for the perforated pieces, and also in order to check the homogeneous quality of the entire bar.

Referring to Table 1 it will be seen that, when the bar of iron experimented upon was perforated by a drilled hole of  $\frac{3}{4}$  inch diameter, the tensile strength across the part fractured was increased from an average of 24·54 tons per square inch of the original area in the unperforated bars to 25·26 tons in the drilled bars. When the bar experimented upon was perforated by having a  $\frac{5}{8}$  inch hole punched in it and afterwards drilled out to  $\frac{3}{4}$  inch diameter, the average

strength was increased from 24·54 tons to 25·28 tons per square inch of the original area across the fracture. This shows that the method of perforating the bars in these two cases did not materially affect the strength of the iron under test, both methods giving results nearly identical and slightly above the strength of the unperforated bar. But when the bar was perforated with a hole punched out to the full  $\frac{3}{4}$  inch diameter, the tensile strength averaged only 19·53 tons per square inch, as against 24·54 tons in the unperforated bar, thus showing a falling off in strength of more than 20 per cent. owing to the method of perforation. Taking the strength of the unperforated bar as unity, the relative strengths of the perforated bars are as follows:—

Unperforated bar of iron	.	.	.	.	.	1·000
Perforated by drilling	.	.	.	.	.	1·029
„ „ punching and drilling	.	.	.	.	.	1·030
„ „ punching only	.	.	.	.	.	0·795

In Table 2 is given a series of tests exactly similar to those in Table 1, except that the metal tested was mild steel instead of iron. The results of these tests, confirming those in Table 1, show that the bars perforated with a drilled hole, and with a smaller punched hole drilled out to the same size, were slightly stronger than the unperforated bar, but that more than 6 per cent. loss of strength ensued when the hole was punched out to the full size. The relative tensile strengths per square inch of original area in these tests are as under:—

Unperforated bar of mild steel	.	.	.	.	.	1·000
Perforated by drilling	.	.	.	.	.	1·068
„ „ punching and drilling	.	.	.	.	.	1·059
„ „ punching only	.	.	.	.	.	0·935

Table 3 gives a series of tests exactly similar to those in Table 1, except that the perforated hole was filled with a rivet put in by a hydraulic machine with a pressure of 31 tons on the head. The results of these tests again confirm those in Table 1, though the putting in of the rivet diminished by about one-half the loss of strength due to perforating the bar by punching only. The relative

tensile strengths per square inch of original area in these tests are as under :—

Unperforated bar of iron	. . . . .	1·000
Perforated by drilling, and filled with rivet	. . . . .	1·012
„ „ punching and drilling, do.	. . . . .	1·008
„ „ punching only, do.	. . . . .	0·894

Table 4 gives a series of tests exactly similar to those in Table 3, except that the metal tested was mild steel instead of iron. The results are similar to those in Table 3, the relative tensile strengths per square inch of original area being as given below :—

Unperforated bar of mild steel	. . . . .	1·000
Perforated by drilling, and filled with rivet	. . . . .	1·103
„ „ punching and drilling, do.	. . . . .	1·110
„ „ punching only, do.	. . . . .	0·927

From all these four Tables alike it will be seen that the metal withstood a slightly greater tensile strain per square inch of original area across the drilled hole than it did across the unperforated bar. This no doubt was owing to the fact that in the drilled bar the slightly greater strain indicated in the Tables was reached only along the transverse diameter of the hole, and that the strain on the metal decreased along the longitudinal diameter of the hole until it was distributed over the whole width of the bar. Thus at the point where it was most severely strained the metal would receive some support from the less severely strained parts adjoining.

This conclusion is confirmed by the first six tests (1319 to 1324) in Table 5, which show that, when a round iron bar of  $1\frac{1}{2}$  inch diameter was reduced in diameter to 0·84 inch at one point only, it fractured under a much greater tensile strain than when it was turned down to the same diameter throughout a length of 10 inches. The relative tensile strengths per square inch of original area in the two cases were as under :—

Round iron bar turned down to 0·84 inch diameter		
through 10 inches length	. . . . .	1·000
„ „ „ grooved to 0·84 inch diameter		
at one point only	. . . . .	1·323

The last five tests in Table 5 show that the elongation of different test bars, all of the same length, is greatly affected by their diameter, those of larger diameter elongating more than those of smaller diameter.





Table 6 gives a series of tests made on flat steel bars to ascertain whether the method of holding the ends of the specimens under test—by grips or by pins—had any effect upon the results of the tests. The figures show that there was a slight advantage in favour of holding the ends by means of grips, instead of by pins passing through: the average breaking strain with the grips being 30·11 tons per square inch of original area, and with the pins 29·97 tons.

In Table 7 is given a series of tests on flat bars of mild steel, showing how the length of the specimen under test affects the tensile strength, elongation, and contraction of area at the point of fracture. It will be seen that, as the specimens decrease in length, the contraction of area at the point of fracture decreases also; and in consequence the tensile strength increases when reckoned on the original area, and decreases when reckoned on the fractured area. The elongation in percentage of the original length is also very much increased in the shorter specimens, owing to the fact that the greater part of the elongation in each specimen, whether short or long, takes place near to the point of fracture, instead of being equally distributed over its entire length; and as this increased elongation near the point of fracture is equal in amount both in the long and in the short specimens, it follows that it gives a higher percentage of elongation when estimated upon the whole length of a shorter specimen than it would upon that of a longer.

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TABLE 1. (see continuation on opposite page)

*Tensile Tests of Flat Iron Bars,  
with Drilled or Punched Hole.*

Test No.	Description and Sketch of Test Pieces. Flat Iron, 3 inches $\times$ $\frac{1}{2}$ inch.	Original Dimensions.	
		Net Breadth and Thickness. Inches.	Area. Square Inch.
787	Test length narrowed to 2.915 inches breadth. 	2.915 $\times$ 0.500	1.457
792		2.915 $\times$ 0.505	1.472
798		2.915 $\times$ 0.500	1.457
788	Hole drilled 0.750 inch diameter. 	2.165 $\times$ 0.500	1.082
791		2.165 $\times$ 0.500	1.082
795		2.165 $\times$ 0.500	1.082
789	Hole punched $\frac{1}{8}$ inch diameter, and drilled to 0.750 inch diameter. 	2.165 $\times$ 0.505	1.093
793		2.165 $\times$ 0.508	1.100
796		2.165 $\times$ 0.510	1.104
790	Hole punched 0.750 inch diameter. 	2.165 $\times$ 0.510	1.104
794		2.165 $\times$ 0.505	1.093
797		2.165 $\times$ 0.510	1.104

*Diagram showing order in which Test Pieces were cut from bar.*

787	788	790	789	791	792	793	794	795	796	797	798

(continued from opposite page) TABLE 1.

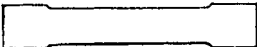



*Tensile Tests of Flat Iron Bars,  
with Drilled or Punched Hole.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Net Breadth and Thickness. Inches.	Area. Square Inch.	Reduction of Area.		Total. Tons.	Per sq. inch of		Ins.	Per cent.
		Square Inch.	Per cent.		Original area. Tons.	Fractured area. Tons.		
2·340×0·375	0·877	0·580	39·8	35·78	24·55	40·7	2·80	28·0
2·335×0·378	0·882	0·590	40·0	36·02	24·47	40·8	2·89	* 28·9
2·350×0·375	0·881	0·576	39·5	35·84	24·59	40·6	2·75	27·5
				27·42	25·34		* In Test No. 792 the extension was 2·60 inches in an original length of 9 inches.	
				27·27	25·20			
				27·32	25·24			
				27·54	25·19			
				27·89	25·35			
				27·95	25·31			
				21·62	19·58			
				21·34	19·52			
				21·53	19·50			

Appearance of Fracture wholly fibrous in every one of these twelve tests.

TABLE 2. (see continuation on opposite page)

*Tensile Tests of Flat Steel Bars,  
with Drilled or Punched Hole.*

Test No.	Description and Sketch of Test Pieces. Flat Steel, $2\frac{1}{2}$ inches $\times$ $\frac{1}{2}$ inch.	Original Dimensions.	
		Net Breadth and Thickness. Inches.	Area. Square Inch.
1349	Test length narrowed to 2.440 inches breadth. 	2.440 $\times$ 0.498	1.215
1350		2.440 $\times$ 0.500	1.220
1351		2.440 $\times$ 0.502	1.225
1358	Hole drilled 0.745 inch diameter. 	1.695 $\times$ 0.500	0.848
1359		1.695 $\times$ 0.500	0.848
1360		1.695 $\times$ 0.500	0.848
1355	Hole punched $\frac{5}{8}$ inch diameter, and drilled to 0.745 inch diameter. 	1.695 $\times$ 0.500	0.848
1356		1.695 $\times$ 0.505	0.856
1357		1.695 $\times$ 0.500	0.848
1352	Hole punched 0.750 inch diameter. 	1.690 $\times$ 0.498	0.842
1353		1.690 $\times$ 0.500	0.845
1354		1.690 $\times$ 0.502	0.848

*Diagram showing order in which Test Pieces were cut from bar.*

1349	1352-55-57	1353-56-59	1350	1354-58-60	1351

(continued from opposite page) TABLE 2.

*Tensile Tests of Flat Steel Bars,  
with Drilled or Punched Hole.*

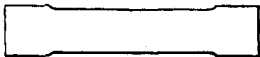



Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Net Breadth and Thickness. Inches.	Area. Square Inch.	Reduction of Area.		Total. Tons.	Per sq. inch of			
		Square Inch.	Per cent.		Original area. Tons.	Frac- tured area. Tons.	Ins.	Per cent.
1.890×0.370	0.699	0.516	42.4	35.71	29.39	51.1	2.66	26.6
1.835×0.345	0.633	0.587	48.1	36.09	29.58	57.0	2.68	26.8
1.850×0.355	0.657	0.568	46.3	36.43	29.73	55.4	2.67	26.7
				26.82	31.62			
				26.83	31.63			
				26.73	31.52			
				26.38	31.11			
				27.05	31.60			
				26.48	31.22			
				23.30	27.67			
				23.23	27.49			
				23.62	27.85			

Appearance of Fracture wholly fibrous in every one of these twelve tests.



TABLE 3. (see continuation on opposite page)

*Tensile Tests of Flat Iron Bars,  
with Drilled or Punched Hole filled with Rivet.*

Test No.	Description and Sketch of Test Pieces. Flat Iron, 3 inches $\times$ $\frac{1}{2}$ inch.	Original Dimensions.	
		Net Breadth and Thickness. Inches.	Area. Square Inch.
1332	Test length narrowed to 2.930 inches breadth. 	2.930 $\times$ 0.517	1.515
1333		2.930 $\times$ 0.515	1.509
1334		2.930 $\times$ 0.517	1.515
1338	Hole drilled 0.750 inch diameter;  rivet snapped with 31 tons pressure.	2.190 $\times$ 0.515	1.128
1339		2.185 $\times$ 0.518	1.132
1340		2.185 $\times$ 0.520	1.136
1341	Hole punched $\frac{5}{8}$ inch diameter, and drilled to 0.750 inch diameter;  rivet snapped with 31 tons pressure.	2.185 $\times$ 0.515	1.125
1342		2.180 $\times$ 0.515	1.123
1343		2.185 $\times$ 0.520	1.136
1344	Hole punched 0.755 inch diameter;  rivet snapped with 31 tons pressure.	2.175 $\times$ 0.515	1.120
1345		2.175 $\times$ 0.516	1.122
1346		2.180 $\times$ 0.515	1.123

*Diagram showing order in which Test Pieces were cut from bar.*

1332    1338    1341-44-39-42    1333 1345-40-43-46    1334

(continued from opposite page) TABLE 3.

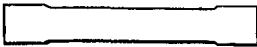



*Tensile Tests of Flat Iron Bars,  
with Drilled or Punched Hole filled with Rivet.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Net Breadth and Thickness. Inches.	Area. Square Inch.	Reduction of Area.		Total. Tons.	Per sq. inch of			
		Square Inch.	Per cent.		Original area. Tons.	Fractured area. Tons.	Ins.	Per cent.
2.390×0.410	0.980	0.535	35.3	39.82	26.28	40.6	2.62	26.2
2.340×0.400	0.936	0.573	37.9	39.80	26.37	42.5	2.78	27.8
2.330×0.395	0.920	0.595	39.2	39.60	26.13	43.0	2.72	27.2
				29.85	26.46			
				30.31	26.77			
				30.08	26.47			
				29.77	26.46			
				29.49	26.26			
				30.31	26.68			
				26.23	23.41			
				26.49	23.60			
				26.35	23.46			

Appearance of Fracture, 98 per cent. fibrous and 2 per cent. crystalline in 1344; wholly fibrous in each of the other eleven tests.

