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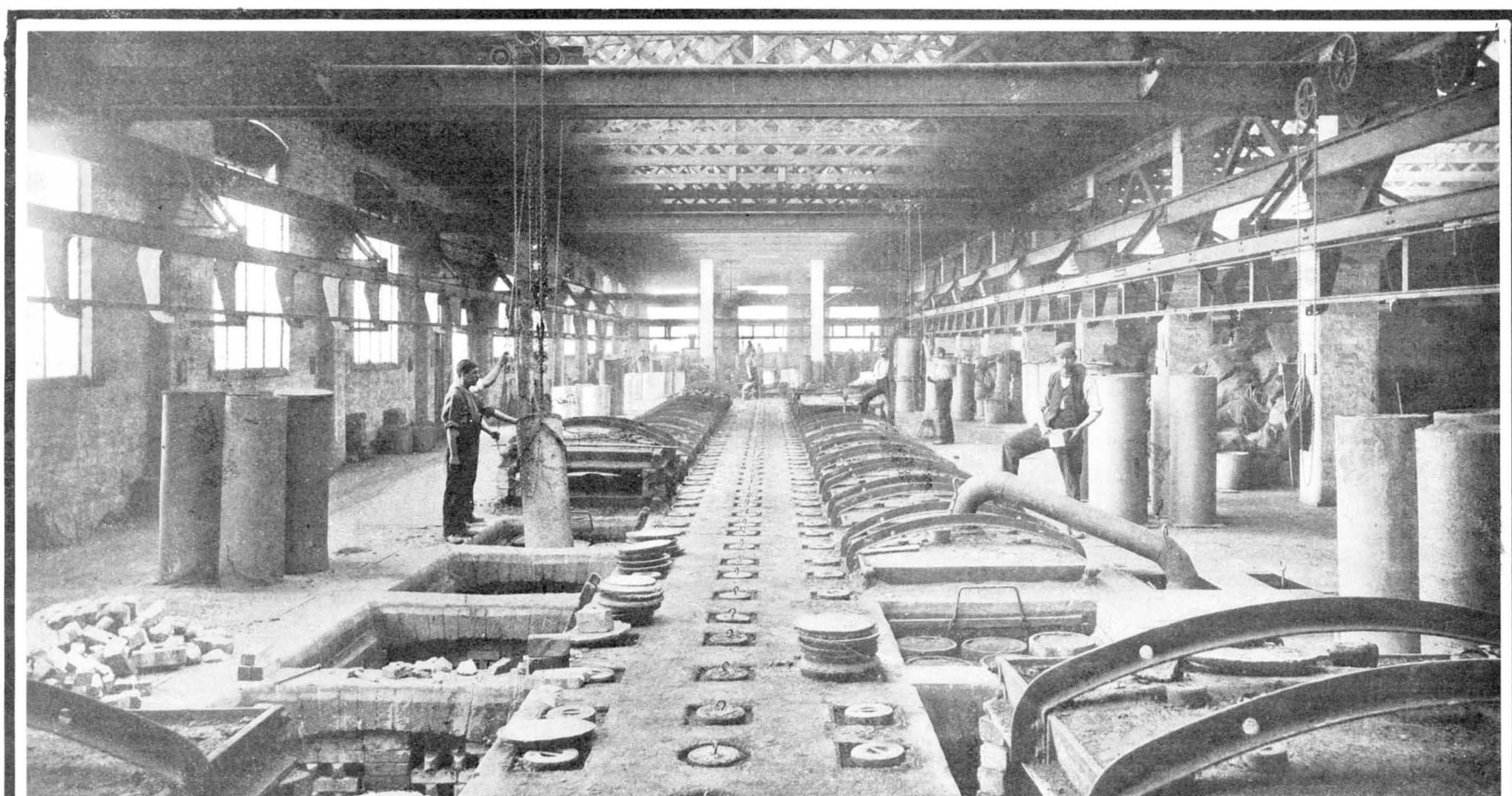
FINISHING ROOM IN THE CARBON WORKS.



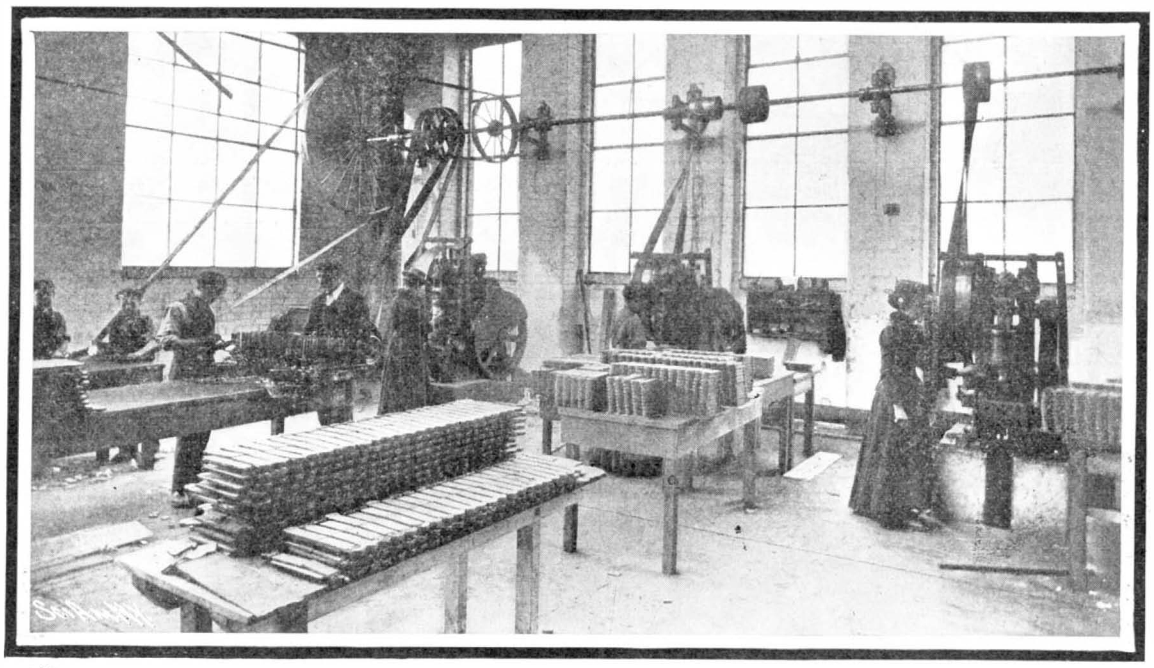
OVEN ROOM OF THE CARBON WORKS.

THE MANUFACTURE OF CARBONS IN ENGLAND.

THE MANUFACTURE OF CARBONS IN ENGLAND.*

By FRANK C. PERKINS.

ELECTRIC-LIGHT carbons and battery carbons are used very extensively in England as well as in this country and in Europe. A great many carbons are imported from Austria and Germany, although the quality of the American cored carbons is said to be now equal to those of European manufacture.



THE BATTERY PLATE ROOM.

The German Electric Company, Ltd., of London, England, have recently constructed a new plant for the manufacture of carbons at Witton near the city of Birmingham, and it may be of interest to note the arrangement and construction of this new installation and the modern machinery employed in this industry.

The accompanying illustrations show the interiors of the grinding room, the oven room, the press and finishing rooms, as well as the battery carbon room of these works. This English carbon works has been equipped with the latest electrically-driven machinery of special construction for securing rapid construction, combined with good quality and accuracy. This factory consists of two buildings containing machinery for crushing, grinding, mixing, pressing and stamping the raw material, as well as the extensive ovens required, and covers an area of several acres. The accompanying illustrations show the interiors of these buildings. The ovens are of the latest construction, and are of great importance in the manufacture of high-grade electric carbons.

In these ovens the carbons are baked, and the most improved and economical type are required, in order to produce the best carbons at prices to meet the present severe competition. The ovens have a capacity of 400,000 pounds of material, and cover a great area. The carbons after leaving the oven house are carried by means of a monorail traveler to another building, which is equipped with machinery for sorting, cutting, coring, and finishing, and then they are passed on to the storeroom and chemical laboratory for tests.

As the burned carbons leave the ovens, they are in long rods, and a large stock of these is stored ready for cutting into special lengths which may be desired. One of the carbons manufactured at this plant is known as the Apostle carbon, which is a commercial carbon largely used, while another known as the Imperial Crown is of superior quality, and is said to burn keener with less ash residue. The highest grade is known as the Witton carbon, and is extensively used for inclosed arc lamps, on account of the high quality, the minimum of ash resulting, and the small amount of discoloration on the globes of the lamps in which they burn.

In this new carbon works special machinery has also been provided for the manufacture of battery plates, manganese agglomerate blocks, carbon brushes for motors and generators and electrodes for electro-chemical works. The present plant is equipped with machinery capable of producing thirteen million carbons per year, while the machinery about to be installed will give an output of about forty million carbons per year.

The carbon works is operated by electric motors of the direct current type, having a total capacity of 250 horse-power, and the current is obtained from the overhead power lines connecting the carbon factory with the electric station. The lighting is effected by means of arc lamps of both open and inclosed types, connected in series on the 460-volt power circuit. The General Electric Works at Witton employ several hundred workmen, and by the elaborate card system used, the shop foreman is able to keep for himself a record of the position of every piece of work under construction. Whenever a new piece of work is given to a workman, he is supplied with a card showing the necessary work to be done, marked with the time of starting, and when the job is completed the card is marked by the foreman with the time of finishing same.

All of the cards belonging to one workman are kept together, and are subject to a comparison with his clock record, and in this way all of the time of the clock register is accounted for in the record of the time cards of the various pieces of work completed by that workman. The orders are subdivided into their various components before they reach the shop superintendent, in order to enable this detail costing to be carried out, and the superintendent then distributes them to his various shop foremen.

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

[Concluded from SUPPLEMENT No. 1510, page 24195.]

THE CONSTITUTION OF PORTLAND CEMENT FROM A PHYSICO-CHEMICAL STANDPOINT.*

By CLIFFORD RICHARDSON.

IN practice our industrial clinkers do not reach the highest percentages of lime because the conditions available for their production commercially do not at present permit of it, and as a matter of fact they contain a larger proportion of celit than they would were it possible to do so. This is illustrated by calculating the theoretical percentages of celit in the clinkers prepared from pure chemicals, on the basis of the composition of the two cements which have been referred to.

To the pure clinker which was made to correspond to the cement which we have called No. 1, it was found that the molecular formula 42 (SiO_2 3CaO) 5 ($2Al_2O_3$ 3CaO) could be assigned. On the alit and celit basis, after equilibrium has been established between the basicity of the silicate and aluminate, this formula might correspond to

Alit.		Celit.	
36 (SiO_2 3CaO)	1 (Al_2O_3 3CaO) + 6 (SiO_2 2CaO)	9 (Al_2O_3 2CaO)	or
32 (SiO_2 3CaO)	5 (Al_2O_3 3CaO) + 10 (SiO_2 2CaO)	5 (Al_2O_3 2CaO)	or
28 (SiO_2 3CaO)	9 (Al_2O_3 3CaO) + 14 (SiO_2 2CaO)	1 (Al_2O_3 2CaO)	

The latter form would be improbable as, in that case, the celit would be too infusible owing to the small percentage of aluminate which it would contain, but in the other two cases the celit would be rich in aluminate and on that account the cement would be quick setting, which was actually the case before it was plastered.

Which of the above formulas would be applicable here depends in any case, probably, on the rate of cooling of the clinker, and this explains why cooling at different rates may result in cement of very different properties.

An increase in the lime in the case of the cement under consideration, No. 1, would have resulted in an increase in the amount of possible tri-calcic aluminate and consequently of alit and the cement would be slower setting. This has been found to be the case in clinkers produced in the laboratory, if they were burned at a sufficiently high temperature.

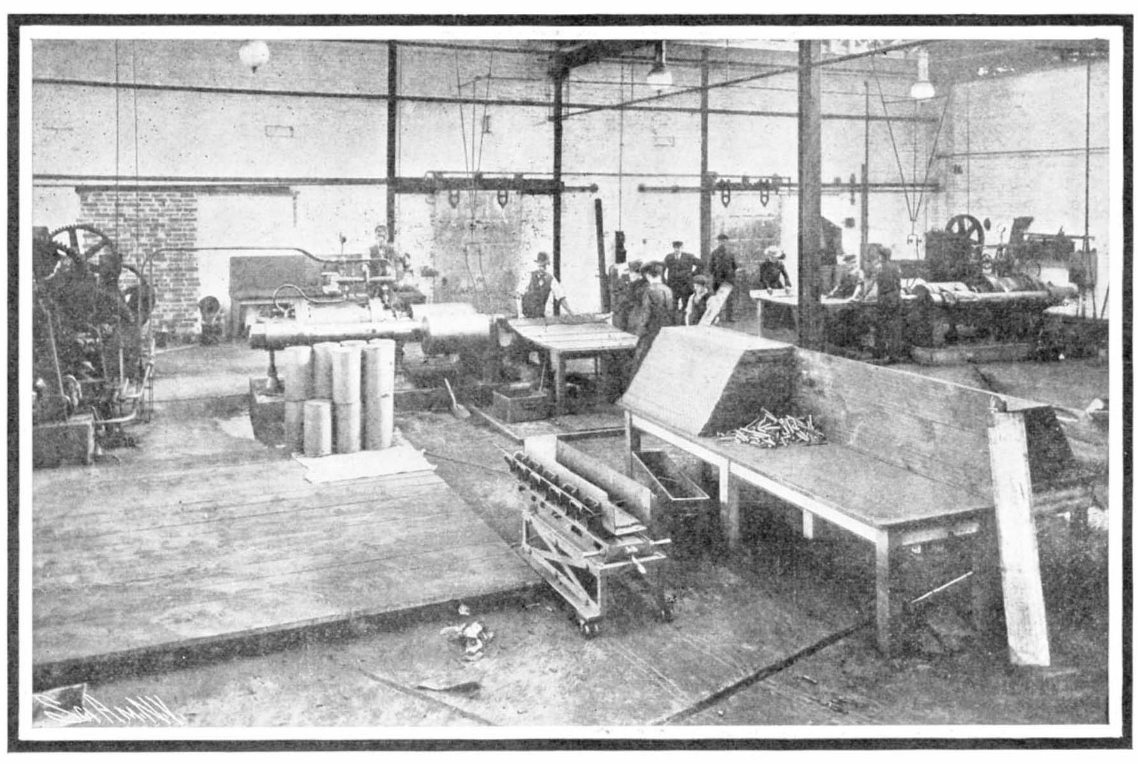
In the same way cement No. 2, to which a formula has previously been assigned, may be regarded as 15 (SiO_2 3CaO) 3 (Al_2O_3 3CaO) + 10 (SiO_2 2CaO), 2 (Al_2O_3 CaO). This clinker contains nearly 1 per cent less alumina, is a much less concentrated solution of the aluminates in the silicates than in clinker No. 1, but, on the other hand, it is much less basic and contains a larger percentage of the dilute celit, 30.8 per cent as compared to 24.4. For the first reason the cement made from the No. 1 clinker should be slower setting than that made from the No. 2 clinker, but for the second the cement from the latter clinker should be less volume constant, since celit, being a di-calcic silicate solution, possesses the characteristic lack of volume constancy of that component.

From the preceding data it is evident that industrially we may have cements of different degrees of concentration as regards aluminates in the same way that steel exists of various degrees of concentration as regards carbon. As steel, low in carbon, is called a mild steel, and as there are cements corresponding to this, in that they contain relatively small percentages of aluminates, these may be very well called mild cements. With increase of the amount of aluminate they may be called medium cements, and with high aluminates hard cements. These designations it may eventually be necessary to modify to express variations in basicity.

The question now arises as to how concentrated the solutions of aluminates in silicate may become before the clinker ceases to be a normal Portland cement. Series of clinkers were prepared in which the components were tri-calcic silicate and tri-calcic aluminate, the proportions varying between 100 per cent of the one and none of the other to the reverse through various percentages, in order to determine what the limits for the formation of alit are under such circumstances; of tri-calcic silicate and di-calcic aluminate to determine what effect a reduction in lime with a consequent increased formation of celit would have on the formation of alit and consequently on the formation of a normal Portland cement clinker, and a series in which the components were di-calcic silicate and di-calcic aluminate for the purpose of determining the limitations of the existence of celit. The first series may be called the "Alit Series," the second the "Cement Series," and the third the "Celit Series," for reasons that are readily understood.

A microscopic examination of thin sections of the clinkers constituting these series has furnished results of the greatest interest, but which are too elaborate to go into detail in regard to in the present place. In a general way it may be said that the members of the celit series possess all the characteristics of celit, as it is seen in Portland cement clinkers made from pure chemicals, up to about the molecular ratio of six of silicate to one of aluminate, 6 (SiO_2 2CaO) (Al_2O_3 2CaO), that at that ratio, or perhaps at a somewhat wider one, a solid emulsion begins to appear as part of the section; at the ratio two to one 2 (SiO_2 2CaO) (Al_2O_3 2CaO) the entire clinker consists of the solid emulsion, probably a eutectic, which begins to disappear again on the further concentration of the solu-

* An Address before the Association of Portland Cement Manufacturers.



THE PRESS ROOM.

THE MANUFACTURE OF CARBONS IN ENGLAND.

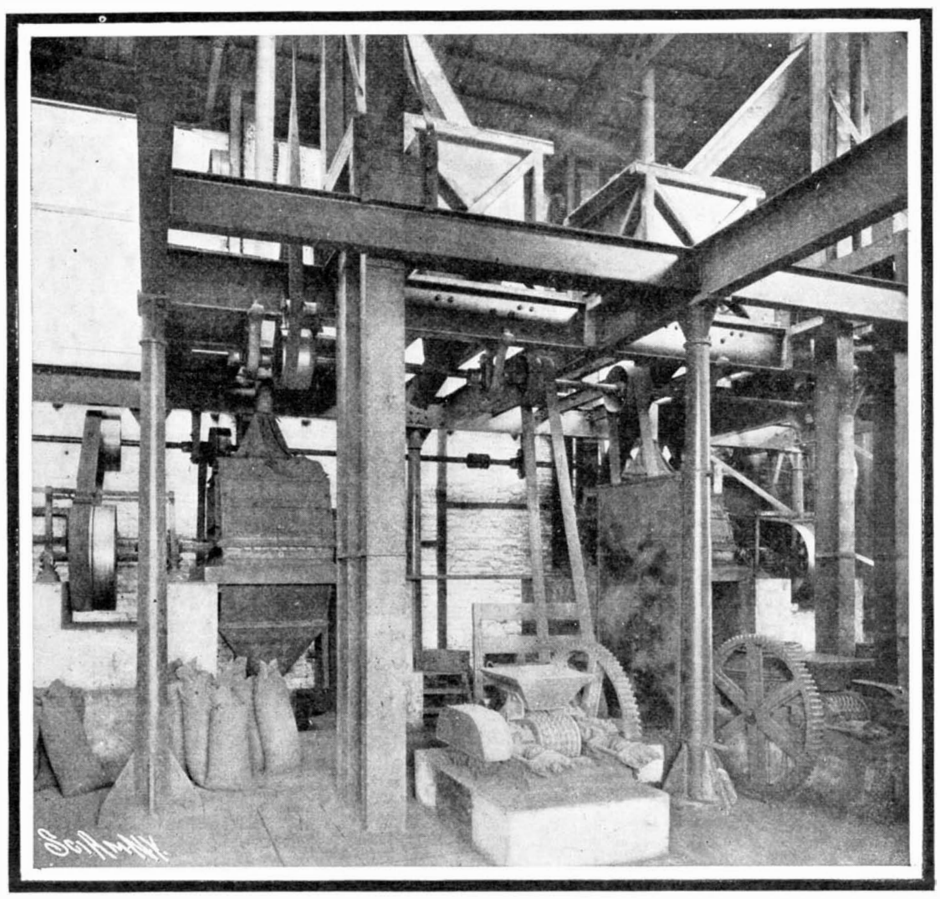
tion as regards aluminate, which phenomenon is accompanied by the appearance of cubes of the latter component before the ratio 3 (SiO_2 CaO) 2 (Al_2O_3 2CaO) is reached, while the cubes become very prominent in the concentration represented by 2 (SiO_2 2CaO). 3($Al_2O_3$2CaO) and still more so in SiO_2 2CaO, Al_2O_3 2CaO, these, of course, being solid solutions of the silicate in the aluminate. They naturally continue to increase as the concentration as regards silicate diminishes, but strangely possess a certain optical activity as if the dissolved silicate was not actually in solution in an isometric form, but was mechanically held in the isometric aluminate in its own orthorhombic form. The eutectic at this end of the series also seems to be present in a form other than that of an emulsion. In this series the solubility of the silicate in the aluminate is much greater than that of the aluminate in the silicate.

Quite a different condition is found in the cement series. Here the normal alit-celit structure of two distinct solid solutions of different basicity is maintained up to the ratio 7(SiO_2 3CaO), 3(Al_2O_3 2CaO) as has been described for the pure Portland clinkers, but when the ratio 2(SiO_2 3CaO), (Al_2O_3 2CaO) is reached there is a decided change in structure, the solution assuming the emulsion form corresponding to that of the clinker of the ratio 2(SiO_2 2CaO), (Al_2O_3 2CaO) of the celit series and having a higher general optical activity, but an absence of a distinct segregation of celit. This may, provisionally, be regarded as the eutectic of this series.

The Portland cement ratio may, therefore, be regarded as extending from:

	SiO_2	Al_2O_3	CaO
Pure tri-calcic silicate	26.4	0.0	73.6
to			
7($SiO_2$3CaO), 3($Al_2O_3$2CaO)	18.9	13.6	67.5

Beyond the latter degree of concentration the solutions or clinkers have not the structure of Portland cement and cannot be regarded as such although they are hydraulic.



THE GRINDING ROOM.

THE MANUFACTURE OF CARBONS IN ENGLAND.

The eutectic, in this series, must be considered as consisting not of an emulsion of saturated solid solutions of one definite silicate in an aluminate of the same basicity and of the same aluminate in the same silicate, as in the celit series, but as of four solutions, viz., of tri-calcic silicate in tri-calcic aluminate, of tri-calcic aluminate in tri-calcic silicate, of di-calcic silicate in di-calcic aluminate and of di-calcic aluminate in di-calcic silicate. This is a complicated situation, but the globulites containing the excess of the optically active silicates can be differentiated from those in which the aluminate is in excess by their brighter interference in polarized light.

As the relative proportions of aluminate to silicate increase, 3($SiO_2$3CaO) 2($Al_2O_3$2CaO), a crystalline element appears again with a diminution in the amount of the eutectic. The crystals, which, theoretically, must be a solution of the silicate in the aluminate, as in the similar ratios of the previous series, possess some optical activity and can hardly be distinguished from alit in form or optical activity. In the ratio $SiO_2$3CaO, Al_2O_3 2CaO they are very well formed and the emulsion is still further reduced in amount, at the same time being segregated again as groups like celit and having the optical activity of the latter. In the clinker in which the ratio is 6($SiO_2$3CaO) 4($Al_2O_3$2CaO) the structure is again completely changed. The main part of the clinker is an emulsion with a segregation of cubical crystals of no optical activity. With continued decrease in the concentration as regards silicate the cubical crystals continue to increase and the optical activity to diminish. At the ratio $SiO_2$3CaO, 6 (Al_2O_3 2CaO) corresponding to the reverse ratio of that for Portland cement at the other end of the series, the aluminate is first found segregated in the form of dendritic crystals constituting the larger part of the clinker, the material of higher freezing point still retaining some optical activity. The proportion of crystalline aluminate continues to increase on still further dilution as regards silicate, but a slight amount of optical activity remains in some particles with only 2 per cent of silicate present.

