THE HARDENING OF SCREW-GAUGES WITH THE LEAST DISTORTION IN PITCH.

(REFERRING TO OIL HARDENING.)

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(Revised by Mr. G. T. WHITE, B.Sc., of the Goldsmiths' College, London.)

The first Paper was complete in itself. A quenching temperature of 700°C . ($1,292^{\circ}\text{F}$.) was indicated as the position of smallest stable distortion in length under the conditions carefully stated. There were six sets of experiments (Experiments 1 to 6), and the time of casing in cyanide was about half-an-hour, calculated to produce a case of about 0.003 inch. It should be noted that the experiments

FIG. 15.—Gauges used in Experiments.

For Figs. 16 to 19, 24, 24A, 25 and 25A.

For Figs. 26 to 29.

did not actually prove more than was claimed in the Paper, and nothing was made evident regarding any other possible conditions. Continuing his experiments, the Author subsequently found other important results, which are now described in this second Paper.

Experiment 7 (22nd May 1918).—An actual screw-gauge was prepared to the dimensions in Fig. 15A. This was screwed in the lathe to 0.0002 inch minus in total pitches and to the diameters

FIG. 16.—Experiment 7.

Cased for 2 hours in box, hardened in water from 695° C.

F.—Front. M.—Middle. B.—Back.

FIG. 17.—Experiment 8.

Cased for 1 hour in cyanide, hardened in water from 695° C.

FIG. 18.—Experiment 9. Cased for 1 hour in cyanide,
hardened in water from 695° C.

FIG. 19.—Experiment 10. Cased for 1 hour in cyanide,
hardened in oil from 695° C.

shown in the chart, Fig. 16. It was cased in barium carbonate and bone black for two hours at a temperature estimated at 800° to 850° C. ($1,472^{\circ}$ to $1,562^{\circ}$ F.). It was immediately afterwards transferred to the cyanide bath, which stood at 770° C. ($1,418^{\circ}$ F.), allowed a quarter hour to even up, and another quarter to lower to 695° C. ($1,283^{\circ}$ F.), at which temperature it was quenched in water at about 15° C. (59° F.).

The results of this test are shown in Fig. 16, from which it will be seen that the length increased by 0.0011 inch, while the diameters were enlarged by about 0.0022 inch, the festoon shape being definitely formed on the rim. It is evident that the extension of 0.0002 inch found in the previous Paper is only correct for gauges cased for half-an-hour in cyanide and does not apply to the treatment under present consideration.

Experiment 8 (24th May 1918).—A similar screw-gauge was heated for one hour in cyanide and afterwards quenched at 695° C. in water (Fig. 17). The extension is now 0.0007 inch, and the increase in diameter about 0.0016 inch. It should be noted that the gauge has a long screw, and the experiment rather suggests that length is a factor in total extension.

Experiment 9 (24th May 1918).—This gauge is another repetition of that in Fig. 15A (page 467), and the results of cyanide casing for one hour with a quenching from 695° C. in water are given in Fig. 18. The extension in length, 0.00066 inch is only a trifle better than the last gauge: while the diameter change, averaging 0.0017, is about the same, but it is much more variable. The general result of Experiments 8 and 9 is to suggest a stable extension of 0.0007 inch under the exact conditions given.

Experiment 10 (25th May 1918).—The same form of gauge, after heating, was cooled in *cold machine oil*, and the chart in Fig. 19 shows an essentially unaltered condition on all dimensions. The quenching temperature was 700° C. ($1,292^{\circ}$ F.). The use of oil as a quenching medium was entirely due to the initiative of Mr. E. W. Eager, and the result is indeed remarkable. The hardness was tested with a new file, and no kind of scratch was obtainable. The Author's attempts to eliminate lapping seemed reasonably in sight, and it

was decided to attack the subject afresh by the use of specimens as in the first Paper.

Experiment 11 (27th May, 1918).—Seven cylindrical specimens

FIG. 20.—*Experiment 11. Heated for 1 hour in cyanide at 770° C., and quenched from temperatures given in oil at 22° to 29° C.*

FIG. 21.—*Experiment 12. Heated for 1 hour in cyanide at 770° C., and quenched in oil from temperatures given.*

FIG. 22.—*Experiment 13.—Heated for 1 hour in cyanide at 770° C., and quenched in oil from temperatures given, oil at 20° C.*

were provided, each about $\frac{9}{16}$ inch diameter and 0.7 inch long, of J and L steel, 0.14 per cent carbon. They were heated in cyanide for one hour, and quenched from the temperatures given in Fig. 20 in

oil at from 22° to 29° C. (72° to 84° F.). The mode of measurement was as described in the first Paper, and the distortions were taken on length only. The hardness in this and the following experiments was excellent. In every case there is a decrease in length, but the amount is generally very small. The best temperatures are between 700° C. and 730° C. ($1,292^{\circ}$ F. and $1,346^{\circ}$ F.).

Experiment 12 (28th May 1918).—The distortions are all extensions,

FIG. 23.—Comparison of the Distortion Curves. The curves show changes in length only.

Fig. 21, and their amounts somewhat larger on the whole, though not really great. No good law is yet apparent.

Experiment 13 (5th June, 1918).—The distortions are again plus, Fig. 22, but they are much larger than in the two previous cases. This the Author believed to be entirely due to the fact that the material was unannealed beforehand, while in Experiments 11 and 12 the material was annealed. It is found that with annealed gauges the first two experiments can be repeated nearly every time, and Experiment 13 may be viewed as abnormal, although it has a certain value in the way of comparison, as follows.

APPLICATIONS OF THE HARDENING LAW FOR OIL. 1 HOUR CASED.

QUENCHED AT $717^{\circ}\text{C. (1,323}^{\circ}\text{F.)}$.FIG. 24.—*Diametral Distortion.*

		Before Hardening.	After Hardening.	After Lapping.		
Diameters.	Full.	F.	1·1495	1·1500	1·1494
		M.	1·1498	1·1504	1·1494	
		B.	1·1498	1·1504	1·1494	
	Effec.	F.	1·1171	1·1180	1·1174
		M.	1·1170	1·1177	1·1174	
		B.	1·1170	1·1179	1·1174	
	Core.	F.	1·0862	1·0864	1·0858
		M.	1·0861	1·0864	1·0858	
		B.	1·0862	1·0864	1·0857	

FURTHER APPLICATIONS.

FIG. 25.—*Diametral Distortion.*

		Before Hardening.	After Hardening.	After Lapping.		
Diameters.	Full.	F.	1·1492	1·1499	1·1495
		M.	1·1494	1·1499	1·1495	
		B.	1·1495	1·1500	1·1494	
	Effec.	F.	1·1178	1·1173	1·1175
		M.	1·1177	1·1173	1·1174	
		B.	1·1178	1·1172	1·1172	
	Core.	F.	1·0853	1·0856	1·0853
		M.	1·0853	1·0856	1·0853	
		B.	1·0855	1·0856	1·0851	

FIG. 24A.—*Applications of the Hardening Law for Oil.*1 Hour Cased. Quenched at 717°C .

Curves of Pitch Error.

FIG. 25A.—*Further Applications.*

Curves of Pitch Error.

In Fig. 23 the three curves are compared on a common base. No crossing law can be found as in the first Paper, but a curious "stream-line" effect is noticeable, and a minimum or dip in the curves at the temperature of $717^{\circ}\text{C. (1,323}^{\circ}\text{F.)}$. This temperature is therefore indicated as that of least distortion on quenching, and is that which the Author definitely adopted in screw-gauge manufacture.

Applications of the hardening law for oil are next given in Figs. 24 to 29. The charts, Figs. 24-25, and diagrams, 24A and 25A, show at a glance the results of the application of the quenching temperature of 717°C. , both on diametral and pitch distortion.

In Fig. 24, the respective gauge in Fig. 15A (page 467) was screwed without serious error and was reduced in total length by hardening to the extent of 0.0001 inch. As the diameters had been turned a little large, lapping had to be used, but the effect was good, and the final gauge was only about 0.00005 inch short.

In Fig. 25A the three curves at bottom of diagram show only small changes due to hardening and lapping. The most serious change is that due to lapping, which has caused a difference between the readings at 0° and 180° of the gauge circumference. This clearly indicates that a small thickness of surface removed may warp a gauge, just as a shaving taken off timber will do, by releasing internal stress.

The remaining diagrams, Figs. 26-29, which show the final combinations only for the smaller gauge, Fig. 15B (page 467), each exhibit distinctive features worthy of study. What is claimed generally is that screw-gauges can now be produced, which if screwed to correct pitch, in a lathe having a correcting bar, and to diameters which are two-thirds of the limit below high and one-third above low, and hardened by quenching from 717°C. in oil, will not need to be lapped at all, but merely polished with a little rouge or other fine powder. The advantage is a great saving in time and cost, both important in wartime, for two-thirds of the cost of a gauge often lies in the lapping. Even if lapping has to take place, it need not be more than the very smallest amount, but the great majority of the gauges should not be lapped at all. The best ideal is to try and retain the perfect work that a good lathe produces,

for lapping nearly always distorts. The only advantage that lapping possesses lies in its partial elimination of bad periodic errors, but these again can be overcome in the lathe itself.

FIGS. 26 to 29.—*Further Applications.*

The conclusions drawn from this Paper are :

- (1.) Hardening screw-gauges in oil, after casing in cyanide, can be performed with less distortion than if water is the quenching medium.
- (2.) The temperature of 717° C. ($1,323^{\circ}$ F.) is the best heat at which to quench.

- (3.) By screwing gauges to accurate pitch and to diameters at two-thirds below high limit and one-third above low limit, lapping can be dispensed with.

The Author tenders his best thanks to The London County Council Education Department, who provided the apparatus, and allowed of its use, and to Mr. E. W. Eager, the Author's works' manager on screws, who prepared and tested the specimens.

The Paper is illustrated by Figs. 15-29 in the letterpress.

Discussion in London, 23rd April 1920.

The PRESIDENT said that, before asking Mr. G. T. WHITE to give an abstract of the Papers by the late Mr. Lineham, he wished to express the regret that he and the members felt that Mr. Lineham was no longer with them.

Mr. G. T. WHITE said that, before reading an abstract of the Papers, he desired to express his own personal sorrow at the fact that the Author was unable, owing to his death, to be present to read the Papers himself. It was especially grievous to him because of his long association with Mr. Lineham as his chief assistant, and as having witnessed Mr. Lineham's efforts—he might say his struggles—to find and to establish some effective system of producing hardened steel screw-gauges in quantity. He had seen Mr. Lineham succeed in that direction, and then break down in health completely. Consequently when the Council had asked him (Mr. White) to revise the Papers in accordance with certain criticisms of the examiners, he had looked upon it as his duty to do so in memory of his old chief. He also felt that it was due to Mr. Lineham to see that nothing was inserted in the Papers that could be classed as his (Mr. White's) work, and not Mr. Lineham's,

and the Papers as the members had them were an honest endeavour to do justice to the examiners' criticisms and to the Author's original matter.

On the motion of the PRESIDENT, a vote of thanks was accorded to Mr. White for bringing the Papers before the Institution.

The PRESIDENT said he thought the members would appreciate that Mr. White, instead of merely cutting out parts of the Papers, had written a special abstract which gave a much better idea of their general trend. As a matter of fact, it was a case of "coming events casting their shadows before them," because that afternoon the Council had appointed a Committee to deal with and "stage-manage" the discussions. That Committee would consist of himself (*ex officio*), Mr. Patchell, Mr. Pendred, and Mr. W. J. Marshall, who was not a Member of the Council. It was hoped that great advantage would accrue to the discussions by the efforts of that Committee.

With regard to Mr. Lineham's Papers, they referred to a most important detail, although some years ago a detail of that kind would have been regarded as somewhat superfluous. The great accuracy required was due to standardization and mass production.

He had little to say on the Papers, but he would like to call the members' attention to the curves in Figs. 7-14 (pages 462-4). They were a good example of the use that could be made of curves. Looking at the curves in Figs. 7-12, it seemed as if there was no law of any sort or kind, but on turning to Fig. 13 the law was immediately obvious. The curves in Fig. 23 (page 472) (oil-quenching) were however not so satisfactory in this respect. The point he desired to make was that by plotting a series of curves, and superposing them, laws of nature could be determined and discovered.