TABLE 4 (see continuation on opposite page)

*Tensile Tests of Flat Steel Bars,  
with Drilled or Punched Hole filled with Rivet.*

Test No.	Description and Sketch of Test Pieces. Flat Steel, $2\frac{1}{2}$ inches $\times$ $\frac{1}{2}$ inch.	Original Dimensions.	
		Net Breadth and Thickness. Inches.	Area. Square Inch.
1363	Test length narrowed to 2.440 inches breadth.	2.440 $\times$ 0.495	1.208
1364		2.440 $\times$ 0.495	1.208
1365		2.440 $\times$ 0.495	1.208
1372	Hole drilled 0.745 inch diameter;	1.705 $\times$ 0.490	0.835
1373		1.705 $\times$ 0.491	0.837
1374	rivet snapped with 31 tons pressure.	1.705 $\times$ 0.492	0.839
1369	Hole punched $\frac{1}{16}$ inch diameter, and drilled to 0.745 inch diameter;	1.700 $\times$ 0.493	0.838
1370		1.700 $\times$ 0.493	0.838
1371	rivet snapped with 31 tons pressure.	1.700 $\times$ 0.495	0.842
1366	Hole punched 0.750 inch diameter;	1.690 $\times$ 0.495	0.837
1367		1.690 $\times$ 0.495	0.837
1368	rivet snapped with 31 tons pressure.	1.695 $\times$ 0.495	0.839

*Diagram showing order in which Test Pieces were cut from bar.*

1363 1366-69-72      1367-70-73      1364 1368-71-74      1365

(continued from opposite page) TABLE 4.




*Tensile Tests of Flat Steel Bars,  
with Drilled or Punched Hole filled with Rivet.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Net Breadth and Thickness. Inches.	Area.  Square Inch.	Reduction of Area.		Total.  Tons.	Per sq. inch of		Ins.	Per cent.
		Square Inch.	Per cent.		Ori- ginal area. Tons.	Frac- tured area. Tons.		
1·900×0·370	0·703	0·505	41·8	35·64	29·50	50·6	2·60	26·0
1·865×0·355	0·662	0·546	45·1	35·69	29·54	53·9	2·60	26·0
1·885×0·360	0·679	0·529	43·7	35·46	29·35	52·2	2·74	27·4
				27·37	32·77			
				27·01	32·27			
				27·27	32·50			
				27·35	32·63			
				27·46	32·76			
				27·56	32·73			
				23·49	28·06			
				23·96	28·62			
				21·19	25·25			

Appearance of Fracture, 95 per cent. fibrous and 5 per cent. crystalline in 1366; defective in 1368; wholly fibrous in each of the other ten tests.

TABLE 5. (see continuation on opposite page)

*Tensile Tests of Round Iron Bars,  
turned down to different Diameters and Lengths.*

Test No.	Description and Sketch of Test Pieces. Round Iron, 1½ inch diameter.	Original Dimensions.	
		Diameter. Inch.	Area. Square Inch.
1319	Turned down for 10 inches length to 0·840 inch diameter, being same diameter as bottom of groove in 1322 and 1323 and 1324. 	0·840	0·554
1320		0·840	0·554
1321		0·840	0·554
1322	Grooved at angle of 55 degrees, to correspond with Whitworth standard 1 inch thread, but rounded off a little more in bottom of groove.  Diameter at bottom of groove 0·840 inch.	0·840	0·554
1323		0·840	0·554
1324		0·840	0·554
1327	Turned down for 10 inches length to 1·440, 1·440, 1·120, 0·755, and 0·500 inch diameter. 	1·440	1·629
1328		1·440	1·629
1329		1·120	0·985
1331		0·755	0·448
1330		0·500	0·196

*Diagram showing order in which Test Pieces were cut from rod.*

1319 - 22	1320 - 23	1321 - 24	1327	1329	1330	1331	1328

(continued from opposite page) TABLE 5.

*Tensile Tests of Round Iron Bars,  
turned down to different Diameters and Lengths.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Dia- meter.	Area.	Reduction of Area.		Total.	Per square inch of			
Inch.	Square Inch.	Square Inch.	Per cent.	Tons.	Original area. Tons.	Frac- tured area. Tons.	Ins.	Per cent.
0·615	0·297	0·257	46·3	13·37	24·13	45·0	2·41	24·1
0·617	0·299	0·255	46·0	13·46	24·29	45·0	2·50	25·0
0·617	0·299	0·255	46·0	13·20	23·82	44·1	2·39	23·9
				17·72	31·98			
				17·62	31·80			
				17·63	31·82			
1·060	0·882	0·747	45·8	39·50	24·24	44·7	2·94	29·4
1·026	0·827	0·802	49·2	39·15	24·03	47·3	2·95	29·5
0·820	0·528	0·457	46·3	23·59	23·94	44·6	2·52	25·2
0·540	0·229	0·219	48·8	10·71	23·90	46·7	2·42	24·2
0·356	0·100	0·096	48·9	4·56	23·26	45·6	1·83	18·3

Appearance of Fracture, 90 per cent. fibrous and 10 per cent. crystalline in 1322;

” ” 95 ” ” 5 ” ” 1323;

” ” 15 ” ” 85 ” ” 1324;

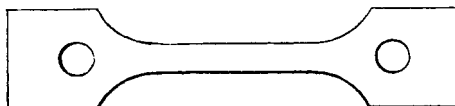
” ” wholly fibrous in each of the other eight tests.

TABLE 6. (*see continuation on opposite page*)

*Tensile Tests of Flat Steel Bars,  
pulled by Grips or by Pins.*

Test No.	Description and Sketch of Test Pieces. Flat Steel, 15-32nds inch thick.	Original Dimensions.	
		Net Breadth and Thickness. Inches.	Area. Square Inch.
1422	Flat bars, 16 inches $\times$ 2 inches, pulled by Grips. Extension measured over 10 inches in centre.	2.000 $\times$ 0.455	0.910
1423		2.000 $\times$ 0.455	0.910
1424		2.000 $\times$ 0.460	0.920
1425		2.000 $\times$ 0.455	0.910
1426		2.000 $\times$ 0.455	0.910
1427	Flat bars, 28 inches $\times$ 6½ inches, narrowed to 2 inches over central 10 inches, pulled by Pins of 2 inches diameter. Pin-hole centres 4 inches from ends. <i>See sketch at foot.</i>	1.970 $\times$ 0.460	0.906
1428		1.970 $\times$ 0.458	0.902
1429		1.980 $\times$ 0.460	0.911
1430		1.970 $\times$ 0.455	0.896

*Sketch of Test Pieces 1427, 1428, 1429, 1430.*



(continued from opposite page) TABLE 6.

*Tensile Tests of Flat Steel Bars,  
pulled by Grips or by Pins.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Extension in original length of 10 inches.	
Net Breadth and Thickness. Inches.	Area. Square Inch.	Reduction of Area.		Total. Tons.	Per sq. inch of		Ins.	Per cent.
		Square Inch.	Per cent.		Original area. Tons.	Fractured area. Tons.		
1.575×0.315	0.496	0.414	45.4	27.58	30.30	55.6	2.55	25.5
1.575×0.312	0.491	0.419	46.0	27.57	30.29	56.1	2.53	25.3
1.685×0.357	0.602	0.318	34.5	27.66	30.06	45.9	2.27	22.7
1.570×0.282	0.443	0.467	51.3	27.35	30.05	61.7	2.61	26.1
1.695×0.352	0.597	0.313	34.3	27.19	29.87	45.5	2.35	23.5
1.545×0.312	0.482	0.424	46.7	27.23	30.05	56.4	2.46	24.6
1.550×0.305	0.473	0.429	47.5	27.19	30.14	57.4	2.29	22.9
1.540×0.288	0.444	0.467	51.2	27.18	29.83	61.2	2.38	23.8
1.535×0.284	0.436	0.460	51.3	26.77	29.87	61.3	2.64	26.4

Appearance of Fracture wholly fibrous in every one of these nine tests.

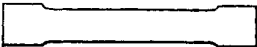
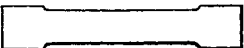
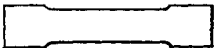
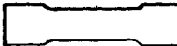
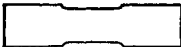
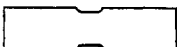
*Diagram showing order in which Test Pieces were cut from bar.*

1422	1427	1423	1428	1424	1429	1425	1430	1426
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TABLE 7. (see continuation on opposite page)

*Tensile Tests of Flat Steel Bars,  
with different Lengths of part tested.*

Test No.	Description and Sketch of Test Pieces. Flat Steel, $2\frac{1}{2}$ inches $\times$ $\frac{1}{2}$ inch.		Original Dimensions.	
			Net Breadth and Thickness. Inches.	Area. Square Inch.
1377	Total length.	Narrowed length.	$2 \cdot 230 \times 0 \cdot 490$	1·093
1378	Inches.	Inches.	$2 \cdot 210 \times 0 \cdot 485$	1·072
1379	16 	10	$1 \cdot 880 \times 0 \cdot 490$	0·921
1380	15 	9	$1 \cdot 875 \times 0 \cdot 485$	0·909
1381	14	8	$1 \cdot 875 \times 0 \cdot 485$	0·909
1382	13 	7	$1 \cdot 875 \times 0 \cdot 485$	0·909
1383	12	6	$1 \cdot 875 \times 0 \cdot 485$	0·909
1384	11 	5	$1 \cdot 870 \times 0 \cdot 490$	0·916
1385	11	4	$1 \cdot 880 \times 0 \cdot 490$	0·921
1386	11 	3	$1 \cdot 880 \times 0 \cdot 488$	0·917
1387	11	2	$1 \cdot 880 \times 0 \cdot 488$	0·917
1388	11 	1	$1 \cdot 880 \times 0 \cdot 485$	0·912
1389	11	$\frac{1}{2}$	$1 \cdot 885 \times 0 \cdot 488$	0·920

*Diagram showing order in which Test Pieces were cut from bar.*

1377	1380	1381	1382	1383	1384	1378	85	86	87	88	89	1379
16	15	14	13	12	11	16	11	11	11	11	11	16

*Lengths of Test Pieces in inches.*

(continued from opposite page) TABLE 7.

*Tensile Tests of Flat Steel Bars,  
with different Lengths of part tested.*

Dimensions after Fracture.				Ultimate Tensile Stress.			Original Length.	Extension in original length.	
Net Breadth and Thickness. Inches.	Area. Sq. Inch.	Reduction of Area.		Total. Tons.	Per sq. in. of Original area. Tons.	Fractured area. Tons.		Ins.	Per cent.
Sq. Inch.	Sq. Inch.	Per cent.							
1.685 × 0.322	0.543	0.550	50.3	32.80	30.00	60.4	10	2.63	26.3
1.655 × 0.332	0.549	0.523	48.7	32.25	30.08	58.7	10	2.62	26.2
1.400 × 0.328	0.459	0.462	50.1	27.69	30.06	60.3	10	2.60	26.0
1.420 × 0.334	0.474	0.435	47.8	27.54	30.29	58.1	9	2.34	26.0
1.390 × 0.330	0.459	0.450	49.5	27.66	30.42	60.2	8	2.24	28.0
1.450 × 0.356	0.516	0.393	43.2	27.71	30.48	53.7	7	1.85	26.4
1.420 × 0.330	0.469	0.440	48.4	27.75	30.52	59.1	6	1.65	27.5
1.400 × 0.333	0.466	0.450	49.1	27.89	30.44	59.8	5	1.38	27.6
1.405 × 0.334	0.469	0.452	49.0	27.96	30.35	59.6	4	1.32	33.0
1.465 × 0.355	0.520	0.397	43.2	28.11	30.65	54.0	3	1.00	33.3
1.485 × 0.364	0.541	0.376	41.0	28.26	30.81	52.2	2	0.77	38.5
1.535 × 0.361	0.554	0.358	39.2	29.42	32.25	53.1	1	0.49	49.0
1.630 × 0.344	0.561	0.359	39.0	30.11	32.72	53.6	$\frac{1}{2}$	0.27	54.0

Appearance of Fracture wholly fibrous in every one of these thirteen tests.