In the alit series the alit form of the solution of tri-calcic aluminate in tri-calcic silicate persists through a wder range of concentration than in the two preceding series. The solutions present the low optical activity of alit and, in fact, consist of pure alit of different degrees of concentration. A eutectic is found in all concentrations that have been prepared beginning with the ratio 6 (SiO_2 3CaO) (Al_2O_3 3CaO) and the cubical crystals of aluminate only appear at dilutions as regards silicate much greater than in the previous series, 2($SiO_2$3CaO) 8($Al_2O_3$3CaO). Needle-shaped crystals are present, possibly a definite compound, at certain concentrations.

This series has yet to be studied closely, however, the only definite conclusions, at present, being that at least up to the ratio 3 (SiO_2 3CaO) (Al_2O_3 CaO) it contains pure alit of different degrees of concentration and that tri-basic silicate and aluminate of lime are more mutually soluble in each other than the di-basic salts or mixtures of the salts of different degrees of bascity.

Further studies of the three series of solutions will be presented in another place in greater detail.

It is evident that basic silicates and aluminates are miscible in all proportions at sufficiently high temperatures, that is to say, in a fused condition, but that they are not thus miscible in a state of solid solution. In the latter condition this results in the formation of heterogeneous solid solutions such as have been found in the various members of the series which have been described. What the solubility of the basic compounds of iron and lime is, as well as that of the magnesia salts, must be worked out later. It is known, however, that the iron compounds are not soluble to any extent in the basic lime silicates in the solid form. This, as will appear later, is an important fact in this connection, but it can hardly be taken up at this time with our limited knowledge of these solutions.

DIFFUSION.

Having determined that alit and celit are solid solutions of aluminates in silicates, the aluminates being present in less than an amount sufficient to make a saturated solution of aluminate in the silicate, it becomes of interest to consider how these solutions are formed during the conversion of a raw mixture or of a mixture of pure chemicals into a clinker. It would be simple to understand this if fusion took place in its formation, but this does not happen, the material is only sintered. If two gases are brought together they diffuse into each other with very great rapidity. If two liquids are poured one upon the other in layers without mixing, they diffuse more slowly. If solids are brought into contact it would be naturally assumed that diffusion would cease. Experiments of Roberts-Austen have shown that molecular mobility in solids exists, since when carefully polished surfaces of gold and lead are brought into contact and left under pressure for some months, at the ordinary temperatures, gold is diffused into the lead and the lead into the gold for an appreciable distance. Mixtures of the components which would produce a fusible Wood metal when subjected to pressure at ordinary temperature become converted into this alloy. Anhydrous sulphate of soda and carbonate of barium also diffuse when brought into close contact with the formation of barium sulphate and carbonate of soda. It is not difficult to understand, therefore, how at a temperature of 1,650 deg. C. the particles of silica, alumina, and lime may diffuse below the melting point of the resulting clinker to form a Portland cement, and the fact that such a clinker is stable depends not only on its composition, but upon the fact that the diffusion has been complete, even in material which is only sintered. Sintering, therefore, may be defined as diffusion at a temperature below the melting point of the components or of the resulting solid solution. That diffusion under such conditions is surprisingly rapid is seen by placing a particle of ferric oxide on the surface of white Portland cement clinker and then submitting it to a moderately high temperature. The rapid diffusion of iron through the white clinker can readily be noticed by the color which spreads through the mass. It is evident that the higher the temperature the more rapid the diffusion until it becomes very rapid on fusion. From this it may be concluded that the length of time during which it is necessary to expose any mixture of silica, alumina, and lime to a temperature is a function of the temperature, and should be longer, the lower the temperature.

SURFACE CONTACT AREA.

There is another condition, however, of great importance and one which affects the rate of diffusion, and that is the area of surface of the different constituents which come into contact. Most investigators up to the time of the Newberrys were unable to prepare tri-calcic silicate or di-calcic silicate in a stable form. The Newberrys showed that if the materials, silica and lime, were finely enough divided there was no difficulty in forming a stable tri-calcic silicate. The writer has shown the same to be true in the case of the formation of stable di-calcic silicate. This result is not unexpected if the law of physical chemistry known as Fick's Law is considered. From a study of salt solutions of different degrees of concentration Fick determined that the "amount of salt which diffuses through a given cross section is proportional to the difference in concentration of two cross sections lying infinitely near each other, or is proportional to the difference in cross section."* This law has been shown by several investigators to apply to metals, and, no doubt, applies to all solids. If the surface area of the different particles entering into the formation of a clinker are not, therefore, great enough to provide a sufficient area of intimate contact for diffusion to be complete at the available temperature and in the allotted time, so that the resulting clinker is homogeneous in constitution, the latter will not be in equilibrium or volume constant. This is a matter of great importance in the production of Portland cement, and together with the temperature and duration of heating makes up the conditions which must be carefully regulated in order to produce a stable clinker, that is to say, to bring about complete equilibrium without which a clinker on cooling will tend to rearrange its structure and dust.

OTHER CONDITIONS RESULTING IN LACK OF VOLUME CONSTANCY.

There are other conditions, however, which may cause instability in the solid solution. It can be readily imagined that as water in the case of many aqueous solutions may dissolve more of a salt at high temperatures, the silicate of lime may be able to dissolve more aluminate at high temperatures than will normally remain in solution in the cold. If such a solution is allowed to cool, the excess will separate out unless solidification is brought about so quickly as to prevent it. Should this take place the solid solution would be supersaturated, under appropriate conditions, and in a state of tension which would result in a tendency to return to equilibrium with a change of volume and breaking down of the clinker. It can be readily imagined that such a state of affairs can exist in industrial Portland cement clinkers which are rich in alumina. The conditions which aid in bringing about such a transformation are an increase in temperature, that is to say, the addition of heat. As in the case of hard steel, where heating it in different degrees according to the temperature attained results in withdrawing the temper, so we can imagine that heating alit, which has been likened to the austenite of steel, may alter its structure. As a matter of fact normal clinker when heated for some time to a red heat will not give a stable cement on grinding. Such a phenomenon as this may serve as an explanation of why cements change their rate of setting with age. A highly aluminous cement which, after plastering, will set in an hour, has often been found to set in three minutes when stored in closed freight cars for some time in a hot summer's sun. Here the temperature of transformation has probably been reached, the tension in the solid solution has been released, with the result that some aluminate is liberated which makes the cement quick-setting again.

FINE GRINDING OF RAW MATERIALS.

With cement of normal composition the finer the grinding of the raw mixture and the more thorough the burning, the more complete the diffusion will be and the less tendency there will be toward a tension in

* Elements of Physical Chemistry, Jones, p. 244.

the solid solution. As an illustration of this the ordinary Lehigh Valley clinker will set rapidly when first made. If the same raw mixture is ground to a much finer and more impalpable powder and burned in the laboratory, a clinker is obtained which yields a cement which does not set in less than one hour immediately after it is made. At the same time, such very fine raw material can be burned at a temperature more than 200 deg. F. below that required for the coarser industrial mixture, and if burned at the usual high temperature fuses quite readily. This leads to the conclusion that a proper balance between fineness of grinding and fuel expenditure must be arrived at in order to reach the greatest economy. In the United States, where fuel is cheap, a coarser raw mixture of the character of that found in the Lehigh Valley may be burned than would be the case in Germany, where, fuel being dear, it would be cheaper to go to a greater expense for finer grinding. On this basis we arrive at the theoretical conclusion that if a very finely ground mixture was submitted to a very low temperature, say 500 deg. below that usually employed, for a very long time, say several weeks, the result would be as satisfactory a clinker as that now produced. It is probable that with the dome kiln the greater length of time of burning which is necessary is due to the fact that diffusion goes on much more slowly than in the rotary kiln and at a lower temperature. It also explains why that portion of the clinker which has not attained as high a temperature as that in the hottest part of the kiln, dusts. Diffusion has not been complete and the solid solutions are not in equilibrium. The higher the percentage of lime the higher the temperature which is necessary to produce complete diffusion in combination with silica, and the greater the necessity for a large surface area of contact. The finer the grinding of the raw mixture the higher the percentage of lime which can be carried.

It appears from the preceding that a proper chemical composition is no guarantee whatever that the cement will be a satisfactory one, as in such a case the materials of which it is composed may not have attained thorough diffusion and not be in equilibrium.

The points which have been brought out by the investigations which have been just described are suggestive, but can only be considered as showing the possibilities of what can be done in the future. There is, of course, a very large field for further investigation, including more particularly the role which iron plays in the formation of clinker. There is no reason, however, why eventually the study of cements along physical lines should not afford as vast a fund of information as it has done in the case of iron and steel. It is the object of the writer to carry these investigations out on these lines in the future, and it would be a most appropriate thing if your Association should also contribute toward this work by establishing a laboratory or making a grant of funds to some committee of your body for the prosecution of such work. The United States might then lead in its scientific study of cement in the same way it now does in the character of its industrial product.

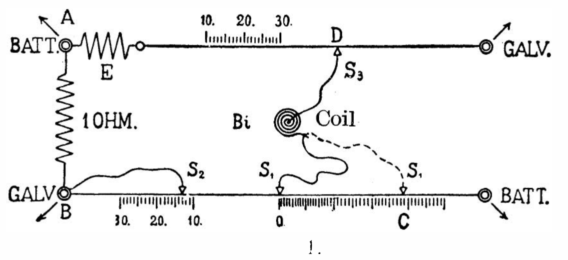
SIMPLE GERMAN INSTRUMENTS FOR TESTING THE MAGNETIC PROPERTIES OF IRON.*

THE accompanying diagram, Fig. 1, and illustration, Fig. 2, show the electrical connections and construction of a simple device for testing the magnetic properties of iron samples especially for determining the H. B. curve, while the drawing, Fig. 3, shows the connections of the Epstein apparatus for testing the magnetic properties of sheet iron, it being intended chiefly for the determination of the "loss factor." These instruments were constructed at Frankfort-on-the-Main, Germany, by the Hartman & Braun Actien-Gesellschaft. The instrument noted in Fig. 2 consists of a yoke to take the cylindrical test bar which is inserted in a magnetizing coil to generate the lines of force which pass vertically through a small bismuth spiral. The length of the interferrical space containing the bismuth coil is constant in all cases and less than 1 millimeter. The winding of the magnetizing coil is designed to produce strong fields with a relatively weak current.

The arrangement of a bridge noted in Fig. 1 is very suitable for testing magnetic fields. The resistances, BA (= 1 ohm) and E (= the resistance of the bismuth spiral at the lowest temperature), are connected to a slide wire with sliders, S_1 and S_2, and to a second slide wire and slider, S_3. The bismuth spiral is connected to S_1 and S_2, the galvanometer to "GALV" terminals and the battery to the "BATT" terminals. With zero lines of force passing through the spiral and S_1 on 0 of the scale, S_2 is moved to the reading corresponding to the temperature and the bridge balanced by adjusting S_3. The spiral is then introduced into the magnetic field under test and S_1 adjusted (S_2 and S_3 are not moved) until the balance is again attained. Then

$$O\,C = \frac{W_f - W_o}{W_o}$$

or the increased resistance Z of the spiral. The scale of the slide wire is marked to read this value direct



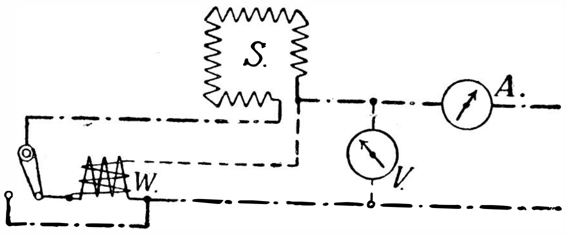


FIG. 3.

and, if the same spiral is always used, can be marked to read in lines of force.

Measurements of the intensity of a magnetic field by the Lenard bismuth spiral are due to the alternation of resistance which this metal exhibits when placed in a magnetic field. A thin insulated wire of chemically pure bismuth is bifilar-wound in a flat spiral form; the ends of this winding are soldered to flat copper strips which are held together in an ebonite handle and fitted with terminals. The spiral is shielded from damage by mica disks cemented on. It is only about 1 millimeter thick and can therefore be introduced into very narrow fields, for instance between the armature and poles of dynamo. The alternation in resistance varies with the lines of force in the field under test, a 5 per cent variation in resistance corresponding practically to about 1,000 lines of force. To insure greater accuracy a calibration curve is supplied with each instrument. The readings vary with the temperature and, in accurate work, the instrument should be used at the temperature of calibration; the coil must therefore not be heated by the current employed, which should not exceed a few milliamperes on a momentarily closed circuit.

The complete set of German apparatus for testing the magnetic properties of iron noted in Fig. 2 is especially suitable for use where quick and accurate tests are repeatedly required without having to assemble the various instruments which are possibly available for exact resistance measurements, and is well adapted for use in iron work. It consists of electromagnet with bismuth spiral as well as a main circuit switch and an ammeter unaffected by external magnetic fields to measure the exciting current of the electromagnet. Also it is provided with a galvanometer and successive key for the measuring circuit and a bridge arranged as noted in diagram, Fig. 1, reading in lines of force direct.

The principle of measurements on which the Epstein apparatus was designed for testing the magnetic properties of sheet iron has been taken by the Institution of German Electrical Engineers as the standard for testing sheet iron.

The apparatus consists chiefly of four coils to receive the test bundles of sheet iron which are arranged at right angles on an adjustable oak cross and clamped closely together by an arrangement worked by a key from the center of the cross to afford a practically closed magnetic circuit. Alternating current at 50 periods is used in these coils, and the pressure adjusted to correspond with a maximum induction in the test sheets of B max. = 10,000 (in machines with a sine wave the pressure in this case = 13.2 × q, where q = the section of the sheet-iron bundle in square centimeters). The net iron loss (= total watts $-J^2W = \frac{E}{W}$) is then determined by simultaneous readings of watts and current. In the above equation J = current measured, E = pressure, W = resistance of wattmeter leads and the apparatus coils, and w = the resistance of the wattmeter pressure coil. $\frac{E^2}{W}$ therefore represents the, for this purpose, assumed constant wattmeter correction. From this net iron loss the "loss factor" or the loss in watts per kilogramme of iron can be calculated. The instrument weighs about 25.0 kilogrammes and in addition to the requisite machine arrangements (a motor-transformer for direct to alternating current, with starting switch and speed control, and the coil system with adjustable wood cross and clamping arrangement), the following electrical apparatus are requisite: A portable standard wattmeter, with two ranges for current and pressure, for 2 and 4 amperes, 60 and 120 volts; a portable hot-wire ammeter, reading to 5 amperes; and a portable hot-wire voltmeter for 60 and 120 volts.

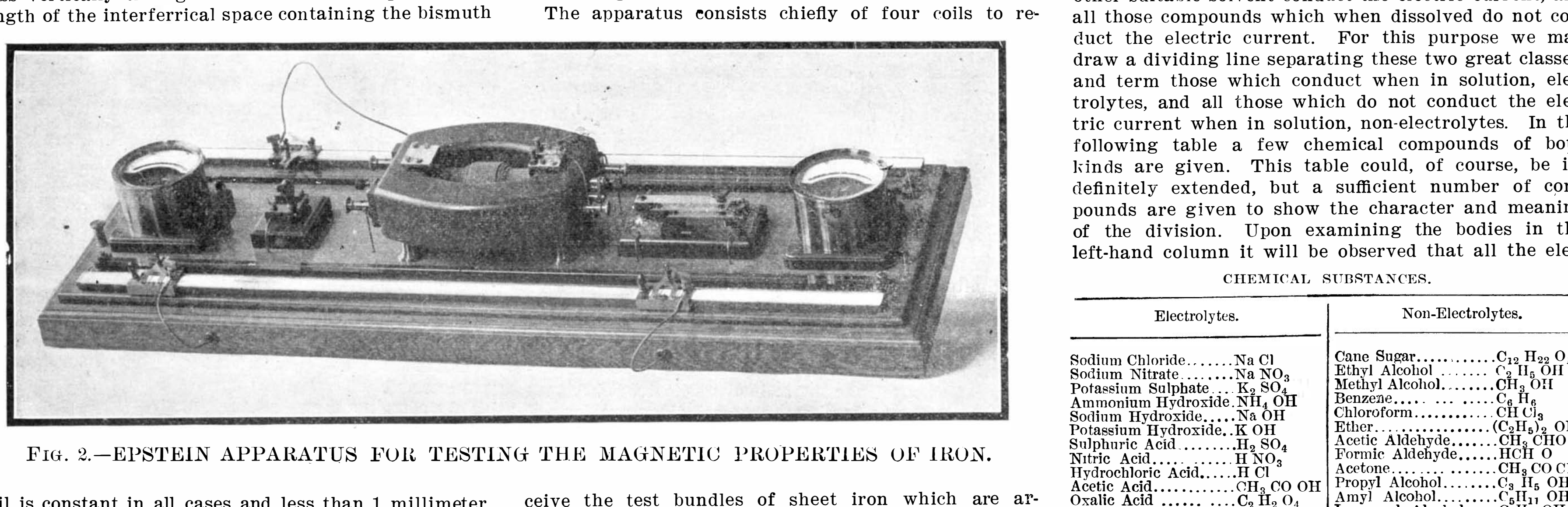


FIG. 2.—EPSTEIN APPARATUS FOR TESTING THE MAGNETIC PROPERTIES OF IRON.

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

[Continued from SUPPLEMENT No. 1509, page 24178.]

EXPERIMENTAL ELECTROCHEMISTRY.*

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SECOND PAPER.

The Theory of Electrolytic Dissociation.

IN the last paper, that beautiful doctrine known generally as the theory of electrolytic dissociation was touched upon, and the reader was asked to accept upon faith the truth of its meaning for the time being. It is the purpose of the present chapter to advance in the clearest possible manner some of the best experimental evidence in its support, and leave the student to formulate his own opinions. Probably there is no generalization in the entire domain of physical chemistry quite so unique and attractive as the theory advanced as recently as 1887 by Svante Arrenius, now professor at the University of Stockholm. Few theories in either chemical or physical science have been the subject of greater dissertation, dispute, or attack than the dissociation theory, and few have served a more useful purpose in accounting for certain vital phenomena. Although the theory of electrolytic dissociation has the most excellent experimental evidence in its favor, and accounts perfectly for many heretofore unexplained facts, those urging objections to its truth have never been able to propose a better one, or even one half as good. It will be the effort of the present writer to advance what he considers to be the best and most forcible evidence for this doctrine, and adopt it throughout in the practical electrochemical studies which are to follow. Until something better is brought forward, we will not waste our time in making attacks upon the doctrine in view of certain arguments against it. The arguments against the theory will not be introduced at this time for fear of confusing the student. We know from previous experience that we have, broadly speaking, two kinds of conductors of the electric current—the metals and alloys on the one hand, and solutions of certain chemical substances on the other. In the case of the metals and alloys, they are called conductors of the first class, and in the case of chemical substances in solution or in a state of fusion, they are called conductors of the second class. The passage of an electric current through a conductor of the second class is believed to be accompanied by the actual movement of ponderable material, or a mechanical transfer of matter. Good evidence in support of this will be introduced a little later.

We may now take all known chemical compounds and divide them into great groups as follows: All those compounds which when dissolved in water or other suitable solvent conduct the electric current, and all those compounds which when dissolved do not conduct the electric current. For this purpose we may draw a dividing line separating these two great classes, and term those which conduct when in solution, electrolytes, and all those which do not conduct the electric current when in solution, non-electrolytes. In the following table a few chemical compounds of both kinds are given. This table could, of course, be indefinitely extended, but a sufficient number of compounds are given to show the character and meaning of the division. Upon examining the bodies in the left-hand column it will be observed that all the electrolytes are among, and constitute, the "chemically active" bodies, whereas the non-electrolytes constitute the "chemically inactive" bodies. It will be noticed that certain chemical substances are electrolytes only when dissolved in water or other suitable solvent, or when in the fused condition, according to the definition

CHEMICAL SUBSTANCES.

Electrolytes.		Non-Electrolytes.	
Sodium Chloride	$Na\,Cl$	Cane Sugar	$C_{12}\,H_{22}\,O_{11}$
Sodium Nitrate	$Na\,NO_3$	Ethyl Alcohol	$C_2\,H_5\,OH$
Potassium Sulphate	$K_2\,SO_4$	Methyl Alcohol	$CH_3\,OH$
Ammonium Hydroxide	$NH_4\,OH$	Benzene	$C_6\,H_6$
Sodium Hydroxide	$Na\,OH$	Chloroform	$CH\,Cl_3$
Potassium Hydroxide	$K\,OH$	Ether	$(C_2H_5)_2\,OH$
Sulphuric Acid	$H_2\,SO_4$	Acetic Aldehyde	$CH_3\,CHO$
Nitric Acid	$H\,NO_3$	Formic Aldehyde	$HCH\,O$
Hydrochloric Acid	$H\,Cl$	Acetone	$CH_3\,CO\,CH_3$
Acetic Acid	$CH_3\,CO\,OH$	Propyl Alcohol	$C_3\,H_5\,OH$
Oxalic Acid	$C_2\,H_2\,O_4$	Amyl Alcohol	$C_5H_{11}\,OH$
Silver Nitrate	$Ag\,NO_3$	Isopropyl Alcohol	$C_3H_7\,OH$

* Specially prepared for the SCIENTIFIC AMERICAN SUPPLEMENT.

of an electrolyte. We may take any of the chemical compounds in the left-hand column, including even the acids, and when absolutely water-free they are non-conductors of the electric current. Water itself, when properly distilled and air-free, is also a non-electrolyte (except to an infinitesimal extent) and yet when certain chemical substances are dissolved in water they become the most excellent conductors of electricity. See Fig. 1 for a simple experiment for showing electrolytes from non-electrolytes. The lamp bank and electric lighting circuit, or the motor generator as described in the first chapter, may, of course, be used instead of the storage battery as given here. Here we may have the case where two bodies, when separated, each prove to be non-conductors, and when brought together, to conduct highly the electric current. What is it due to? To take a special case, a crystal of rock

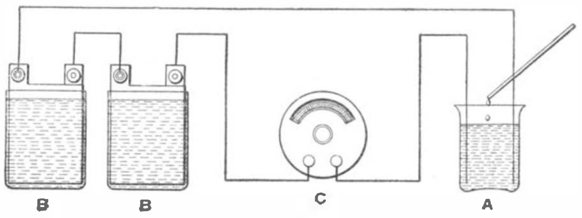


Fig. 1.—Simple Apparatus for Distinguishing Electrolytes from Non-Electrolytes. A. Glass Beaker Containing Distilled Water with Platinum Electrodes into which the Compound to be Tested is Dropped. C. Milli-Amperemeter. B, B. Cells of Storage Battery. There is Always a Slight Indication of Conductivity upon a Sensitive Milli-Amperemeter when Only the Glass Beaker and Distilled Water are Present, Due to the Dissolving of a Minute Trace of Glass Together with Impurities in the Distilled Water. The Amount of Deflection can be Noted and Applied as a Correction.

salt (sodium chloride) and carefully distilled water. Neither of these substances has the power to conduct the electric current to any appreciable extent. Dissolve the salt in the water and we have high conductivity. Something must have taken place within the water, and yet we know we have only made a simple solution of salt in water, which when evaporated to dryness, gives us back our salt unaltered, and if we catch and condense the water driven off, we have ordinary distilled water again. What is the condition of the salt in the water then to so greatly change its physical behavior toward the electric current? In terms of the theory of electrolytic dissociation, as was pointed out in the previous chapter, the chemical molecule is broken up into "ions" or ultimate parts, and these ultimate parts bear electrical charges upon them. The molecular formula of sodium chloride is represented simply thus, NaCl, so familiar to all general chemists. Now upon immersion in water the molecule is believed to be broken up as follows into two "ions," $\overset{+}{Na}$ and $\overset{-}{Cl}$, the bond or valence between the two former atoms being broken, and the sodium ion with its electrical charge is existing independently of the chlorine ion with its electrical charge of unlike polarity or sign. The mere act of passing into solution is believed, in terms of our theory, to separate the atoms of certain molecules, the atoms becoming ions at once by taking upon themselves electrical charges of opposite sign, the one becoming a positive ion, the other a negative ion. It was pointed out by means of a small diagram in the previous chapter, how the positive ion traveled to the negative electrode, and how the negative ion traveled toward the positive electrode. Now it is the purpose of this chapter to show that we have excellent reasons for believing in the existence of these little electrically-charged particles when certain chemical substances are dissolved in water. We will take up the study of the evidence first by comparing the deportment of substances in aqueous solution to substances in the state of a gas, and for this purpose we will first set down the three familiar gas laws so well known to all students of later chemistry and physics.

