DISCUSSION.

Sir Robert Hadfield opened the general discussion which followed the reading of the Papers.

I much regret that Professor Cohen is unable to be present to-night. We all greatly admire his country—Holland—and feel special sympathy with her at the present time owing to the difficult position in which she is placed during the present lamentable war.

The Society is to be congratulated on receiving a Paper all the way from America from the eminent Professor Howe, also on the presence of Professor Turner of Birmingham, who was one of the first of those who started systematic observations on the hardness of metals.

I am glad to see that Professor Howe seems to be coming back to the carbonist theory, and that Dr. Stead emphasizes the fact that the main factor in the cause of the hardness of iron alloys is carbon.

I still think that the question we should study is that relating to the combinations which take place between iron and carbon and from which we obtain intense hardness when heated carbon steel is quenched. I feel so strongly on this matter that I have offered through the Iron and Steel Institute a prize of £200 for the best study of carbides of iron and iron alloys generally. I hope Members of this Society will take part in the competition, full particulars of which can be obtained from the Secretary of the Iron and Steel Institute, 28 Victoria Street, Westminster, London, S.W.

Dr. J. E. Stead, F.R.S. (communicated) : It is my opinion that we have not sufficient data on which to found any definite conclusion as to why steel is hardened by heating to above Ac_{1-2-3} followed by quenching. One thing is certain, and that is the carbon is mainly responsible. The different behaviour of hardened and annealed steel when treated with chemical reagents, such as sulphuric and nitric acid, is absolute proof that there is a difference in the chemical constitution of the two. The difficulty is, and always will be, to determine what that difference really is. The inducement the President has offered to study this side of the question is likely to lead to valuable results.

It is certain that the discussion of the question of hardening of steel and other metals must lead to the elimination of untenable hypotheses and to a clearing of the way for the truth.

The allotropic theory of Osmond was never advanced as other than one of a tentative character, and I have always regarded it as such.

The purely solid solution hypothesis, that the hardness of quenched metals is due to intimate solid mixture, may be one of several which point in the right direction.

The internal stress hypothesis may be more or less correct, but it is well to remember that no matter how much 0.90 per cent. carbon steel is stressed and distorted in the cold, it never can be made intensely hard or become what is described as glass hard.

I have for many years studied the iron and phosphorus compounds. Iron saturated with about 1.7 per cent. phosphorus is difficult to drill, yet Cleveland pig iron or any grey iron containing even 2 per cent. or more phosphorus can be readily machined. The difference is coincident with a difference in the "phosphorus condition."

In the hard material the phosphorus is in solid solution, Fe_3P in iron, while in the pig irons the Fe_3P is in a free and separate state. In one of the Mexican meteorites the greater part of the phosphorus, 0.27 per cent., exists

in independent crystals of $(\text{FeNi})_3\text{P}$, and the metal is very soft. On heating to near the melting-point the free phosphides diffuse and pass into solid solution, and the metal when cold is sensibly harder. Further, when treated with acid practically no phosphine gas is evolved from the metal containing free phosphide, but much of that gas is evolved on similarly treating the other metal containing the phosphide in solid solution.

There is a close analogy between steel, and iron containing phosphorus, but it is premature to state that solid solution alone is responsible for the hardness of quenched steel.

The meteorite above referred to contained at least 100 twin laminae of the Neumann type in each eighth of an inch (produced no doubt by strain during very slow cooling of the original large mass). These laminae, which I have proved beyond all doubt to be twins, are eliminated on heating to 800°C ., but the metal did not become sensibly softer. Twinning, therefore, is no indication of hardness. The same steel containing 0.90 per cent. carbon, whether quenched from 750°C . or 900°C ., is equally hard. The first has a barely detectable martensite structure, while in the second that structure is very pronounced. The internal stresses presumably must be greater in the second than in the first, yet the hardness is not different.

Nickel steel containing 4 per cent. nickel and 0.4 per cent. carbon on quenching from 900°C . is hard and brittle, and has a pronounced and gross martensite structure, yet after re-heating to 500°C . for a short time the structure remains; indeed, one structure may be taken for the other, but the quenched metal is hard and the annealed metal soft. Difference in this case is certainly in the carbon condition.

In conclusion, I must again state that we have not sufficient data on which to base dogmatic conclusions. All hypotheses, the results of careful observation, may be tentatively held till they are proved untenable or otherwise. It is most desirable that more attention should be given to the study of carbon condition, for there can be little doubt that on this mainly depends the extreme hardness of steel.

Mr. Andrew McCance (*communicated*): I do not wish to question in any way the immense service that Dr. Beilby's theory of the amorphous phase has done to the study of the hardening of metals by deformation, and the insight and knowledge it has given us of the crystalline structure. But as new facts appear it is necessary to inquire if these are explained with the same elegance and facility as the older facts were. I would emphasize particularly the hardness produced without permanent deformation by the action of high pressures on metals, and the difficulties of explaining the magnetic properties of strained alpha and strained gamma iron. Has it been shown experimentally that the amorphous state is *actually* harder than the crystalline state for any substance at the same temperature; is transparent glass, for instance, at ordinary temperatures harder than the same glass devitrified? Also, since the temperature-hardness curve for the crystalline state is different from that of the amorphous state, and at higher temperatures the amorphous state is the softer, it follows that there will be a temperature at which permanent deformation softens instead of hardening.

Regarding the magnetic properties, it is admitted, of course, that our knowledge of magnetism is still highly speculative, but the electron theory of Langevin-Weiss gives values of fundamental physical constants which are in complete agreement with other independent calculations, proving that the primary assumptions are true.

Mr. Humfrey's views are almost identical with those which I have published, and he also realizes the difficulty regarding the correlation of the

magnetic properties with the amorphous phase theory. He gets over this, however, by assuming that in the crystalline space lattice it is the molecules which are regularly spaced, and that in each molecule the atoms are similarly arranged. This is in contradiction with the stated views of physicists that in a crystalline space lattice it is the atoms which are alone concerned, and that in a binary compound the space lattices of the two elements are mutually interlaced, giving rise to a new space lattice formed by the atoms of both.

His view that structureless martensite is formed when all the austenite has been transformed to amorphous alpha iron, while the more prominent structure obtained from a higher quenching temperature is due to the retention of much austenite, is not in accordance with experimental evidence, for I have shown that a steel containing 0.86 per cent. carbon has the same percentage of gamma iron whether it is quenched from 760° or 1,000° C. The martensitic structure represents the cleavage planes of the gamma iron crystals at the temperature of quenching, and the apparent lack of structure of steels quenched from 760° C. is due to the very small size of these crystals.

In general, however, my own views and his are in complete agreement.