Col. R. E. B. CROMPTON, C.B., R.E. (T.), said that it was not a pleasant task to criticize with any degree of severity posthumous

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Papers such as those before the Meeting. It was unfortunate that Mr. Lineham appeared to have been a self-contained man, and had not troubled to find out what others had been doing. If he had gone to the offices of the Engineering Standards Association, he would have been informed that three members of the late Sir Frederick Donaldson's Screw-Gauge Committee had occupied themselves for several years on the same class of investigation. In fact they had gone over the same ground, made the same experiments, and found that it was difficult, if not impossible, to control the distortion of water-quenched objects, that oil-quenching offered advantages, and that there was no difficulty in producing case-hardened gauges of sufficient accuracy, but, he should add, of insufficient hardness and durability.

Unfortunately the Paper was concerned with the production of screw-gauges and gauges only, whereas the Screw-Gauge Committee had realized from the first that the tap was the master of the situation, that there was no use in hardening accurate gauges if the taps used to cut the nuts were themselves inaccurate. So that the determination of the means of hardening taps, with a minimum of pitch and diameter distortion, had since the death of Mr. Clements, of the Birmingham Small Arms Co., fallen to the speaker, and for the past sixteen years he had spent much time on it. The all-important question of pitch error in screws as affecting their interchangeability was first noticed and discussed in Sir Joseph Whitworth's lifetime, when the British Association Committee was formed at the instigation of Sir William Preece to investigate the best means of obtaining a good fit with small screws, so that the minds of engineers had been directed to this matter for over thirty years.

The War had caused a sudden demand for the production of screw-gauges of a high degree of accuracy and in great quantity. The National Physical Laboratory had prepared a great deal of useful information for the new screw-gauge industry, but in spite of all they could do, the case-hardened gauges such as those described in the Papers could not be considered the successful solution of the problem of obtaining the requisite interchangeability, that is to

say, unless very great tolerances were allowed on the work, to compensate for several factors, one being the wear of the gauges themselves, another being the roughness and dirtiness of the newly-finished screwed material. For this reason, he believed, and he thought that he had the majority of the makers of screwed work with him when he said, that other means had to be found to obtain accurately fitting screwed work which would be interchangeable. This could not be done by gauges alone, and optical methods had to be substituted.

Turning now to the Papers themselves, it was necessary to point out one or two important errors which, if allowed to pass, might greatly mislead readers of the Proceedings. The first law of hardening was incorrectly stated (page 451), and as given was absolutely wrong. All carbon alloys at 900°C . ($1,652^{\circ}\text{F}$.) were non-magnetic, whereas after quenching and returning to the lower temperatures they were magnetic, so that the Author's assertion that sudden quenching after heating retained the steel in the condition in which it was at the quenching temperature, that is to say, in a non-magnetic and soft condition, was precisely the reverse of what the Author intended to say. This was a metallurgical question; metallurgists agreed that just above the top absorption point the carbon alloys consisted of nothing but solid solution, but if a large piece weighing more than 1 lb., such as a large gauge or a large tap, were quenched just above the top absorption point, the solid solution would decompose into troostite, which of course was not the condition it was in at the quenching temperature.

The Author's second law of hardening was also wrong, in fact dangerously misleading, for although it was true that quenching while crossing the recalescent point produced minimum distortion, it did not give sufficient hardness, so that all quenching temperatures must be from above the recalescent point, as the Author acknowledged later in the Papers.

The Author pointed out (page 453) that the Wild-Barfield process, which depended on the change of magnetic properties, was a success, which was perfectly true, but he ought to have stated that one of the Members of the Council, Mr. William Taylor, of

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Leicester, had discovered that fact long ago, and that Taylor's apparatus was extraordinarily useful for the hardening of small articles; this was equally true of the Wild-Barfield process, but neither process was successful with large objects and for the same reason given above, that for the larger articles, in order to get the necessary rate of cooling through the carbon change-point, a higher initial temperature (which varied with the mass) than that which could be given by the magnetic change-point was necessary; consequently the larger masses fell away and got quenched before their proper time.

In conclusion, he was glad of the opportunity of informing the members of the Institution that some progress had been made in minimizing distortion of tap steels, and that he had hopes that they might be able to produce taps commercially within very narrow percentages of pitch error. This matter had now become the special study of a Panel of the Cutting Tools Research Committee appointed by the Institution, and in this case much time and thought had been spent in getting together a very complete bibliography of the matter, and he hoped that rapid work would follow. It was necessary that, in order to progress, engineers should pool their ideas, as isolated work was very often time wasted, and he thought that they should freely interchange ideas with American engineers who were as keenly interested in this matter as they were themselves.

Mr. J. F. DRIVER said it was stated on page 454 that blanks from 0.14 per cent carbon steel could be cut with a polish. It would be interesting to know what cutting compound was used and whether the blanks were subjected to any previous heat-treatment, and, if so, whether steps were taken to ascertain that the total carbon-content of the steel was the same after treatment as before. At the Loughborough Technical College it was the custom to use steel having 0.2 per cent carbon. After roughing down, the blanks were embedded in charcoal and heated at a temperature of 850° C. (1,562° F.) for four hours. They were then allowed to cool down and were reheated to 830° C. (1,526° F.) and quenched in water at

20° C. After that they were tempered by immersion in a salt bath containing tempermelt at a temperature of 370° C. (698° F.). In that condition the blanks had a hardness of about 67 by scleroscope, which was just soft enough for a cutting tool to work upon them, and an excellent finish was received. The point that was interesting was that in all cases the carbon content of the steel appeared to increase. The previous day he had had a piece of 0.15 per cent carbon steel treated in that manner, and a very careful analysis, taken from drillings down the centre core of the steel—not in the casing—showed that the carbon content rose from 0.15 to 0.23 per cent.

He would like to know why sodium cyanide was used in preference to potassium cyanide, unless it was on account of the cost. In that connexion he would like to suggest that the pyromelt store furnace mentioned in the Papers was unnecessary. Experience showed that, if the gauge were taken from the cyanide bath and put straight into an ordinary muffle furnace, sufficient cyanide clung to the surface to prevent any appreciable scale forming. It would be interesting to know at what temperature the pyromelt bath was maintained, and what was the estimated fall in temperature of the gauge in its passing from the pyromelt bath to the quenching bath. Experience showed that specimens of commercial steel varied considerably, and he would like to know whether the seven specimens in the tests were taken from the same bar and whether the analyses given were verified by experiment.

Annealing was mentioned in one portion of the Papers, and more details of that would be of interest. He inquired whether water hardening at 700° C. and oil-hardening at 717° C. were suggested for all classes of steel, irrespective of their carbon content. It was mentioned that the specimens were glass hard. He had had to do years ago with thousands of samples of mild steel hardened in cyanide, and he had never met one which could be classed as glass hard. In his opinion a file test was unreliable. It was to be regretted that hardness numbers had not been given. Glass hard was usually reckoned as 100 on the scleroscope scale,

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although it was quite easy to scratch glass with a hardness number of 95. He had scratched steel showing a hardness number of 103 with an ordinary file—a selected file, it was true. He had a number of samples with him of gauges hardened in various cyanide baths, and found that the average for water hardening gave a scleroscope number of 85, and for oil hardening, by the methods suggested in the Papers, the average hardness number was 67. Steel having a hardness of 67 was easily filed; 85 was easily filed; whilst a good file would cut 95.

The distortion of the gauge radially was dealt with on page 473, and he would like to know whether the gauge maintained its cylindrical shape. He rather suggested that the gauge would go oval. It appeared to him that a gauge which went oval was very difficult to deal with. Both the examples on page 473 were far beyond the tolerance which could be allowed on the gauge. It had been known in the tool-room for many years that turned pieces of steel did not distort longitudinally. If it were so, it would be a very difficult and almost impracticable thing to make a tap at all. He had with him several curves, obtained by means of a pitch measuring machine, of a number of samples of English and American taps. Generally, the pitch errors of those taps were within the limits assigned by the N.P.L. for gauges of the sizes in question. He suggested that the distortion radially rather than longitudinally might have something to do with the rolling process. When the steel was rolled, it was compressed sideways; it was free longitudinally. He had a sample with him of a piece of mild steel cut from a $3\frac{1}{2}$ -inch diameter bar and turned across its diameter, so that the grain was across the section. That piece of steel when hardened in exactly the method suggested by the Author, gave a change in length of $34/10,000$, whilst its maximum change in diameter was only $3/10,000$ at any point. In other words, roughly speaking, if its change in diameter was zero, its change in length was great.

A MEMBER inquired what was the ratio between the diameter and the length.

Mr. DRIVER replied that the diameter was about $\frac{3}{4}$ inch and the length 3.6 inches, taken from a $3\frac{3}{4}$ -inch bar. He had taken tests of some samples of high carbon steel which were hardened in a vertical tube furnace. The samples had been made a considerable time ago, and he dared not give the change in size due to ageing without further examination of other samples which he had, as it was so extraordinary that he might be criticized. The change was great, due to ageing, but in hardening this high-carbon steel, the change in length was $1/10,000$ inch, whereas the change due to elliptical distortion—because the cylindrical piece had gone elliptical—was $6/10,000$. The ratio of diameter to length would be about 3. He had figures of gauges hardened in cyanide by various methods (*see* pages 487–490), but one thing which he would like to suggest was that other liquids than oil and water might be tried.

In his tool-room experience as a student, great success had been obtained by hardening in cutting compound, which was an emulsion of lard, oil, soda, and so on, and this seemed to make a step between the two extremes. He had also seen good results as regards distortion in screw-gauges hardened in urine after treating in cyanide. With water hardening and cyanide bath at 700°C ., and subsequent heating in a muffle furnace at 750°C ., the average hardness number was 74 by scleroscope, and the elliptical distortion was comparatively small, the maximum being $8/10,000$. With water hardening with the bath at 740°C . a hardness number of 88 was obtained, with elliptical distortion slightly greater. With water hardening at 800°C . a hardness number of 89 was obtained, with the elliptical distortion still greater. If one had the bath at a low temperature, one got little distortion but a soft gauge. With oil hardening the hardness number which he obtained was 67, with an elliptical distortion of $7/10,000$. With a gauge hardened the previous day in an emulsion, the hardness number was the same as the oil hardening and the elliptical distortion was less. He would like to show later the curves of American and English taps, showing that with high-carbon steel he had obtained the same effects, namely, very small linear distortion and large distortion

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across the diameter. He suggested that was largely due to the way the steel was rolled.

With the amount of radial distortion shown on page 473 and those in the Tables annexed, he could not see how the Author could substantiate the claim No. 3 (page 478), which stated that by screwing gauges to accurate pitch and to diameters at two-thirds below high limit and one-third above low limit, lapping could be

FIGS. 30 to 33.—*Mild Steel Gauge*

	A	B	C	D
FIG. 30.	1.25"	1 $\frac{1}{2}$ "	2 $\frac{3}{4}$ "	1"
" 31.	1.375"	1 $\frac{5}{8}$ "	2 $\frac{7}{8}$ "	1"
" 32.	1.492"	1"	3"	1"
" 33.	1.5"	1"	3"	1"

dispensed with. If it had been found possible to harden and get distortion within the limits required, it was surprising that examples had not been submitted. The following tests (Figs. 30-33, and Tables, pages 487-493) were made in the Heat Treatment Laboratory of the Loughborough Technical College, under the supervision of Dr. Bramley, to whom he was indebted. Experiments taken some time ago on the rates of cooling suggested that the drop in temperature during the passage from the cyanide bath to the quenching bath would be about 20°. It was probably much less than this in the case of the vertical tube furnaces.

Mild Steel Gauges, FIG. 32 (page 486).