Law of Boyle.

The pressure exerted by a gas, the temperature remaining the same, is proportional to the concentration of the gas. The concentration of the gas is directly proportional to the number of molecules or ultimate parts of molecules present.

Law of Gay-Lussac.

The presure of a gas increases a constant amount for every increase in temperature of 1 deg., and the increase in pressure is equal to 1-273 of the original pressure of the gas at 0 deg. Centigrade.

Law of Avogadro.

Equal volumes of all substances in the gaseous state, under the same conditions of temperature and pressure, contain the same number of molecules or ultimate parts of molecules.

Consequently

The molecules of all substances, or the ultimate parts of all molecules when in the gaseous state, under the same conditions of temperature and pressure, *occupy the same space.*

Having the three fundamental gas laws before us, we will take them up separately in the order given, and learn what bearing they have upon the behavior of substances in solution. What possible application can these gas laws have to chemical compounds dissolved in water? There appears to the general student to be no connection whatever, and yet there is the most vital application of the gas laws in support of the theory of electrolytic dissociation. Let us first take up Boyle's law, which has to do with the pressure exerted by substances in the state of a gas. This tells us facts based upon experimentally determining the pressures exerted by gases of different concentrations. The pressures of gases confined in given volumes at constant temperature can be readily measured by manometers or pressure gages, as set forth in detail in any good text book on physics. This we know; but can we measure the pressures exerted by substances in solution? We can convert a given mass of certain substances into a gas, by heating, and measure the pressure at different concentrations, or volumes, by means of suitable manometers. Can we dissolve the same quantity of the compound in water, and will the molecules exert a pressure in the dissolved condition, and can we measure it? Both these questions can be answered in the affirmative, and it is the purpose of the author to show that such pressures exist, and to describe how they may be measured. All substances when dissolved in water exert a pressure, and this pressure has been termed "osmotic pressure."

Osmotic Pressure and Method of Measuring It.

If a gas, oxygen or hydrogen for example, be liberated in a given space, the gas will expand in all directions and completely fill the containing vessel. If all parts of this containing vessel are at the same temperature, the gas will expand and distribute itself uniformly throughout the volume. There will be repellent forces between the molecules of the gas driving them to the remotest recesses of the containing vessel, and there will be consequently a pressure against the walls of the same. The more concentrated the gas, or in other words, the greater the number of molecules present or ultimate parts of molecules, the greater will be the pressure within the fixed or given volume. What can be said about substances in solution? The behavior is the same. Let us take a large vessel of water, for example a tall glass jar full, and introduce a little mass of sugar. The sugar will immediately fall to the bottom, a small portion dissolving and passing into solution on the way down. What will be the ultimate result on standing? The sugar at the bottom will all pass into solution, rise against gravity, and in time distribute itself uniformly throughout the solvent. The sugar in the dissolved state will behave exactly as it would when in the state of a gas, and will exert a pressure when in solution which may be measured. This is due to the phenomenon of diffusion, and, not so many years ago, was wholly unaccounted for. Here we may have some heavy substance dissolving at the bottom of a tall glass cylinder filled with water, and rising to the top against the attraction of gravity. There is a pressure and this pressure has only recently been accounted for. Another phenomenon which until recently could not be explained, was the bursting of an animal bladder when filled with a mixture of alcohol and water, and immersed in a vessel containing pure water. The bladder under these conditions, if it has been closed up properly at the openings, will be burst by a gradually developed pressure within. It is easy to show in this way that we have a pressure, and this pressure has been termed osmotic pressure. This is only a very crude method of showing qualitatively that we have a positive pressure, and it never occurred to the earliest workers that this pressure was a definite thing and could be quantitatively measured. This osmotic pressure is a very peculiar thing, when one considers the manner in which the pressure is measured. We can not place a solution within a closed vessel and get an indication of pressure upon a gage glass or manometer, as we very well know, but must resort to some kind of a membrane, as exists in the animal bladder. Strange to say, the pressure depends upon a differential, or selective ac-

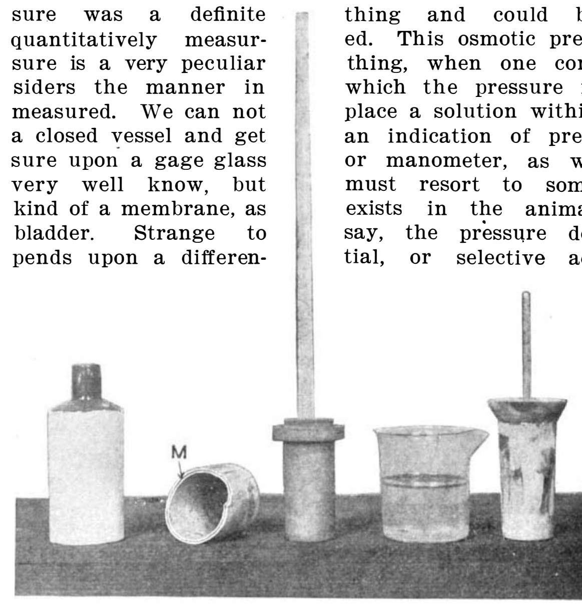


Fig. 2.—Some Forms of Porous Pots with Semi-Permeable Membranes for the Measurement of "Osmotic Pressure." The Broken Exhibit Shows the Semi-Permeable Membrane at M.

tion, so to speak, of the necessary membrane. It must allow the solvent to pass through, but not the dissolved substance, a sort of filter, roughly speaking, and because of this principle, the membrane has been called "semi-permeable." Now if we can really produce a semi-permeable membrane or diaphragm, we will be able to measure the pressure due to substances in solution. Take, for example, a solution of cane sugar. If we have at hand a membrane which will be permeable to water and impermeable to sugar, we can ascertain the pressure due to the presence of the sugar molecules, and demonstrate how this pressure varies with concentration of the solution, and with changes in temperature. We have at hand, in other words, means for comparing the behavior of gaseous molecules, with the behavior of molecules in solution. Let us prepare such a semi-permeable membrane in the laboratory, and examine some of the compounds given in the preceding table constituting electrolytes on one hand and non-electrolytes on the other. The accompanying photograph illustrates some simple forms of

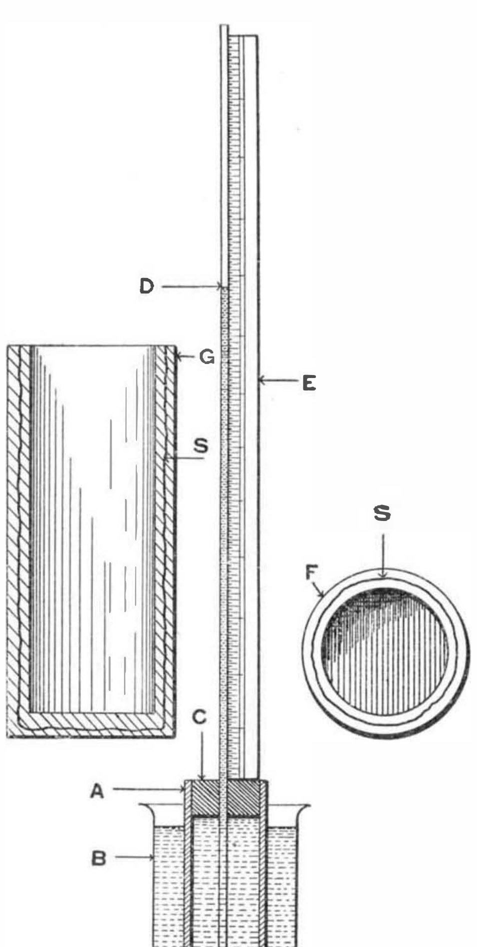


Fig. 3.—A. Porous Pot. B. Glass Beaker. C. Tight Fitting Stopper. D. Height to which the Contained Solution has Risen. E. Graduated Scale. F. Transverse Section of Porous Pot. S. Semi-Permeable Membrane Within the Wall of the Pot. G. Enlarged Vertical Section Through Porous Pot with Semi-Permeable Membrane Showing at S.

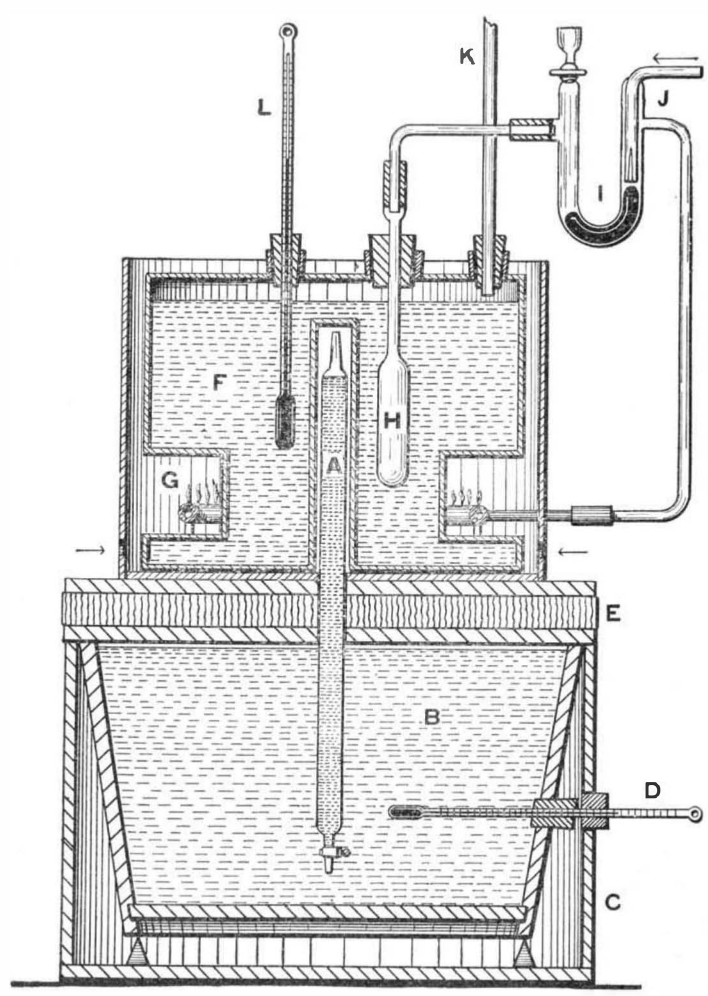


Fig. 4.—The "Principle of Soret" (Author's Apparatus). A. Vertical Glass Tube Containing the Substance. B. Water in Wooden Tub. C. Outer Wood Casing to be Filled in Between with Charcoal or Sawdust. D. Thermometer. E. Heavy Layer of Hair Felt Between Thick Boards Supporting Top of Apparatus. F. Water in Copper Heater. G. Ring Gas Burner for Heating Water. H. Bulb of Air Thermostat for Holding Temperature of Water Constant. I. Mercury of Air Thermostat for Cutting Off Gas Supply, J, if the Temperature Rises too High. K. Long Vertical Glass Condensing Tube to Prevent Loss of Water, F, by Distillation. L. Thermometer for Observing Temperature.

porous pots, and Fig. 3 gives a section through such a typical pot, as well as a completed piece of apparatus for experimentally measuring osmotic pressure. Moritz Traube, and Pfeffer, the celebrated plant physiologist, were the first to discover and make use of the properties of semi-permeable membranes. It is to Pfeffer, however, that we owe the first really serviceable artificial semi-permeable diaphragm or partition. It was discovered by Pfeffer that plant and animal membranes could be discarded, and that certain chemical precipitates, when properly supported, met the requirements almost perfectly. Copper ferrocyanide was found to give the most satisfactory results when formed right in the walls of a very fine-grained porous pot of unglazed finish. In order to produce such a precipitate within the walls of the porous pot, it was filled with a solution of potassium ferrocyanide, and immersed in a solution of copper sulphate. The two solutions meet within the walls, and there form the semi-permeable membrane with the resistant support of the porous pot. When such a prepared pot is broken open, the membrane appears in the form of a fine line, as indicated in Fig. 2 at *M*. There are many necessary precautions to be taken in the preparation of successful semi-permeable membranes, it being an art requiring not a little patience and skill. The following is taken from one of Pfeffer's writings on the subject: "The porcelain cells were first completely injected with water under the air pump, and then placed for at least some hours in a solution containing at least 3 per cent of copper sulphate, and the interior was also filled with this solution. The interior only of the porcelain cell was then rinsed out quickly with water, well dried as rapidly as possible by introducing strips of filter paper, and after the outside had dried off, it was allowed to stand some time in the air until it just felt moist. Then a 3 per cent solution of potassium ferrocyanide was poured into the cell, and this immediately reintroduced into the solution of copper sulphate. After the cell had stood undisturbed for from twenty-four to forty-eight hours, it was completely filled with the solution of potassium ferrocyanide and closed. . . . A certain excess of pressure of the contents of the cell now gradually manifested itself, since the solution of potassium ferrocyanide had a greater osmotic pressure than the solution of copper sulphate. After another twenty-four to forty-eight hours the apparatus was again opened, and generally a solution introduced which contained 3 per cent of potassium ferrocyanide, and 1½ per cent of potassium nitrate (by weight), and which showed an excess of osmotic pressure of somewhat more than three atmospheres."

In all this work as reproduced by the present writer it was found most essential to obtain a special close-grained grade of porous cup or pot. A common porous pot, or one the least faulty, such as containing minute invisible fissures, will defeat the object of the entire experiment. With faulty pots the writer has frequently had a completed piece of apparatus assembled, indicating a height of only 2 or 3 feet of the contained solution, when the semi-permeable membrane gave way, and oozed through the side of the porous pot. It has been found that a dilute solution of cane sugar in water would rise to a height of 66 feet. Referring once more to Fig. 3, we will note a rise of about 2 feet, a one-half normal sugar solution being used in this case. As strange as it may seem, the sugar solution is placed within the porous pot *A* and the pot is in turn immersed in the beaker *B*, containing distilled water. The pressure is developed within the porous pot by a very curious action, forcing the liquid up into the manometer tube. The conditions are as follows: The semi-permeable membrane will allow water to pass through freely, but will not allow the sugar molecules to pass. Within we have sugar molecules and water molecules attempting to get out, and outside we have all water molecules attempting to get in, as it were. Now per unit area of the porous pot and semi-permeable membrane, we have a more effective bombardment from the pure water molecules without, than we have from within with the mixed molecules. We may think about the thing also as follows: Every water molecule striking the diaphragm from the outside gets in, but many of the water molecules before striking the diaphragm from the inside, collide with sugar molecules, which cannot get through, and thereby their effectiveness is lost. As a result of such a differential action, we have a slow ingress of water molecules attempting to dilute the sugar by driving the molecules farther apart, and thereby establishing a pressure. Let us leave the theory of the apparatus now, and look at the facts in some actual and carefully-conducted experiments.

Osmotic Pressure of Non-Electrolytes and Electrolytes.

For the sake of simplicity, we will record the result of an osmotic-pressure determination upon a non-electrolyte. For this purpose we will choose the first non-electrolyte appearing at the top of the column in the little table already given. This is ordinary cane sugar, a solution of which in water does not conduct the electric current. The following table shows the result of one of Pfeffer's carefully conducted determinations upon this substance:

Cane Sugar $C_{12}H_{22}O_{11}$.

Concentration in per cent by weight.	Osmotic Pressure in Millimeters of Mercury.
1	535
2	1,016
4	2,082
6	3,075

Let us now examine the figures standing for osmotic pressures, and interpret their meaning.

Concentration C.	Pressure P.	P. / C.
1 per cent	535	535
2 per cent	1,016	508
4 per cent	2,082	521
6 per cent	3,075	513

In the above table the pressure in each case has been divided by the concentration with practically a constant resulting. What little discrepancy exists is due to experimental error. Here we have an analogy with the law of Boyle as applied to gases. We know that the pressure of a gas increases with its concentration in a direct proportion, and we see from the above tabulated data, that the osmotic pressure of a solution increases directly with the concentration. In experimental work of this character there are naturally sources of error which must be expected. For example when we start with a 1 per cent sugar solution, and begin to measure its osmotic pressure by such a piece of apparatus as described, the solution is weakened by the inflow of water, and unless the manometer tube is very small, the volume of sugar solution rising to make the indication will constitute a high percentage of the entire volume in the porous cup. Pfeffer also showed, at the instigation of Van't Hoff, that the osmotic pressure of solutions increases slowly with rise in temperature, and that this pressure is analogous to the increasing pressure of a gas as set forth in the law of Gay-Lussac. Here a solution of sugar was taken again, but instead of varying its percentage strength, the temperature of the solution was gradually increased. For this purpose a one-tenth normal sugar solution was employed (a normal solution of cane sugar is made by dissolving a gramme-molecular weight of this compound in a liter of water. A one-tenth normal solution is made by dissolving the gramme-molecular weight in ten liters of water), gradually increasing its temperature. The accompanying table shows the facts of an actual experiment.

Temperature.	Osmotic Pressure in Cm. of Mercury. Experimental.	Calculated from Gas Law of Gay-Lussac.
6.8 deg.	50.5	50.5
13.5 deg.	52.1	51.7
14.2 deg.	53.1	51.8
22.0 deg.	54.8	53.2

While there are slight discrepancies due to experimental error, the striking application of Gay-Lussac's law to substances in solution is to be noted. Although Pfeffer was the first to successfully measure osmotic pressures, it remained for Van't Hoff to bring out the striking comparisons with the gas laws already set forth. Having observed the strong tendency of solutions to behave like substances in the state of a gas, by experimenting with semi-permeable membranes, this great Dutch scientist investigated other possibilities for showing analogies, among them what is known in physical chemistry as the "principle of Soret."

The Principle of Soret.

If a vertical glass tube is filled with a solution of a chemical compound, copper sulphate in water, for example, and the two ends of the tube are kept at different temperatures, the copper sulphate will eventually become more dilute where the temperature is highest, and more dense where the temperature is lowest. This distribution of the dissolved molecules by diffusion due to differences in temperature is known as the principle of Soret. The apparatus shown in Fig. 4 is the design of the present writer for bringing about such concentration changes. The tubes filled with various solutions were allowed to stand for a long time with the top and bottom at different temperatures, when some of the solution was allowed to run out from the bottom and analyzed for density, and some of the solution drawn out from the top by means of a pipette, and also analyzed for density. The early experiments showed smaller differences in concentration than would be called for if Gay-Lussac's law applied to the temperature coefficient of the osmotic pressure of solutions. The tubes were then allowed to stand for longer periods of time, with the result that the figures obtained approached closer and closer to the value expected from the law pertaining to gases. Diffusion of molecules takes place very slowly and the tubes had to stand for many weeks undisturbed before an equilibrium was finally established. In one experiment where the tubes stood for about twelve weeks, a copper sulphate solution gave the following results:

Upper end of tube 80 deg. C.; lower end of tube 20 deg. C. The difference in density between the respective ends of the tube upon analysis was found to be 14.03 per cent. The difference in density calculated from the law of Gay-Lussac is 14.3 per cent. Another experiment gave 24.87 per cent change in concentration, and according to Gay-Lussac's law the figure should have been 24.8 per cent.

Here the application of Gay-Lussac's law to the behavior of compounds in solution is very striking. With the apparatus as illustrated, the large mass of water in the wooden tub, together with a laboratory kept at practically constant temperature, the lower end of the tube is consequently very uniformly maintained in temperature. The upper end of the tube is kept at an elevated temperature by means of the ring gas burner, and the sensitive air-bulb thermostat. The expansion of the air contained in the air-bulb *H* forces the mercury at *I* up to the tube *J* which is slotted, and gradually cuts off the supply of gas which enters as indicated by the small arrow. Should the temperature of the water *F* fall below the required temperature, the air in the bulb *H* will contract and allow the mercury to fall away from the tube *J*, thus uncovering the slot and allowing more gas to flow to the burner. Two very sensitive thermometers give the readings for the top and bottom of the tube respectively. The apparatus is so designed that the top portion may be lifted off, when the tube containing the solution experimented upon may be readily removed. There remains now to compare the behavior of substances in solution with the third and last gas law, namely, that of Avogadro. This was also done by the chemist Van't Hoff. He worked again with a solution of cane sugar, and compared the osmotic pressure of such a solution with a volume of hydrogen gas of equal concentration. For this purpose he made a cane sugar solution having the same number of sugar molecules in a given volume of solution as there are hydrogen molecules in the same volume of the gas. The experiment fully justified the statement that the sugar solution gave an osmotic pressure equal to the gas pressure. *We may then say that equal volumes of all chemical compounds in solution, giving the same osmotic pressure at the same temperature, contain the same number of molecules or ultimate parts of molecules.* Now this is only true for dilute solutions. Very concentrated solutions of chemical substances do not obey the law, and when we look about we are struck by the fact that very densely compressed gases do not obey the law of Boyle. This makes our comparisons all the more brilliant, for where we have exceptions in the case of gases, we also have exceptions in the case of solutions. It has now been shown that the three fundamental gas laws apply to compounds in a state of solution, but what has this, although striking and of vital interest to the physical chemist, to do with the theory of electrolytic dissociation? To answer this let us turn once more to the first table of this chapter, where we have electrolytes on the one hand and non-electrolytes on the other. It was pointed out that all those bodies have been classified according to their ability to conduct the electric current. All those on the left conduct when in solution, and all those on the right do not. All those on the left are called electrolytes, and in terms of the theory of electrolytic dissociation, the molecules break up into ions, each ion of course being an ultimate part of a molecule. Now as a matter of fact, only the non-electrolytes, when dissolved in water, obey the gas laws. It is only the non-electrolytes which give an osmotic pressure comparable with substances in the state of a gas, the electrolytes all giving an abnormally high osmotic pressure. This is just what we would expect if one molecule breaks up into two ultimate parts, and each ultimate part occupies the same space as the original molecule. Our sugar molecule does not conduct the electric current when in solution, it does not break up into ions, and gives as evidence a normal osmotic pressure. Our sodium chloride, or common salt, does conduct the electric current when dissolved, and gives an abnormally high osmotic pressure. Of course in comparing the osmotic pressure of sugar with sodium chloride, two solutions are made in which the same number of molecules are dissolved in each case. In order to accomplish this the gramme-molecular weight of each compound is taken. By gramme-molecular weight of a compound, we mean the molecular weight of the substance expressed in grammes. For example, the gramme-molecular weight of sodium chloride is 58.5 grammes, 58.5 being the molecular weight of sodium chloride. So much for the theory of electrolytic dissociation and the gas laws, and the evidence the measurement of osmotic pressure gives us in favor of ionization. We will take up additional evidence in support of the theory of electrolytic dissociation in our next chapter.