*Discussion.*

Mr. WICKSTEED exhibited a large number of specimens which had been tested in the machine, and to which their respective self-recorded diagrams were attached. He also showed a volume containing a collection of various test diagrams from samples of iron, steel, copper, brass, and delta metal.

The scale of stress in the self-recorded diagrams was of course created by the elasticity of the indicator spring; and he had therefore been particularly anxious to establish its correctness. The spring was 15 inches long, and had a range of 5 inches, being one-third of its length. Throughout its range it was supposed to be perfectly elastic, in the sense that it would contract equal distances for equal additions of load; if this was not the case, the scale would not be correct. For establishing its correctness, the following mode was adopted. The poise-weight was set to begin with at 1 ton on the steel-yard, and the water pressure was applied until the lever floated. As soon as the lever floated, a pencil mark was ruled across the diagram paper, by pulling the indicator barrel round. In the same way a second mark was made on the paper at 10 tons stress; and ultimately, when the maximum carrying power of the specimen was reached, good care was taken to have the lever floated again, so as to mark correctly on the paper the exact position corresponding with the maximum load. In this way three positions were marked on the paper which were ascertained with absolute correctness from the steel-yard itself. The zero or base line was not obtained in the same way as the three foregoing, because allowance had to be made for the initial friction of the leathers; it was therefore fixed at a distance below the 1 ton line equal to one-ninth of the distance between 1 ton and 10 tons. The paper bore therefore a base-line correct by inference, a correctly ascertained maximum line, and an accurately ascertained line at 10 tons. Then supposing the maximum load on the sample were 24 tons, the total height of the diagram was carefully divided into 24 equal parts; and if the tenth

division hit upon the line which had been accurately ascertained to be the position of 10 tons, a positive proof was thereby obtained that the scale was a correct one; because it could not be correct at the top, and correct at the bottom, and correct also at an intermediate place, without being correct throughout the whole of its subdivisions. This he believed was the most important point to which he had paid attention since the preparation of the paper.

With reference to the wavering and falling off in the load at the elastic limit or critical point B in the diagram, Fig. 8, Plate 4, this feature in the diagram must be attributed in his opinion to the peculiar behaviour of iron and steel. It could not be attributed to any peculiarity of the machine or of the recording apparatus, because, if instead of iron or steel a piece of delta metal or copper or brass were tested in the machine, no such fluctuation was then found at that point in the diagram; and if two samples of the same material were tested, the second never repeated the first exactly in its behaviour at that point. There were very noticeable differences to be seen in the behaviour of almost all the individual samples, although the general characteristics were the same for the same metal.

Professor ALEX. B. W. KENNEDY said the automatic drawing of a stress or strain diagram would be an exceedingly simple thing if the pull in the specimen could be made to increase continuously; but unfortunately always at the climax C in Fig. 8, Plate 4, and very commonly also at B as pointed out by the author, there were fluctuations: the stress at those points underwent a falling-off, which was peculiar to the particular material tested, and quite independent of any method of manipulation. The real difficulty of designing apparatus such as that described in the paper was met with at those points in the diagram, and lay in the means of providing some measurement for stress which should be independent of the position of the weight on the steel-yard, because the weight itself did not automatically move back along the steel-yard at those points. So far as he knew, Mr. Wicksteed's exceedingly ingenious and workmanlike apparatus—which no doubt most of the members had seen last year at the Inventions Exhibition, if nowhere else—was the first machine in

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which the behaviour of the material both at the elastic limit and at the maximum load was automatically registered; it was at least the first that had come to his own knowledge in which both those points were autographically recorded. He was therefore especially glad that the Institution should now be enabled to introduce into its proceedings the author's own description of so highly important and ingenious an apparatus.

In regard to the friction of the leathers on the main ram, it had been stated in the paper that, after their initial resistance had been deducted, the friction was exactly proportional to the total pressure, and so affected only the scale of the diagram. This was just a point with regard to which no opinion was worth anything until it was based on experiments such as had been made by the author; and he therefore felt bound to accept it. At the same time he should be very glad if the records could be given of two or three sets of measurements such as were referred to on page 31, so that it might be seen within what limits of accuracy that statement was exact.

As to the elimination of the friction of the indicator ram in its longitudinal movement, by the expedient of keeping it revolving, it was rather stretching a point to say that absolutely no friction would then be left to come into play longitudinally. The cause of the disappearance of longitudinal friction from the ram when it was made to revolve he considered was simply the alteration of the direction in which the surfaces were rubbing upon each other. Each point on the surface of the revolving ram, instead of simply moving longitudinally, was really moving in a very finely pitched screw. If the ram simply revolved without moving longitudinally at all, there would be no longitudinal friction, because there would be no longitudinal component of motion along the axis. It was the longitudinal friction that had to be got rid of, not the circumferential; and he imagined that the ratio of the former to the latter must be equal to the ratio of the pitch of the screw motion to the circumference of the ram; and as the ram revolved at considerable speed, while longitudinally it moved very slowly, the pitch of the screw motion was a very fine one. The amount of the friction to be encountered longitudinally was therefore not zero, but some small

fraction of the whole friction on the ram; and no doubt it could be reduced so much that it might practically be neglected.

The greater part of the work he had himself done in regard to elasticity had been in connection with the behaviour of materials under such loads as occurred actually in practice. For this purpose he was afraid there was as yet no autographic apparatus capable of rendering very much help; because the fluctuations of strain which occurred within the range of elasticity from A to B in Fig. 8, Plate 4, were so very small that they did not even appear at all in that diagram; and they had to be magnified so enormously before they were made visible that so far as he knew they had not been autographically registered in any satisfactory way. But he had been so much struck some time ago with some of the diagrams which Mr. Wicksteed had been good enough to show him, especially with the autographic record of the elastic limit or breakdown point at B in Fig. 8, that last spring he began again seriously to try to scheme for his own use a diagram-drawing apparatus, his previous attempts in that direction having come to nothing. His reason for not adopting Mr. Wicksteed's plan had been, not that he had any objection to it on principle, but mainly that it was too expensive for the limited resources available in his own case. He had therefore decided upon utilising a plan that he had previously adopted for other purposes, namely putting into the testing machine two bars "in series," as electricians would say: one bar to be tested or broken, and another bar which might be called a spring-piece, being made so much stronger than the test-piece that when the test-piece was broken the spring-piece should still not be strained beyond its limit of elasticity. He had put his roughly sketched plans into the hands of his friend, Mr. A. G. Ashcroft, by whom they had not only been worked out into the apparatus now exhibited to the meeting, but had also been so greatly modified and improved that his own interest in them was not any longer paternal, but merely friendly and critical. As shown in the drawings of the apparatus, Figs. 13 to 15, Plate 7, the test-piece T and the spring-piece S were arranged horizontally in line with each other, and secured together by a screwed joint J, and pulled endways. Two

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spring clips  $C_1C_2$  were fixed as gauging points, one on each end of the test-piece, at ten inches apart, or whatever the length to be tested might be. The extension on that ten inches was communicated to a sheet of smoked glass  $G$  sliding in a frame  $F$  fixed upon the spring-piece  $S$ ; any increase in the distance between the two gauge points  $C_1C_2$  caused the sheet of glass to slide towards the test-piece  $T$ . The lever and sector arrangement  $LMN$  through which this motion was communicated was of a differential nature, and served the purpose of making the motion of the glass entirely independent of any possible give in the screwed joint  $J$ , or any extensions in any other part of the apparatus than the part between the two gauge points  $C_1C_2$  on the test-piece. The lever  $L$ , centred at its lower end upon the frame  $F$ , was pulled in the middle of its length by a rod connected with the nearer clip  $C_2$ ; and on its upper end was centred the sector  $M$ . The rod  $N$  from the further clip  $C_1$  actuated the sector by means of a silk thread, attached by each of its ends to the rod and wrapped round the shorter arc of the sector; and the glass  $G$  was pulled forwards by a silk thread  $H$  from the longer arc, and was pulled back by another thread  $K$  passing over a pulley on the frame to a counterweight. The distance through which the sliding glass moved was equal to twice the extension of the test-piece. On the extremities of the spring-piece  $S$  were clamped at  $UU$  the outer ends of two rods, of which the inner or meeting ends carried two saddles  $VV$  sliding freely past each other along the spring-piece. Across the saddles lay the axis or centre pin  $R$  of a light brass pointer or vibrating arm  $P$ ; and each saddle carried a silk thread, attached by each of its ends to the saddle, and wrapped round the axis  $R$ . By this means the extensions of the spring-piece moved the pointer  $P$ ; and the angular movement of the pointer was in proportion to the extension of the spring-piece. In this particular spring-piece he had previously ascertained that, up to its limit of elasticity, or at least as far as the load which would break the test-piece, the extension was proportionate to the load: so that the part  $AB$  of the diagram corresponding with Fig. 8, Plate 4, was a straight line. That was not so of course for all materials; nor was it always so for every individual piece of any particular

material. But for this individual spring-piece he had found that it was so, having tried it several times. Consequently the angle moved through by the pointer, being proportional to the extension of the spring-piece, was proportional to the pull in the spring-piece; and therefore also to the pull in the test-piece, because the pull in the two was the same. Accordingly the pointer arm vibrated up or down on its centre through an angle exactly proportional to the real pull in the test-piece, no matter where the weight was on the steel-yard which produced the pull in the test-piece; and at the same time the glass slid horizontally alongside the pointer in proportion to the extension of a known length upon the test-piece itself. The pointer scraped the smoke off the glass; and after varnishing the diagram so got, the smoked glass could be used as a negative, and prints, such as those shown in Fig. 16, Plate 8, could be photographed directly on blue paper in the ordinary way. The drawback in this arrangement was that the motion of the pointer was of course in a circular arc, and consequently the diagram had not a straight axis of stress, but was like the old curved indicator diagrams produced by Mr. Gooch's indicator. That result was inherent in this particular form of apparatus; and it was a disadvantage which he was afraid it would be difficult to overcome. Thus far the apparatus had not been adapted for any test-pieces which had to be held in wedge grips; probably that might come afterwards, but at present its use was limited to particular forms of test-piece. The delicacy of the apparatus rendered it peculiarly sensitive at the point corresponding with B in Fig. 8, Plate 4, representing the elastic limit. It would be seen that some of the diagrams in Fig. 16, Plate 8, showed the stress line going back at that point by an amount equal in one case to 15 per cent. and in another to 13 per cent. of the whole load.

The labour involved in constructing non-autographic diagrams, on which some stress had been laid by the author, was not perhaps so serious after all, especially as non-autographic diagrams had to be constructed under any circumstances for the part of the test before the limit of elasticity was reached. The mechanical work expended on a test-piece was no doubt in many respects a most excellent measure of the value of a material. As he had pointed out on a



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previous occasion,\* it was very easy to get an approximation to the area A B C F, Fig. 8, Plate 4—that is, to the whole mechanical work expended on the test-piece—by means of a simple formula which included only the measurements always given to engineers, namely the maximum stress and the total extension and the ratio of the limit of elasticity to the maximum stress. The following were a few results showing the comparison of the measured areas of such diagrams as those shown in Fig. 16, Plate 8, with the calculations so made.

Test No.	Steel or Iron.	Work in Inch-Tons per cubic inch.		Error per cent.
		Actual.	Calculated.	
5400	Basic Steel Plate . .	5.49	5.57	+1.5
5401-2	" " . .	5.33	5.21	-2.3
5401-1	" " . .	5.91	5.67	-4.2
8397	Perkins' Steel Bar . .	5.26	5.12	-2.7
9047	Landore Rivet Steel . .	7.56	7.09	-6.1
9048	" " . .	6.29	6.17	-2.0
8391	S.C. Crown Bar Iron . .	4.85	4.55	-6.3
8592-3	Rivet Iron . .	4.47	4.52	+1.2
9461	Swedish Bar Iron . .	5.00	4.94	-1.3
9462	" " . .	5.29	5.44	+2.7

It would be seen that occasionally there was a difference of 6 per cent.; more generally the difference was only 2 or 3 per cent. It was well for engineers to know that there was a mode of getting the total work done in breaking a test-piece, not accurately but with a reasonable approximation, from data which were entirely at their own command.

