Rowland (H. A.) and C. T. Hutchinson. On the Electromagnetic Effect of Convection Currents. 8vo. [*London*] 1889. The Authors.

Rutley (F.) On Composite Spherulites in Obsidian, from Hot Springs near Little Lake, California. 8vo. [*London*] 1890. The Author.

Sang (E.) Exhibition of Curves produced by the Vibration of Straight Wires. 8vo. [*London*] 1889. The Author.

Slater (J. S.) Description of an Improved Armillary Sphere. 8vo. [*London*] 1890. The Author.

Tuckermann (A.) Index to the Literature of Thermodynamics. 8vo. *Washington* 1890. The Author.

March 12, 1891.

Sir WILLIAM THOMSON, D.C.L., LL.D., President, in the Chair.

The Right Hon. Lord Hannen, whose certificate had been suspended as required by the Statutes, was balloted for and elected a Fellow of the Society.

The Presents received were laid on the table, and thanks ordered for them.

The following Papers were read:—

I. "On the Plasticity of an Ice Crystal." By the late J. C. McConnel, M.A. Communicated by R. T. Glazebrook, F.R.S. Received January 24, 1891.

Two years ago, in the 'Proceedings of the Royal Society,' was published an account of some experiments on the plasticity of ice made by Mr. Kidd and myself. We proved the oft-repeated statement, that glacier ice is not plastic under tension, to be erroneous, and showed that any ordinary bar of ice composed of several crystals will yield continuously either to pressure or tension. But we found that a bar cut out of a single crystal with its length at right angles to the optic axis showed no signs of continuous stretching even under half the breaking tension, and other experiments convinced us that an ice crystal will not change its shape under either tension or pressure applied at right angles to its optic axis. These results seemed to render it highly probable that an ice crystal was not in any way plastic, and though after the winter was over we wished we had varied our experiments more, yet we quite expected that further investiga-

tion would only have corroborated the perfect "brittleness" of a single crystal.

Since our paper was written, my attention has been called to a passage in Professor James Thomson's masterly article on "The Lowering of the Melting Point of Ice by Distorting Stress" ('Phil. Trans.,' 1849), in which he expresses the opinion that crystals, whether of ice or other substances, are not plastic.

If we reject the idea of internal distortion of the crystals, we are driven to the conclusion that the observed plasticity must be due to some action at the interfaces, whereby the crystals alter their shape sufficiently to allow them to alter their relative positions. As to the nature of the action, various suggestions occurred to me. James Thomson explained the plasticity of ice at 0° C. by supposing the ice to melt at those interfaces where the stress was great, and the liberated water, after flowing to points where the stress was small, to again solidify. This might be extended to low temperatures by supposing a certain quantity of water to be kept in the liquid state by the pressure of residual impurities. But the process would be enormously retarded by the constant necessity for the distribution of salt being equalised by diffusion. Again, it is not clear how a bar of ice during this process would be able to resist a tension considerably greater than the pressure of the atmosphere. With more probability we may suppose one crystal to grow at the expense of another owing to the stresses and strains on the contiguous parts being different. Though the stresses were the same, the strains might be different, owing to æolotropic elasticity. But the elasticities are not likely to be very different in different directions, so for a very small extension of the bar we should expect considerable movement of the interfaces. There is, however, nothing to prevent the stresses being different. The tension in any direction parallel to the interface might be greater in one crystal than in the other. The migration of matter from one crystal to another under less stress would probably in almost all cases be accompanied by yielding to the external force producing the stresses. But in this case the effect would be very indirect, and again we might look for large movement of the interfaces.

Some such speculations had occupied my mind last autumn, and it was with considerable curiosity that I began experiments in December on the puzzling question of the real cause of the plasticity of ice. I took a bar of ice consisting of half a dozen crystals, made a drawing under the polariscope of the relative position of the interfaces, and then set up the bar with the ends supported and a weight hung from the middle. After two days, it had bent a good deal, yet, under the polariscope, I could detect no material change in the position of the interfaces. One crystal, however, had completely changed its appearance. It now strongly reminded me of a piece of unannealed

glass. There were two centres of colour encircled by irregular rings, and these remained much the same when the two faces through which the light passed were rubbed quite flat and the other crystals cut away. There could be no doubt that this crystal had suffered something more than mere elastic distortion.

The next experiment was very instructive. A thin slip of ice, being a single crystal, was subjected to bending stress as before, and left for several hours. It apparently bent very quickly, for after a few hours it was found crescent shaped, and luckily unbroken, lying at the bottom of the box. The optic axis was bent, and, though its change of direction was rapid where the bend was sharp, there appeared to be no break in continuity. On the other hand, the long narrow bubbles, which were originally no doubt parallel to each other and perpendicular to the slip, were still parallel to each other throughout. In fact, as I noted at the time, the crystal behaved as if it consisted of an infinite number of indefinitely thin sheets of paper, normal to the optic axis, attached to each other by some viscous substance which allowed one to slide over the next with great difficulty. This comparison proved to be the key to the whole question of the plasticity of a crystal of ice.

Further experiment showed that if a bar of ice consisting of a single crystal with the axis perpendicular to two of the side faces was subjected to bending stress, it would bend freely in the plane of the axis either at or below the freezing point, but not at all in a plane perpendicular to it. In the bent crystal the optic axis in any part was normal to the bent faces in that part. But any series of lines drawn in the substance of the ice which were originally parallel to the optic axis and to each other remained parallel to each other, though not, of course, to the optic axis. This was evidenced by the position of long narrow bubbles which frequently form at right angles to the planes of freezing, and also by the end faces of the bar remaining parallel to each other. When the optic axis was longitudinal, the bar bent indeed, but not very readily, and the general behaviour was more obscure. Still, this case, too, was in satisfactory agreement with the analogy mentioned above.

Let us state this analogy more fully. The sheets of paper offer no resistance to bending, but utterly refuse to stretch except, of course, elastically. Initially they are plane and perpendicular to the optic axis, and, after they have been deformed by bending, the optic axis at any point is still normal to the sheet at that point. They are of uniform thickness, whence it easily follows that the directions of the optic axis in any crystal form a series of straight, though not parallel, lines.