(To be continued.)

THE N-RAYS: ARGUMENTS AGAINST THEIR EXISTENCE.*

By PROF. R. W. WOOD.

THE inability of a large number of skilled experimental physicists to obtain any evidence whatever of the existence of the N-rays, and the continued publication of papers announcing new and still more remarkable properties of the rays, prompted me to pay a visit to one of the laboratories in which the apparently peculiar conditions necessary for the manifestation of this most elusive form of radiation appear to exist. I went, I must confess, in a doubting frame of mind, but with the hope that I might be convinced of the reality of the phenomena, the accounts of which have been read with so much skepticism.

After spending three hours or more in witnessing various experiments, I am not only unable to report a single observation which appeared to indicate the existence of the rays, but left with a very firm conviction that the few experimenters who have obtained positive results have been in some way deluded.

A somewhat detailed report of the experiments which were shown to me, together with my own observations, may be of interest to the many physicists who have spent days and weeks in fruitless efforts to repeat the remarkable experiments which have been described in the scientific journals of the past year.

The first experiment which it was my privilege to witness was the supposed brightening of a small electric spark when the N-rays were concentrated on it by means of an aluminium lens. The spark was placed behind a small screen of ground glass to diffuse the

* Reprinted from Nature.

light, the luminosity of which was supposed to change when the hand was interposed between the spark and the source of the N-rays.

It was claimed that this was most distinctly noticeable, yet I was unable to detect the slightest change. This was explained as due to a lack of sensitiveness of my eyes, and to test the matter I suggested that the attempt be made to announce the exact moments *at which I introduced my hand* into the path of the rays, by observing the screen. In no case was a correct answer given, the screen being announced as bright and dark in alternation when my hand was held motionless in the path of the rays, while the fluctuations observed when I moved my hand bore no relation whatever to its movements.

I was shown a number of photographs which showed the brightening of the image, and a plate was exposed in my presence, but they were made, it seems to me, under conditions which admit of many sources of error. In the first place, the brilliancy of the spark fluctuates all the time by an amount which I estimated at 25 per cent, which alone would make accurate work impossible.

Secondly, the two images (with N-rays and without) are built of "instalment exposures" of five seconds each, the plate holder being shifted back and forth by hand every five seconds. It appears to me that it is quite possible that the difference in the brilliancy of the images is due to a cumulative favoring of the exposure of one of the images, which may be quite unconscious, but may be governed by the previous knowledge of the disposition of the apparatus. The claim is made that all accidents of this nature are made impossible by changing the conditions, i. e., by shifting the positions of the screens; but it must be remembered that the experimenter is aware of the change, and may be unconsciously influenced to hold the plate holder a fraction of a second longer on one side than on the other. I feel very sure that if a series of experiments were made jointly in this laboratory by the originator of the photographic experiments and Profs. Rubens and Lummer, whose failure to repeat them is well known, the source of the error would be found.

I was next shown the experiment of the deviation of the rays by an aluminium prism. The aluminium lens was removed, and a screen of wet cardboard furnished with a vertical slit about 3 millimeters wide put in its place. In front of the slit stood the prism, which was supposed not only to bend the sheet of rays, but to spread it out into a spectrum. The positions of the deviated rays were located by a narrow vertical line of phosphorescent paint, perhaps 0.5 millimeter wide, on a piece of dry cardboard, which was moved along by means of a small dividing engine. It was claimed that a movement of the screw corresponding to a motion of less than 0.1 of a millimeter was sufficient to cause the phosphorescent line to change in luminosity when it was moved across the N-ray spectrum, and this with a slit 2 or 3 millimeters wide. I expressed surprise that a ray bundle 3 millimeters in width could be split up into a spectrum with maxima and minima less than 0.1 of a millimeter apart, and was told that this was one of the inexplicable and astounding properties of the rays. I was unable to see any change whatever in the brilliancy of the phosphorescent line as I moved it along, and I subsequently found that the removal of the prism (we were in a dark room) did not seem to interfere in any way with the location of the maxima and minima in the deviated (!) ray bundle.

I then suggested that an attempt be made to determine by means of the phosphorescent screen whether I had placed the prism with its refracting edge to the right or the left, but neither the experimenter nor his assistant determined the position correctly in a single case (three trials were made). This failure was attributed to fatigue.

I was next shown an experiment of a different nature. A small screen on which a number of circles had been painted with luminous paint was placed on the table in the dark room. The approach of a large steel file was supposed to alter the appearance of the spots, causing them to appear more distinct and less nebulous. I could see no change myself, though the phenomenon was described as open to no question, the change being *very* marked. Holding the file behind my back, I removed my arm slightly toward and away from the screen. The same changes were described by my colleague. A clock face in a dimly-lighted room was believed to become much more distinct and brighter when the file was held before the eyes, owing to some peculiar effect which the rays emitted by the file exerted on the retina. I was unable to see the slightest change, though my colleague said that he could see the hands distinctly when he held the file near his eyes, while they were quite invisible when the file was removed. The room was dimly lighted by a gas jet turned down low, which made blank experiments impossible. My colleague could see the change just as well when I held the file before his face, and the substitution of a piece of wood of the same size and shape as the file in no way interfered with the experiment. The substitution was of course unknown to the observer.

I am obliged to confess that I left the laboratory with a distinct feeling of depression, not only having failed to see a single experiment of a convincing nature, but with the almost certain conviction that all the changes in the luminosity or distinctness of sparks and phosphorescent screens (which furnish the only evidence of N-rays) are purely imaginary. It seems strange that after a year's work on the subject not a single experiment has been devised which can in any way convince a critical observer that the rays exist at all. To be sure the photographs are offered as an objective proof of the effect of the rays upon the luminosity of the spark. The spark, however, varies greatly in intensity from moment to moment, and the manner in which the exposures are made appears to me to be especially favorable to the introduction of errors in the total time of exposure which each image receives. I am unwilling also to believe that a change of intensity which the average eye cannot detect when the N-rays are flashed "on" and "off" will be brought out as distinctly in photographs as is the case on the plates exhibited.

Experiments could be easily devised which would settle the matter beyond all doubt; for example, the following: Let two screens be prepared, one composed of two sheets of thin aluminium with a few sheets of wet paper between, the whole hermetically sealed with wax along the edges. The other screen to be exactly similar, containing, however, dry paper.

Let a dozen or more photographs be taken with the two screens, the person exposing the plates being ignorant of which screen was used in each case. One of the screens being opaque to the N-rays, the other transparent, the resulting photographs would tell the story. Two observers would be required, one to change the screens and keep a record of the one used in each case, the other to expose the plates.

The same screen should be used for two or three successive exposures, in one or more cases, and it should be made impossible for the person exposing the plates to know in any way whether a change had been made or not.

I feel very sure that a day spent on some such experiment as this would show that the variations in the density on the photographic plate had no connection with the screen used.

Why cannot the experimenters who obtain results with N-rays and those who do not try a series of experiments together, as was done only last year by Cremieu and Pender, when doubt had been expressed about the reality of the Rowland effect?

EXPLANATIONS AND STATEMENTS CONCERNING N-RAYS: A REPLY TO PROF. WOOD.*

By M. R. BLONDLOT, Professor in the University of Nancy.

1. PROF. WOOD has made the following objection to my photographic experiments: The person charged with the duty of moving the plateholder to and fro may be influenced by a wish that the experiment shall succeed, and may therefore unconsciously prolong the exposure of that half on which a stronger photographic impression is expected.

Since Prof. Wood's visit I have measured the times of exposure with precision. For this purpose I constructed an apparatus which recorded on a revolving cylinder, electrically and *automatically*, the durations of the successive exposures while other marks were made on the cylinder at intervals of exactly a second.

The experiments were crossed, that is to say, the person who moved the plateholder counted 25 seconds for the first exposure of the right side (for example), then 25 seconds for the first exposure of the left side, then 25 seconds for the second exposure of the right side, and, finally, 25 seconds for the second exposure of the left side. The record on the cylinder showed that the total exposures of the two halves of the plate never differed by more than half a second. It may be assumed that a variation of half a second in a total exposure of 50 seconds would have no appreciable effect on the photographic impression, but to remove all uncertainty on this point I contrived that the side acted on by the spark in the absence of N-rays should always have the longer exposure. An additional second was counted during this exposure, and the record on the cylinder showed that it actually exceeded the other by from $\frac{1}{2}$ to $\frac{3}{2}$ seconds.

With the kind assistance of M. C. Gotton, I made, in the most careful manner, twelve such experiments on four different days. The N-rays were produced by a Nernst lamp between which and the spark, 50 centimeters distant, were intercalated three sheets of aluminium, a sheet of zinc, a spruce board two centimeters thick, a sheet of cardboard, a sheet of paper, and, finally, a plano-convex lens of aluminium 6 millimeters thick at the center.

In order to eliminate all electrostatic influence of the movable screen upon the little spark generator, the latter was protected by a fixed aluminium screen connected to earth. Furthermore, the movable screen, which in my first experiments was only half as wide as the plateholder, was replaced by a sheet of zinc of the full width of the latter. Zinc is transparent to N-rays, but either half of the screen could be made opaque to them by covering it with wet paper, which in successive experiments was applied alternately to the two halves of the sheet of zinc.

In every one of these twelve experiments the half of the plate during the exposure of which N-rays had acted on the spark developed more quickly and more strongly than the other half, although it had been exposed for a shorter time. The same result was obtained even with only two exposures of 50 seconds each. The photographs thus preserve irrefutable evidence of the effect of N-rays on the electric spark.

2. Attempts made by one person, observing a phosphorescent screen, to detect the moment at which another, without the observer's knowledge, throws the N-rays on the screen are not necessarily successful.

Prof. Wood having asked me if such an attempt would succeed, I told him repeatedly, "*Nicht sicher, nicht mit Sicherheit.*"

We were obliged to converse in German. This was a great inconvenience to me, and probably caused more than one misunderstanding, besides preventing me from explaining many points clearly.

I had warned Prof. Wood of this, and it was solely to gratify him that I consented to make the attempt with him. As I had foreseen, it was not successful. To explain the failure of experiments of this sort, I will make use of an electrical analogy. Let us suppose that we seek to detect a very weak current with the aid of a very sensitive astatic galvanometer, the needle of which (as is but too often the case) is never at rest, but, affected by disturbing influences which cannot be suppressed, undergoes irregular deviations comparable in amplitude to the deviation which would be produced by the current sought. If this current is applied without the knowledge of the observer the latter cannot distinguish its effect from that of the disturbing influences and cannot determine whether the current exists or not. If, on the other hand, the observer himself closes or opens the circuit at a moment of his own choosing, he can, by selecting a moment when the needle is nearly at rest, and by repeating the action at pleasure, determine whether or not the closing of the circuit is always followed by a small deviation in one direction, and its opening by a deviation in the contrary direction.

The case of the N-rays is strictly analogous. In the first place, their effect on the phosphorescent screen is slight and, in the second, many influences which it has not yet been found possible to avoid may produce effects altogether similar, but intermittent and irregular. Among such disturbing causes are noises (as Macé de Trépinay has proved), currents of air and even the observer's body. Evidently it is impossible to separate the effect of the N-rays at any given instant from these purturbations. Besides (and this is of prime importance) the eye becomes fatigued rapidly by watching and loses the ability to make delicate observations. If, on the contrary, the observer is permitted to introduce at will the agent under investigation he need not tire his eyes to no purpose and may, by repeatedly selecting calm intervals, determine whether the agent produces any effect.

3. I affirm most positively that the phenomena of N-rays have for me the same certainty that other physical phenomena have. Several of my colleagues and a number of other persons say the same. To see these phenomena it is necessary to look without accommodating the eyes, as painters, especially those of the impressionist school, have learned to look. This mode of vision is contrary to custom and particularly to the habit of physicists expert in photometric measurements. Helmholtz ("Handbuch der physiologischer Optik," p. 440) makes a remark* which applies to this case.

"Alles dies erfordert grosse Uebung und eine Menge der hierher gehörigen Thatsachen können deshalb nicht einmal ohne vorgängige lange Uebung in physiologisch-optischen Beobachtungen beobachtet werden, selbst nicht von Männern die in anderen Arten von Beobachtungen wohl geübt sind. In vielen Punkten ist man also auf die Beobachtungen sehr weniger Individuen reducirt."

A few persons succeed at once, others after more or less practice, *in everyday life*, in observing objects without visual accommodation; a few others never succeed.

It is quite natural that persons who cannot see the effects of N-rays on phosphorescent screens should find it difficult to believe that others see those effects with absolute certainty. I shall make no attempt to convince them, but, in case they wish to repeat the experiment, I beg to recommend the following procedure: Place the back and the head against a wall and the screen lower than the eyes, so that a plane drawn through them and its center is inclined 40 degrees or more to the horizon. The screen should be normal to the line of vision and none of the precautions previously indicated should be neglected.

I may be permitted to recall here, among the names of men of science who have published investigations of N-rays, those of two physicists whose researches have been wholly independent of mine. The regretted Macé de Lépinay, of Marseilles, with whom I had no personal acquaintance or correspondence, discovered and described in several notes published in the Comptes Rendus de l'Académie des Sciences the action of sound waves on phosphorescent screens, therewith confirming several of my previous statements about N-rays. Is it conceivable that this physicist, celebrated for his lifelong and universally esteemed labors in optics and metrology, would have published observations of whose accuracy he was not absolutely sure?

M. Jean Becquerel, of Paris, has published in the Comptes Rendus many notes on N-rays and N'-rays, which notes were presented by his father, the eminent physicist, M. Henri Becquerel. Who will believe that M. Jean Becquerel has, with his father's consent, risked compromising one of the most illustrious names of science by publishing observations on which rests the shadow of a doubt?

* This reply was prepared by Prof. Blondlot at the request of the Editor of the SCIENTIFIC AMERICAN.

* "All this demands much practice and consequently many facts of this nature cannot even be observed without long preliminary practice in physiologico-optical observation, not even by men expert in observations of other sorts. On many points, therefore, we are restricted to the observations of very few individuals."

NORTH POLAR EXPLORATION: FIELD WORK OF THE PEARY ARCTIC CLUB, 1898-1902.*

By Commander R. E. PEARY, United States Navy.

INTRODUCTION.

IN January, 1897, I promulgated before the American Geographical Society of New York city my plan for an extended scheme of arctic exploration, having for its main purpose the attainment of the North Pole. During the spring of 1897 Morris K. Jesup, now president of the Peary Arctic Club, became interested in the matter and suggested the idea of this club. His example was followed by other prominent men, and late in May, through the persistent personal efforts of Charles A. Moore, backed by letters from these and other influential men, five years' leave of absence was granted me by the Navy Department to enable me to carry out my plans.

It being too late that season to get the main expedition under way, the summer of 1897 was devoted to a preliminary trip to the Whale Sound region to acquaint the Eskimos with my plan for the coming year and in setting them to work laying in a stock of skins and meat. These objects were successfully accomplished, and, in addition, the great "Ahnighito" meteorite of Melville Bay, the largest known meteorite in the world, was brought home. In December, 1897, while in London, the schooner yacht "Windward," which had been used in his Franz Josef Land expedition, was tendered to me by Alfred Harmsworth, who offered to have her re-engined and delivered to me in New York. This generous offer I accepted. In the spring of 1898 the Peary Arctic Club was organized, Morris K. Jesup, Henry W. Cannon, H. L. Bridgman, all personal friends of mine, forming the nucleus about which the rest assembled. In May the "Windward" arrived, but to my extreme regret and disappointment she still retained her antiquated and puny engine (the machinists' strike in England prevented the installation of new ones), and was practically nothing but a sailing craft. The lateness of the season was such that nothing could be done but make the most of the "Windward" as she was. But her extreme slowness (3½ knots under favorable circumstances) and the introduction of a disturbing factor, in the appropriation by another of my plan and field of work, necessitated the charter of an auxiliary ship if I did not wish to be distanced in my own domain. The "Windward" sailed from New York on the 4th of July, 1898, and on the 7th I went on board the "Hope" at Sydney, Cape Breton, and sailed just as the first two-line cablegram came of the battle of Santiago.

1898-99.

Pushing rapidly northward and omitting the usual calls at the Danish Greenland ports, Cape York was reached after a voyage uneventful except for a nip in the ice of Melville Bay which lifted the "Hope" bodily and for a few hours seemed to contain possibilities of trouble. The work of hunting walrus and assembling my party of natives was commenced at once; the "Windward" soon joined us, after which the hunting was prosecuted by both ships until the final rendezvous at Etah, from whence both ships steamed out on August 13, the "Windward" to continue northward, the "Hope" bound for home. The "Windward" was four hours forcing her way through a narrow barrier of heavy ice across the mouth of Foulke Fjord. Here the "Hope" left us, straightening away southward toward Cape Alexander, and the "Windward" headed for Cape Hawkes, showing distinctly beyond Cape Sabine. At 4 A. M. Sunday we encountered scattered ice off Cape Albert. About noon we were caught in the ice near Victoria Head, and drifted back several miles. Finally we got round Victoria Head into Princess Marie Bay at 6 P. M. The bay was filled with the season's ice, not yet broken out, while Kane Basin was crowded with the heavy, moving polar pack. Between the two, extended northward across the mouth of the bay, was a series of small pools and threads of water, opening and closing with the movements of the tide. At 11:30 P. M. on the 18th the "Windward" had worried her way across the bay to a little patch of open water close under Cape D'Urville. Here further progress was stopped by a large floe, several miles across, one end resting against the shore and the other extending out into the heavy ice. While crossing the bay the more important stores had been stowed on the deck in readiness to be thrown out upon the ice in the event of a nip. Pending the turning of the tide, when I hoped the big floe would move and let us proceed, I landed at Cape D'Urville, deposited a small cache of supplies and climbed the bluffs to look at the conditions northward.

August 21.—I went on a reconnoissance along the ice foot to the head of Allman Bay and into the valley beyond. The night of the 21st young ice formed which did not melt again. On the 28th I attempted to sledge over the sea ice to Norman Lockyer Island, but found too many weak places, and fell back on the ice foot. The night of the 29th the temperature fell to 13 deg. F., and on the 31st the new ice was 4¼ inches thick. On this day I went to Cape Hawkes and climbed to its summit, whence I could see lakes out in Kane Basin, but between them and the "Windward" the ice was closely packed—a discouraging outlook. Only a strong and continued westerly wind would give me any chance. The uncertainty of these two weeks was very annoying to me. Had I been sure that we could not get away from here I could have been making an inland trip. As it was I could not leave the ship for fear an opportunity to advance would occur in my absence.

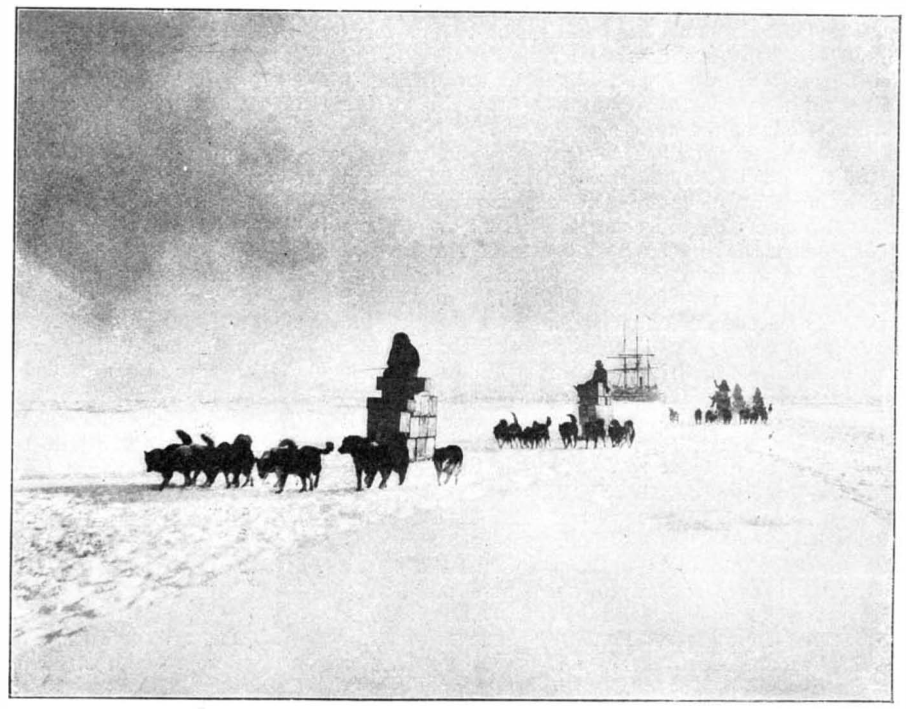
September 2.—I started on a sledge trip up Princess Marie Bay. At Cape Harrison the strong tidal current kept the ice broken, so I could not round it, and the ice foot was impracticable for sledges. I went on foot to the entrance of Copes Bay, surveying the shore to that point, then returned to the ship after four days' absence. During this trip I obtained the English record from the cairn on the summit of Norman Lockyer Island, deposited there twenty-two years ago. This record was as fresh as when left.

September 6.—I left the ship to reconnoiter Dobbin Bay, the head of which is uncharted, returning three days later. During this trip the first real snowstorm of the season occurred, 5½ inches falling.

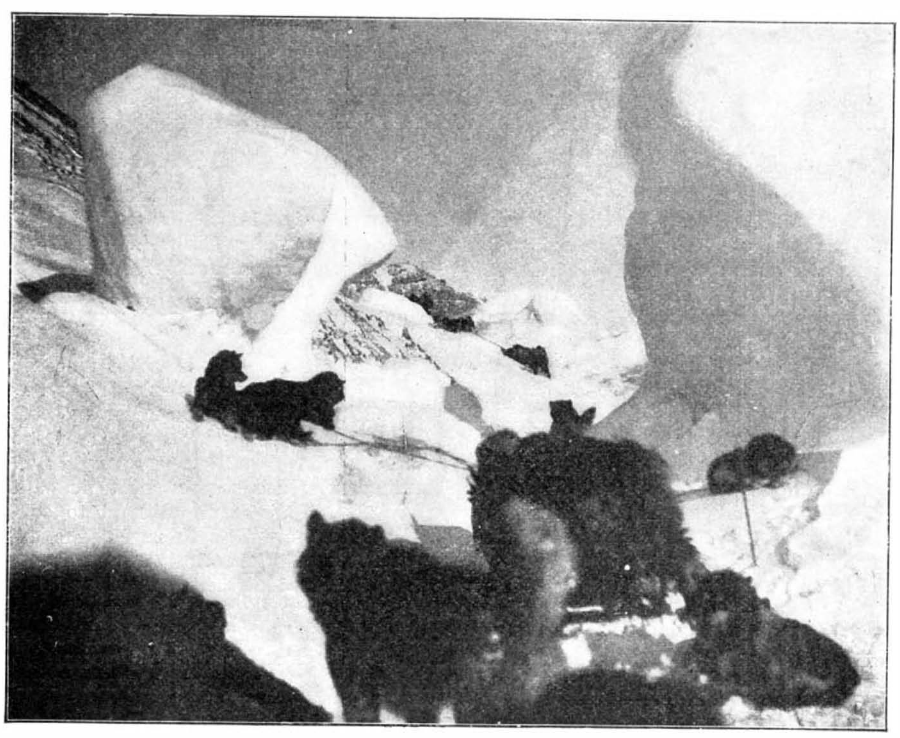
September 12.—One-third of my provisions, an ample year's supply for the entire party, was landed at Cape D'Urville, my Eskimos sledging loads of 700 to 1,000 pounds over the young ice. The night of the 13th the temperature dropped to —10 deg. F., and all hope of farther advance was at an end.