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\* See Proceedings of the Institution of Civil Engineers, 1882, vol. lxi., page 30. The formula there given may be written,  $work = s x \left( \frac{r+2}{3} \right)$ , where  $s$  is the maximum load per square inch of original area,  $x$  the total extension in inches, and  $r$  the ratio  $\frac{\text{limit of elasticity}}{\text{maximum load}}$ . This gives the work in inch-tons (or inch-lbs. &c., as the case may be) on the tested length of the given material if it were one square inch in cross section. Divided by the tested length in inches, it gives the work per cubic inch.

The proposal to measure the extensions upon a length which did not include the short portion near to the place of fracture was of course an old one. He had himself adopted, as he had no doubt many others had done, the plan of dividing into inches the total length of the test-piece before testing it, marking the inches upon the piece itself, so that the extension in any number of inches, not including the inches near the place of fracture, could after the test be separately measured if required. He was rather surprised that engineers generally did not more often want that measurement of the extension, which, as shown in Mr. Wicksteed's paper, was a most important matter.

In comparing specimens of different shapes, he presumed Mr. Wicksteed meant to limit the comparison to specimens of similar section. It would not hold good, he thought, between a square and a round section, for instance; and this he believed was a general opinion among engineers.

MR. WICKSTEED did not know that the shape of the section made any difference up to the commencement of local extension.

Professor KENNEDY mentioned, in regard to the concluding paragraph of Mr. Wicksteed's paper, that there was at least one instance of manipulation sufficiently skilful to adjust back a poise fast enough for keeping pace with the decreasing resistance of the test-piece just before fracture; for he had himself had the good fortune to see this done by Professor Unwin in his testing apparatus, about which he hoped something would be said on the present occasion.

With regard to Mr. Bennett's paper, the experiments recorded in his tables showed that in all cases the drilling of a hole through the test bar had resulted in that excess of tensile strength which had already been discussed at previous meetings. What he wished now to point out was that the excess in these experiments was sometimes rather small, on account of the test-pieces having flat sides and a circular hole drilled through the middle. It was a well-known fact that the excess was always greater in a test-piece shaped like

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the portion of plate between two adjacent rivet holes along the line of a riveted joint: that is, in a test-piece bounded by two semicircular arcs, one on each side, and without a hole through the middle. From his own notes of some experiments of this kind he found that test-pieces, such as those used by Mr. Bennett, gave  $34\frac{1}{2}$  tons; while pieces of the same material and area, bounded not by flat sides, but by semicircular arcs corresponding with the sides of two rivet holes, gave  $36\frac{1}{2}$  and 37 tons. He had read the experiments given in Mr. Bennett's paper with very much interest, and he was glad they had been presented to the Institution.

Mr. CHARLES COCHRANE wished Professor Kennedy would supplement his last remarks by pointing out how the increase of tensile strength was the result of the shape of the material. To himself it was difficult, though it might not be so to others, to see how, after a bar or plate had been drilled with a hole through it, the increase of tensile strength per square inch arose in the material that was left.

Professor KENNEDY said he had not anything further to add in reply to that question beyond the explanation which he had already attempted on previous occasions (Proceedings, 1881, pp. 217-218; 1885, pp. 287-288).

Mr. BENJAMIN WALKER considered the apparatus described in Mr. Wicksteed's paper would be found very useful indeed in connection with the manufacture of steel. Iron was not so uncertain or so irregular as steel. Steel of the very highest qualities would now and again show most fantastic behaviour. The autographic test-recording apparatus would act as a sort of policeman, checking the quality of the material by showing what was the load actually carried, and also what was the rate of extension at the same time. There was some steel that he would trust without any fear as to the consequences, so long as the load upon it was steady and uniform; but there was other steel with regard to which he should require to be very sure that it possessed sufficient elasticity and stretching

power before he should be satisfied to trust it. This testing machine enabled a steel-maker to discriminate one kind of steel as suitable for a concussive strain, and another for a steady strain: the one as suitable for a piston-rod, and the other for a slide-bar. Where the strains were concussive, sudden, and uncertain, steel capable of a large amount of extension was the best. Sir Joseph Whitworth had been quite right in calling the attention of mechanical engineers to the fact that the combined estimate of the quality of steel, by what it would carry and what it would extend, was the correct mode of measurement for that material.

In confirmation of the statement made in page 31 of the paper—that the friction of the hydraulic leathers appeared to be uniformly proportional to the water pressure—he remembered that some ten or twelve years ago the late Mr. George Wilson of Sheffield had undertaken to make some very large links of flat steel for a bridge in America, which were required to be tested. A large number of these links had already been completed, and a number of beautiful test-pieces had been received from Mr. Kirkaldy, showing the excellent quality of the metal, as proved by its high breaking strength and the large extent of its stretching in these samples. But the samples were not accepted, and it was required that each separate link should itself be put into a testing machine, and tested to carry a certain fixed load. At that time there was no machine in the country that would carry such a load as was required; and he had therefore been engaged to make a testing machine for the purpose. As no steel-yard that he knew of could be constructed to carry the load required, it was decided to employ a pair of hydraulic rams of 20 inches diameter, placed horizontal and moving a crossbeam, to which was attached the link to be tested. The friction of the glands of the rams was arrived at by a species of approximation, whereby a very fair test was obtained of each link, the test load being of course applied after the glands had been screwed up water-tight. One of the glands was packed with a cup leather, and the other with common hemp packing. In order to arrive at the friction of each gland packing, each gland in turn was gradually screwed up very carefully until it just stopped leaking while carrying an

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experimental load; then the load was taken off, and by means of a steel-yard the exact weight was ascertained that was required to move the unloaded ram through the gland. The experimental load was successively increased, the glands successively tightened up to be just watertight, and the increasing friction successively measured in the same manner, until the full amount of the required test load was reached. In this way it was found that the friction of each packing alike was most uniform, increasing in regular proportion with the load moved by the rams. By this means the bridge links of 10 or 12 feet length were satisfactorily tested to the required load. As a further check upon the friction of the glands, the water pressure on each ram was noted by three pressure-gauges, and the total force was thus calculated; then the amount of the dead test-load being deducted, the difference represented the friction of the packings, and confirmed the results obtained with the steel-yard as to the uniform increase in friction with the increasing pressures.

He was quite sure there would be yet more to be heard of the machine described in the paper. Even if it was not yet so far perfected as to be useful in the hands of a common workman, it was clearly the right beginning and in the right direction; and he had no doubt that ultimately these machines would be employed to act as policemen in steel works, for testing the quality of the steel.

Professor W. CAWTHORNE UNWIN said he was much obliged to Professor Kennedy for the remark he had made about keeping the lever floating during the test, because the evidence of an independent witness was better than anything that he himself could say. The fact was that with three specimens out of four, if ordinary pains were taken, the lever could be kept floating throughout the whole of the test, and the whole of the diagram to the very end could thus be drawn with apparatus which was connected with the poise-weight. In certain specimens however he must admit that Mr. Wicksteed was quite right in saying that this could not be done. With a very ductile bar it was not possible to keep the lever floating during the whole of the test, although it was possible to get over that difficulty in a way which was quite satisfactory. The experiments he had made

in that direction had led him to doubt very much whether even with Mr. Wicksteed's form of apparatus, or with his own, it was possible to get at all a perfect registration of the part of the curve at B in Fig. 8, Plate 4, or the final part of the curve from the climax C down to the point of fracture F. But it was very doubtful whether this was a matter of any particular importance. It so happened that in 1882 he wanted a large testing-machine; and between that time and the present Mr. Wicksteed had made two 100-ton machines for him, one for Cooper's Hill and one for the Central Institute, both of which were very fine machines. One of the reasons which had determined him in selecting this machine was that he saw from the first that it lent itself very conveniently to the application of an autographic apparatus which would not in any way interfere with the ordinary processes of testing. During the last three years Mr. Wicksteed and himself had been in constant communication upon this subject; they had both been working at the same problem, but had come to two different solutions of it.

Engineers generally he thought were not at all aware how much had been done in the direction of getting autographic diagrams. The first machine, as far as he knew, was that of Professor Thurston in America; it was perfect in principle, but he thought it did not give very perfect diagrams. It was however an exceedingly interesting machine; and it brought out in a very striking way one peculiarity in the behaviour of metals, namely the rise of the elastic limit after removal and re-application of the load above the stress previously on the bar. Two or three different machines had also been made in America, by Fairbanks and others; and two or three also in Germany, of different types again. He had himself constructed one machine; his successor at Cooper's Hill had constructed another; and Professor Kennedy had arranged the beautiful machine now exhibited, the principle of which he had previously understood, although he had never seen it till now, and in some respects it was better than any of the others. A good deal therefore had been done in that direction. The peculiarity and originality of Mr. Wicksteed's autographic apparatus lay in his having taken the indication of the load not from the poise-weight, from which

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it was ordinarily taken, but from the pressure in the main hydraulic cylinder. When Mr. Wicksteed first told him that he was going to work in that way, he confessed he had not expected him to succeed as well as he had done; for he had really made a very great success of the plan which he had adopted. What occurred to himself as an objection to going that way to work was that in that plan the thing which was directly measured was the pressure in the main hydraulic cylinder. Now that pressure was made up of several items. The whole pressure  $P$  in the cylinder was made up firstly of the tension  $T$  on the test specimen. In the second place it comprised the constant pressure  $C$ , which was due to the unbalanced part of the counterbalance weight attached to the piston-rod of the main hydraulic cylinder. Thirdly, while the piston was moving there was the friction  $F$  of its own cup leather, and of that through which the piston-rod worked. Fourthly, there were some smaller items of friction  $f$  due to certain guides, and to the journals of the rather heavy counterbalance weight, and to the pin joints connecting it with the piston-rod. Fifthly, there was an item which at first sight it might be thought ought to be omitted, namely the inertia  $I$  of the moving parts. In the 100-ton machines supplied to himself, he believed the weight of the whole mass that moved with the piston was between 2 and 3 tons. Now if that weight changed its velocity of motion at all, its inertia came into play; and though the change of motion was not very quick, the weight was very heavy. The inertia he thought did not at all affect the diagram generally, that is from A to B in Fig. 8, Plate 4, and also through the greater part of the line from B to C: but he believed it did affect the diagram to a certain extent in the two parts he had mentioned, namely the short fluctuation immediately following the point B, and also the falling line from C to F. Finally there was also a very little friction in the pipe connecting the indicator cylinder with the main hydraulic cylinder; but this might be neglected, because only in certain cases would it be of any importance. The whole pressure  $P$  in the main hydraulic cylinder was therefore seen to be made up as follows:—

$$P = T + (C + F + f + I)$$

It would here be seen that Mr. Wicksteed was obliged to assume

that all those quantities which he did not want to measure—namely the four items enclosed within the brackets in the above expression—formed together a constant percentage of the tension  $T$  on the specimen. At first it looked as if this must be impossible; and his first view of the apparatus, when he heard of the method Mr. Wicksteed was going to adopt, before the machine was constructed, was that it would be impossible to get a very good result in that way. On the whole however he believed a very good result had thereby been obtained. By certain slight adjustments of the counterbalance weight acting on the hydraulic piston, and of the initial tension of the indicator spring, a load scale was obtained on the diagrams which certainly was tolerably constant all the way from 1 ton up to 50 tons. He was therefore quite ready to admit that the difficulty had been nearly conquered by Mr. Wicksteed; though the fact that some adjustment was necessary constituted he thought an objection to that type of machine, because it was impossible to be certain that those adjustments would remain always constant, both in themselves and in their effect, and that the person in charge of the machine always took the trouble to see whether the adjustments had changed or not.

The uniformity of the load scale in Mr. Wicksteed's diagrams had been determined by experiments, in which, before the mark was made with the pencil, care was always taken to get the piston into motion downwards. As long as this was done, the load scale so obtained was tolerably uniform. But with that apparatus it was not possible to do what could be done well with his own:—namely, reverse the direction of motion of the piston, and still keep the load scale equally uniform. This was no great objection to the machine as a practical one, because the difficulty had been nearly conquered in the way already explained by Mr. Wicksteed; but from a purely scientific point of view this apparatus was not quite so satisfactory as one in which the indication of the load was taken with equal correctness whether the poise-weight was being run backwards or forwards.

