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DEVOTED TO

SCIENCE AND THE MECHANIC ARTS.

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Mining and Metallurgical Section.

Stated Meeting, held Wednesday, November 13, 1901.

UPON THE CONSTITUTION OF BINARY ALLOYS.

By JOHN ALEXANDER MATHEWS, PH.D.

HISTORICAL INTRODUCTION.

In our museums and collections may be found metallic weapons, coins and utensils of various sorts whose antiquity is undoubted. Many of them long antedate the Christian era, and show that the people of those remote times and civilizations had an empirical knowledge of the art of metallurgy in both its branches, *i. e.*, they knew how to extract metals from their ores, and they knew how to fit them for use. To be sure, some metals occur native, but it is impossible to suppose that all the metals used by the ancients were of this kind. In the Old Testament scriptures, gold, silver, copper, iron, lead and tin are mentioned, and also brass, though copper or bronze is probably meant.

The art of refining in furnaces seems to have been known long before the Christian era. Pliny and Dioscorides give fairly accurate accounts of the smelting operations of their times, but do not explain or attempt to explain the chemistry of these processes. The behavior of mercury and gold—that is, the general facts concerning amalgamation—was known long before Pliny's time, and cupellation, as a refining process for gold, was certainly practiced before the second century B.C. The method of separating silver from gold by a process of cementation, in which the silver was "sweated out" by heating the gold silver alloy with salt and alum-shale, originated before the time of Christ, and is mentioned by Pliny.

Cupellation for the sake of quantitative estimation of gold and silver in alloys seems to have been first introduced by Roger, Bishop of Salisbury, in the reign of Henry I, early in the twelfth century. It must be remembered that in those days, and even for centuries later, the right to coin money was conferred by contract upon private individuals. It had also been the custom with those favored gentlemen to amass great fortunes by debasing the coinage of the realm and to escape punishment by occasional costly gifts to their king. When Roger applied cupellation as a method of assaying coins, the coiners were called to account, and of fifty examined, only four escaped punishments, which were doubtless more effectual than humane.

We have already mentioned bronze and gold-silver alloys; the cupellation process implies the existence of lead-gold and lead-silver alloys. Tin, though of relatively early discovery, was known in its alloys still earlier than as a distinct metal. Its alloys with lead were used as solder by the Romans. Brass, the most useful of all alloys, save only steel, was described by Aristotle, but was not recognized as an alloy. The people of his time knew that when copper was fused with a certain earth (zinc ore) its color was changed. It became more yellow and golden in appearance, and this phenomenon doubtless had considerable influence in strengthening the faith of the alchemists in later centuries in the doctrine of the transmutability of metals.

In succeeding centuries, such metallurgists as Geber, Biringuccio and Agricola wrote, speculated and experimented much upon the phenomena of reduction and oxidation of metals. Nearly three hundred and fifty years ago the Royal Society took up the problem of the oxidation of molten lead in air, and discovered that its increase in weight was due to some constituent abstracted from the atmosphere. During all the centuries down to the phlogistic period, other metals were discovered and other alloys produced either by chance or design; and the profound changes which metals undergo when heated together were probably recognized by the shrewd observers of all ages. In the phlogistic period, which may be said to comprise the end of the seventeenth and most of the eighthteenth centuries, metallurgy had so advanced that a crude explanation of the distinction between wrought-iron, cast-iron and steel had been made. It does not appear, however, that any very decided opinions as to the constitution of metallic mixtures were advanced at this time and, indeed, one could scarcely expect that the profound questions of molecular physics, which a study of alloys involves, could have been solved or even approached before the announcement of a molecular theory of the constitution of matter, nor could it have made any progress earlier than the period of chemical reform instituted by Lavoisier, and properly known as the period of quantitative investigation.

Just at this time, however, when all conditions seemed favorable for investigating the molecular constitution of metals and alloys, Dalton announced the atomic theory and the law of simple numerical proportions. Thus, the great aim of the early chemists, that of determining the exact composition of substances, was directed toward bodies which were recognized as true chemical compounds whose composition might be expected to confirm Dalton's law. Alloys manifestly did not, in many instances, come under the provisions of the atomic theory, and hence were neglected by chemists and largely, too, by physicists. More recently the relations of physical properties to chemical constitution have attracted chemists and physicists alike, but in this field of investigation, alloys were last to receive serious attention or, at least, to yield fruitful results. The physical methods successfully applied to bodies of other kinds failed utterly to bring to light a rational explanation of the molecular conditions existing in alloys. The solution of these problems required new methods and new apparatus. These have been slowly forthcoming, and the past decade has been most fruitful of results, and problems of inestimable practical importance and extreme difficulty have been solved.

MODERN ALLOYS RESEARCH.

With a few exceptions, the great leaders in this work are a contemporary school of English and French metallurgists and molecular physicists. In the pages that follow, we aim to give a résumé of their achievements, and to outline our present knowledge of the nature and constitution of binary alloys. There are three important factors which account for the recent progress in alloys research. (1) increased knowledge upon the subject of solutions. (2) the development of the science of matallography. (3) improvements in pyrometry. To these we might add a fourth, namely, the official recognition of the importance of investigation along these lines which, over ten years ago, led the Institution of Mechanical Engineers of Great Britain to appoint an "Alloys Research Committee," which receives financial aid from the Institution; and the Société d'Encouragement pour l'Industrie Nationale to create a "Commission des Alliages," for whose researches nearly all the railway companies of France and many metallurgical concerns contributed generously.

For advancing our knowledge of solutions we must give credit to the great physical chemists of Germany and Holland —Ostwald, Nernst, Van't Hoff, Roozeboom, and others. The microscopic study of metals seems to have originated with Dr. Sorby, an Englishman, who wrote upon the microstructure of meteoric iron as early as 1864. Only within the past ten or fifteen years has this method of investigation obtained just recognition among scientists as a valuable aid to metallurgical research, and not even yet has it obtained proper recognition among manufacturers. Recent progress in metallography is most nearly associated with the names of Stead, Martens, Osmond, Le Chatelier, Charpy, Roberts-Austen, Howe and Sauveur.

In the purely scientific study of the cooling curves of metals and alloys, two systems of pyrometry have given valuable results. The Siemens electrical resistance pyrometer, perfected by Callendar and Griffiths, is a reliable instrument, and, within certain limits, gives wonderfully accurate readings. For use both in the laboratory and in the factory it does not seem so satisfactory or so popular as the Le Chatelier thermo-electric couple. Not least among the advantages of the platinum, platinum-rhodium couple are, its accuracy at high temperatures, its ability to be used with small masses of heated substances, and, indirectly, its adaptability to photo-autographic recording of the temperatures indicated by such an instrument as the one invented by Sir William Roberts-Austen.

CRYSTALLINE STRUCTURE OF PURE METALS.

Before considering the constitution of alloys, it is well to know something of the general properties of metals. All solid metals are crystalline, although in microsection the individual crystals or crystalline grains may not present a simple geometrical outline. Professor Ewing, of Cambridge, has pointed out in this regard that the essential point is that the particles composing the mass of a crystal lie in one direction, *i. e.*, have the same plane of orientation. He also explains crystallization in a metal in this way: The formation of crystals must be assumed to start simultaneously at many points. The crystals grow until they touch one another; thereafter their symmetrical growth is impeded, but it is not necessary that the corresponding axes of any two of them be lying in exactly the same plane. In the case of commercial metals, more or less impure, the impurities are cast out by the growing crystals, and being, in fact, alloys of the admixed impurities with a little of the principal metal, have a lower melting point than the pure metal. Hence they solidify last and form an investing cement which holds together the primary crystals.

THE EFFECTS OF STRAIN IN PURE METALS.

Within the elastic limit no change is noticeable upon the polished surface of a metal or alloy when the same is under stress. When, however, the plastic stage is reached, there will appear dark lines more or less perpendicular to the direction of stress. This, Professor Ewing says, is not due to fissuring, for by changing the direction of the rays which illuminate the specimen under observation from vertical to oblique, it will be seen that the erstwhile dark lines appear light, while the background has changed from light to dark. Fissures would not reflect light in any case; the permanent elongation is due to slipping of the components of the crystals past one another. The slip lines need not necessarily lie in one plane. As many as four sets of parallel slip lines have been noticed by Professor Ewing.

My colleague, Mr. William Campbell, studied this phenomenon independently at about the time that Professor Ewing was making these interesting observations. Mr. Campbell has very kindly allowed me to exhibit here for the first time some of his results. Mr. Campbell worked with tin, while Professor Ewing experimented chiefly with lead. It is very interesting to notice how completely their results agree.

Fig. 1 shows the growth of crystals in tin. Two samples were rolled and then annealed for ten days upon a hot plate, below 200° C. The annealed specimens were etched and show in a very striking manner, not only the growth of crystals, but also the different planes of orientation. The figure shows these crystals slightly reduced from their natural size. *Figs. 2, 3* and *4* show respectively the same specimen of hammered tin; 2 is the original, etched in HCl, magnified 33 diameters, oblique illumination. On standing eighteen months the appearance changed to that shown in 3, also magnified by 33 diameters, oblique; and 4 shows the effect of annealing 3 for ten days on the hot plate, but it is only magnified by 16 diameters. In *Fig. 5* we have

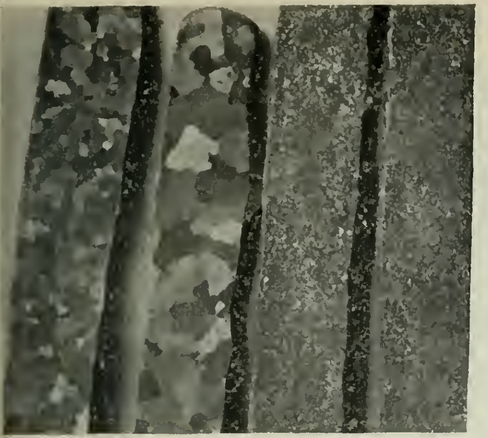
FIG. 1.

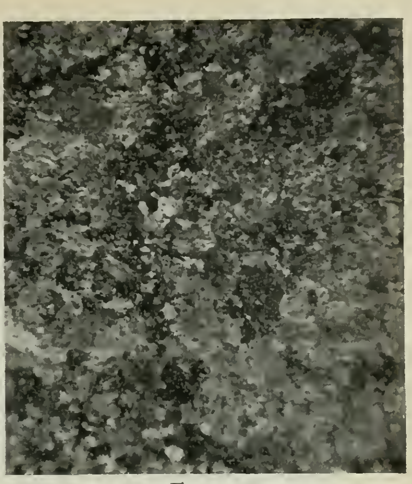
FIG. 2.

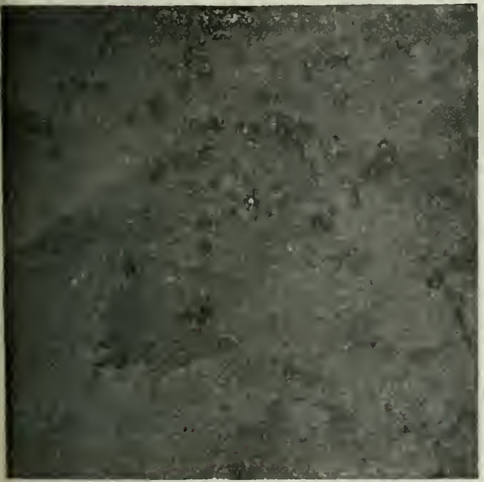
FIG. 3.

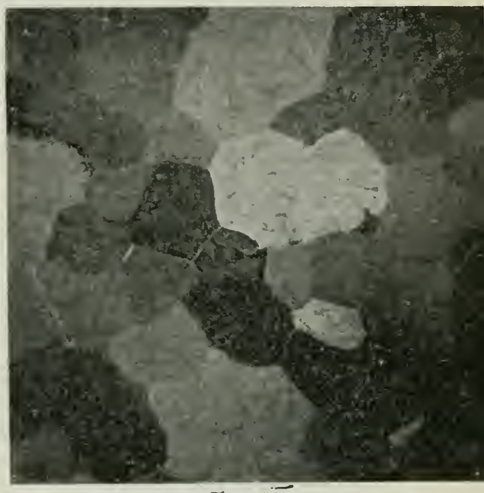
FIG. 4.

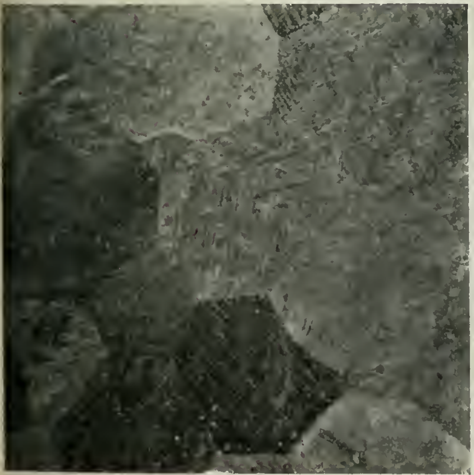
FIG. 5.


FIG. 6.

a microsection of No. 4, which shows wonderfully well lines due to strain which Mr. Campbell thinks was set up by the rapid growth. The fact that in different crystals the lines are parallel tends to confirm this view, the strain not having been manifested in a single direction. When such is the condition due to stress, a condition such as is illustrated in *Fig. 6* is set up, in which the general direction of the slip lines is normal to the direction of stress.

FREEZING-POINT OF METALS.

With water and many other liquids it is quite possible to lower the temperature to a considerable amount below their freezing-points without any separation of solid taking place. On the other hand, there is no known substance which can be heated above its melting-point without becoming liquid. When a liquid which exhibits this phenomenon of surfusion is thus cooled, and solid matter begins to separate, the temperature rises rapidly to the true freezing-point and remains constant until the whole mass is solid. The same is true of pure metals. Sir William Roberts-Austen cites an instance in which tin was cooled 20° C. below its freezing-point without solidifying. The author has frequently observed the same thing in tin and other metals, but to a less degree. In working with metals we take the first point at which solids separate as the freezing-point. For practical purposes we may consider the freezing-point and the melting-point as identical, but the latter is very difficult to determine, while the former is not. The phenomenon of surfusion need not obscure the determination of the correct point, particularly if we obtain an autographic record of the cooling curve. If we plot a typical cooling curve of a pure metal, using temperature and time as the coördinates, we obtain a curve like *AB* (*Fig. 7*) n which the angles are very distinct, *i. e.*, the temperature remains constant during the whole of solidification. If surfusion takes place—and it is more common in pure metals than in alloys—the curve *A′ B′* represents what happens. The temperature falls below the real freezing-point and then rises abruptly to that point and remains constant as in AB until complete solidification. In impure metals and solid solutions the temperature does not remain constant at all, but there is a decided change in direction when solid first begins to separate, as shown in *Fig. 11*. The greater the amount of impurity, the more rounded will be the cooling curve after it has passed the first angle. Thus, the nature of this line is an indication of the purity of the material under examination. To explain surfusion, we need only remember that fusion is always attended by absorption of heat and solidification by evolution of heat. When a superfused substance begins to solidify it disengages heat, and the temperature rises until the melting-point is reached. No further rise takes place, because at

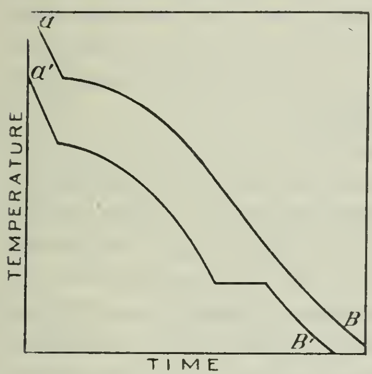

FIG. 7.

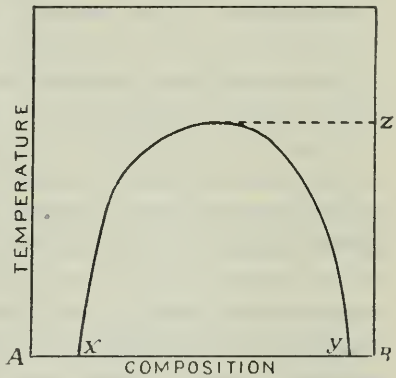

FIG. 8.

this point liquid and solid are in equilibrium. Any further change of liquid to solid or solid to liquid must be effected by abstracting or adding heat by independent means; in practice either heat is lost by radiation or is supplied by artificial warming.

ALLOYS AS SOLUTIONS.

Bearing in mind these few facts concerning metals, we are ready to consider what may take place when two metals are melted together. First, then, they may, or second, they may not, mix in all proportions. In the first instance, under proper conditions, an approximately homogeneous mass is likely to result. In the second case, upon cooling, it will be found that the metals have separated into layers, the lighter metal on top. Leaving out of consideration for the present the metalloids, we would find upon analysis of two such metallic layers that each metal contained a little of the other in solution, just as ether and water dissolve small quantities each of the other. In the case of certain metals and metalloids, the degree of mutual solubility is negligible at ordinary temperatures, although they may be quite soluble at high temperatures. For example, M. Moissan tells us that metals of the platinum group readily dissolve several per cent. of carbon at the temperature of the electric furnace, but cast it *all* out as graphite on cooling.

It is the conception of alloys as solutions that we wish to emphasize. Very striking analogies exist between ordinary liquid solutions and alloys (either liquid or solid), and to point out these similarities is to give the present conception of the constitution of alloys.

Some of the pairs of metals which do not mix in all proportions at ordinary temperatures are Zn-Pb, Zn-Bi, Pb-Al, Bi-Al and Cd-Al. These have all been studied by Dr. Alder Wright. (Proc. Roy. Soc., 1889–1893.) Since the relative solubility is a function of the temperature, we can assume that there is a temperature at which solubility is complete. Accordingly, we may represent the conditions existing between these and similar pairs of metals by a "critical curve." (*Fig. 8*) A and B are the metals which are not entirely miscible at ordinary temperatures, but which form layers. The metal A contains the quantity of B represented by the space $A\,x$ on the composition line. Similarly the metal B contains $B\,y$ per cent. of A. With increasing temperature the mutual solubilities increase, and at the temperature z there is but one solution. Alloys represented by percentage compositions which fall outside of the curve $x\,y\,z$ are homogeneous solutions. Those whose composition are represented by percentages within $x\,y\,z$ must be considered as comparable with emulsions. They are incapable of remaining homogeneous. To such alloys Sir George Gabriel Stokes gives the name of *ideal* (*i. e.* unreal, imaginary) alloys, while those falling outside the area $x\ y\ z$ are *real* alloys.

MOLECULAR FREEDOM IN SOLIDS.

It must not be supposed that because certain bodies are designated "solid" that entire absence of molecular mobility is implied. Such an idea is far from the truth; the difference in mobility of the molecules in "solids" and "liquids" is one of degree only. On the boundary between solids and liquids substances are said to be viscous, plastic, etc. Prof. W. Spring, of Liege, demonstrated the fluidity of metals, if I may use that expression, in his famous experiments upon the behavior of metals under exceedingly high pressures—ten thousand atmospheres, more or less. He showed that by pressures alone, (1) metals can be made to flow.

The familiar operation of striking coins illustrates this property of metals and alloys. A medal has recently been successfully struck in steel.

(2) Solids, both metals and salts, like liquids and gases, possess perfect elasticity and suffer no permanent diminution in volume by pressure. Strain, torsion or bending may produce permanent deformation in a metal or alloy, but there is no "limit of elasticity" in regard to diminution of volume, for when the pressure is removed the solid expands to its original volume. An exception to this constitutes Professor Spring's third conclusion, viz.:

(3) Allotropic changes may result from pressure. Primatic sulphur becomes octahedral sulphur, and amorphous arsenic becomes crystalline. In both these cases the second allotropic form is denser than the first, and from these and other experiments Spring argues that allotropic transformations by pressure show that matter takes the state which corresponds to the volume it is obliged to occupy. Moissan's brilliant experiments upon the production of artificial diamonds illustrate this principle.

(4) Spring showed that metallic filings are converted by pressure alone into solid masses, just as if they had been fused. Similarly, mixed filings yield alloys; for example,

brass was produced by pressing together zinc and copper. Reactions of double decomposition and the production of the highly colored regulus of Venus—Sb_2Cu_5—show that not only agglutination but also chemical combination may take place between bodies in the solid state. I have confirmed some of Professor Spring's results, but was unsuccessful in attempts to produce several such typical intermetallic compounds as $AuAl_2$, SbSn, $CuSn_4$, etc.

Prof. Sir William Roberts-Austen has also demonstrated the fact of molecular mobility in solids in a prolonged series of experiments upon the diffusion of gold into lead at temperatures varying from 250° down to ordinary temperatures. Graham showed long ago that the diffusion of gold, silver and platinum into liquid solvents, such as lead, tin and bismuth conformed to Fick's law (Roberts-Austen's Graham Lecture, Philosophical Society of Glasgow, 1900); that is, solid metals dissolve in molten ones, and diffuse against gravity, just as salts dissolve in water. Were it not for the application of this law to alloys it would not be such an easy task to prepare homogeneous alloys. For instance, with relatively little stirring, and relying mostly upon diffusion, Professor Roberts-Austen finds it an easy matter to make 1,200 ounces of coin gold alloy, the first and last pourings of which do not differ by more than $\frac{1}{10,000}$th part in fineness. The idea that interchange of matter can take place between solids is not a new one. The very antiquated cementation process used by the Hebrews perhaps 2,000 years ago for removing impurities from gold shows an empirical knowledge of the fact. Coming down to much later times, we find that Robert Boyle thought that "even gold" had its "little atmosphere;" that is, exhibits a minute tendency toward evaporation at ordinary temperatures.

The very beautiful experiments upon the diffusion of gold into solid lead which Roberts-Austen has recently reported to the Royal Society, may be summarized as follows: Gold placed in the bottom of tubes filled with lead and maintained at 250° C. (about 75° below its melting point) appeared at the top in notable qualities in a month.

At 100° C. the rate of diffusion was $\frac{1}{100,000}$th of that in just fluid lead, and in solid lead at the ordinary temperature, allowed to stand in contact for four years, unmistakable diffusion took place, but its rate is such that in 1,000 years the diffusion would equal that taking place in just molten lead in one day.

PRACTICAL APPLICATIONS OF MOLECULAR MOBILITY IN SOLIDS.

The practical applications of molecular mobility are many. We need only mention the annealing operations so common in all sorts of industries, by which means the ill effects of strain due to rapid cooling are remedied, and equilibrium is established in articles of metal, glass, enamel and other materials. The rate of diffusion of gold in lead at the various temperatures cited by Professor Roberts-Austin is an indication of the rapid increase in molecular mobility which results by very moderate changes in temperature. Steel is very sensibly annealed at temperatures more than 1,000° C. below its melting-point.

THE PHENOMENA OF SOLIDIFICATION OF BINARY ALLOYS.

Let us resume our classification of the phenomena which may result when two metals are melted together. It was stated that they may or may not mix in all proportions; now we may add that, if completely miscible, (1) chemical combination does not take place, or (2) chemical combination does take place, and one or more intermetallic compounds result.

The reality of the existence of intermetallic compounds is undoubted, yet, so far as I am aware, no mention of them is to be found in books on general chemistry. Indeed, very little is known in regard to the nature of the affinity between metals. It does not seem necessary to think that the laws applying to combination between the members of pairs of elements or radicles which are respectively electro-positive and electro-negative be equally applicable here; and our ideas of valency are seriously shattered if we attempt to reconcile them to intermetallic compounds.

The difficulties in studying such compounds are very great, because they are difficult to isolate; they are frequently quite unstable and show marked dissociation. Chemical methods of studying them are of very limited application; filtering off solidified intermetallic compounds from a still fluid metallic mother liquor presents extreme difficulties at high temperatures; and volatilization of the excess of solvent is rarely practicable. In a few cases the heat of formation can be determined, and the electrical method of comparing the potential of binary alloys with that of the more positive metal in the alloy, using as the electrolyte an aqueous solution of a salt of the more positive metal, may yield important information as to the solubility of one metal in another in the solid state. The microscopic evidence and that of the freezing-point curve are not wholly satisfactory in this regard. (See Hershkowitz, *Zeits. Phys. Chemie*, xxvii, p. 113, and Laurie, *Trans. Chem. Soc.*, 1888, p. 104.)