Detailed Account of the Experiments.

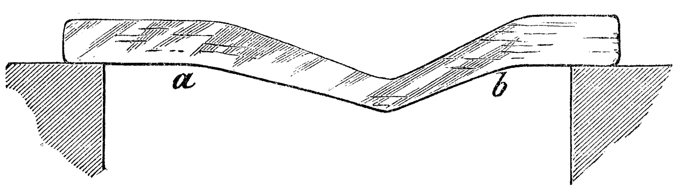
The first two experiments have been sufficiently described already.

The place of experiment was a north room in the Buol Hotel, Davos. A box without a lid was placed on a wooden table, and across the top of this box were laid two pieces of wood, which served to support the ends of the bar of ice. From the middle of the bar was suspended a weight with a loop of thick string. In the bottom of the box, but at the other end, *i.e.*, about a foot from the ice and 6 inches below it, was placed a registering thermometer of the Six pattern. Over the whole was put a thick wooden cover. As there was nothing inside the cover of great capacity for heat, I believe that any variation of the temperature of the ice was nearly simultaneously felt by the thermometer. This thermometer, which was used throughout, was divided into Fahrenheit degrees; its correction at freezing-point was tested both before and after the experiments. The error did not exceed $\frac{1}{4}°$ F. At 6° F. I compared it with a spirit thermometer which had been verified at Kew; it read $\frac{1}{2}°$ F. too high. These errors are negligible in the present work.

Exp. 3.—A bucket of water left in the ice room over night was found in the morning covered with ice about 15 mm. thick, consisting of several crystals. From this I sawed out a bar and planed it smooth and straight. The breadth was 10 mm.; the depth, 9 mm.

The bar contained many long bubbles in a vertical position. All the middle of it was one crystal with the axis nearly vertical. The two ends of the bar were composed of many crystals. A weight of 1·29 kilograms was applied from 11.20 A.M. to 8.30 P.M. on December 14. During this time the maximum temperature was $-2°·8$ C.; the minimum, $-5°·6$ C.; and the mean, about $-3°·6$ C. The bar had taken the shape of the diagram, fig. 1, which is copied from a trace

FIG. 1.

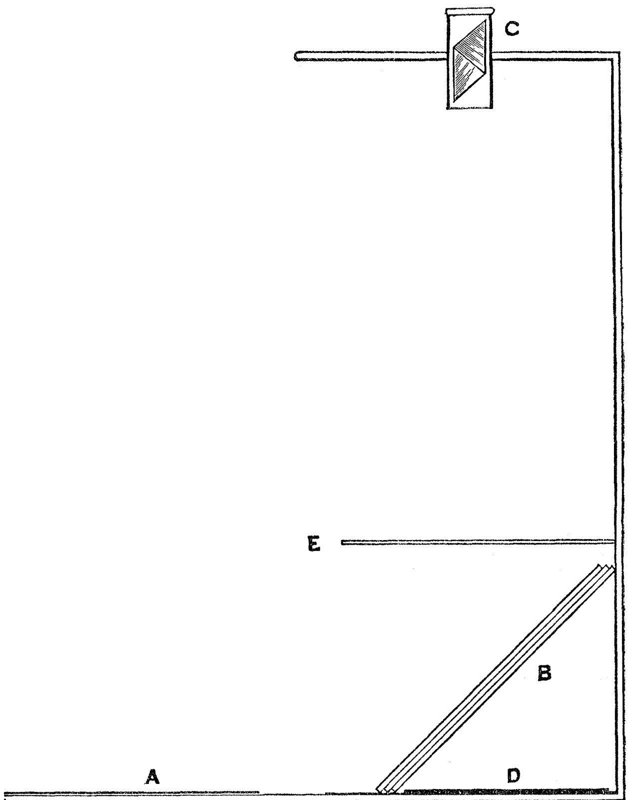


made soon after the experiment. The bends at the points indicated by a and b were more decided in the bar than in the trace. The exact position of the supports was not noted at the time, but they certainly did not extend right up to the bends at a and b. The fact that the two end pieces are still nearly in line suggests that the end surfaces

of the middle crystal are in the same position as before the bending. The question immediately suggested itself whether the bend was due to a limited number of layers sliding over each other by finite amounts, or to a true shearing strain. I examined the surfaces of the bubbles very carefully with a magnifying glass, and could find no trace of projecting edges or "faults," so I concluded it was a true shear. My polariscope was the same as was used two years ago.

Light from the white paper A, fig. 2, was reflected by the three

FIG. 2.



plates of glass, B, upwards through the Nicol C, and then the ice was laid on the glass stage E, or held in the closed hand. D was lined with black velvet. This simple apparatus served its purpose excellently, and it was seldom that I wished for a more elaborate apparatus with convergent light. The bent bar under this polariscope was found to have the optic axis as nearly as I could tell normal to the

bent faces throughout. If the black centre was near the middle of one half, the sharp bend was crowded with narrow coloured bands which moved slowly along as the bar was tilted, till as each band reached the straight piece beyond the bend it moved rapidly and broadened out.

The movement of the bands across the bend, though slow, was quite regular, so the direction of the optic axis changed quickly but not *per saltum.*

Exp. 4.—A similar arrangement. The bar was all one crystal except the parts actually on the supports. The optic axis was transverse, but horizontal. Depth, 9·5 mm.; breadth, 10 mm.; supports, 75 mm. apart. The weight of 1·29 kilos. was applied over 42 hours from 4.15 P.M. on December 15 to 10.35 A.M. on December 17. The minimum temperature was $-7°·8$ C., the maximum $-1°·1$ C., the mean about $-3°·3$ C.

Decided evaporation had taken place; the edges of the bar were rounded and the string which had stuck to the bar was raised on ridges. The greater part of the bar was $8\frac{1}{4}$ mm. deep, 9 mm. broad. In comparing the traces taken before and after the experiment I could find no bending. It certainly did not amount to half a millimetre. The traces were taken by laying the bar on a sheet of paper and following the upper and lower edges with a pencil.

Exp. 5.—The same bar, turned so as to put the optic axis vertical, bent rapidly.

