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unannealed piano-steel rise of temperature caused a very appreciable temporary *twist*, instead of untwist, even with small loads on.

A remarkable feature about all these experiments was the great difference with different metals in the facility with which the effect of permanent torsion in one direction could be reversed by permanent torsion in the opposite direction. With copper, for example, one complete revolution in the opposite direction was sufficient to reverse the effect of 200 complete turns of permanent torsion, whilst with iron the difficulty of reversing the effect of previous torsion was considerable.

I am inclined to believe that the phenomenon observed by Mr. Bosanquet is to be, at any rate partly, attributed to unequal expansion in different directions. I have always found in wires which have been hard-drawn a certain amount, and sometimes a considerable amount of permanent torsion: this, we have seen, will cause temporary twist or untwist to be produced by rise of temperature. The amount of twist or untwist in any case, however, observed by myself was very much less than that observed by Mr. Bosanquet; and it would be of interest to ascertain how far the comparatively very large variation of torsion with small rise of temperature which occurred with the platinum wire used by him, is to be attributed to the comparatively great longitudinal stress on the wire*.

XXXII. *Remarkable Effect on raising Iron when under Temporary Stress or Permanent Strain to a Bright-red Heat.*

By HERBERT TOMLINSON, B.A.†

IT has been shown in the preceding paper that an annealed iron wire which has been permanently twisted is temporarily untwisted when the temperature is raised to 100° C., provided there is not too great a load on the end of the wire; but that if the load on the end of the wire is sufficiently great, a temporary twist follows on the rise of temperature. Fresh experiments were therefore entered on with the view of ascertaining whether, with a small load on the end of the wire, the temporary untwist produced by rise of temperature would be changed to twist when the temperature exceeded a certain limit. A few preliminary trials were made by merely heating a portion of the permanently twisted wire with a burner; and it was found that when the wire reached a bright red heat a most remarkable and sudden change occurred, the wire sharply

* See above, the effect of increasing the load on the wire.

† Communicated by the Physical Society: read June 25, 1887.

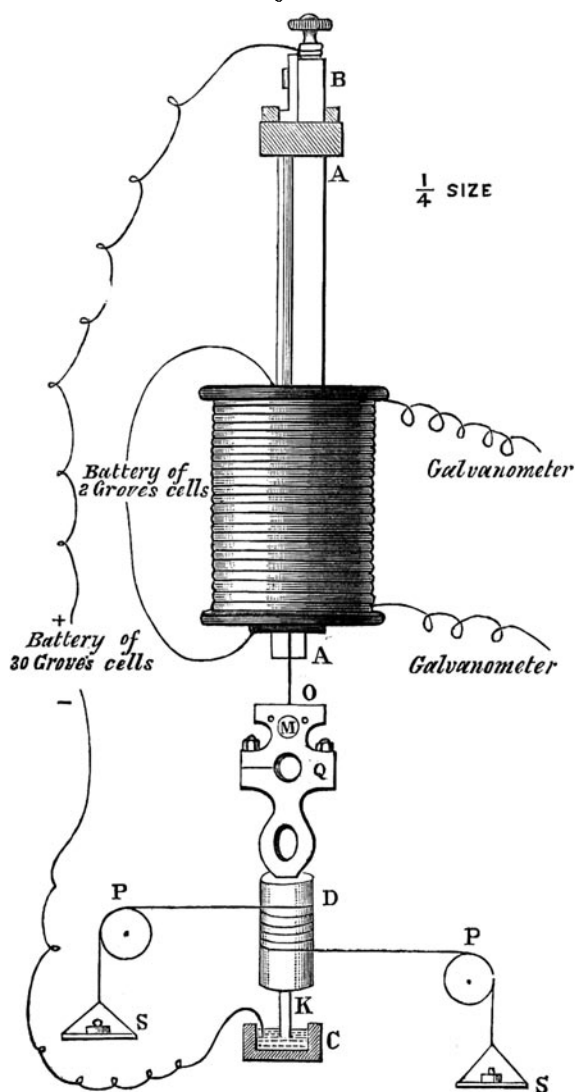
twisting in the same direction as that in which it had previously suffered permanent torsion. When the burner was removed and the wire was cooled to a temperature which seemed slightly lower than that at which the phenomenon occurred in the first instance, it as suddenly untwisted; and when it had again attained the temperature of the room, there remained a decided *permanent twist in addition to that which had been originally imparted**. This additional permanent twist is rendered more remarkable by the fact that rise of temperature beyond a dull red, but not extending to a bright red, is attended with a considerable *permanent untwist*. The phenomenon is evidently closely associated with one discovered by Mr. Gore so far back as 1869, and described in the 'Proceedings of the Royal Society' for that year. Mr. Gore's experiments were conducted in the following manner:—A thin iron wire, fixed at one end to a binding-screw, is attached at the other to an index which multiplies any motion of the wire; the wire is stretched horizontally by a feeble spring, and is heated by an electric current or by a row of gas-jets. According to Mr. Gore, no anomalous action is observed on *heating* the wire to bright incandescence; but when cooling begins, the index moves back until a moderate red heat is attained, when suddenly the pointer gives a jerk, indicating a momentary *elongation* of the wire during the progress of its contraction. In 1873 Professor Barrett extended Mr. Gore's researches; and in a paper full of interest†, entitled "Certain remarkable Molecular Changes occurring in Iron Wire at a low red Heat," showed that, under suitable conditions, the sudden change could be observed *not only on cooling, but on heating*. It is apparent, from the description given above, that the phenomenon observed by Mr. Gore resembles the one observed by myself, in that there is a sudden change in the iron when it has been raised to a bright red and afterwards cooled to a certain temperature. But in Mr. Gore's experiment the wire was under temporary *stress*, whilst in mine it was permanently *strained*; and whereas in the one case, on cooling below a certain temperature, there was a sudden *yielding to the stress*, in the other there was a sudden *diminution of permanent strain*. A careful consideration of the results of my own experiments, and of those of Mr. Gore and Professor Barrett, satisfied me that in this, as in many other instances, temporary stress and permanent strain act in oppo-