Professor T. Turner (University of Birmingham) said they as visitors were much indebted to the Faraday Society for providing an opportunity of discussing a subject of so great importance in the arts and manufactures. Sometimes a metal is required to be soft, and sometimes its value depends on its being hard, and it is important to know the reason why a particular sample is hard or not hard. Dr. Beilby's remarks in reference to the importance of further knowledge of the molecular structure of the metals and the variation of specific heat were very suggestive. It has long been noticed that the hardness of pure metals is proportional to their density divided by their atomic weight.* It therefore varies inversely as the atomic volume, and is greater the more closely the atoms, or rather the molecules, are packed together in the material.†

Passing from pure metals, there would appear to be two distinct kinds of increased hardness, each of which depends on an alteration in molecular distances. The mental conception that the difference in hardness ultimately depends on the alteration in the molecular structure of the material appears to be justified. Mechanical deformation increases the hardness, and chemical alteration, which changes the packing and nearness of the molecules, also produces additional hardness. These two kinds of added hardness should be kept mentally distinct.

Metals are practically incompressible and are impermeable by gases. Hence the molecular interspaces are probably very small, and this is in accord with the fact that the change of volume due to the result of work upon a metal is relatively very small, although the hardness may be enormously increased. Therefore, as the change in the molecular distance caused by work, or by solution of one metal in another, is very small, the actual molecular distances themselves must also be very small.

It has been pointed out by Dr. Brislee that metals may become lighter by rolling. As a matter of fact the first effect is generally that of increasing the density as a result of squeezing and consequent closing of the pores. With further work the density is diminished. The effect of rolling a metal is usually supposed to be that of compression, but examination will show that the material just before it leaves the rolls is in tension, while as it enters the rolls it is in compression.

* Bottone, *Chem. News*, 1873, p. 215.

† T. Turner, *Chem. News*, 1887, vol. Iv., p. 179.

The next point to consider is the effect produced by solution on molecular distance and therefore on hardness. Dr. Lowry mentioned, in reading Dr. Desch's paper, that in solid solutions we always have an increase in hardness. I believe he also said that the intermetallic compounds are softer than the metals of which they are composed. The latter rule is by no means invariable, as has been shown in work on the copper-zinc series of alloys, in which the compound, Cu_2Zn_3 , is very much harder than either of its constituents.* In the copper-tin series also a hard compound is well known.

When one substance dissolves in another, very large forces must be brought into play, as there must be a very considerable attraction between the particles of the solvent and the particles of the solute. Now those forces must bring the molecules closer together, as Tammann points out, and therefore we can have a similar effect produced by chemical means to that produced by mechanical means in the first kind of hardness. One important difference between the two means of producing hardness is that as far as mechanical means are concerned it is seldom possible to more than double the tenacity of the metal, but with solid solutions it is frequently possible to obtain a very considerable increase of tenacity accompanying the increase of hardness.

He (the speaker) preferred to explain almost the whole of the changes which take place in various steels by the suggestion that the carbides are taken in or out of solution at different temperatures, and as the solid solutions vary in character and concentration, different degrees of hardness result. The solubility of the carbides would doubtless depend, at least in part, on any allotropic changes in the iron. Internal strain may also produce considerable effect.

Although the presence of twinned crystals indicates there has been a previous strain, it does not prove that the metal is hard. The material may have been hard at one stage of its existence, but the presence of twinned crystals does not prove the metal to be still hard. The suggestion is rather the other way, namely, that annealing has ensued. Thus hard rolled brass does not show twinned crystals, but when it has been annealed twinning is plainly seen. This indicates that the metal has been in the hardened state once, and now it has an opportunity of showing the result of what then occurred.

Dr. Lowry referred to annealing at a specially low temperature. The speaker used to show an experiment to his students with a fusible alloy containing 8 parts of bismuth, 3 of tin, and 5 of lead, which is brittle when cast quickly in water, but which is annealed if held in the hand. At the same time very appreciable heat is evolved.

Dr. W. H. Hatfield (Sheffield): I have been extremely interested in the whole of the Papers given this evening, and I am sure that the Society will have done much to advance the theoretical side of the subject by this discussion. It seems to me that at least three of the theories brought forward to-night are the direct results of Dr. Beilby's work and his subsequent writings on the amorphous theory of the hardening of metals. Dr. Beilby's work has undoubtedly led to a considerable advance in our understanding of this subject of hardness, and, whether or not the amorphous theory will finally prove tenable, his contributions have led to such a wide discussion of the subject that they have undoubtedly been responsible for considerable advances in our knowledge. Some time ago I had the pleasure of receiving from Dr. Beilby a complete set of his works in this field, and there is no doubt it ought to receive the attention of any one interested in the problem before

* *Journ. Institute of Metals*, 1909, vol. ii., p. 130.

us. As an iron and steel metallurgist I naturally endeavoured to see how and where his theory would fit in with the facts which I meet in my daily practice. What do I find? As regards the effect of cold work on pure metals, his amorphous theory would appear to apply, but as regards the question of the hardening of steel, I am doubtful. The quenched hardness of carbon steel is one of the outstanding important phenomena in physical metallurgy, and to my mind appears quite unique. For instance, if you take a piece of pure iron and submit it to cold work, you increase in considerable measure both the tenacity and hardness. Take the same piece of pure iron and quench it from the gamma range and the hardness produced is inconsiderable. Now, if carbon be added to this pure iron and the quenching is done from the gamma range, you produce with the greatest ease a hardness that is out of all proportion to the hardness obtained by cold work or by quenching of other industrial metals and metallic compounds. I should like to recall to the meeting the interesting experiments executed by Dr. Arnold a number of years ago. As he had not at his disposal pure iron, he experimented with iron containing 0.07 per cent. of carbon, and this material he quenched at different temperatures up to well within the gamma range of temperatures. This material, quenched from 780° C., had a tensile strength of 29 tons per square inch, and when quenched from well within the gamma range its tensile strength was still only 32 tons. Obviously then, after putting upon the iron all the "work" and internal stress which quenching from such high temperature must do, hardness and hence the hard amorphous phase is not obtained. But, put the necessary quantity of carbon into the composition and the quenched hardness of steel is obtained out of all proportion to any hardness to be obtained by cold working. In my opinion this central point must always be kept before us when considering the subject. I have already said I was interested and indeed fascinated by the logic of the amorphous theory as originally advanced by Dr. Beilby and subsequently developed in different directions by Dr. Rosenhain and Mr. Humfrey, and in endeavouring to explain hardness by quenching in its terms I arrived logically and quite independently at the identical point of view expressed by Mr. Humfrey in his Paper of this evening. It is obvious that when the gamma iron changes to alpha iron (beta iron being considered non-existent) the metal passes through the mobile or amorphous phase. Now *if* the amorphous phase is intensely hard and *if* in quenching you can trap it *en masse*, you would appear to have a logical explanation of the quenched hardness according to the amorphous theory. But, it may be pointed out, there are a number of considerations which require carefully weighing before such a view can be accepted. For instance, Mr. Humfrey suggests that troostite, the first decomposition product from the solid solution, is amorphous iron intermixed with cementite. Now, if this troostite is amorphous iron, it should be equally hard with the martensite from which it is derived. That is not the case, troostite being distinctly softer. Furthermore, if the troostite is amorphous iron, is it not fair to presume that the cementite is in solution? At any rate this is the view generally held as to the solubility of the cementite in amorphous iron. The original idea still held by a number of the exponents of the amorphous theory is that the amorphous phase produced in the transference from gamma to alpha iron is identical with the undercooled liquid phase, and this being so, the cementite would be soluble. As a matter of fact, the amorphous phase that might be considered to be produced during the change from gamma to alpha must be an entirely distinct phase from that of the undercooled liquid, and Humfrey is assuming this position when he says that the secondary amorphous phase does not dissolve carbon or carbide; hence his suggestion that troostite