Depth $8\frac{3}{4}$ mm., breadth $8\frac{1}{4}$ mm. Distance between supports 73 mm. The weight of 0·62 kilo. was applied from 11.10 A.M. to 9.5 P.M. on December 17. The minimum temperature was $-4°·4$ C., the maximum $-1°·7$ C., mean about $-3°·0$. The depression of the middle measured on the trace was about 4·4 mm., which had taken place in 10 hours. Assuming that in Experiment 4 the depression was less then 0·5 mm., the bending of the bar in the new position must have been at least thirty-seven times as fast. It is true the depth and breadth were slightly less, but the weight was less than half as great. The results of Exp. 3 as to bubbles and optic axis were confirmed.

Exp. 6.—A bar with the axis longitudinal.

I obtained a large lump of thick ice from the Davos lake, and from this cut a bar which appeared to be all one crystal, with the axis longitudinal. I need not enter on the details of the experiment, especially as the temperature rose above freezing-point. But at the end the bar had the shape shown in the diagram, Figs. 3, 4.

The dotted line indicates a division between the crystals. The double-headed arrows show the direction of the optic axis in different parts, or at least the projection of that direction on the plane of the paper. This was determined by making the field of the polariscope as dark as possible, putting the part of the bar in question in the middle

FIG. 3.

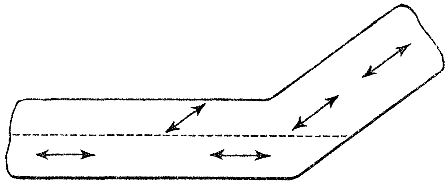
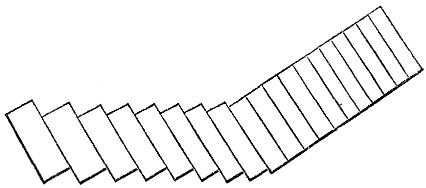


FIG. 4.



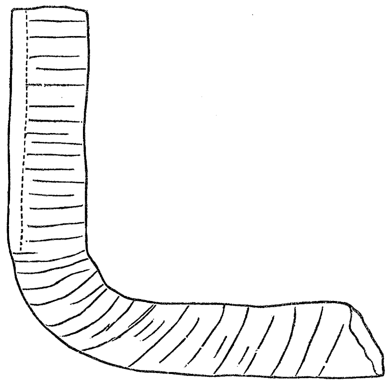
of the field and then turning it till it looked as dark as possible. When this is done the axis lies in the principal plane of the instrument. It will be noticed that in each crystal the direction of the optic axis is almost uniform. I imagine that the two crystals existed virtually in the bar, but that their optic axes were so nearly parallel that in the polariscope they behaved as one crystal. The kind of shear that must have taken place in the upper crystal is represented in fig. 4 by a number of layers of finite thickness slipped over one another.

I cannot say definitely that the bending was either slower or faster than in a bar all one crystal with the axis vertical.

The part beyond the dotted line is perhaps due to the intrusion of another crystal completely overlapped by the main crystal, or perhaps to some alteration of the optical qualities due to elastic strain.

Exp. 7.—Another bar cut from the same lump was a single crystal with the axis nearly longitudinal, inclined perhaps at 5° to the side of the bar. Breadth 10·7, depth 10·5, distance between supports 84 mm., weight 1·29 kilos. After six hours, during which time the temperature had been between $-1^\circ{\cdot}7$ and $-0^\circ{\cdot}6$ C., the bar was found lying at the bottom of the box broken into two pieces. It had bent so much that it must have slipped down between the supports and been broken in the fall. The two parts could be accurately pieced together. At the dotted line there was a very rapid but not sudden change in the direction of the optic axes. The shape of the surfaces normal to the optic axes is shown in fig. 5 (p. 330). These sliding surfaces must have the geometrical property that the normal drawn at any point to any point is also normal to all the surfaces it cuts within the bar. It is in fact parallel to the optic axis all along its course.

FIG. 5.



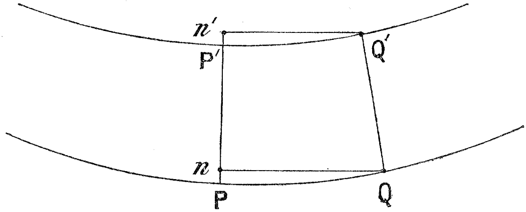
It will be noticed that the directions of the optic axis in different parts form a series of straight lines. This is an immediate consequence of the hypothesis of the existence of sliding surfaces, and may be shown in the following way.—In the part of the crystal beyond the dotted line, however, this rule does not hold good.

In the original unstrained crystal the optic axis is in the same direction everywhere. Hence layers perpendicular to it are of equal thickness throughout. Their subsequent bending and slipping does not affect their uniformity of thickness.

We need only consider one of the principal directions of curvature.

Draw PP′, QQ′ normal to the surface at P, Q, to meet the next surface at P′Q′. On PP′ drop perpendiculars Qn, $Q'n'$. All the quantities in small distances are small except the radii of curvature ρ, ρ' at P and P′. Since the thickness of the layer is uniform, $PP' = QQ' = nn'$. Thus to the second order of small quantities $Pn = P'n'$.

FIG. 6.



But since Pn is normal to the curve PQ at P, $Pn = Qn^2/2\rho = Q'n'^2/2\rho'$ to the second order. Now this would have been the

expression for $P'n'$ if it had been normal to the curve $P'Q'$ at P' and $Q'n'$ had been drawn perpendicular to it, and PP' is normal to the curve $P'Q'$ at P'.

Exp. 8.—This was an experiment on a bar composed of three crystals designed to investigate the action at the interfaces of crystals. The bar bent a good deal, but nearly the whole bend occurred in the middle of one of the crystals. I had cut nicks in the sides of the bar to test for migration of the interfaces within the ice, but found none. It appears, in fact, that the interfaces do not in any way assist the plasticity, but hinder it by fettering the sliding of the layers in the separate crystals.

Exp. 9.—Out of some thick ice formed on the surface of the water in a foot-bath I cut a bar which was all one crystal. When the bar was in position the optic axis was horizontal, and inclined at about 60° to the length of the bar.

Breadth, 13 mm.; depth, 11·7 mm.; distance between supports, 38 mm.