* This is not so if the wire be heated rather slowly; but, on the contrary, in this case there is a very decided *permanent untwist* (see experiment V.).

† Phil. Mag. ser. 4, vol. xlv. p. 472.

site directions as regards their effects on the physical properties of matter. I was moreover led to test, not only the effects of torsional stress and strain, but also those of other mechanical stresses and strains, for the most part with arrangements which will now be described.

Fig. 1.



The wire was suspended vertically in the axis of a glass tube, A (fig. 1), being clamped at its upper extremity into a

brass block, B, resting on a wooden support fitting on to the top of the tube, and provided with a terminal for making connexion with one pole of a battery of thirty Grove's cells, arranged ten in series and three in parallel arc. The current from the battery passed through a set of resistance-coils arranged so that the resistance could be altered by small amounts at a time; the current was also conducted through an amperemeter and through the wire, passing in or out of the latter through the intermediation of a mercury-cup, C. The lower extremity of the wire was clamped into a second brass block, O, to which was secured a mirror, M, reflecting the light of a lamp on to a scale placed at a distance of one metre. This block was provided with a circular aperture, Q, into which, if necessary, a bar could be introduced, and at its lower extremity terminated in a brass cylinder, D, having a piece of rather stout brass wire, K, projecting from its centre, vertically downwards, and dipping into the mercury-cup C. When it was required to subject the wire to torsional stress, two fine silk threads were wrapped in opposite directions round D, and passed, as in the figure, over two fixed pulleys, P, to two small cardboard scale-pans, S, on which weights could be placed. When the wire was not required to be torsionally stressed the fine silk threads and the pulleys were dispensed with.

Experiment I.—A piece of very soft and carefully annealed iron wire*, 30 centim. long and 1 millim. in diameter, was subjected to torsional stress in the manner described above. The pans S each weighed 10 grms., and in each of them was placed a load of 20 grms., so that the torsional couple amounted to 30×1.6 in gramme-centimetre-units†. A current of gradually increased amount was passed through the wire, and when the temperature approached a bright red the wire began to twist rapidly and permanently under the influence of the stress‡. The current was shortly afterwards stopped; and when a temperature between bright red and dull red had been reached the wire began suddenly to twist further, the amount of the sudden twist being about 90 degrees. When the wire had cooled down to the temperature of the room there was left a considerable permanent twist. In this experiment there was no perceptible temporary untwist on heating, but merely a slight check in the rate at which the wire was permanently twisting as soon as the critical tempe-

* This wire was specially prepared for me by Messrs. Johnson and Nephew, and is capable of suffering a permanent elongation of 25 per cent. before breaking.