Carbon content of bar 0.2 per cent. Rough turned and heat treated as previously explained. After finishing, the gauge was immersed in cyanide of potassium at 740° C. for 80 minutes and afterwards quenched in water at 20° C.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.4887 & 1.4885 \\ 1.4885 & 1.4885 \\ 1.4883 & 1.4882 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.4896 & 1.4900 \\ 1.4891 & 1.4892 \\ 1.4898 & 1.4895 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0009 & +0.0005 \\ +0.0006 & +0.0007 \\ +0.0015 & +0.0013 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0004 \\ 0.0001 \\ 0.0002 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.5055 & 1.5055 \\ 1.5059 & 1.5055 \\ 1.5054 & 1.5055 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5075 & 1.5073 \\ 1.5072 & 1.5074 \\ 1.5077 & 1.5074 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0020 & +0.0018 \\ +0.0013 & +0.0019 \\ +0.0023 & +0.0019 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0002 \\ 0.0006 \\ 0.0004 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.7160 & 1.7160 \\ 1.7158 & 1.7159 \\ 1.7160 & 1.7165 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.7165 & 1.7165 \\ 1.7160 & 1.7162 \\ 1.7166 & 1.7170 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0005 & +0.0005 \\ +0.0002 & +0.0003 \\ +0.0006 & +0.0005 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0000 \\ 0.0001 \\ 0.0001 \end{array} \right.$
Hardness by Sclero- scope.	$\left\{ \begin{array}{l} 67 \end{array} \right.$	$\left\{ \begin{array}{l} 86 \end{array} \right.$	$\left\{ \begin{array}{l} — \end{array} \right.$	$\left\{ \begin{array}{l} — \end{array} \right.$
Total pitch change less than 0.0001 inch.				
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.4891 & 1.4896 \\ 1.4895 & 1.4893 \\ 1.4896 & 1.4896 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.4914 & 1.4915 \\ 1.4911 & 1.4912 \\ 1.4918 & 1.4919 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0023 & +0.0019 \\ +0.0016 & +0.0019 \\ +0.0022 & +0.0023 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0004 \\ 0.0003 \\ 0.0001 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.5065 & 1.5065 \\ 1.5072 & 1.5070 \\ 1.5065 & 1.5065 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5088 & 1.5085 \\ 1.5090 & 1.5093 \\ 1.5089 & 1.5087 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0033 & +0.0020 \\ +0.0018 & +0.0023 \\ +0.0024 & +0.0022 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0013 \\ 0.0005 \\ 0.0002 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.7170 & 1.7175 \\ 1.7167 & 1.7170 \\ 1.7165 & 1.7165 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.7189 & 1.7185 \\ 1.7182 & 1.7182 \\ 1.7185 & 1.7186 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0019 & +0.0010 \\ +0.0015 & +0.0012 \\ +0.0020 & +0.0021 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0009 \\ 0.0003 \\ 0.0001 \end{array} \right.$
Hardness	64	90		

Pitch Change + 0.00008

,, ,, — 0.00002

(Mr. J. F. Driver.)

Mild Steel Gauges, Fig. 33 (page 486).

Carbon content of bar 0.2 per cent. Rough turned and heat treated as previously explained. After finishing, the gauges were immersed in cyanide of potassium for $1\frac{1}{2}$ hours at 700° C., heated in muffle to 750° C. for 15 minutes, then quenched in water at 20° C.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.5002 & 1.4995 \\ 1.500 & 1.4997 \\ 1.500 & 1.4995 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5005 & 1.5000 \\ 1.5007 & 1.5003 \\ 1.5004 & 1.5000 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0003 & +0.0005 \\ +0.0007 & +0.0006 \\ +0.0004 & +0.0005 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0002 \\ 0.0001 \\ 0.0001 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.5295 & 1.5294 \\ 1.5207 & 1.5294 \\ 1.5295 & 1.5293 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5305 & 1.5290 \\ 1.5310 & 1.5305 \\ 1.5310 & 1.5305 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0010 & -0.0004 \\ +0.0013 & +0.0009 \\ +0.0015 & +0.0012 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0014 \\ 0.0004 \\ 0.0003 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.6297 & 1.6296 \\ 1.6297 & 1.6293 \\ 1.6297 & 1.6291 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.6295 & 1.6293 \\ 1.6295 & 1.6295 \\ 1.6305 & 1.6295 \end{array} \right.$	$\left\{ \begin{array}{ll} -0.0002 & -0.0003 \\ -0.0002 & +0.0002 \\ +0.0008 & +0.0004 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0001 \\ 0.0004 \\ 0.0004 \end{array} \right.$
Hardness by Sclero- scope.	$\left\{ \begin{array}{l} 75 \end{array} \right.$	—	—	—
Total pitch change less than 0.0001 inch.				
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.500 & 1.500 \\ 1.500 & 1.500 \\ 1.500 & 1.500 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.500 & 1.500 \\ 1.5003 & 1.5005 \\ 1.5002 & 1.500 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0000 & +0.0000 \\ +0.0003 & +0.0005 \\ +0.0002 & +0.0000 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0000 \\ 0.0002 \\ 0.0002 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.5295 & 1.5294 \\ 1.5297 & 1.5294 \\ 1.5295 & 1.5293 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5290 & 1.5288 \\ 1.5295 & 1.5296 \\ 1.5293 & 1.5290 \end{array} \right.$	$\left\{ \begin{array}{ll} -0.0005 & -0.0006 \\ -0.0002 & +0.0002 \\ -0.0002 & -0.0003 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0001 \\ 0.0004 \\ 0.0001 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.6295 & 1.6297 \\ 1.6295 & 1.6295 \\ 1.6298 & 1.6295 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.6290 & 1.6300 \\ 1.6294 & 1.6297 \\ 1.6294 & 1.6300 \end{array} \right.$	$\left\{ \begin{array}{ll} -0.0005 & +0.0003 \\ -0.0001 & +0.0002 \\ -0.0004 & +0.0005 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0008 \\ 0.0003 \\ 0.0009 \end{array} \right.$
Hardness by Sclero- scope.	$\left\{ \begin{array}{l} 72 \end{array} \right.$	—	—	—
Total pitch change less than 0.0001 inch.				

Mild Steel Gauges, FIG. 30 (page 486).

Carbon content of bar 0.2 per cent. Rough turned and heat treated as previously explained. After finishing, gauge was immersed in potassium cyanide for 80 minutes at 740° C. and afterwards quenched in oil at 24° C.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.2496 & 1.2498 \\ 1.2495 & 1.2495 \\ 1.2495 & 1.2495 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.2501 & 1.2513 \\ 1.2505 & 1.2500 \\ 1.2503 & 1.2500 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0005 & +0.0015 \\ +0.0010 & +0.0005 \\ +0.0008 & +0.0005 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0010 \\ 0.0005 \\ 0.0003 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.3169 & 1.3170 \\ 1.3167 & 1.3165 \\ 1.3165 & 1.3166 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.3177 & 1.3180 \\ 1.3177 & 1.3172 \\ 1.3175 & 1.3180 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0008 & +0.0010 \\ +0.0010 & +0.0007 \\ +0.0010 & +0.0012 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0002 \\ 0.0003 \\ 0.0002 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.4098 & 1.4097 \\ 1.4095 & 1.4095 \\ 1.4097 & 1.4099 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.4102 & 1.4098 \\ 1.4100 & 1.4099 \\ 1.4098 & 1.4101 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0004 & +0.0001 \\ +0.0005 & +0.0004 \\ +0.0001 & +0.0002 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0003 \\ 0.0001 \\ 0.0001 \end{array} \right.$
Hardness by Sclero-scope.	63	70	—	—

Mild Steel Gauges, FIG. 30.

Carbon content of bar 0.2 per cent. Rough turned and heat treated as explained. Immersed in cyanide of potassium for 1 hour at 800° C.; temperature of bath lowered to 740° C., then quenched in oil at 20° C.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.2497 & 1.2495 \\ 1.2494 & 1.2494 \\ 1.2494 & 1.2494 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.2501 & 1.2503 \\ 1.2500 & 1.2500 \\ 1.2505 & 1.2500 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0004 & +0.0008 \\ +0.0006 & +0.0006 \\ +0.0011 & +0.0006 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0004 \\ 0.0000 \\ 0.0005 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.3174 & 1.3170 \\ 1.3173 & 1.3170 \\ 1.3173 & 1.3172 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.3184 & 1.3182 \\ 1.3172 & 1.3183 \\ 1.3175 & 1.3180 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0010 & +0.0012 \\ -0.0001 & +0.0013 \\ +0.0002 & +0.0008 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0002 \\ 0.0014 \\ 0.0006 \end{array} \right.$
Core (over vee pieces.)	$\left\{ \begin{array}{ll} 1.4099 & 1.4100 \\ 1.4098 & 1.4099 \\ 1.4098 & 1.4100 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.4109 & 1.4107 \\ 1.4105 & 1.4099 \\ 1.4098 & 1.4101 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0010 & +0.0007 \\ +0.0007 & +0.0000 \\ +0.0000 & +0.0001 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0003 \\ 0.0007 \\ 0.0001 \end{array} \right.$
Hardness by Sclero-scope.	60	66	—	—

(Mr. J. F. Driver.)

Mild Steel Gauge, Fig. 31 (page 486).

Carbon content of bar 0.2 per cent. Rough turned and heat treated as previously explained. After finishing, gauge was immersed in potassium cyanide for 80 minutes at 740° C., and afterwards quenched in "*Strong Cutting Compound*" *Emulsion* at 23° C.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.3746 & 1.3746 \\ 1.3746 & 1.3746 \\ 1.3746 & 1.3746 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.3753 & 1.3754 \\ 1.3760 & 1.3760 \\ 1.3757 & 1.3756 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0007 & +0.0008 \\ +0.0004 & +0.0014 \\ +0.0011 & +0.0010 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0001 \\ 0.0000 \\ 0.0001 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.4033 & 1.4031 \\ 1.4030 & 1.4030 \\ 1.4031 & 1.4029 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.4039 & 1.4038 \\ 1.4045 & 1.4044 \\ 1.4045 & 1.4045 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0006 & +0.0007 \\ +0.0015 & +0.0014 \\ +0.0014 & +0.0016 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0001 \\ 0.0001 \\ 0.0002 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.5035 & 1.5035 \\ 1.5033 & 1.5030 \\ 1.5031 & 1.5035 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5033 & 1.5033 \\ 1.5033 & 1.5035 \\ 1.5036 & 1.5034 \end{array} \right.$	$\left\{ \begin{array}{ll} -0.0002 & -0.0002 \\ 0.0000 & +0.0005 \\ +0.0005 & -0.0001 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0000 \\ 0.0005 \\ 0.0006 \end{array} \right.$
Hardness by Sclero- scope.	60	66	—	—

Total pitch change less than 0.0001 inch.

*Gauge similar to that shown in Fig. 30 (page 486).*Treated as above, but hardened in "*Weak Cutting Compound*" *Emulsion*.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inches.	Inches.	Inch.	Inch.
Full.	$\left\{ \begin{array}{ll} 1.3071 & 1.3075 \\ 1.3075 & 1.3070 \\ 1.3072 & 1.3075 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.3077 & 1.3079 \\ 1.3082 & 1.3081 \\ 1.3085 & 1.3081 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0006 & +0.0004 \\ +0.0007 & +0.0011 \\ +0.0013 & +0.0006 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0002 \\ 0.0004 \\ 0.0007 \end{array} \right.$
Effective (over needles).	$\left\{ \begin{array}{ll} 1.3250 & 1.3255 \\ 1.3235 & 1.3235 \\ 1.3237 & 1.3233 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.326 & 1.3264 \\ 1.3244 & 1.3245 \\ 1.325 & 1.3241 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0001 & +0.0009 \\ +0.0009 & +0.0010 \\ +0.0013 & +0.0008 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0001 \\ 0.0001 \\ 0.0005 \end{array} \right.$
Core (over vee pieces).	$\left\{ \begin{array}{ll} 1.5340 & 1.5336 \\ 1.5339 & 1.5340 \\ 1.5339 & 1.5337 \end{array} \right.$	$\left\{ \begin{array}{ll} 1.5335 & 1.5336 \\ 1.5337 & 1.5340 \\ 1.5341 & 1.5339 \end{array} \right.$	$\left\{ \begin{array}{ll} +0.0005 & 0.0000 \\ -0.0002 & 0.0000 \\ +0.0002 & +0.0002 \end{array} \right.$	$\left\{ \begin{array}{l} 0.0005 \\ 0.0002 \\ 0.0000 \end{array} \right.$
Hardness by Sclero- scope.	74	80	—	—

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Total pitch change less than 0.0001 inch.