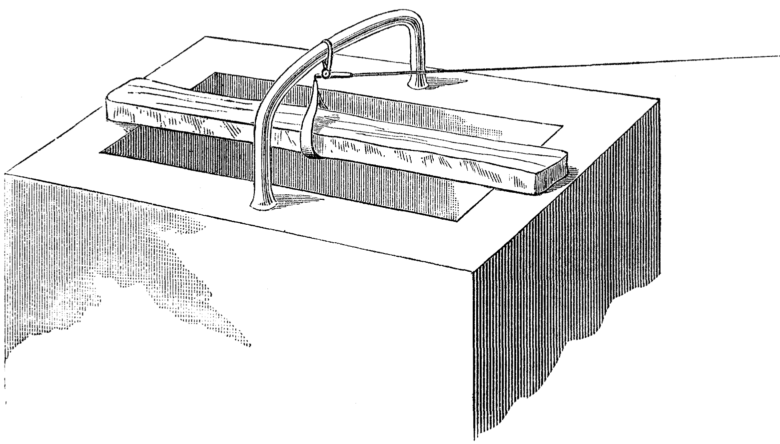
A weight of 1·29 kilos. was applied for $16\frac{1}{2}$ hours from 4.50 P.M. on January 29 to 9.15 A.M. on January 30, during which time the maximum temperature was $-5°$ C., the minimum $-12°·7$ C., and the mean about $-8°·6$ C. The depression of the middle was 1·9 mm. A little consideration will show that by the theory of the sliding layers, the upper and lower surfaces of the bar should be bent in such a manner as to still contain straight lines perpendicular to the optic axis. Some such deformation was observed, but it was not very definite. I noticed that the numerous bubbles which were originally parallel to the axis were still parallel to the upper and lower surfaces in their neighbourhood.

I now set up an arrangement for obtaining more accurate measurements of the rate of bending. A large square aperture in an iron plate was bridged by a curved iron bar rigidly attached to the plate. The bar of ice was placed across the aperture. A loop attached to the curved bar supported a wire lever, of which the long arm served as a pointer on a scale, and the short arm carried a stirrup which embraced the ice. When the bar bent the stirrup was depressed and the pointer raised about twenty-eight times as much.

This part of the apparatus was placed in a cigar box, at one end of which the pointer projected through a slit, while there was a hole in the bottom to allow the string, to which the weight was attached, to pass through. The Six thermometer was on a level with the ice, and could be read by gently lifting the lid without disturbing anything. The mirror and scale with which the position of the pointer was read were fastened to the box.

The only part of the stirrup that touched the ice was the flat piece of tin at the bottom. This was slightly roughed and made flat, so

Fig. 7.



that it should not slip off the projection left by the gradual evaporation of the unsheltered surface.

The string carrying the weight was put as close as possible to the stirrup without risk of touching it, and so that the central point of the aperture came somewhere between the two.

Exp. 10.—The bar was taken from the same bath ice as in the last experiment. It was all one crystal with the axis vertical. The first attempt was a failure, owing, I believe, to some snow getting underneath the iron plate, and, by giving way gradually, tilting up the plate. I had put a good deal of snow inside the cigar box, with the hope of preventing evaporation. This made the readings erratic and unreliable, so the next day I turned the bar over to give the stirrup a smooth surface to bear upon, and started fresh. The results are given in the table (p. 333). I think the amount of depression may generally be trusted to within 0·01 mm.

Several interesting points are brought out in this table. When the weight is changed, the alteration in the rate of depression is great out of all proportion, *e.g.*, the alteration from 0·0058 to 0·410 when the weight is changed from 0·174 to 1·47 per square cm. During the course of the experiment there was a decided rise in plasticity; compare the earlier with the later rates under 1·47 per square cm. at similar temperature. This is corroborated by the increase of speed under 0·85 kilo.

The only exception, viz., the decrease of speed at first under 41·7 kilos. was due, I believe, to the elastic strains which had been set up in the preliminary bending. The effect of these elastic strains is shown by the undoubted rise of the middle of the bar when the weight was removed at the end of the experiment.

Exp. 10.

Table I.—Ratio of magnification, 28. Average breadth, 14·2. Depth, 12. Area, 1·7 sq. cm.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos. per sq. cm.	Temperature.	
							Maximum.	Mean.
Jan. 30. 9.30 A.M. 10.0 ,, 10.31 ,,	3·3 2·5	0·118 0·089	0·5 0·517	0·236 0·172	2·5 ,,	1·47 ,,	−6·7 −7·2	−6·7 −7·2
10.33 ,, 11.43 ,,	2·4	0·086	1·17	0·0735	1·445	0·85	−6·1	−6·7
11.45 ,, 12.55 ,,	3·6	0·128	1·17	0·109	,,	,,	−6·1	−6·7
12.59 ,, 2.3 P.M. 3.55 ,,	−3·5 +3·5	−0·125* +0·125	1·07 1·87	−0·117 0·067	0·915 ,,	0·54 ,,	−7·2 −8·3	−7·8 −8·9
4.3 ,, 5.32 ,,	+2·4	+0·086	1·48	0·058	,,	,,	−9·2	−10·0
5.35 ,, Jan. 31. 8.25 A.M.	+2·4	0·086	14·8	0·0058	0·295	0·174	−10 0	−15·0
8.28 ,, 9.16 ,,	9·2	0·328	0·8	0·410	2·5	1·47	−7·8	−8·9
9.18 ,, 9.36 ,, 10.34 ,,	−1·3 −0·6	−0·046 −0·021	0·3 0·97	−0·153 −0·022	0·0 ,,	0·0 ,,	−7·8 −6·7	−7·8 −7·2

* This was almost certainly a mistake in the reading.

Table II.—Ratio of magnification, 28. Breadth, 13·7. Depth, 11·95. Area, 1·63.

Exp. 11.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos. per sq. cm.	Temperature.	
							Maximum.	Mean.
Jan. 31. 12.30 P.M.								
8.50 „	−0·5	−0·018	8·33	−0·00215	2·5	1·53	−7·5	−8·9
Feb. 1. 10.8 A.M.	−0·3	−0·011	13·3	−0·00083	„	„	−11·1	−17·2

Into this matter I enter more fully below.