† The diameter of D was 1.6 centimetre.

‡ The *first* appearance, however, of sensible permanent twist occurred rather suddenly at a *dull red heat*.

perature was reached. Other experiments were made with smaller and smaller torsional stresses until only the cardboard scale-pans were left to produce torsion : even here, however, it was impossible to get any sign of temporary untwist on heating. This mode of producing torsion was accordingly abandoned, and the stress was applied in the manner described in Experiment II.

Experiment II.—A cork was fitted into the central aperture of the block O, and through the centre of the cork was passed a knitting-needle about 25 centim. in length and $1\frac{1}{2}$ millim. in diameter, which was magnetized rather feebly by rubbing it with a small bar-magnet. The knitting-needle, when first placed in position, was nearly in the magnetic meridian, but afterwards the block B was turned until the needle eventually was nearly at right angles to the plane of the magnetic meridian, so that the wire might be under a feeble torsional stress due to the action of the earth's horizontal magnetic force on the needle. Immediately the wire reached a bright red temperature there was a very perceptible sudden temporary untwist, and on cooling a sudden temporary twist at nearly the same temperature*. This experiment was repeated with another piece of the same iron wire ; but now the needle, instead of being twisted through 90 degrees from the north and south position, in which it originally lay when there was no torsion on the wire, was only twisted through about 10 degrees. Even the extremely feeble torsional stress now acting on the wire was not only sufficient to produce the phenomenon, but also a very decided *permanent* twist. The permanent twisting began directly the wire, on being heated, reached a *dull red heat*, and continued with increasing rapidity until the temporary untwist occurred at a bright red heat.

Experiment III.—The temperature at which, on cooling, the sudden change took place was evidently very much higher than that at which it occurred with the specimens of iron used by Mr. Gore and Professor Barrett; and this I felt inclined to attribute to the comparative softness of the iron used by myself. I accordingly tried several other specimens of iron and steel, both in the annealed and in the unannealed condition. With some of these the phenomenon did not occur on cooling until a dull red, or even at a still lower temperature ; but it was found essential to its production that the iron *should have been previously raised to the temperature of bright incan-*

* These effects can easily be shown by merely heating a small portion the wire with a Bunsen-burner.

*descent**. On heating, the untwisting always showed itself at the temperature of bright red.

In consequence of the sudden change on cooling not occurring till a dull red heat had been reached in his experiments, Professor Barrett connected the phenomenon in question with another of equal interest. It is well known that, at a temperature of dull red, iron begins very rapidly to lose its magnetic properties; and, according to Professor Barrett, the two phenomena occur simultaneously. It is no doubt the case that, on cooling, the sudden jerk occurs simultaneously with the sudden regaining of magnetic properties in *some* specimens† of iron or steel; but it by no means follows that we have not two very distinct critical temperatures—one at or about a dull red, at which iron loses or regains its magnetic properties according as the wire is being heated or cooled; and another at a much higher temperature, namely near a bright red, at which sudden changes, certainly not less profound, occur. This is shown in the next experiment, which at the same time illustrates the fact already mentioned, that the effect of permanent strain is opposite to that of temporary stress.

Experiment IV.—The glass tube (fig. 1) was placed inside a magnetizing solenoid‡, consisting of a single layer of cotton-covered copper wire $\frac{1}{20}$ inch diameter, and connected through a key with a battery of two Grove's cells. Surrounding the solenoid and concentric with it is a secondary coil, consisting of 840 turns of cotton-covered copper wire partly $\frac{1}{20}$ inch diameter and partly $\frac{1}{10}$ inch diameter. This coil has a resistance of about 1 ohm, and is connected through a key with a very delicate Thomson's reflecting-galvanometer of about 7 ohms' resistance. A piece of the same soft iron wire already mentioned was subjected to thirty complete revolutions of permanent torsion, and was finally released from all torsional stress. When the wire, on heating, reached the temperature of dull red, a momentary deflection of the needle of the Thomson's galvanometer took place, indicating a sudden loss of magnetic permeability; and as soon as a bright red heat had been attained it, as in the preliminary observations, *twisted* sharply and suddenly. The battery which was employed to heat the wire had its circuit now broken, and the

* This fact, which was also noticed by Mr. Gore and Prof. Barrett, is significant.