does not contain the carbon in solution. After careful consideration, however, I have come to the conclusion that the amorphous theory does not offer a completely satisfactory explanation either when considered from Dr. Edward's and Dr. Carpenter's or from Mr. Humfrey's standpoint.

Turning to Mr. McCance's Paper, I should like to draw his attention to his preliminary statement as to the three methods of producing hardness in a pure metal. His second method, i.e. that of adding other metals, etc., cannot be considered, since the hardness which may, or may not, result is not the hardness of the *pure* metal but of the *alloy* produced. In summing up the question of the hardening of steel in my own mind, I feel distinctly driven back upon the original idea, well supported as it is by Dr. Desch's Paper before us this evening, that hardness of quenched steel is due to the inherent hardness of the solid solution of carbide in iron trapped by quenching. This quenched solid solution of such outstanding properties is hard like a number of other hard substances, and will probably have to be treated from an entirely different standpoint to that before us in the amorphous idea. It must be borne in mind that whilst Dr. Beilby has established the existence of the mobile or amorphous phase under certain conditions, the explanations offered by the amorphous theory of the hardening of steel, and indeed the hardening of cold-worked metals, are, as yet, only theories, and it is well that we should bear in mind that the so-called amorphous phase, whether in cold-worked metals or in quenched metals, is "ultramicroscopic."

In conclusion, might I ask Dr. Beilby, whose brilliant work we all so much admire, to recapitulate from his point of view the definite facts upon which the amorphous idea stands as distinct from the bulk of hypothetical matter that has accumulated round it owing to the efforts of some of his strenuous and enthusiastic disciples?

Mr. T. G. Elliot : Speaking as a chemist it is perhaps not unnatural that, I think, since the physicists have taken hold of the subject it has not been simplified in any way. Indeed, a perusal of the papers presented for discussion shows how involved it has become. This is particularly the case with regard to the hardening of iron, which years ago was thought to be dependent on the addition of carbon.

After an interval during which the hard beta iron theory has been strongly supported, it is interesting to see that Dr. Rosenhain, in his new book on *Physical Metallurgy*, gives a certain amount of support to a combined, carbon and amorphous cement theory; in other words, carbon may have something to do with the hardening of steel after all!

Professor Howe's Paper has particularly interested me, dealing as it does so much with Hadfield's manganese steel. In Sir Robert Hadfield's laboratory we have long noted the blackening effect of sodium picrate on the martensitic like needles mentioned in the footnote on page 267. In other words, there we have a martensitic structure which, according to Professor Howe, is not martensite. But what is martensite? Osmond described a *tempered steel* exhibiting a martensitic structure as a pseudomorph; it had the structure of martensite with the properties of troostite. At any rate, it is apparent that there is a very close connection between the martensitic structure, the falling out of solid solution of carbide, and the hardness of steel. Whilst there are undoubtedly other contributing causes of this hardness—such as the effect of stress—in my opinion the study of the relationship between iron, carbon, and manganese opens the most promising field for research on the subject.

Dr. F. J. Brislee (Roby) : I have come rather to learn than to teach. I would like to ask Dr. Beilby one or two questions. When a metal changes

from the crystalline to the amorphous state by mechanical work, as is often the case, should the tensile strength necessarily increase? I found that the density of aluminium decreased with working and increased with annealing. When worked, all the desirable properties disappeared. High tensile strength was aimed at, and the resulting metal may have had a strength of 16 tons per square inch, but it could not be bent at right angles without breaking.

I should like to know from Dr. Beilby if the passage from the amorphous to the crystalline state would have a definite transition-point, and if so, whether transition from the amorphous to the crystalline state, which in the case of aluminium increases its density, is identical with the annealing-point. I found that a sharp change took place in overworked aluminium at 230° C. Further, it might be pointed out that when a metal assumes an amorphous state, this is not necessarily an allotropic form of the metal.

Professor Cohen's contribution on the disintegration of lead is very interesting, as this may explain the cause of the trouble in underground lead, especially lead-covered electrical cables.

Mr. S. A. Main (Sheffield): In considering the various theories put forward it will be noticed how really little work has been done towards correlating in a quantitative manner the degree of hardness and the cause assigned for it. In fact, hardness is to a large extent regarded merely as a quality in contradistinction to softness. Hardness is of course a measurable quantity and there are degrees of hardness. It must be admitted that such quantitative confirmation is in many cases difficult to obtain. The difficulty is not lessened by the unsatisfactory nature of our present means of measuring hardness, especially with regard to degrees of hardness exceeding what is known as "glass-scratching hardness," in which is comprised the hardness of quenched steel, which it will be agreed is one of the chief problems in connection with the hardness of metals. It is to be hoped that the efforts of the Committee of the Institution of Mechanical Engineers may lead to hardness testing being placed on a more satisfactory basis. Whatever the cause, however, this lack of quantitative connection is an indication of the weakness of our present knowledge of the cause of hardness in metals.

For this reason the theory of the hardness of solid solutions dealt with in the Paper by Dr. Desch has one advantage over many other theories, in that the connection between hardness and its assigned cause—the attractions of the molecules—is carried to the extent of a quantitative relation which is even expressed by a physical equation. Moreover, it is much easier to associate hardness, a mechanical quality, in one's mind with the forces of attraction or cohesion between the molecules, than with the more or less hypothetical attributes of a more or less hypothetical constituent.

The simple faith of some allotropists in a hard form of iron is not much disturbed by the failure of beta iron. With characteristic resource they turn to yet another, but more obscure, form of iron; more obscure, that is, because we could always get a tangible specimen of unadulterated beta by heating pure iron to, say, 800°, while apparently we cannot handle a specimen of amorphous iron by itself and without the awkward interference of carbon.