September 15.—The boiler was blown off and preparations for winter commenced.

BRINGING OUT THE GREELY RECORDS.



LANDING SUPPLIES AT CAPE D URVILLE



ALONG THE ICE FOOT.

WINTER QUARTERS AT CAPE D'URVILLE, 1898-99.

NORTH POLAR EXPLORATION. FIELD WORK OF THE PEARY ARCTIC CLUB, 1898-1902.

* From manuscript, as read before the Peary Arctic Club, by courtesy of the National Geographic Society, and reprinted in Smithsonian Institution Report.

On the 17th I broached my plans for the winter campaign as follows:

The autumn work was simple enough and outlined itself. It comprised two items—the securing of a winter's supply of fresh meat for the party and the survey of the Buchanan Strait-Hayes Sound-Princess Marie Bay region. In spite of the peculiarly desolate character of that part of the Grinnell Land coast immediately about the "Windward," and the apparent utter absence of animal life, I felt confident of accomplishing the former. Various reconnoissances thus far on the north shore of Princess Marie Bay had given me little encouragement, but I knew that the Eskimos had killed one or two musk oxen in years past on Bache Island, and that region looked favorable for them. As regards the survey, a presentiment that I must get at that at the earliest possible moment had already led me to make attempts to reach the head of Princess Marie Bay.

As to the spring campaign, I could not be reconciled to the idea of losing a year from the main work of the expedition, and proposed to utilize the winter moons in pushing supplies to Fort Conger, then move my party to that station early in February, and on the return of the sun start from there as a base, and make my attempt on the Pole *via* Cape Hecla. I might succeed in spite of the low latitude of my starting point, and in any event could be back to the ship before the ice broke up, with thorough knowledge of the coast and conditions north of me.

September 18.—I left the ship with two sledges and my two best Eskimos, with provisions for twelve days, for a reconnoissance of Princess Marie Bay.

September 20.—I reached the head of a small fjord running southwest from near the head of Princess Marie Bay, and found a narrow neck of land about 3 miles wide separating it from a branch of Buchanan Strait. Bache Island of the chart is, therefore, a peninsula, and not an island. From a commanding peak in the neighborhood I could see that both arms of Buchanan Strait ended about south of my position; that the "strait" is in reality a bay, and that Hayes Sound does not exist. On the 21st and 22d I penetrated the arms of Princess Marie Bay, designated as Sawyer and Woodward bays on the charts, and demonstrated them to be entirely closed.

September 23.—While entering a little bight about midway of the north shore of Bache Peninsula, I came upon two bears. These my dog chased ashore, and held at bay until I could come up and kill them.

September 25.—I crossed Bache Peninsula on foot with my two men, from Bear Camp to the intersection of the northern and southern arms of Buchanan Bay. Here we found numerous walrus, and could command the southern arm of the large glacier at its head. Comparatively recent musk-ox tracks convinced me of the presence of musk ox on the peninsula. The next day I returned to the "Windward" to refit and start for Buchanan Bay *via* Victoria Head and Cape Albert, in the quest of walrus and musk oxen. Henson, in a reconnoissance northward during my absence, had been unable to get more than a few miles beyond Cape Louis Napoleon, sea ice and ice foot being alike impracticable. A day or two after my return I started him off again to try it.

September 30.—I started for Buchanan Bay. Between Victoria Head and Cape Albert found fresh tracks of a herd of musk oxen and followed them until obliterated by the wind. Reached the walrus grounds in Buchanan Bay late on October 4, and the next day secured a walrus, and the remainder of my party arrived. The following day everyone was out after musk oxen, but, finding it very foggy on the uplands of the peninsula, I returned to camp and went up to Buchanan Bay in search of bears, the tracks of which we had seen. Returning to camp, I found that one of my hunters had killed a bull musk ox.

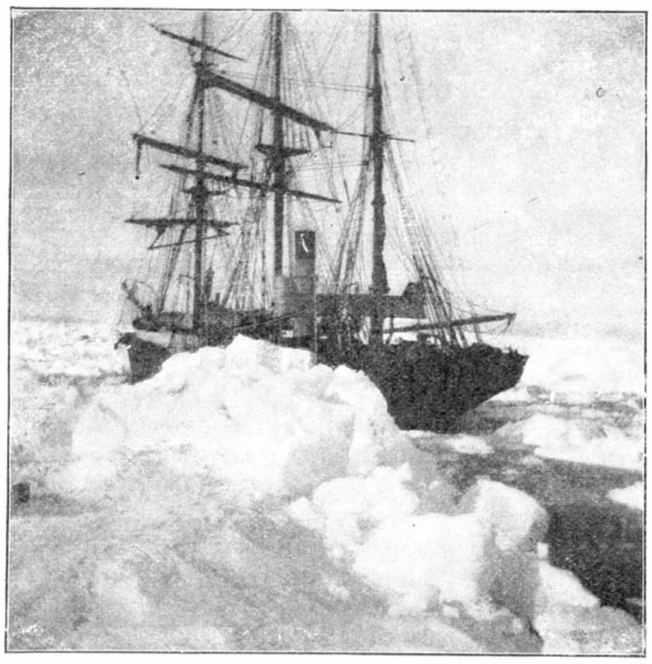
On the 7th of October I sent two men to bring out the meat and skin, while I went up Buchanan Bay again. Returning to camp, I found it desolated. A little later some of my party returned, reporting a herd of fifteen musk oxen killed. The next two days were consumed in cutting up the animals, stacking the meat, and getting the skins and some of the meat out to camp. The latter had to be dragged to the top of the bluffs and thrown over.

October 10.—We started for the ship, which was reached late on the 12th. The ice in Buchanan Bay was very rough, and a snowstorm on the 11th made going very heavy. Five days later, October 17, I went with two men to locate a direct trail for getting the meat out to the north side of the peninsula, but found the country impracticable, and returned to the ship on the 21st. The sun left on the 20th.

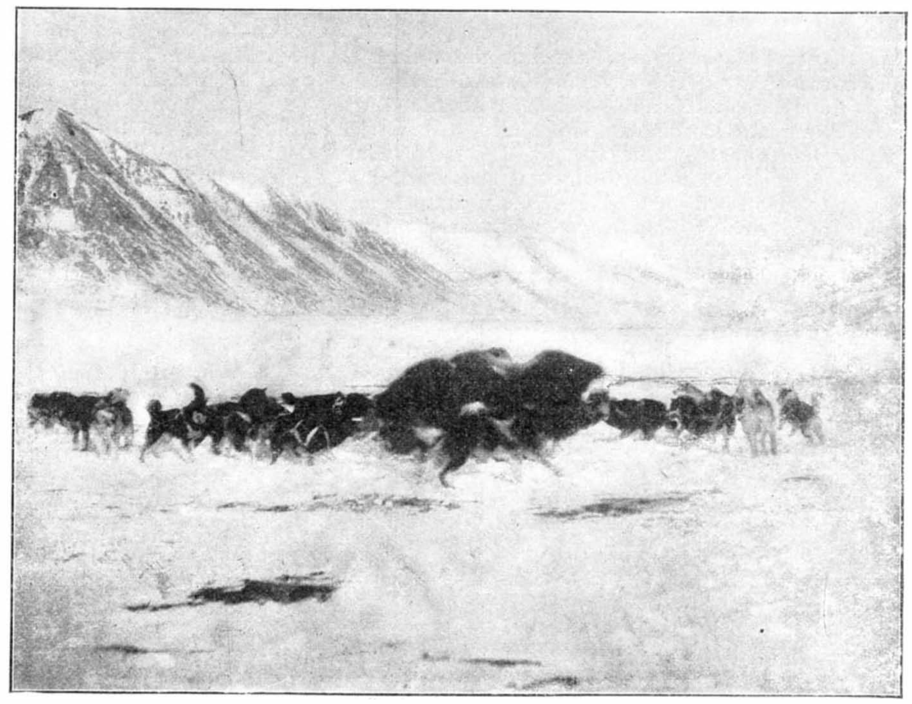
The following week was devoted to the work of preparation for the winter. A reconnoissance of Franklin Pierce Bay developed nothing but hare tracks, but Henson came in from Copes Bay with a big bear, killed near the head of the bay. This marked the end of the fall campaign, with our winter's fresh meat supply assured, and the Bache "Island"-Buchanan "Strait"-"Hayes Sound" question settled.

The next step was the inauguration of the teaming work, which was to occupy us through the winter. I already had my pemmican and some miscellaneous supplies at Cape Louis Napoleon, and two sledge loads of provisions at Cape Fraser. The rapidly disappearing daylight being now too limited for effective traveling, I was obliged to wait the appearance of the next moon before starting for a personal reconnoissance of the coast northward. On the 29th I left the ship with Henson and one Eskimo. The soft snow of the last two storms compelled me to break a road for the sledges with my snowshoes across Allman Bay and along many portions of the ice foot, but in spite of this delay we camped at Cape Louis Napoleon after a long march.

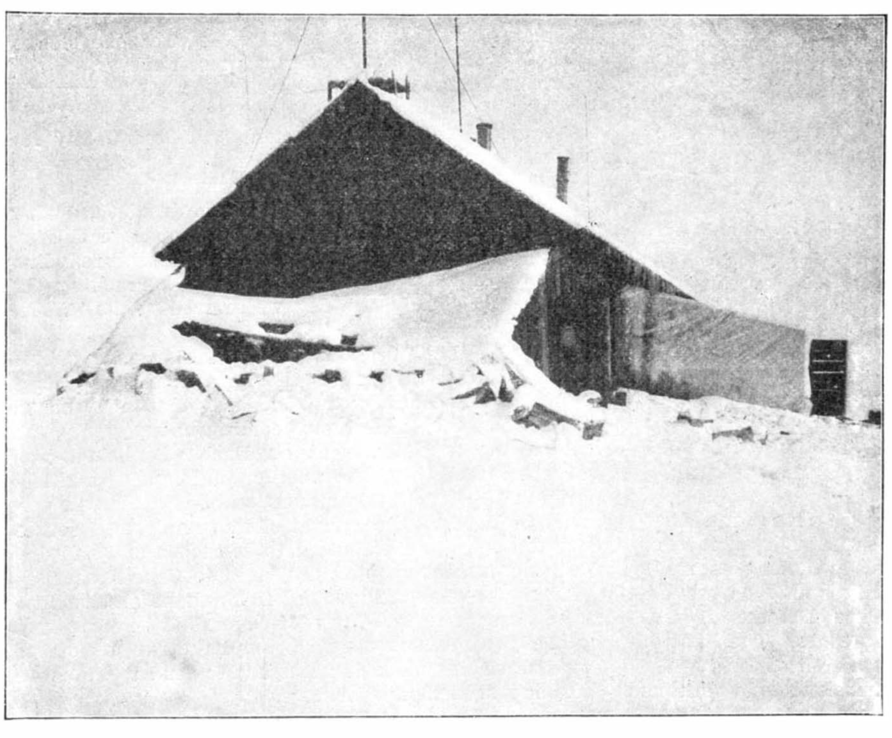
The next day we reached Cape Fraser, having been impeded by the tide rising over the ice foot, and camped at Henson's farthest, at the beginning of what seemed an impracticable ice foot. It was the only possible way of advance, however, as the still moving pack in the channel was entirely impassable. The following day I made a reconnoissance on foot as far as Scoresby Bay, and though the ice foot was then entirely impracticable for sledges, I was convinced that a good deal of earnest work with picks and shovels, assisted by the leveling effects of the next spring tides, would enable me to get loaded sledges over it during the next moon. From Cape Norton Shaw I could see that by making a detour into Scoresby Bay the heavy pack could be avoided in crossing. This stretch of ice foot from Cape Fraser to Cape Norton Shaw is ex-



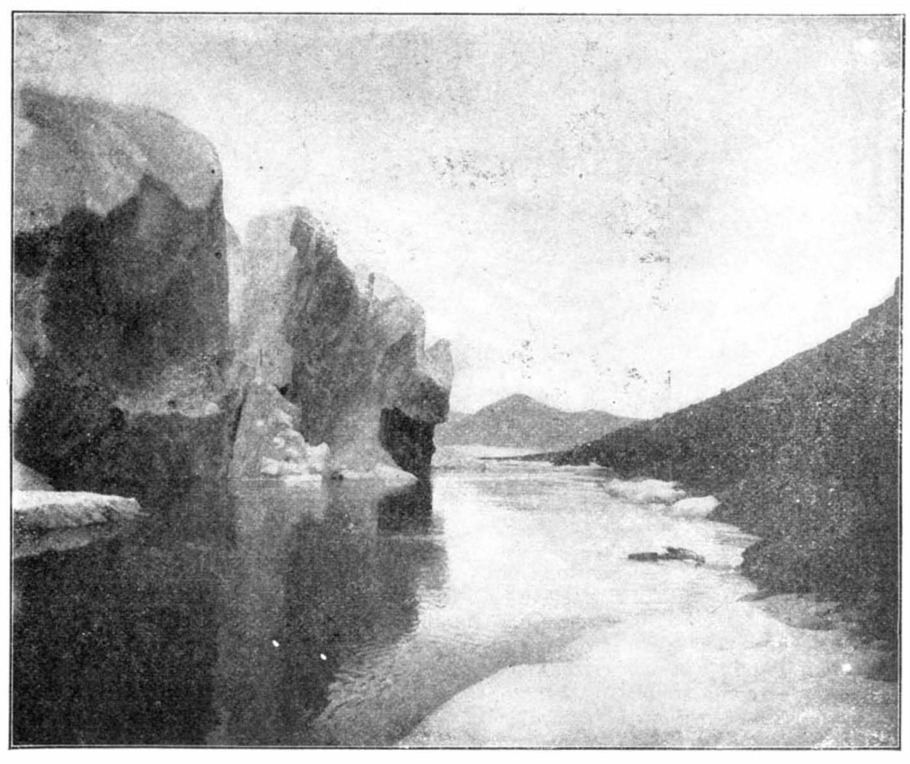
CROSSING PRINCESS MARIE BAY.



MUSK OXEN NEAR CAPE JESUP.



FORT CONGER.



LATERAL RIVER OF BENEDICT GLACIER.

NORTH POLAR EXPLORATION: FIELD WORK OF THE PEARY ARCTIC CLUB, 1898-1902.

tremely Alpine in character, being an almost continuous succession of huge blocks and masses of bergs and old floes, forced bodily out of the water and up onto the rocks. At Cape John Barrow a large berg had been forced up on the solid rock of the cape, until one huge fragment lay fully 100 feet above the tigh-tide level.

Returning from my reconnoissance, I camped again at Camp Fraser, building the first of my snow igloos, which I intended should be constructed at convenient intervals the entire distance to Fort Conger. The next three days were occupied in bringing the supplies at Cape Louis Napoleon up to Cape Fraser, and on the 4th day of November I returned to the ship. The time until the return of the next moon was fully occupied in making and repairing sledges, bringing in beef from the cache on Bache Peninsula, and transporting supplies and dog food to Cape Hawkes, beyond the heavy going of Allman Bay. During much of this time the temperature was in the —40 deg. F.

November 21.—Henson and three Eskimos left with loads, and on the 22d I followed with a party of three to begin the work of the November moon. This work ended just before midnight of December 4, when the last sledges came in. It left 3,300 pounds of supplies and a quantity of dog food at Cape Wilkes on the north side of Richardson Bay. These supplies would have been left at Cape Lawrence had it not been for the desertion and turning back of one of my men, discouraged with the hard work, while crossing Richardson Bay. Knowing it to be essential to prevent any recurrence of the kind, I pushed on to Cape Wilkes, camped, and turned in after a twenty-five-hour day, slept three hours, then started with empty sledge, 8 picked dogs, and an Eskimo driver, to overtake my man. He was found at Cape Louis Napoleon, and after receiving a lesson was taken along with me to the ship.

OVERLAND ACROSS ELLESMERE LAND.

My party was left with instructions to bring up supplies which the wrecking of sledges had obliged me to cache at various places, assemble all at Cape Wilkes, and then, if I did not return, reconnoiter the ice foot to Rawlings Bay and return to the ship. The distance from Cape Wilkes to the "Windward" was 60 nautical miles in a straight line (as traveled my me along the ice foot and across the bays, not less than 90 statute miles), and was covered in twenty-three hours and twenty minutes, or twenty-one hours and thirty minutes actual traveling time. Temperature during the run, —50 deg. F. Every sledge was more or less smashed in this two weeks' campaign, and at Cape John Barrow sledges and loads had to be carried on our backs over the ice jams. The mean daily minimum temperature for the thirteen days was —41.2 deg. F., the lowest, —50 deg. F., which occurred on four successive days. The experience gained on this trip led me to believe that the conditions of travel from Cape Wilkes northward, as far at least as Cape Defosse, would not differ materially from those already encountered and enabled me to lay my plans with somewhat greater detail. With the light of the December moon I would proceed to Cape Wilkes with such loads as would enable me to travel steadily without double banking, advance everything to Cape Lawrence on the north side of Rawlings Bay, then go rapidly on to Fort Conger with light sledges, determine the condition of the supplies left there, that I might know what I could depend upon, and thus save transportation of unnecessary articles, then return to the ship.

In the January moon I would start with my entire party; move my supplies from Cape Lawrence to Fort Conger; remain there till the February moon, the light of which would merge into the beginning of the returning daylight; then sledge the supplies for the polar journey to Cape Hecla, and be in readiness to start from there, with rested and well-fed dogs, by the middle of March. In pursuance of this plan, the two weeks intervening between the departure of the November moon and the appearance of the December one were busily occupied in repairing and strengthening sledges, and making and overhauling clothing and equipment, to enable us to meet this long and arduous journey in the very midnight of the "great night." During this interval the temperature much of the time was at —50 deg. F. and below.

December 20.—In the first light of the returning moon I left the "Windward" with my doctor, Henson, four Eskimos, and thirty dogs, all that were left of the sixty-odd of four months previous. Thick weather, strong winds rushing out of Kennedy Channel, heavy snow, and an abominable ice foot in Rawlings Bay delayed me, and it was not until the 28th that I had all my supplies assembled at Cape Lawrence, on the north side of Rawlings Bay.

Cape Lawrence presented the advantage of two possible routes by which these latter supplies could be reached from Conger, one through Kennedy Channel, which I was about to follow, and the other via Archer Fjord and overland. In spite of the delays, I felt on the whole well satisfied with the work up to the end of the year. I had all my supplies halfway to Fort Conger, and had comfortable snow igloos erected at Cape Hawkes, Cape Louis Napoleon, Cape Fraser, Cape Norton Shaw, Cape Wilkes, and Cape Lawrence.

December 29.—I started from Cape Lawrence with light sledges for Fort Conger, hoping to make the distance in five days. The first march from Cape Lawrence the ice foot was fairly good, though an inch or two of efflorescence made the sledges drag as if on sand. The ice foot grew steadily worse as we advanced, until after rounding Cape Defosse, it was almost impassable even for light sledges. The light of the moon lasted only for a few hours out of the twenty-four, and at its best was not sufficient to permit us to select a route on the sea ice.

Just south of Cape Defosse we ate the last of our biscuit, just north of it the last of our beans. On the next march a biting wind swept down the channel and numbed the Eskimo who had spent the previous winter in the States, to such an extent that to save him we were obliged to halt just above Cape Cracroft and dig a burrow in a snowdrift. When the storm ceased I left him with another Eskimo and nine of the poorest dogs and pushed on to reach Fort Conger.

The moon had left us entirely now, and the ice foot was utterly impracticable, and we groped and stumbled through the rugged sea ice as far as Cape Baird. Here we slept a few hours in a burrow in the snow, then started across Lady Franklin Bay. In complete darkness and over a chaos of broken and heaved-up ice we stumbled and fell and groped for eighteen hours, till we climbed upon the ice foot of the north side. Here a dog was killed for food. Absence of suitable snow put an igloo out of the question, and a semicave under a large cake of ice was so cold that we could stop only long enough to make tea. Here I left a broken sledge and nine exhausted dogs. Just east of us a floe had been driven ashore, and forced up over the ice foot till its shattered fragments lay 100 feet up the talus of the bluff. It seemed impassable, but the crack at the edge of the ice foot allowed us to squeeze through; and soon after we rounded the point, and I was satisfied by the "feel" of the shore, for we could see nothing, that we were at one of the entrances of Discovery Harbor, but which one I could not tell. Several hours of groping showed that it was the eastern entrance. We had struck the center of Bellot Island, and at midnight of January 6 we were stumbling through the dilapidated door of Fort Conger. A little remaining oil enabled me, by the light of our sledge cooker, to find the range and the stove in the officers' quarters, and after some difficulty fires were started in both. When this was accomplished, a suspicious "wooden" feeling in my right foot led me to have my kamiks pulled off, and I found to my annoyance that both feet were frosted. Coffee from an open tin in the kitchen, and biscuit from the table in the men's room, just as they had been dropped over fifteen years ago, furnished the menu for a simple but abundant lunch. A hasty search failing to discover matches, candles, lamps, or oil, we were forced to devise some kind of a light very quickly before our oil burned out. Half a bottle of olive oil, a saucer, and a bit of towel furnished the material for a small native lamp, and this, supplemented by pork fat and lard, furnished us light for several days, until oil was located. Throwing ourselves down on the cots in the officers' rooms, after everything had been done for my feet, we slept long and soundly. Awakening, it was evident that I should lose parts or all of several toes, and be confined for some weeks. The mean minimum temperature during the trip was —51.9 deg. F., the lowest —63 deg. F.

During the following weeks our life at Conger was pronouncedly *à la* Robinson Crusoe. Searching for things in the unbroken darkness of the "great night," with a tiny flicker of flame in a saucer, was very like seeking a needle in a haystack. Gradually all the essentials were located, while my two faithful Eskimos brought in empty boxes and barrels and broke them up to feed the fire. The dogs left on Bellot Island were brought in, but several died before they got used to the frozen salt pork and beef, which was all I had to feed them. The natives made two attempts to reach and bring in the two men left at Cracroft, but were driven back both times by the darkness and furious winds. Finally, some ten days after we left the dugout, they reached it again, and found that the two men, after eating some of their dogs, had started for the ship on foot, the few remaining dogs following them.

On the 18th of February the moonlight and the remaining twilight afforded enough light for a fair day's march in each twenty-four hours; we started for the "Windward." My toes were unhealed, the bones were protruding through the raw stumps on both feet, and I could hardly stand for a moment. I had twelve dogs left, but their emaciated condition and the character of the road precluded riding by anyone but myself. Lashed firmly down, with feet and legs wrapped in musk-ox skin, I formed the only load of one sledge. The other carried the necessary provisions.

On the 28th we reached the "Windward," every one but myself having walked the entire distance of not less than 250 miles in eleven days. Fortunately for us, and particularly for me, the weather during our return, though extremely cold, was calm, with the exception of one day from Cape Cracroft south, during which the furious wind kept us enveloped in driving snow. The mean minimum daily temperature while we were returning was —56.18 deg. F., reaching the lowest, —65 deg. F., the day we arrived at the "Windward."

March 3.—I started one of my Eskimos for Whale Sound with a summons to the hunters there to come to me with their dogs and sledges. Between the 3d and 14th a party of Eskimos coming unexpectedly, the last of the musk-ox meat on Bache Peninsula was brought to the ship, and another bull musk ox killed.

March 13.—The final amputation of my toes was performed. Pending the arrival of more natives, I sent a dory to Cape Louis Napoleon to be cached and had dog food and current supplies advanced to Cape Fraser.

March 31.—A contingent of five natives and twenty-seven dogs came in. My messenger had been delayed by heavy winds and rough ice, and the ravages of the dog disease had made it necessary to send to the more southerly settlements for dogs.