Cooling-curves.—During the cooling of a molten alloy various constituents may crystallize out successively; definite compounds, which are stable only at high temperatures, may split up into simpler constituents or new compounds, impossible at high temperatures, may be formed as the temperature falls. All such molecular changes are accompanied by corresponding thermal effects, such as the disengagement or absorption of heat. It is by the accurate measurement of the temperature at which these changes take place that we obtain most valuable information in regard to the molecular movements in the mass. The Le Chatelier pyrometer used in conjunction with an autophotographic recording device seems best suited to measure and record these changes. The general principles of thermo-electric pyrometry are too well known to require discussion here. Indeed, were they not well known, I could not do better than refer to H. Le Chatelier and O. Boudouard's volume, "Mesure des Températures elevées," or to its English translation with supplement by George K. Burgess.

As there are many who are more familiar with the theory than with actual pyrometric measurements, I will show by the accompanying illustrations the system of appliances which I used during several months when it was my privilege to work in Sir William Roberts-Austen's laboratory. The apparatus used in his own valuable researches upon steel and other alloys has been described and illustrated by Professor Roberts-Austen in the reports of the Alloys Research Committee, published by the Institution of Mechanical Engineers. The apparatus shown here for the first time differs in some respects from that described in these reports, and was in part designed for my use. The recorder, which is the only original feature, was designed by Dr. Stansfield, and constructed by the Cambridge Instrument Company. *Fig. 9* shows in the lower right-hand corner a crucible containing the alloy. In practice this is set inside of a larger crucible and surrounded by asbestos to prevent too rapid cooling. Just at the top of the crucible is seen the little fire-clay or porcelain tube, within which is placed the thermo-junction, consisting of a platinum and a platinum rhodium (10 per cent.) wire. Except at the point where their extremities are soldered together the two wires are separated by strips of mica, which also serve to prevent access of cold air to the thermo-junction. In the picture the Pt — Pt,.Rd wires are just discernible leading to the "cold-junction" at the upper right-hand corner. Here they are soldered to two heavy copper leads, each lead being immersed in a test-tube filled with alcohol, and the two test-tubes are surrounded by water at the room temperature, or a freezing mixture may be used. The copper leads are connected with a D'Arsonval reflecting galvanometer—dead beat and of high internal resistance. Additional resistance is placed in the circuit; by this means the deflection produced by any degree of temperature may be cut down to any convenient angle. With considerable resistance in the circuit, the change of resistance due to heating the short portion of the couple which is encased in the fire-clay tube becomes negligible. A beam of light is thrown from the galvanometer mirror upon the horizontal slot in the front of the recorder shown in *Fig. 10*. The front view

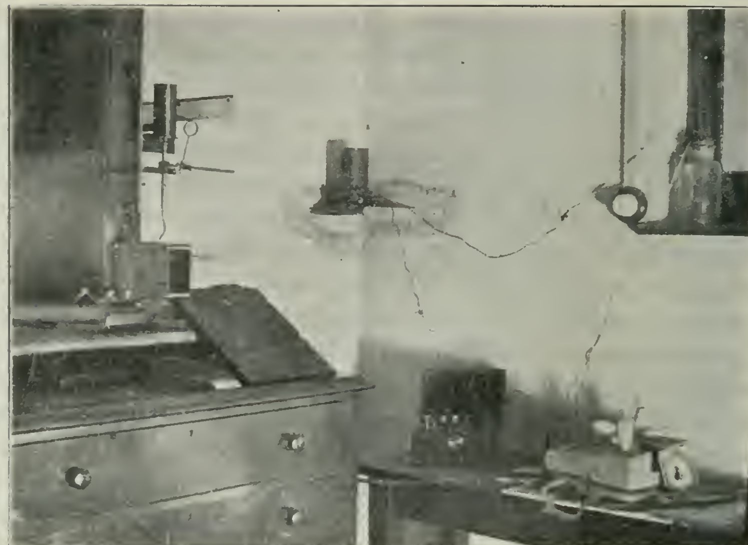

FIG. 9.

FIG. 10.

shows the slot with a centimeter scale attached. At either side is an incandescent lamp. Ordinarily but one lamp is used, a portion of its filament, shining through a vertical slit in the brass shade, is focussed by means of the small lens (attached to an arm projecting from the lamp) upon the mirror, and is reflected back to the horizontal slot, traversing it as a narrow line of light. At the back of the slot, by which light is admitted to the photographic plate, the opening is very narrow, and so the beam becomes reduced to a point of light which travels horizontally as the deflection of the mirror changes. The back view of the recorder shows a ground-glass focussing screen, which is used in getting the spot of light focussed to such proportions that its tracing on the plate shall be a slender one. Before putting the plate-holder into the carrier this screen is removed. The plate-holder carrier, clock-work, etc., require no explanation, except that the plate in descending drives the clock-work, resistance being offered by a vane whose size may be varied. There are three gears in the mechanism of the clock itself, which also assist in regulating the speed of descent of the plate to any desired rate.

Before using the instrument it must be calibrated by obtaining such fixed points as are given by the boiling-point of water (100°) and sulphur (444.53°), the melting-points of tin (232°), aluminum (654.5°), gold (1064°), and pure electrolytic copper (1083°). Not all these points need to be determined for a single calibration; three points, if determined with great accuracy, are enough. Other points may be determined by way of verification.

When the instrument is ready for use it is only necessary to insert the couple, suitably protected by a fire-clay or porcelain tube, into the molten alloy or pure metal. The beam of light rapidly moves from left to right until the thermocouple attains the temperature of the mass, then it begins to move slowly in the opposite direction. The photographic plate is set in motion, the shutter on the front of the slot is opened, and we get a tracing known as a "cooling-curve." The vertical axis of this curve represents time, and the horizontal axis represents temperature. The type of curve given by a pure metal has already been shown (*Fig. 7*); the same sort of curve is produced when a definite intermetallic compound or a eutectic solidifies, *i. e.*, these three kinds of substances have a single freezing-point at which the entire mass becomes solid. The nature of eutectics will be considered later; suffice it here to say that by a eutectic we mean that alloy of a series which has the lowest melting-point. Its composition is constant in any series of alloys. In *Fig. 11* the curve *AB* represents that of a homogeneous solid solution. It shows but one break, though the temperature does not remain constant during the whole period of solidification as it does with pure metals, compounds and eutectics. In *A′ B′* we see a curve with two breaks; the upper

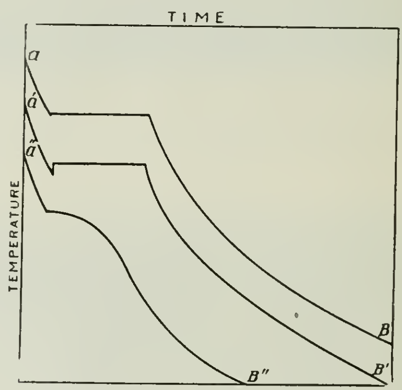

FIG. 11.

one resembling that of a solid solution, and the lower one shows the form of a eutectic. The explanation of these curves will appear as we proceed. The abrupt change in direction in *AB* occurs at the temperature at which that metal or component which is in excess begins to crystallize. As the excess of one metal solidifies the concentration of the residual fluid is increased with respect to the metal which is not in excess. The ultimate cause of the change in direction is the liberation of latent heat of solution.

As is well known, when concentrated hydrochloric acid (*i. e.*, solution of HCl gas in water) is boiled, the hydrochloric-acid gas evaporates faster than the water. If dilute hydrochloric acid is boiled, the water evaporates relatively faster than the gas. The inevitable result is that, at a certain concentration, hydrochloric acid and water vapor leave the liquid at a rate proportional to their concentration; thereafter the composition of the liquid remains constant and the boiling-point fixed.

A somewhat analogous case is that of the solidification of two metals, M and N; either of them may be considered as the solvent. When one of them, say M, contains more of N than it is capable of retaining in the form of a solid solution, and supposing that M has a higher melting-point than N, then—though the fluid mixture may be perfectly homogeneous—on cooling, the metal M with the higher freezing-point, begins to solidify first at some temperature *below* its true freezing-point, for it is not pure metal, but M containing some N in solid solution. But as the temperature falls, and more of M separates, the residual fluid becomes concentrated with respect to N, and the freezing-point is lowered progressively as the excess of M (+ more or less of N in solid solution) separates.

Some students of alloys consider that at temperatures between the first solidification of solid and the point at which the residue solidifies as a whole there does not exist a homogeneous condition. They liken alloys within this range of temperature to an emulsion, to conjugate solutions, etc. The reason assigned for this is that, ultimately, the excess of solvent having crystallized, the residual metals crystallize out side by side giving the usual laminated structure of eutectics. These laminæ consist not of pure M and N, but solid solutions each of the other; each contains the maximum amount of the other which is possible at the given temperature. Now, is it necessary to suppose that these two solid solutions existed as such in an emulsified state above the freezing-point of the eutectic? To be sure, the microscope reveals a very marked separation of the alloy into two constitutents, usually with characteristically laminated structure, but these two components show no tendency to separate into distinct layers when the alloy is kept for a long time just above its eutectic point; on the other hand, the magnitude of the eutectic structure is markedly influenced by the slowness of cooling below the eutectic temperature. Furthermore, the evolution of heat during the freezing of a eutectic is very considerable, and may perhaps be accounted for by such a decided molecular rearrangement as the formation of a laminated structure from a homogeneous fluid-mass would involve; and lastly, let me give some concrete evidence on this point, which

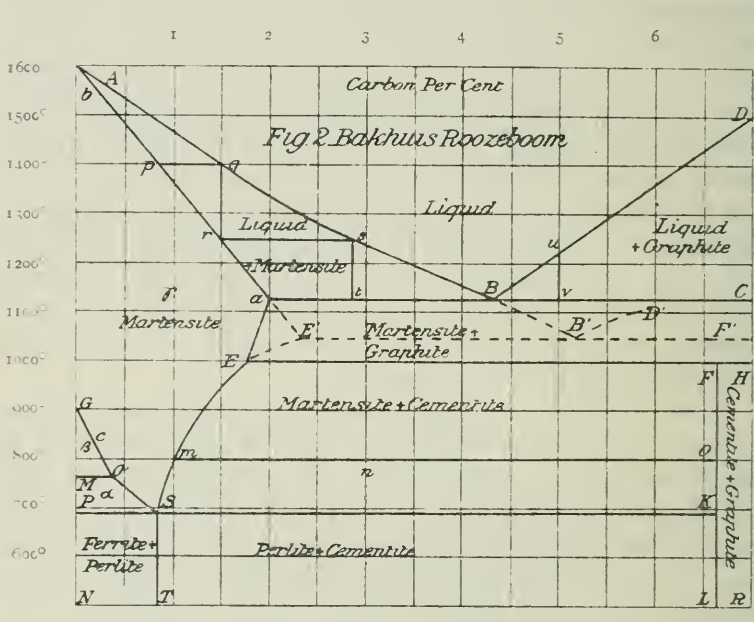

Fig. 12.

was brought to my attention by Mr. Campbell. In a lecture before the Society of Arts, of Boston, Mr. Sauveur stated: "It has been conclusively established that, in unhardened steel, at least, the totality of the carbon is combined with a portion of the iron forming carbide (FeC_3), which is then distributed through the balance of the iron." Therefore it appears that in Prof. Roozeboom's equilibrium curves of carbon-iron alloys (taken from the *Iron and Steel Institute Journal*) that the area marked Martensite (A,a,E.S.G.) in *Fig. 12* consists of a solid solution of carbide of iron in gamma-iron. But this alloy, Martensite, containing, say, 0.89 per cent. C. is capable of splitting up at about 690° C., or nearly 800° below its freezing-point, to the beautifully characteristic structure of pearlite; accompanying this change, there is a marked evolution of heat—the critical point *Ar 1* of Osmond. If such a decided transformation can take place in solid steel, it surely ought not to be considered improbable that a similar one could take place in any alloy at the instant of final solidification when the molecular freedom is doubtless many times as great as in the example.

[*To be concluded.*]

THE HEAT OF THE INCANDESCENT ELECTRIC LAMP.

The *London Lancet* offers some very timely comments on certain popular misapprehensions regarding the incandescent electric lamp. These relate to the very common belief that the electric glow lamp emits very little heat and to the consequent carelessness with which such lamps are disposed in the neighborhood of combustible materials without knowledge or consideration of the danger thereby incurred.

The general heedlessness on this point may be observed almost everywhere in shop windows where electric lamps may be seen in close proximity to combustible goods, and the wonder is that losses by fires from this cause are not very general.

As the *Lancet* states, the electric incandescent lamp is an apparatus for the conversion of one form of energy into another. In this case, the object is to secure the conversion of electricity into light, but it is not generally known or appreciated that only six per cent. of the current energy is thus transformed, the remaining portion being transferred into heat. While the amount of heat evolved from the carbon filament is not comparable with that of a naked gas flame, it is nevertheless great enough to cause the igniting of combustible objects brought in contact with it, and the danger of accidental fires arising from carelessness in placing readily inflammable objects—paper textiles, woodwork, etc.—in contact with or in close proximity to lamps of this kind, is not imaginary, but very real.

The *Lancet* lays special stress on this point, in connection with the very general use of glow lamps in shops for the purpose of decoration as much as for illumination. When thus used, there is often, no regard whatever paid to their proper and careful disposition, with the view of avoiding possible accidents from fire.

To emphasize its caution to the public the *Lancet* gives the following instructive experimental examples:

"We have found by experiment that on immersing a 16 candle-power lamp (100 volts pressure) in half a pint of water the water boils within an hour, and in proportionately less time when a 32 candle-power lamp is substituted. If again the lamp be buried in cotton-wool the wool soon begins to scorch and ultimately to burst into flame. In one experiment which we made the bursting into flame of the wool was accompanied by a loud report due to the explosion of the lamp. It clearly appears from this that the incandescent electric lamp cannot be regarded as an unlikely means of starting a serious fire, and shopkeepers, especially those who exhibit highly inflammable fabrics, should know that there is risk in placing such goods too close to the lamp. The lamp in contact with celluloid fires it in less than five minutes, and therefore the danger is particularly obvious in the case of toy shops where electric incandescent lamps are often suspended in the midst of toy celluloid ball."

W.

SCIENCE AND INDUSTRIAL SUPREMACY.

M. Levy, writing in the *Revue des Deux Mondes*, draws the attention of his countrymen pointedly to the remarkable development of the chemical industries in Germany and indicates the cause. He takes as an example the manufacture of the coal-tar derivatives.

The total European production of these compounds is valued at 125,000,000 francs, of which Germany is credited with 90,000,000, or approximately three-fourths of the whole. Switzerland follows with 16,000,000. France and England come next with 10,000,000 and 9,000,000 respectively.

The author very properly assigns the cause of this remarkable supremacy of Germany in this branch of the industries to the systematic employment of the highest scientific skill in the German manufactories. He makes the interesting statement that a single factory in Ludwigshafen in Baden employs a larger force of technical chemists than are working at the same branch in the whole of England, and he proceeds with this form of comparison, much to the disparagement of his own country.

The Germans not only cultivate theoretical science with the utmost thoroughness in their high schools, but also have the keenness to take the discoveries of their scientific investigators and extract therefrom the greatest amount of practical utility. *Science* makes the following additional extracts from M. Levy's highly interesting paper:

"The secret of the aniline dyes was discovered in England. France took up the investigations, but it was in Germany that particular attention was first paid to their commercial perfection as regards brilliancy, permanence and cheapness. As a natural consequence, Germany now almost monopolizes their production. The same is true in respect to electricity. The monetary benefit of the researches of Lord Kelvin and the late Professor Clark Maxwell is mainly reaped by the Allgemeine Elektricitæts Gesellschaft and the great firms of Siemens, Loewe and Schuckert, which have a combined capital of 15,000,000 pounds. Almost the only chemical industry which holds its own in England is the manufacture of alkali, and this owes its success to a distinguished pupil of Bunsen's, Mr. Ludwig Mond. M. Levy points out that Bunsen and Liebig were the founders of the German chemical industry. It was Liebig who devoted his energies and influence to obtaining State aid for laboratories of technical science. From these laboratories issue annually some 800 doctors of science to undertake the direction of factories or the investigation of rare products of commercial value. Their education has been assisted by the State, and the knowledge they have acquired will be devoted to increasing, by the surest of all means, the wealth of the nation." W.

Mining and Metallurgical Section.

Stated Meeting held April 10, 1901.

THE INSPECTION AND TESTING OF CEMENTS.

BY RICHARD L. HUMPHREY,
Member of the Institute.

(*Continued from vol. clii, p. 461.*)

NORMAL CONSISTENCY.

The percentage of water to be used in making tests of setting, briquettes and pats is of the greatest importance, for upon this depends the results obtained. The paste used in these tests should be of a definite or what is called a standard consistency. The same consistency should be used for all tests.

The method, therefore, for determining the quantity of water required to give the paste a standard consistency is of the greatest importance. This consistency varies in the different European countries. In this country there is no well-defined consistency. This is because every person has a different idea of what constitutes a "stiff plastic paste."

I prefer a wet rather than a dry mixture for a normal consistency, because the more wet the paste the less danger there is of compressing the briquette; and since there is no practical method for molding briquettes under uniform pressure, a difference of density in the briquettes results, which produces decided variations in the tensile strengths.

Besides, it is much easier to thoroughly mix a wet paste, especially by hand methods.

On the other hand, the paste should not be so wet as to produce shrinkage of the briquettes after molding.

I have for many years used the normal consistency apparatus used in Germany; this is the Vicat needle apparatus in which the needle is replaced by a rod having a section of 1 centimeter and weighted with 300 grams. The penetration of the rod to a given point established the percentage of water required. At the Charlottenburg testing laboratory in Germany, it is the practice to determine the proper percentage of water to be used by mixing the cement to a syrupy paste, so that it will run from a knife (6 by $1\frac{1}{2}$ inches) in long, thin threads without forming lumps.

Representing the quantity of water required to produce this condition by N, then the percentage of water (W) required to produce a normal consistency is obtained from the formula.

$$W = \frac{N + 1}{2} \text{ for neat tests, and}$$

$$W = \frac{N - 3}{4} \text{ for sand tests (one to three)}$$

This gives a consistency suitable for use with the Boehme-Hammer apparatus, and is, therefore, much dryer than is the practice in this country.

A rather crude method, yet one which gives excellent results, consists in molding the paste into a ball and dropping it from a height of 1 foot. The ball to be of the proper consistency should not flatten materially or crack. I find that this method gives results which correspond very closely to those obtained by means of the normal consistency apparatus.

This consistency is suitable for the pat, set and neat tensile tests. For sand mortars I have obtained very uniform results by calculating the percentage of water required from the following formula:

$$E — \tfrac{2}{3}\, N A + 60.$$

N—Weight in grams of water required for 1,000 grams of neat cement.

A—Weight of cement in kilograms in 1,000 grams of sand mixture.

E—Weight in grams of water required for mixture.

The percentages obtained from this formula must be slightly modified, especially for the mixtures containing more than three parts standard sand. These modifications must be ascertained by experience.

STRENGTH.

The setting of cement is the change from a condition of fluidity to a solid state. When cement has set, the process of hardening is said to commence. The relative degree of hardening at any age is measured by determining its transverse, compressive, adhesive or tensile strength in pounds per square inch.

There has been no very radical change in the principles of the tension testing machines, as originated by Grant and Michaelis, although the machines are better constructed.

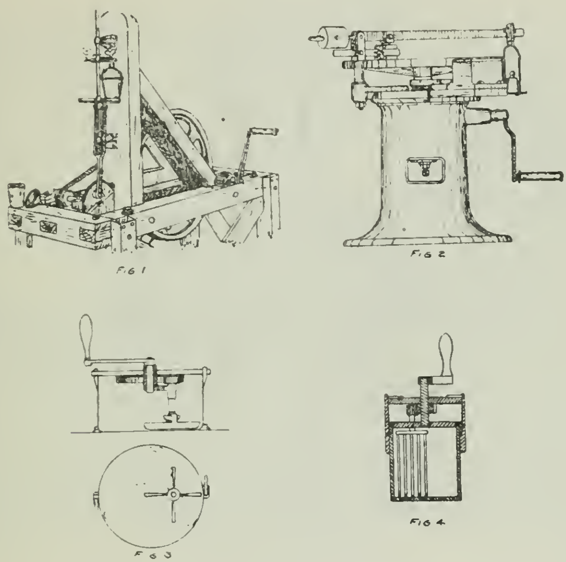

PLATE VIII—Fig. 1, Russell mixing device; fig. 2, abrasion testing machine; fig. 3, Faija mixer; fig. 4, modified Faija mixer.

Many improvements are required to make the tension tests more accurate. As a rule the Fairbanks machine is the best machine to use in making tension tests, since it is more accurate in the hands of the inexperienced than either the Riehle or the Olsen. For compression tests, the test specimens are usually 2-inch cubes.

The machine on which the compression tests are made should not be of too large a capacity, since the friction, which increases with the capacity, has an important influence on the results of the tests, especially on cubes tested at the shorter periods of time.

The test can be made on either a regular compression machine or on the machine made by Amsler-Laffon (Plate XIX) or on the Riehlé and Olsen machines in which the usual clips for tension tests are replaced by the compression tools. The latter machines are not of sufficient capacity for testing cubes over six months old, and generally the

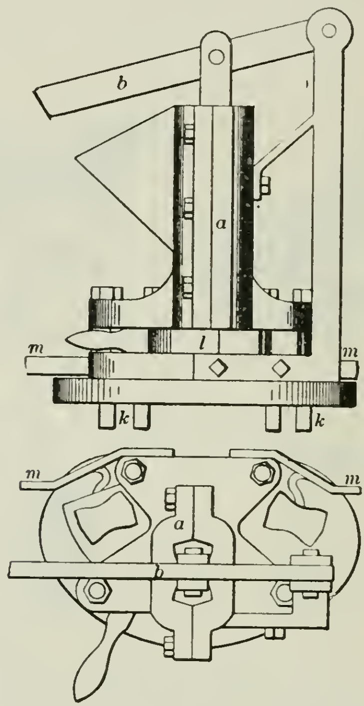

PLATE IX—Jamison molding machine.

test-specimen should be a 1-inch instead of a 2-inch cube, in order to be within this capacity. Transverse tests can be made on a compression testing machine—the test specimens should be 4 inches long and have a 2-inch section.

SECTION OF BRIQUETTE.

In tension tests, the shape of the briquette is very important.

In Plate XII are shown the various briquette sections. *Fig. 1* shows the original shape ($1\frac{1}{2}$ inches section) suggested by Grant. *Figs. 2, 3* and *4* show some of his subsequent experimental forms. It is interesting to note that *Fig. 3* had a 2-inch section, while we observe that *Fig. 2* shows that holes were molded in the head and the briquette was tested by passing a pin through each hole; these pins were connected with the grips of the testing machine. The bearing against the head of the briquette being distributed by means of a flat plate.

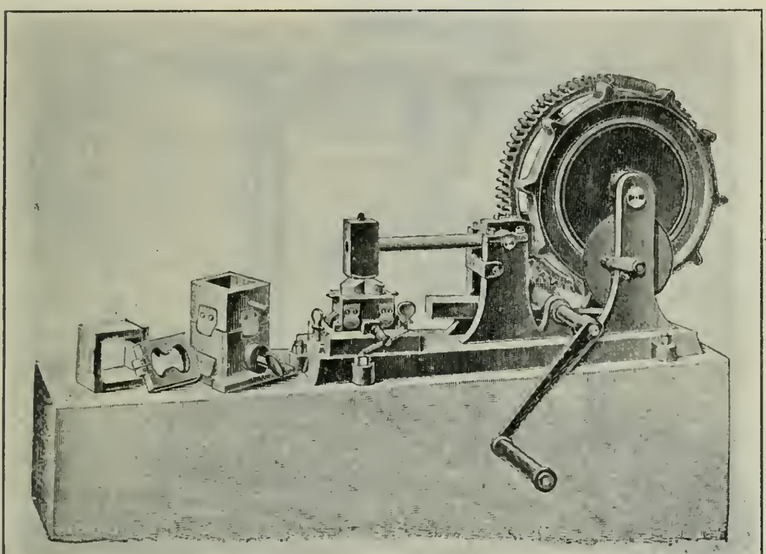

PLATE X—Boehme hammer apparatus.

In *Fig. 5* is shown the form finally adopted by Grant and one which is in general use in England at the present time.