Unfortunately for amorphous iron, it is just as vulnerable as beta at vital points. The grounds on which its intrinsic hardness is based are left very much to the imagination; the fact that some of the hardest materials known, e.g. diamond, quartz, carborundum, corundum, are essentially non-amorphous does not help us, nor does the method of reasoning afforded by the allotropists for the hardness of beta iron; the latter at about 780° C. having been

demonstrated by tenacity tests to be a little harder than alpha, we were asked to imagine that therefore at ordinary temperatures beta was many times harder than alpha; at this same temperature, 780°C., alpha iron is, we are told, rapidly softening with a view to becoming liquid alpha or amorphous iron (being prevented only by the timely intervention of A_2); that is, amorphous iron or liquid alpha softer than solid alpha, as we might expect. Is not amorphous iron therefore many times *softer* than alpha at ordinary temperature? On the contrary, we are to understand it is many times harder.

Again, from the point of view of magnetic qualities, quenched carbon steels, though hard, have a saturation specific magnetism of as much as 90 per cent. that of pure iron. We are not told what the magnetic properties of amorphous iron are, but as liquid alpha it is presumably, like other liquids and like liquid iron as we know it, practically non-magnetic. There is not much room for a non-magnetic constituent in a material of 90 per cent. magnetism—no more, in fact, than there was for beta.

How does this theory account for the hardness in the case of a 0.6 per cent. carbon steel, heated and allowed to cool through the A_{r32} point as slowly as possible, giving every opportunity for the complicated processes described, from gamma into amorphous and amorphous into alpha, to take place, including most of the residual gamma iron supposed to be in solution below A_{r32} , but quenching from just above A_r ; hardness, that is to say, with little or no amorphous iron?

Surely when the hardness of solid solutions is such an acknowledged fact, and indeed is confirmed by physico-mathematical investigation in some simple cases, it is not claiming too much to say that the hardness of quenched carbon steel is the hardness of a solution of carbon, an element which is itself capable of the highest degree of hardness, in iron—whether as carbide or not. At least, this aspect of the question appears to offer more substantial grounds for investigation than the hypothesis of inherent hardness in the iron.

Sir T. Kirke Rose: With regard to the twinning produced in tin by bending, I should like to ask Professor Edwards and Professor Carpenter how long they left the tin before examination, because tin anneals itself at the ordinary temperature in 24 hours. It follows that unless the photomicrographs were taken immediately after bending, the twinning shown in them might be due not to straining but to subsequent annealing.

(*Written communication*): As the temperature of annealing of tin does not appear to be generally known, I may point out that the results of my observations on the subject are given in the 43rd Annual Report of the Mint, 1912, p. 54. They are to the effect that the lowest temperature at which the softening of hard-rolled tin begins to be perceptible is about 10°C., and that the softening is practically complete in 24 hours at 15° and in less than a minute at 100°.

A summary of the observations themselves, which have not been published previously, is as follows:—

The mean hardness numbers given below are measurements by the Shore scleroscope, with the magnifier hammer. On this scale soft lead is 2, soft gold 5.5, and hard-rolled gold 33 to 35.

	Hardness Number.
Tin, cast	4.0 to 4.2
Tin rolled at — 25° C. (ratio of thickness, 8 to 1) ...	13
The same after 30 min. at — 20°	14
" " " " 0°	13
" " 1½ hours at 14°	10 to 10.5

						Hardness Number.
The same after 20 hours at 13°	...	...	...	...	...	6·3 to 6·6
„ „ 24 „ „ 15°	...	...	...	...	...	4·5
Tin rolled at 15°	...	...	...	...	...	9·5 to 10
The same after 97 days at the ordinary temperature	...	...	...	...	...	4·5
„ „ 217 days	...	...	...	...	...	4·2
Heated to 50° and quenched	...	...	...	...	...	8·6
„ 100° „ „	...	...	...	...	...	5·2

A different sample was used for each of the above experiments.

A strip of tin is "springy" just after being rolled, but by the next day the springiness has disappeared. As in other metals,* so in tin, there does not appear to be any critical *point* in the temperature of annealing, but there is a *critical range* of temperature within which the rate of annealing undergoes a rapid change. In the case of pure metals, the critical range of annealing is short, usually about 20°, but in impure metals and alloys the range is much longer, often amounting to 200° or 300°. Annealing here denotes simultaneous softening and recrystallization, but the new crystals proceed to grow for some time if the temperature is maintained or increased, without any further marked diminution of hardness. These facts do not rest on any theoretical considerations.

Dr. Lowry's observation as to the temperature of annealing of his dental alloy, AgSn₃, falls into line with those of other alloys, whose temperatures of annealing are often above that of the predominant metal present.†

Dr. G. T. Beilby (*partly communicated*): In connection with experimental work in the field covered by Section I, I have made a number of observations on the behaviour of pure iron. These observations show that in most respects iron falls in line with the other pure ductile metals, but the allotropy indicated by its thermal behaviour necessarily complicates the observations of electrical conductivity and thermoelectric potential. The magnetic properties may be expected to supply valuable information on the molecular arrangement in the two states, but I venture to suggest that these ought to be worked out exhaustively on the purest iron obtainable. Among the impurities to be shunned, hydrogen and nitrogen must not be ignored. My experiments show that while it is possible by cold working to raise the hardness or rigidity of pure iron to a very high point, it never reaches the hardness of tool steel. As regards the part played by carbon in the hardening of steel, I have not the expert knowledge of the subject which would entitle me to express strong views on the various theories which have been advanced, but from the point of view of a worker in the field of Section I it would appear to me perfectly natural that the presence of the small, strongly-cohesive molecules of carbon in an aggregate consisting mainly of iron molecules might add not only to the "intrinsic cohesive force" of the aggregate but also to its "effective cohesive force." This is only another way of putting an idea which we really owe to Roberts-Austen.

I do not like the term "amorphous" any more than Mr. McCance does. I often regret not having adhered to "vitreous," which is the term I applied to the flowed surface layer on metals in 1901. "Amorphous" is too purely negative, while "vitreous" is specific and at the same time non-committal. But I gather from Mr. McCance's communication that he objects not merely to the name but also to the explanation of the hard and soft states with which

* *Nature*, November 21, 1912, p. 330.

† See *Jour. Inst. of Metals*, vol. viii. (1912), p. 113.

it is associated. In the argument which has been developed in the Paper which will be printed with this discussion I have tried to make a clear distinction between the facts and the inferences from these facts on which the amorphous theory is founded. From this it will be obvious that the keystone of the theory is *the instantaneous liquefaction and solidification of sheets of molecules during mechanical deformation*. This assumption is based on a very strong body of circumstantial evidence, and if it is true in the cases which constitute this body of evidence, I think it must be admitted that it is a truth of general application. There is evidence that in particular cases small displacements of orientation in lamellæ or grains may be restored to their original state without any disturbance of the original crystalline arrangement. If our observations had to be limited to cases of this description, it is to be feared that our knowledge of the subject would be very much more imperfect than it is to-day. But the more obvious and well-marked cases are fortunately at our disposal, and it is a strength rather than a weakness in our position that we should develop these fully before dealing with the less pronounced cases.