The indicated rise between 12.59 and 2.3 P.M. is, I feel sure, simply due to a misreading. Whenever the weight was altered the apparatus was unavoidably disturbed, so I had to take an entirely fresh reading of the pointer. Generally this only differed by fractions of a millimetre from the previous reading, but in the case in point it was nearly 6 mm. greater. The ice showed an inconvenient tendency to slip backwards on the iron plate, thus bringing the end of the pointer forwards till it almost touched the edge of the slit. The ice had to be pushed forwards three or four times during the experiment. Of course a fresh reading was taken after each such displacement, so that no error resulted. This trouble was caused doubtless by the plate not being accurately level. In subsequent experiments I was more successful in avoiding it.

Exp. 11.—I desired to establish with the more delicate system of measurement that the plasticity is inappreciable when the bending stress is applied at right angles to the axis. I cut a bar, all one crystal, from the bath ice, and planed it so that the upper and lower surfaces were as accurately as possible parallel to the optic axis. In the polariscope, when the middle of the black cross was in the middle of the bar, the two faces were equally inclined to the lines of sight. I then set up the apparatus in the usual way. The results are seen in the annexed table (p. 334).

It will be seen that the pointer indicated a rise of the stirrup amounting in the $21\frac{1}{2}$ hours to 0·29 mm. As was before mentioned, the stirrup was slightly roughed to prevent it from slipping, so at first it would make contact with the bar at only a few points. Evaporation would help to extend the contact to large surfaces, and admit of a slight movement of the stirrup relatively to the ice. Thus the experiment was not as satisfactory as could be wished. It is possible that a very slight depression of the bar might be masked by this effect of evaporation. But even supposing that the rate of real depression was twice as great as that of the apparent elevation, viz., 0·0043 mm. per hour, it would still be very small compared with the rates of the next experiment. I am at any rate entitled to say that within the limits of error of experiment there is only one kind of plasticity in an ice crystal, viz., that due to the sliding layers at right angles to the optic axis. It is probable that the same source of error was active in other experiments, but in them the effect would be almost negligible.

Exp. 12.—The same bar was turned on its side so that the optic axis was vertical.

Exp. 12.

Table III.—Ratio of magnification, 2. Breadth, 11·28. Depth, 13·2. Area, 1·48.

Time.	Apparent extension.	Real extension.	Interval in hours.	Rate in mm. per hour.	Kilos. per sq. cm.	Temperature. Maximum.	Temperature. Mean.
Feb. 1. 10.15 A.M. 10.57 „ 1.17 P.M.	0·9 7·6	0·032 0·272	0·7 2·33	0·0457 0·117	1·69 „	−14·4 − 8·9	−15·3 −11·1
1.21 „ 2.2 „ 2.18 „	7·3 4·4	0·26 0·157	0·683 0·267	0·38 0·59	„ „	− 6·1 − 4·7	− 7·0 − 5·6
2.20 „ 2.36½ „ 2.48 „ 2.54½ „ 3.1 „	7·2 7·2 4·6 5·7	0·257 0·257 0·164 0·204	0 275 0·192 0·108 0·108	0·97 1·34 1·52 1·89	„ „ „ „	− 3·3 − 2·2 − 2·0 − 1·1	− 3·9 − 2·8 − 2·2 − 1·7
3 4 „ 3.15 „ 3.46 „	−0·4 −0·4	−0·014 −0·014	0·183 0·517	−0·076 −0·027	0·0 „	− 2·5 − 3·6	− 3·3 − 4·7
3.47 „ 4.1½ „	8·7	0·31	0·262	1·18	1·69	− 5·8	− 6·1
4.4 „ 8.11 „	−2 9	−0·104	4·12	−0·0252	0·0	− 6·4	− 9·7
8.14 „ 8.38½ „ 9.17 „	8·5 12·3	0·304 −0·44	0·408 0·642	0·745 0·685	1·69 1·69	−13·0 −13·0	−13·0 −13 2
9.21 „ Feb. 2. 9.10 A.M.	−2·0	−0·072	11·8	−0·0061	0·0	− 6·7	− 7·8

We first notice that the plasticity exists down to $-14^{\circ}\cdot 4$. At this temperature the bending was slow, but this was due in great part to the fact that it came at the beginning, and the bar was as usual. The rapid growth of plasticity, independently of the temperature, is shown by the rate of 0·59 mm. per hour at a mean temperature of $-5^{\circ}\cdot 6$, being raised in less than two hours to 1·18 mm. at $-6^{\circ}\cdot 1$. The tendency to recover when the weight is removed is shown three times over in the table. As might be expected, it soon becomes very slow, and in that case after twelve hours, when the recovery amounts to 0·72 mm., it has probably stopped altogether. In the fall of rate from 1·89 at $-1^{\circ}\cdot 7$ to 1·18 at $-6^{\circ}\cdot 1$ and 0·745 at -13°, in spite of the natural tendency for the rate to rise, we seem to have a real effect of temperature. After 8.38, the cigar box had to be left open as the pointer had almost reached the lid of the box, and so the subsequent temperatures are unreliable. I imagine that the change from 0·685 to 0·745 was due to a fall of temperature.

At the beginning of Exp. 11 the bar measured 14 mm. by 12·3 mm., which was reduced at the end of Exp. 12 to 13 mm. by 10·8 mm. The evaporation had been rather more rapid just at the bend of the bar. This was owing, I believe, to the circulation of air through the hole by which the string passed out.

I measured the total depression on the trace as 2·6 mm. As measured by the pointer it is 2·45. The agreement is as good as could be expected.

Exp. 13.—In this experiment I used a thicker bar and tried a variety of weights. The bar was only just small enough to go into the stirrup. (See Table IV, next page.)

The stiffness of the bar in the first three hours is surprising.

Exp. 14.—In all the experiments hitherto on bars composed of single crystals it happened that the optic axis had been vertical when the ice was formed, so that the planes of freezing coincided with the sliding layers. I fully believed that this coincidence was merely accidental, and what happened in Exp. 8 had confirmed this idea, but I thought it desirable to have a more direct proof. So I cut a piece out of a good large crystal in the ice, found on the surface of the water in the bucket, in which the optic axis was not vertical. When the bar was put in position the planes of freezing were vertical and parallel to the length, and the optic axis was normal to the length and inclined at about 50° to the vertical. The bar was about 8 mm. square, and the distance between the supports was 51 mm.