The form adopted by the committee of the American Society of Civil Engineers is shown in *Figs. 6, 7, 8, 9* and *10*. *Fig. 11* is the form adopted by France and Germany; the section *Fig. 11* has considerably more material in the head, while an attempt is made to have the briquette break in the central section by means of the cunettes at this point. Experience does not show that this tends to increase central section breaks, they being dependent on many other causes, the principal cause being the uniform bearing of the clips or grips of the testing machine. A variety of the grips is shown in Plate XII.

These vary from the rigid close-fitting kind shown in *Fig. 7*, to the adjustable type shown in *Fig. 10*. I prefer the type shown in *Fig. 6* without the use of rubber, thin sheet of lead, paper, or other material to give a bearing.

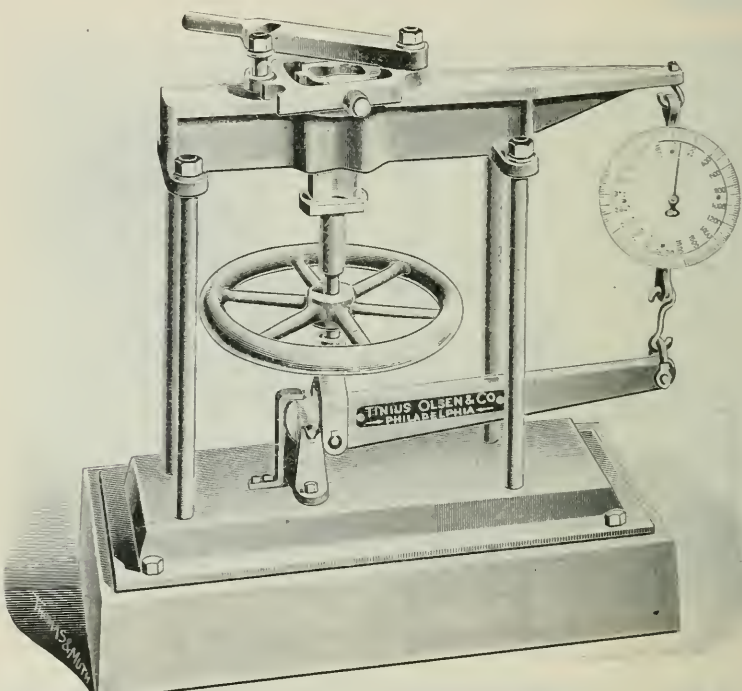

PLATE XI—Olsen molding machine.

The shape adopted by the American Society of Civil Engineers is universally used in this country, and, while not fully up to the requirements, gives fairly satisfactory results. I have found that the briquettes could be molded much more readily if the sharp corners of the American Society of Engineers' shape be rounded off with curves of ½ inch radius.

MOLDS.

There are a number of styles of molds in use in this country, both of the single and gang type, some of which have clamps in the middle, while others have them at both ends.

The style shown in Plate VI *Fig. 6* is the most convenient and satisfactory. There should be sufficient metal in the sides of the mold so as to prevent spreading of the mold when in use. A mold which spreads produces briquettes with enlarged heads and sections, causing variations in the strength, and preventing the proper adjustment of the clips in testing.

The metal used in the molds should be brass, bronze, or some equally non-corrosive material.

Gang molds are far preferable to single molds and yield more uniform results; besides, where the number of briquettes to be made is large, the work can be done much faster by making eight or ten briquettes from one mixing.

The molds should be wiped with an oily cloth before using; this prevents the cement sticking to the mold and possibly damaging the briquette when removing from the mold.

MIXING.

About 1,000 grams of material make sufficient mortar for this number of briquettes. The French system of weights and measures is the most convenient to use. The mixing and molding should be done on some non-absorbing non-corroding surface, preferably plate glass, although marble or slate will do. If the mixing be done on a surface of marble or of slate, it will be advisable to keep this surface covered with a wet cloth when not in use, or thoroughly wet the surface previous to being used. A surface of this character, when not in use, becomes quite dry, and absorbs some of the water from the first few batches mixed on it; this renders the mortar much dryer and materially affects the results especially with sand mixtures. The material is placed on mixing slab and formed into a ring into which the proper percentage of clean water is added. The material is turned into the ring by the aid of a trowel, and the mixing is completed by thoroughly kneading with the hands. Where the person making the test has very little experience, the mixing should last a given length of time. The German plan of mixing slow setting cement three minutes and quick setting cement one minute gives very good results. An experienced operator gauges the amount of mixing by the appearance of the paste.

This constitutes the method of hand mixing, which I have used for the last six or seven years, and have found it to be most satisfactory. Numerous mechanical mixing

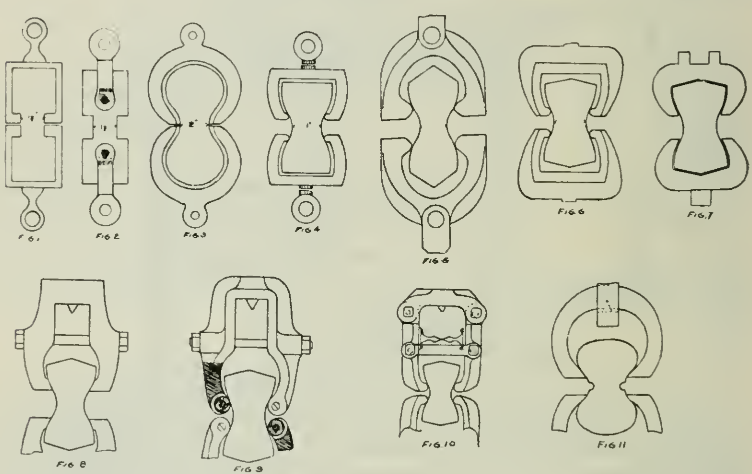

PLATE XII—Testing clips and shapes of briquettes.

machines have been devised, but the objection to them are, first, the neat cement has a decided tendency to ball, and, second, it is difficult to tell when the mixing is complete, while quick setting cements often set during the mixing. In hand mixing, the operation is under full control, and for this reason I have found the results far more uniform. Among the mechanical mixing machines may be mentioned the "Russell Jig," the "Faija Mixer," and that in use in the Cornell laboratory, a modification of the Faija mixer. Plate VIII, *Figs. 1, 3, 4*, shows some of the types of mechanical mixing machines.

The cubical box revolving at a high speed mounted on trunnions, located on the axis passing through opposite corners, gives good results with sand mixtures, but has the same defect with neat cement, viz., the tendency to ball up.

Unless the tester has considerable experience, it may be necessary for him to use one-half the quantity, especially with quick setting cements, since it is impossible to get the mortar into the molds before the mass begins to set. I have frequently seen an inexperienced person working on cement which had set and wondering why they could not get the mass into a plastic condition. When the moisture disappears from the mass and it becomes meally and does not stick together, the cement has begun to set and should be thrown away. Many cements which appear quite dry at first, after a few minutes mixing become quite wet; this is due entirely to the kneading to which the cement is subjected.

In sand mixtures, the mixing should be thoroughly done, as this insures each grain of sand being thoroughly coated with cement. This is a very important feature in sand tests, and is the principal reason why some inspectors get so much higher results than others. Mixing machines yield much better results with sand mixtures.

The temperature of the room in which the mixing is done should be uniform and the air moist. A high temperature and dry air often produce checks in the briquettes and dries them out; this checks the process of hardening and yields low tensile strengths.

MOLDING.

Having worked the mortar to the proper consistency, it is placed at once into the molds; this is done by hand—the molding being completed by hand or by a mechanical molding device. Of the latter there are several types of which the Boehme hammer apparatus, Jamison and the Olsen machines are the best. These machines are shown in Plates IX, X and XI.

The operation of machine molding is very slow and with the present types it is only possible to mold a single briquette at the time. This renders the use of a large quantity of mortar impossible, as incipient set is liable to take place during the molding, besides the moisture evaporates, rendering the mortar drier; so, as a consequence, the last briquette molded is likely to be more dense than the first.

In hand molding, the molds can be filled at once, pressed in and smoothed off with a trowel on both sides in a very few minutes. Indeed, I think greater uniformity can be secured by this rapid handling than in any other manner.

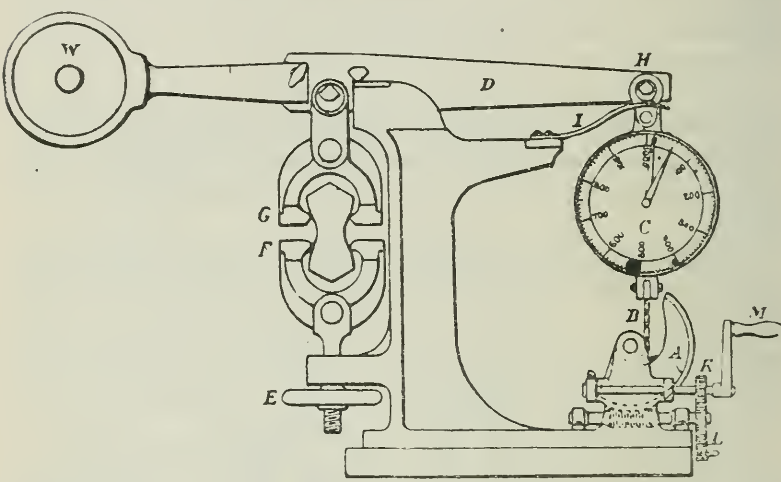

Plate XIII—Faija testing machine.

The mortar should be heaped up on each mold and then pressed in by drawing the trowel over the surface of the mold.

The objection to the mechanical molding machines is the difficulty in securing a uniform density, due to a lack of control over the molding pressure. Again, in such machines, the pressure being applied on one surface only, renders the density unequal, the briquette being more dense on the side which received the greatest pressure.

Briquettes trowelled on both sides are far more uniform in density.

The briquettes can be marked with a soft lead pencil, or with steel dies. If the latter are used the marking should be done on the head and not at the central section.

An excellent idea of the uniformity of the mixing and molding is afforded by weighing the briquettes before immersion in water; this is especially valuable when comparing the results of tests made by different persons.

It is the practice in some laboratories to throw out all briquettes which vary more than 3 per cent. from the mean weight.

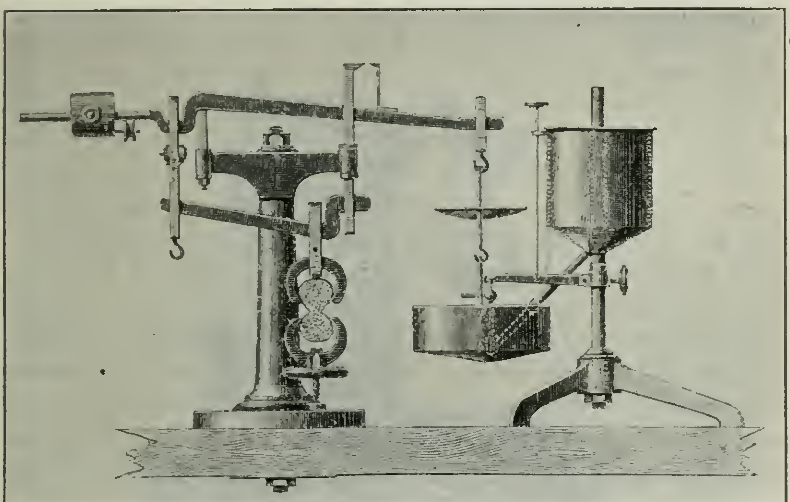

PLATE XIV—Mechalis testing machine.

PRESERVATION OF BRIQUETTES.

After the molding is completed, care should be taken to keep the briquette moist, thus preventing them from drying out. The evils resulting from this cause are shrinkage, air cracks or checks, and the retardation of the hardening; the latter being a chemical process dependent on the water used in mixing. The briquette should be sheltered from the direct rays of the sun. I have in mind an instance in which the failure of cement was traced to the morning sun shining on a portion of the mold containing the briquettes.

The briquettes should be preserved in moist air. Covering with a damp cloth is objectionable and should be avoided, as the cloth dries out in places, or the damp cloth coming in contact with the briquettes makes them too wet. In case this method is used, the cloth should be kept from direct contact with the briquettes by means of a wire screen or some similar arrangement. The most suitable method is the moist closet. This can be made of soapstone or slate, or can be a wooden box lined on the inside with zinc, covered with felt, the former being preferable. The closet should have water in the bottom. The closet should

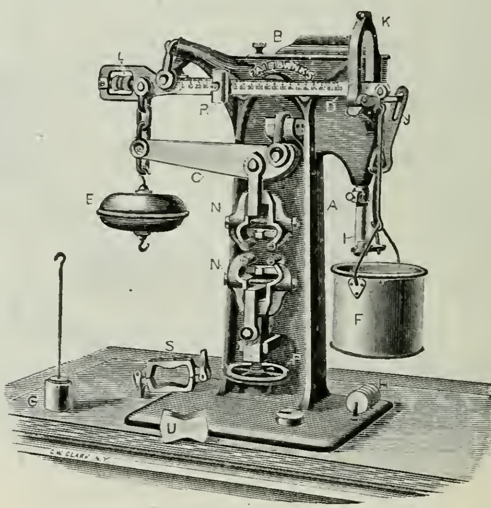

PLATE XV—Fairbanks testing machine.

also be provided with shelves on which to place the briquettes or the molds containing the briquettes. See *Engineers' Club Proc.*, Vol. XIII, No. 3.

For the twenty hour tests the briquettes should be placed in the moist closet immediately after molding, and kept there until broken; the longer time tests should be immersed in water at the end of twenty-four hours in moist air. For preserving the briquettes in water, either pans or large tanks can be used; the former should preferably be the agateware roasting pans, since there is less danger of corrosion. The tanks can be of soapstone, and arranged as those in use in the laboratory of the city of Philadelphia, or they can be arranged as those in use in the laboratory of Lathbury & Spackman. The latter consists of a series of enamelled iron sinks arranged one over the other and supported by a frame made of wrought iron pipe; each sink being provided with a separate supply tap and an overflow pipe. (For information in connection with the city of Philadelphia laboratory mentioned above, see *Proceedings, Engineers' Club of Philadelphia*, Vol. XIII, No. 3.)

Where pans are used, the water should be renewed at least once each week. Where briquettes are preserved in pans, care should be taken to keep them covered with water, as the drying out of the briquettes checks the process of hardening and thereby impairs their strength—lowering of the temperature of the water much below 60° F. also tends to retard the hardening of the cement.

Where running water is used, there should be a supply of hot and cold water, so that the water can be kept temperate, *i. e.*, 60° to 75° F. It is still an open question as to whether still or running water is the best in which to preserve the briquettes. I do not regard the theory tenable which attributes the falling off in the strength (observable in the present high testing Portland cement) to the solvent action of running water. It would be fair to suppose that sand mixtures, having more voids than neat cement pastes, would show the greatest loss from this action. Such is not the case, however, as the loss is greater in the case of neat cement.

Again this loss is frequently observable at the end of twenty-eight days or two months; it is hardly likely that the solvent action of running water would be great enough to exert an appreciable effect in this short period of time; besides the loss is just as apparent in still water.

While it is true that cement is soluable in water, and the action may, after many years, be great enough to produce an appreciable loss of strength, yet I do not believe that the loss of strength, which is generally observable, can be attributed to this cause.

It should be borne in mind that the briquettes which show this loss of strength are quite hard, usually have a brittle fracture and show no signs of softening or of disintegration.

Running water offers many advantages over the pan method. However, the pans are less expensive and are

PLATE XVI—Olsen testing machine (hand driven).

better adapted for the laboratory on the works. Permanent laboratories should use the large tanks, which can be provided with running water or not, as desired.

BREAKING BRIQUETTES.

Briquettes should be broken as soon as taken from the water. Care should be taken to center the briquettes in the machines, as cross strains, produced by improper centering, tend to lower the breaking strength of the briquettes. The breaking load should not be applied too suddenly, since in the Riehlé or Olsen machines this produces vibrations which tend to snap the briquette. Neat cement briquettes

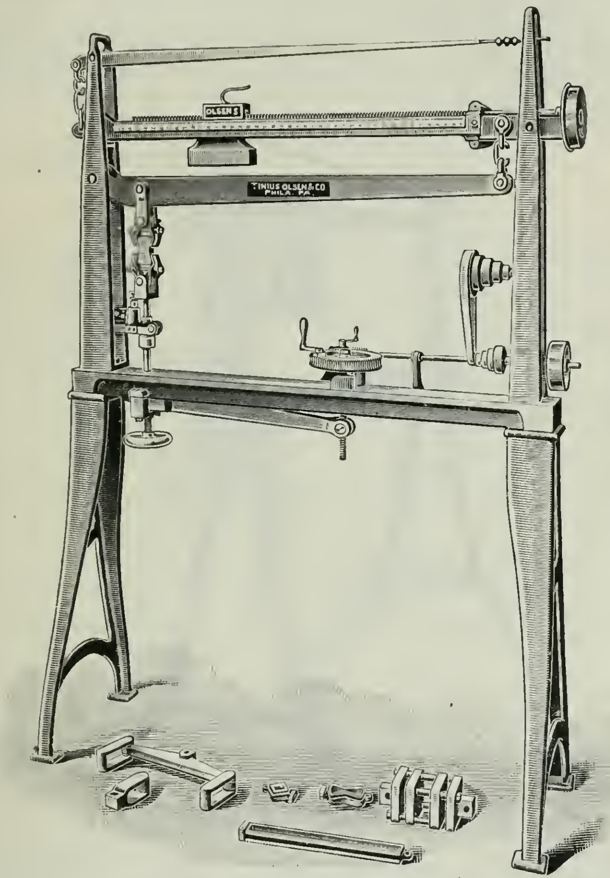

PLATE XVII—Olsen testing machine (power driven).

are often shattered by the sudden application of the breaking load, particularly if the cement be very old. The clips should be clean and the briquettes free from grains of sand or dirt, which tend to prevent a good bearing. Care should also be observed in applying the initial load; this is particularly the case with the Fairbanks machine and constitutes the chief objection to this machine. In long time tests the initial load must be very great, and, as there is no way of regulating this load satisfactorily, the variations in the results are often largely due to variations in the amount of the initial load applied.

In order to regulate the application of this initial strain, it is the practice in some laboratories to place weights in

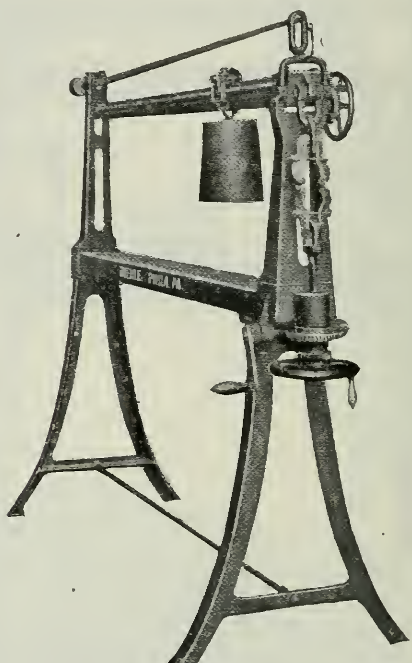

PLATE XVIII—Riehlé testing machine.

the shot pan at the commencement of the test, the amount of the weight being dependent upon the age and character (natural or Portland) of the cement under test, the weight increasing with age—it being greater for Portland than for natural cement, and also greater for neat than for sand tests.

It often happens that the last molded and usually the densest briquettes are broken at twenty-four hours or seven days, and the first molded or less dense at twenty-eight days or longer. This difference in density may be considerable, in which case the tests may show an apparent falling off in strength. Again, the cement may begin to set before the last briquette is molded, and, should these briquettes be broken at the long time period, a loss of strength might be again apparent, or even indications of disintegra tions appear. All these facts tend to emphasize the neces sity of uniformity in mixing and molding in order to secure uniform density in the briquettes, and thus avoid the resultant apparent losses in the tensile strength.

CONSTANCY OF VOLUME.

One of the most important tests to which cement is subjected, and one which is the most difficult to make, is that which pertains to the soundness. The methods that have been suggested are legion. This test cannot be used by a novice with safety, and even in the hands of an expert all tests for soundness must be made with extreme care.

Since Faija suggested his celebrated steam and hot water apparatus, the hot water or boiling tests have been universally used. In this country the hot water or boiling tests are the *bete noir* of the cement manufacturer, and in his endeavor to make his cement stand these tests he has made use of an adulterant in the shape of gypsum or sulphate of lime to the possible detriment of the quality of the cement. I do not wish to be understood as condemning the use of sulphate of lime in Portland cement manufacture. On the contrary, I believe the careful and rational use of sulphate of lime is necessary in order to render this cement slow setting, and, therefore, more adaptable for general use. What I do condemn, is its present indiscriminate use in the effort to make cement "boil." Some of the other tests for determining the soundness of a cement are the high pressure steam test, the boiling test, the Michaelis hot water test, the Faija test, the cake test, the oven test, the ball test, and the kiln test; while several appliances have been devised for measuring the expansion of cement by means of rods or prisms of cement immersed in water at a normal temperature. The standard test in Germany for constancy of volume is that of a pat of neat cement immersed in water at normal temperature for twenty-eight days. This test is not, however, sufficient to develop the unsoundness of cement in time to be of practical value.

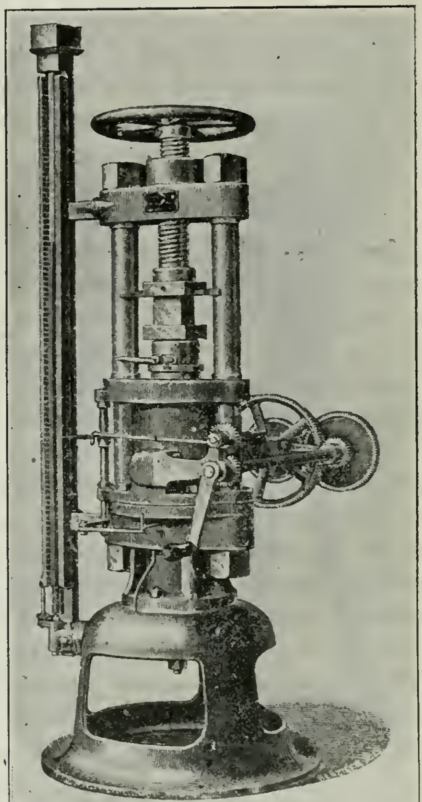

PLATE XIX.—Amsler Laffon compression machine.

I have found that usually all well-made Portland cements when made into pats and immersed in steam as soon as made, or in water (maintained a temperature not exceeding 170°) after hard set, will develop no objectionable qualities at the end of twenty-four hours, unless it be of very poor quality; such cements are also perfectly sound in cold water. One of the difficulties encountered in these tests is in making the pats; these are made of neat cement, about three or four inches in diameter from one-quarter to one-half of an inch thick at the center and tapering to thin edges at the circumference. The pats should be made with the same percentage of water as in the case of the other tests. As simple as the making of these pats may appear to be, it is extremely difficult for

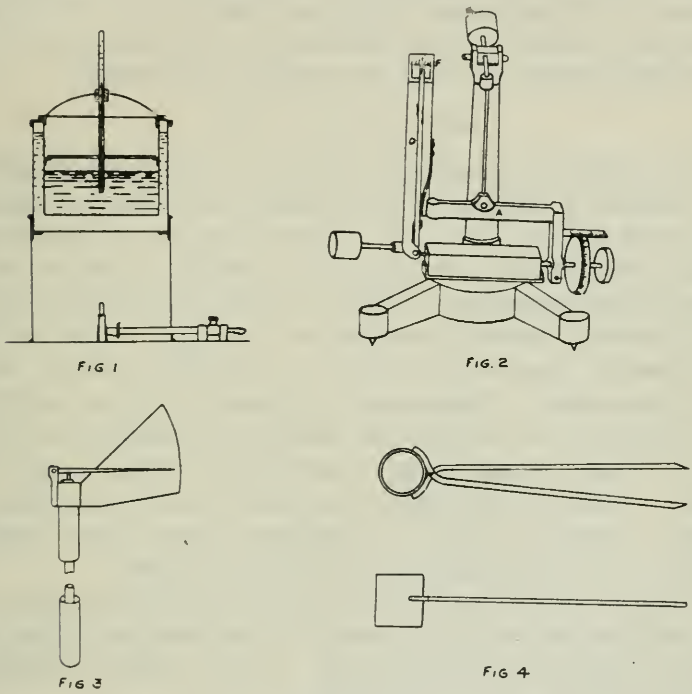

PLATE XX—Fig. 1, Faija hot water apparatus; fig. 2, Bauschinger expansion apparatus; figs. 3 and 4, expansion apparatus.

inexperienced persons to make them correctly. Pats may be so trowelled as to give initial strains which develop cracks during the test. A good plan is to strike the glass on which the pat is made after molding, this rearranges the mass, drives the moisture through the pat and makes the density of the pat more uniform. Care should be taken that the pats do not dry out—this produces shrinkage cracks, which give a false impression of unsoundness. Most pats leave the glass, and, unless this is accompanied by swelling, curvature of the pat, or cracking at the edges, it should not be taken as evidence of unsoundness. In some cases the cement may set before the pat is finished, and, when placed in steam or hot water, the outer edge may lift off—this to the inexperienced is also misleading. There is no doubt the soundness or constancy of a volume of cement is of paramount importance and it is to be hoped that some method will be devised by which these tests can be made quickly and with some degree of accuracy.