Mr. McCance believes that the magnetic phenomena in the two states tell against the existence of a really non-crystalline state. This matter has already been referred to in the Paper above. He also cites the supposed cases of hardening by a hydrostatic pressure of 10,000 kgs. per cm². I have not seen Hanriot's paper, but shall make a point of doing so. In the absence of details of experimental methods and data, it would be unfair to criticize his results, but as these are presented in Mr. McCance's Paper they do not satisfy me that they prove that permanent deformation can result from uniform compression in all directions. It is quite conceivable that internal movements due to differential pressures within M. Hanriot's cubes might occur even under externally applied hydrostatic pressure. The paper by Messrs. Johnston and Adams in the *Journal of the American Chemical Society* in May 1912 contains much interesting information on changes of density in solids under high pressures.

Sir Robert Hadfield, who had continually interpolated valuable and illuminating comments during the discussion, in his reply drew special attention to the Paper by Professor Howe.

The more one investigates, said Sir Robert, the more it is apparent that there is some extraordinary change in the combination of the carbon with iron, and it was for this reason that he had offered his prize for a research. This had been offered both in this country and in America because, in his opinion, we in this country on the scientific side, and the Americans on the practical side, had done more than any other countries in the world to elucidate the difficulties surrounding the problem. Between us we ought to find some means of more clearly understanding what it is that takes place in the hardening of carbon steel. With all due deference to the Papers placed before the meeting, he continued, he could not say there was one that threw light on what really happened when a piece of carbon steel was changed from a soft to a hard condition. All these investigators were doing good work, but they did not seem to bring us any nearer the solution of what was taking place. The point to be studied was to try and find out what is the combination that takes place between the iron and the carbon which brings about the intense hardness when a piece of carbon steel is quenched, and it was in this connection that he regarded as of special importance the "balance of probabilities" in Professor Howe's Paper.

Dr. J. E. Stead had expressed the opinion that we have not sufficient data upon which to found any definite conclusion as to the process known as the hardening of steel by quenching. It was most desirable, therefore, that more

attention should be given to the study of the carbon condition, for there could be but little doubt that on it depends mainly the extreme hardness of steel.

With this view Sir Robert Hadfield fully agreed. There was, he said, one thing certain, viz., that carbon was mainly responsible. Some investigators seemed to get away from this important fact, but there was undoubtedly a difference in the chemical constitution, and the difficulty was to determine that difference. Osmond's theory he regarded as only tentative, and the theory of internal stress, an attempt to explain the hardening of steel in a new direction, put forward in Mr. McCance's Paper, might be the correct one; but it was well to remember that, no matter how much carbon steel might be stressed or disturbed in the cold, it could never be made glass hard. We had not, he concluded, sufficient data upon which to base dogmatic conclusions and hypotheses, and results of careful observations must hold until they had been proved untenable.

Referring to Mr. Heathcote's demonstration of instruments for the determination of hardness, Sir Robert Hadfield, as an instance of the lack of information on these matters, said that a short time ago he wished to obtain some particulars of the hardness of quartz and diamond compared with the hardness of steel, but he could not find the slightest information. This only served to show how ignorant we all were. He made some tests of his own, and found by the Shore scleroscope a rebound test of not less than 120, whereas the highest hardness obtained with steel up to the present was between 80 and 90. The 120 was equal to about 700 Brinell hardness number. The current idea conveyed in some of the Papers seemed to be that a hardened metal was in a state of strain, but he could not agree with that for a practical reason. A projectile would go through a plate of calibre thickness unbroken. It was impossible to conceive this happening if the metal was in a strained condition. This related to capped shot, but the cap was dispersed so quickly that the point of the shot had still to take an enormous crushing stress, and it was in an exceedingly hard condition, probably over 700 Brinell hardness number. If the point was in a strained condition, surely the projectile would go to pieces, but it did not do so.

Dr. C. A. Edwards wrote that he wished briefly to refer to a few points that had been raised in the general discussion.

Dr. Stead cited two instances which he considered were incompatible with the theory advanced by Professor Carpenter and the writer. The first was the case of the meteorite which contained Neumann lines or twin crystals, but which did not become "sensibly softer" when these twin crystals were removed by annealing at 800° C. In the absence of more complete data it is not an easy matter to discuss this question, but it would seem from Dr. Stead's remarks that this treatment does bring about a certain amount of softening. Heating to 800° C. to remove the Neumann lines may also cause some of the free phosphorus to go into solution, and this would harden the mass, and thus work in the opposite direction to the softening brought about by removing the twin crystals.

In connection with the above and other criticisms of a similar nature, it seems necessary to reiterate that Professor Carpenter and the writer do not regard the great hardness which is produced by quenching steels as being entirely caused by crystal twinning. The chemical composition is admitted to play a most important rôle in this respect.

The second case mentioned by Dr. Stead was the hardened nickel steel which becomes soft, although the martensitic markings remain, after annealing at 500° C. The writer's explanation of this is, that at 500° C. the greater part

of the amorphous material becomes recrystallized, but the structure as seen under the microscope is not thereby materially influenced. This is very similar to what Goerens finds in the case of cold-worked steels, who states that the effect of cold working disappears at 520°C .

The fact that a 0.9 per cent. carbon steel possesses approximately the same degree of hardness when quenched from 750° and 900°C . is exactly what would be expected from the theory of hardening which is based upon the chemical composition and crystal twinning. According to this theory the degree of twinning is proportional to the amount of the carbide change which is suppressed by quenching. Hence it is practically independent of the quenching temperature: the twinning will be approximately the same whether the steel is quenched from 750° or 900°C .

The more obvious acicular markings which are evident after quenching from high temperatures do not indicate a greater degree of twinning.

Dr. W. Rosenhain, F.R.S., in a communication dated February 15, 1915, wrote that absence from England had unfortunately prevented him from taking the part which he would have wished in the discussion on the hardening of metals. In the present written contribution to the discussion, made at the eleventh hour at the request of the President and Secretary, he felt that it would be difficult to deal adequately with the numerous questions which had been raised.