Comparative Tests with oil and water quenching after casing in a bath of cyanide of potassium for 1 hour at 800° C., which was afterwards lowered to 740° C. Temperature of quenching baths 20° C.

Mild steel test-pieces 0.2 per cent. carbon heat-treated as previously explained.

Specimen No. 4.

—	DIAMETERS.			
	Before Hardening	After Hardening.	Change.	Elliptical Distortion.
Water Hardened.	Inch.	Inch.	Inch.	Inch.
	0.8764 0.8765	0.8766 0.8771	+0.0002 +0.0006	0.0004
	0.8765 0.8765	0.8764 0.8762	-0.0001 -0.0003	0.0002
	0.8765 0.8764	0.8766 0.8760	+0.0001 -0.0004	0.0005
	Hardness by Scleroscope 87.			

Specimen No. 7.

—	DIAMETERS.			
	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
Water Hardened.	Inch.	Inch.	Inch.	Inch.
	0.8767 0.8766	0.8768 0.8770	+0.0001 +0.0004	0.0003
	0.8766 0.8766	0.8763 0.8761	-0.0003 -0.0005	0.0002
	0.8766 0.8765	0.8765 0.8765	-0.0001 0.0000	0.0001
	Hardness by Scleroscope 89.			

Specimen No. 6.

—	DIAMETERS.			
	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
Oil Hardened.	Inch.	Inch.	Inch.	Inch.
	0.8764 0.8765	0.8766 0.8765	+0.0002 0.0000	0.0002
	0.8765 0.8766	0.8768 0.8767	+0.0003 +0.0001	0.0002
	0.8765 0.8765	0.8768 0.8768	+0.0003 +0.0003	0.0000
	Hardness by Scleroscope 67.			

(Mr. J. F. Driver.)

Specimen No. 9.

—	DIAMETERS.				
	Before Hardening.		After Hardening.		Elliptical Distortion.
	Inch.		Inch.		Inch.
Oil Hardened.	0·8769	0·8760	0·8768	0·8765	—0·0001 +0·0005 0·0006
	0·8769	0·8761	0·8772	0·8769	+0·0003 +0·0008 0·0005
	0·8769	0·8761	0·8772	0·8769	+0·0003 +0·0008 0·0005
			Hardness by Scleroscope 62.		

Mild steel cylinder 0·2 per cent carbon. Cut from $3\frac{3}{4}$ inches diameter bar *at right angles* to the axis. Heated to 800° C. for one hour in cyanide bath, cooled to 740° C. and quenched in water at 20° C.

—	Before Hardening.		After Hardening.		Change.	Elliptical Distortion.
	Inch.		Inch.		Inch.	Inch.
Diameter.	0·7368	0·7368	0·7366	0·7366	—0·0002 —0·0002	0·0000
	0·7368	0·7368	0·7367	0·7365	—0·0001 —0·0003	0·0002
Length.	3·6170		3·6203 3·6205		0·0034	—

Mild steel cylinder 0·2 per cent carbon. Taken from *centre core* of 3 inches diameter bar. Axis of cylinder *same* as that of bar.

—	Before Hardening.		After Hardening.		Change.	Elliptical Distortion.
	Inch.		Inch.		Inch.	Inch.
Diameter.	0·7835	0·7838	0·7844	0·7848	+0·0009 +0·0010	0·0001
	0·7834	0·7837	0·7844	0·7847	+0·0010 +0·0010	0·0000
	0·7833	0·7836	0·7844	0·7846	+0·0011 +0·0010	0·0001
	0·7834	0·7837	0·7837	0·7833	+0·0003 —0·0004	0·0007
Length.	3·7303		3·7312		0·0009	—

*Specimen No. 5.**Tool-Steel Cylinder.*

Heated in atmosphere of coal gas at 740° C. for 45 minutes, and quenched in water at 20° C. without being allowed to come into contact with air.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inch.	Inch.	Inch.	Inch.
Diameter.	0·9477 0·9478	0·9483 0·9490	+0·0006 +0·0012	0·0006
	0·9477 0·9479	0·9483 0·9489	+0·0006 +0·0010	0·0004
	0·9477 0·9478	0·9483 0·9488	+0·0006 +0·0010	0·0004
Length.	2·7586 2·7585	2·7585	{ —0·0001 —0·0000	—

Hardness by Scleroscope 87.

*Specimen No. 8.**Tool-Steel Cylinder.*

Heated in an atmosphere of coal gas at 800° C. for 45 minutes, and quenched in water without being allowed to come into contact with air.

—	Before Hardening.	After Hardening.	Change.	Elliptical Distortion.
	Inch.	Inch.	Inch.	Inch.
Diameter.	0·9686 0·9685	0·9696 0·9702	+0·0010 +0·0007	0·0003
	0·9687 0·9686	0·9709 0·9710	+0·0022 +0·0024	0·0002
	0·9690 0·9687	0·9696 0·9690	+0·0006 +0·0003	0·0003
Length.	2·8265 2·8265	2·8264	—0·0001	—

Hardness by Scleroscope 80.

Mr. G. H. AYRES said his remarks would be principally in the form of questions, rather than by definite criticism. In the first place, dealing with water hardening, it would be interesting to know whether any allowance was made in the temperature for cooling in passing from the furnace to the tank, and, if so, what allowance was actually made. He would also like to know whether

(Mr. G. H. Ayres.)

any local chilling due to draughts had been experienced. With regard to the remark on page 457 that the tank was provided with inflow and outflow cocks, he would like to know exactly what that meant. Did it necessarily mean there was simply an inlet and outlet for filling the tank and emptying it, or did it imply a regular circulating system in order to get a constant temperature? He noticed throughout Paper No. 1 there was no mention made of the actual temperature of the quenching bath, but in Paper No. 2 the suggestion was made on page 470 that the temperature of the water was 15°C . (59°F .), which appeared to him to be unusually low. He suggested that the temperature of the water would be between 20° and 26°C . (68° and 79°F .), which, incidentally, was the temperature given for the oil. Had the temperature been tested in each case? There was the suggestion that the oil was 22° to 29°C .; did that imply that the temperature gradually increased from 22° on the first experiment to 29° on the final experiment, or alternatively what was the actual temperature in each case? It certainly seemed to him, in view of the physical properties of oil and water, that the higher temperature should have been for water and the lower temperature for oil. A few experiments which he had made during the War, but which, unfortunately, he was not in a position to give any definite figures about, led him to suggest that the distortion bore some definite relation to the quenching temperature consequent upon the actual shock received, and it appeared to him that in many cases the actual conditions of quenching were not so carefully studied as the actual conditions of releasing an article to be quenched.

With regard to the curves on pages 474-5, the curves were marked 0° and 180° , and he gathered that referred really to half-pitch. In that case it appeared to him that the gauges had been dropped horizontally, and the distortion was caused for that reason. He certainly suggested in a case like that, that the gauges should be dropped vertically. He would also like to know whether any definite experiments had been carried out, using a layer of oil on the top of the water—for the following reason, that, whilst in a perfectly plain cylinder the layer of oil might have no effect on

relieving the shock caused by water direct, in the case of a thread there was a possibility of the oil being carried down due to the V-shape of the thread. He would like to know whether any experiments had been made in that direction. It might be possible even to reduce the distortion caused in the water to an equivalent amount to that of the oil, and also to get the reliability which appeared from Fig. 13 (page 464), where all the curves crossed at the one point. [See also page 512.]

Dr. A. BRAMLEY said that it was stated near the bottom of page 458 that the volume of the piece of metal depicted in Fig. 5* had not changed during the process of heating to redness and quenching in cold water a thousand times, and that displacement experiments were conducted in order to ascertain this. This point was emphasized by the statement (page 466) that "Nature insists on keeping the volume constant."

It would be interesting to know exactly how the displacement experiments were carried out, as it seemed hardly credible that the dimensions of pieces of steel should have been determined with that very high degree of precision necessary for them to be of any use in a test of volume change, and then handed over to a garage boy, in whose hands they must have been subjected to very rough treatment indeed during the time he had them. Further, after all these heatings and sudden coolings, what must the state of the surface be? A very considerable amount of oxidation must have taken place, and it was quite likely that scales of oxide fell off at times. It would be most interesting to know what the state of the metal was throughout the mass of one of these very much distorted blocks of steel. This could only be ascertained by cutting the block in two, preferably through a plane passing through the axis of the disk. The statement about the constancy of the volume of a piece of steel during such heat treatment as it received when hardened was most certainly wrong, as his own examples showed. In Figs. 8, 9, 10, 11, and 12 (pages 462-3), the changes in length produced by hardening of thirty-six steel cylinders were

* See revised figures (page 459), and Mr. White's Note (page 529).

(Dr. A. Bramley.)

shown, and all with but one exception showed an increase. It was unfortunate that the changes in diameter of these cylinders were not available, but it was stated (page 465) that in a few cases which were measured there was always an increase in this dimension. Obviously, the volume increased on hardening, and Mr. Lineham's figures served to illustrate this fact which had long been known, but which was in direct opposition to the statement about constancy of volume referred to on page 458 and again so strongly emphasized on page 466.

Grave exception must be taken to the method described on page 461 of deducing the most favourable temperature at which to quench the steel. The curves given in Figs. 8 to 11 showed nothing in common, and the fact that when they were all superposed, as was done in Fig. 13 (page 464), a common point was shown at 700°C . ($1,292^{\circ}\text{F}$.), must be regarded as a remarkable coincidence, rather than indicative of some fundamental law. Had the curves all shown a steady fall or a steady rise but of varying gradients, the case might have been different, but they could not by any stretch of imagination be said to resemble each other in any respect. Further, objection must be taken to the omission of the curve shown in Fig. 7 in the final deduction. On page 460 mention was made of bulging at the ends of the cylinder caused by hardening, and it was understood from the remarks at the bottom of the same page, that if the lengths of these cylinders had been determined by measuring near the edge of the circumference instead of near the axis, the curve so obtained would have fallen below that given in Fig. 7 by an amount representing the height of the bulge. This would lead to an even greater discrepancy than that actually shown.

The irregular variation of the results shown was not at all surprising, because the great majority of steel hardeners would agree that 700°C . was too low a temperature for the bath from which the article had to be transferred to the quenching bath. The margin between 700°C . and the temperature at which transition took place was too small. No figures were given for the hardness of the specimens, but the statement was made on page 466 that in every case they were glass hard. Glass hard was a very vague term.

Experiments conducted at the Technical College, Loughborough, had led them to the conclusion that it was almost impossible to obtain a hardness much above 90 on the scleroscope scale by the cyanide process, a hardness which fell considerably short of 100, which was taken as the standard for glass hard. Quenching in water an article taken out of a salt bath at 700° C. might result in great hardness in some places, but there would be very grave risk of soft patches, a risk which would be considerably diminished by quenching from a somewhat higher temperature.

As regards using oil instead of water for quenching, it was generally agreed that, for ordinary carbon-steel, the hardness obtainable by quenching in oil was always less than that obtainable by quenching in water. For example, a number of specimens of mild steel were treated side by side in the cyanide bath so that each had exactly the same time—about 1 hour—at the same temperature—about 780° to 800° C. in the cyanide case-hardening bath. The temperature of the bath was lowered to 740° C., at which temperature the specimens were withdrawn and quenched, some in water at 20° C. and some in oil at the same temperature. The hardness of the water-quenched specimens averaged 87 and the oil quenched specimens averaged 65 on the scleroscope scale.

The absence of references to previous work carried out by other investigators in this field of research was somewhat surprising.