Under a weight of 0·62 kilo. in 4 hours 28 minutes at a mean temperature of $-4^{\circ}\cdot 4$ (the maximum $-1^{\circ}\cdot 4$) it bent downwards about 4 mm. There was a large lateral bend, which made the vertical bend very difficult to measure.

If the sliding layers had been necessarily the same as the planes of

Exp. 13.

Table IV.—Ratio of magnification, 26. Depth, $17\frac{1}{2}$. Breadth, 15·7. Area, 2·75.

Time.	Apparent depression.	Real depression.	Interval in hours.	Rate in mm. per hour.	Weight in kilos.	Kilos. per sq. cm.	Temperature.	
							Maximum.	Mean.
Feb. 2. 10. $40\frac{1}{2}$ A.M. 2. $35\frac{1}{2}$ P.M. 4. 0 ,,	3·8 5·9	0·146 0·227	3·92 1·41	0·0372 0·160	2·5 ,,	0·91 ,,	−3·9 −3·9	−5·0 −5·0
4. 1 ,, 5. $33\frac{1}{2}$,,	2·2	0·084	1·56	0·054	1·29	0·47	−6·1	−6·7
5. 35 ,, 6. $2\frac{1}{2}$,,	1·9	0·073	0·46	0·159	2·5	0·91	−7·2	−7·5
6. 6 ,, 6. 30 ,, 7. $33\frac{1}{2}$,,	3·1 8·9	0·119 0·342	0·40 1·06	0·297 0·323	3·79	1·38 ,,	−7·8 −8·0	−8·0 −8·9
7. 36 ,, 8. $20\frac{1}{2}$,,	3·8	0·146	0·74	0·197	2·5	0·91	−9·7	−10·0
8. 26 ,, Feb. 3. 9. 16 A.M.	7·5	0 288	12·83	0·0225	0·62	0·225	−6·4	−7·8
9. $18\frac{1}{2}$,, 9. 54 ,,	4·3	0·165	0·59	0·28	2·5	0·91	−6·1	−6·1
9. 57 ,, 10. $21\frac{1}{2}$,,	6·1	0·235	0·41	0·57	3·79	1·38	−5·9	−5·9

freezing, this bar should not have bent at all. If, however, the sliding layers are necessarily perpendicular to the optic axis, this bar should have been free to bend on the plane of the optic axis, but not in the perpendicular plane. In the experiment the plane of the total bend contained the optic axis. Thus the experiment was decisive.

In attempting to discover the manner in which the rate of the molecules sliding over each other depends on the driving force, we are met by the difficulty that the rate of depression depends on at least three other circumstances, the temperature, the previous history of the bar, and the irregularity of the stresses and strains within the bar. The second is to some extent avoided by only considering the rates observed immediately before and immediately after the change of weight. The third is probably not very important. In the following table are collected all the instances which occurred, with the attendant changes of temperature. The changes of rate are not so great as the square, but greater than the first power of the changes of the applied force. In the table may be seen the amount of correspondence with the power $\frac{3}{2}$. The two most glaring discrepancies are in the second

Table V.

Change mean temperature.	Change of weight in kilos. per sq. cm.	$\left(\frac{\text{Old force}}{\text{New force}}\right)^{\frac{3}{2}}$.	Change of rate in mm. per hour.	Ratio of rates.
− 7·2 to − 6·7	1·47 to 0·85	0·442	0·172 to 0·0735	0·427
−10·0 to −15·0	0·54 to 0·174	0·182	0·058 to 0·0058	0·100
−15·0 to − 8·9	0·174 to 1·47	24·6	0·0058 to 0·410	70·7
− 5·0 to − 6·7	0·91 to 0·47	0·373	0·160 to 0·054	0·338
− 6·7 to − 7·5	0·47 to 0·91	2·70	0·054 to 0·159	2·95
− 7·5 to − 8·0	0·91 to 1·38	1·87	0·159 to 0·297	1·87
− 8·9 to −10·0	1·38 to 0·91	0·537	0·323 to 0·197	0·610
−10·0 to − 7·8	0·91 to 0·225	0·122	0·197 to 0·0225	0·114
− 7·8 to − 6·1	0·225 to 0·91	8·15	0·0225 to 0·28	12·4
− 6·1 to − 5·9	0·91 to 1·38	1·72	0·28 to 0·57	2·04

and third instances given in the table, when the power 2 is well satisfied. But these discrepancies may be largely, if not entirely, explained by the great change of temperature. Without elevating the statement to the rank of a law, we may say that fairly close agreement with the observed facts is obtained by supposing that when the molecules of ice slide on each other the cube of the friction varies as the square of the velocity.

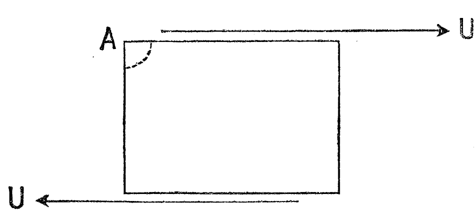
In attempting to pass from the rate at which the centre of a loaded bar sinks to the coefficient of plasticity, we meet with considerable difficulties, and shall have to content ourselves with a rough approximation. It might well be thought that the problem of a rectangular

elastic bar, supported at either end and loaded in the middle, had been fully worked out. But this does not appear to be the case. The ordinary elementary treatment makes the gigantic assumption that plane cross-sections of the unbent bar remain plane, and that the lateral contraction or expansion of elementary strips parallel to the ength of the bar under longitudinal pulls or thrusts are the same as in free space. It does not consider any shearing stresses or strains. It is true that Rankine ('Applied Mechanics,' p. 338), assuming the results of this method, proceeds to find an expression for the shearing stress. He makes it proportional to $a^2 - x^2$, where the origin is at the centre of the bar, the axis of x is drawn upwards, and $2a$ is the depth of the bar. But this expression is inconsistent with the general equations of an elastic solid. St. Venant's solution of the bending of a bar, given in Thomson and Tait's 'Natural Philosophy,' postulates equal and opposite couples applied at the two ends, so that the bending moment is uniform throughout. The importance of the absence of this uniformity is not trifling but fundamental, for in our case everything depends on the shears, and in St. Venant's solution there are no shears.