April 3.—Henson left with these natives and thirty-five dogs, with instructions to move the supplies at Cape Lawrence to Carl Ritter Bay, then push on with such loads as he could carry without double banking to Fort Conger, rest his dogs and dry his clothing, and if I did not join him by that time to start back.

April 19.—My left foot had healed, though still too weak and stiff from long disuse for me to move without crutches. On this day I started for Fort Conger with a party of ten, some fifty dogs, and seven sledges loaded with dog food and supplies for return caches.

April 23.—I met Henson returning with his party at Cape Lawrence. From there I sent back my temporary help and borrowed dogs, and went on with a party of seven, including five natives.

April 28.—We reached Conger.

May 4.—Having dried all our gear and repaired sledges, I started for a reconnoissance of the Greenland northwest coast. I should have started two days earlier but for bad weather. Following a very arduous ice foot to St. Patrick's Bay, I found the bay filled with broken pack ice covered with snow almost thigh deep. From the top of Cape Murchison, with a good glass, no practicable road could be seen. The following day I sent two men with empty sledges and a powerful team of dogs to Cape Beechy to reconnoiter from its summit. Their report was discouraging.

Clear across to the Greenland shore, and up and down as far as the glass could reach, the channel was filled with upheaved floe fragments, uninterrupted by young ice or large floes, and covered with deep snow.

Crippled as I was, and a mere dead weight on the sledge, I felt that the road was impracticable. Had I been fit and in my usual place, ahead of the sledges breaking the ice with my snowshoes, it would have been different. One chance remained—that of finding a passage across to the Greenland side at Cape Lieber.

Returning to Conger, I sent Henson and one Eskimo off immediately on this reconnoissance, and later sent two men to Musk Ox Bay to look for musk oxen. Two days afterward they returned reporting sixteen musk oxen killed, and Henson came in on the same day, reporting the condition of the channel off Capes Lieber and Cracroft the same as that off Capes Beechy and Murchison, and that they had been unable to get across. I now gave up the Greenland trip, and perhaps it was well that I did so, as the unhealed place on my right foot was beginning to break down and assume an unhealthy appearance from its severe treatment. As soon as the musk-ox skins and beef were brought in, the entire party, except myself and one Eskimo, went to the Bellows and Black Rock Vale for more musk oxen. Twelve were killed here, and the skins and meat brought to Conger. Not believing it desirable to kill more musk oxen, and unable to do any traveling north, I completed the work of securing the meat and skins obtained; getting the records and private papers of the United States International Expedition together; securing, as far as possible, collections and property; housing material and supplies still remaining serviceable, and making the house more comfortable for the purposes of my party.

May 23.—We started for the ship, carrying only the scientific records of the expedition, the private papers of its members, and necessary supplies. I was still obliged to ride continuously. Favored with abundant light and continuously calm weather, and forcing the dogs to their best, the return to the ship was accomplished in six days, arriving there May 29. During my absence Capt. Bartlett had built at Cape D'Urville, from plans which I had furnished him, a comfortable house of the boxes of supplies, double roofed with canvas, and banked in with gravel.

June 1.—I sent one sledgeload of provisions to Cape Louis Napoleon, and four to Cape Norton Shaw.

June 6.—I sent three loads to Carl Ritter Bay and two to Cape Lawrence. On the 25th of June the last of the sledges returned to the "Windward," and the year's campaign to the north was ended. The return from Carl Ritter Bay had been slow, owing to the abundance of water on the ice foot and the sea ice of the bays, and the resulting sore feet of the dogs.

June 28.—A sufficient number of dogs had recovered from the effect of their work to enable me to make up two teams, and Henson was sent with these, four of the natives, and a dory, to make his way to Etah and communicate with the summer ship immediately on her arrival, so that her time would not be wasted even should the "Windward" be late in getting out of the ice.

June 29.—I started with two sledges and three natives to complete my survey of Princess Marie and Buchanan bays, and make a reconnoissance to the westward from the head of the former. My feet, which I had been favoring since my return from Conger, were now in fair condition, only a very small place on the right one remaining unhealed. Traveling and working at night and sleeping during the day, I advanced to Princess Marie Bay, crossed the narrow neck of Bache Peninsula, and camped on the morning of July 4 near the head of the northern arm of Buchanan Bay. Hardly was the tent set up when a bear was seen out in the bay, and we immediately went in pursuit, and in a short time had him killed. He proved to be a fine large specimen. While after the bear I noticed a herd of musk oxen a few miles up the valley, and after the bear had been brought into camp and skinned, and we had snatched a few hours' sleep, we went after the musk oxen. Eight of these were secured, including two fine bulls and two live calves, the latter following us back to camp of their own accord. The next three days were occupied in getting the beef to camp. I then crossed to the southern arm of Buchanan Bay, securing another musk ox. Returning to Princess Marie Bay, I camped on the morning of the 14th at the glacier which fills the head of Sawyer Bay.

During the following six days I ascended the glacier, crossed the ice cap to its western side, and from elevations of from 4,000 to 4,700 feet looked down upon the snow-free western side of Ellesmere Land, and out into an ice-free fjord, extending some fifty miles to the northwest. The season here was at least a month earlier than on the east side, and the general appearance of the country reminded me of the Whale Sound region of Greenland. Clear weather for part of one day enabled me to take a series of angles, then fog and rain and snow settled down upon us. Through this I steered by compass back to and down the glacier, camping on the 21st in my camp of the 15th. The return from here to the ship was somewhat arduous, owing to the rotten condition of the one-year ice and the deep pools and canals of water on the surface of the old floes. These presented the alternative of making endless detours or wading through water often waist deep. During seven days our clothing, tent, sleeping gear, and food were constantly saturated. The "Windward" was reached on the 28th of July.

In spite of the discomforts and hardships of this trip, incident to the lateness of the season, I felt repaid by its results. In addition to completing the notes requisite for a chart of the Princess Marie-Buchanan Bay region, I had been fortunate in crossing the Ellesmere Land ice cap and looking upon the western coast. The game secured during this trip comprised a polar bear, seven musk oxen, three oogsook, and fourteen seals.

When I returned to the "Windward," she was round in the eastern side of Franklin Pierce Bay. A party had left two days before with dogs, sledge, and boat, in an attempt to meet me and supply me with provisions. Three days were occupied in communicating with them and getting them and their outfit on board. The "Windward" then moved back to her winter berth at Cape D'Urville, took the dogs on board, and on the morning of Wednesday, August 2, got under way.

During the next five days, we advanced some twelve miles, when a southerly wind jammed the ice on us and drifted us north abreast of the starting point. Early Tuesday morning, the 8th, we got another start, and the ice gradually slackening, we kept under way, reached open water a little south of Cape Albert, and arrived at Cape Sabine at 10 P. M.

At Cape Sabine I landed a cache and then steamed over to Etah, arriving at 5 A. M. of the 9th. Here we found mail, and learned that the steamship "Diana," which the club had sent up to communicate with me, was out after walrus. Saturday morning the "Diana" returned, and I had the great pleasure of taking Secretary Bridgman, commanding the club's expedition, by the hand.

Though the year had not been marked by any startling results, it was a year of hard and continuous work for the entire party. During the year I obtained the material for an authentic map of the Buchanan Bay - Bache Peninsula - Princess Marie Bay region; crossed the Ellesmere Land ice cap to the west side of that land; established a continuous line of caches from Cape Sabine to Fort Conger, containing some fourteen tons of supplies; rescued the original records and private papers of the Greely expedition; fitted Fort Conger as a base for future work, and familiarized myself and party with the entire region as far north as Cape Beechy.

With the exception of the supplies at Cape D'Urville, all the provisions, together with the current supplies and dog food (the latter an excessive item), had been transported by sledge.

Finally, discouraging as was the accident to my feet, I was satisfied, since my effort to reach the northwest coast of Greenland from Conger in May, that the season was one of extremely unfavorable ice condition north of Cape Beechy, and doubt, even if the accident had not occurred, whether I should have found it advisable, on reaching Cape Hecla, to attempt the last stage of the journey.

My decision not to attempt to winter at Fort Conger was arrived at after careful consideration. Two things controlled this decision: First, the uncertainty of carrying dogs through the winter, and, second, the comparative facility with which the distance from Etah to Fort Conger can be covered with light sledges.

After the rendezvous with the "Diana," I went on board the latter ship and visited all the native settlements, gathering skins and material for clothing and sledge equipment and recruiting my dog teams. The "Windward" was sent hunting for walrus during my absence. The "Diana" also assisted in this work.

August 25.—The "Windward" sailed for home, followed on the 28th by the "Diana," after landing me with my party, equipment, and additional supplies at Etah. (To be continued.)

EXCAVATIONS AT THERMOS, GREECE.

By the Paris Correspondent of SCIENTIFIC AMERICAN.

THE remains of a large temple, surrounded by an inclosure, and other edifices have been found not long ago at Thermos, in Etolia. The temple was erected upon the site of a more ancient structure which seems to have been built of wood and dates from a very early epoch. The wood temple was covered with a plating of terra cotta and ornamented with metopes containing painted figures.

Thermos was not a city, in reality, but was the most ancient religious center of Etolia. During the third century B. C. it was also the political center of the Etolian confederation. Polybius gives a description of it (Book V., chap. v., etc.) on the occasion of Philip's expedition into that country in 218 B. C., and the remains which have been brought to light by the present excavations fully confirm his account of the place. It is situated upon a plateau which lies near the Etolian mountains, to the east of Lake Trichonitis, and is about 1,400 feet above the sea level.

The sacred inclosure of Thermos is still surrounded by its ancient and massive walls which are preserved up to a certain height. The excavations which have been already made here brought to light a number of different edifices belonging to the third century B. C. and a considerable number of inscriptions of the third and second centuries. But the most important of the discoveries is the temple of the Thermian Apollo which lies within the sacred inclosure. This temple is no doubt very ancient. At first it seems to have been a Doric temple built of wood. The interior *cella* of the temple was surrounded by columns, with five columns on the front and fifteen on a side. The length of the temple is about 130 feet and the width 40 feet. The *cella* measures 90 by 15 feet. It is placed exactly north and south. This primitive temple was destroyed by fire, very probably about 218 B. C. The substructure which is found at present seems to belong to the reconstruction which was made after that date. Stone columns now replaced the old wood columns. Some of the bases show signs which make it almost certain that they carried wood columns. The ancient wood temple had a plating of terra cotta, and many pieces of the latter have been found. They are analogous to the terra cotta specimens from the temples of Sicily and the treasure structures of Olympia. The architecture of the Thermos temple, however, has some striking peculiarities. One of the most noteworthy is the use of specially-formed antefixes for the front tiling of the roof and the second peculiarity lies in the painted metopes, which are quite remarkable. The antefixes of the roof have different forms, men's heads, Silenus, and women's figures, which are of archaic style in general, and the rest belong to the still severe style of the fifth century. The color was applied to all these relief objects upon a thin surface layer of pure Corinthian clay which covered a core made of common earth. As to the tiles which are used here, their form and size recalls the tiles of the Neandria temple.

The metopes are formed of seven plates about 3 feet square and 2 inches thick. Like the figures of the roof, the surface of the rough material is covered with a fine layer of clay. In connection with the painted figures there is a rosette ornamentation which is also painted. A great number of fragments of the metopes have been found, so that it was possible to reconstruct five figures almost completely. The first is a Gorgon of archaic style, while a second represents Perseus carrying the head of Medusa. Another represents two female figures facing each other, but these are entire only as far as the waist. One figure represents a hunter carrying a stick over his shoulder from which hang one or more birds in front, and in the back a deer. The fifth shows three female divinities seated upon a throne. These figures belong to an archaic art which is already developed and are to be assigned probably to the school of Corinth, but there seems to be some influence of Ionic art as well. The outline of the figures was first engraved by a line. Then the painter colored them in red and black, and in part in white, but not always keeping exactly to the engraved outline. If we judge from the form of the letters (one of the figures bears the inscription *XEAIΔFON* in large Corinthian letters), we may assign the metopes to the first half of the sixth century B. C. The fifth metope is the only one which bears traces of repairs and it perhaps belongs to a later date.

The temple cannot be posterior to 600 A. D., but below it were found two layers which are still more ancient and go back certainly to the tenth century B. C. At this epoch the Etolians established themselves in the country and the very ancient sanctuary of Apollo was built. This primitive sanctuary seems to have been simply an altar, and the excavators found the site which it no doubt occupied. It is formed of a very thick layer of ashes and countless fragments of calcined animals' bones. Around this center, and lying underneath the whole of the existing temple they discovered a layer of ashes and calcined bones nearly a foot thick. A great number of jars of unusual size (*pithos*) were found in this layer, and as many as seven jars were assembled in a single point. The jars were full of earth mixed with charcoal and numerous burned animals' bones. Some of the jars have relief designs. In one case the designs are of a curvilinear geometric style. In the ashy *débris* of the central point were found several bronze swords, bronze statuettes of the archaic style, small figures of chariots with wheels, some long iron swords and *débris* of vases with geometric patterns.

This altar seems to have been leveled in very ancient times, and a temple was built on the site. The walls of this temple subsist in a greater part underneath the upper temple. Four very large frontal tiles belonging to it were found. These are quite different from the tiles of the upper temple and the spot in which they lay is quite near the ancient structure. They undoubtedly belong to the first temple which was built upon the site of the altar. These objects represented are, first, two heads of a very ancient style in which Egyptian influence is visible. Second, a palmette in relief which is like the more ancient forms found at Tirynthe. This archaic temple seems to have been surrounded by an elliptical inclosure. The lower part of the wall is still in place. It contains a series of bases which undoubtedly supported wood columns.

Another edifice which must have been built entirely of wood, was brought to light. It is not certain as to whether it was a temple or not. A great number of pieces of the terra cotta plating which covered the structure were found here, including metopes with painted figures and inscriptions in Corinthian letters designating the persons of the groups. These metopes have been discovered more recently and a full description of them has not yet been given, but one of them is said to represent Hercules and the Hydra.

Within the temple itself were found a number of objects of considerable interest. Among these is a sphinx of archaic style and a bronze stele with two inscriptions. The first of these relates to an arbitrage between two cities of Etolia, Œnodae and Matropolis, upon the question of boundary lines (middle of the third century). The second is entitled "Treaty and Alliance between the Etolians and the Acarnanians"; it belongs to the period 280-270 B. C. No fragments of marble sculpture have been found at Thermos. The statues which were mounted on the numerous pedestals were of bronze, seemingly. Only a few fragments of these have been found as yet.

[Continued from SUPPLEMENT No. 1510, page 24202.]

CURRENT WHEELS: THEIR USE IN LIFTING WATER FOR IRRIGATION.*

Construction for a Swift Current in Idaho.

THE wheel shown in Fig. 12 has been in use on Lost River, Idaho. It was built to raise water about 10 feet for the irrigation of 2.25 acres in garden and grain. It is 14 feet in diameter, with paddles 6 feet in length mounted on a shaft 18 feet long, spanning the stream. The shaft is an 8 by 8-inch square timber. For 6 inches near each end, it is turned round to form a bearing. The spokes are very substantial, being made of 2 by 6-inch material. Each paddle carries a 3-gallon pickle keg on one end. The kegs are set on a bevel, as shown in the figure. The device for raising and lowering the wheel is very simple, consisting of two uprights which support a pulley, beneath which a wooden bearing is hung by a ¾-inch rope. A piece of gas pipe is used as a windlass (Fig. 13).

When the wheel was built, it was set between two supports at a point where the river is about 40 feet wide, the supporting posts being driven into the bed of the stream at either side of the deeper current. The swift current in the center of the stream turned the wheel very satisfactorily for a time, but owing to the soft nature of the bed, which at this point is composed of coarse gravel, during high water the current washed out a deep channel directly under the wheel and left it high and dry when the flood subsided. To obviate this difficulty, the wheel is to be remounted between two crib piers at a point where the river channel at high water is only about 15 feet wide. In low water the channel is only about 10 feet wide, the current being about 5 feet per second. This narrow channel of the river is only a few years old, and although it appears to consist of a hard cemented gravel, still it is by no means certain that in the course of time the wheel may not again be left stranded above the current.

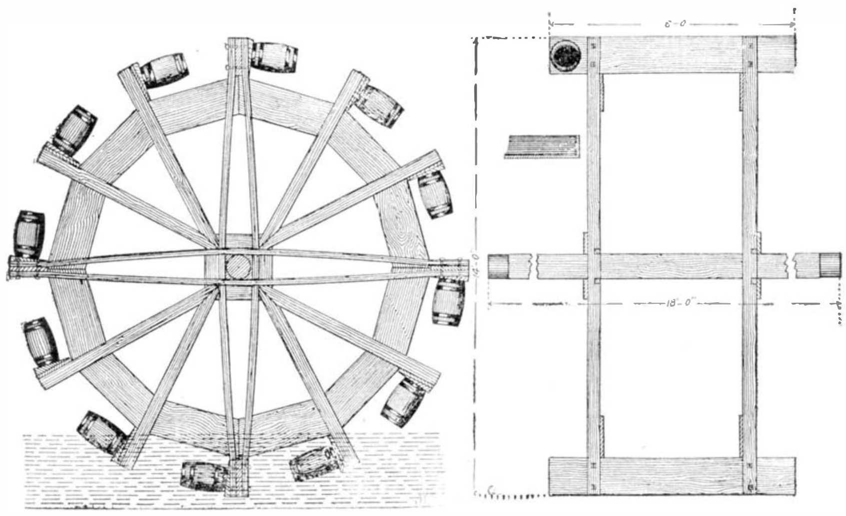


FIG. 12.—WHEEL ON LOST RIVER, IDAHO.

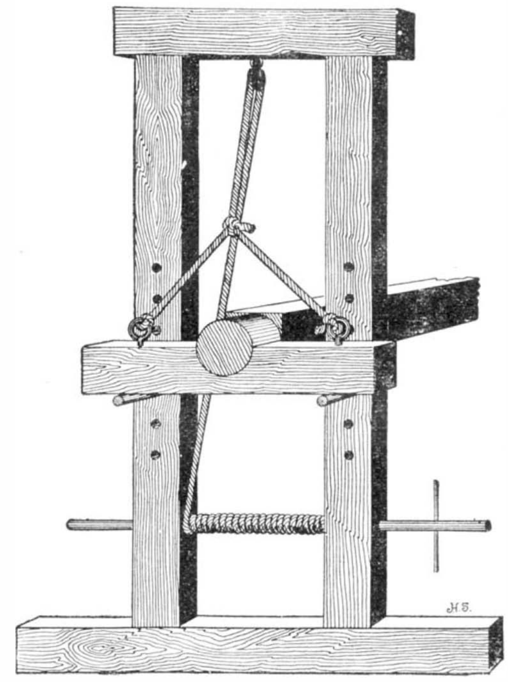


FIG. 13.—LIFTING DEVICE FOR CURRENT WHEEL ON LOST RIVER, IDAHO.

Four hundred feet of lumber were used in building the wheel and 25 pounds of bolts, making the cost of materials about $12, not including the buckets. The heavy construction of the wheel would be unwise under most conditions; but in a current as swift as 5 feet per second in low water and much swifter when the water is high, any but the most substantial construction would prove unsatisfactory. The notching

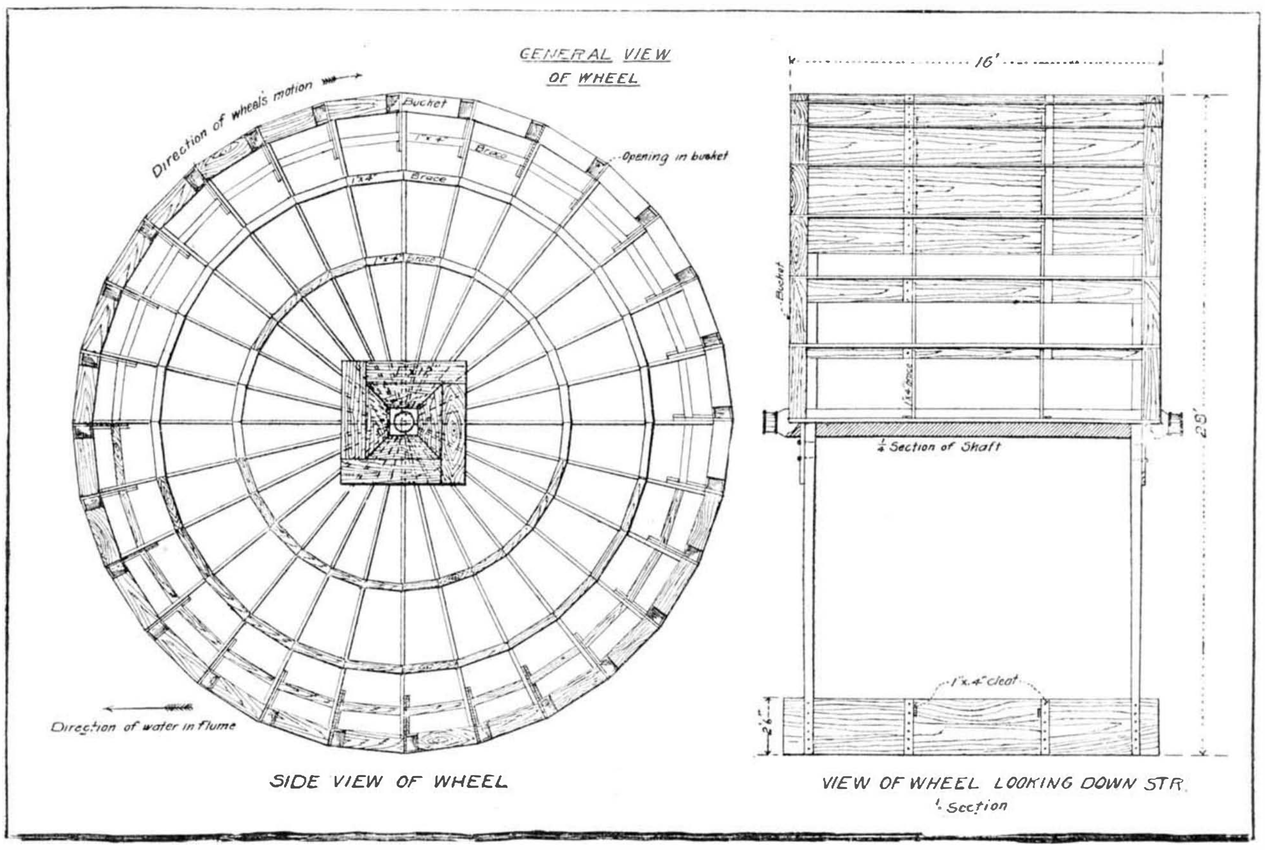


FIG. 14.—KIND OF WHEEL IN PAYETTE VALLEY, IDAHO.