Mr. J. E. BATY said he was very sorry indeed to have to add another criticism of the Papers, but he hoped that Mr. White would take the criticism on his own shoulders. He had carried out one or two experiments during the previous week with regard to the particular results which were given for the test-pieces $\frac{9}{16}$ inch in diameter and 0.7 inch long. He thought the whole of the results which were shown on the superposed curves, Fig 13 referred to pieces of that particular size, and that size alone.

In the first place he (Mr. Baty) wished to show that one could not experiment with plain cylinders to determine what the result would be in hardening a screwed cylindrical sample. The particular piece which he had taken was like Fig. 34. This was carefully

(Mr. J. E. Baty.)

FIG. 34.—Gauge.

FIG. 35.—Curve of Pitch.

screwed fourteen threads per inch, Whitworth form, and, after screwing, the flat faces were surface ground parallel. Diametric measurements of major, effective and core, were taken across the axes 1-3 and 2-4, a reading being taken at the centre and each end of the gauge in every instance. Further measurements were taken

FIG. 36.—Gauge.

FIG. 37.—Curve of Pitch.

across the parallel faces T in figure at the position one, two, three, and four, and the pitch of the screw was charted in these four positions, that is 90° apart. The pitch was very uniform; an average of the readings had therefore been taken to avoid confusion and was shown in the top curve of Fig. 35. In order to get maximum distortion, the screw was heated in a cyanide bath at 760°C. and

(Mr. J. E. Baty.)

plunged direct from there into cold water. The screw was again carefully measured on all points, and it was found that whereas the cylinder, considered apart from the screw, had shortened from 1.0151 to 1.0147 inch, that is by 0.0004 inch, the helix of the screw had distorted independently and was now 0.00167 inch shorter than in the soft condition. This was shown in the lower curve of Fig. 35. The diametric measurements were all carefully taken, but as they did not have any bearing in this particular point, he had omitted them; they were, however, all greater after hardening.

A further gauge of different form was treated in an exactly similar manner, and the results were given in Figs. 36 and 37, which would be clearly followed from the foregoing explanation. It was necessary to point out in this case, however, that the plain part of the cylinder lengthened, whereas the helix of the screw again shortened. Anyone who had the necessary measuring appliances could easily verify these experiments, but he (Mr. Baty) had had numerous cases during the last few years which proved the above statements.

He contended that, in any cylinder which was taken, the distortion due to hardening depended absolutely upon the length of the cylinder compared with the diameter. There were numerous variables to be taken into account which the Author had certainly not detailed, one of them just mentioned being the connexion between the length of the screw portion and its diameter. The next was the pitch compared with the diameter. If one had a very coarse helix or screw, one would find the pitch error due to hardening was very different indeed to what one would get with a very fine thread. Also there was the length of time in the bath, which the Author had mentioned.

With regard to hardening from a cyanide bath, cyanide had a very great tendency to stiffen the steel in the hardening, and consequently the strength of the cyanide bath had a very great deal to do with it. Going upon figures given by Brearley in his book on the Case-Hardening of Steel, the loss of carburizing properties in the cyanide bath after the first heating was something like 75 per cent, so that, if one did not add constant quantities and did not

keep very constant temperatures, one had a constantly fluctuating carburizing power in that cyanide bath. Then of course there was the effect of the temperature of the quenching bath.

In Experiment No. 8 (page 470), the Author found out for himself what he (Mr. Baty) had been trying to demonstrate, that where he went to a different size—in that particular instance it was a gauge 1.15 inch in diameter and 1 inch in length of screw—he got a totally different result, which the Author himself said “rather suggests that length is a factor in the total extension.” Yet the Author went on in his oil-hardening experiments cutting off specimens $\frac{1}{16}$ inch diameter and 0.7 inch long for test purposes. Apart from that, both Experiment No. 8 and Experiment No. 9 (page 470) disproved, he contended, the whole of the first Paper.

There was one other point. The cyanide hardening was all right for small gauges, but, when one had a gauge of very large diameter and a very big body of metal inside, all the heat had to get out, and the only place for it to get out was through the walls which had been cyanided, and consequently, in a large size gauge, one almost invariably got a soft surface. Then there was a point which had been mentioned by one or two speakers, that if one withdrew the gauge at the recalescence point, one had to be very quick in transferring it from there into the quenching bath, whereas it was generally contended by steel-makers that one ought to harden on a rising temperature, which gave a greater range and consequently more time to quench, before the carbon had gone out of solution again. In the first Paper the Author had allowed for a distortion of 3/10,000 in pitch, and thought that that was all right because it came within the error the National Physical Laboratory allowed. As a matter of fact, if Mr. Rolt had spoken he would have told the members that the tolerance allowed by the N.P.L., 6/10,000 for small gauges, included the pitch error, the error over the effective diameter, and also the error of the angle. Therefore if one had 3/10,000 error in pitch, one had nothing to spare either for diameter errors or for errors of angle.

Mr. F. H. ROLT said it was a great pleasure to have had the

(Mr. F. H. Rolt.)

Papers from Mr. Lineham. He had had occasion to visit the Goldsmiths' College during the War, whilst Mr. Lineham had been engaged on his screw-gauge problem, and he had tested a good many of Mr. Lineham's gauges after they had been made, and generally speaking, they were really excellent. Most of the gauges were quite small—small B.A. gauges and others of that nature—and with that particular size of gauge the results were quite first-class. The limits to which the Author had been working on some of those gauges had been extremely fine—not the ordinary limits of $6/10,000$ but the check-gauge limits, which were about $3/10,000$ inch—and he thought he was right in saying that the Author's percentage of passes was quite large. The Ministry of Munitions towards the latter end of the War had placed great reliance on Mr. Lineham for producing those very small gauges.

Mr. Baty had emphasized one point, namely, the question of making experiments on the specimens of steel $\frac{3}{16}$ inch diameter by 0.7 inch long, and using those results for gauges of quite a different size. He had an idea himself that the distortion produced in hardening depended largely upon the form of the gauge. For instance, if one had a spindle about 0.1 inch in diameter by 6 inches in length, it would distort quite differently from a 1 inch cube. That was his reason for saying that he did not think one was really safe in making experiments on a sample of steel which was of a different size from the actual gauge which was finally to be produced.

The remark which had been made previously with regard to the pitches of taps had quite surprised him. Generally speaking, commercial taps were not as good as they might be in pitch, simply because, he presumed, the methods of hardening were not very carefully studied. As Colonel Crompton had said, the question of taps and screwing tackle generally was at the root of the whole screw-gauge problem. It was useless to go into the question of making accurate screw-gauges, if the work which was generally produced was of poor quality.

With regard to the question of quenching, there were charts given in the Papers which showed the various quenching temperatures.

Those temperatures, he presumed, were the temperatures of the furnace; but what was the temperature of the gauge when it was just going into the quenching tank? Surely there was an appreciable drop which took place between the time of taking it out of the furnace and immersing it into the tank. There were furnaces now to be obtained whereby one was able to quench straight out of the furnace into the quenching bath. The general principle applying in those electric furnaces was to have a tube surrounded by an electric coil. Moreover, the heating was done in a reducing atmosphere. Gas went into the bottom of the furnace tube and emerged at the top where it was ignited. The tube itself came down very close to the annealing tank, which contained either oil or water. A pyrometer was inserted in the tube and the gauge was heated up to the required temperature, and all one had to do was simply to take away the supporting bar and the gauge dropped straight away into the tank. He would admit it had to pass a slightly cooler temperature towards the lower end of the tube, but there was no taking it out of the furnace through fairly cold air where it might cool down appreciably before going into the tank. He was not a metallurgist, but he gathered that the question of hardening depended upon cooling the metal *suddenly*. It was no good letting it cool over a range of a few seconds. The electric furnace, in which the heating was done in a reducing atmosphere, had the great advantage of the fact that there was no oxidization taking place. One great trouble in hardening was due to the fact that in most cases a scale was formed on the gauge, which had to be removed. Even supposing that the gauge had not distorted, it was not serviceable until the scale had been removed. Therefore, obviously one great thing was to do the heating in a reducing atmosphere.

Mr. WILLIAM H. PATCHELL (Member of Council) said that, with regard to the President's opening remarks about future discussions, he thought the President's expression "stage manage" might be misunderstood. All that was meant was that the discussions should be so arranged that there should be full and adequate discussions on the Papers. The Council had no idea of limiting

(Mr. William H. Patchell.)

the discussions or limiting what a man might say either for or against a Paper. Papers, in the past, had sometimes not got into the right hands, and people had come into the room and prefaced their remarks by saying that they had not read them! That was not fair to the Institution.

The PRESIDENT said that Colonel Crompton had just handed him a note to the effect that Dr. Agnew, the Secretary of the new American Standards Committee, was present. He welcomed Dr. Agnew, and invited him to take part in the discussion on the Papers.

Dr. PAUL G. AGNEW said he was quite unprepared to discuss the Papers, but he was very glad to have had the opportunity of attending a Meeting of the Institution, and to have met several of its members during the previous week. He would like to say that the experience which the British Engineering Standards Association and its affiliated Societies had placed at the disposal of the Americans had been of inestimable benefit in forming their new organization and getting it well under way. He would like to add that in their opinion the co-operation between the two great English-speaking nations in engineering standardization was of prime importance to them as citizens as well as engineers.

Mr. G. T. WHITE, in reply, said that if the members expected him to answer all the very severe criticisms which had been passed upon the Papers, he was afraid he would have to disappoint them. The criticisms had been exceedingly numerous; he would not say they were unexpected. What he would say in defence of the Papers was that the experiments had been carried out at a time of very great stress, when it had been found very difficult at the Goldsmiths' College at New Cross to get definite information in connexion with the distortion of steel on quenching. They had to attack the problem there of turning out gauges in probably the most severe months of trial that this country had ever experienced, namely, in the early part of 1918, so that there

had been no time to devote to what one might call very fine scientific details.

With regard to Mr. Lineham's experiments, they had been made solely with the object of finding sufficient information for himself in order to enable him to make gauges, so that the manufacture of aeroplanes could be hastened, and this country probably saved from disaster.

He would endeavour to reply to the various points of detail by correspondence, as some of the questions could not be answered at once. He thought the members had been exceedingly interested, and they had probably gained more useful information from the criticisms and the questions than from the original Papers.

Discussion in Manchester, Thursday, 29th April 1920.

In the absence of Mr. G. T. White, an abstract of the Papers was read by Mr. DANIEL ADAMSON (Member of Council).

The CHAIRMAN (Mr. HENRY PILLING, M.B.E., *Member*) said possibly the small attendance was due to the subject. Men concerned in the manufacture of screw-gauges were few in number, and in all probability there were no experts present. Speaking himself as a non-expert, he would like very much to have further information respecting the steel disk, Fig. 5 (page 459), which had been passed round.* If one put a poker into the fire and heated it to red heat a thousand times, one would expect the point to be gone and the poker much less in weight. What was the original weight and the present weight of that disk? The diminution of the cubical contents was stated in the Paper to be practically nil, but it seemed a curious thing, one which required some explanation, that a disk, after being heated presumably to redness a thousand times, had lost little or nothing by scale. Some light would be thrown upon that question if the weights were known.

* See Note by Mr. White on page 529.

(Mr. Henry Pilling, M.B.E.)

He observed that all these experiments had been made with minute quantities of steel. The experiments with water quenching were made with a screwed portion which was only one-sixth of a cubic inch; the experiments with oil quenching were made with a piece of steel of which the screwed portion was only 1 cubic inch. It was obviously very difficult to draw deductions as to what would happen with larger specimens. At the present time he was interested in purchasing some taps 1 inch in diameter, 15 inches long, continuous threads. He could quite understand that the manufacturer of those taps must have extreme difficulty in getting an over-all length of 15 inches within the very narrow limits which had to be laid down. In many forms of locomotive boilers there was used a continuous tapped thread—for example, in the Belpaire fire-box—between points quite distant apart, with the stud or stay produced by a machine which also made a continuous thread. Obviously it was a very difficult matter to produce the very long and perfectly true taps, which were required.