MISCELLANEOUS TESTS.

The tests just described constitute those most essential for general purposes in determining the value of cement delivered for use. Tests for determining the compressive, transverse, adhesive or abrasive strength, together with those for determining the effect of frost, action of sea water and porosity, furnish information having a value for the purposes of research, or where the conditions render such data desirable. Permanent laboratories, where work of this kind can be carried on, should be equipped for such tests. Tests of still greater importance, which cannot be used as tests of reception, are those made on the work. These consist in tests of briquettes made from mortar taken from the mixing box or cubes of concrete.

Data obtained from such tests is valuable, inasmuch as it furnishes information concerning the strength of the concrete or mortar as actually used. It also reveals improper mixing of the mortar or concrete.

[*To be concluded.*]

——*The Electro-Chemist* states that Dr. RATHENAU, of Berlin, has taken out an Austrian patent for a process of manufacturing calcium carbide of reasonable purity from materials loaded with silica by making use of the property which certain metals possess of combining with silicon in the electric furnace. Thus, when a mixture of 60 parts of anthracite containing 25 per cent. of silica is smelted electrically with 56 parts of lime and 28 parts of metallic iron, a carbide of good quality is produced, while a ferro-silicon containing 20 to 25 cent. of the latter element rests on the top of the solidified carbide and can be easily separated from it. This ferro-silicon is also capable of finding employment in the arts.

THE HALFTONE TRICHROMATIC PROCESS.

From a painting by W. J. McCloskey. Printed on treadle press, without overlay or other make-ready. Dill & Collins' "Trichromatic" paper.

THE FRANKLIN INSTITUTE.

Stated Meeting, held Wednesday, November 20, 1901.

THE HALF-TONE TRICHROMATIC PROCESS.

BY FREDERIC E. IVES.
Member of the Institute.

Until recently, all of the finest color printing has been done by the chromolithographic process, employing from seven to twenty stones, with as many inks and impressions, and the necessary drawings upon these stones have been made by specially trained lithographic artists.

It has long been thought by some that in accordance with the trichromatic theory of color vision, three printing surfaces, colors and impressions might be substituted for the seven to twenty of the chromolithographer and that the preparation of these surfaces might be accomplished by photography.

The only commercially successful development of this idea to-day is by the employment of three half-tone process blocks made from a trichromatic negative color record, and printed with three colored inks in the type press.

It will no doubt interest those present to know that the first public exhibition of a reproduction by this method was at the Novelties Exhibition of the Franklin Institute, in 1885.* This example was made by me in 1881, when I was the only man in the world engaged in the production of half-tone process blocks † and ten years before anybody else is known to have made a half-tone trichromatic reproduction.

The time was not then ripe for the commercial development of such a process, which demands conditions in the printing office which have been realized only after many years, by a process of evolution which was greatly stimu-

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* Catalogue of the Novelties Exhibition, 1885, p. 36. See also *The Photographic News*, London, Sept. 5, 1884, first page.

† U. S. Patent No. 245, 501, Aug. 9, 1881.

lated by the introduction of the half-tone photo-engraving process.

The half-tone trichromatic process was first commercially exploited less than ten years ago, and is already a competitor of chromo-lithography, because cheaper and more direct; but the quality of the product has been, up to the present time, so uneven as to have brought the process somewhat into disrepute. This state of affairs is due largely to the fact that such success as is achieved generally depends very largely upon the degree of skill available in the correction of errors and defects in the operation of the process by re-etching the half-tone blocks—a procedure technically and appropriately known as "faking."

I have always contended that conditions could be secured which would make it possible to obtain the best results almost automatically, and it is my purpose in preparing this paper to show how this may be accomplished.

As in all other practical forms of the trichromatic process, we commence with three photographs to represent the analysis of all colors into proportions of three "primary" or fundamental colors, and it is evident that correct results cannot be obtained without the aid of hand-work unless this photographic record correctly differentiates all the hue and luminosity values of the copy. How to secure this perfect differentiation of hue and luminosity value is a problem which certainly baffled the earlier experimentors in trichromatic photography; but its solution by working to the Maxwell color curves, as first proposed by me in a paper read at this Institute in 1888,* has been sufficiently demonstrated by the results obtained in the photo-chromoscope.

In the photo-chromoscope, however, we make up our white light, not of all of the spectrum rays, but of a mixture of three practically isolated groups from the two ends and middle of the spectrum. If we were to employ all of the spectrum rays equally, but sharply divided into three groups, we should find that when we attempted to repro-

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* *Journal of the Franklin Institute*, January, 1889, p. 54. U. S. Patent, No. 432,530, July 22, 1890.

duce certain hues of the spectrum, they would be somewhat degraded in purity, because pure spectrum reds, greens and blue-violets cannot be reproduced by any *mixtures* of spectrum rays. From this I have argued that color prints from Maxwell curve photographic records, if made upon paper which reflects ordinary white light, must show a little degradation of color. Comparison of such prints with the originals and with the photo-chromoscope reproductions shows that this degradation, otherwise almost unnoticeable, actually occurs and disappears when the prints are viewed in a white light made up of a mixture of isolated groups of red, green and blue spectrum rays.

From this it might be argued that the photographic color record which is suitable for one purpose is unsuitable for the other. The fact is, however, that for synthesis by any other light than the artificial white of the photo-chromoscope, no trichromatic analysis can be theoretically perfect; and I hold that the Maxwell curve analysis introduces, even for synthesis in ordinary white light, less serious errors than any other; and that the best compensation for the modicum of degradation of purity should be sought in later stages of the process, in the lining of the process blocks, the absorption and transparency of the printing inks, and the paper and press-work.

This question has been the subject of much controversy, and as it would take a volume to fully elucidate the subject, I will only add here that the demonstration which I now present is based upon Maxwell curve analysis, the results of which, under the conditions which I have secured, will speak for themselves.

The perfection of each individual element of the trichromatic negative record depends upon the relations of the source of illumination, the absorption of the selective color screen and the color sensitiveness of the photographic plate, one to another. Each of the three negatives may, however, be substantially perfect in itself, but out of key, so to speak, with the others, though being relatively less or more exposed or developed to lesser or greater density; and in the practice of the process this want of harmony of relationship is a source of error quite as serious as errors of principle in the analysis, and it is probable that two-thirds of the "faking" that is found necessary in practice is due to errors of this character, which may be introduced in any of the photographic operations, or even in the etching process, so long as the three operations are conducted separately. My cure for this difficulty consists in the production of the three images of the color record by one exposure upon a single sensitive plate, for which purpose I have devised several special cameras, one of which,* together with triple negatives produced in it, is submitted for examination. After once correctly adjusting these cameras, triple negatives can be turned out with perfect regularity, in which the relation of the images one to another, in exposure and density, is bound to be correct. The printing upon the zinc or copper-plate and the etching is also thereby reduced from three operations to one. The importance of this procedure from a labor-saving point of view is very great, because it saves much time and labor in the initial stages of the process, besides eliminating errors which frequently necessitate costly and time-consuming "faking" operations in the later stages.

In the most approved method of producing the half-tone process blocks, the half-tone process negatives are made through cross-line sealed screens, in order to translate the smooth gradations into definite line and dot, as is required by the typographic printing process. There are a few operators who employ the cross-line screen in the first instance, making the half-tone negative directly on the color-sensitive plate; but very much longer exposures are then required, and the translation of body shade into line and dot is far less definite and satisfactory than when a transparency from the smooth color record negative is made the copy for a half-tone process negative by the wet-plate process, or on special "contrast" dry plates.

My special triple cameras can be fitted with the cross-line screens, so as to make the line negatives direct; but

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* U. S. Patent, No. 668,989, February 26, 1901.

after some experience of both methods I at present favor making first a triple color record, then a contact positive, and from the latter a triple half-tone process negative in the copying camera. Allowing for the saving in time in making the original exposure through color screens, no time is lost, and the half-tone process negative, made by the wet-plate process, or on a special "contrast" dry plate, is of a quality not obtainable by the first method.

Compared with the most usual procedure, fifteen operations* are thus reduced to five,† and compared with the shortest method practiced with a single camera, nine operations are reduced to five, with gain in the quality of the screen process negatives and elimination of the defects commonly introduced by separate exposure, development, printing and etching. If the screen negatives are made direct with the triple camera, the total number of operations is reduced to three.

Another reason for favoring the use of a transparency as copy for the half-tone process negative, is the fact that original triple-color records can most advantageously be made on a standard size of plate in a camera which gives images either larger or smaller than the plates required, and the images readily brought to the required size in the copying camera, when the triple half-tone process negative is made. The original triple negative is then available at any time for plates of different sizes, or with screens of different degrees of fineness, or for Kromskop slides or color print lantern slides, or for any other kind of trichromatic process reproduction which may at any future time be desired.

Three of the ruled cross-line screens are used in the copying camera in front of the one negative plate, and the arrangement is such that three small images can be made on plates of one size, or three larger images on a larger

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* Three separate color-screen negatives, three separate transparencies, three separate half-tone process negatives, three separate prints on the zinc or copper-plates, and three etching operations.

† One color record negative, one transparency, one half-tone negative, one print on copper or zinc, and one etching.

plate without change of screens or alteration of adjustment. It is necessary to use three screens in order to have the lines differently disposed in the different images to avoid an offensive watered-silk pattern. This disposition of the lines at different angles to avoid this effect is seen in my original example of 1881, a copy of which I have with me, and was patented by some one else twelve years after.

By the means which I have described, sets of half-tone trichromatic printing plates may be produced with great regularity and precision, frequently requiring no re-etching to perfect them, and never any considerable amount of it. It remains only to reduce the printing to one run through the press in order to eliminate errors due to unequal inking and unequal expansion of the paper with changes of atmospheric condition, to make the production of half-tone trichromatic process reproductions as nearly as possible automatic, and of the highest average quality. Already it has been proved that the printing can be done successfully upon this principle with a special press which is now on exhibition in this city, but was not available for printing my specimens. They have been run off on an ordinary treadle press without any overlay or other "make-ready."

The depth and "openness" of the etching of the plates, and the color and transparency of the inks are very important factors in this work. To fulfill theoretical requirements, the ink must be perfectly transparent and as specifically antichromatic as possible to the respective "primary" colors in ordinary white light. If they absorb too broad bands of the spectrum, or if the blocks are not etched sufficiently open, the colors will be degraded with black—"muddied" is the technical term. If they absorb too narrow bands of the spectrum, or if the two top colors are not perfectly transparent, the superposition of the three inks will not produce a good black, and some colors must be falsely rendered. If the absorptions are diffuse, a little difference in the relative amounts of the inks put down will make great changes in the colors of the print. If the absorptions are in sharply defined bands, which meet each other over the Fraunhofer *D* and *F* lines of the spectrum, after putting down enough to make a good black by superposition, an excess of any one will scarcely alter the colors of the print except there be lateral spreading, increasing the area of the printed lines. Inks having broadly overlapping absorptions, or not perfectly transparent, produce a quite different hue when printed in lines side by side than when the lines superpose, while perfectly transparent inks with sharply defined absorptions which just come together in the spectrum will produce a tint of almost exactly the same hue where the lines overlap and where they do not. All of these facts should be taken into consideration in the selections of printing inks, and constitute a basis for predicting what will prove most suitable.

The inks which I employ are a *minus* red (peacock blue), the absorption of which is strong and pretty even from the red end of the spectrum right up to the *D* line, but falls off to nothing in the greenish yellow; a *minus* green (crimson), the absorption of which is strong and pretty even from over the *D* line to the *F* line, falling quickly to nothing outside of those limits; a *minus* blue (yellow), the absorption of which is strong in the violet and blue, and falls off gradually between the *F* and *E* lines. A mixture of these three inks to make a transparent neutral gray, when analyzed in the spectroscope, shows somewhat more absorption in the yellow and blue-green of the spectrum than in the red, green and blue-violet parts, proving that they effect a color synthesis in some degree approximate in character to that in the photo-chromoscope, while still in bands sharp enough to make easy printing without the hue depending too much upon the flow of ink, or whether the lines or dots fall side by side or in superposition. These inks are not quite ideal, but they are the nearest approximation that I can now obtain, and they yield more accurate rendering of colors from Maxwell curve-record printing plates than the "regular" three-color process inks.

ARTIFICIAL INDIGO.

The Textile World, of Boston, devotes some space in one of its late impressions to the consideration of the new and formidable competition of natural indigo that has arisen in the synthetical indigo now placed upon the market by the Badische Anilin and Soda Fabrik, and which promises in due course of time to do for the natural product what artificial alizarine has done for the natural product derived from the madder.

The World makes the following comments on this interesting topic.

"The indigo pure (synthetic indigo) of the Badische Anilin and Soda Fabrik is nothing else but indigo, which is the same blue coloring matter as contained in natural indigo. Its properties are therefore identical in every respect with those of the latter.

"Wool, as well as cotton and silk, can be dyed with it in any vat used for indigo, and it cannot be detected even by analysis whether the natural or the artificially made indigo has been used. It consequently would be to the dyer a question of price only which of the two to employ, did not the artificial indigo possess other advantages overbalancing the natural. It is a well-known fact that natural indigo varies in strength and purity, and that the price, owing to the good or bad crops, is subject to great fluctuations.

"The synthetic indigo, on the other hand, is delivered of uniform quality and of standard strength. Fluctuations, such as occur with the natural indigo, are therefore excluded. The paste has, besides the advantage that it is sold in the ground state, thus saving grinding and being always ready for use.

"Different kinds of natural indigo are in the market; for instance, Java, containing from 70 to 80 per cent. indigotine; Bengal testing 60 to 70 per cent.; Kurpah, from 30 to 55 per cent., and even qualities below 30 per cent. In the face of such differences it is extremely difficult, and in fact almost impossible, to judge the exact coloring strength by its appearance. In analyzing, it has been found that lots sold as one quality of indigo vary in themselves, and even samples drawn from the same chest have been proven to differ as much as 7 to 8 per cent. in indigotine, though by appearance it was scarcely distinguishable.

"For some time indigotine has been extracted from natural indigo, and this product brought into the market under the name of refined indigo. Of course it is also subject to the same fluctuations in price as natural indigo itself. Efforts have also been made in India and other countries to obtain a more uniform quality in the manufacture, but it appears that the experiments have not been successful, for the qualities which are sold in the market still differ just as much as ever."

The World concludes its comments with the statement that the dyer has, when using pure indigo, a product of uniform strength, which is furnished in the form of a paste ready to use. The method of using the artificial product differs in no respect from those in vogue with the natural indigo. W.

Section of Photography and Microscopy.

Stated Meeting, held Thursday, December 5, 1901.

A NOVEL STEREOGRAM.

By Frederic E. Ives,
Member of the Section.

About sixteen years ago I was making that study of the dioptrics of half-tone screen photography which led me to devise and adopt the cross-line sealed screen and special diaphragm control now universally employed in the production of half-tone process plates. At the same time other applications of the line screen occurred to me, which I did not regard as of sufficient importance to justify me in devoting time to exploiting. One of these applications was to the production of a stereogram which should require no stereoscope or other optical aid to be seen in relief, like the ordinary double stereogram in a stereoscope.

It recently occurred to me that a very simple modification of my "Kromolinoskop" camera would enable me to produce such a stereogram, and that it might be of sufficient interest as a scientific novelty to be worthy of presentation at a meeting of this Section of the Institute.

The single example which I have found time to produce is in the form of a transparency on glass, which, when held 12 inches squarely in front of the eyes, instead of looking like a flat photograph, appears to be the front of a box within which, in full stereoscopic relief, and at different distances from the eye, are a statuette and two glass vases.

This result is obtained by placing a line screen in front of the sensitive plate in the camera, slightly separated from it, and forming the image with a 3½-inch diameter lens, behind which are two small apertures placed at the pupillary distance apart, and viewing the resulting photograph (a positive from the original negative) through a similar screen, from approximately the same distance as the focal length of the lens.

Each aperture of the lens forms an image made up of shaded lines, and, owing to difference of parallax, the lines constituting the two elements of the stereogram are separated and alternate with each other; and for the same reason, each eye picks up all of the lines belonging to the respective element of the stereogram, while the lines constituting the other element are hidden from that eye by the opaque lines of the covering screen.

In the small example exhibited, the lines of the screen are somewhat in evidence, but there is nothing to prevent the production of these pictures in such large sizes that at the proper viewing distance the lines cannot be separately perceived, and in such large sizes they would prove interesting, and might have some vogue as window transparencies, and in the production of popular illusions, such, for instance, as a magical gallery of sculpture, the statures appearing to be of life-size, and as if seen through and beyond a glass behind which there was in reality nothing but empty space.

In order to give this kind of picture a distinctive name, I propose to call them "parallax stereograms."

VENEERED DOORS.

The rapidly extending uses of hard woods have given rise to the manufacture of veneered doors. The base or core for these doors is some light wood, such as pine, etc., over which is laid a veneer of oak, birch, mahogany, or any hard wood, thus producing a door that is to all intents and purposes a hard-wood affair, combined with the lessened weight of the wood, while effectually preventing the warping and twisting that very often ruins a solid door. Such doors cost only about one-third more than a pine door.

The waste-products of sawmills are sold by the owners to a great factory in the Northwest, where they are treated and made into a "compo-board." This is in most respects superior to laths and plaster in the construction of inside walls.

The wood-pulp industry particularly commends itself to the manufacturer of furniture. He can safely say, when he turns out a lot, that he has something that will not "split" or warp. The enormous pressure to which the article is submitted effectually prevents this, aud, in addition, the furniture is lighter. Thus, from one base, may be made all kinds of furniture. If you want mahogany (?) you can have it, or oak, or rosewood, walnut, ebony, and in fact any kind or style, at the same price, all due to the new industry that seems to be of unlimited applicability and value to those who cannot afford to buy furniture in the original wood.—*The Manufacturer.*

CHEMICAL SECTION.

Stated Meeting, held Thursday, October 23, 1901.

WHAT DOES THE DESIGNATION C.P. MEAN AT PRESENT?

By Lyman F. Kebler,
Member of the Section.

C.P. in the chemical world undoubtedly meant, as it should to-day, a chemical of very high purity. But the expression as used at present by the chemist, the pharmacist, physician, the photographer, the broker, etc., may mean almost anything from a chemical substance of high purity to one of very secondary quality. For ample evidence of the latter it is only necessary to read over the advertising portions of some of the journals, where can be seen some of the absurdities of the designation C.P. For example, one manufacturer advertises chemically pure tar.

That the designation C.P. may mean chemicals of ordinary purity can be seen by the following: An order was placed for C.P. zinc chloride and C.P. sodium bisulphite, two very difficult chemicals to make even of a fairly high degree of purity. What was wanted in this case were articles of good quality, "commercially pure," if you please.

Nearly all manufacturers of glycerin mark their best quality C.P. and deliver it in iron drums, supposed to be galvanized inside. If such glycerin ever was C.P. it certainly cannot be expected, in many cases, to remain so very long under these conditions. Some C.P. drum glycerin contains iron to such an extent as to render it unfit for use in mixtures which contain carbolic acid or similar chemicals. As a matter of fact, very little available glycerin answers even the U.S.P. requirements, to say nothing of C.P. These glycerins almost all contain fatty acids and substances which reduce Fehling's solution.

The photographer needs most of his chemicals of a very good quality. To many of the druggists and physicians, U.S.P. and C.P. are synonomous, and the writer has met with persons whose only criterion for C.P. was physicial appearance or a certain crystalline form. The interchanging of C.P. and U.S.P. in this way may have some *raison d'être*. The Pharmacopœia requires in a number of cases a purity of 100 per cent. which must of necessity carry with it absolute purity. A condition very rarely met with. The above authority is, however, somewhat inconsistent about it's 100 per cent. purity, as the following will show: potassium bicarbonate is to be 100 per cent. pure, yet there is allowed a limit of *iron*, *chlorides*, and *normal carbonate*. Even Rochelle salt is expected to be of the above high degree of purity, but here again a limit of chlorides is prescribed.

In looking over Merck's Index for 1896 we frequently see a chemical of the same degree of purity marked both C.P. and U.S.P. A little further on the same chemical, probably of a higher purity (usually doubled and trebled in price), is marked "Guaranteed Reagent." Under these chemicals marked G.R. there is generally given a series of limit tests. For example, silver nitrate, which the present Pharmacopœia requires to be 100 per cent. pure when furnished as G.R. is permitted to be contaminated with a limit of inorganic matter. How the designation C.P. must have fallen from grace, if one of the foremost chemical manufacturers treats it with such contempt. But this is not all. The June number of Merck's Report marks only two chemicals C.P. and a slightly larger number U.S.P. These abbreviations have all been replaced with such terms as regular, pure, superior, and highest purity. Other large chemical manufacturers, like E. deHaen, Gehe & Co., Boehringer & Söhne, and others, have all generally dropped the time-honored C.P. from their price-lists. Several firms in this country still use C.P. very freely. Exactly what meaning it has to them is hard to say: for example, in one price-list is quoted sodium carbonate crystalized C.P. and sodium carbonate dry, absolutely C.P. The same list contains magnesium oxide C.P. free from sulphur, and zinc oxide C.P. free from manganese. These two chemicals are used in the estimation of sulphur and manganese respectively, and while other impurities may be present, those specified above should be absent. Of what value is C.P. in such cases?

All chemists are very well aware that it is practically impossible to make an absolutely pure chemical in any quantity. Every investigator of atomic and molecular weights has encountered the difficulty of preparing absolutely pure material for his work, even in small quantities. Even the time-honored arsenic-free-zinc is hard to be secured. Most of it contains a trace of arsenic by the Marsh-Berzelius method, when permitted to act for a considerable length of time, and allowance must be made for the presence of the impurity in toxicological work.

The writer would like to see a set of standards or system of abbreviations established for chemicals and chemical compounds, so that upon proper designation the same quality would be furnished when and wherever called for. This would require the coöperation of those engaged in the various chemical industries in formulating requirements of purity for the chemicals used by them, paying attention not only to the impurities, which would interfere with the use of the chemical, but also to impurities allowable because of their non-interference.

THE WOOD-PULP INDUSTRY.

For an extended period, ever since the great impetus of paper manufacturing by the discovery that it could be used for a great many purposes besides writing upon it, experts have been constantly looking for new uses for it. The same may be said of lumber ; that is, pulp lumber, that which when transformed by chemical process becomes paper. Now comes a process whereby wood-pulp is to be molded under great pressure, which is colored while in the soft condition to give rich and varied effects in paneling and frame, while receiving all the strength required in a door. The same, too, may be said of small brackets, picture molds and balustrades, which are molded hollow and with which is thoroughly incorporated a waterproof cement, with the claim that they will withstand the weather as well as terra cotta. It can be readily seen how great is the future for this branch of lumber manufacture, when one considers the use to which terra cotta is put in the construction of the great steel structures that are daily being erected in the large cities.

Pulp, molded to cover steel and iron posts, becomes almost, if not quite, non-inflammable ; and in this respect will dispute with terra cotta its place for such uses, as it can be worked up into elaborate cornices and protecting and ornamental shields at a price considerably under that of terra cotta, if the latter were molded on the same lines, and can also be so artistically finished that its general use in the decorative arts, already well begun, is only a question of time.