The arrangement of the wire in its course from the test specimen to the indicator barrel was identical with that in the Polmeyer machine



(Professor Unwin.)

which he had seen in 1883 at Dortmund, with the exception of the ingenious pair of radius-links which had been added by Mr. Wicksteed. The principle of those links was absolutely perfect, and consequently no bodily motion of the specimen affected in the least the proper indication of the elongation. In the first machine which he himself had constructed he got seriously involved in the difficulty that the bodily motion of the specimen relatively to the indicator barrel affected the indications. The difficulty in that case had been very serious, because for various reasons he had chosen to put the indicator barrel on the short arm of the poise lever, and when the lever moved up and down there was a good deal of motion to deal with. He therefore found another method of compensation, also perfect in principle, by which the unnecessary motion was got rid of and a true motion was given to the pencil. But by placing the indicator barrel in the right position relatively to the specimen, with diagrams of the size taken by Mr. Wicksteed or of double that size such as those taken by himself, there was not the slightest need of any compensating arrangement such as the pair of radius-links. There was one plane in which the specimen had no transverse motion, but only a small up and down motion—namely the plane of the knife-edge from which the specimen hung; and by leading the wire from the specimen parallel to the knife-edge it was possible to do without any compensation whatever. He had found that, even with diagrams of the size that he took, no movement of the specimen occurring from any movement of the lever produced any measurable distortion of the diagram. He had taken a great number of diagrams of the kind shown in Fig. 17, Plate 9, in which, after having reached a certain load, the poise-weight had been run back, and the stress on the specimen diminished; the pencil then ran down a vertical straight line, as at M and N, which was so perfectly straight that a straight-edge placed against it showed no deviation. Putting the weight on again, the pencil ran up again over the very same line. Any motion of the specimen which affected the diagram would be shown on that vertical line; because, if the motion of the specimen affected the diagram in any way, it affected it in the horizontal direction, that is, it affected the measurement of elongation.

In some scores of the diagrams which he had taken he had never found those vertical lines show the slightest sign of a loop, although they had been traced in the two directions—down and up again. This was a pretty sensitive test of the fact that it was possible to lead off the wire, for diagrams even of the size he employed, in such a way that no compensation was wanted for the movement of the specimen. It would be noticed how at the points M and N in Fig. 17, Plate 9, the diagram showed the rise of the elastic limit beyond the previous load.

Passing from the recording apparatus to the rest of Mr. Wicksteed's paper, he did not absolutely dissent from anything that it contained, but a few things he thought had been stated much too positively. He heartily wished everything stated in the paper was strictly true, because the estimate of the quality of a material would then be rendered somewhat easier than it really was. From many materials the diagrams obtained were not so nicely shaped as that shown in Fig. 8, Plate 4. In many cases flat-headed diagrams were produced, in which the determination of the climax C was more difficult.

Mr. WICKSTEED said that was true; and explained that, in enlarging from the original autograph the typical diagram shown in Fig. 8, Plate 4, the line just preceding the climax C had purposely been drawn rather steeper than ordinary, in order to bring out more prominently what he meant by the climax at C.

Professor UNWIN observed that the method of using as a measurement of the quality of the material the mechanical work done in producing its fracture had been proposed long ago by Mr. Mallet. Mr. Wicksteed had also proposed to take only the work done in breaking the specimen up to the climax C; and had given certain reasons—good reasons as far as they went—for considering that this portion of the work done furnished a better representation of the quality of the material, than if the whole work up to the final rupture were taken into account. That suggestion again was not a new one, having been made several times before. It

(Professor Unwin.)

had been dealt with very fully in a careful paper by Professor Hartig (Proceedings Inst. Civil Engineers, 1884, vol. lxxviii., pp. 462-4), by whom it had also been applied rather fully. In Mr. Wicksteed's paper no figures had been given for showing whether by taking the work only up to the climax C any discrimination was obtained of the quality of a material. It had simply been stated that figures so arrived at would afford such a discrimination, and he himself was by no means sure they would not; but the matter wanted yet a good deal more investigation, and he would give a few figures which he thought might perhaps cause a little hesitation before accepting definitely the method proposed to be adopted as a test of the quality of the material. These figures had been worked out by Professor Hartig, exactly in Mr. Wicksteed's way, for a series of steel bars containing severally the following percentages of carbon:—0·14, 0·19, 0·46, and 0·54 per cent.; showing therefore a pretty wide range in the quality of steel. The work done in breaking those bars, estimated up to the climax C, Fig. 8, Plate 4, in kilogrammetres per gramme of material—which was exactly Mr. Wicksteed's way of calculating, though with different units—came out respectively 1·12, 1·11, 1·11, and 1·14; so that it would be seen that this test of the quality of the material showed nothing whatever. He did not mean to say that such a result would always occur; but it was rather surprising that the work came out so very constant for such very different materials.

Mr. WICKSTEED considered it showed that the stiffness made up for the want of extension.

Professor UNWIN said that was so; but if engineers wanted for a structure steel containing 0·14 per cent. of carbon, they would not like, in consequence of the work done up to rupture being the same, to get one without knowing it that had 0·54 per cent. of carbon.

Other figures, which were perhaps even more startling, had also been obtained by Professor Hartig by estimating the work expended in rupturing a number of different materials. Taking the work done up to the climax C, it had thus been found that phosphor-bronze

was better than mild steel; that whalebone was very considerably better than phosphor-bronze; that raw silk was better than whalebone; and that vulcanised rubber was six times as good as steel.

Mr. WICKSTEED asked whether Professor Hartig's comparison meant weight for weight, or section for section, of the different materials. Would the rope of silk be the same weight per foot run as the bar of steel? Or would the transverse sectional area be the same in both?

Professor UNWIN replied that the work done was given in kilogrammetres per gramme of the material tested, and consequently the comparison was between equal weights of the different materials.

Mr. DRUITT HALPIN considered a great step in advance had been taken by the introduction of the autographic apparatus, although he did not altogether like its form as described in the paper so well as Professor Unwin's, which he thought had less errors of friction and worked more correctly. But in regard to self-recording machines generally and the diagrams they gave, it would be a very great advantage he considered if any means could possibly be devised for producing what might be called a sort of reversal of the diagram represented in Fig. 8, Plate 4, so as to magnify the amount of the horizontal extension within the elastic limit from A to B, and make this portion occupy as great a horizontal length on the diagram as from B to C or from B to F. The subsequent extension, after the elastic limit had been passed at B, was not wanted to be shown on any larger scale than at present; for he regarded the extension from B to F as merely a matter of curiosity, to show what happened when once the elastic limit at B had been passed. What engineers wanted to know was what the material would do during its life, and not any vagaries it might go through after it had passed into its death throes. It was of course a very difficult matter to magnify to so great an extent as he had hinted such extremely small quantities as the minute extension within the elastic limit; but when it was recollected that one-millionth of an inch could be measured by the Whitworth

(Mr. Drnutt Halpin.)

machine and other suitable apparatus, the hope need not be abandoned of accomplishing a matter of such great moment as the magnifying of these minute extensions. That would be greatly preferable to giving a large diagram for the remainder of the test, which was really not wanted.

With regard to the other factor involved in the production of the test diagram—namely the position of the poise-weight on the lever, and the consequent amount of the load—he had seen both Professor Kennedy and Professor Unwin work the lever testing-machine, and they both did it so beautifully as to keep the lever floating. But they themselves would be the first to value any appliance by which the lever could be kept floating automatically. The case seemed to him exactly the same as that of a steam-engine indicator. What would be the use of an indicator diagram if the string were pulled by hand, while watching the motion of the piston-rod and judging by eye whereabouts the cross-head was? The test-recording apparatus he hoped would be made as automatic in this respect as was already the case in the steam indicator.

With regard to the coefficient of mechanical value, referred to in page 35 of the paper, the coefficient obtained by adding together the load and the extension, as proposed by Sir Joseph Whitworth, had been taken up by the German Union. But this was a figure that had never had any value to his own mind, as he did not see what it had to do with the question. If however the extension were multiplied by the load, instead of being added to it, the coefficient so arrived at would possess some value.

As to the length of test-pieces, unfortunately testing-machines were as yet very rare, and when engineers had to test material at works they generally had to do it without the advantage of these machines. It would therefore be much better he suggested if a universal length could be adopted for test-pieces; and to his mind the most convenient length was  $12\frac{1}{2}$  inches, for this reason, that it consisted of one hundred eighths of an inch, and by means of the two-foot rule which every engineer carried he could very easily measure eighths of an inch.

Mr. THOMAS TURNER, referring to Table 5 in Mr. Bennett's paper, considered the results given in the three tests Nos. 1322-3-4, where the round bars had been simply grooved, were very interesting, and showed the necessity for engineers to specify the length of the turned portion which they required to be tested. But the tests here given were from iron bars; and he had had one or two made from steel bars and from composite bars, which would compare with these and show rather different results. The first six tests in Table 5 showed an increase of 32 per cent. in the tensile strength of the bar grooved with a narrow groove at one point only, as compared with the same bar when turned down to the same diameter through a length of 10 inches. In round bars of mild steel, of the same size and similarly turned, he had found the increase in tensile strength by grooving amounted to 43 per cent.; and to 38 per cent. in a composite bar formed of steel with strands of iron running through it for the purpose of obtaining a fibrous character and ease in welding.

Referring also to the last five tests in Table 5, which showed much less elongation and slightly less tensile strength per square inch of original area when the bar was turned down small, it could hardly be expected from the method of the manufacture of iron that tests of iron bars would always give similar results. Iron was not homogeneous, and it might happen that the centre of the pile from which the bar had been rolled had been puddled from a different pig from the outside. But steel was fairly homogeneous, and therefore the comparison of the centre of a steel bar with the outside was fair. He had known cases of iron bars being turned down small, and giving then a higher tensile strength than was obtained before turning; but generally with a smaller diameter of bar the elongation was less, for the reason stated in Mr. Wicksteed's paper, namely that the local extension was less, because of the smaller diameter.

In a mild steel bar of the same size as the round iron bar tested by Mr. Bennett, he had found that by turning it smaller the tensile strength, instead of decreasing, increased considerably as follows:—

Diameter of mild steel bar when turned down, inch	1·31	1·12	0·75	0·50
Tensile strength per sq. inch of original area, tons	24·8	25·9	27·0	28·0
Elongation in percentage of original length, per cent.	28·7	25·2	25·0	25·0

(Mr. Thomas Turner.)

It seemed curious that the centre of a steel bar should thus be found stronger than the outside; and it was difficult to explain the reason. The only suggestion he could offer was that it might be due to the internal stresses set up within the bar during cooling, because of course those internal stresses would be more likely to produce results of that kind in a homogeneous material than in one which was not homogeneous. In testing composite bars similarly turned down he had obtained the following results:—

Diameter of composite bar when turned down, inch .	1.25	1.12	0.75	0.50
Tensile strength per sq. inch of original area, tons .	26.1	26.0	25.9	26.1
Elongation in percentage of original length, per cent.	28.0	27.0	22.4	22.5

In this case therefore it would be seen that there was practically no difference in tensile strength between the centre of the bar and the outside.

Mr. CHARLES COCHRANE considered engineering practice was at present in a transition stage in regard to the use of steel instead of wrought-iron in structures in which it had not been the practice hitherto to use steel, especially in girder work; and he thought it was of the greatest importance to mechanical engineers that they should have a testing machine which could be adopted throughout the engineering world, and on which an engineer in London, for example, could rely to furnish him with trustworthy results as to materials tested at places where he was unable to be present. Hitherto the testing machines had been of very variable make, and had been deemed to be uncertain in their results: so much so that specimens in large numbers—not in tens but in hundreds—had to be sent up from the provinces to London, in order to be tested by machines specially constructed, and accepted as reliable by engineers in London. It was therefore of great importance he thought to know of such a testing machine as Mr. Wicksteed's: so that, whether at works at which the steel or wrought-iron was manufactured, or at works where bridges were constructed of either material, the testing done by such a machine might be accepted as reliable, without the useless expense of sending

to London in order to have the materials tested. One of these machines he believed had been adopted by Mr. Bennett at the Horseley Iron Works, and had been used by him for making the tests described in his paper; and he understood the London engineer or engineers who were concerned in the work that was being carried on there had been perfectly satisfied to accept in London the tests recorded by this machine in the country. A great move he considered had thereby been made; and he hoped one of Mr. Bennett's sons was present to bear testimony to these facts.