Mr. C. E. STROMEYER said he had very little to say on the subject of the Paper. As had been already pointed out, the specimens were very small, and he thought they could not draw definite conclusions as to what would happen when hardening large pieces, for stresses increased with size. Recently he came across a main shaft 11 inches or 12 inches in diameter, which was found to have two cracks about 5 inches long. They were not noticed until the bearing cap was taken off, and as they were opposite each other it was feared that the shaft was hollow and the crack extended right through. The shaft was therefore cut in two, but was perfectly sound, except that the whole of the outer surface was covered with small longitudinal cracks; and the conclusion they came to was that the bearing had been running hot and the engine-driver had poured cold water on it. With a small shaft nothing would have happened, but with a large shaft the outer surface would get cool and contract, or wish to contract; and as the cold could not travel fast enough to the core, an enormous stress would be put upon the outer surface and crack it. He thought a similar

action would probably take place when hardening very large specimens, and that very great stresses would be created. Large masses, even when cooled slowly, sometimes cracked. Such spontaneous cracks in large steel forgings were called clinks. Sometimes they were visible on the outside, but not always. He had seen a few ingots which were broken in two, and he had also seen an armour-plate about 5 feet square and about 1 foot thick, which had fractured horizontally and was only held together at one edge by a very small portion. This armour-plate showed that the stresses or strains produced by sudden cooling were very enormous in large pieces. Of course they did not occur in the small ones. By increasing the size, difficulties would be encountered which were not apparent in the small specimens.

With regard to the Chairman's remarks about long taps, there was the consolation that most probably they would be slightly bent in hardening, and he understood a tap that was slightly bent was a much better cutter than if it were mathematically straight; a springy tap was very much better than a perfectly solid one. He also understood it was found by tap-makers that much of the distortion of the tap was due to the bending which occurred while heating, and not while hardening, and that the safest plan was to suspend them—while being heated. Then distortion would be practically nil. But he believed that another precaution which had to be adopted, and which was mentioned in the Papers, was that the gauge or tap to be dealt with should be annealed before being finished. The stresses as it came from the works were very irregular, and when the tap or gauge had been finished, distortion took place which could not be rectified. He was wondering with regard to large gauges whether it would not perhaps be an advantage to harden from the inside, by drilling a hole through the gauge and letting a stream of water go through the inside. Certainly as regards glass tubes, which naturally were cooled from the outside, there was a very severe tension in the tube, and one could easily crack the glass tube by letting a steel nib or any hardened steel particle glide down the inside. Very slight scratches started cracks which led to fracture. So if one

(Mr. C. E. Stromeyer.)

could get at the inside of a hollow gauge, he believed the outside would in a way take care of itself.

There was one point which he would like to be answered by those responsible for the Papers, but he did not know whether it was possible. That was, whether these gauges altered their shape and dimensions with time. Many years ago he had had some steel bars hardened and magnetized. They came to him slightly bent. Now after many years of waiting these bars had become nearly straight again. A very slow process of alteration of form seemed to have gone on. He would not be surprised if something of that sort happened with gauges and with taps. He believed that Colonel Crompton, who was at present engaged on investigations with regard to taps, had a special steel—which was supposed to possess the property of not changing shape on being hardened.

In reply to remarks by Mr. Daniel Adamson, he had several broken bolts of which he felt quite sure that fracture was produced by the fact that the nut was a finer pitch than the bolt. Most of the stress was thrown on the threads at the bottom of the nut, where of course the longitudinal tension was also a maximum.

The combination of severe shear-stress with a severe pull-stress evidently resulted in fracture. If that theory were correct, and he thought it was reasonably correct, it would be desirable to allow the taps during hardening to become longer than the nominal length, thereby making the pitch in the nuts longer than that on the bolts. The making of the gauges had nothing to do with the best form of nut or tap, and therefore he had not touched on this subject when opening the discussion.

Mr. P. R. DAVENPORT said he was a gauge-maker, and the troubles he experienced during the War proved to him conclusively that the question of hardening screw-gauges was a very difficult one. Various experiments had been tried at the Westinghouse Works in Trafford Park, but so far they had not met with real success. Most of the gauges used during the War were made to very fine limits, both in the number of threads per inch and also in the diameter.

A number of gauges were submitted to the National Physical Laboratory at Teddington for approval before they were sent out of the works, or before any munitions were adapted to the gauges.

He had made several experiments in connexion with the hardening of both male and female gauges. During the last six months he had made tests with caustic soda and common salt, equal quantities of each, melted down at a heat of approximately 750°C . ($1,382^{\circ}\text{F}$.). The gauges were then heated to a dull red and quenched in the molten caustic soda and salt, in which they remained for about three minutes, and were then quenched in cold water. The result from that operation, especially with female gauges, was quite satisfactory, very little distortion being noticeable. To a certain extent this had done away with a great deal of lapping. The tolerance allowance from the finishing of the gauge previous to hardening was minus three-tenths of a thousandth inch. From that hardening process he found the result of the gauge proved about minus two-tenths of a thousandth; so there was very little lapping needed to bring it up to the standard size. The tests given to the female gauges were very minute. They used what were called reference gauges, which were designed to give combined measurement of pitch and outside diameter. For example: a $\frac{3}{4}$ -inch screw-gauge with the core diameter absolutely taken away, the pitch diameter being minus three-tenths and the outside diameter finished exactly to standard. Halfway along the gauge they turned the outside diameter off a ten-thousandth below the standard. That gave the first half of the gauge—the pitch diameter only. For the core diameter they used a plain gauge, hardened and ground. The first thing they did after the hardening to find out the extent of distortion, was to use the plain core gauge, and a defect there carried with it generally the same defects both in pitch and outside diameter. This process of hardening had been the most successful up to now.

With water hardening, it was heated to 800°C . ($1,462^{\circ}\text{F}$.), and he found that the distortion was very great. It contracted greatly in the middle. Generally speaking, out of six gauges that were hardened in this way, four were scrapped before the standard size

(Mr. P. R. Davenport.)

could be got; they were so bell-mouthed at the front that the inspector would not pass them.

In the process of hardening in whale oil, the gauges were heated to a dull red and quenched in whale oil; and the chief trouble he experienced was that whale oil did not produce the grade of hardening that was required. Most of the gauges were used on automatic work and were not used with the greatest care. He had made various experiments with different grades of steel. At the time the War broke out, there was the Bohler steel, a German steel, which he considered was the best that had ever been used in gauge-making, but of course the supplies were stopped, and since then they had not been able to get any of it. He understood the firm who were making it at that time had now gone out of existence. Other steels from Sheffield had been tried, and the most successful steel they were using to-day was made by Messrs. Keyser Ellison. He had experimented with several gauges made of mild steel case-hardened in potassium cyanide, but had found the defect in the pitch longitudinally was very bad.

Some remarks had been made with regard to the distortion of taps. In his opinion, the error of pitch in a tap of the length and diameter mentioned was only in the distortion after hardening. If he had a specially long tap to make, his process was to cut the thread down to plus 5 thousandths, then anneal it, stretch it, and finish it to size. He found that, in the hardening, it distorted very little as to pitch and with regard to being out of truth in that length. The length was 15 inches to 18 inches, and the diameter about $1\frac{1}{8}$ inch. He did not attach much importance to annealing before threading, as he did not think it altered very much in lengths, say 4 inches to 8 inches, but a great deal depended upon the quality of the steel. With regard to being out of truth and out of pitch, he had made an experiment on a piece of drill rod about 2 feet 6 inches length, by centring it absolutely true with the grain of the steel itself at the outside diameter. He then got one of the lads to centre a piece up similarly in an indirect fashion. Both those pieces of steel were turned to a given diameter and then hardened. He found out that the one that had been centred true by himself was very little

distorted, and required very little stretching, but the one that had been centred up indiscriminately by the lad could not be made straight at all. So even that was an item well worth noting in making a long tap, providing they got the centres true with the grain of the steel.

The CHAIRMAN asked whether, if the keynote of the Papers was that by hardening at a certain prescribed temperature, there was an end of the difficulty, did Mr. Davenport agree with that point of view?

Mr. DAVENPORT inquired if the Chairman meant given degrees of heat?

The CHAIRMAN replied in the affirmative.

Mr. DAVENPORT said he did not think so. The experiments they had carried out were on a large scale. No pyrometer or anything of that description was used. It was only experiment and experience that taught him the real grades for arriving at, for instance, a dull red or a glowing red, and between the two of them he could determine the different sizes of gauges that he made. He might mention, by the way, a very successful process—and a very easy one too. Suppose, for instance, they were hardening a female screw-gauge $\frac{3}{8}$ inch. They took a receptacle holding about a quart of water, and put in a handful of ordinary table salt; the water was raised to lukewarm heat; then the gauge was heated to a dull red and quenched in the salt water. There was very little distortion by that method which he had used to some extent on the very small pitch screws which they had been making.

The CHAIRMAN, in proposing a vote of thanks, said they were indebted not only to the late Mr. Lineham, but also to Mr. White. What appeared to him to be the essential point of the Papers was the statement that there was a certain prescribed quenching

(Mr. Henry Pilling, M.B.E.)

temperature which was advantageous. If they did not keep to it closely, they had trouble. That root notion of the Papers was at the back of his question to Mr. Davenport.

The vote of thanks was carried unanimously.

Communications.

Mr. G. H. AYRES wrote, supplementing his remarks at the Meeting, with respect to Mr. Rolt's interesting reference to the tubular electric hardening furnace, that he thought a detailed description would be of service, as to his mind it was one of the most important of recent developments in the heat treatment. Fig. 37 showed a cross-sectional elevation of Dr. Walter's Electric Hardening Furnace referred to by Mr. Rolt, which furnace could be adapted to heating by gas, oil, or solid fuel, and could, moreover, be built to almost any dimensions. The heating chamber consisted of a silica tube around which, but insulated from it, was the heating element of Nichrome wire either wound in one continuous coil or in sections operated in parallel or series-parallel combinations; the winding itself was coated with an insulating cement, and the whole of the element was readily removable from the body of the furnace. The body was usually a cast-iron casing, and the space between the casing and heating element was filled with anti-thermic packing. To the bottom plate was fixed the dip-pipe which formed a seal for the coal gas used as the reducing or non-oxidizing agent. In the top plate there was the inspection door and sliding doors through which the article for treatment was admitted; the article was suspended by a wire or other suitable means and located concentric with the heating element; on the article attaining its correct temperature and allowed the requisite time for soaking, it was released and fell into the quenching bath without loss of heat, risk of local chilling, or any possibility of

FIG. 37.—*Electric Hardening Furnace* (Dr. C. M. Walter's).

(Mr. G. H. Ayres.)

coming in contact with air to cause scaling. The temperature could be regulated to very fine limits and kept constant for long periods if desired, a thermo-couple being used for measuring the temperature. The main advantages over any form of salt bath were the extra cleanliness and lower cost of operation.

Though he had not had experience of treating gauges in this type of furnace, he had treated large articles of 0.5 per cent carbon steel in such a furnace with absolute freedom from scaling, and had proved a very high degree of uniformity of temperature throughout a length of element of approximately 10 feet, and he understood that several Midland firms were treating the whole of their gauges in this manner with complete success.

Mr. J. FERDINAND KAYSER (Sheffield) wrote that the change of dimensions on hardening, as well as the quality of the hardness after quenching, depended to a certain extent on the initial condition of the steel. The American gauge manufacturers clearly recognized that point, and their experience, which he (Mr. Kayser) had confirmed, was that the most desirable initial microstructure was one giving fine sorbitic pearlite with any cementite (which was always present in hypereutectoid steels) present in a fine emulsified form. It had to be remembered that a very large number of gauges had to have a hardness sufficient to enable them to cut and produce other threads, and hence it was necessary to consider the quality of the hardness obtained. Many of the treatments mentioned in the Papers would not produce hardness at all, and the recommended treatments would not harden properly any gauge weighing more than say $\frac{1}{2}$ lb., unless the section was very thin.

When using Roberts-Austen's iron-carbon thermal equilibrium diagram, it was necessary to remember that there was considerable "thermal hysteresis," that was to say that the arrest points on heating were considerably above those found on cooling. The above remarks were clearly illustrated by the following typical results of actual measurements made after the stated heat treatment upon pieces, 12 mm. thick by 50 mm. diameter of a crucible cast carbon tool-steel containing 1.2 per cent carbon.