I fancy that I see my way to obtaining the complete solution in the form of infinite series. But, since it ceases to be applicable the moment plastic strains take place, it would only enable us to determine the initial stresses, and this would hardly justify the insertion here of such a long investigation.

The following simple but imperfect treatment must suffice. Let us first define the coefficient of plasticity. Take a rectangular element with two faces normal to the optic axis, and let these faces be subjected to a tangential force U per unit of area in opposite directions, parallel to another pair of faces.

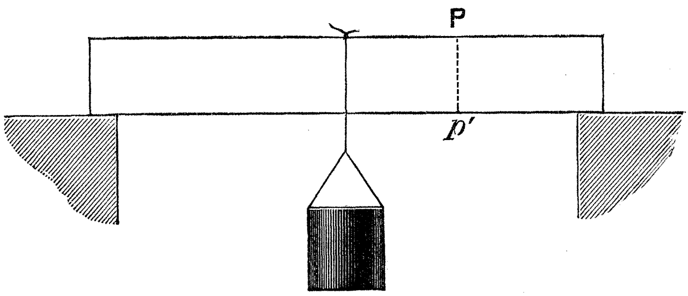
FIG. 8.



Then if the rate of growth of two of the angles, or rate of diminution of the other two be denoted by $d\chi/dt$, the coefficient of plasticity p may be defind by the equation

$$\frac{d\chi}{dt} = p\mathrm{U} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (1).$$

FIG. 9.



The bar is represented in fig. 9, with a weight W hanging from the middle. The length between the supports is l, the breadth b, the depth d. U is the force per unit area which acts on a small vertical interface in a vertical direction, and when U is positive the matter to the left of the interface is urged upwards. The force per unit area on a horizontal interface in a direction parallel to the length of the bar is necessarily the same, and is also denoted by U. Consider the equilibrium of the part of the bar to the right of any cross section PP′. It is urged upwards at the support by a force equal to $\frac{1}{2}$W; therefore, if we neglect its weight, the total vertical force on the section PP′ is also $\frac{1}{2}$W.

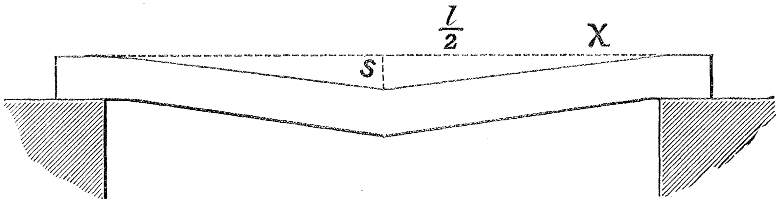
If $\overline{\mathrm{U}}$ be the average of U over the section

$$bd\overline{\mathrm{U}} = \tfrac{1}{2}\mathrm{W} \quad \ldots\ldots\ldots\ldots\ldots\ldots \quad (2).$$

U cannot be constant over the section, for it necessarily vanishes at the upper and lower surfaces of the bar.

The average shear over any cross section being the same, except that the sign suddenly changes at the middle of the bar, it is reasonable to suppose that the same amount of plastic shearing strain would take place between the layers perpendicular to the optic axis at every cross section. This condition makes the bar bend sharply when the weight is applied, and keeps the two halves straight. In the earlier experiments, where the bending was considerable, this form was observed before its cause had been perceived. For this form to be assumed without elastic strain, the plastic strain must be the same, not merely in corresponding points of different cross sections, but also throughout each cross section itself, and, in fact, throughout the entire half of the bar. But as we have seen, the shearing stress must vanish at both the upper and lower surfaces. Doubtless the truth is that the state of shearing strain is nearly uniform throughout the bar, except close to the surface, where it rapidly diminishes to zero. Probably in these regions the elastic strains are very great, and quite different from what they are elsewhere.

FIG. 10.



Let s be the depression of the middle of the bar, χ the angle either half makes with the horizontal. We have $s = \frac{l}{2}\chi$. When χ is small,

$$2\frac{ds}{dt} = l\frac{d\chi}{dt} = lp\overline{\text{U}} = lp\frac{\text{W}}{2bd},$$

and

$$p = \frac{4}{l}\frac{bd}{\text{W}}\frac{ds}{dt} \dots\dots\dots\dots\dots\dots \quad (3).$$

This gives the coefficient of plasticity in terms of the unsupported length of the bar, the weight per unit area of cross section, and the observed rate of depression. We have employed equation (2), which is strictly applicable only when the bar is straight and horizontal. But, in the cases to which we have to apply these results, χ is so small that the error is negligible. It was hardly worth while calculating the numerical value of p, especially as it has been shown to depend on the temperature, on the value of $\overline{\text{U}}$ nearly, and also on the previous history of the bar. But the above investigation will assist any one in estimating, as far as can be done from my experiments, the rate of distortion of an ice crystal in any given case.

In several cases in the experiments, after a heavy weight was removed, a slight gradual unbending of the bar took place. At first I thought this a mere consequence of the irregular elastic strains on the bar, the parts most severely strained gradually bending back the rest. But the magnitude of the recovery seems, on closer examination, to put this explanation out of the question, and I have now little doubt that it is a true molecular effect.

In Exp. 12, after a stress of 1·69 kilos. per sq. cm. had been removed, the middle of the bar rose 0·0104 cm. in four hours. According to an experiment by Moseley ('Phil. Trans.,' 1871), Young's modulus for ice is 92,700 kilos. per sq. cm. Hence, if we neglect the effect of the plastic strains in one bar of ice, the elastic depression under 2·5 kilos. should have been 0·00138 cm., less than one-seventh of the recovery observed. The permanent or plastic strains in Moseley's bar are considerable, so that the deduced value of Young's modulus may be too great. Bevan, also by flexure of bars

of ice, found the value 60,000. Reusch ('Nature,' vol. 21, p. 504), by experimenting on the sonorous vibrations of rectangular plates of ice, found Young's modulus to be 23,632 kilos. per sq. cm. (this last method seems rather dangerous). In attempting to devise an imaginary system of strains sufficiently great to render such a recovery as 0·1 cm. possible, we are soon brought up by the breaking tension of ice. Direct experiments by Moseley give this as 7 or 8 kilos. per sq. cm., and Kidd and myself found it in one case to be 8·3 kilos. per sq. cm., but the fact that the bar of ice in Exp. (11) bore the weight of 2·5 kilos. before any plastic strains had taken place brings it out greater than 15·5 kilos. per sq. cm., and the bar in Exp. (13) was able to endure an even greater stress.