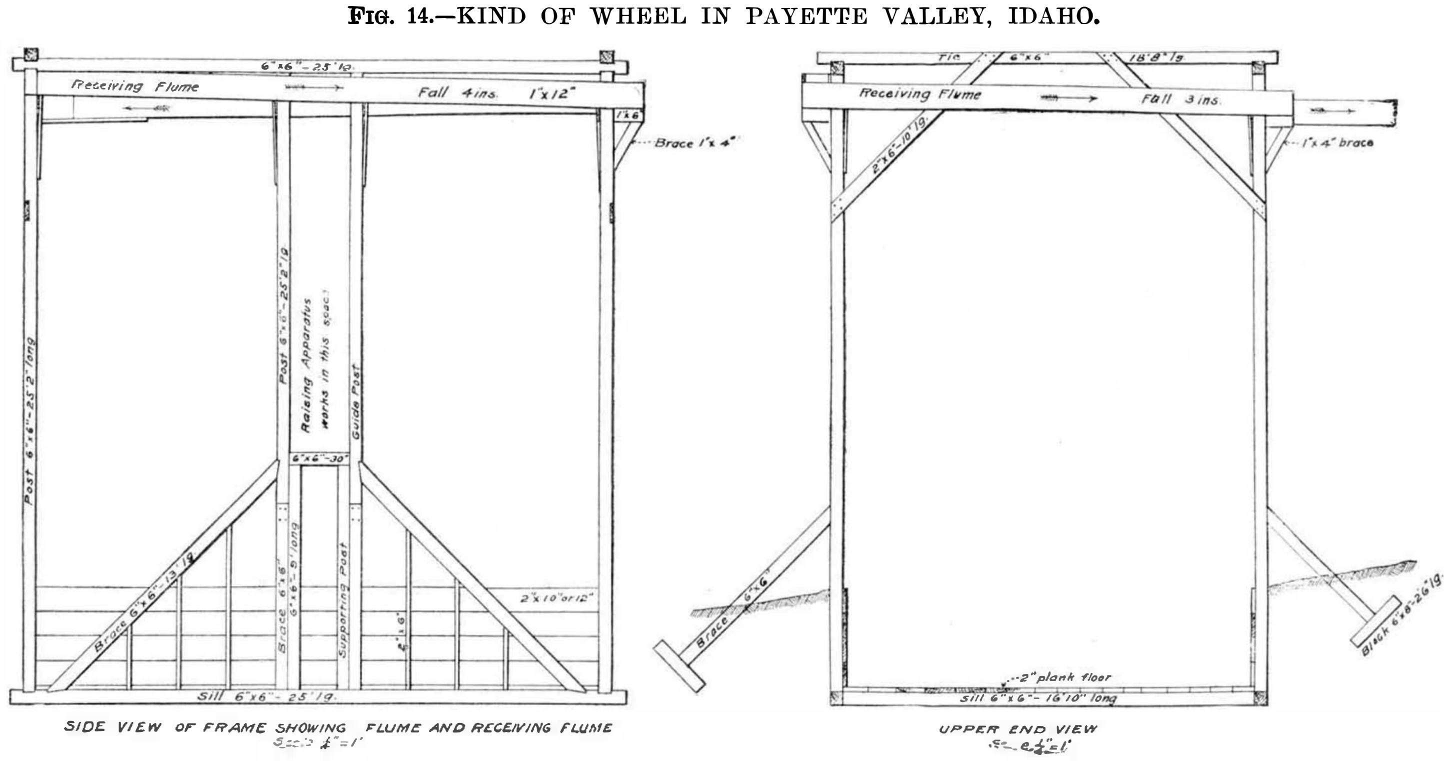


FIG. 15.—FRAMING FOR FLUME FOR WHEEL SHOWN IN FIG. 14.

* Bulletin 146 of the United States Dept. of Agriculture.

of the main arms where they cross at the center of the wheel weakens them seriously. Were this avoided by placing one set nearer the middle of the shaft, leaving space enough so that the rim of 1-inch boards could be nailed on the inside of one set and on the outside of the other, 2 by 4-inch material would be strong enough. While the 8 by 8-inch shaft would sustain a weight at the center of over 10,000 pounds, still it is none too heavy for the wheel weighing 1,600 pounds; since it is

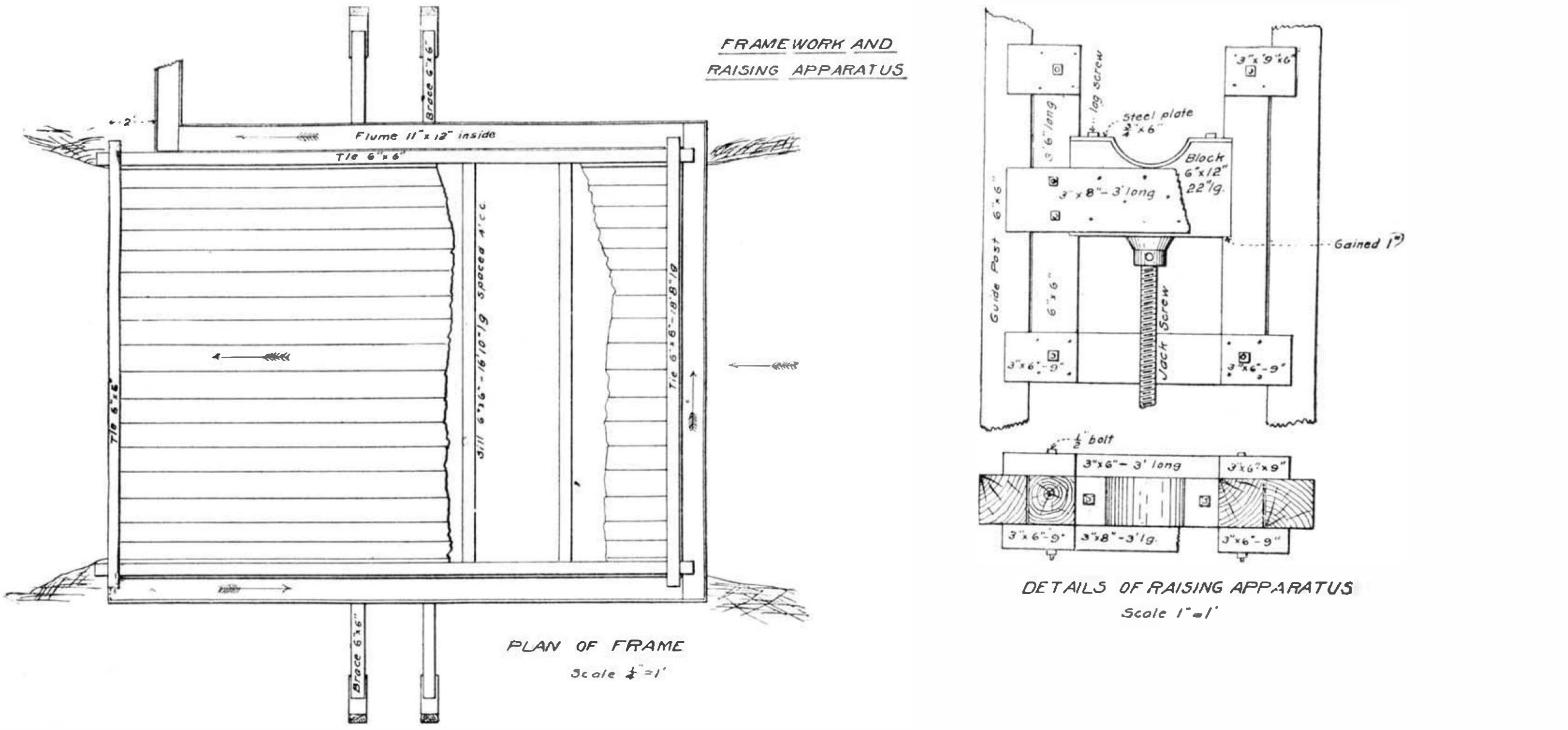


Fig. 16.—FRAMEWORK AND RAISING APPARATUS FOR WHEEL SHOWN IN Fig. 14.

evident that the friction in the bearings is greatly increased by a comparatively slight bending of the shaft.

The wheel raised sufficient water to irrigate the 2.25 acres in forty-eight hours, the water being applied four or five times in the season. It should then raise sufficient water for the successful irrigation of forty acres using the water for one hundred and sixty days.

Direct-Lift Wheels in Idaho.

In the Payette Valley, Idaho, are a dozen direct-lift wheels of the same general type shown in Fig. 14. This large wheel is very carefully made, fitting into a flume with only 2 inches clearance. The construction is shown in Figs. 14, 15, 16, and 17. The crude method of raising and lowering the wheel contrasts with its excellent workmanship. At the end of the season it is laboriously raised out of the water by jacks and is blocked up till the opening of another season. While in use it remains at one height regardless of the stage of the water.

In several ways the efficiency of this wheel could be raised. When the water is too high to run the wheel to advantage, part of it could be carried away in a second flume, leaving just enough running under the wheel to give the greatest speed. Or, better still, a "stop" could be placed in the ditch and the water run into the flume under a gate, giving it great velocity. In a great many cases a "stop" or "drop" already existing in a ditch could be utilized to good advantage in this way.

The cost of the wheel, flume, and supports was $150. For six years there were no repairs and no running expenses except for grease and for raising and lowering the wheel twice in a season. In the seventh year, 1903, repairs cost $50, mainly for a new shaft, and in subsequent years repairs will doubtless be required to the extent of $10 or $15 a year.

Twenty-five acres in alfalfa and fruit are irrigated by this wheel, the value of the crops raised being estimated at $2,337 annually.

DIAMOND TOOLS.*

By G. C. Henning, New York, Member of the Society.

Steel is, of course, the one material in almost universal use for cutting and working stone, metal, wood, and other materials, because of its great strength and the degree to which it can be hardened. There are some materials, however, which, because of their hardness, structure, or non-conductivity of heat, cannot be worked economically by means of steel tools. The latter become worn rapidly, losing their shape and dimensions to such degree and extent that the work produced becomes inaccurate, causing constant interruption of operation, loss of time, and the use of new tools or frequent regrinding or shaping of the old ones. This causes great expense and delay in production.

The great friction produced by cutting materials in some cases draws the temper of steel tools, making them useless.

Hard rubber, paper and hardened steel cannot be readily worked by use of steel tools, as is also the case with hard stone. In these cases a much harder material is required, and for this reason diamond is used. The diamond which is used is of two kinds, totally different in appearance and quality.

BLACK DIAMOND.

This has a very dark purple brown color, is an amorphous, granular stone with rarely any crystallization visible or traceable, and is called carbon or black diamond. It is the hardest material known and has great strength.

BORT.

This, called bort, is entirely crystalline, and generally transparent and of all colors of the rainbow, as well as clear and transparent as glass. The latter is considered of greater hardness than all other bort except some which is almost black. Bort is extremely brittle and is readily fractured or "cleaved" in the three directions of its cleavage planes parallel to the sides of the octahedral crystal, in which shape it is most commonly found. The dodecahedral crystals are also readily cleaved in a similar manner.

In spite of the very great hardness of all kinds of diamonds, they are readily sawn, drilled, cut and polished; carbon (black diamond) cannot, however, be polished, as is the case with bort. Diamond cuts diamond, while steel saws and drills and cast-iron disks, charged with diamond dust, are used for the other operations.

All kinds of grinding wheels, being made of extremely hard materials, are most readily kept free from filling or glazing and in perfect shape by diamond tools. In certain classes of work, where great accuracy and precision are primary requirements, or extremely fine lines are essential, the diamond is the only material that answers the purpose. Thus lithographers, engravers, and scale-makers use them for fine work. There is another very important field of production in which diamond is all but imperative to obtain satisfactory results at reasonable cost, viz., that of wire drawing. Formerly all small wire was drawn through holes in hardened steel plates, but these wear so rapidly that the wire soon loses its caliber and becomes unround. As it is all-important, especially in electric work, that the wire be of absolutely uniform size, so as to maintain constant resistance and permit symmetrical distribution of weight about spindles and shafts, it became necessary to use a material harder than steel, and hence diamond was again resorted to. This made it possible to avoid delays in replacing worn dies, and because of the great permanence of accuracy of the calibers of the holes in the diamonds, materially reduced the cost of producing fine wire of copper, brass, steel, iron, nickel, and of other metals.

It is of course well known that stone is drilled and sawed by the use of diamonds, these having been used in core drills, which, in an extreme case, have cut solid cores of about 21 inches diameter. In diamond drills, stone saws and grinding wheel dressers, the rough diamond is used in appropriate holders, set either by staking, brazing, soldering, or by casting molten steel around the diamonds. A peculiar property of the diamond is that it can be plated like any metal; this property is made use of in the galvanoplastic setting. The galvanoplastic setting consists in first plating the diamonds and then casting other molten metal around them, which alloys with the deposited metal. Thus an absolutely firm and rigid setting is produced. Very high temperature does not affect the diamonds either in their hardness nor, when sound, in their solidity, and does not produce checks or other flaws. A temperature higher than that sufficient to melt steel will, however, burn the diamond, and that of the electric arc will do so readily. The diamonds in tools used for doing accurate work are, however, all "shaped" by cutting and polishing, so as to imitate the customary shapes of steel tools.

Glass and china are also drilled by shaped diamonds, in which case a triangular splint is generally provided with a flat triangular pyramidal point, which, when using turpentine as a lubricant, penetrates glass and china more readily than any other tool, and lasts for from one to two years, unless broken by carelessness or accident.

One other reason why it is economically advantageous to use diamond tools for turning hard rubber is that very high speeds can be used, 450 to 500 feet

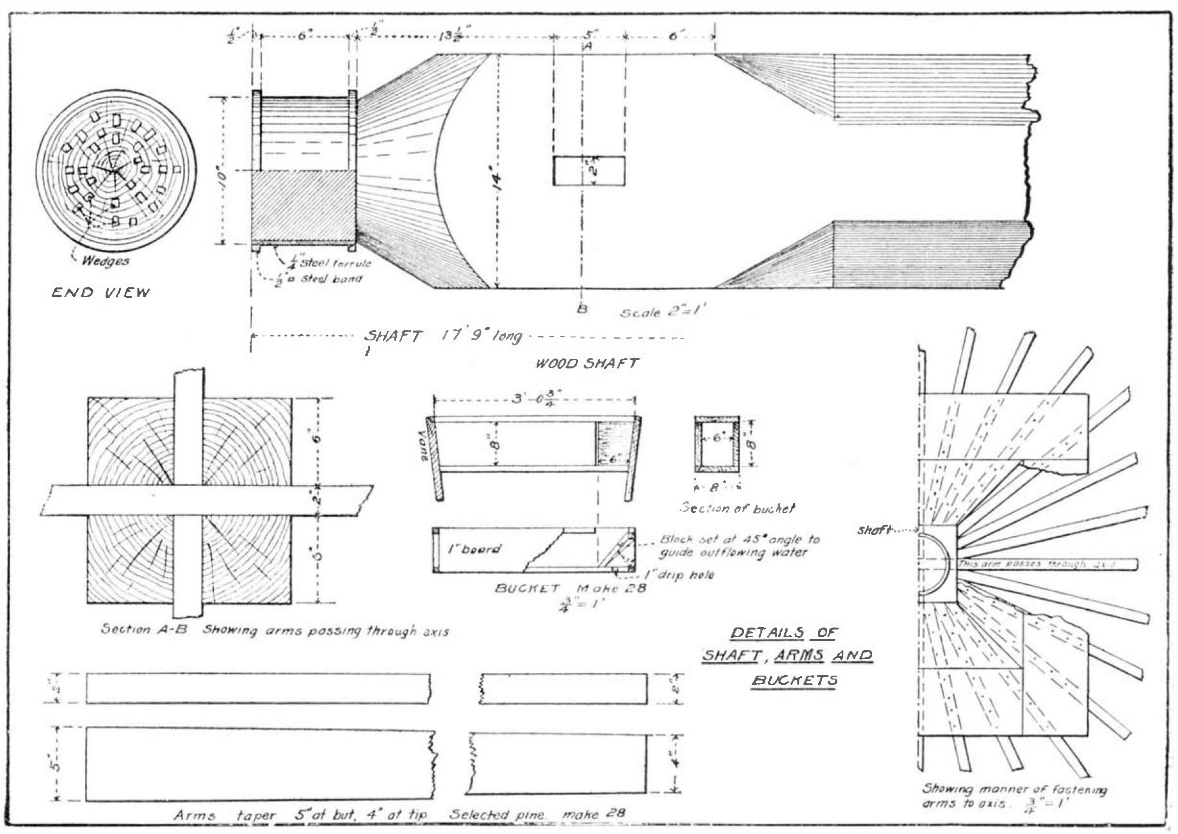


Fig. 17.—DETAILS OF WHEEL SHOWN IN Fig. 14.

* Read before the American Society of Mechanical Engineers, and forming part of Volume XXVI. of the Transactions.

per minute being common. It may here be added that diamond tools are most suitable for working carbon used for electrical purposes.

In drawing copper wire, it is customary to draw a .064 wire in one pass from a rough wire of .072 diameter. Smaller sizes are then produced by the following consecutive reductions: to .053, .045, .040, .036, .032, .028, .025, .022, .020, .019, then by 1-1000 down to .0075 and by half-thousandths down to .001. It may be mentioned that diamonds wear increasingly when drawing the following metals in the order stated, viz.: gold, silver, copper, brass, bronze, platinum, soft steel, nickel, iron and crucible steel (piano wire).

In order to show why such expensive material as diamond can be used economically it may be stated that diamond dies wear up to eight years under constant use. One die of .004 caliber has, according to the record, drawn over 550,000 pounds of soft copper wire. Diamond drills for drilling glass wear from one and one-half to two years before requiring recutting. As is well known, diamonds are also used for spindle bearings in watches, and most recently have been introduced as cupped bearings for the pivots of electric meters, because they produce the minimum of friction, and do not wear out in many years.

Another purpose for which diamonds are used is that of drilling teeth, especially artificial teeth. In these drills minute chips of diamonds soldered into steel shanks are used. These diamonds are not prepared in any manner, as their points and edges when properly selected are sufficiently hard and sharp to penetrate bone and porcelain. The shapes of chips most generally used are flat, triangular points and three-sided pyramids. The most perfect drills for this purpose have diamonds of triangular sections with a pyramidal polished point.

THE PROCESSION OF THE SUNS.*

By AGNES CLERKE.

PHENOMENA are functions of time; and the form of the function has to be determined in each particular case. That is what the historical method comes to; and its use is prevalent and almost compulsory. We can no longer be satisfied with a simple bird's-eye view of the universe; our thoughts are irresistibly driven to grope into its past, and to divine its future. Static conceptions sufficed for our intellectual forefathers. They aimed at establishing the equilibrium of things, while we see them in a never-ending flux. One aspect of them calls up the next, and that another, and so on *ad infinitum*; we cannot, if we would, balance our ideas on the pivot of the transient present.

The immutable heavens of the ancients strike us to-day as the invention of a strange race of beings. We, on the contrary, see them with Shelley as a "frail and fading sphere"—a "brief expanse," the seat and scene of change. The "fixed" stars long ago broke away from their moorings, and began to flit at large through space. Of late, a less obvious, more intimate kind of mobility has been attributed to them. Grooves of individual development seem prepared for them, along which they shift as the tardy ages go by; and since everything that grows must decay, the orbs of heaven, too, incur the doom of mortality. Modern science, however, has done much more than extend to them the dismal philosophy of the phrase, "*tout passe, tout casse, tout lasse.*" The grandiose enterprise has been not unsuccessfully essayed of tracing in detail the progress of sidereal evolution, and of marshaling the vast stellar battalions in order of seniority. This has been rendered feasible by the disclosures of the spectroscope. Apart from their guidance, the track might have been glimpsed here and there, but could never have been laid down with any approach to definiteness. Herschel found for it a *terminus à quo* in nebulæ of various forms, but attempted to pursue it no further. We do not hesitate to run it on, from station to station, right down to the *terminus ad quem*—not, indeed, without the perception of outstanding difficulties and insecurities. They appear, however, to be outweighed by a certain inevitableness of self-arrangement in the visible facts.

The argument from continuity is that mainly relied upon. An unbroken succession of instances is strongly persuasive of actual transition, provided only that a principle of development (so to call it) may reasonably be assumed as influential. A series of mineralogical specimens, however finely differenced, does not suggest the progressive enrichment of one original mass of ore. In the stars, on the other hand, a species of vitality may be said to reside. They are not finished-off products, but self-acting machines. They are centers of energy, which they dispense gratis, supplying the cost out of their own funds. And the process is not only obviously terminable, but must be accompanied by constitutional alterations, which might be traceable by subtle methods of inquiry. They are traceable, unless we are deceived by illusory appearances.

Secchi's classification of the stars was unwarped by any speculative fancy. It was purely formal; it aimed only at providing distinct compartments for the convenient arrangement of a multitude of differently characterized items of information. Then by degrees, the close gradation of one class into the next came to be noticed; the partitions melted away; the methodized array showed itself to be in movement; and the bare framework took shape, under the auspices of Zöllner and Vogel, as a cosmic pedigree. The white stars were set forth as the progenitors of yellow, yellow of red stars; and the insensibly progressive reinforcement of the traits of relationship between the successive types went far toward demonstrating some partial, if not a complete, correspondence of the indicated order with the truth of things. It has since been found necessary to divide the first stellar class into helium and Sirian stars; and here, too, essential diversity shades off imperceptibly into likeness approximating to identity. All the groups hang together; the entire scheme is on an inclined plane of change. Helium stars, as they condense, pass into Sirian, these into solar stars; which finally, reddening through the increase of absorption, exhibit the badge of post-meridional existence in fluted spectra. The finality of the red stage is, indeed, very far from being absolute, but what lies beyond is matter of conjecture.

There are several good reasons for taking helium stars to be the "youngest," or most primitive of the amazing assemblage that sparkle in the vault of the heavens. The first is their affinity with nebulæ. Every star, perceived to be involved in folds or effusions of shining haze, has yielded—if bright enough for profitable examination—a spectrum of helium quality. Further, they are remarkably tenuous bodies. It has been ascertained with some definiteness, from the investigation of stellar eclipses, that helium stars are commonly, perhaps invariably, of far slighter consistence than the sun. Radiation, however, is maintained by contraction; hence, orbs at the outset of their course must be, on the whole, the most diffuse. A third note of youth is membership of embryo systems; and this is affixed very markedly to helium stars. One-third certainly, probably one-half of those lately submitted to trial by Profs. Frost and Adams proved to have spectroscopic companions. They are pairs believed to have been recently (in the cosmic sense) divided by fission. And this is an operation which must, we should suppose, be undergone early, or not at all.

The spectra of helium stars are peculiar and suggestive. Those belonging to Miss Maury's earliest groups —many of them visibly nebulous—bear next to no traces of metallic absorption, showing instead lines of oxygen, nitrogen, and of hydrogen in all its three series. The conditions, accordingly, needed to produce the "cosmic" modification of hydrogen are realized in these inchoate bodies. What those conditions actually are, we cannot tell; yet it may be confidently surmised that they will prove to be of an electrical nature. Hydrogen resembles the metals in being electro-positive; it collects at the negative pole during the electrolytic decomposition of water. There is, however, an unmistakable tendency in primitive sidereal objects to display absorption-rays of electro-negative rather than of electro-positive elements. It is conceivable that hydrogen may be capable of altering its behavior in this respect; and that the molecules radiating the Pickering and Rydberg series, in addition to the more familiar Huggins series, have, in fact, through some corpuscular re-arrangement, assumed the electro-negative quality properly characterizing a non-metallic substance. The association of this form of hydrogen with oxygen and nitrogen in early helium stars could thus be more naturally related to the simultaneous quasi-disappearance from them of the spectral badges of metals.

The helium-line most distinctive of this stellar family is situated well up in the blue. It appertains to the same vibrational sequence with D_3, which is also represented, at any rate in Rigel, a somewhat "advanced" Orion-star. Here, too, we meet a fairly prominent magnesium-ray, lying below the blue helium emanation; while as yet iron is unapparent. Numerous fine, faint streaks, due to its absorption, emerge, however, when the Sirian type is fully reached, and they are mostly of the "enhanced" kind. When the spark-discharge is substituted for the arc as the source of illumination, certain lines in the resulting spectrum brighten relatively to the others; and these have been distinguished by Sir Norman Lockyer as "enhanced." Now, the rule is strikingly prevalent that the absorption-rays in white stars are of this class; yet it can no longer be interpreted as indicating for them an excessively high temperature. Rather, it would seem that electrical conditions, still imperfectly defined, are in question; and their gradual removal, or subsidence, is, beyond doubt, largely instrumental in bringing about the transition to the solar stage. The effacement of helium-absorption is even more perplexing. No sooner does iron begin to show than it vanishes. There is still a faint trace of its "blue" line in Vega; none survives in Sirius.