Critical point on heating . . .	736° C. (1,357° F.)
Critical point on cooling from above	
Ac 1.2.3	695° C. (1,283° F.)

Treatment.	Brinell Hardness.
Heated to 700° C. (1,292° F.) and quenched in cold water	180
Heated to 730° C. (1,346° F.) and quenched in cold water	180
Heated to 740° C. (1,364° F.) and quenched in cold water	477
Heated to 750° C. (1,382° F.) and quenched in cold water	635
Heated to 750° C. (1,382° F.), cooled in furnace to 730° C. (1,346° F.), and quenched in cold water . . .	635
Heated to 750° C. (1,382° F.), cooled in furnace to 720° C. (1,328° F.), and quenched in cold water . . .	635
Heated to 750° C. (1,382° F.), cooled in furnace to 700° C. (1,292° F.), and quenched in cold water . . .	555
Heated to 750° C. (1,382° F.), cooled in furnace to 695° C. (1,283° F.), and quenched in cold water . . .	532
Heated to 750° C. (1,382° F.), cooled in furnace to 695° C. (1,283° F.), and quenched in cold water . . .	460
Heated to 750° C. (1,382° F.), cooled in furnace to 690° C. (1,274° F.), and quenched in cold water . . .	277
Heated to 750° C. (1,382° F.), cooled in furnace to 650° C. (1,202° F.), and quenched in cold water . . .	241
Heated to 750° C. (1,382° F.), cooled in furnace to 15° C. (58° F.), time about four hours . . .	203

Mr. WILLIAM TAYLOR, O.B.E. (Member of Council) wrote that the Author had contributed very interesting Papers on a branch of Engineering Workshop Science—that part of engineering which had hitherto received comparatively little attention from scientific men. The Author gave credit to the Wild-Barfield apparatus as the first practical example, the principle of which was to use the change in magnetic properties of steel to indicate the temperature at which to quench. This idea occurred to the writer, however, in 1905, when, with the assistance of Mr. F. J. Mudford, he experimented on the subject and worked out practical examples which were described by him before the Engineering Section of the

(Mr. William Taylor, O.B.E.)

British Association, and furnaces working on this principle were made by Messrs. Fletcher-Russell.

In the writer's experiments, an induction balance was used, consisting, as shown diagrammatically in Fig. 38, of two coils of insulated wire A, B, supplied with a rapidly pulsating current, and a third coil C between them and connected to a telephone D. The coil C was placed in the magnetic centre between the coils A and B,

FIG. 38.—*Induction Balance.*

whose effects on C thus neutralized one another, and, under these conditions, no sound could be heard in the telephone. When, however, a piece of iron or steel such as E was placed in the field, for example between the coils C and B, so that the magnetic balance was disturbed, sound was heard in the telephone.

Using this apparatus, he heated pieces of steel until they became non-magnetic, that is, until the sound in the telephone ceased, which it always did somewhat abruptly. He found that carbon steel or case-hardened mild steel would harden properly if quenched immediately the steel became non-magnetic, providing the pieces were less than about 0.3 inch diameter. If they were larger, then they had to be heated somewhat above the magnetic change-point or they would not harden properly. It was not, he believed, generally appreciated, and the Author did not mention that, in order to secure hardening, it was necessary to heat the steel above

this temperature to an extent which depended on the size of the mass. Mr. Lineham's experiments on gauges appeared to have been conducted with gauges of substantially one size.

During the War the writer's Firm (Messrs. Taylor, Taylor and Hobson) made large numbers of screw-gauges, latterly of mild steel case-hardened. The threads were milled on these gauges in special machines, and were generally untouched by lapping or otherwise after milling. They were merely case-hardened in cyanide, much in the way described by Mr. Lineham. At first they found the results in change of size to be somewhat erratic, but this difficulty was surmounted by heat treatment of the steel previous to the milling of the threads. They adopted the practice of first heating the annealed blanks to about 850°C . ($1,562^{\circ}\text{F}$.), that is, above the Ar_2 point, quenching in water, and then annealing from about 740°C . ($1,364^{\circ}\text{F}$.). In this way the steel was put through its paces before the hardening proper. After the blanks were threaded, the screws were cased in cyanide heated to about 740°C ., and, according to the measurement of pitch which had been previously made, so they were quenched either in water or in oil. If the pitch had been shown by previous measurement to be short, the gauge was quenched in water to lengthen it, while if the pitch had been long the gauge was quenched in oil, which treatment shortened the pitch. The hardener became so skilled that he could, with practical certainty, expand or shorten the pitch of a gauge by 0.0001 inch or more, as required. The gauges quenched in oil were not so hard as those quenched in water.

The Author mentioned the subject of ring-gauges. Change of size of hole when hardening rings might be largely prevented by proportioning the rings suitably. It was common practice with certain press dies and drawing dies to make the die-blank large in relation to the hole in it. By rehardening such a die, the hole might be closed in, presumably because the outer zones of the die were cooled first and shrunk upon the inner zones. On the other hand, if the hole were large relatively to the diameter of the blank, then generally the hole became expanded in hardening the blank. Between these two extremes there was some ratio of blank diameter

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to hole diameter at which the diameter of the hole was fairly stable. This depended also upon the thickness of the blank. It was a subject which should have systematic research, being, like most of the questions raised in these interesting Papers, of great practical importance in the workshop, particularly to tool-makers.

Mr. RALPH WOLFENDEN, M.B.E., M.Sc., wrote that, during the War, he was Assistant-in-Charge of the Plain Gauge Testing Department at the National Physical Laboratory, and a great many gauges made by the late Mr. Lineham at the Goldsmiths' College passed through his hands. He thought that the remarks made by Colonel Crompton were much too severe and were probably made without cognizance of the circumstances under which Mr. Lineham was working. At the time of these experiments, the Author was making very large quantities of gauges—screw-gauges, plug, ring, form, and position gauges—and he was pressed by the Ministry of Munitions for delivery as quickly as possible. In carrying out these experiments he was not making an elaborate research on hardening, but was endeavouring, in as short a time as possible, to get over some of his difficulties in obtaining a hardened gauge accurate to within very small tolerances. He succeeded in doing this to quite a remarkable extent, and, during the time the writer was at the National Physical Laboratory, he regarded the Author's plain gauge work as equal to the work of the best gauge-makers in the country, both for accuracy and hardness. The writer's experience at the National Physical Laboratory led him to believe that Colonel Crompton's own work on hardening without distortion was not known to the general body of gauge-makers, and was probably not easily accessible, as the difficulties Mr. Lineham experienced were shared by a large number of makers, and few got over them as satisfactorily as Mr. Lineham did.

The remarks made by one of the speakers on the change of dimensions with time were very interesting, and were fully borne out by the writer's experience. The writer believed that the importance and magnitude of this change was so recognized by the Canadian Munition Gauge Department, that every gauge issued

from their storeroom was retested before issue, and that many gauges, which were within limits at the original test, had to be rejected for service on the second test.

Mr. G. T. WHITE wrote, in reply, that it was difficult to understand why Colonel Crompton should attack the Author of the Papers for not knowing where particular information was hidden. Surely there was less discredit in ignorance than in possessing knowledge and remaining dumb when that knowledge was badly needed for the general welfare of one's country. If Colonel Crompton had gone to the Ministry of Munitions or to the National Physical Laboratory with the information he possessed, that information would have been passed on to those who were concerned with its useful application, and time spent on independent research would have been saved and the delivery of munitions speeded up. The Author of the Papers, immediately he had discovered and proved the value of a certain process, took steps to make it known, first through this Institution and then through the Ministry of Munitions, to others engaged upon similar work to his own. There was no withholding information. He (the Author) threw his contribution into the pool without invitation or pressure.

The tap difficulty mentioned by Colonel Crompton and Mr. Rolt, and at the Manchester Meeting, was experienced at Goldsmiths' College in the making of screw ring-gauges, but it was overcome sufficiently to be able to produce very small screw ring-gauges with great ease. It was the Author's intention to investigate the distortion of taps, but ill health prevented.

The criticism on the First Law of Hardening was upon the words in which it was expressed rather than upon its existence. The statement of any law with its qualifications omitted was open to the kind of objection raised. The root idea, however, was contained in the words used by the Author of the Papers, and they scarcely required defence; but, for the benefit of any reader who might be misled, the following words were taken from Sauveur's "Metallography and Heat Treatment of Iron and Steel," chap. xviii., p. 308.

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"The retention theories claim that, in hardening steel, a condition or set of conditions existing normally above its critical range is retained unchanged in the cold or but partially changed because of the rapid cooling through and below the critical range. In other words, such very quick cooling through the range denies the necessary time for the transformations to take place, at least fully, and *a condition is preserved in the cold which is stable only above or within the critical range.*"

The Author's Second Law of Hardening claimed least distortion only; no claim was made for any particular degree of hardness; but it should be pointed out that gauges quenched at 700° C. showed excellent hardness. Manufacturers who visited Goldsmiths' College, at the invitation of the Ministry of Munitions to see the methods pursued there, all expressed the same high opinion, and the N.P.L. has already given its testimony in that direction.

If the Author had been aware of Mr. William Taylor's discovery and use of the change of magnetic properties of steel at certain temperatures, he would unhesitatingly have mentioned the fact. On behalf of the Author he asked Mr. Taylor to pardon the sin of ignorance.

The information required by Mr. J. F. Driver was that the cutting compound was lard, oil and turpentine. Blanks were heated in a muffle furnace to about 800° C. and allowed to cool down in the furnace. No chemical analyses were made; the figure 0.14 per cent carbon was that given by the steel maker. Sodium cyanide was more easily obtainable and certainly cheaper than potassium cyanide. The seven specimens of each set were from the same bar. The Author advanced the opinion, though unable to support it by the results of experiments at the time of writing the Papers, that the quenching temperatures advocated were equally suitable for other carbon steels.

Mr. Driver objected to the store furnace, suggesting it was unnecessary. This was true, but its advantage was considerable, as pointed out in the text of the Paper; and it might be to no small extent responsible for the uniformity of results obtained on the gauges. He and others raised the question of fall of temperature

in passing from the salt bath to the quenching tank (a distance of 6 feet, centre to centre). That a fall would occur had not escaped notice; it was a fairly constant subject of discussion; but as furnace (salt bath) temperatures were the only ones that could be known with certainty and used by the operator, naturally they were the ones quoted. Since the specimens and the gauges were put through exactly the same movements in their heat treatment, it was reasonable to assume that the fall in temperature during transport to the quenching tank was the same in each case and did not need compensating in any way. It would be hardly fair to put forward as a manufacturing possibility the temperature at entering the quenching bath without at the same time indicating how it could be obtained. The use of the vertical tube furnace mentioned by Mr. Rolt and Mr. Ayres reduced the loss in question to practically nil, and furnished a variation in the heat treatment; for repetition work in this case a fresh set of quenching temperature experiments would be required.

Concerning scleroscope hardness, referred to by Mr. Driver and Dr. Bramley, the reader of the Paper (Mr. White) was of the opinion that scleroscope tests did not compare surface hardness where the hard skin was only 0.003 inch to 0.005 inch thick and had a soft backing. The scratch test appeared much more correct. It was significant that Mr. Driver found the average hardness of a particular batch of gauges, oil hardened by the methods suggested in the Paper, to be 67, the same figure as that given for his blanks before machining. Obviously some measurements were unreliable in his tests. Mr. Driver's experiment upon a specimen taken from across the diameter of a $3\frac{1}{2}$ -inch bar was more than interesting, and supported the remarks of other speakers concerning longitudinal distortion; it was not specifically stated, however, whether the test-piece received any annealing treatment before machining and hardening.

With regard to the tolerances of the examples (page 473), the differences between the high and low limits of diameter were:— 0.0008 inch on the full, 0.001 inch on the effective and 0.001 inch on the core; so that the diameter distortion fell within the

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tolerance. It should, however, be pointed out that these two gauges were some of the first to go through the new treatment, and were not selected as being specially good. Later, much better results were obtained as the Table (page 526) would show.