† The reason of this will be found in the remarks on the recalescence of iron.

‡ Only the ends of this solenoid can be seen in the figure, the rest being enveloped by the secondary coil.

wire was allowed to cool : first ensued a sudden momentary *untwist* of the wire, whilst *a few seconds afterwards* a kick of the galvanometer-needle in the opposite direction to that which had occurred on heating, indicated that the iron had suddenly regained its magnetic properties. This experiment was repeated several times ; and though the sudden jerk on heating and cooling became less and less in intensity, and probably would eventually have vanished, it did not do so after six heatings and coolings*.

Experiment V.—The last experiment had shown the great persistency of the permanent torsional strain, even after the wire had been several times heated to a white heat. A fresh series of trials was made with iron which had suffered more or less permanent torsion, for the purpose of examining this persistency more closely. In these trials it was attempted to reanneal the wire after the permanent torsion had been imparted, by passing the flame of a Bunsen-burner very slowly up and down it so as to heat all parts in turn to a white heat, the wire being in some cases entirely free from any load on it, and in others having merely the slight load due to the block and its appendages. In no case could the sudden jerk at a bright red heat be got rid of entirely ; though the same wire, when tested previously to imparting permanent torsion, had shown no trace whatever of the phenomenon. It is evident that iron cannot be satisfactorily annealed by the process mentioned above†.

These trials also brought out distinctly the fact that at a dull red heat the iron begins to *permanently* untwist rapidly, so that a considerable amount of the permanent twist originally imparted can be got rid of by maintaining the temperature for some time between bright red and dull red. Directly, however, the higher critical temperature is reached there is a sudden twist, which in the case of very soft iron is partly temporary and partly permanent, and in the case of steel wire which is unannealed, or hard iron wire, is principally permanent‡.

Experiment VI.—A piece of the well-annealed iron wire, about 16 inches long, was clamped at one end and sustained in a horizontal position, save in so far as it was bent by its own

* I had the pleasure of repeating the experiment before the Physical Society.

† I write this because some observers seem to think that iron can be satisfactorily annealed in this way. In this, I believe, they are mistaken ; the iron should be maintained at a high temperature for a considerable period, and afterwards be allowed to cool very slowly.

‡ With some unannealed specimens I found it difficult to detect any trace whatever of *untwisting* temporarily when the iron cooled below the critical temperature.

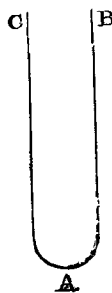
weight. The wire was heated by a burner about three inches from the clamp. When the temperature reached a bright red, the wire began to bend permanently very rapidly by its own weight; and when the burner was removed and the wire cooled, a *sudden further bending took place* at a temperature a little below bright red; and when cooled to the temperature of the room, the wire remained permanently bent. Here the attempt to unbend, which the wire no doubt made at the critical temperature when it was *heated*, was masked by the permanent bending. Another piece was therefore taken and heated at a point further away from the clamp, so that the bending-stress should not be so great. With care it was found possible to detect that the wire suddenly *straightened* itself when the critical temperature was reached on *heating*.

Similar trials were made with other specimens of iron and steel not so well annealed; and with these, when cooling, the sudden bending took place at a temperature lower than that at which it occurred with the very soft iron. With one specimen of wire, $1\frac{1}{2}$ millim. thick, it occurred at a temperature *apparently* below that of visible red*; but, as Professor Barrett justly remarks, the internal temperature of the thicker wires is no doubt masked by the cooling of the surface, whereas in thin wires the cooling throughout is extremely rapid. I found also that a piece of this specimen of iron *appeared* to lose its magnetic properties at a temperature below visible red, whereas with the thinner wires the *apparent* temperature at which this took place was somewhere about a dull red.