There is almost no limit to the uses to which wood-pulp can be put, and these will be rapidly multiplied as the supply of pine wood becomes scarcer and scarcer. The wood-pulp manufacture is also a blessing to owners of vast quantities of hitherto supposedly worthless hemlock and spruce timber, of too small a size for sawing purposes, and large quantities of "popple" and kindred wood which abounds in some portions of the North, and with which vast plains in northern and western Canada are covered, have been utilized and will be available for generations to come.—*The Manufacturer.*

THE SPARSE DISTRIBUTION OF TIN.

In one of the recent publications of the geological survey of Australasia, the writer calls special attention to the singular fact—well known to metallurgists though not to the non-expert—that tin is the most sparingly distributed metal in common use. In this respect it compares interestingly with gold, the known workable deposits of which cover 1,500,000 square miles, while the workable tin deposits are confined to less than 125,000 square miles; from which it appears that for every square mile of tin ground there are 132 square miles of gold-yielding territory. Furthermore, while there is scarcely a country in the world in which gold might not be profitably obtained, or from which it has not been obtained in the past, there are at the present time probably not more than a dozen districts in the world from which tin is obtained. The tin deposits of Cornwall famed in history as the source from which the Phoenicians obtained their tin supplies, and which have yielded immense quantities continuously down to the present, are now practically exhausted, and the same may be said of the condition of the ore deposits of Bohemia, Tuscany, Southern Spain and the Pyrenees.

In all the vast area of Asia, but two districts are known in which workable deposits of tin exist; namely, the Hunau district of China, and the Straits settlement and adjacent principalities, from the latter of which by far the largest quantity of the metal has been obtained for many years.

Thus far no workable tin deposit has been discovered in Africa. In North America at present there is not a single tin mine in operation, though promising deposits have been located in at least three widely separated localities, viz.: Virginia, Dakota and California, which, however, proved unprofitable. In Mexico extensive deposits of tin are known to exist in the province of Durango, which one day, when facilities for transportation are provided, will doubtless add greatly to the world's supply. At present, however, they remain untouched. Brazil, Bolivia and Peru in South America have paying tin ground, but the yield thus far is comparatively small, and is confined to the last-named two states. Lastly, Australasia is the source of a considerable supply of tin from lately discovered deposits in Australia and New Zealand. At present the product is only about one-tenth that of the Straits, but it is steadily increasing. W.

A RAPID METHOD FOR DETERMINING THE VALUE OF "CHROMIC ACID" AND THE SOLUBLE CHROMATES.

By Lyman F. Kebler,
Member of the Section.

The principle involved in the beautiful and exact method for estimating iodine by means of sodium thiosulphate was brought forward by A. du Pasquier,* but the original method gave neither satisfactory nor concordant results. Bunsen † took up the process and pointed out the cause of its shortcomings. These researches on the volumetric estimation of iodine, in connection with Schwarz's ‡ proposed use of sodium thiosulphate instead of sulphurous acid, produced a very beneficial effect on the whole domain of chemical analysis. The value of the process is not so much in the estimation of iodine in iodine compounds, but rather in the determination of such substances as will liberate iodine when brought in contact with posassium iodide, either by direct displacement (*e. g.*, the chlorinated compounds, chlorine water, bromine, etc.), or by reduction in the presence of hydrochloric acid, either with or without heat (*e. g.*, CrO_3, PbO_2, MnO_2, As_2O_5, $FeCl_3$, etc). The details of the various methods must be worked out for each substance to be estimated. If a chromate is boiled with an excess of strong hydrochloric acid, chlorine is liberated, which can be distilled and conducted into a solution of potassium iodide, contained in a suitable apparatus. The distillation may be avoided by mixing the chromate, a saturated solution of potassium iodide and the hydrochloric acid in a strong bottle provided with an accurately ground stopple. The stopple is firmly tied in, the bottle with its contents

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* 1840, *An. de Chimie et de Physique*, **73**, 310; *Silliman's Jour.* **40**, 123.

† 1853 (Liebig) *Anal.*, **86**, 265–291.

‡ 1853, *Anleit. zu Maassanal.*, *Nachträge*, 22.

immersed in water and the temperature raised to boiling, where it is kept for one hour. The bottle is then removed, cooled and the amount of liberated iodine estimated by means of N/10 sodium thiosulphate.

Both of the above methods are more or less tedious, and are liable to give abnormally high results, on account of the proneness of the hydriodic acid formed to decompose. A blank should always be carried.

F. Mohr recommends the method of decomposing the chromate with hydrochloric acid and distilling the resulting chlorine into a solution of potassium iodide. He also observes that "Die Zersetzung der chromsauren Salze durch Jodkalium in salzsaurer Loesung gibt keine konstaten Zahlen, und die Methode ist zu verwerfen." The author has not had any experience with the latter method, hence reserves comment, but the former method has given him anything but satisfactory results.

The following method has, however, been used with considerable satisfaction: Dissolve about one gram (accurately weighed) in enough distilled water to make exactly 100 centimeters. Of this solution transfer 20 cubic centimeters into a porcelain evaporating dish containing 75 cubic centimeters of water, add 2 grams of potassium iodide, 15 centimeters of 10 per cent. sulphuric acid, and mix well. Then add, from a burette, N/10 sodium thiosulphate until a distinct blue color, without yellowish cast, results, or, better, the end reaction may be determined by means of a starch solution.

It was at first thought that some time must be allowed for complete reaction of the above mixture, before the liberated iodine could be estimated, but the writer soon found that the reactions were almost instantaneous.

The reactions involved are represented by the following equations:

(1) $2\,CrO_3 + 6\,KI + 6H_2SO_4 = 3\,I_2 + Cr_2(SO_4)_3 + 3\,K_2SO_4 + 6\,H_2O$.

(2) $3\,I_2 + 6\,Na_2S_2O_3 = 3\,Na_2S_4O_6 + 6\,NaI$.

(3) $K_2Cr_2O_7 + 6KI + 7\,H_2SO_4 = 3\,I_2 + 4\,K_2SO_4 + Cr_2(SO_4)_3 + 7\,H_2O$.

(4) $3\,I_2 + 6\,Na_2S_2O_3 = 3\,Na_2S_4O_6 + 6\,NaI$.

According to equations (1) and (2), one equivalent of CrO_3 requires three equivalents of $Na_2S_2O_3$, or the decinormal factor of CrO_3 is one-third of 0·009988 or 0·003329. In the same way the decinormal factor of potassium bichromate is one-sixth of 0·029378 or 0·004896.

An examination of several samples of "chromic acid" gave the following results:

Number.	Color of Crystals.	Sulphates.	Per Cent. of CrO_3.	Aqueous Solution.
1	Brownish red	Much	66·66	Clear
2	Dark crimson	None	95·71	"
3	Light brick red	60·53 per cent. calculated as $NaHSO_4, H_2O$	38·28	"
4	" " "	59·76 per cent. calculated as $NaHSO_4, H_2O$	38·89	"
5	Crimson	4·2 per cent. calculated as H_2SO_4	93·83	"

There certainly is a marked difference in the physical appearance of the above samples. No. 2 was of satisfactory quality. No. 5 was a beautiful crystalline product, and the writer was very much surprised to get a reaction for sulphates. It also gave evidence of containing a sodium salt. Nos. 3 and 4 were fairly good physically. According to the above analysis they consisted of nothing but a mixture of chromic acid and sodium acid sulphate, obtained by mixing the proper proportions of sulphuric acid and sodium bichromate; $Na_2Cr_2O_7, 2\ H_2O + 2\ H_2SO_4 = 2\ CrO_3 + 2\ NaHSO_4 + 3\ H_2O$; then evaporating the mixture to dryness. These samples may have been intended for technical purposes, but no such information could be found anywhere on the container. It appears to be the custom of some manufacturers, however, to deliver some of their goods without the semblance of a label as to contents or quality; which appears to the writer to be a very dangerous practice.

LABORATORY OF SMITH, KLINE & FRENCH CO.

WASTE OF NATURAL GAS.

The recklessness with which Americans are prone to use or rather to misuse the products with which nature has so bountifully endowed the country is conspicuously illustrated by the manner in which the vast supplies of natural gas have been carelessly permitted to go to waste.

The State of Indiana, by reason of its geological features, is probably more favorably situated in the matter of natural gas supplies than any of the neighboring states, and the manufacturers of Indiana have availed themselves of the product more extensively than elsewhere, and to their great advantage. From a recent report of the State Geologist, it appears, however, that they will not long enjoy the benefits of this cheap and admirable fuel. He reports that the people of the State not only waste the gas in furnaces, grates and stoves, but through sheer indifference have for years been permitting the gas to escape into the air at the rate of perhaps 20,000,000 cubic feet per day, simply through neglect to cap the wells which have been bored for oil.

Already there are ominous signs of the approaching exhaustion of the gas supply. Originally the gas-producing territory embraced 3000 square miles, to-day it has shrunk to one-half this area. In the beginning of the gas era, the average gas pressure as it issued from the wells was 325 pounds to the square inch; now it does not exceed 200 pounds. During the past year the gas pressure in the center of the Indiana gas area diminished 30 pounds per square inch. Taking all the circumstances into consideration, the Geologist estimates that at the present rate of consumption, the gas supply will be practically exhausted within five years, and he even estimates that it may not last more than one year.

Oddly enough the manufacturers, who should be the most careful conservators of this product, appear to take the matter quite complaisantly, with the knowledge that they can fall back upon petroleum (of which the State is a large producer) as an efficient substitute for the gas fuel.

It is impossible to form an estimate of the enormous quantities of natural gas that have been altowed to go to waste in the oil fields of the United States since the period when deep drilling first tapped the gas reservoirs. It is within bounds to say that for every cubic foot that has been utilized a thousand have been wasted.

In its want of thought for the conservation of this overbountiful gift of nature, Indiana has only imitated the bad example of her neighbors, Pennsylvania and Ohio. W.

The lightest solid known is said to be the pith of the sunflower, with a specific gravity of 0·028, while elder pith—hitherto recognized as the lightest substance—has a specific gravity of 0·09, reindeer's hair 0·1, and cork 0·24. For saving appliances at sea, cork with a buoyancy of 1 to 5, or reindeer's hair with one of 1 to 10, has been used, while the pith of the sunflower has a buoyancy of 1 to 35.

DARLING'S ELECTROLYTIC PRODUCTION OF METALS AND NITRIC ACID FROM FUSED NITRATES.

[*Being the report of the Franklin Institute, through its Committee on Science and the Arts, on the invention of J. D. Darling, Philadelphia. Sub-Committee: Joseph W. Richards, Chairman; C. J. Reed, H. F. Keller, Samuel P. Sadtler, Carl Hering.*]

HALL OF THE FRANKLIN INSTITUTE,
[No. 2188.] PHILADELPHIA, June 3, 1901.

The Franklin Institute of the State of Pennsylvania for the Promotion of the Mechanic Arts, acting through its Committee on Science and the Arts, investigating the merits of Darling's Electrolytic Production of Metals and Nitric Acid from Fused Nitrates, reports as follows:

The process described and shown in operation is the electrolytic decomposition of fused sodium nitrate, producing therefrom metallic sodium and acid vapors, which are converted into nitric acid. The process is applicable to the nitrates of other alkaline metals.

Applicant's patent 517,001, of March 20, 1894, describes the continuous electrolysis of fused sodium nitrate contained in a metallic pot serving as cathode, using carbon or platinum anodes, enclosed by inverted metallic cups, which serve to collect the gases and prevent their dissemination in the electrolyte, and consequent re-combination with the liberated sodium. The arrangement of apparatus shown and method of operation described in the judgment of your committee are not such as would be conducive to high efficiency, or even admit of practical continuous working; but, since Mr. Darling, in his application for this investigation, has indicated as the particular novelties of his process, the porous cell and shunt current described in his subsequent patents, we refrain from a detailed discussion of the merits or demerits of this first patent.

Patent 590,826, of September 28, 1897, describes a porous diaphragm intended for use in a fused salt, such as the sodium nitrate of the previous patent, and meant to increase the efficiency of the operation by preventing re-combination of the liberated ions. The great difficulty of using a material for the diaphragm which would resist the intense fluxing action of the fused salt was met by the discovery that the vitrified earthy oxides, such as pure magnesia fused in an electric furnace, were practically unattacked. The additional difficulty of forming such material into a partition was met by the device of using it as coarse powder or sand between perforated supporting walls, which may be of perforated sheet-steel.

Patent 641,276, of January 16, 1900, supplements the preceding by cheapening the material used as the filling of the diaphragm. The fused earthy oxides are costly, and applicant discovered that, contrary to expectation, a considerable proportion of Portland cement could be introduced into the partition without impairing its life, and even making a diaphragm more solid and with less tendency to crack. In such a mixture, however, ground, burned magnesite can be substituted for the more costly fused or vitrified magnesis. Improvements in the mechanical details of the construction of the cell are also shown.

Patent 641,438, of January 1, 1900, describes a complete apparatus for conducting the process, in which are shown arrangements for heating, etc., but which contains as its principal feature the novel idea of connecting the sheet-metal supporting walls of the porous diaphragm by a shunt circuit with the anode, in order to minimize the destructive action upon them of the liberated ions. When not so connected they act as "middle" or "secondary" electrodes, and a small fraction of the current liberates at their inner surface, sodium; at their outer surface, nitric-oxide gas and oxygen, and the combined effect of both ions upon these supporting walls causes their rapid destruction. By connecting them through a resistance, with the anode, they become positive, and are subjected only to the action of whatever anions may be liberated there. The statement is made that the supporting walls, with this device, have a ten-times longer life than without it.

The plant, in working operation, was very interesting. The *debris* of former types of pots and partitions gave evidence of the assiduity with which the problem had been attacked and followed up. The actual pots in operation worked without the explosions characteristic of other electrolytic sodium processes. The ladling-out of the metal was witnessed, and showed that the workmen had become familiar with the manipulations required. The current kept the electrolyte molten without the assistance of extraneous heating, except in starting. The arrangements for collecting the gases and producing nitric acid worked apparently as well as could be desired. The principal drawback to the process is the large amount of electrical power absorbed in the resistance of the porous partition, an inevitable concomitant of its use, but a disadvantage more than offset by the other features of the process, provided the process is run where power can be obtained cheaply.

Your committee admires the energy and pertinacity with which a very difficult problem has been attacked; the finding of a material which will be substantially unattacked by the fused salts used, and also act as a porous diaphragm, has been accomplished; the details of construction of the porous diaphragm have been well studied out; the idea of protecting its supporting walls by making them secondary anodes is ingenious and of practical advantage.

In view of the above conclusions, we recommend the award of the John Scott Legacy Premium and Medal to J. D. Darling, of Philadelphia, Pa.

Adopted at the stated meeting of the Committee on Science and the Arts, held Wednesday, October 2, 1901.

Attest: WM. H. WAHL, *Secretary.*

THE SAPPHIRES OF MONTANA.

Mr. George F. Kunz, the noted gem expert, in a recent publication, makes some interesting remarks on the occurrence of sapphires in Montana, which was first noticed in 1891, and appeared to be so promising that several companies were formed to mine for them systematically. The sapphire region extends for a distance of six miles along the Missouri River, the central point being Spokane Bar, about twelve miles from Helena; while another region lies about seventy-five miles east of this, centering at Yogo Gulch.

The gems of these two districts exhibit marked differences by which they may readily be distinguished. Mr. Kunz says of them: "Much beautiful material has already been obtained, but little of high value. Those from the Missouri bars have a wide range of color—light blue, blue-green, green and pink—of great delicacy and brilliancy, but not the deep shades of blue and red that are in demand for jewelry. . . . The Judith River region is more promising, the colors ranging from light blue to quite dark blue, including some of the 'cornflower' tint so much prized in the sapphires of Ceylon. Others incline to amethystine and ruby shades. Some of the more peacock-blue, and some . . . show a deeper tint in one direction than in another. Some of the 'cornflower' gems are equal to any of the Ceylonese, which they strongly resemble. W.

USES OF INFUSORIAL EARTH.

Infusorial earth, sometimes known as fossil meal, diatomaceous earth, and by other names, has of late years found numerous applications in the arts. Its principal use is in the manufacture of dynamite, the virtue of its property of absorbing and holding in suspension several times its weight of oily substances, such as nitro-glycerine. It is used also as an ingredient of soaps, the cleaning powers of which it assists principally by its mechanical action. It also affords a source of silica in the manufacture of the so-called soluble glass, or silicates of the alkalies. It is used in the manufacture of extremely light bricks and other compositions for fire-proof linings of magazines and the like, and in similar compositions as a filtering medium for water, and for other uses less important.

The *Chemical Trade Journal* mentions another suggestion in connection with the material, which is interesting: An argillaceous earth named "tfol," which contains free gelatinous silica, is largely used in Northern Africa by the Arabs as a substitute for soap in washing linen. Lahache finds that it has great capabilities of absorbing oil, one part of this substance completely absorbing five parts of heavy tar oil. When the compound is mixed with the water a perfect emulsion is formed, which does not adhere to the sides of the vessel. It is proposed to employ this earth for the purpose of emulsifying heavy tar oil for disinfecting purposes. For this purpose the tfol is first mixed with an equal weight of water, and then intimately incorporated with sufficient heavy tar oil to make a paste. W.

PROGRESS IN TELEGRAPHY.

The enormous strides that have attended the development of telegraphy during the nineteenth century are strikingly illustrated by some statistics recently issued by Sir W. H. Preece, K. C. B., late electrician to the English Post-office. In 1870 the number of words transmitted per minute was only 80; in 1890 the number had been increased to 450. In 1870, 9,850,177 messages were dispatched throughout the United Kingdom at a cost of $3,061,505, while in 1900, 89,576,961 telegrams were sent, bringing in a revenue of $17,296,765. The total number of government and private cables encircling the globe is at present 1624, covering a total length of 187,353,172 nautical miles.

MANUFACTURE AND USES OF METALLIC SODIUM.

BY JAMES D. DARLING.
Member of the Institute.

Sodium is one of the most abundant of metals, and is widely distributed in nature, as silicate in felspar, as a borate in borax, as fluoride in cryolite, and especially as chloride in sea-water and rock salt, and as nitrate in Chili saltpetre. It is, of course, never found occurring naturally as metal; and, on account of its strong chemical affinity for oxygen and chlorine, its production in the metallic state has always been a more or less difficult and expensive proceeding. Although the sodium unit in common salt is worth only a small fraction of a cent per pound, even to-day, with the most improved electrical methods of manufacture, sodium is far from being what can be called a cheap metal.

It was first isolated by Davy in 1808, who obtained it by electrolyzing fused sodium hydroxide. In 1808, the electric dynamo had not been discovered, and the current developed from batteries being entirely too costly for the production of sodium on a manufacturing scale, other means were devised for separating it from its compounds. Curandau, Gay Lussac, Thenard and Deville are names intimately associated with this early stage of the metallurgy of sodium. Deville, especially, brought its manufacture to a high degree of perfection, reducing the cost of a kilo from 2,000 francs in 1855, to 10 francs in 1859. He produced it at Salindres, France, in considerable quantities, to be used in the manufacture of aluminum, by strongly heating a mixture of sodium carbonate, coal and chalk in an iron retort, and condensing the volatilized alkali metal.

No improvements of any importance were made in the methods of production until Castner, after having worked for some time at the works of the Aluminium Company in England with a modified Deville process, devised (1890) a suitable furnace, and made sodium by the Davy method of electrolyzing fused caustic soda. Castner greatly reduced the cost of the metal and, in consequence, most of the sodium used in the arts is now made in this manner.

Many attempts have been made to make sodium electrically from the fused chloride, as that material is by far the most abundant and cheapest source of the metal, but up to the present time none of the numerous processes and furnaces patented has proven commercially successful. The high melting-point of the chloride and its strong corrosive action when in a molten state are the principal difficulties to be overcome.

Sodium nitrate and sodium hydroxide melt at 313° C. and 320° C., respectively, and there is no tendency of the sodium to volatilize at these temperatures, therefore, its collection is an easy matter. Another important point in their favor is that iron or nickel can be used for the anodes and for the containing vessels. With sodium chloride it is different. Its melting point is about 800° C., and sodium begins to distil below 900° C., so that when the fused chloride is electrolyzed, the metal comes off mostly as a vapor, which greatly increases the difficulties of collection. It is also necessary to use carbon for the anode and the anodes and everything that has so far been tried for a containing vessel has but a short life.

In 1889, the writer, while trying to devise a practical method of electrically reducing fused sodium nitrate to nitrite, conceived the idea of making metallic sodium and nitric acid from fused sodium nitrate. On açcount of the stress of other work, nothing was done to develop the idea until 1894. The difficulties attending the separation of such an easily oxidizable substance as sodium from so powerful an oxidizing agent as fused sodium nitrate seemed unsurmountable, and innumerable experiments were tried before a successful method was obtained.

The experimental work was done at the works of Harrison Bros. & Co., Inc., Philadelphia, and about a year ago a twelve-furnace plant, capable of decomposing from 700 to 800 pounds of nitrate per day, was erected there to demonstrate the practical working of the process. The first form of furnace, or electrolytic cell,* was only partly successful, but it served to emphasize what had already been recognized, viz., that to successfully separate sodium in the metallic state from a nitrate, the metal should not be liberated in contact with the fused nitrate, and there should be absolutely no opportunity for a recombination of the liberated ions to take place. This could only be accomplished by having an efficient porous diaphragm interposed between the positive and negative electrodes. The difficulty was, however, how to construct such a diaphragm. The high temperature and the destructive fluxing action of the fused electrolyte precluded the use of any of the porous diaphragms successfully used in electrolyzing aqueous solutions of salts, and it was only after a long series of disappointing trials of various substances and devices that seemed to promise a solution of the problem that the idea of a granular body of vitrified magnesium oxide, confined between walls of perforated sheet iron, was hit upon and a successful furnace constructed.

The plan ultimately adopted was to take pure magnesium oxide, fuse it to a vitreous mass in an electric furnace and, after grinding it to the proper degree of fineness, press it into a round cup having double walls of perforated sheet steel.† The porous cup thus made was 30 inches in height by 16 inches in outside diameter, with walls 4 inches thick, thus leaving the inside dimensions of the cup 26 inches deep by 8 inches in diameter. The thickness of the walls of the cup and the degree of fineness of the granular fused magnesia governed the electrlcal resistance, as well as the resistance to diffusion of the electrolytes, and numerous experiments were required to determine these two important factors correctly.

The rest of the furnace was very simple in construction. A cast-iron pot set in a brick furnace to contain the nitrate to be decomposed also acted as the anode, or positive electrode. A 6-inch layer of refractory insulating material was placed

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* U. S. Patent 517,001, March 20, 1894.

† U. S. Patent 590,826, September 28, 1897.

in the bottom of the pot, and the porous cup rested centrally on it, leaving a 3-inch space between the cup and the pot. This space was then filled with sodium nitrate, and the cup itself nearly filled with melted sodium hydroxide. The cathode or negative electrode, consisting of a short length of 4-inch wrought-iron pipe provided with proper electrical connections, was suspended inside the cup, reaching nearly to the bottom thereof. *Fig. 1* shows a general view of twelve such furnaces, showing the cast-iron pots imbedded in brick work; also the bridges made from wrought-iron pipe for supporting the cathodes in the porous cups.

When external heat is applied to the furnace the electrolytes melt and, permeating the walls of the cup, allow the current to pass. The result of the passage of a current of electricity of a suitable strength through such an arrangement is the decomposition of the sodium nitrate into sodium, nitrogen dioxide and oxygen. The nitrogen dioxide and oxygen are liberated as gases at the positive electrodes, and escape through the hole in the cover provided for that purpose. The positive sodium ions pass through the walls of the cup and on through the molten sodium hydroxide, to be ultimately liberated in the metallic state at the cathodes. The first sodium liberated is absorbed by, or combines with, the sodium hydroxide, hydrogen gas being evolved, and sodium monoxide probably being formed. After some time sodium rises to the top of the electrolyte in the cups, and at intervals of about one hour is dipped off with a spoon and preserved under mineral oil.

This style of porous cup and furnace gave excellent results. The use of two electrolytes of different characters, yet having a common base, allows of the sodium being liberated in a neutral medium away from all danger of oxidation by the nitrate from which it is obtained.