Great service he considered had been rendered by Mr. Bennett in establishing the comparative results of punching and of drilling, and of punching and drilling combined. It must be a great source of satisfaction, and must have its due weight with all engineers, to find that steel as well as wrought-iron could be first punched and then drilled, not only without any deleterious influence on the plate or bar, but apparently with a little advantage to its tensile strength. It was certainly very extraordinary that it should be so; but all the results tended in that direction, both with the punching and drilling combined, and with the drilling alone—namely that the tenacity of the material so treated was absolutely a little greater per square inch than that of the original plate or bar out of which the holes were so drilled. Why the material left after the drilling should be stronger per square inch than the same material before the hole was drilled, was a question which he understood had not yet been thoroughly solved, although the matter had been discussed at previous meetings of this Institution as well as elsewhere. No special experiments he believed had been made upon the ultimate section of the material and its extension before fracture; but he thought it would be found that the ultimate section was really greater when buttressed up or supported by adjacent parts, as referred to in page 46 of Mr. Bennett's paper, than it was when the material was allowed to be equally subject to extension throughout the whole length of the piece tested. Nevertheless it seemed puzzling to explain how a round bar having a little groove turned round it would carry more per square inch before fracture than if the whole bar were turned down to the same



(Mr. Charles Cochrane.)

diameter as the groove. This was a question which he hoped at a future time some one would be able to solve, as he believed it had not yet been solved completely.

Mr. HERBERT B. S. BENNETT said he could confirm Mr. Cochrane's statement in regard to the use of one of Mr. Wicksteed's testing machines at the Horseley Iron Works with the autographic apparatus; and it certainly did give most valuable results. The whole of the tests contained in the tables forming his father's paper had been carried out in that machine; and he exhibited an extensive collection of the autographic diagrams taken in these tests as well as in many others.

Mr. THOMAS TURNER suggested that the reason of the increase in tensile strength consequent upon drilling a hole through a test bar was the same as the reason of the increased tensile strength obtained when the bar was notched down in width along each edge through a length equal to the diameter of bar: namely that the length actually tested was thereby shortened. And as to why the tensile strength should go up in consequence of shortening the length upon which the test was taken, there was a suggestion in page 46 of Mr. Bennett's paper that the tensile strength was increased in the drilled bars because of the support which was received from the extra section that was so close to the point of fracture; and with this explanation he was disposed to agree. The increase in tensile strength he therefore considered was not a question of drilling, or of punching first and drilling afterwards; it was simply because the length really tested was thereby so greatly shortened.

Professor ROBERT H. SMITH thought a proof of the skill with which the design of Mr. Wicksteed's autographic apparatus had been worked out was supplied in the statement that according to careful measurement the proportion between the rise of the pencil upon the diagram and the actual increase of load was found to be constant. If indeed that proportion were accurately and absolutely constant, it would show that all the difficulties of this very complicated problem

had been perfectly got over; but inasmuch as in any testing or experimental work the results rarely came out with anything like absolute accuracy, it would be interesting to know within what degree of accuracy the proportion was found to hold, or what was the magnitude of any small errors which might have been observed. In the means here adopted for moving the pencil in proportion to the load, it seemed to him that an unnecessary difficulty had been dealt with. The plan of merely connecting the pencil direct with the moving jockey-weight was not only much simpler, but he thought for all practical purposes it was also sufficiently accurate.

The most ingenious part of the author's arrangement was in his opinion the rotation given to the small ram in order to overcome its friction through the gland, which appeared to him to be a device of very high merit for overcoming friction at such a joint. The fact being proved that the difficulty of friction was thereby practically got rid of at that particular part of the mechanism, the question arose why this result was obtained. The explanation that occurred to himself was the following. Friction was a force which always opposed motion; and the direction of the frictional force was exactly opposite to that of the motion. Now in the gland there was only a certain amount of frictional force to be overcome. The author's device was to make the direction of the motion almost wholly circumferential, and therefore the frictional force was almost wholly in the circumferential direction also. Consequently the resolved component of the frictional force in the longitudinal direction was extremely small; and therefore the rotation of the ram enabled it to move endways readily, in response to the slightest variation of end pressure upon it from the fluctuation of the load.

The mechanism consisting of the horizontal and vertical radius-links for conducting the wire from the test specimen to the indicator barrel, so as to rotate the barrel in proportion to the extension of the specimen, was mathematically perfect. But from a practical point of view he was a little afraid lest the friction of the wire in passing over the three pulleys that intervened in its course might involve some slight variable extensions of the wire, which might be inconvenient; and he should like to know whether any measurable

(Professor Smith.)

error had been found to arise from that source. Having lately been using very fine wires for similar purposes in recording instruments, he had found the friction of the pulleys so great in comparison with the extensibility of the wires that he had everywhere endeavoured as far as possible to do without any pulleys at all, and to lead the wire direct in a straight line from beginning to end of its course.

The final portion of the autographic curve, from C to F in Fig. 8, Plate 4, he agreed with the author in thinking was of very little value. This portion of the curve, so far as he knew, had first been shown distinctly some nine or ten years ago by Professor Thurston's autographic apparatus for twist tests. In that apparatus he considered the character or shape of this part of the curve was chiefly due to the character of the mechanism. Reference had been made in the paper to the difficulty of keeping the lever floating during this part of the curve; but even though in some cases it was found possible to do so, yet the mere fact of the lever being kept floating did not show that this part of the curve was really characteristic of the material; and he thought it was much more characteristic of the testing machine than of the material which was being tested. In order to keep the lever floating, the jockey-weight had to be run back along the lever at a certain definite rate corresponding with the rate at which the water was being pumped into the main hydraulic cylinder; and if the water were pumped in at a slightly fluctuating rate, the immediate result would evidently be to throw the lever against the upper or the lower stop, if the jockey-weight continued to be run back at the same rate as before.

Mr. WICKSTEED explained that it was essential the water should come in at a uniform rate. The pump was worked from the main shaft driven by the engine, which was governed; and it might be assumed as true that the water was throughout coming into the pulling cylinder at a uniform rate, no matter whether the resistance was great or small.

Professor SMITH said in that case, if the jockey were run backwards along the lever at a rate varying from that needed to

maintain equilibrium, while the water was pumped in at a uniform rate, the result would evidently be still the same, namely to throw the lever against the upper or the lower stop. Therefore the shape of the curve from C to F, in Fig. 8, Plate 4, depended on the rate at which the jockey was run backwards relatively to the rate at which the water was pumped in. A similar case was that of a small testing machine used by himself, in which the test-piece was pulled by means of worm gear and screw, and the pull was conveyed to it through a large spring-balance, indicating up to nine tons only. When the final stage was reached, there was of course in that kind of apparatus very great difficulty in keeping the full force up while the test-piece was yielding so much faster. In order to keep the full tension upon it, there would be required practically unlimited power of varying the speed of rotation of the screw by which the test-piece was pulled. As a matter of fact in most experiments he had found it impossible to keep the pulling force always up to its original amount; and the proportion between the continually increasing extension of the test-piece and the slightly decreasing magnitude of the pull was evidently a function, not only of the increasing extension of the piece itself, but also of the decreasing length of the spring through which the pull was conveyed. Exactly the same thing occurred in the apparatus which had been shown by Professor Kennedy. The character of the curve drawn by that apparatus beyond the climax of resistance must depend upon the modulus of elasticity of the strong spring-piece S in Figs. 13 and 14, Plate 7. The final portion of the curve from C to F in Fig. 8, Plate 4, he therefore thought was of very little value indeed as a practical indication of the character of the material. The same remarks applied also to the drop in the diagram immediately after the elastic limit had been reached at the point B in Fig. 8; this drop he considered to be characteristic of the design of the testing machine and of its manipulation only, and not characteristic at all of the nature of the material tested.

It was certainly a most interesting and important fact which had been stated by Professor Unwin, that the mechanical work expended in producing rupture, as represented by the area of the autographic

(Professor Smith.)

diagram, was the same for different specimens of steel with such different degrees of carburisation. While thinking therefore that it was highly desirable to record the quantity of this mechanical work, he altogether agreed with Professor Unwin that by itself it could never be looked upon as a complete test of the character of the material. It was necessary to see the actual curve itself throughout its whole length, and not merely to know its area, in order that the extensibility and other characteristic qualities of the material might be completely understood.

In reference to the assumption (page 39) of uniform plasticity up to the climax of resistance in a test sample, its real meaning, as stated and used in the paper, was simply that when the sample was elongated its volume remained constant. This however was not supported by the most careful experiments that had been made upon that special subject. The experiments of Wertheim showed that the volume always increased under a tensile stress in the case of the three materials on which he had experimented, namely iron, brass, and glass. It seemed to himself that all materials would become greater in volume when they were subjected to a tensile stress; but he could not say that this was the necessary result in all. There might be some special materials which might become contracted under tension, in consequence of the ratio of contraction in area being greater than the ratio of elongation; but he believed that in all the plastic materials which engineers had to do with for constructive purposes the volume really did become greater under tension.

At the Mason Science College, Birmingham, he was using an apparatus for testing the rate of flow of specimens under a constant load, that is, the relation between the increase of strain and the time that had elapsed after the constant load had been put on: this relation being one which he considered of great importance, and which had not yet been sufficiently investigated. The strain was communicated from the specimen by means of a very fine phosphor-bronze wire to a pencil, which marked a curve upon a piece of paper mounted on a drum; and the drum was made to revolve at a uniform rate by the weight of a plunger in a small cataract filled with

glycerine; by closing or opening more or less the cock of the cataract the rate of rotation of the drum could easily be regulated to any speed that was most convenient for the special experiments in progress. This plan was more convenient than ordinary clockwork, because of the great facility with which the rate of motion of the paper could thereby be varied.

Mr. JOHN A. F. ASPINALL said he had been using one of the 50-ton testing machines made by Mr. Wicksteed's firm, and had been trying some experiments in the same direction as the author for the purpose of getting an autographic test-recorder. The arrangement he had devised, as shown in Figs. 20 to 25, Plates 10 and 11, consisted simply of a horizontal indicator barrel D, carried upon two parallel horizontal bars B, so as to permit it to slide backwards and forwards; and a pencil P was similarly carried in a slide mounted upon the same parallel bars, so that the pencil could travel either forwards or backwards along the indicator barrel, irrespective of the independent longitudinal movement of the barrel itself. The pencil was connected by means of a cord or wire L with the lower end of the test-specimen S, and the barrel was similarly connected with its upper end by another cord U. The rotary movement was given to the barrel by means of a cord R from the poise-weight W, through the intermediate reducing gear G. This arrangement certainly seemed to give a very nice and apparently an accurate diagram, as shown full size in Fig. 19. Of course as soon as the climax of resistance had been reached at the point C, there arose the objection which had been pointed out by the author at the end of the paper, namely that there was a certain amount of difficulty in winding the poise-weight back by hand so as to keep it exactly in the proper position in relation to the work that was then going on in the specimen, and thereby to keep the lever floating. But it was a question in his own mind whether this final portion of the diagram from C to F was really worth having at all. Surely when the point had been reached at which the specimen might be said to yield absolutely and finally, any further information was scarcely wanted, at any rate for ordinary purposes. In the last part of the curve, from

(Mr. John A. F. Aspinall.)

C to F, was simply recorded what went on in the specimen after it had become like an attenuated streak of treacle. The apparatus he had described had been found to do very well for enabling test-diagrams to be taken rapidly in testing pieces of boiler plate; and it certainly had the advantage of being uncommonly cheap and simple.

Mr. THOMAS W. TRAILL remarked that, after so much had already been said about the very neat autographic instrument described in Mr. Wicksteed's paper, he would merely express his own opinion that it was an apparatus of which the author need not be ashamed. The testing machine to which it was applied had proved a very useful one to steel manufacturers. It was a machine which could do its work quickly, although perhaps doing it too quickly might not do fair justice to the material. That however would not be the fault of the machine, but of the person working it, and wanting to get too much out of it.