The very small amount of bending-stress which is required to bring out the phenomenon makes it a little difficult to detect with certainty the *opposite* effect of bending-strain; but it may be managed after a few trials in the following manner:—

Experiment VII.—A piece of the very soft iron wire was bent, as in fig. 2, with the portions AC, AB in a vertical position; the end C was secured to a clamp, and the bend A heated by a burner. If AB has been so arranged that its centre of mass is very nearly vertically above the part heated, there will be little bending-stress. In this experiment, on heating the bend A to a bright red, the end B jerked suddenly towards C, and when the burner was removed almost immediately jerked back again. If AB is arranged so that there is a little more bending-stress, it is curious to notice the struggle which sometimes ensues at a bright red heat as to

Fig. 2.



* Prof. Barrett also remarks that the phenomenon occurs on cooling at an *apparently* lower temperature with thick wires than thin ones.

which way the end B shall be jerked*. This experiment was repeated with other specimens of iron and steel and with similar results, except that on cooling, as with the torsional stress and strain, the phenomenon occurred at a lower temperature than with the very soft iron wire.

The question now arises, Is there any sudden *molecular* change at a bright red heat even where there is *no stress or strain affecting the wire*? Professor Barrett brings forward evidence in support of the view that there is, which I am afraid is not conclusive. He says:—"If, however, this molecular change be entirely due to alteration in cohesion, then the removal of the spring ought to cause the anomalous behaviour to disappear. But it does not. Without the spring, an iron wire can be seen by the naked eye to undergo a momentary contraction during heating, and a momentary and more palpable elongation during cooling†. Fixing one end of the wire, and bending the other extremity at right angles so that it may dip into a trough of mercury, and thus preserve contact with the battery, both actions can be seen." I have little doubt myself that the effects observed here were really due to the stress produced by the *weight of the wire itself*. So far as my own experiments go, I have not been able to detect any sudden change in the wire when sufficient care is taken to prevent the weight of the wire itself from producing an effect‡. Nevertheless I am inclined to believe, with Professor Barrett, that a sudden *molecular* change does occur at the critical temperature even when the wire is quite free from any mechanical stress or strain, though as yet experimental evidence is wanting to furnish sufficient evidence of such a change.

The Recalescence of Iron.

This curious phenomenon was, I believe, first discovered by Professor Barrett, and mention is made of it in his paper, already alluded to, in the following words:—"On September 12th I was examining the condition of the wire in a darkened room, when a new and unexpected change revealed itself. During the cooling of the wire it was found that just as it reached a very dull red heat, a sudden accession of temperature occurred, so that it glowed once more with a bright red

* That is, as to whether the effect of stress or that of strain shall predominate.

† The effect on cooling could always be got in my own experiments also with greater distinctness than the effect on heating.—H. T.

‡ It is difficult to realize, without actually putting the matter to the test of experiment, what a very small amount of stress or strain suffices to bring out the phenomenon.

heat. Illuminating the index and scale of the apparatus, which was watched by an assistant, it was at once found that the *reheating of the wire occurred simultaneously with the momentary elongation.*" Professor Barrett also ascertained that, in order to bring out the phenomenon, it was necessary *previously to heat the wire to a white heat*; and, further, that *wherever the momentary expansion of the wire is feeble or absent, there likewise this recalescence is also feeble or absent.*

In my own mind I have little doubt that this beautiful phenomenon may be accounted for in the following manner:—When the iron has been heated above a bright red and is then cooling, it reaches the critical temperature at which there is a *tendency* for the sudden change to take place in the permanently strained or temporarily stressed wire: the change does not, however, *actually* take place at this temperature in consequence of the so-called coercitive force of the iron. When the iron is very soft and well annealed, the temperature at which the jerk takes place is comparatively near the temperature at which the jerk occurred on heating. But when the iron is hard-drawn or only imperfectly annealed, there may be a considerable difference in the two temperatures. Suppose, then, that the iron has cooled to the temperature at which its magnetic properties are suddenly restored. At this temperature a commotion more or less profound takes place among the molecules; and this is sufficient to give them a start towards those positions which they have all along, after the higher critical temperature was passed, been trying to assume. When once started the molecular motion continues, the energy of position is rapidly converted into the energy of motion, and this again into the energy of heat, so that the wire once more glows. It by no means follows that the phenomenon of recalescence occurs with *every* specimen of iron at the critical temperature at which iron loses its magnetic properties; indeed I have noticed sometimes *more than one* evident sudden accession of heat* during the *same* cooling. Moreover, in the case of the very soft iron wire, with which the sudden jerk occurred at a much higher temperature on cooling than with the harder specimens, there was no perceptible trace of the phenomenon at a dull red heat. Probably there was a sudden check of the rate of cooling when the jerk occurred, though reglowing at this point was not noticed. I did not, however, pay much attention to the matter.