It was interesting to notice that three of those who contributed papers, viz. Dr. Beilby, Mr. Humfrey, and Professor Edwards, were agreed that the hardness of quenched steel, and indeed of all metals and alloys hardened by quenching or by cold working, was due to the presence of metal in the amorphous condition. Professor Howe appeared yet to hesitate between the "beta-iron" theory and the "amorphous" theory; while apparently Mr. McCance preferred a newly-coined word, "interstrain," to any theory at all. In the verbal discussion Dr. Hatfield associated himself with the "amorphous" view. It seemed, therefore, that there was a considerable tendency to accept that view, but there were somewhat wide divergences in regard to the mode of origin or formation of the amorphous metal present in quench-hardened materials. The writer's own view at present leaned very decidedly towards the "amorphous" theory of hardening—a theory which he had had the privilege of discussing with the late M. Osmond some six or seven years ago, but he (the writer) held a somewhat different view as to the formation of the amorphous metal in a quenched material. He would, therefore, briefly discuss this question.

It would be convenient to consider first the ideas put forward by Professor Edwards, which ascribed the formation of the hard amorphous metal in quenched steel and other alloys to a highly-multiplied process of twinning supposed to take place during rapid cooling as the result of contraction stresses. The first step required to secure the acceptance of such a view was to establish the simple fact that hardened steel really did consist of intensely multiplied twinned lamellæ. Such proof had not been given, and, indeed, microscopic examination of a very large number of samples of martensite had convinced the writer that there was in that structure nothing whatever to suggest twinning. The characteristic features of twin lamellæ were a certain regularity of outline—rectilinear boundaries and repetitions of similar orientation. In martensite there were leaf-like bodies, with typically curved outlines, interlacing but without showing the least sign of repetition of orientation. In fact, it was very difficult to obtain any direct evidence of the truly crystalline character of martensite at all. It was, therefore, an assumption that martensite consisted of twinned lamellæ,

and such an assumption could only be justified if it led, without further assumptions, to a simple explanation of known facts. The second step in the theory of Edwards and Carpenter, however, required an assumption which was equally without proof, and which was in itself entirely contrary to the principles of crystallography, and also to certain observed experimental facts. This was the assumption that layers of amorphous metal were formed at the boundaries of twinned lamellæ of the same crystal. The authors of the theory apparently based this assumption on what was really a false analogy with what occurred—according to the views first put forward by Beilby—on planes of slip. On those planes there was relative *translational* movement (actual slip) between two adjacent parts of a crystal, and a profound disturbance of the arrangement of the atoms or molecules resulted. Edwards and Carpenter extended this conclusion to surfaces on which twinning took place, being apparently misled by the idea that *any* kind of internal movement must disorganize a crystal. But this is by no means necessarily the case. In slip there is relatively large linear displacement between adjacent layers of molecules or atoms, but in twinning there is nothing of the kind; the crystal elements merely rotate into a position of alternative symmetry, and there will be, as a rule, no relative displacement of the centres of gravity of adjacent molecules. Thus the process of twinning need put no strain on the internal arrangement of the crystal, particularly as the final arrangement, when twinning is completed, results in a position of equilibrium almost, if not quite, as stable as that of the un-twinned crystal. One consequence of this state of affairs is the slowness with which twinning disappears on annealing; were amorphous metal present in the boundaries of twin lamellæ, the energy stored in these amorphous layers would tend to cause coalescence of the twinned portions at a rate quite comparable with that at which crystals elongated by strain tend to coalesce. We have here one line of experimental evidence which serves strongly to support the view that twinning does not involve the formation of amorphous layers in the twin boundaries. A further line of evidence in this direction is obtained from the observations of the present writer and D. Ewen,* which showed a marked difference between the behaviour of the boundaries of twin lamellæ and the ordinary crystal boundaries when the metal is heated *in vacuo*. The deep grooves formed along the ordinary crystal boundaries, and ascribed to a consequence of the volatilization of the amorphous inter-crystalline layer present in those boundaries, were strikingly absent from all twin boundaries. Here again is evidence to show that there is no formation of amorphous layers at the surfaces of twinning. It thus appears that the twinning theory of martensite, as advanced by Edwards and Carpenter, rests upon two separate assumptions, both of which appear to be not only without any direct foundation on experimental fact, but to be directly opposed to such experimental evidence as is available. The theory may, therefore, be put on one side until its authors can bring forward direct experimental evidence in support of its fundamental assumptions.

The writer's own view of the origin of amorphous metal in material hardened by quenching has recently been expressed by him as follows†: "The gamma iron solid solution on reaching the temperature of A_{r_3} begins to undergo decomposition by the formation of a large number of minute crystals of alpha iron. These minute crystals will be formed on the planes of slip and of cleavage of the gamma crystals, as some little movement must take

* Rosenhain and Ewen, "Intercrystalline Cohesion in Metals," *Journ. Inst. Metals*, 1912, ii., p. 169.

† Rosenhain, *Introduction to the Study of Physical Metallurgy* (Constable), p. 182.

place at these points, thus causing disturbance, which is favourable to transformation. Each of these extremely numerous but very minute alpha crystals will be surrounded by a film of amorphous metal which is, at any moment, in the act of passing to the growing alpha crystal and is continuously renewed from the mass of the surrounding gamma crystals. By rapid cooling, however, the growth of these alpha crystals is stopped at a very early stage, and the quenched steel is arrested in a condition in which it consists of numberless minute alpha crystals surrounded by layers of hard and strong amorphous metal, and possibly embedded in some unchanged gamma iron. The hardness of the quenched steel is then ascribable to the presence of an extremely minute network of amorphous layers. The amorphous material of these layers will not only possess the hardness of amorphous iron, but will be rendered still harder by the presence of carbide in a high state of concentration. The minute alpha crystals as they are formed must reject the carbide which was present in solution in their gamma-iron mother crystals, and this carbide will be rejected into the surrounding amorphous film."

While this view cannot and does not claim to be more than a useful working hypothesis, it has the merit of being based upon assumptions which have the support of considerable experimental evidence. The first of these fundamental assumptions is simply that every crystal—and particularly every growing crystal—is surrounded by an amorphous layer. For this assumption considerable evidence has been adduced by the work of the writer in conjunction with Humfrey and Ewen,* and although there are admittedly some who do not yet accept the existence of an inter-crystalline amorphous cement as definitely proved, there is at all events a *prima-facie* case for its existence. But once this assumption is granted, the rest of the theory follows as a matter of simple deduction. The hardness of the amorphous layer is admitted by the majority of those who have taken part in the present discussion, while the additional hardness due to the rejection of carbide from the growing alpha crystals can hardly be doubted. In this way the theory gives due weight to the profound effect of carbon on the hardening of steel. It is, in fact, the time occupied by this rejection of carbon that renders possible the partial suppression of the transformations by quenching.