In spectra of the solar type, two great bars of violet light are stopped out by calcium; otherwise, metallic arc-lines predominate, while those of hydrogen are no longer so powerfully emphasized as in white stars. Moreover, the whiteness of the unveiled Sirian photospheres has become tinged with yellow owing to the development of a shallow envelope partly impermeable to blue rays. For this reason, the comparative extension of their ultra-violet spectra affords, for stars of different types, no secure criterion of relative temperature. Sound in principle, it becomes inapplicable when the unknown factor of general absorption comes into play. The energy-curve of the solar spectrum as it is, can be determined; the energy-curve of the solar spectrum as it would be if unaffected by general absorption, has to be constructed from inference. But only bare photospheres give congruous results. Hence, there are no valid grounds for asserting that Sirius is hotter than the sun, or the sun than Betelgeuse. It may be so, but the evidence at present available is inconclusive. The appearance expounded in this sense may bear quite different meanings.

The reasons for holding that solar stars mature into Antarian stars are of the same nature, and of equal cogency with those tending to prove their own development from luminaries of earlier types. There is a similar continuity of specimens. They can be ranged one after another in an unbroken series, in which, as we descend the line, primrose shades into orange, and orange into red, general absorption arrests an increasing percentage of the blue radiations, while specific absorption becomes strengthened by dusky flutings of titanium. Carbon-stars are less easily located. Dr. Vogel regarded them as co-ordinate with the Antarian class. The two varieties of red stars with banded spectra descend, in his opinion, from the common stock exemplified by our sun. Prof. Hale also favors this view, some attendant anomalies notwithstanding. His photographs have certainly established for carbon-stars links of relationship both with the Antarian and the solar families; yet the fact remains indisputable that the carbon type is, to a great extent, isolated from all the rest. Tokens of a genuine migration toward it are few and obscure.

The ultimate fate of both tribes of red stars can only be conjectured. Most vary in brightness, some to the verge of periodical extinction; and variability may be a symptom of interior dilapidation. The constitution, however, of such objects is still enigmatical. They appear to be exceptionally remote and inaccessible to inquiry. No indications have been gathered as to their density or intrinsic light-power. Very little is known about their movements. They rarely form binary combinations, and those that they do form are almost always relatively fixed. No red star travels in a computed orbit; only one, η Geminorum, occurs on the long list of spectroscopic binaries. The revolutions of this curious system ought to prove, when thoroughly investigated, of high interest and instruction.

Coupled stars offer special opportunities to students of cosmogony. They are obviously contemporaries; they have started fair; identical influences have acted upon them; hence differences in their standing can only result from dissimilarities in mass or composition. It is commonly taken for granted that a body containing less matter than its fellow must develop faster, and incur the final quenching sooner. But Sir William and Lady Huggins have adverted to the probability of the very opposite being the case. Powerful surface gravity may, they consider, serve to hasten the transition from a Sirian to a solar spectrum; and we should then have giant suns like Capella advanced in type while at a very early stage of condensation. This, perhaps, explains the remarkable spectral relations of contrasted stellar pairs. Always, so far as we yet know, the Sirian spectrum is yielded by the lesser star, the mass of which, judging by analogy, must be small even below the proportion of its faintness. It is true that the distribution of mass in binary systems is often widely different from what might have been anticipated. Certain purplish satellites, for instance, of undetermined spectral quality exercise a gravitative sway of surprising force. Some results of this kind, lately obtained by Mr. Lewis and others, are likely to prove of fundamental importance to theories of stellar evolution.

What we know of "dark stars" has been mainly derived from the observation of stellar systems. They are assumed to be the denizens of a stellar Hades, dim wanderers amid the shades, who "have had their day, and ceased to be" as suns. In the "cold obstruction" of these viewless orbs the grand cosmical procession is held to terminate. Their presence attests the downward progress of decay, and gives logical completeness to the argument for development. Yet there are circumstances warning us against too full an assurance that their status is really that of skeletons at the feast of light. They are very frequently found to be in close attendance upon brilliant white stars. Thus intimately, if incongruously coupled, they circulate, and compel circulation in brief periods, as members of systems just, it might be said, out of the shell. What are we to think, for instance, of the obscure body spectroscopically discovered to control the revolutions of the chief star in the Orion trapezium? It is evidently comparable in mass with that imperfectly condensed luminary; is it credible that it has already traversed all the stages of stellar existence, and cooled down to planetary rank? So violent an assumption should, at any rate, not be made without due consideration; and we may more prudently hold our judgment in suspense as to whether globes so circumstanced—and they abound —should be regarded as effete, or as abortive suns.

Speculations on the exhaustion of stellar vitality have, however, lately become inextricably involved with the complex problem of elemental evolution. A dim inkling has been acquired of the working in the universe of obscure forces, availing, we can just see, to falsify many forecasts. The theory, at least, of the dissipation of energy needs important qualifications. Nor was it propounded by Lord Kelvin with dogmatic certainty. He carefully noted the possibility that in "the great storehouses of creation" reserves of energy might be provided by which the losses incurred through radiation could be, wholly or in part, made good.* The anticipated possibility is, perhaps, realized in the phenomena of radio-activity. But if we inquire how, we are met at the threshold by difficulties connected with the origin of helium. Helium appears to result from the disintegration of radium, its generation being accompanied by the setting free of enormous quantities of energy. Its copious presence,

* Knowledge.

* Thomson and Tait, "Natural Philosophy," Appendix E, p. 494, edition 1890.

then, argues long-continued and lavish expenditure of heat and light. Yet it is as a constituent of highly primitive orbs that it is chiefly conspicuous. Gaseous nebulæ, too, include immeasurable supplies of it, while it is incompatible with whatever we seem to know about them to suppose that radium at any time entered into their composition. In truth, however, the genesis of the elements has not yet been made the subject of coherent speculation. Current ideas regarding it imply a double course of change, by aggregation first, and subsequently by disintegration. And this should give us a two-fold series of elements. On one side, there should be fixed survivals of the advancing process, on the other, products of decomposition, continuously evolved, and even now accumulating. If the claim of helium to take rank among these last should be finally established, our conceptions of the nature and history of nebulæ might have to undergo a strange inversion; but the outcome of the researches in progress is still uncertain, and may be far off.

It is, however, quite clear that the electronic theory of matter supplies no genuine explanation of the source of energy in the universe. What is given out when the atoms go to pieces must have been stored up when they were put together. Whence was it derived? This is the fundamental question which underlies every discussion concerning the maintenance of the life of suns. It is unanswered, and probably unanswerable.

MYSTERY OF THE MONOLITH.

Among the greatest mysteries in the mystery of human history on earth, the monoliths that stand in various parts of the globe have always been chief. These strange stone monuments, generally rude, with hardly any sculpture, but wonderfully large in size, are scattered everywhere. The most famous of them are the cromlechs, such as those in Stonehenge, in England. Others are found in the western part of France, in the northern part of Germany as far as the Oder River, in Denmark, and in Sweden. Northern Africa, Madagascar, and Asia, from Mount Sinai and the Caucasus to India, all have some here and there. Lately they have been discovered even in Siberia. There are several places in Japan where such stones stand. Others have been found in North and South America. Among the most wonderful and mysterious of them all are the vast, almost terrifying, stones, rudely sculptured, that have been found on lonely Easter Island in the Pacific Ocean. The mystery as to what these stones meant was not the only puzzle. The greatest puzzle was how primitive people without knowledge of engineering and without known appliances could have moved and erected such giant blocks, some of which weigh as much as two hundred and fifty tons. In many districts where the great memorials stand there are no stones of such size to be found in the earth. Consequently they must have been transported from places long distances away. For many generations, ever since men have spent any time in thinking over these matters, scientists have puzzled their heads over the monoliths in vain. Whether they classed them as all being the same or whether they divided them into different classes, they remained the same riddle. Lately, however, there has been a systematic study of all the monoliths of the world, and as a result a novel theory has been advanced. It is that one race—that of the Phœnicians—erected all these monuments. Of course, if this is correct, it means that history will have to be revised, for it would show that the Phœnicians once knew all about the whole globe, and that they had visited America and Asia long before modern days. The first thing that led to the assumption that the Phœnicians had raised the monuments was the observation of M. Levistre, a French archæologist, that almost all of the monoliths were to be found near the mouths of great rivers or along their courses. This pointed to a people that used ships. Now the Celts were never strong on the water. Their boats were primitive, being made mostly of wickerwork, covered with hides, and they were unable to move any distance from land in these clumsy and unseaworthy contrivances. Consequently, when the question arose as to what race was advanced in navigation in that early time when these monuments were put up, the answer naturally came that the only race known to be a race of sailors and merchants then was the race of Phœnicians. And if they were the Phœnicians, how did they come to reach America? They had colonies on the islands of the Western Mediterranean, and even in the islands of the Atlantic Ocean, as history proves. It is not hard to imagine that they may have found their way to America. Some archæologists are even willing to believe that they reached America by way of the lost Atlantis, that legendary continent that is now sunken somewhere in the Atlantic Ocean, if it ever existed. At any rate, the theory that the Phœnicians were the erectors of the monoliths of the world to commemorate discoveries or for religious purposes, has gained strength through the recent discovery of a great upright stone near the River Loire, which has Phœnician words engraved deeply on it. Translated, these words say: "On this spot was slain our brave comrade." In San Luis, in Bolivia, is one of these stones, which has on it the engraving of a footprint and a snake with its head raised. The footprint is a well-known Phœnician symbol of death, and is found on many stones that cover ancient Phœnician burial sites. And the snake with head raised is another acknowledged Phœnician emblem. It signifies a march forward. Exactly the same symbols as those on the South American stone have been found on a stone in Central France.—**Chicago Chronicle.**

SCIENCE NOTES.

Mr. Seth Smith is the first to have succeeded in breeding the Tataupa Tinamou (*Crypturus tataupa*) in confinement, and as nothing was hitherto known of its habits at this time his short description thereof in the Avicultural Magazine for August is of considerable interest. The eggs are incubated by the male only. From the moment he began to sit the female resigned all interest in the matter; indeed, if she approached, her mate rushed at her open-mouthed so that she fled in terror! Before leaving them the eggs were most carefully covered up. After the escape of the young from the shell the male broods them for some hours before bringing them into the open. The female does the courting, calling to her mate and then running to him, and displaying in the most curious attitudes. When alarmed these birds adopt the peculiar device of throwing themselves forward on the breast and throwing the tail in the air so that the under-tail-coverts form a screen to hide the body, which in consequence becomes hard to distinguish from the surrounding herbage and undergrowth. Even very young chicks, when they suspect danger, squat and turn up their sprouting tails, but whether instinctively or in imitation of the parents the author does not say.

A rose which has created a great deal of interest in horticultural circles is the subject of one of the plates in the September number of the Botanical Magazine. The late Sir Henry Collett met with this rose, to which he gave the name of *Rosa gigantea*, as a very striking object in the forests of the Shan Hills in Northern Burma, and it was through him that seeds were received at the Royal Botanic Gardens, Kew, in 1888. No difficulty was experienced in getting the seeds to germinate, and the seedlings soon developed into plants remarkable for the enormous length of their shoots, one of these in the Temperate House reaching a length of fifty feet. Visitors to the Succulent House may have observed the robust specimen planted in the central bed there, which had grown along the roof, and then out through a ventilator into the open air. But though growths were produced in almost embarrassing freedom, no flowers have ever been borne by the Kew plants. Indeed, it is believed that only in two gardens in this country has the plant flowered at all. From one of these—Albury Park, Guildford—the material was obtained from which the Botanical Magazine drawing was prepared. The flowers are white, or white tinged with yellow, and are from four to six inches in diameter. The same rose was found first in Manipur, and it is now known to occur in Western China.

The Zoologist contains some extremely interesting notes on the range of variability in the weight of eggs of wild birds, which is much greater than one would have imagined. The eggs of the Charadriidæ were used to furnish the material for this investigation, and the strictest care was used to select only unincubated eggs, thus eliminating the error due to loss from this cause. The weight of the whole clutch, and not of single eggs, is given. Altogether, about a dozen species have been studied in this connection, and four or five clutches of each species have been weighed. In *Aegialits hiaticula*, the lightest clutch weighed 45.148 grammes, the heaviest 50.450 grammes; in *Charadrius pluvialis* the differences were 130.167 grammes and 151.299 grammes; in *Vanellus vulgaris*, 106.621 and 117.434 grammes; in *Numenius arquatus*, 320.114 and 348.116 grammes; in *Totanus calidris*, 82.164 and 92.687 grammes. It would be interesting to compare the relative differences in weight between the birds of the species enumerated and their egg clutches, and to note the difference between the activity of their young on hatching. So far, no one seems to have noticed whether this differs to any appreciable extent among the different species of Charadriidæ.

Mr. Bertling, in the August number of the Avicultural Magazine, concludes his notes on the breeding of the brush turkeys (*Talegalla lathami*) in the Gardens of the Zoological Society.

His account, though short, is extremely interesting, and of considerable scientific value.

Some time since, these birds constructed a mound of the usual type, and deposited therein a number of eggs. The nestlings being overdue, it was at last decided to at least partially explore the mound, and this resulted in exposing three eggs. These lay about one foot apart from each other, and some 18 inches from the surface. They were placed with the large end upward, and had certainly not been turned, as a deep hole, of the shape of the egg, was left on its removal. Moreover, the egg did not touch the bottom of the hole, inasmuch as the small end was quite white, while the rest of the shell was stained by contact with the mold.

A further search revealed a chick, evidently dazzled by the sudden glare of the light. The "quill" of this bird were nearly three inches long, and as it could fly fairly well, he says, "I have come to the conclusion that the young remain at least thirty-six hours, or longer, in the mound before making their appearance, as three others, hatched in an incubator, were not nearly so advanced when hatched."

The shell is very thin, so that the young do not chip around the upper part of the egg in order to make their escape, but appear to shatter the walls of their prison by giving a violent wriggle. They do not immediately obtain freedom, however, but still remain incased in the inner membrane of the shell, which is ruptured some hours afterward.

When first hatched the primaries and secondaries are ensheathed in a "thin filmy covering" which gives the wings the appearance of being still undeveloped, but directly the chick dries this membrane peels off, leaving the bird ready for flight.

At three weeks the black feathers of the adult plumage are distinctly visible through the "down," and at six weeks the birds are almost indistinguishable from the parents.

TRADE NOTES AND RECIPES.

Transparent Cement for Glass.—Digest together for a week in the cold, 1 ounce of india rubber, 67 ounces of chloroform, and 40 ounces of mastic.—Hannoversches Gewerbeblatt.

Color Stamps for Rough Paper.—It has hitherto been impossible to get a satisfactory application for printing with rubber stamps on rough paper. Fatty vehicles are necessary for such paper, and they injure the india rubber. It is said, however, that if the rubber is first soaked in a solution of glue, and then in one of tannin, or bichromate of potash, it becomes impervious to the oils or fats. Gum arabic can be substituted for the glue.—Hannoversche Gewerbeblatt.

To Remove Aniline Stains from Ceilings, etc.—In renewing ceilings, the old aniline color stains are often very bothersome, as they again and again pentrate the new coating. Painting over with shellac or oil paint will cause relief, but other drawbacks appear. A very practical remedy is to place a tin vessel on the floor of the room, and to burn a quantity of sulphur in it after the doors and windows of the room have been closed. The sulphur vapors destroy the aniline stains, which disappear entirely.—Deutsche Maler Zeitung.

Restoring Faded Manuscripts.—In the library of the city of Breslau there were several old manuscripts from the sixteenth century which had become entirely illegible through age and dampness, and had faded almost entirely. The chemical laboratory of the university was asked for advice what to do with them. The latter ascertained, as was to be expected, that gall-nut ink had been used in the writing. The manuscripts were coated with a 1 per cent. alcoholic solution of tannic acid, whereupon the writing became again quite visible. A subsequent treatment with ammonia or sulphite caused the writing to reassume its whole sharpness.—Technische Mittheilungen.

Manufacture of Glue.—The usual process of removing the phosphate of lime from bones for glue-making purposes by means of dilute hydrochloric acid has the disadvantage that the acid cannot be regenerated. Attempts to use sulphurous acid instead have so far proved unsuccessful, as, even with the large quantities of it, the process is very slow. According to a German invention, this difficulty with sulphurous acid can be avoided by using it in aqueous solution under pressure. The solution of the lime goes on very rapidly, it is claimed, and no troublesome precipitation of calcium sulphite takes place. Both phosphate of lime and sulphurous acid are regenerated from the lyes by simple distillation.—Chemiker Zeitung.

To Produce Stuccoed Flowers from Gypsum, etc.—Take natural flowers, and coat the lower sides of their petals and stamens with paraffine or with a mixture of glue, gypsum, and lime, which is applied lightly. Very fine parts of the flowers, says the Deutsche Maler Zeitung, such as stamens, etc., may be previously supported by special attachments of textures, wire, etc. After the drying of the coating the whole is covered with shellac solution or with a mixture of glue, gypsum, lime with lead acetate, oil, mucilage, glycerine, colophony, etc. If desired, the surface may now be painted with bronzes in various shades. Such flowers are now much employed in the shape of festoons for decorating walls, etc., and are very handsome.

Treatment of Cast-Iron Grave Crosses.—The rust must first be thoroughly removed with a steel-wire brush. When this is done, prime the cross with red lead or graphite paint, twice preferred. After this priming has become hard, coat the cross with a paint from double-burnt lampblack and equal parts of oil of turpentine and varnish. This coating is followed by one of lampblack ground with coach varnish. Now prime the single portions with "mixtion" (gilding oil) and gild as usual. Such crosses look better, says the Deutsche Maler Zeitung, when they are not altogether black. Ornaments may be very well treated in colors with oil paint and then varnished. The crosses treated in this manner are preserved for many years, but it is essential to use good exterior or coach varnish for varnishing, and not the so-called black varnish, which is mostly composed of asphalt or even tar.

Wood Staining.—Thin wood is dyed by heating it in a bath containing 1-5 to 1-2 pound of dye to every 25 gallons of bath, according to the dye. The heating is done under from 7 to 14 pounds pressure according to the closeness of the grain of wood, and has to be maintained for from two to six hours. The wood is then dried, sandpapered, and polished. Large masses of wood, which cannot be heated in a boiler, are mordanted with soap or tannin, the former if acid dye is to be used, the latter if a basic dye. The dye is then applied with a brush. Three coats are usually needed to get a uniform color. The wood is then dried and polished. To get black on small wood, mordant with caustic soda lye of 10 per cent strength, and dye under slight pressure in a bath of logwood, pyrolignate of iron, nigrosine, and acetic acid, for twelve hours. Gray is obtained by soaking the wood for a few days in a cold dilute solution of pyrolignate of iron. The same procedure applies to large wood, but the liquids are applied with a brush.—Hannoversches Gewerbeblatt.

ENGINEERING NOTES.

The power consumption of the Glasgow tramways per car mile is now about 1,030 watt-hours. There are about seventy miles of double track, serving a population of over 750,000.

A correspondent of the Woodworker says that a good rust joint mixture is made of ten parts of iron filings (or borings) to three parts of chloride of lime. Mix into a paste with water, apply to joint and clamp together. In twelve hours it will be set solid.

The old New Bedford whalers are mines of copper wealth according to Marine Engineering. The timbers were heavy and white oak put together "on honor," which apparently meant the liberal use of copper fastenings. It is said that from $2,000 to $3,000 worth of scrap copper is often realized when one of these old vessels is broken up.

The German Society of Mechanical Engineers offers a prize of 6,000 marks for a treatise on locomotive construction embracing the theoretical discussion of its fundamental principles. Its announcement says that in spite of the many treatises on the locomotive, there is still lacking an exhaustive theoretical discussion of the thermal, mechanical, and geometrical principles involved, such as may serve as a guide in designing engines for special purposes.

In a recent interview Mr. Charles M. Schwab, the former president of the United States Steel Corporation, said that in 1879 the steel production of this country was 1,000,000 tons per annum; in 1889 it had increased to 7,000,000 tons; and in 1899 it was from 12,500,000 to 15,000,000 tons. The consumption is now 15,000,000 to 16,000,000 tons a year, and in ten years will be over 20,000,000 tons. The consumption of steel rails required to replace those worn out is 2,500,000 tons annually, and 600,000 tons are used in the manufacture of steel cars.

Near Pontefract (France) lives a banker who has a museum of old doors. They are from old houses, castles or abbeys that have some historic interest. Quite lately he bid $5,000 in Paris for the door through which, during the French Revolution, Marie Antoinette, Charlotte Corday, Danton, and Robespierre passed to the guillotine. One of his doors is said to have shut off Charles II. from his Roundhead pursuers, and it bears marks of a battering ram. A collection of ancient weathercocks is also one of this gentleman's possessions.

Men get the notion into their heads that you cannot run woodworking machinery fast enough, and speed up their planers and saws to the last limit. This is probably the poorest kind of policy. Saws running above a normal speed will not run as easy or do as good work as at much lower speed. If any one does not think so, let him take a common bench saw, speed it up high and feed it my hand himself. He will soon find that the stock pushes hard. Then he concludes the saw is dull. After filing, he tries it again, with the same result. The trouble is, one cut follows the other too quickly and glazes it over. Every woodworking machine shows the same effect following a too high speed. There is a normal speed for saws and planers.—Woodworker.

The liquid condition of petroleum is ordinarily an advantage when considered for fuel purposes, but sometimes it is a decided disadvantage, especially when it is to be used temporarily in coal furnaces or where the conditions are such that the liquid fuel is a grave fire risk. Considerable attention has been given to briquetting petroleum so as to make it available where liquid fuel is not permissible. A recent consular report gives a brief outline of a German process for petroleum briquetting which appears simple and apparently requires practically no machinery. To one quart of petroleum is added soft soap, 150 grains; rosin, 150 grains; and caustic soda lye wash, 300 grains. The mixture is heated and thoroughly agitated. This takes about forty minutes and during this time care must be taken to prevent the liquid running over; this is achieved by adding a little soda. The mixture is poured into briquette molds to solidify and allowed to cool, after which the briquettes are dried in an oven for an hour or two.

A new process of galvanizing, known as "Sherardizing," has been developed abroad to a commercial basis, which promises to overturn the present hot galvanizing process used on iron and steel. By the new process the work is covered with an even coating of zinc without dipping it into a molten bath, and it is done at considerably less than the melting temperature of zinc so that the deteriorating effect of high temperature is considerably reduced. The work to be galvanized is thoroughly cleared of surface oxide or rust by sand-blasting, acid bath, or other preferred means, and is then placed in an air-tight cast-iron muffle or oven charged with the zinc dust of commerce. After being kept at a temperature of from 500 to 600 deg. F. for a few hours, the work is removed and allowed to cool. The process of coating with zinc has been perfectly effected during the baking process; the thickness of the coating of zinc depends upon the time the work is kept in the muffle. The galvanizing is thus done at a temperature about 200 degrees less than that necessary by the hot bath process; moreover, it has the advantage of wasting none of the zinc. The waste of zinc by the common process is a quite considerable percentage of the total amount of the bath. Although common bar zinc melts at something over 700 deg. F., the zinc dust of commerce does not melt at an even much higher temperature, so that there is no danger of its melting in the muffle.—Machinery.

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