With regard to the paper by the late Mr. Bennett, he fully shared the sorrow felt by all the members at the event which had prevented their having the benefit of the author's presence on this occasion. Had Mr. Bennett been spared, he thought it more than probable that he would have supplemented his paper with a few further remarks to pretty much the same effect as those which it now occurred to himself to make. From the tests recorded in Table 7 the conclusion was drawn that the contraction of area was less in short specimens; and no doubt this was the fact when the length tested came down to 1 inch. From 10 inches down to 4 or 5 inches length it would be seen that the contraction was pretty constant at about 48 to 50 per cent.; and only when the tested length was reduced to 1 inch did the contraction fall to 39 per cent. On this point engineers who were not experienced in testing might come to a wrong conclusion; for although it was certainly most essential that test-specimens should be prepared with due care and be properly proportioned, yet it was evident from these particular results that no practical error would accrue from taking the contraction when the length was about 5 inches instead of 10.

Nevertheless he was himself a strong advocate of the 10-inch length, as used by the author, instead of the 5-inch or any other length less than 10 inches. The 10-inch length had many advantages, and was a good English standard length; moreover it required no calculation to get the percentage of elongation, thus saving both the time and the trouble of calculating and also the consequent risk of error. It was desirable that as far as practicable a uniform length should be adopted for test-pieces, for enabling comparisons to be made; and a 10-inch length was that which he thought most English engineers of experience would prefer. Too frequently the percentage of elongation was mentioned, without the length in which it was taken being stated; such information was not only of no value, but it might be misleading. For an elongation of 25 per cent. in 5 inches was inferior to the same percentage in 10 inches, because the elongation was both general and local, and the local elongation was nearly a constant quantity. Therefore if the total elongation was 25 per cent. in a test-piece 5 inches long, and was also 25 per cent. in another test-piece of 10 inches length, this did not indicate that the two pieces were equal in ductility; on the contrary it showed that the material of the 5-inch piece was inferior in ductility to that of the 10-inch piece. Possibly some of those who adopted a short length for the test-pieces might like to adhere to that practice, because a high percentage of elongation sounded well. When the test-piece was 10 inches long, the elongation could be measured near enough with a two-foot rule, because no practical engineer would cavil at a trifling variation of only 1 per cent. less or more in regard to the elongation, or a total variation of only 2 per cent.

Another point to which he thought the author would probably have drawn attention more fully than he had done in the paper, if he had extended his experiments a little further, was the injurious effect of the punch. It was true that the tests recorded in Table 2 showed a loss of 6 per cent. in the tensile strength of mild steel bars after punching; and certainly even 6 per cent. was a loss which engineers did not like to incur. But coming to the thicker steel plates which he had himself had occasion to test, he was sorry to say that the loss through punching was not confined to 6 per cent., but was something



(Mr. Thomas W. Traill.)

like 25 per cent. or more, which was a very serious thing indeed. Moreover not only was there so great a loss in tensile strength, which if known might be provided for, but in addition the steel was so injured by the effect of the punch that in the neighbourhood of the punched hole it became brittle and like glass: so much so that there was no reliance to be placed upon a piece of steel after such a barbarous tool as a punch had been through it.

Mr. ALFRED A. LANGLEY desired to confirm the remarks of Mr. Traill about punching: it was a barbarous process, especially when applied to rails, for which it was much in vogue for putting bolts through them for the fish-plates. In his own experience he had had an immense number of rails with their ends broken, and nearly always broken through the punched hole. He had accordingly caused them to be drilled with an oblong hole; and since then no rail ends had broken. It was a favourite saying that punching formed a sort of test for the rails; but it was so poor a test that it was of very little consequence compared with the importance of using rails that could be depended upon.

With regard to the autographic registering apparatus on the testing machine, he had himself been using extensively an autograph pen for recording the movements of a railway carriage both longitudinally and transversely. The pen made a mark on a paper disc, which was caused to revolve by means of a clock; and every movement of the carriage, whether lengthways or crossways, was thus registered automatically. After each hour the paper disc was taken off, and the pen went on recording upon a fresh paper every movement caused by the imperfections of the road and of the carriage.

Mr. HENRY ROBINSON asked how it was that, with a total pressure of  $52\frac{1}{2}$  tons on the main piston in the author's testing machine, the margin of  $2\frac{1}{2}$  tons, or only 5 per cent., was sufficient to overcome the friction of the hydraulic leathers surrounding both the piston and the piston-rod; whereas for the small indicator-ram the friction of the gland alone had been given as one-sixth of the total pressure on the ram, or 16 per cent.

Mr. WICKSTEED replied that the difference was owing to the very small size of the gland, which made the friction greater in proportion to the area of the indicator-ram.

Mr. E. H. CARBUTT, M.P., recalled the time when the quality of a piece of iron used to be judged entirely by the sight; even for the best Yorkshire iron the only mode adopted was to have it broken and to judge by the fracture whether it was good or bad. Those days had gone by, and at the present time every ironworks had its own chemist for analysing the composition of the iron or steel manufactured. Apart from chemical analysis, Mr. Wicksteed's excellent machine now gave a mechanical analysis, of which every detail was accurately recorded in autographic diagrams. The question of both chemical and mechanical analysis he considered was one of the utmost importance. A subject was at the present time before the government, which was of vital importance to the country, namely the defective bayonets in the army. It appeared that the bayonets supplied to the troops had been defective to the extent of one-third. If the troops went into battle and one-third of them were not able to do their duty because their bayonets were defective, what a loss of strength that would mean to this country, or what a cost it would be to have an army one-third bigger than would otherwise be necessary. If a supply of proper bayonets were ensured by the aid of chemical and mechanical analysis, there would be no such trouble as had been experienced in the Soudan war. It was therefore of the utmost importance that mechanical engineers should give their best attention, as Mr. Wicksteed was doing, to the means of obtaining all possible information concerning the qualities of the different kinds of steel they were using.

In reference to the drop shown in the autographic diagram, Fig. 8, Plate 4, at the point B where the elastic limit was reached, he did not quite understand why the line after rising up to 17 tons at that point should then go down again to 16 tons, unless it was that the skin resistance was equivalent to a certain amount of hardness, and when that skin resistance had gone, a certain amount of strength had gone with it.

Mr. WICKSTEED said that in Mr. Bennett's paper there was one statement which he thought deserved special attention, because he believed it was the first time it had been put forward in any authentic manner: namely that a test-specimen pulled by grips had actually done a little better than a similar specimen prepared with enlarged ends and with holes bored through them for pulling by pins. It had often been maintained that in order to get the best results it was necessary to make an extravagantly expensive specimen, beginning with a very wide strip for the sake of getting large ends with pin-holes through them, and then reducing the width very greatly indeed over the portion between the datum points. In the discussion upon his former paper on this testing machine (Proceedings 1882, pages 402-3) he had expressed the opinion that, if the machine itself was in perfect alignment so as to induce no cross strains upon the sample, there could not be a more favourable method of holding the sample than by ordinary parallel grips. This view he was very glad to see substantiated by Mr. Bennett's experience in the use of one of these testing machines specially designed for testing by means of grips. It was only if a testing machine was used of rude construction, and it was wanted to make a sort of universal joint for the sample to adjust itself upon in default of the true alignment of the machine, that there was any advantage at all in having enlarged ends with big pin-holes through them. With grips swivelling in grip-boxes, all tooled and properly fitted in alignment in the machine itself, no cross strains were induced in the sample, and it was pulled true in parallel lines from top to bottom by the grips closing upon its ends; and the ends themselves were either exactly the same width as the part between the datum points, or they were only a very little enlarged.

Passing to the remarks which had been made upon his own paper, the question had been asked why the autograph line in the diagram Fig. 8, Plate 4, descended at B after the elastic limit had been reached. He had not the remotest notion, and could not explain it in the least; and he doubted very much whether any satisfactory explanation could yet be given. Of the fact of the drop he was thoroughly convinced, and he thought the explanation would

ultimately be found in the molecular behaviour of the material. Special attention was being devoted to that particular part of the diagrams by Professor Unwin, by whom he had no doubt that in the end the matter would be fully elucidated; but it was so far from being understood at present that the drop had been spoken of by Professor Smith as a function of the machine only, and not of the specimen tested. In direct opposition to that view he himself maintained that it was not a function of the machine, but that it was a true record of the molecular stress in the specimen. This he maintained on the grounds that in the very same machine he had tested specimens of copper, of brass, and of delta metal, from which diagrams were shown in Figs. 10 to 12, Plates 5 and 6; and it was seen that the line ran up as smoothly and steadily from the elastic limit at B as in any other part of the diagram. Moreover occasional specimens of wrought-iron or mild steel would draw a straight horizontal line from the point B through two-tenths of an inch extension in a specimen ten inches long, as shown in Fig. 9, Plate 5; and sometimes this horizontal line would be ruled perfectly straight, with no zigzag, no depression, no irregularity. With yet other specimens of wrought-iron or mild steel the line on passing the elastic limit would start off at once in a rising curve, without making any horizontal jump at all. With Professor Kennedy's beautiful instrument he was delighted to find very good diagrams which showed the same thing. Although that instrument looked so different from his own apparatus, the two were each based upon essentially the same principle. In Professor Kennedy's the stress was measured by the elasticity of a straight steel bar; in his own it was also measured by the elasticity of a steel bar, but here the bar instead of being straight was coiled round and round into a helical spring. The consequence was that the spring of 15 inches length was composed of a bar that was three times 15 inches long; and he thought the molecular action on the bar when in the form of the spring was of the most favourable kind possible, being of the nature of torsion. As pointed out by Professor Kennedy, it did not follow that, because the straight bar in his instrument gave a true elastic stretch, that is an equal amount of stretch with an equal increment

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of load, therefore every straight bar did so ; and neither did it follow that, because this coiled spring gave a correct scale, therefore every such spring would do so. All he need say was that there was here a spring at one end of the apparatus, and at the other end a steel-yard which was absolutely correct, and against which therefore the spring was checked throughout its entire range. If any spring was found not to give a correct result in agreement with the steel-yard, it could be rejected and another tried. Hitherto he had been fortunate, having got springs of the same character as were used for a Salter's weighing machine ; he had not had to reject any spring, and in subdividing the height of the test-diagrams in the manner already explained, he had found that the lines which ought to coincide did really cover each other. What the degree of microscopic accuracy was he did not know ; but as the scale was a small one, he considered it might be reckoned to be correct well within a quarter of a ton. Alike in his own apparatus therefore and in Professor Kennedy's the stress was shown by an elastic bar, which in the latter would bear a very great stress, but gave very little motion ; and accordingly in that case, in order to produce a diagram, the motion had to be multiplied perhaps a hundredfold, the point of the pencil that pressed upon the smoked glass having probably a radius of something like a hundred times that of the cylindrical needle which was rotated by the silk cord as the bar extended. Corresponding with that, in his own apparatus the spring gave all the motion for producing a good-sized diagram without multiplying gear ; but the spring itself was pressed upon by a stress which, by the difference in the area under water pressure, was reduced to one-fiftieth of the stress coming upon the specimen ; and this was the distinctive feature in his own plan, which enabled a manageable spring of ample range to be used, instead of a strong bar giving only microscopic extensions. Still the two instruments he considered were based on the same principles, though his own application of those principles was kept free from any delicate multiplying gear involving the use of a smoked glass for minimising the resistance of the trace, and straight lines were obtained for the ordinates in the diagrams.

In respect to the friction on the wire transmitting the extension of the sample to the indicator barrel, he had found it was a very great difficulty indeed to catch the very first microscopic extension that took place in the specimen. To have succeeded in doing so he considered a very great feat, because the friction of motion was different from the friction of rest; and although there was a weight hung on each end of the wire, yet, as soon as the specimen began to stretch, the wire's first tendency was to straighten itself a little, and not to transmit to the indicator barrel that first extension of the specimen. But as the lines upon which the whole of the apparatus had been constructed were not at all delicate, the whole being made very strong, the wire was infinitely fine in proportion to the strength of all the other parts. The weight hung at each end was about as much as it would bear. The response was certainly very prompt; with good eyesight a horizontal departure could be traced in the extension line from the very first ton of load. There was no loss of motion, because the barrel was kept up by the counterweight hanging below it; and as soon as ever the specimen began to extend, the barrel responded. This was one of the difficulties that had had to be tackled. It was true that all the friction which could be eliminated from the system would increase the perfection of transmission; but convenience in use had also to be studied, and in spite of the three pulleys and the angle of  $180^\circ$  the transmission as arranged had approached wonderfully near perfection in actual practice. While fully appreciating however the value of Professor Smith's observation on this point, he would remark that the principle of the compensating pulleys would equally apply if the wire were led almost straight from the sample to the indicator barrel. It was immaterial that there should be  $180^\circ$  between the two ends of the wire;  $90^\circ$  would do, or even  $10^\circ$ , if only there were angle enough to allow for the general movements of the sample. The great advantage of having got a transmitting gear theoretically right to begin with, was that then considerations of convenience could be allowed to determine the position of the indicator barrel, and a little more work could be put upon the transmitting gear with impunity.