Various improvements have since been made in the construction of the cup. At first the perforated sheet steel walls had a very short life, being quickly eaten away by local action caused by the secondary effects of the current. This trouble was overcome by shunting about 5 per cent. of the current directly through the metal walls of the cup,

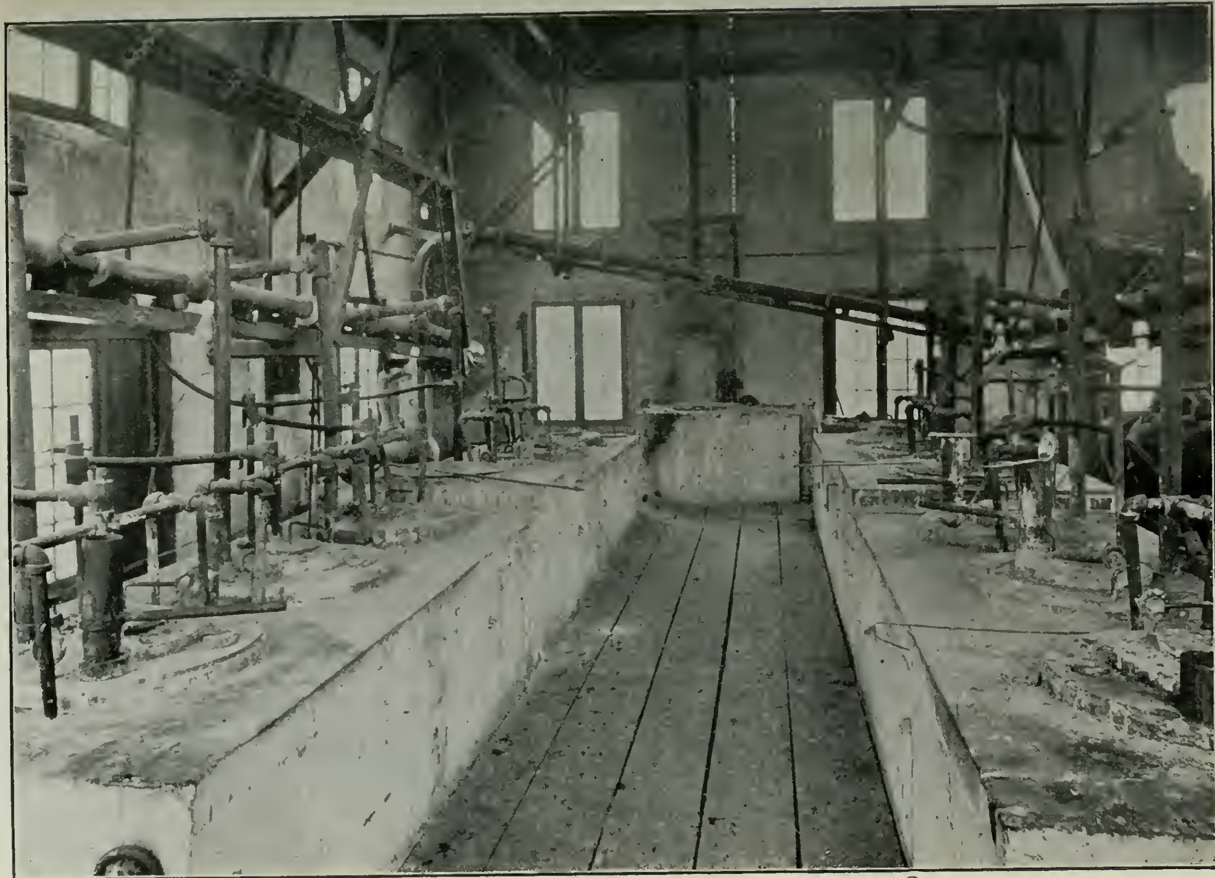

FIG. 1.—General view of sodium furnaces.

making them positive. This plan greatly reduced the local action and increased the life of the cup about ten times.*

The first cost of the electrically fused magnesia filling for the cup was very high, so a cheaper material was looked for. This was found in a mixture of ground, dead-burned magnesite and Portland cement. It is mixed with water, poured between the perforated walls of the cup, and after it has set and hardened it makes a very efficient and satisfactory diaphragm.† Reasoning *a priori* one would expect Portland cement to be readily attacked by fused sodium hydroxide and sodium nitrate, but experience has shown that it stands the fluxing action of these two substances remarkably well.

The nitrogen dioxide and oxygen evolved at the positive poles are conducted by means of earthenware pipes to a number of receivers or Woulff bottles, connected together and containing water. (See *Fig. 2*). The NO_2 gas coming in contact with the water combines with it to form nitric acid, $3NO_2 + H_2O = 2HNO_3 + NO$. The NO takes up a molecule of oxygen to again form NO_2, and more nitric acid is formed. If it is desired to make a very strong acid for use in the manufacture of high explosives, a system of towers that automatically brings the strength of the acid up to required degree is used.

Each furnace takes a current of about 400 ampères at an average E.M.F. of 15 volts. External heat is used only when starting up and when changing the cups, which have a life of 425 to 450 hours; at other times the heat generated by the resistance to the passage of the current is sufficient to keep the eleetrolytes melted. *Fig. 3* shows the switchboard with rheostats, meters, etc., for controlling and measuring the current, also the connections to the furnaces and the workmen engaged in dipping off sodium from the furnaces.

The general efficiency of the process is high, and although a higher voltage and, consequently, more power is required than is needed to decompose caustic soda, yet, on account

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* U. S. Patent 641,438, January 16, 1901.

† U. S. Patent 641,376, January 19, 1901.

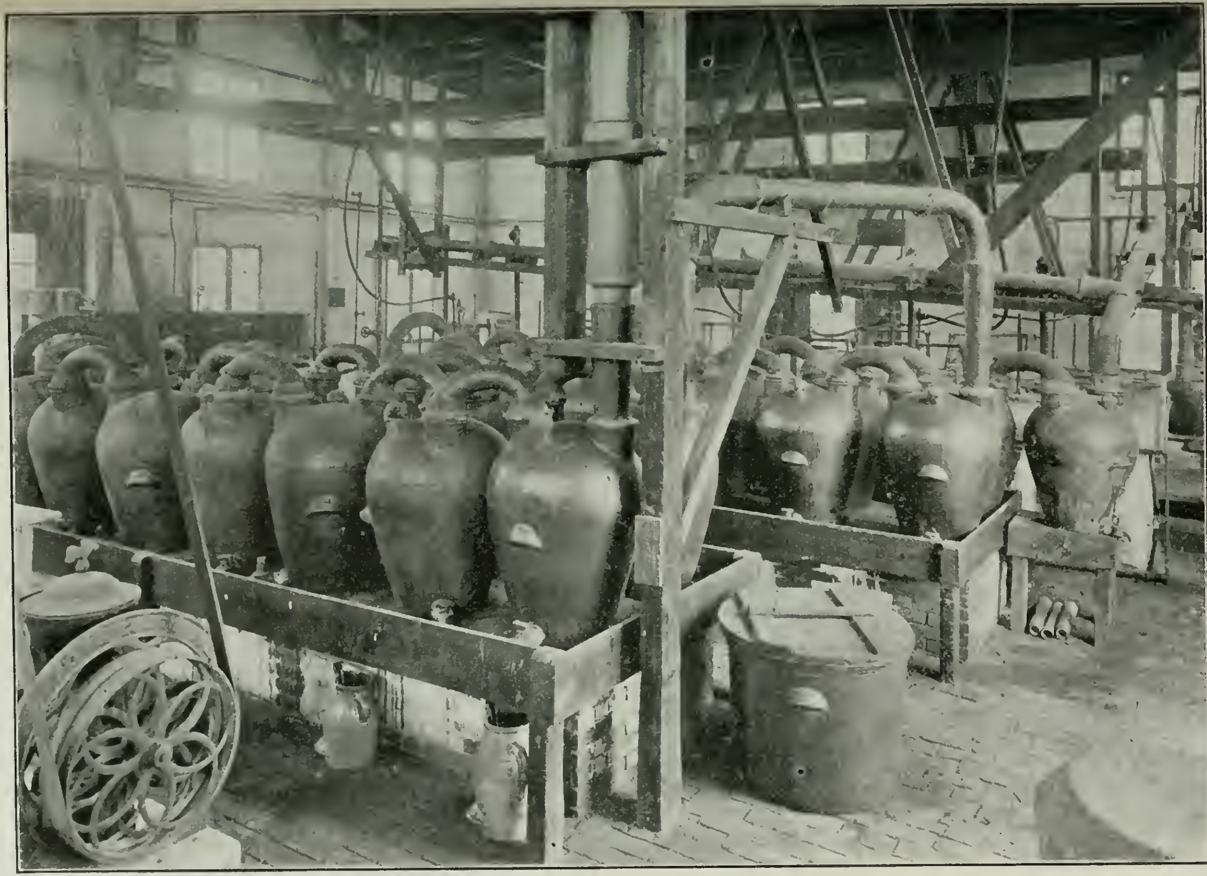

FIG. 2.—Apparatus for converting nitrogen dioxide into nitric acid.

of the valuable by-product of nitric acid obtained, the cost of the sodium produced is much less than when it is made from caustic soda.

Up to ten or eleven years ago about the only use for metallic sodium, outside of the laboratory, was in the manufacture of aluminum, and when the electrolytic method of making aluminum was discovered, it looked as if the production of sodium on a large scale would cease. It was only when electricity was also applied to the production of sodium that it could be produced cheaply enough to be used in fields that had hitherto been closed to it on account of its high cost. Chief amongst these new uses is the manufacture of alkaline cyanides so largely used in the extraction of gold from low-grade ores and tailings for electro-plating in photography, and other minor uses. Large amounts are also converted into sodium peroxide, to be used in bleaching wool, silk, feathers, etc., replacing the more expensive hydrogen peroxide in that work.

It is also used in making certain aniline colors and organic compounds, and wherever a powerful reducing agent is needed.

The conversion of sodium into sodium peroxide is easily accomplished by burning it in an excess of dry air, free from carbon dioxide, in a suitable externally-heated retort. On removal from the retort it is ground while still hot to a fine powder, and, as it deteriorates quickly on exposure to the air, it is packed and shipped in air-tight cans. It is a most powerful oxidizing agent, and must be handled with great care. The commercial article contains about 18 per cent. of available oxygen when fresh.

The old method of making potassium cyanide, first described by Leibig, was to take dehydrated ferro-cyanide of potassium and heat it with potassium carbonate—$2K_4Fe(CN)_6 + 2\,K_2CO_3 = 10\,KCN + 2\,KCNO + Fe + CO_2$. The resulting cyanide contained cyanate, but this did not materially interfere with its use. An almost pure cyanide can be obtained by heating the ferro-cyanide alone until it decomposes into potassium cyanide, nitrogen, and a compound of iron and carbon — $K_4Fe(CN)_6 = 4\ KCN + N_2 + FeC_2$.

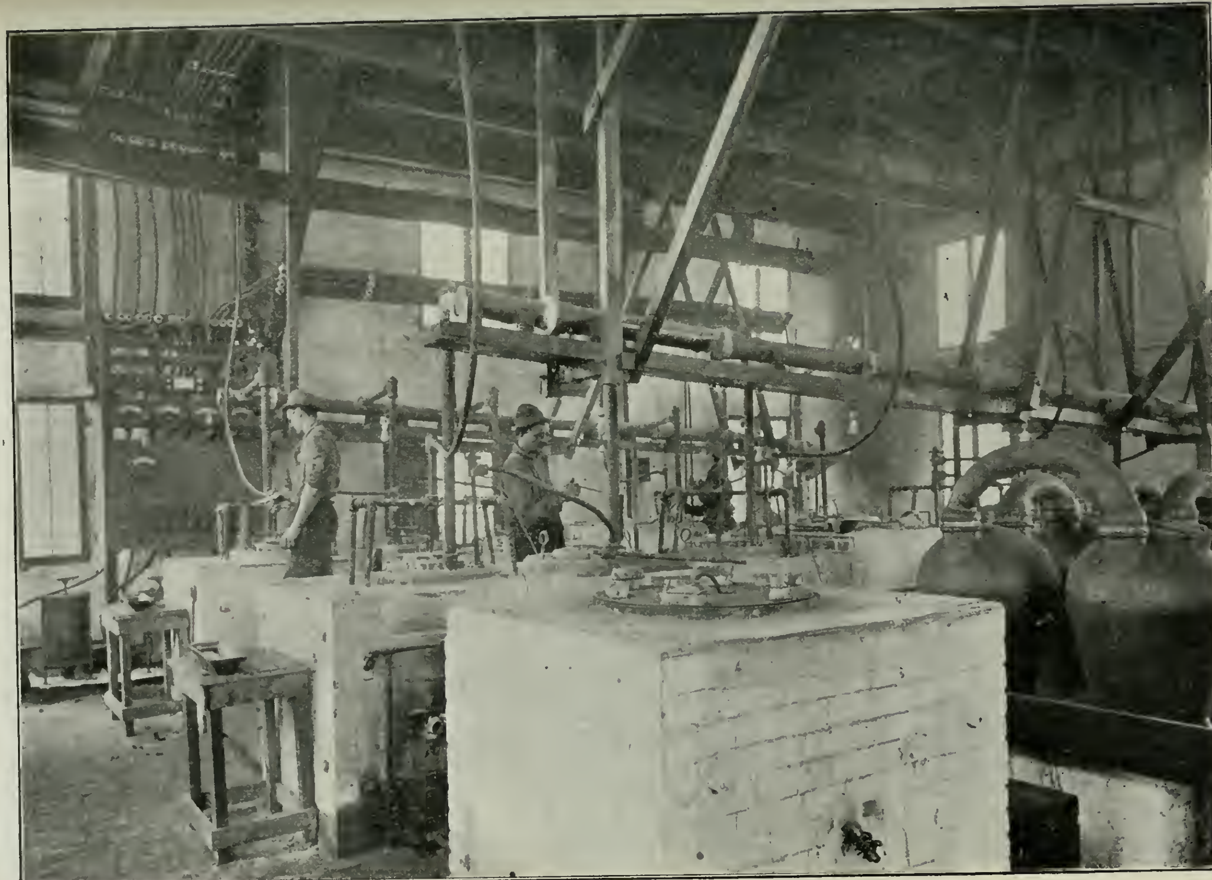

FIG. 3.—Switchboard and connections to sodium furnaces.

This method entails the loss of one-third of the nitrogen contained in the ferrocyanide, and to avoid this waste of nitrogen, Erlenmeyer proposed to add the proper amount of an alkali metal to the melted ferrocyanide—$K_4Fe(CN)_6 + 2\ Na = 4\ KCN + 2\ NaCN + Fe$. It is in this way that the most of the so-called chemically pure potassium cyanide now sold is made. The product is not a pure potassium cyanide, but a mixture of potassium and sodium cyanides. It also contains a considerable quantity of potassium carbonate, which is added to it during the course of manufacture to reduce its strength, for the combined cyanides, made according to the above formula, have a higher percentage of cyanogen than chemically pure potassium cyanide could possibly have. An inert material like potassium carbonate is therefore added in sufficient quantities to reduce the cyanogen contents to 39–40 per cent., which is equivalent to 98–100 per cent. cyanide of potassium.

Other processes have been devised for using sodium in making cyanides. One is to first convert the sodium into sodium amid by heating it in contact with ammonia gas, $Na + NH_3 = NaNH_2 + H$, then heating the amid with carbon to form cyanide $NaNH_2 + C = NaCN + H_2$.

Another and later method by which it is claimed a better yield is obtained, is to form at a temperature of about 400° C.. a stable cyanamid from alkali amid and carbon, according to the formula—$2\ (NaNH_2) + C = Na_2N_2C + H_4$, then reacting the produced cyanamid with a further quantity of carbon at a temperature of 800° C. to form cyanide, according to the reaction—$Na_2N_2C + C = 2\ NaCN$. The objection to these methods is the large amount of expensive sodium metal needed for a given output of cyanide.

The writer has lately devised a process for using sodium in the synthetic production of sodium cyanide that gives good results, and in which the larger portion of the metallic base required is furnished in the form of caustic soda, and only a small amount of sodium is needed to finish the process. By this process a moderate sized sodium plant can produce enough metal to manufacture a large amount of the cyanide. The details of this new and interesting method of making sodium cyanide will be published later.

THE LIQUEFACTION OF HYDROGEN.

A note by Mr. M. W. Travers, as abstracted in the *Transactions* of the British Institution of Civil Engineers, gives an account of the method whereby the author has succeeded in obtaining liquid hydrogen in quantity. The apparatus consists essentially of a modified Hampson air-liquefier. The hydrogen, at a pressure of 200 atmospheres, undergoes a preliminary cooling —80° C. in solid carbonic acid and alcohol. It is then successively cooled by liquid air boiling under atmospheric pressure, and under a pressure of 100 millimeters, after which it escapes from the Hampson valve, and being sufficiently imperfect gas at the low temperature (—200° C.) obtained by the liquid air boiling under low pressure, regenerative cooling is produced, as in the liquefaction of air by the Hampson machine, and liquid hydrogen is obtained. It is collected in a vacuum vessel which is sepecially insulated from external heat.

BOOK NOTICES.

Practical Electric Railway Handbook. By Albert B. Herrick. Pocketbook form, pp. 407. New York : Street Railway Publishing Company. (Price, $3.00.)

The author of this work is well known to the electric railway fraternity as an expert engineer of tests in electric railway operation. He has embodied in his work above entitled the leading facts and data bearing on the efficiency of electric railway operation and has endeavored to make the subject as free from mathematical abstrusities as possible. W.

Lessons in Practical Electricity. By C. Walton Swoope. 8vo, pp. XIII + 413. New York : D. Van Nostrand Company, 1901. (Price, $2.00.)

The work above entitled originated from the need felt by the students of the Spring Garden Institute, with which the author was associated for years as an instructor, for an elementary text-book elucidating the principles of the science, and illustrating its application in practice.

The original work entitled "Lessons in Practical Electricity," fulfilled its purpose so well that the present enlarged and thoroughly revised edition has been issued. W.

Electrical Catechism. An introductory treatise on electricity and its uses. By Geo. D. Shepardson, M E. Large 8vo, pp. 403. New York : American Electrician Company, 1901. (Price, $2.00.)

The author has endeavored to present in this volume, in non-technical language, the information sought by electrical workmen, superintendents and others employed in electrical factories, the essential principles of the science. The work should prove of very decided benefit to the class of electrical operatives. W.

Jahrbuch der Elektrochemie. Berichte über die Fortschritte des Jahres, 1900. Von Dr. W. Nernst und Dr. W. Borchers, VII, Jahrgang. 4to pp. 496. Halle a/S. Verlag von Wilhelm Knapp, 1901.

The yearbook of progress edited by Drs. Nernst and Borchers has for years been the only source of reference to the progress of the electrochemical industries available to students of this branch of applied electricity. It is simply invaluable and indispensable to all who are interested in the electrolytic and electrothermic industries. W.

The Induction Motor. A short treatise on its theory and design, with numerous experimental data and diagrams. By B. A. Behrend, M.I.C.E., etc. 8vo, pp. 105. New York: Electrical World and Engineer, Inc., N. D. (Price, $1.50.)

The author justifies the writing of this treatise by the fact that the induction motor has thus far received little attention from the authors of electrical literature—the few whose experience and knowledge of the subject would entitle them to speak authoritatively on this subject being deterred from publishing for commercial reasons.

The author treats the subject under eight chapter heads, respectively, as follows: I. The general alternating-current transformer; II (*a*) the character of the magnetic field in the polyphase motor; (*b*) the formulæ for the 3-phase-current motor; III, the short-circuit current and the leakage factor; IV, the leakage factor; V, design of a 3-phase-current motor for 200 horse-power; VI, the single-phase motor; VII, calculation of a single-phase current motor; VIII, the polar diagrams of the general alternating-current transformer. Appendices. W.

Franklin Institute.

[*Proceedings of the stated meeting held Wednesday, December 18, 1901.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, December 18, 1901.

President JOHN BIRKINBINE in the chair.

Present, 204 members and visitors.

Additions to membership since last month, 17.

The Secretary reported the gift of a portrait in oil of the late Joseph Harrison, Jr., well known in connection with the development of the locomotive engine, the equipment of the Russian railways, and safety steam-boiler design. The donor is Mr. Henry Harrison Suplee.

The special committee appointed at the previous meeting to consider and report upon certain preambles and resolutions "to promote commerce," offered by Prof. Louis M. Haupt, made the following report:

To the Franklin Institute.

Your committee appointed at the regular meeting of the Institute, held November 20th last, to consider the development of the rivers, harbors and other waterways of the country in order to promote both domestic and foreign commerce, and also to consider the establishment by the Government of a Department of Commerce and Industries, have given the two subjects careful consideration, and submit herewith a set of preambles and resolutions embodying the results of their deliberations.

RESOLUTIONS TO PROMOTE COMMERCE.

WHEREAS, Greater and cheaper transportation facilities are essential to the development of our domestic and foreign commerce ;

WHEREAS, The limited appropriations made by the National Government are inadequate to meet the requirements of the country, covering only a small percentage of the approved projects ;

WHEREAS, The former policy of authorizing the improvement of our waterways by private capital resulted in a marked development without material cost or risk to the general Government ;

WHEREAS, Early and economical results may be secured by a partial return to this method in localities where no provision has been made for immediate improvement ; therefore, be it

Resolved, That the proper department of the general Government should be empowered to authorize the improvement, by individuals or corporations, of such rivers, harbors, canals or other waterways as are not provided for under the River and Harbor or the "Sundry Civil" Bills, with authority to collect tolls, upon plans and regulations to be approved by the said department ; *Provided*, that no part of the funds for such work shall be drawn from the public treasury of the United States. Also,

WHEREAS, The commercial and industrial interests of the country have attained such magnitude as to require a more systematic organization for their regulation and development ;

WHEREAS, A measure known as Senate Bill No. 738 has been introduced into Congress for the purpose of establishing a Department of Commerce and Industries ; therefore, be it

Resolved, That the establishment of such a Department of Commerce and Industries has the hearty approval of the Franklin Institute, and its early inauguration is recommended.

(Signed) THOMAS P. CONARD, *Chairman*.
LEWIS M. HAUPT,
L. Y. SCHERMERHORN.

The report was unanimously adopted, and the Secretary was instructed to send a certified copy of the same to both Houses of Congress.

General Joseph Wheeler, U.S.A., was introduced and made an address "On The Evolution of Small Arms and Ordnance," with especial reference to the improvements in automatic, rapid-fire, non-recoiling guns made by Dr. Samuel N. McClean.

General Wheeler's remarks were supplemented by Dr. McClean, who described his invention in general terms, and gave a demonstration of the capabilities of the weapon.

The address of General Wheeler was referred to the Committee on Publication, and the invention to the Committee on Science and the Arts for investigation and report.

The following nominations for officers, managers and members of the Committee on Science and the Arts were made, to be voted for at the annual election to be held on Wednesday, January 15, 1902, viz.:

For President	(to serve one year)	JOHN BIRKINBINE.
" *Vice-President*	(" three years)	WASHINGTON JONES,
" *Secretary*	(" one year)	WM. H. WAHL.
" *Treasurer*	(" ")	SAMUEL SARTAIN.
" *Auditor*	(" three years)	WM. H. GREENE.

For *Managers* (to serve three years).

BENJ. S. LYMAN,	THOS. P. CONRAD,
COLEMAN SELLERS,	JAMES M. DODGE,
ISAAC NORRIS,	C. LELAND HARRISON,
STACY REEVES,	FRANCIS SCHUMANN.

For *Members of the Committee on Science and the Arts* (to serve three years).

HUGO BILGRAM,	LEWIS M. HAUPT,	C. J. READ,
FRANK P. BROWN,	FRED. E. IVES,	E. ALEX. SCOTT,
J. J. DE KINDER,	A. E. KENNELLY,	COLEMAN SELLERS,
W. C. L. EGLIN,	WILFRED LEWES,	H. W. SPANGLER,
A. M. GREENE, JR.,	EDGAR MARBURG,	MARTIN I. WILBERT.

Adjourned. WM. H. WAHL, *Secretary*.

SECTIONS.

(*Abstracts of Proceedings.*)

PHYSICAL SECTION.—*Stated Meeting*, November 27th, 8 P. M. Dr. Stradling in the chair.

The nomination of officers of the Section for the year 1902 was referred to a special committee composed of Mr. Wm. McClellan, Dr. Wahl and Prof. Richard Zeckwer.