The manner in which the theory here outlined fits most of the facts connected with hardened steel may be very briefly indicated. We have, first, the fact that if the transformation were completely suppressed, the source of hardness invoked by the theory would become inoperative. Where severe quenching, plus the restraining influence of high carbon content, is sufficient to suppress the transformation entirely at certain points, we have the appearance of austenite. With the aid of nickel or manganese the suppression can be rendered more complete, and we have austenitic steels which, while "hard" in one sense, are not hard in the sense of hardened carbon steel. Yet the austenite of these steels is strongly inclined to undergo twinning, and if extensive and minute twinning and consequent hardening resulted from contraction stresses, this type of steel should be strongly hardened. On the writer's views its relative softness is at once explained. Next, the magnetic properties of hardened carbon steel are readily explained by this theory. The ferro-magnetism resides in the numerous minute alpha crystals formed at the transformation temperature; it is probably right to suppose that the amorphous iron is more or less completely non-magnetic, but its presence in a network minutely interlacing the small alpha crystals serves to affect the

* Rosenhain, "Metals, Amorphous and Crystalline," *Engineering*, October 13, 1913. Rosenhain and Ewen, *Journ. Inst. Metals*, 1912, ii., 1913, ii. Rosenhain and Humfrey, *Journ. Iron and Steel Inst.*, 1913, i.

magnetic properties of the latter by imposing constraints upon them, in such a way as to lessen permeability and to increase retentivity. As some misconception appears to exist on this point, it may be here emphasized that the assumption that amorphous iron is practically non-magnetic is entirely consistent with the magnetic properties of cold-worked iron. Strain-hardened iron still consists very largely of crystalline alpha iron, embedded in a relatively wide network of amorphous iron. The total quantity of amorphous iron formed in the case of specimens of moderate size is probably quite small. Beilby has shown that even when extremely severely treated, the conversion of the crystalline into the amorphous phase is never even approximately completed. It cannot, therefore, be argued, as Mr. McCance appears to do, that the magnetic properties of strain-hardened iron are in conflict with the "amorphous" theory.

The form of the "amorphous" theory of hardening indicated by the author above also offers good explanations for the phenomena met with in the tempering of hardened steel. This becomes a matter of two processes which must take place simultaneously, viz. the growth of the alpha crystals and the rejection of increasing quantities of carbide from the amorphous layers as these diminish in quantity concomitantly with the growth of the alpha crystals. The former process is principally responsible for the diminishing hardness and increasing toughness of the tempered steel as the reheating temperature rises, while the latter accounts for the formation of that somewhat puzzling transition product known as troostite, which offers such striking evidences of the presence of carbide in a state of extremely fine division—the "colloidal suspension" of Benedicks.

In this connection it is interesting to draw attention to a very striking analogy in regard to "tempering" which exists between steel hardened by quenching and iron or mild steel hardened by strain. Muir* has shown that iron hardened by strain can be "let down" in stages, according to the temperature of reheating, which exhibit a striking resemblance to the stages of tempering of quenched steels. Where the analogy both in regard to hardening itself and then in relation to tempering is so complete, it is surely the most logical attitude to seek for a common cause at work in both cases. Such a common cause is offered by the "amorphous" theory.

Some reference is, perhaps, required to the "theory of beta iron." In the first place, while he himself inclines to the "amorphous" theory as outlined above, the writer would point out that the much-vaunted "burial" of beta iron was entirely premature. A vigorous attempt has been made to "talk down" the whole idea of beta iron, and in the papers here under discussion at least one writer simply states his own views on the subject—which are shared by only a few investigators—as if they were generally admitted. As against this, the work of Burgess and Crowe has established the heat-evolution at A_2 more firmly than ever, the causes of error which led Carpenter to deny the existence of A_2 in pure iron having been demonstrated. The explanation of these thermal phenomena as due to a "sudden change of specific heat" appears to the writer as entirely unsatisfactory. Even if specifically correct, it yet remains to prove to what discontinuous change in the iron this "sudden change of specific heat" is really due. The magneton theory offers an explanation, but that theory itself has yet to be established. On the other hand, it has recently been shown by Ruer† that a magnetic transition-point in at least one case is accompanied by thermal phenomena of such magnitude

* Muir, "On the Recovery of Iron from Overstrain," *Phil. Trans.*, A, 1899.

† Ruer and Kancko, *Physikalische Zeitschrift*, 1914.

that the explanation offered by the magneton theory is obviously quite inapplicable and that an "allotropic" change must be invoked.

On the other hand, it may be well to point out that the "amorphous" and the "beta iron" theory of the hardening of steel by quenching are not so much opposed to one another as might at first sight appear. In both theories it is assumed that when gamma iron decomposes at the critical temperature, before passing into the stable form of alpha iron it passes temporarily through a state in which it is neither gamma nor alpha iron, but an entirely different form. According to Mr. Humfrey's views, this transition takes place *en masse*, while in the writer's opinion it occurs only in minute layers at the surfaces of the newly-formed alpha crystals; this is a difference as to the exact mechanism, but not as to the fact itself. The amorphous theory regards this transition form as "amorphous," while the "beta-iron theory" identifies it with that form of iron stable between the temperatures of Ar_2 and Ar_3 in pure iron. It has hitherto been supposed, and there is some reason for so doing, that stable beta iron in pure iron and low-carbon steels between Ar_3 and Ar_2 is crystalline, but actual proof of this fact is not really available. On the other hand, according to Mr. Humfrey's views the metal in the transition state between gamma and alpha might be both amorphous and in the beta state at the same time. While the writer cannot see his way to accept the latter view, he quotes it in order to show that there is no vital antagonism between the two theories. He prefers, for the moment, to adopt the simple "amorphous" view because as yet there is no direct evidence to identify the transition stage in question with the stable beta phase. The evidence from the mechanical hardness of the beta phase at high temperatures is admittedly not as definite as would be required to furnish a satisfactory basis for a permanently-tenable "beta-iron theory." The position of the matter is such, however, that it is entirely premature to set the beta theory aside as definitely disproved. Professor Howe's attitude very clearly illustrates this position.

Dr. Desch refers to the recent work of Bragg on the atomic structure of crystals of copper. If the views of Bragg are correct—and it is difficult to evade the bearing of his experimental evidence—then it will become necessary to adopt entirely new conceptions as to the nature of allotropy. If the crystal is really built up, as Bragg sketches it, out of the single atoms, both of elements and also in the case of crystalline compounds, then the conception of a "molecule" existing in the solid state becomes meaningless. We have learnt to distinguish between the allotropes of sulphur, with their differences of crystalline form as " S_6 ," " S_8 ," etc., but if the crystals of all the allotropes of sulphur should really be built up of the single S atoms, then we can no longer account for the different crystalline forms by supposing a different molecular weight; we are, in fact, driven to think of allotropy as a change in the atoms themselves, and not merely in their grouping to form molecules of different sizes. If this is admitted, it is quite permissible to adopt the view put forward by Mr. Humfrey, that the amorphous metal may also exhibit allotropic changes—that, in fact, amorphous alpha iron, consisting of atoms of alpha iron, will be different from amorphous gamma iron. This is a complication of our conception of the amorphous phase which the writer is reluctant to accept unless the logic of experimental facts requires it. It is certainly a matter demanding experimental study, although the exact method of approaching the problem is not by any means obvious.