A similar discrepancy has been noticed in the case of cast iron (Rankine, 'App. Mechanics,' § 297).

Using the latitude given by the uncertain values of the constants to the utmost, I have not been able to devise any system of elastic strains which could possibly make the bar rise 0·01 cm., and there is no reason to suppose that the unknown system of strains actually occurring in the experiments would be exceptionally well adapted to such a purpose. I conclude, then, that we have to deal with a real tendency of the forcibly displaced sliding layers to slide back. The rate of recovery, rapid at first, soon falls off. Thus in Exp. (10) there was a recovery of 0·046 mm. in the first 18 minutes, and only 0·021 in the next 58. In Exp. (15) after 0·014 in the first 11 minutes, and the same in the next 31, the motion probably came to a standstill after a few hours, practically, if not absolutely. Thus in Exp. (12) the bar was left with no weight on for 12 hours, and the recovery was only 0·072 mm.

[Mr. McConnel died suddenly at Davos while engaged on the foregoing paper, which has been printed from his rough copy with some few alterations of no great importance. I thought it better to do this than to attempt to edit it; though I know from his last letters to me that the author would have himself, if he had lived, been able to leave it in a more finished state than that in which it now appears. —R. T. G.]

II. "On the Effect of Temperature upon the Refractive Index of certain Liquids." By W. Cassie, M.A. Communicated by Professor J. J. Thomson, F.R.S. Received February 19, 1891.

In my paper "On the Effect of Temperature on the Specific Inductive Capacity of a Dielectric" ('Phil. Trans.,' A, 1890), the

Fig. 7.

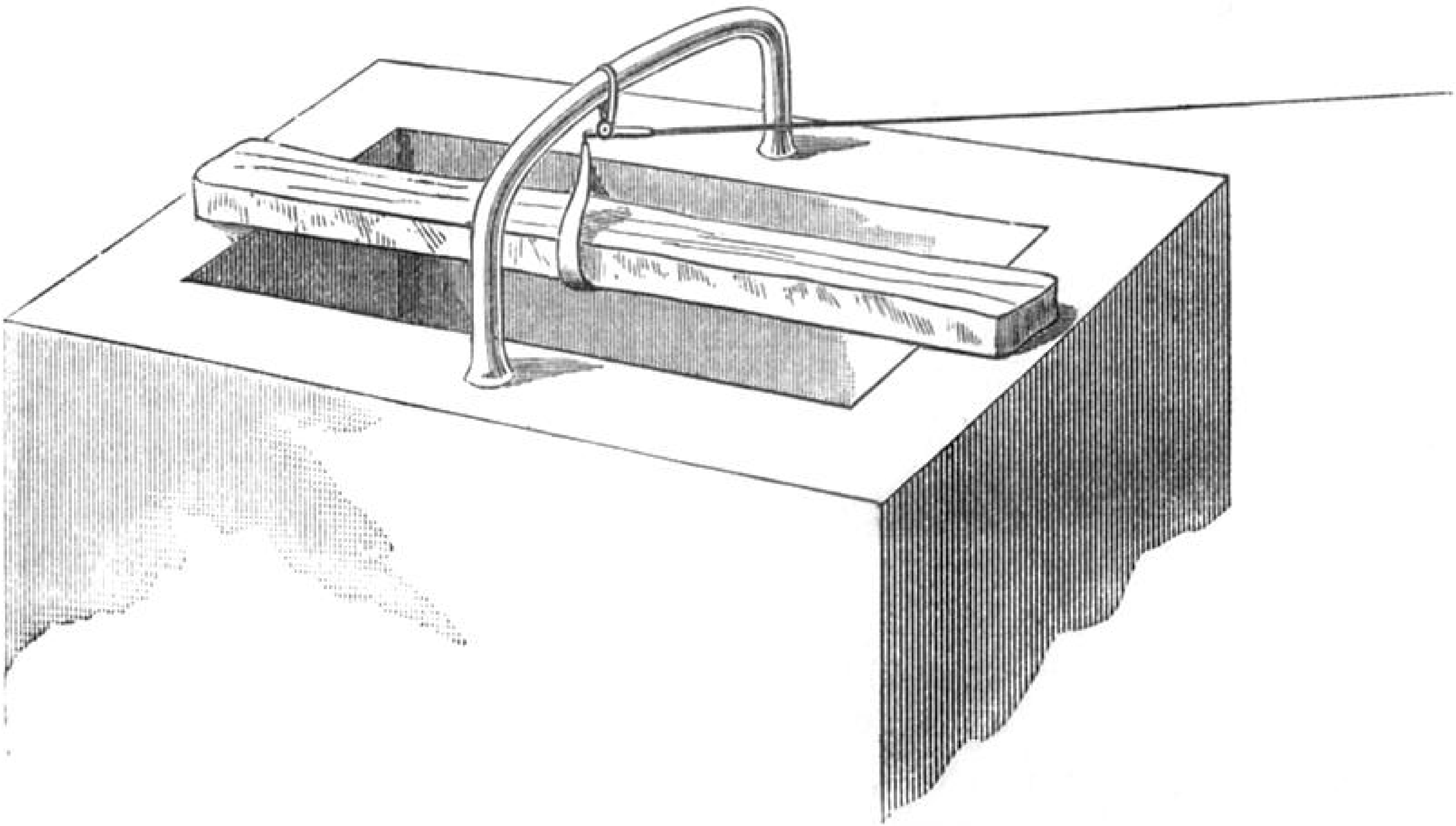


FIG. 9.