Dr. A. Stanley Mackenzie presented a paper on "The Question of the Divisibility of the Atom." Discussed by Drs. Stradling, Goldschmidt, Leffmann, and Messrs. E. A. Sperry (visitor) and Pawling. (The paper is reserved for publication).

The same speakers gave the results of two investigations made at the Physical Laboratory of Bryn Mawr College. The first, by Mary J. Northway and Professor Mackenzie, was "On the Period of a Rod Vibrating in a Liquid;" the second, by Elizabeth R. Laird, was "On the Absorption Spectrum of Chlorine." Both papers were freely discussed.

Dr. Geo. F. Stradling followed with a brief résumé of some recent work in acoustics and gravitation, viz.:

E. H. Stevens has determined the speed of sound in air up to a temperature of 1000° C. by an interference method, finding it to be 700·3 meters per second.

The work of C. Sabine, which appeared in the *American Architect*, was referred to, in which the damping effect of various substances upon sound waves was determined.

Bergen Davis has found a new form of rotation produced by some waves. Four cylinders having one end open are attached perpendicularly to the ends of as many arms extending from a pivot. They are in a plane perpendicular to the axis of rotation. If this is placed in a sound wave so that the axis is parallel to the direction of the motion of the air particles, rotation takes place. By using it, nodes and loops of stationary waves can be located.

About thirty years ago Cornu and Mercadier determined the intervals between the notes occurring in a melody and seemed to show that they followed the Pythagorean scale. Zambiasi has taken up the question and concludes from his experiments that the relations in melody are those of the theoretical acoustical scale, 5/4, 5/3, etc.

The apparent change of weight occurring when chemical actions take place has once more been observed. Heydweiller finds in nearly all the reactions with which he worked a decrease in weight, the largest being 21 milligrams, when iron is in contact with basic cupric sulphate. None of the reactions was very vigorous. The reacting masses were about 200 grams.

Stated Meeting Tuesday, December 17th. Dr. Stradling in the chair.

Mr. Harvey M. Watts, of Philadelphia, presented a paper "On the Mechanism and Causation of Hot Waves," illustrating the subpart with the aid of a series of lantern slides. Discussion followed. [The paper will appear in full in the JOURNAL]. Adjourned.

JESSE PAWLING, JR.
Secretary.

CHEMICAL SECTION.—*Stated Meeting* held Friday, November 29. Prof. Joseph W. Richards, in the chair. Present, 48 members and visitors.

It was resolved, on the recommendation of Dr. Keller, Chairman of the Executive Committee, that the stated meeting of the Section in December be omitted, and that the Section members be requested to participate instead in the Annual Meeting of the American Chemical Society which is to be held in Philadelphia in the latter part of December.

On Dr. Wahl's motion, the President of the Section was authorized to appoint a special committee to report nominations for officers for the year 1902, at the stated meeting of January.

Dr. Robt. H. Bradbury gave an account of the Goldschmidt method of reduction and the production of extremely high temperatures by the use of powdered aluminum. The special novelty of this procedure resides in the circumstance that the substances (usually mixtures of finely powdered metallic oxides with powdered aluminum) are caused to react without the aid of external heat, the reaction being initiated by the ignition of a fuse of magnesium (or its equivalent) inserted in the charge. When once started the heat developed in the reaction suffices to spread the reaction throughout the entire charge.

The speaker illustrated the process by a number of experiments showing the reduction of MnO_2, Fe_2O_3, $CaSO_4$, and numerous other interesting reactions. Of special interest was the reaction of powdered aluminum with Al_2O_3, demonstrating apparently the formation of a lower oxide; the energetic decomposition of water; and the reduction of sodium phosphate with the production of phosphorus.

Dr. H. F. Keller followed with an exhibition of a fine series of lantern slides illustrative of the history of chemistry. One series consisted of portraits of the masters of the science, and another principally of apparatus employed in epoch-making discoveries.

The speakers were given a vote of thanks and the meeting adjourned.

W. E. RIDENOUR,
Secretary.

MECHANICAL AND ENGINEERING SECTION.—*Stated Meeting* held Thursday, December 12th. Mr. A. M. Greene, Jr., in the chair. Present, 27 members and visitors.

Mr. James O. Nixon, of the Link-Belt Engineering Company, gave an account of the Renold Silent Chain Gear, illustrating the subject by specimens of the device, and by a series of views exhibiting its application for driving machinery where its advantages were specially apparent. The discussion which followed was participated in by Messrs. Fullerton, Greene, and the author.

Mr. Greene made some remarks on the so-called tangential or impulse water-wheels, illustrating the subject by the exhibition of Pelton and Dobel styles of buckets.

On Mr. Greene's suggestion a special committee was appointed to arrange for the presentation at the meetings of abstracts on current advances in engineering and the mechanical arts.

The Executive Committee was instructed to present nominations for officers for the year 1902, at the stated meeting of January 8th. Adjourned.

DANIEL EPPELSHEIMER, JR.,
Secretary.

MINING AND METALLURGICAL SECTION.—*Stated Meeting* of Wednesday, December 11th. F. L. Garrison, President, in the chair.

Mr. Wm. Campbell, of Columbia University, New York, presented a supplement to the paper of Dr. Mathews on "The Constitution of Metals and Binary Alloys." The subject was profusely illustrated with lantern slides.

G. H. CLAMER,
Secretary.

ELECTRICAL SECTION.—*Stated Meeting* held Thursday, December 19th. Mr. Morris E. Leeds in the chair. Present, 50 members and visitors.

The Executive Committee was instructed to report to the stated meeting of January a list of nominations for officers to serve for the year 1902.

Mr. Paul M. Lincoln, of Niagara Falls, presented a paper entitled, "On the Parallel Operation of Alternating Current Generators," which was freely discussed. [The paper will appear in full in the JOURNAL]. Adjourned.

THOS. SPENCER,
Secretary pro tem.

JOURNAL

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THE FRANKLIN INSTITUTE.

Stated Meeting, held Wednesday, November 20, 1901.

THE NATIONAL BUREAU OF STANDARDS.

BY SAMUEL W. STRATTON, Director.

From the beginning of this republic many of the foremost statesmen and scientists have worked assiduously to bring our system of weights and measures to a more satisfactory condition. Washington repeatedly urged upon Congress the necessity for uniform and reliable standards, and in his third annual message to Congress states that "an improvement in the weights and measures of the country is among the important objects submitted to you by the Constitution, and if it can be derived from standards at once invariable and universal may be not less honorable to the public councils than conducive to the public convenience." Thomas Jefferson, then Secretary of State, was directed by Congress to report upon this subject and, after a most careful consideration, submitted a report in which he outlined two alternative plans, one based upon the retention of the then existing standards, fixing them, however, by some invariable standard, and the other a decimal system based upon the length of a pendulum beating seconds. President Madison, in 1817, reminded Congress that nothing had been accomplished in reforming and unifying the system, whereupon the whole subject was referred to John Quincy Adams, then Secretary of State. Mr. Adams gave four years of historical research and mathematical study, and prepared a report which has become a classic in metrology, but which advised delay until the time when nations had agreed on a universal standard, or until the subject of a universal standard had received more attention.

Notwithstanding these efforts and the reports of various individuals and committees, Congress has never exercised the power delegated to it by the Constitution, with the exception of an Act of May 19, 1828, relative to the adoption of a troy pound as a standard to be used in the coinage of money, the language of which is as follows: "For the purpose of securing a due conformity in weight of coins in the United States to the provisions of this title the brass pound weight procured by the Minister of the United States at London, in the year eighteen hundred and twenty-seven, for the use of the Mint, and now in the custody of the Mint at Philadelphia, shall be the standard Troy pound of the Mint of the United States, conformably to which the coinage of the United States shall be regulated."

Previous to this, by an act passed in 1799, Fifth Congress, second session, it was ordered that "The Surveyor (of each port of the United States) shall, from time to time, and particularly on the first Monday in January and July each year, examine and try the weights, measures and other instruments used in ascertaining the duties of imports, with standards to be provided by each collector at the public expense for that purpose, and when disagreements or errors are discovered, he shall report the same to the collector, and obey and execute such directions as he may receive for the correction thereof, agreeably to the standards aforesaid.

Apparently this act was not enforced, probably for the reason that no standards had been adopted by Congress or by the Treasury Department. On May 29, 1830, the Senate passed a resolution directing the Secretary of the Treasury to have an examination made of the weights and measures in use at the principal custom-houses. The duty of making the examination was intrusted to Mr. F. R. Hassler, Superintendent of the United States Coast and Geodetic Survey, who was at that time the only man of recognized scientific attainments in the employment of the Treasury Department. Mr. Hassler, under date of January 27, 1832, reported that he had found large discrepancies among the weights and measures in use. He was thereupon directed by the Secretary of the Treasury to secure apparatus and establish a shop wherein copies of certain standards adopted by the Treasury Department could be made for distribution to the various custom-houses. The avoirdupois pound adopted was derived from the troy pound of the mint, and the distance between certain lines on a metal bar in the possession of the Department, and supposed to conform with the English yard, was taken as the standard of length.

In June, 1838, Congress passed the following resolution:

"*Resolved*, That the Secretary of the Treasury be, and he hereby is, directed to cause to be made a complete set of all the weights and measures adopted as standards, and now either made or in progress of manufacture for the use of the several custom-houses, or such persons as he may appoint, and for the use of the States respectively, to the end that a uniform standard of weights and measures be established throughout the Union."

On July 27, 1866, the following joint resolution was adopted: Be it

"*Resolved*, by the Senate and House of Representatives of the United States of America in Congress assembled, that the Secretary of the Treasury be, and he is hereby, authorized and directed to furnish to each State, or to be delivered to the Governor thereof, one set of standard weights and measures of the metric system for the use of the States respectively." Here, again, the selection of the standards necessary to carry out the foregoing resolutions was left to the Secretary of the Treasury, who procured from abroad copies of the original metric standards.

Since that time Congress has provided for the construction and verification of the standard weights and measures for the custom-houses of the United States and for the several States. The custody of the standards adopted by the Treasury and the carrying out of the provisions made by Congress remained until July 1, 1901, under the direction of the Superintendent of the Coast and Geodetic Survey, who served also in the capacity of Superintendent of the Office of Standard Weights and Measures.

In 1866 the metric system, while not adopted by Congress, was made lawful throughout the United States, but the standards of this system were not yet satisfactory, and in 1875, more than half a century after Adams had recommended a conference between nations for the purpose of establishing world-wide uniformity in standards, such a conference was held, and, as a result, there was established in Paris a permanent international bureau of weights and measures. In the negotiations leading up to the establishment of this bureau, Prof. Joseph Henry and Mr. J. E. Hilgard represented the United States. Many of the great scientists of the world were engaged for several years in perfecting prototypes of metric standards, and in 1899 these were ready for distribution among the seventeen nations represented in the International Conference. So important were considered the details of bringing the standard meter and kilogram to the United States that the Department sent special commissioners to Paris for them. Mr. George Davidson, then assistant in the United States Coast and Geodetic Survey, and Mr. O. H. Tittman, now superintendent of that department, accompanied these valuable standards from Paris to Washington, where they were opened in the presence of the President, the Secretary of State, and a distinguished company of scholars invited to the White House on that occasion. These standards were then placed in the custody of the office of Standard Weights and Measures. They are now used as the basis of all comparisons of mass and length in this country, even in the common systems. There is at the present time no satisfactory standard yard in the possession of the Government. The early standards procured by the Secretary of the Treasury are so poorly constructed, and the lines so broad that a more accurate yard may be made by means of the standard meter. Troy and avoirdupois pounds submitted for inspection are tested with metric weights or with standards which have been derived from the standard kilogram. That is to say, the standard meter and kilogram are so well constructed that more accurate standards in our common systems may be produced in this manner than by comparison with any standard pounds or yards in existence.

It has been seen that, until very recently, the provisions made by Congress covered only the common measures of weight, length, and capacity. The facilities of the office of Standard Weights and Measures were exceedingly limited, and the exercise of its functions confined to departments of the general government and the States. The set of standards furnished each State consisted originally of the most ordinary standards of length, weight and capacity. Later, a few metric standards were added to these, but in few instances have these States provided facilities for the proper comparison and use of these standards by the general public. In some States the standards have been destroyed or lost track of, and we find little legislation pertaining to weights and measures other than to fix the number of pounds to the bushel, to be used in measuring grain and other products. An investigation carried on last summer by a government official disclosed a condition of affairs in regard to the common weights and measures of the country that would hardly bear publication. I am told that in your own city of Philadelphia there is no official whose duty it is to inspect the weights and measures in common use. This statement, however, has not been verified, and I sincerely hope it is not true. The duties of State and city dealers should include the testing of water and gas meters, and perhaps, eventually, electric meters; but certainly all of these standards and measuring instruments are in common use by the people. And it is the duty of the general government to provide such officers with suitable standards, measuring instruments, and instructions governing their use. As it is, the general public, scientific institutions and other vast interests are unprovided for, even in the most common cases.

The progress of science and the employment of exact scientific methods by the great industrial and commercial enterprises have brought about conditions which demand a radical change in matters pretaining to standards. Whatever may be said as to the necessity of improvement in the common measures of weight, a length and capacity is *equally* true of the more accurate measurements of these same quantities as applied to scientific investigations in the laboratories of our educational institutions, scientific societies, and manufacturing interests, which demand for the successful completion of their work standards of the very highest order of accuracy. The public interest demands a far greater variety and range of standards than heretofore, such as those used in the measurement of high and low temperatures, pressure, standards of illumination, electrical standards of resistance, current, electromotive force, capacity and self induction, polariscopic, barometric, and many other standards. In many cases the problems involved are far more difficult and complex, and reliable standards difficult to procure. The examination and comparison of standards require the use of the most delicate instruments known to science under the most advantageous conditions as to temperature, freedom from vibration, and laboratory conveniences.

In recent years the demand for accurate scientific standards, and in many cases commercial standards has been met by the standardizing institutions of foreign governments (to which our scientific laboratories and manufacturers have had to repair for them). Frequently the benefits to be derived by the public from fixed and reliable standards are through the medium of a great variety of meters and precise measuring apparatus, such as balances, apparatus for the measurement of lengths, capacity, graduated glassware, pressure-gauges, thermometers, barometers, metrological apparatus, pyrometers, gas and water meters, voltmeters, ammeters, wattmeters, oil-testers, the instruments used in surveying, navigation and hydrography, apparatus for testing the strength of materials and other properties, and many other instruments; that the graduations and indications of these instruments should agree with the fundamental standards is a question of the most vital importance, and without the facilities for such tests and comparisons the public is deprived of the greatest benefits to be deprived from such standards as may have been recognized by the Government.

The manufacture of physical, astronomical, chemical, and other scientific apparatus has been confined almost exclusively to foreign countries, but this industry is growing in the United States at a rate which will soon place our productions on an equality with those of any other country. Our manufacturers of such apparatus have shown that they can compete with the foreign products in workmanship and design. They have, however, been placed at a great disadvantage, owing to the fact that this Government has not provided them with the necessary standardizing facilities. German and English manufacturers furnish official certificates with their apparatus, and the value of such certificates is so well recognized that we find our own manufacturers quoting prices on their apparatus which has been verified in the institutions of foreign governments.

It was this condition of affairs that led Congress to enact a law March 3, 1901, establishing under the Treasury Department, the National Bureau of Standards, the functions of which include the custody of the official standards, and comparison of all standards used in scientific investigations, engineering, manufactures, commerce, and in educational institutions, with the standards adopted or recognized by the Government, the construction when neccessary of standards, their multiples, and submultiples, the testing of standard measuring apparatus, and the solution of problems which arise in connection with standards. It is also authorized to make physical and chemical researches for the purpose of determining physical constants, and the properties of materials when such data are of great importance to scientific or manufacturing interests. The Bureau is authorized to exercise its functions for the departments of the Government, for any State or municipality within the United States, scientific society, educational institution, firm, corporation, or individual engaged in manufacturing or other pursuits requiring the use of standards, or standard measuring instruments.

A suitable site has been purchased by the Government adjacent to the picturesque national reservation known as Rock Creek Park in the northern suburbs of Washington, a locality free from mechanical and electrical disturbances. Plans are being prepared for a physical laboratory which will be equipped with apparatus and conveniences for carrying on investigations and testing standards and measuring instruments of all kinds, and a smaller building to be known as a mechanical laboratory which will contain the power and general electrical machinery, the instrument shop, refrigerating plant, storage batteries, dynamos for experimental purposes, and laboratories for electrical measurements requiring heavy currents.

These laboratories will be constructed immediately, but the plans are being made with reference to the future growth of the Bureau, which will soon become one of the most important scientific institutions of our Government. The plans will include at least four buildings in addition to those now provided for.

The scientific work of the Bureau will be under the immediate supervision of a director, assisted by a corps of physicists and chemists designated as laboratory assistants, assistant physicists or chemists, and physicists or chemists with salaries ranging from $1,000 to $5,000. These salaries are somewhat more liberal than the Government has hitherto paid the scientific men in its employ. These men have been carefully selected by civil service examinations prepared with reference to the kind of work to be undertaken. No attempt has been made by politicians or others to influence the appointments in the Bureau, and the

Bureau once established upon this basis will, it is believed, be free from political influence. The Senators and Representatives who were instrumental in the passage of the bill would first of all resent any interference with the selection of these men upon any other basis than merit. To guard against any possible contingency of this kind, the law provides for a visiting committee composed of prominent specialists, each serving for a period of five years. The members of the present Visiting Committee were appointed to serve for periods of one, two, three, four and five years respectively; but their successors will be appointed for the full period. The committee consists of President Ira Remsen, of John Hopkins University; President H. S. Pritchett, of the Massachusetts Institute of Technology; Prof. Elihu Thomson, electrical engineer; Prof. Edward L. Nichols, Professor of Physics in Cornell University, and Mr. Albert Ladd Colby, metallurgical engineer. This committee, composed of men of world-wide reputation, will be consulted in all matters pertaining to the organization of the Bureau, and its existence ensures the establishment of an institution of the highest type.

The value of such an institution has been clearly demonstrated by nearly every government of Europe. England has established a standards department of the Board of Trade, also the electrical standardizing laboratory, and recently a physical laboratory; Kew observatory, an institution famous for its testing of meteorological instruments, has also become a Government institution. Germany has established the Normal Aichungs Commission, with its laboratories and facilities for handling commercial weights and measures, and in 1887, the Physikalisch-Technische Reichsantalt, an institution costing more than a million dollars, which makes provisions for scientific standards and investigations. It has been frequently said that this institution more than pays for itself annually in the assistance it gives to German manufacturers and scientists. The scientific work of our own Government has received great benefit from this institution during recent years. At the hearing before the House Committee on Coinage, Weights and

Measures in reference to the Bureau of Standards Bill, the Secretary of the Treasury presented letters from every scientific bureau in the Government, stating that they were compelled to go to this institution for certain standards. It was this fact, coupled with statements to the same effect from the prominent manufacturers, scientists and others present at the hearing, that led the committee to report unanimously in favor of the bill establishing the bureau. There is scarcely an interest in the country which will not be vitally benefitted by this new institution. Its relation to foreign commerce will be far-reaching, and its establishment means that the United States has taken a long step forward in the international race for commercial supremacy.

The Secretary of the Treasury, in his statement to Congress of the conditions necessitating the establishment of a National Bureau of Standards, made the following statement, which is worthy of our deepest consideration: "There is another side which occurs to me: it may appear to many to have a more sentimental than practical value, but it gives the proposition to my mind great force, and that is what might be called the moral aspect of the question; the recognition by the Government of an absolute standard, to which fidelity is required in all relations of life affected by that standard, is greatly to be desired. We are the victims of looseness in our methods, of much looseness in our ideas, too much of that spirit born out of rapid development and perhaps of a disregard or lax comprehension of the binding sanction of accuracy in every relation of life. Now, the establishment of a bureau like this, where the Government is the custodian and the originator of these standards of weight and measure as applied to all scientific aspects of life in which we are rapidly developing, has, to my mind, a value far above the mere physical considerations which affect it, although these physical considerations are fundamental and most important. Nothing can dignify this Government more than to be the patron and founder of correct scientific standards and such legislation as will hold the people to faithfully regard and obey the requirements of law in adhesion to those standards."

Whatever may be said of the advantages to be gained by the adoption of uniform and correct standards in any country applies still more in regard to uniformity of standards between nations. The introduction of modern methods of transportation and communication have brought the countries of the world into closer relation than were the counties of Great Britain a century ago, and while the question of a universal system of weights and measures has been discussed for many years, the adoption of such a system has become a necessity at the present time.

The choice of the fundamental standards for this system is a matter of convenience, but that it be a decimal system is of paramount importance. The meter may be no better than the yard, but the metric system is a decimal system, and has been adopted by all civilized countries except our own and Great Britain, and in the latter it has received a favorable report by the Parliamentary Committee having the subject under consideration, and it will undoubtedly be adopted by that country in the near future.

For several years the advisability of adopting the metric system of weights and measures as the standard of our country has been considered by Congress, and several bills have been favorably reported upon by the committee to which they have been referred. Some of these reports show a most thorough comprehension of the subject and set forth many urgent reasons why the metric system should be adopted.

The House Committee on Coinage, Weights and Measures, of which the Honorable James H. Southard, of Ohio, is chairman, has given a great deal of attention to this subject, and will undoubtedly present to the coming Congress a bill looking forward to the adoption of the metric system, and it will unquestionably be one of the leading measures discussed by that body. It should be stated that the passage of the Act establishing the National Bureau of Standards was largely due to the efforts of this committee, and especially of its chairman.

There is no denying the fact that a change in the weights and measures of the country will involve temporary confusion and expense, but can it even be adopted with any less expense or inconvenience? There is no doubt in the mind of any thinking person but that a universal system must soon be adopted, and no system offering advantages equal to that of the metric system has yet been devised.

AMERICAN ELECTRO-CHEMICAL SOCIETY.

From the *Electrical World* we learn that a movement was recently initiated, having for its object the founding of a national Electro-Chemical Society to be modelled on the same general plan as the American Institute of Electrical Engineers. The products of electro-chemical industries in this country at the present time amount to $100,000,000 per year. The growing importance of these industries and the fact that scientists and engineers interested in electro-chemistry are now distributed among at least half a dozen different societies, and heretofore have had no common medium of communication, are considerations which are believed to fully justify the movement. As noted at the time in our columns, a preliminary meeting in the interest of such an organization was held November 1st at the Engineers' Club of Philadelphia, Professor J. W. Richards, vice-president of the American Chemical Society, acting chairman and Mr. Carl Hering, past president of the American Institute of Electrical Engineers, as secretary. All those present were heartily in favor of forming such a society, and numerous encouraging letters were received from all parts of the country, those from representatives of the electro-chemical industries being especially favorable. It was unanimously decided that such a society should be founded as soon as a sufficient number of members could be secured to insure success. Committees were appointed to secure members and to arrange for a meeting in the near future, at which the society will be formally organized. It was the opinion of those present at the preliminary meeting that the annual dues for membership should not exceed $5, and that there should be only one class of members. Those who desire to become members of the society may send in their adhesion to Dr. Charles A. Doremus, 17 Lexington Avenue, New York City, chairman of the committee on membership.

ELECTRICITY ON SHIPBOARD.

It is intended to utilize electricity in lieu of steam for subsidiary purposes upon the vessels of the English navy more extensively than at present. A series of prolonged experiments are to be made to ascertain the range of the practicability of using this power for this purpose. At present the capstan, steering engines, ventilating fans and derrick hoists on the vessels are manipulated by steam, necessitating the construction of a bewildering network of pipes in the interior of the ship. The new armored cruiser "Hogue" is being fitted with electric wires, and the entire subsidiary gear will be controlled by electricity. Should the experiment prove successful, the system will be extended to all other vessels refitting, as well as those under construction. A modern English battleship now carries a small staff of electrical engineers, so that no alterations will have to be made respecting the crew. W.

Mining and Metallurgical Section.

Stated Meeting held April 10, 1901.

THE INSPECTION AND TESTING OF CEMENTS.

BY RICHARD L. HUMPHREY,
Member of the Institute.

(*Concluded from vol. cliii, p. 42.*)

RECORDS.