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With regard to the question which he was very glad Mr. Robinson had raised about the friction, it certainly did look at first as though there were some mistake. But the figures given in the paper as to the friction of the indicator-ram were the result of actual observation. Its friction had been found really to form as large a proportion as one-sixth of the whole pressure on the area of the ram. Being  $1\frac{1}{8}$  inch diameter, the ram had an area of only 1 square inch against a circumference of  $3\frac{1}{2}$  inches rubbing in the gland. But the main hydraulic piston of 9 inches diameter had an effective area of 50 square inches against a joint total circumference of only 40 inches for the piston rubbing in the cylinder and its rod rubbing in the gland. It was thus clear that the proportion borne by the friction changed entirely as the size of the ram changed: so that, whereas in the main cylinder the combined friction of both the piston and its rod was approximately only one-twentieth or 5 per cent. of the whole pressure on the piston, yet in the gland of the indicator ram it amounted to as much as one-sixth or 16 per cent.

To his intercourse with Professor Unwin for some years past he was greatly indebted for many of his own ideas in connection with this apparatus; and also for some of the details, which had proved very useful. Among the latter were the small clips which had been shown to him at Cooper's Hill for attaching the wire to the sample tested; and these were what he had adopted for the purpose. They had a vertical knife-edge on one side, and the points of two set-screws on the other side. The vertical knife-edge steadied the clip from tilting or skewing vertically, and the two screw-points steadied it from skewing round horizontally. So that with a reasonable amount of nipping to begin with, and a spring that followed up and closed upon the specimen as it became attenuated, a firm clip was obtained, which preserved its horizontal position.

The recording apparatus described by Mr. Aspinall was a very beautiful arrangement; and was of the same character as Professor Unwin's, with some exceptions. In one respect he thought that Professor Unwin's was better, on account of its being free from the sag of the long horizontal wire; for in Mr. Aspinall's the wire was dragged out horizontally by the poise-weight as it ran out along the

lever, and there would undoubtedly be some little sag in the wire when the poise-weight was getting towards the end of the lever of 16 or 20 ft. length. In Professor Unwin's apparatus the sag of the wire was obviated by taking the motion from the poise-weight in another way, which answered the same purpose and was perhaps subject to less error. But anyhow Mr. Aspinall's recording apparatus he considered was certainly a very great success, and the diagram shown from it in Fig. 19, Plate 10, was identical with his own diagrams; only just at the very end it seemed to have missed the final record at the moment of rupture.

There was one great difference that occurred to him to point out in comparing the arrangement described in the paper, which was his own notion of what a self-recording apparatus ought to be, with Professor Unwin's and Mr. Aspinall's: namely that theirs could not do what Mr. Walker had said that his own did—act the part of a policeman. Those machines could really be made to draw whatever curve was wished, by simply travelling the weight along the lever, irrespective of whether the sample supported the lever or not. It was quite enough to know that the diagrams were drawn by Professor Unwin and Mr. Aspinall themselves, to feel satisfied that they were correctly drawn; but it would not be enough to know that a diagram was drawn with the same apparatus, if that apparatus was sent into the country to take the place of an inspector. In this respect he thought the perfectly self-acting nature of his own apparatus, which could not be coaxed into making a line in any way different from the actual water pressure that was putting the stress upon the sample, constituted so far a practical advantage. Still he considered Professor Unwin might perfectly well argue that in his own hands he should place every reliance upon his own apparatus, because it carried with it a check, ton by ton: so that he should know absolutely that, when the poise-weight was pointing to 25 tons, the connecting mechanism must be marking 25 tons on the diagram; and it did not require anything further than one trial of the mechanism to prove this. The remarks of Professor Smith, as to the position of the poise-weight affecting the curves of the diagrams, might apply to recording apparatus on this principle; but had no application to his



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own, which drew its curves wholly irrespective of the behaviour of the lever.

The seemingly recondite question of the inertia of the moving parts in the testing machine had not really to be taken into consideration at all. For it must be borne in mind that the rate at which the water was entering the pulling cylinder was maintained uniform throughout, as a necessary consequence of the construction of the machine. The pump was not a plunger pump, but a screw pump. The screw being revolved by belt and gearing was steadily forcing forwards a horizontal ram, which in its turn was forcing the water at one uniform rate into the pulling cylinder. Whether the stress on the sample fell off towards the end, or whether it was maintained, made no difference in the rate of advance of the pulling piston, which inevitably went on advancing, whether it met with no resistance at all, or with the maximum resistance. Therefore the inertia of the pulling piston, the inertia of the counterbalance tail-weight, and the friction of the pin on which the tail-weight was hinged, were all constants—not a constant percentage, but a constant absolute amount, this being the initial friction spoken of in page 31 of the paper as being balanced by the initial pressure put upon the indicator spring. The water column in the pipe making fluid connection between the main cylinder and the indicator cylinder was almost stationary, the pipe having a good sized bore; and there was nothing small about the testing portion of the machine. At any rate the pencil was so lively that sometimes it dropped faster than it could be followed by eye, when a test-piece gave out in stress; and at the breaking of the sample the indicator-ram went home like a shot.

The PRESIDENT understood there was no moving back at all of the poise-weight along the lever during any part of the autographic diagram from the commencement at A to the climax at C in Fig. 8, Plate 4. If therefore the lever was kept floating during the whole of that period, and did not go down upon its lower stop when the line sank after reaching the elastic limit at B, the curve from the point B to the same level on the opposite side would have to be made almost instantaneously.

Mr. WICKSTEED replied that it was not so quick as that, and the lever did sometimes go down on the stop at that part of the diagram; but this recorder was not dependent on the floating of the lever. The width of the gap at B when testing mild steel was generally two-tenths of an inch in the 10-inch specimens. The rate of motion might be timed to any speed that might be thought proper, and substantially the same result was still obtained; the rate might be so slow that it took a minute to travel through that extension of two-tenths of an inch, and the pencil would then gradually creep down the drop and up again. In order however to dispel any question as to whether the autographic curves were at any part a function of the machine—whether they were due to the dynamical effect of the moving poise-weight at the end of the lever—he had taken a good many diagrams with the poise-weight stationary at the far end of the lever, so as to keep it down, over-balancing the pull on the sample, and keeping perfectly stationary the whole mechanism from which the sample was held, thereby leaving the water to tell its own tale without any floating weight; and in all cases he had got substantially the same curves.

There was no difficulty, such as Professor Smith had experienced, in keeping up the pulling force; for the sample did not yield any more than there was pulling force to make it yield; and the curve was a record of the yielding of the sample, together with the amount of pulling force exerted simultaneously with that yielding. The pull in this machine was at a uniform rate; but when the sample began to give out after having passed the climax of resistance, then to increase the rate of pulling, as suggested by Professor Smith, would only make the sample yield the faster, and the character of the curve would remain the same.

The observations of Professor Smith incidentally pointed to the desirability of keeping the steel-yard free from motion; and in this he himself entirely concurred, believing it to be an important feature of his apparatus that it enabled testing to be conducted entirely free from the influences of a moving steel-yard, and also that it would act more instantaneously than the adjustment of a poise could be effected. Where one of these recorders was employed, the testing operation as he recommended it to be performed would become entirely automatic.

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The poise-weight would be at the extreme end of the lever, where it would hold the lever down at rest. The recorder would produce a trace on scaled paper which was absolutely reliable, inasmuch as that very scale had been constructed from the steel-yard itself, as described in the paper, and could be verified from the steel-yard every day or every hour or at every test if required.

The adjustments of this indicator were not in the least degree delicate or troublesome. In fact there was only one adjustment to make at any time, and that was to screw up the indicator spring till its pressure balanced the initial resistances of the machine, which were all constants. This adjustment could be tested for accuracy, as described in the paper. Many thousands of diagrams had been made where these recorders were in operation at various works, and no part of the apparatus had been found subject to derangement. The effect of combining this recorder with any testing machine was that a scale of load was thereby obtained at each end of the system: at one end it was a steel-yard lever and dead weight; at the other end it was a water lever and a spring. The action of the one was as reliable as of the other, only that the value of the water lever and spring could not be proved except by the application of dead weight; and hence the need of the steel-yard for evaluating the net stress resulting upon the sample from the motion of the water lever and the spring.

In regard to the use of the diagram when made, it had been remarked by Professor Kennedy that the proposal to measure the extension upon a length which did not include the short portion near the place of fracture was of course an old one; and it had been mentioned by Professor Unwin that this point had been dealt with very fully by Professor Hartig. The importance of that method was thereby confirmed, and the diagram made by his own apparatus furnished the means of carrying out that method with exactness and rapidity. In the latter part of the paper it was shown, he believed for the first time, how the diagram could be cut off as with a knife just where the local extension began; and why it was that localised extension was certain to be found concurrently with a falling load. It was quite true that, by giving the value of materials per cubic

inch in a single column of figures, it was only the quality termed "resilience" that would be compared; but this was a valuable comparison to make, and it could be made very readily by the information which an autograph diagram afforded. The suggestion he had offered in page 38 of the paper—about presenting in a single column of figures the essential comparison as to quality of material in all tests, however diverse—had never been intended to be applied to anything but metals, which alone had been treated of in the paper. The results quoted by Professor Unwin as obtained from silk and india-rubber and other materials, and estimated in work done per gramme of weight of the material tested, should at any rate be considered be translated into the corresponding work done per cubic inch of volume, in order to make the comparison apply fairly to the suggestion as offered in the paper. Of course the other qualities could also be obtained from the autograph diagram, namely the first permanent set, the maximum load, and the percentage of extension; the record of these qualities it was by no means desired to eliminate, but on the contrary to ensure the accurate registration of each one of them. For with a practised eye every quality relating to the strength of the material could be realised at a single glance from the configuration and the scale of the record.

Mr. L. STERNE had been hoping Mr. Wicksteed would be able to give some explanation of that drop in the diagram after reaching the elastic limit. In 1876, when Messrs. Hoopes and Townsend of Philadelphia first punched through  $1\frac{3}{4}$  inch thickness of cold iron with a  $\frac{3}{8}$ -inch punch, they obtained diagrams of precisely the same character, which they attributed to the flow of the metal under the continuous load on the punch. By thus introducing the element of time, giving the punch only such a load as it could comfortably sustain, the same laws appeared to hold good both for compression and for tension up to the elastic limits.

The PRESIDENT was sure the members would agree with him that they had had two exceedingly valuable papers, and that the discussion which had followed had been most interesting and instructive. No

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one could have designed the machine which had been brought before them by Mr. Wicksteed unless he was a good mathematician, a good physicist, and a good mechanic; and certain it was that to overcome the difficulties presented must also have called for the exercise of no inconsiderable amount of perseverance and pertinacity.

At the same time it should be remembered that, although engineers had here a machine which enabled a test specimen to write its autobiography, there was some danger of their relying rather too much upon its evidence. It had been aptly likened to a policeman, an officer who needed strict supervision by the magistrate. The autographic diagram might tell the whole story of a particular test-piece; but it did not necessarily tell the whole story of the quantity of iron from which that test-piece was selected. A short time ago when visiting Mr. Kirkaldy's testing works he had been shown by him a large heap of test-pieces, which had been sent to him from the country by various inspectors for testing, but which he had declined to test because he considered they were unfair. Either they had little defects in them, or they were cut out of line, or in other ways he considered them not fair samples to test. These considerations should always be borne in mind. Both Mr. Wicksteed and Mr. Bennett had pointed out in their papers how very great a difference there was in the evidence given by test-pieces of different lengths, of different diameters, and of different shapes. All these things had to be taken account of; therefore testing machines of this kind should be valued only up to a certain point. To put the thing in a homely way—too big a sermon ought not to be preached from too little a text.

He had now the pleasure of proposing a vote of thanks to Mr. Wicksteed for his valuable paper; and also to Mr. Herbert B. S. Bennett, Mr. Henry S. Bennett, and Mr. Charles S. Bennett, sons of the late Mr. Peter D. Bennett, for their kindness in completing their father's posthumous paper and allowing it to be read at this meeting.

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