The view just advanced respecting the phenomenon of recalescence is, I think, justified by the following considerations:—

* Sometimes there is merely a sudden check in the rate of cooling without any sensible recalescence.

(1) The phenomenon is entirely absent in very well-annealed iron wire which has not, after annealing, suffered strain, nor is at the time under stress.

(2) It is not sensible unless the temperature of the wire has been *previously* raised to a bright red*.

(3) It becomes more and more sensible as the temperature at which the sudden jerk occurs in the wire becomes lower and lower, and is most pronounced in those wires in which the jerk does not take place until the wire suddenly loses its magnetic properties.

(4) It is not sensible unless there is a sudden jerk; the jerk and recalescence occur at the same instant.

Gore's phenomenon and the phenomenon of recalescence do not occur in pure, or nearly pure, specimens of nickel or cobalt; though, as is well known, nickel loses its magnetic properties at a much lower temperature than iron, namely from about 350° C. to 400° C. A most careful examination of three different specimens of nickel wire, procured from Messrs. Johnson and Matthey, was made†. Unfortunately pure nickel wire cannot be drawn; but two out of the three specimens showed no trace whatever of the phenomena. The third specimen showed very obvious signs of both phenomena; but when tested was found, like iron, to lose its magnetic properties at a dull red heat instead of at the lower temperature at which pure nickel loses its magnetic properties, and therefore probably contained rather a large quantity of iron. The other two specimens lost their magnetic properties at a temperature of about 400° C. Only one specimen of cobalt was examined. Cobalt has not been drawn into wire as yet, though both Messrs. Johnson and Matthey, and Mr. W. Wiggin, jun., have kindly attempted to do so for me. Mr. Wiggin was, however, so good as to have rolled for me a specimen of cobalt, which is very nearly pure, in a strip about $\frac{1}{2}$ millim. thick, 12 millim. broad, and 60 centim. long. This strip was tested with a Bunsen-burner and with a large blowpipe, and showed no trace at any temperature of either phenomenon when subjected to bending-stresses of various amounts. Unlike nickel and iron, cobalt does not lose its magnetic properties at any temperature at which it has been tested.

* This is not the case as regards the phenomenon of iron suddenly losing its magnetic properties at a dull red heat.

† Professor Barrett had previously shown the phenomenon to be absent in nickel.

The Working of Iron at High Temperatures.

There can be little doubt that such experiments as these are important from a technical point of view. It is known, I am not aware whether generally or not, that it is dangerous to work iron within certain ranges of temperature. This one can easily understand from what has gone before; for it is manifest that if the temperature of a mass of iron is not the same throughout, and if the *mean* temperature of the mass be near the higher of the two critical temperatures, or if it is cooling from a bright red heat near either of the two critical temperatures, the effect of a blow or of any stress will be very different on different parts of the mass, and will be fatal to that uniformity of structure and strength which it is so desirable to procure.

Summary.

(1) There are two distinctly marked critical temperatures for iron; the lower somewhere about dull red, and the higher somewhere about bright red*.

(2) At the former of these critical temperatures the iron begins suddenly to lose its magnetic properties if it is being heated, and to gain them if it is being cooled.

(3) At or *near*† the latter critical temperature the iron, on being heated, if under the slightest torsional, longitudinal, or flexural stress or strain, begins to exhibit a remarkably sudden change. If the metal be under stress, the sudden change resembles that which would result from a sudden increase of elasticity. If the metal be permanently strained, there is a sudden increase in the amount of the strain.

(4) When the iron is cooling, a sudden change, but opposite in direction to that which occurs on heating, takes place. The temperature at which the sudden change takes place is, for very well annealed iron, nearly the same as that at which the change takes place on heating. If, however, the wire be not annealed, the change may be delayed by the so-called coercitive force of the metal until the lower critical temperature is reached. When the change is so delayed, the phenomenon of recalescence occurs.

* I hope at some future time to be able to fix these critical temperatures more exactly.

† It is not unlikely that the so-called coercitive force may cause the change to take place at a slightly higher temperature than the critical temperature.