Several typical gauges, from 1.5 inch diameter down to 0.1038 inch, together with many actual screw docketts (sheets on which the measurements of a gauge at the several stages of its manufacture were recorded) were at the Meeting for inspection.

Oval distortion was not so serious as to attract attention nor to cause rejection of the gauges by the N.P.L.

Mr. G. H. Ayres' point concerning fall of temperature in the passage to the quenching bath was answered in the reply to Mr. Driver on the same subject. As to chilling due to draught, no observation was made.

Inflow and outflow cocks to the cooling tank were to facilitate keeping the temperature of the water in the quenching tank near that of tap-water which was approximately 8° C. (46° F.) in winter and 12° C. (54° F.) in summer. The oil tank was deep and suspended in the water tank, and the provision of water circulation served to keep the oil cool. The figures 22° C. (72° F.) and 29° C. (84° F.) covered the rise in temperature of the oil over the series of dippings; the actual temperature of the oil at each individual immersion was not taken. No experiment was made using a layer of oil on water. All gauges were made to enter the quenching bath in the direction of their length, that is, they were dipped vertically.

Dr. A. Bramley seemed to have missed the fact that the specimens, Figs. 5 and 6 (page 459), came into the Author's possession after the heating and cooling treatment. Both specimens were at the Meeting in London. There was oxidation and scale on them, and some material might have been lost by scaling off, also some weight and volume might have been gained by oxidation; but that the loss or gain had been small was evidenced by the sharpness of the original edges. A third specimen, not illustrated, subjected to the same treatment and exhibiting the same kind of distortion, though not carried quite so far, when cut through showed a perfectly sound interior.

The words used by the Author (page 466), "Nature insists on keeping the volume constant," were perhaps unhappily chosen. Their intention was to convey the idea that successive heating and cooling alone would not tend to increase volume indefinitely; that at the end of each cycle of operations (a heating and a cooling) the volume would be the same and the metal in the same state, or that the well-known phenomenon of increase of volume of carbon steel on hardening was not accumulative. It might not be so well known as the phenomenon just referred to, that successive hardening tended to reduce the difference between the hardened and annealed states of a piece of steel. Whether this reduction went on until the difference became zero, the writer (Mr. White) could not say. In the case of the higher carbon steels, where the difference was more marked, it might be impossible to determine, on account of possible loss of carbon changing the nature of the steel. With the specimens of Figs. 5 and 6 the limit of difference was probably reached, but what *exact* relation the present volume bore to the original could not be stated. The specimens were introduced as being remarkable examples of distortion and bearing on the subject of the Papers in that they illustrated the tendency of a quenched piece of steel to assume the spherical form.

Dr. Bramley took grave exception to the Author's deduction from his experiments that 700°C. was the most favourable temperature at which to quench the steel, and looked upon the crossing of the curves, Fig. 13, at 700°C. as a remarkable coincidence rather than indicative of a fundamental law. The reply to this was that the success following the adoption of the Author's conclusions was also remarkable, and was good evidence of their value, Fig. 40 (page 524). In connexion with this fundamental law, he (Mr. White) would like to add that it might be confidently looked for in the near future that reliable authoritative experiments would demonstrate that quenching within the lower critical range, and probably the nearer its lower limit the better, would result in the least increase in volume and least distortion whatever the carbon percentage of the steel; hardness might, however, suffer by using the lower temperatures.

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FIG. 40.

Screw-gauge Manufacture for Aeroplane Engines, 1918, at Goldsmiths' College.

Curve of Weekly Deliveries and Curve of Rejections (temporary and all subsequently corrected).

Concerning the omission of Fig. 7 (page 462) from the final deductions to which Dr. Bramley objected, omitting the effect of the bulge from the end measurements would certainly bring the curve lower. The experiment was included in the Paper in order that nothing should be withheld that had any bearing on the object sought after. The difficulty of determining the average dimension over two bulged surfaces, and as the Author admitted initially not quite parallel, would appeal to Dr. Bramley. Also as an experimenter he would appreciate that, in first entering an unknown field, inconsistent results were likely to be obtained until the methods pursued were reduced to some rigidity. The curve of Fig. 7 detracted very little indeed from the combination of Figs. 8 to 12 in Fig. 13. It was comparatively so widely separated from them that there was strong suspicion of a different set of conditions existing. The matter of hardness and the use of the scleroscope had already been replied to. Trouble from soft patches was not experienced. Reference to previous work carried out by other investigators in this field of research was not given, because the Author knew of none bearing sufficiently closely upon the matter.

Mr. J. E. Baty's experiments on the screwed specimens seemed to support, rather than to disprove, the Author's conclusions. He (Mr. Baty) had treated two pieces in an exactly similar manner and had obtained different results, which the Paper distinctly warned him would occur if water quenching were carried out even a few degrees above 700°C . With regard to the influence of the ratio of length to diameter, a quick survey of a number of screw dockets, chosen so as to illustrate the influence of the ratio was given in the following Table (page 526).

From that Table it might be inferred that diameter suffered when the ratio of length to diameter varied much from unity. More careful analysis of these and several hundreds more dockets, covering the work at Goldsmiths' College, might alter the figures given for average diameter change, but the alteration would be slight. In nearly every case the effective diameter showed the greatest expansion, and the difference between core and full diameters was increased after hardening.

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*Diameter Changes of Oil-Hardened Screw-Gauges for various
Ratios of Length to Diameter.*

Number of Dockets in the batch.	Screwed Portion : Length Diameter =	Max. Change in Diameter after quenching.	Average Change in Diameter after quenching.
66 (20 thd Whit)	$\frac{1}{1.525 \text{ to } 1.2} =$ $0.65 \text{ to } 0.83$	9 gauges ; +0.0005 5 gauges ; - 0.0005	+0.0002 to +0.0003
130 (19 thd Whit) some water- hardened	$\frac{0.281}{1.06} = 0.265$	30 gauges ; +0.0005 and above 20 gauges ; -0.0005	+0.0003 to +0.0004
44 (No. 4, B.A.)	$\frac{0.4875}{0.14173} = 3.08$	17 gauges ; +0.0005 and above 2 gauges ; -0.0005	+0.0003
34 (No. 4, B.A.)	$\frac{0.5}{0.14173} = 3.5$	20 gauges ; +0.0005 up to +0.0009 None minus	+0.0004
Pitch difference after hardening ; generally + 0.0001 to -0.0001			

The objection to the use of test specimens of different dimensions from the gauges would be removed if it were remembered that the tests in both cases, water and oil quenching, had been made with the object of finding the temperature that gave least distortion in pitch. If such temperature existed, what mattered the size of the test-pieces used in searching for it? The actual distortion on any particular size and shape of article was another consideration, and could be found with less expense and trouble when the temperature of least distortion had been found by the aid of the simple and cheap test-pieces.

Mr. Baty stated (page 501), "it was generally contended by steel-makers that one ought to harden on a rising temperature," but how the steel-maker or anyone else could perform such an operation

he did not suggest. Dr. Rosenhain's special quenching apparatus* was probably the only piece of apparatus that could effectually quench on a rising temperature. The vertical tube furnace made it possible to quench on a practically stationary temperature, but in all cases where the object to be cooled was removed from the furnace before immersion in the cold bath the temperature must be a falling one. Quenching on a falling temperature would seem to be better than quenching on a rising one, for it offered the advantage of the delay in the transformations that took place within the critical range.

On behalf of the Author of the Papers and himself, he thanked Mr. F. H. Rolt for his good report on the gauges manufactured at Goldsmiths' College. With regard to Mr. Rolt's doubts about the size of the test-specimens being well chosen, in the reply to Mr. Baty it was pointed out that their size had no bearing upon the object of their use. And with regard to the fall of temperature between the furnace and the quenching tank, an endeavour had been made in the reply to Mr. Driver on the same subject to show that allowance for the fall was unnecessary, on account of the similarity of treatment of the gauges and the specimens.

The vertical tube furnace, mentioned by Mr. Rolt, with its reducing atmosphere and close proximity to the quenching tank offered advantages that could not be denied, especially so if the gauge or other hardened article presented after quenching the smooth untarnished surface it had in its soft state.

Mr. Pilling asked for further information respecting the steel disk, Fig. 5 (page 459). By some unfortunate circumstance the original information at the top of Fig. 5 did not agree with the dimensions on the illustration.† The note (page 529) contained all the information that was now obtainable.

Mr. C. E. Stromeier's query about the gauges changing shape with age was answered in a general way in the affirmative. It was admitted that hardened carbon-steels altered their form with age

* Journal, Iron and Steel Inst. 1908, I, page 87.

† The figures have now been corrected.

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Probably the higher the carbon percentage, the greater the liability to change. Mechanics who handled very accurately finished hardened steel tools, such as straight edges and squares, had constantly to rectify changes of shape which were certainly not due to wear, especially during the first few months of the life of the tool. The rejection of some of the gauges made at Goldsmiths' College could only be accounted for by spontaneous distortion between the times of final measurement at the College and measurement at the National Physical Laboratory.

Mr. P. R. Davenport's experience of great distortion when quenching at 800°C . was not surprising, as the temperature at which he quenched would give almost maximum increase in volume on hardening.

Mr. Pilling in his concluding remark, expressed very accurately the essential point of the Papers, which attempted to prove that the prescribed temperature was at the lower critical point.

Thanks were due to Mr. Ayres for the detailed description of the Electric Hardening Furnace, and to Mr. Taylor for the description of his furnace and for the correction of the Author's statement concerning the first practical application of the change of magnetic properties of steel at high temperatures, each of which he (Mr. White) was sure would be welcome as a fitting addition to the Papers.

Mr. Ferdinand Kayser referred (page 514) to a gauge having to cut other threads. The gauge under these circumstances became a tap, and the thin case produced by cyanide was not advocated for tapping purposes, although such taps were used at Goldsmiths' College for the final sizing operations on the small screw ring-gauges. It was cheap and easy to make accurate taps on this principle, but their life was short.

Mr. Kayser's statement that many of the treatments mentioned in the Papers would not produce hardness at all, was disproved by the fact that between 2000 and 3000 gauges so treated (oil hardened) were passed by the N.P.L. The heaviest ones weighed 18 oz., the screwed portion being 1.525 inch diameter by 1.06 inch length and the proportion by volume of the screwed part to handle 0.415 to 0.71.

Mr. Kayser also pointed out that "thermal hysteresis" caused the arrest points on heating to be considerably above those on cooling. This was to the advantage of the article being quenched on a falling temperature, as mentioned in the reply to Mr. Baty's remarks at the London meeting.

Referring to Mr. William Taylor's description (page 516) of the methods employed by his Company, the lengthening of the gauge by water hardening and shortening it by oil hardening, was in fair harmony with the curves on pages 464 (the water curves) and 472 (the oil curves); the former showed expansion in nearly every case and the latter, omitting the unannealed specimens, showed contraction as well as expansion, both very close to the zero line.

On behalf of the Author, he thanked Mr. Wolfenden for his appreciative remarks concerning the gauges made at Goldsmiths' College.

Note of Correction of Fig. 5 (page 459). Mr. WHITE regretted that an error in the Paper on Water Hardening was allowed to pass uncorrected, before the issue of the preliminary copies. On pages 8 and 9 of those copies the linear dimensions and volume as given for the original disk, Fig. 5, were incorrect. The size of the original was not known. When the Papers were under revision, the specimens were not available. They however came into Mr. White's possession three days before the reading of the Papers in London, and since then he had measured the distorted [piece, Fig. 5, and found the linear dimensions as given on the sketch to be correct, but the volume measured by displacement of distilled water was 9.315 cubic inches. Not having the Author's rough notes, he could not say how the error crept in. It would appear that the dimensions given at the top of Fig. 5 must refer to another piece of steel, which he had been unable to obtain. He felt confident that Mr. Lineham had good grounds for stating the figures he did, and that the volumes before and after distortion as given on page 9 of the preliminary copies were correct for some specimen now lost. The present weight and volume of the piece illustrated at Fig. 5 were now inserted on page 459.