Summing up the opinions which the writer has desired to express and, as far as may be, to substantiate in his somewhat lengthy contribution, he would say that the whole question of a satisfactory and complete theoretical

FIG. 1

FIG. 2

FIG. 1.—Quadrant Sclerometer for Testing Hard Bars.

FIG. 1A.—Quadrant Sclerometer for Testing Soft Bars.

FIG. 2.

explanation of the hardening of steel by quenching has yet to be finally solved. For the moment he regards the "amorphous theory" as offering the best working hypothesis, although the final form even of that hypothesis has yet to be stated. The "twinning" theory of Edwards and Carpenter he feels obliged to reject as contrary to known facts, while he does not regard the introduction of the term "interstrain" as throwing any new light whatever on the subject.

Mr. F. C. Thompson (Sheffield, *communicated* April, 1915): Any theory which has for its object the explanation of the remarkable properties of quenched steels must be of the greatest interest to metallographers. The one recently advanced by Professors Carpenter and Edwards is certainly not the least interesting of these.

There are, however, many difficulties in the way of accepting this idea, not the least of which appears to be the fact that repeatedly twinned austenite is frequently seen whilst remaining indubitably austenite, and, for instance, non-magnetic.

Fig. 1 (facing p. 290) is a micrograph of an austenitic nickel steel, containing 33 per cent. of nickel and 0.1 per cent. of carbon. As a result of rolling at a red heat repeated twinning occurred in practically every crystal, as is readily seen despite the rather severe etching. A similar structure is often seen in the Hadfield manganese steels rendered austenitic by water toughening (Fig. 2).

Osmond and many others have published similar photographs, and twinning is so frequently present that it has been characterized as one of the chief crystalline features of alloys with γ -iron.

Twinning in austenite follows exactly the same lines as in any other solid solution, arising in practically all cases from plastic deformation, and rendered visible by crystalline growth at high temperatures. The austenitic steels as cast rarely if ever show the structure. This necessity for plastic deformation is probably one of the difficulties which has prevented the full realization of the frequency of the structure in true austenite. As one of the authors himself showed, if the work be put upon the alloy in the cold, especially if the carbon content be high, the solid solution undergoes a certain decomposition, and martensite results.

The difficulties of explaining the magnetic properties of quenched steels on this theory are great. Benedicks has shown, in a most beautiful manner, that austenite is non-magnetic, and hence the amorphous layers formed during slipping, which should approximate most closely to this condition, may be reasonably supposed to be non-magnetic also. The hypothesis advanced by McCance, that in martensite work hardened α -iron is produced as a result of the partial resolution of the solid solution, affords almost for the first time some explanation of the magnetic properties of such steels.

DEMONSTRATION BY MR. H. L. HEATHCOTE.

At the conclusion of the meeting, **Mr. H. L. Heathcote, M.Sc.** (Coventry), showed some novel devices for testing the Hardness of Materials.

He said that the most commonly used commercial method was still the old test with a smooth file. With the object of eliminating the personal element he had devised an instrument (Fig. 1) which consisted essentially of two files and a graduated arc. The bottom file was attached to the base and could be adjusted until horizontal by the three levelling

screws attached thereto. The upper file was held down at one end by an inverted knife edge, the other end being free to move in a vertical arc. When a round article was placed in the angle between the two files and the upper arm allowed to fall slowly, the movement ceased when the forces due to friction between the files and the article were equal to the opposing forces tending to push the article out. The theory of the instrument showed that the angle at which equilibrium occurred was independent of the diameter and weight of the article, provided the coefficient of friction was the same. Since the friction depends on the amount of interlocking between the article and the projections of the file, and the interlocking on the hardness, the instrument serves to measure hardness—or more precisely it affords a measure of such properties of the metal as are, and in so far as they are, concerned in resisting the succession of changes resulting in abrasion by the files. Forms of the instrument adapted for testing heavier and non-portable articles were exhibited. In each case the principle was the same. Specially cut and hardened files were employed. Such instruments were in use for testing hardened motor and other parts, the method adopted being to set up angular limits corresponding to the various degrees of hardness required, e.g.—

Crank- and gudgeon-pins (maximum hardness) ...	...	15–20°
Twist drills, etc. ...	...	30–35°
Not hardened steel ...	...	50–70°

Another simple device was a modified automatic centre-punch in which the hardened point was replaced by a ball-holder (Fig. 2). Inside is a spring which is carefully calibrated to release the hammer at a standard pressure

Brinell Hardness No. (3,000 kg.).	Diam. (3,000 kg.).	Diam. by Auto-punch with 3-16th-inch Ball.
121	5·40	1·93 mm.
163	4·70	1·80 "
170	4·60	1·73 "
286	3·60	1·63 "
578	1·65	1·47 "

when held normally to the surface to be tested and pressed down sharply. The result is a depression similar to, but smaller than, that produced by a Brinell Hardness testing machine and is measured in the same way. The relation between the readings is indicated in the table above. Since the Brinell test applies unequal and the auto-punch equal doses of energy, and further, since the degree of mechanical hardening during the two tests may be very different, it does not follow that they should give parallel readings. The advantages of such an instrument are pretty obvious ; it is portable, can be used for testing thin sheets, bars, and parts of machinery *in situ* ; also for brass, bronze, delta metal, aluminium, wood, etc. New balls can be used as often as desired, as they are inexpensive and very accurately made. A solid foundation under the test piece is essential for comparative measurements. The readings indicate hardness—or more precisely they depend on and are some measure of the properties that are concerned in resisting penetration under the conditions of the test.

Such instruments, though very useful, do not supply the need (in the commercial testing of hardness) for some method of testing every part of the hardened surface. To achieve this recourse was had to baths of reagents capable of doing for hardened steel articles what the photographic developer will do for the exposed plate, viz. develop latent chemical differences.

A mixture of alcohol, nitric acid and water has been found very suitable. Clean articles immersed in such a liquid for 10 to 30 seconds are coloured a brownish-blue where the surface is martensite, and nearly black where there is troostite (due to splashing before quenching or to softening by grinding), and not appreciably changed where the surface consists of ferrite or pearlite. This test is admirably adapted for the testing of large quantities of articles, e.g. after being ground to the finished size : darker or lighter patches are readily detected and such occasional specimens can be checked with a file or sclerometer.

These methods of testing had been evolved to meet the requirements of a special industry, but they had a much wider scope in other industries, and the author hoped that they might be found useful to those who, like the previous speakers, were engaged in unravelling the intricate subject of the hardening of metals.