There should be some system under which the tests are made; that is, there should be a regular number of briquettes made from each sample, and they should be broken at regular intervals; whenever possible these tests should be extended beyond the regular twenty-eight day period, as it is very desirable to know what the strength is at the end of several years. In addition to the tensile tests, each sample should be submitted to all the tests usually employed. The data obtained from these tests should be carefully recorded in a book kept for the purpose. At regular intervals the data obtained should be compiled in tabular form showing the average tests.

INTERPRETATION OF TESTS.

Having made the above tests, the interpretations of the results obtained is the next and most serious difficulty which confronts the inspector. It is impossible always to insist on a rigid compliance with the requirements of the specifications, since the failure to meet these requirements may be due entirely to faults in the testing. For example; if a cement was finely ground and yielded high neat tests, but was deficient in the tests with standard sand, it might be reasonable to suppose the sand tests were not properly made, or that the sand used was of poor quality. Again, if the fineness and tensile strength with sand were above the requirements and the cement failed in the neat tests, it might be due to an excess or deficiency of water used in mixing, or it might be occasioned by the drying out of the briquettes, or they may have been affected by the conditions under which they were preserved.

It often happens that the person who makes the tests does not use the same amount of energy in each test; this is particularly the case where the number of tests made is large.

In cases where the cement fails to meet the requirements, it should be given a retest before condemning it.

CONCLUSION.

Our methods of testing still leave much to be desired.

The elimination of the personal equation and the adoption of methods by which uniform and comparable tests can be obtained, are matters of vital importance which are claiming the attention of all those identified with the production and use of cement.

There are a number of committees of the International Association for Testing Materials, engaged in investigating various phases of this question and it is expected that some valuable reports will be forthcoming at the congress to be held at Budapest in September of the present year.

Numerous laboratories, both municipal and private, in this country, and the laboratories of the Ponts et Chausses, in France, the Royal Testing Laboratory at Charlotten burg, Germany, the Testing Laboratory at Zurich, Switzerland, and others on the continent, are engaged in the investigation of this subject, and it is not improbable that some definite conclusions will be reached in the near future.

The researches of Vicat, Pasley, Grant, Le Chatelier, Candlot, Michaelis, Tetmajer, Newberry and others, have brought forth valuable tentative deductions; but all of these deductions are yet susceptible of proof.

The complexity of the compounds formed during the process of hardening render any synthetic work on this subject very difficult, particularly so since these compounds are radically effected by variations, in the composition of the raw materials, or in the burning. As has been already stated, it is impossible to tell just what changes take place during the process of hardening.

While the methods of manufacture now in vogue in the modern American Portland cement factory are unquestionably carried out with extreme care, and the greatest pains are taken to produce the best and most reliable cement possible, yet the value of these methods has yet to be demonstrated. Nor has the value of our methods of testing and the relative importance which we give to the various tests been established.

We should bear in mind that the present rotary kiln product is a comparatively new article, with whose properties we are only partially familiar. We do know that this cement hardens very rapidly and develops great strength in a very short time.

I have observed, and I have no doubt that other engineers will agree with me, that there is a period in the hardening of these cements when the tensile strength falls off very seriously. When this phenomenon came to my notice, it was confined entirely to neat cement; but latterly I have observed a decrease in strength of the sand mixture, although to a lesser degree.

Whether this action can be ascribed to fine grinding, overliming, hardburning or to the addition of sulphate of lime is very difficult to say. I have not observed the same falling off in compressive tests. It is noticeable in making long time tests of cement manufactured six or eight years ago, that the fracture of the briquette is very brittle, frequently shattering into numerous pieces.

Of the 60,000 and more briquettes which have been broken by myself or under my direction, the tensile strength of the neat cement did not exceed 1,100 pounds per square inch.

This strength is attained by Portland cements at the end of a long or short period of time, depending on the process of manufacture.

The great strength attained at a very early period by the cement manufactured in these later years, is due, no doubt, to the fine grinding of the raw material and finished cement and hard burning. This strength is further increased during the early stages by the use of sulphate of lime.

The period of apparent retrogression is claimed to occur when the effect of the sulphate is neutralized, and that the real strength due to the formation of the silicates still continues.

The briquettes which show a loss of strength are perfectly sound and hard, and present no evidences of unsoundness or disintegration.

The chemical composition and the physical condition of the product of the modern rotary kiln process is such that the crystallization or hardening can take place in a very short period of time.

This process consists, briefly, in grinding the ingredients composing the raw mixture, from which Portland cement is made, to an impalpable powder and passing this powder through a rotary kiln where the intense heat (which enables the requisite chemical reactions to take place) converts it into a clinker. This clinker is in turn reduced to a powder so impalpably fine that when water is added to it the process of crystallization or hardening is started, which eventually converts the mass into stone.

We are enabled, therefore, through the perfection of this process, to artificially produce, in a very short period of time, that which nature requires centuries to accomplish.

During the early stages of this phenomenon the mass is in a tough and more or less amorphous condition, and the tensile strength is high; but as the crystallization proceeds, the mass becomes more brittle, and increases in compressive strength. It seems to me that this loss of tensile strength can be attributed to crystallization; and I think that the reason the loss is not greater in the case of sand mixtures than neat cement is, that the voids in sand mixtures enable the expansion, produced by the process of crystallization, to take place with less injury to the adhesive qualities of the cement. It should be remembered that stone is tested in compression; we do not think of testing it in tension. It would seem logical, therefore, to test the hardened mass of cement mortar or concrete in compression. This, in my opinion, is the proper method for ascertaining the real strength of cement, especially at the end of a long period of time.

Tension tests should be used for the purpose of determining the relative value of shipments of cement, and should be confined to tests not extending over twenty-eight days.

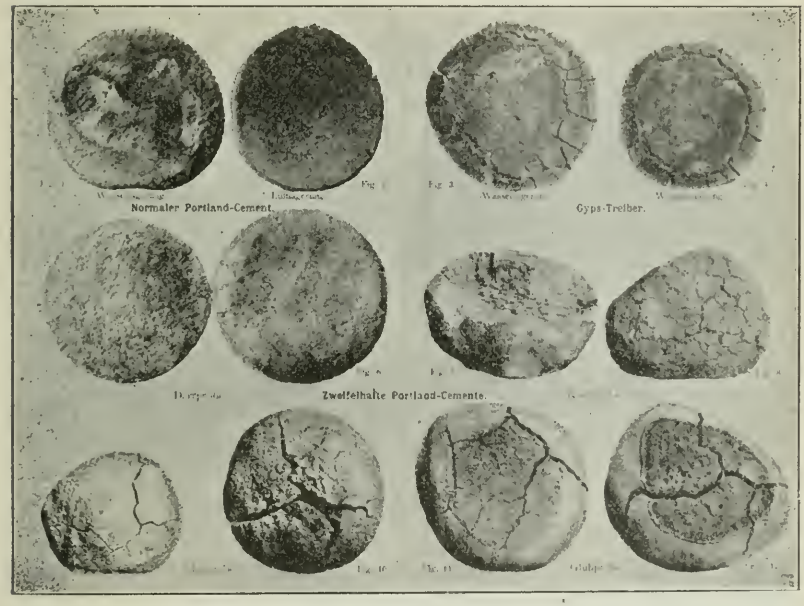

PLATE XXI—Result of tests for constancy of volume.

Whatever may be our doubts on the subject, and notwithstanding the obscure points yet to be explained, the fact remains that the masonry and the concrete in which the modern Portland cements have been used show no signs of deteriorating, but, on the contrary, show increasing hardness. And it is especially noticeable (when it is necessary to tear out portions of the structure) that the cement mortar is frequently stronger than the stone or other material which it binds together.

I do not know of an important piece of work (in which the cement was tested and accepted) where the failure of the Portland cement mortar or concrete used in its construction could be ascribed to the bad quality of the cement.

The late John McAlpin is credited with the statement that the only way to test cement is to wait fifty years and see whether the structure still stands.

We will unquestionably have to wait in order to prove the correctness of our present theories. In the meanwhile, in lieu of something better, we must accept the present

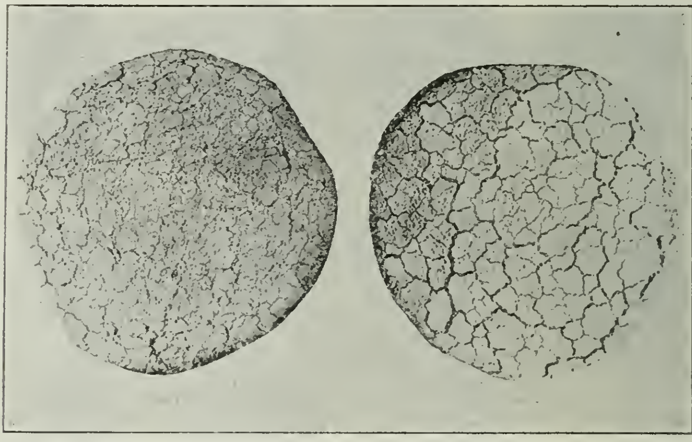

PLATE XXII—Result of test for constancy of volume.

high testing Portland cement with unlimited faith in its high excellence as a building material.

History teaches us that engineers prior to 1756 and subsequently, who used lime mortar, were compelled to frequently renew the mortar in the structures in which it had been used on account of its being washed out of the joints. While it is true there are structures built thousands of years ago in which the mortar is exceedingly hard, nevertheless we have reason to believe that it required centuries to acquire this hardness.

On the other hand, we do know that the modern Portland cement can be depended upon to develop great strength in a very short time. This is a feature of great importance in our present methods of rapid construction; and we further know that thus far the cement has withstood all normal forces tending to destroy it.

Having consented to accept the cement on faith, we should not be too rigid in testing it, especially in the absence of any infallible system by which to gauge its quality.

The system used in inspecting and testing cement should be a rational one, and the interpretation of it should be governed largely by the results of experience, rather than by theoretical requirements.

DISCUSSION.

MR. ROBT. W. LESLEY:—I have listened with great interest to Mr. Humphrey's remarks, and having, in 1898, prepared a paper which I read before the Engineers' Club, of Philadelphia, on the same subject, it is gratifying to know that the world "do move," as shown by the new facts and later experiences adduced by the writer of to-night's paper.

There is no question that "from the moment the clinker is reduced to impalpable powder its physical and chemical qualities are constantly undergoing changes which affect its quality as a building material;" "and even after it is made into mortar, these changes are constantly going on." In this country we are confronted, as Mr. Humphrey well states, with the manufacture of Portland cement by what is known as the rotary process, and we have enormous amounts of work done with material of this character, which, as yet, has to prove itself by the tests of time, the equal of cement made in the old-fashioned way with dome kilns. So far, experience has shown that this rotary cement is meeting all the requirements, though the results in long time tests seem to indicate that the maximum of strength is reached at an earlier period than in the dome kiln cements, and it remains for the tester and experimentor of the future to determine whether this lack of gain in strength at long periods is due to any deterioration in the material itself, or is due to a lack of elasticity caused by the material attaining its greatest strength and highest crystallization at the earlier periods.

After all, however, whether the cement be made by the rotary kiln process or be made by the dome kiln process, cement is always tested in a different condition from that in which it is manufactured. The cement briquette is a new material, and is not the material that the manufacturer ships to the work; and, therefore, the testing involves the greatest skill, the greatest fairness and the greatest judgment in coming to conclusions.

All other materials are tested as finished products, while cement is tested in the form made by the tester, and not by the manufacturer, and the test is the result of the cement, plus the experience of the tester, plus the variations of water, temperature and laboratory conditions. It is to obviate these that the tester must devote his highest talents. The nearest approach to fairness in this direction is for the testing to be done at the place of manufacture, wherever the contract is large enough to warrant such conditions.

The most recent illustration of this method is that practiced by the Rapid Transit Commission of New York, which has introduced this system to its fullest extent on their work in the city of New York.

By arrangement with the general contractor and the cement manufacturer, it is provided that all the cement for that work shall be tested at the place of manufacture; that it shall be run into bins subject to the inspection of the testing department of the Rapid Transit Commission; that there shall be on hand a sufficient quantity of cement to admit of twenty-eight days' test; and that the cement, after the bin has been accepted, shall be placed in bags and sealed with the seal of the Rapid Transit Commission, indicating on the seal the date of the shipment and the number of the bin. It is further provided that this cement shall be sent to the warehouse of the cement company in Jersey City, and at that point sufficient supply of this cement, sealed as above, for at least a week's operation, shall be held. From this warehouse deliveries are made on the work in the sealed packages.

Under this method the inspectors of the Rapid Transit Commission have their own building, provided by the manufacturing company, with their own chemical and physical laboratory provided for them for the purpose of making all their own tests; they have full access to the mills of the company; privilege in examining the raw materials; the mixtures made; the grinding, and sample the cement from day to day as it is ground, thus measurably controlling the product that is intended for their use.

After the cement has been thus tested, it is then ready for use on the work, and it can readily be seen that an immense saving is effected to the contractor by obviating all necessity of storing cement pending test in the crowded city of New York, and that further, the cumbering of the streets of New York with thousands and thousands of barrels of cement awaiting test is thus clearly obviated.

Under this method, the best results have been attained. The cement has shown itself equal to any of the requirements of the specification, and the actual physical results have been remarkable: so remarkable in fact, that in one case, two walls 175 feet long, 15 feet high at one end and sloping down to one foot at the other, forming two sides of a double-track railroad, were each moved without break a distance of four feet back from original foundation in order to widen the roadbed at that point to admit of a four track railway.

Such a method as this, where coöperation can be had between the inspector and manufacturer, and the consumer and the producer, eliminating the difficulties that have frequently occurred by the variation of the personal equation in the testing at a point distant from the work, of the new material known as the briquette, has obviated many of the difficulties that have hitherto arisen, and is to be highly recommended wherever the work is large enough to warrant the establishment of such a laboratory or the testing in such a way. It can only be done successfully in large contracts, 10,000 barrels and over, small amounts not warranting the outlay.

The testing of cement certainly should be done in the simplest way, as the purpose is not the establishment of standards, but the ascertainment that a brand of established reputation, which has been adopted for the execution of an important work, will come up to its own standard. It is not for the ascertainment of the varying qualities of various brands, but is for the ascertainment of the fact whether or not the manufacturer of the accepted brand has or has not "put all his big strawberries on the top of the basket" in sending the first sample that he puts into the work.

Assuming that a brand of well-established reputation (and there are many in the market) has been selected for the work, there is no question but that chemical analysis to determine that this brand as manufactured is of a suitable character has great value, and for purposes of verification such analyses should be made from time to time during the progress of the testing.

Fineness is an element that is within the control of the manufacturer, and is a question of machinery and care, and under the specifications can usually be obtained. In my own experience, the only question that has ever arisen as to fineness has come about through misunderstandings, not as to the fineness of the cement test but as to the character of the sieve through which the fineness is to be determined. It may be a strange statement to make, when I say that upon a recent occasion where a certain fineness through a 100-mesh sieve was required, that out of some eighteen 100-mesh sieves purchased in various parts of the country, or procured from various cement works and testing laboratories, no two were alike, and no two were actually 100 x 100-mesh sieves. Upon investigation it was ascertained that these sieves are woven by hand with great labor, and that in the single piece of wire-cloth out of which a number of sieves may be made, very material variations occur, owing to the drawing of the sieve on the loom, owing to the carelessness of the man at the loom, or owing to the pulling of the cloth at the time of putting it over the sieve itself. Generally speaking, a 100-mesh sieve will measure about 96 x 100, and this is what may be for all practical purposes considered to be a 100-mesh sieve. Other difficulties in fineness may occur by the sieve becoming choked with fine dust, thus not allowing the material to go through, and as stated by Mr. Humphrey, by slight tears or holes in the sieve which permit an excess of coarse material to pass through.

What has been said with reference to the 100-mesh sieve applies much more to the 200-mesh sieve, where all the difficulties are aggravated. Moisture is the cause of many failures to obtain the requisite fineness from the extremely fine sieves, but this can be obviated by drying as stated in the paper.

At Professor Tetmajer's laboratory in Zurich, probably the best mechanical sieves are to be seen, the operation there being from one sieve to another, and the whole being moved by steam power.

Without desiring to be radical or to institute new theories it has been my privilege to see—and I understand the figures will be shortly published—a series of tests of re-tempering mortars of natural and Portland cements. These tests, conducted by one of the most prominent engineers in the country, give results of mortar re-tempered at the end of an hour, at the end of two hours, and kept constantly in the condition of tempering for a period of two hours. From them, as shown by the result of the briquettes, it would seem that the whole question of re-mixing and re-tempering mortars has not been thoroughly understood, and further, that it might be concluded that the actual question of time of setting is not so important or vital a one as has hitherto been supposed. Of course, common experience itself shows that re-tempered mortar is not as good as the original mortar, but in the face of these later figures, which certainly will create considerable interest in the engineering profession, it would seem by scientific tests on this subject that there is considerable ground for doubt as to the whole present knowledge on the subject of re-tempering mortars and of the whole value of tests for time of setting, so far as practical experience is concerned. This time of setting, however, is always an important factor in the manufacture of briquettes and in the manufacture of sample pats because, without the ascertaining of this condition, many briquettes and many pats would be put into the water long before the period at which the actual setting had occurred, with the certain result of causing their disintegration.

Too much stress cannot be laid on the question of the percentage of water to be used in making the mortar. This will materially affect the percentage of gain that cements will show between the seven-day and twenty-eight-day tests; as small a difference as 1 per cent. in the water will cause variations of from 10 to 15 per cent. in the ratio of gain between the periods above stated; the wetter moistures show the greater percentage of gain, while the dry mixtures show little or no increase in strength. The percentage of water is a factor that cannot be determined once for all with every cement, but is one that must be felt for tentatively from time to time in the testing.

In a laboratory where a boiling apparatus or steaming apparatus is in operation, and samples are laid on shelves, it is a frequent case for the cement powder to absorb a considerable amount of moisture; and in such cases where the percentage of water to be used has been passed upon the dry sample, it can readily be seen that such a percentage would be in excess if the sample was subjected to moistures. On the other hand, where the water is very hot and the cement from the mill contains the latent mechanical heat of grinding, or where the testing-room is absolutely dry and no moisture is in the air, a cement to which a certain percentage of water has been allotted will, under these conditions, obtain too little water for its best development or manufacture into pats or briquettes. While, therefore, there is no question as to the advantage of having a formulæ such as the speaker gives for dealing with this subject, yet these formulæs would still be subject to the variations above mentioned. On the other hand, the practice described by the Testing Station at Charlottenburg of determining the percentage of water to be used by mixing the cement to a syrup paste so that it will run from the knife in long thin threads without forming into lumps, would seem to indicate, though in itself only a "rule-of-thumb" method, a means of everyday practice that might yield the better results.

In the paper which I read before the Engineers' Club, of Philadelphia, I gathered some 200 odd specifications of various engineers in charge of Army, Navy, public and private work, and showed the general trend of specifications, and what was required by these many men of many minds. It is a source of gratification to find that the present period shows that within the last three years there is a trend—first, toward the standardization of tests, and second, toward the discarding of many of the abnormal seven-day tensile-strain requirements that form parts of some of the older specifications. The recent report of the Board of Engineering Officers to the Chief Engineer of the United States Army shows a most important step by a most important branch of the Government toward this unification of standards; and the seven-day requirements there adopted are fair, and can be met by the average of American Portland cements without running the risk of over-plastering or over-liming. The Navy Department has under consideration a standardization of their various specifications, and it is to be hoped that these two important branches of the Government, doing millions of dollars' worth of work a year, will come to some uniform and general standard, both of methods and of requirements, that will take out of consideration both of engineers and manufacturers the hundreds of specifications of varying characters that have hitherto been before the public.

If it were possible for the Committee on Cement of the American Society of Civil Engineers to correlate in their work with these two branches of the Government service, cement manufacturers would be only too glad to lend a willing hand toward the production of a uniform specification, such as exists all over Germany, where the German Government and German engineers are actually represented and take part in the proceedings of the German Cement Makers' Association.

As a digression from so serious a topic, I would say that I agree entirely with Mr. Humphrey in the fact that hand mixing is the best and most satisfactory method of making briquettes; and while I note the various machines he describes for the purpose of mixing, he missed one which afforded me a great deal of amusement, viz., the milk-shake apparatus which was used on the Chicago drainage canal. In the laboratory there, four men were employed in the making of briquettes: one prepared the raw material; one added the water; another operated a milk-shake apparatus in which water and cement were rapidly whirled around; and a fourth man took the paste and put it in the briquette molds. This application of a very simple arrangement produced very good briquettes and fairly good results.

At the time of making the briquettes too much consideration cannot be given to the temperature of the room, to have it uniform and to have the air moderately moist. The variations in the manufacture of briquettes have been well ascertained in some experiments by Prof. J. Madison Porter, of Lafayette College, who collated them and gives his results in the *Engineering News*, March 5, 1896. They are given in the accompanying table, and indicate how important it is to have the briquettes trowelled on both sides, and to have uniformity in the mixing and moulding of briquettes.

Just as it is a pleasure to one who has struggled with cement and cement testing for twenty-seven years to see a swing of the pendulum in the matter of rational requirements in tensile strains at seven and twenty-eight days, as contradistinguished from the high specifications that were eliminated a short period ago, so it is equally gratifying to find men who have had the experience of Mr. Humphrey—Professor Dow, of the Washington Laboratory; Colonel Powell, of Pittsburg—abandon the radical boiling test for constancy of volume. The indiscriminate use of the boiling test is doing more harm than good to the cement consumer, for, while this test is the *bête noir* of the cement manufacturer, he is forced to use sulphate of lime to the possible detriment of the cement, and this is to overcome the *bête noir* that has been put in his path unnecessarily.

TABLE SHOWING RESULTS OF TENSILE TESTS OF MORTAR, ONE PART CEMENT, THREE PARTS SAND (SAME SAMPLE OF CEMENT) MADE BY DIFFERENT PERSONS IN ACCORDANCE WITH THEIR UNDERSTANDING OF THE METHOD PROPOSED BY THE COMMITTEE OF THE AMERICAN SOCIETY OF CIVIL ENGINEERS.

(*Extract from Article by J. Madison Porter, Engineering News, March 5, 1896.*)

Laboratory.	Tensile Strength in Pounds of Briquettes. No. 1.	No. 2.	No. 3.	No. 4.	No. 5.	No. 6.	Av.	Range in Pounds.	Ratio of Range to Maximum.	Ratio of Average to Maximum.	Water Per Cent.	Molds.	Struck Off.	How Placed in Water.	Water-bath.	Kind of Machine.
1. R. W. Hildreth & Co., New York	68	72	74	78	82	—	75	14	17·1	30·4	12	Single.	Both sides.	Flat.	Still.	Riehlé. Driven by hand.
2. Washington University, J. B. Johnson	77	94	108	110	122	—	102	45	36·9	41·8	Not known.	Gang.	One side.	"	"	Fairbanks. Johnson's clips.
3. City of Easton, H. R. Fehr, England . .	106	112	123	—	—	—	114	17	13·8	46·5	10·4	Single.	Both sides.	Edge.	"	Fairbanks.
4. Columbia College, W. H. Burr	125	126	130	137	140	144	133	19	13·2	54	10	"	One side.	Flat.	"	Riehlé. Driven by hand.
	126	132	138	144	150	153	140	27	17·7	56·8	8	"				
5. C. F. McKenna, New York	148	150	151	155	160	—	153	12	7·5	62	12	"	Both sides.	"	"	Riehlé-McKenna. Rubber clips. Power driven.
6. Cornell University, F. P. Spalding . . .	155	160	164	166	172	—	163	17	9·9	66	Not given.	"	One side.	Edge.	Running.	Lever with bucket.
7. Lafayette College, J. M. Porter	171	177	177	178	179	—	176	8	4·5	71·4	11	Gang.	Both sides.	"	"	Olsen-Porter. Porter's clips.
8. Clifford Richardson, Washington, D. C.,	220	224	226	228	228	—	225	8	3·5	91·2	10	Single.	Both sides	Flat.	Still	Riehlé. Driven by hand. Rubber clips.
9. Booth, Garrett & Blair, Philadelphia . .	240	246	249	250	252	—	247	12	4·8	100	12	"	Both sides.	Edge.	"	Olsen. Power driven Rubber clips.
Average	—	—	—	—	—	—	153	17·9	12·9	62	10·8	77·8% single; 22·2% gang.	66⅔% both; 33⅓% one.	55·5% flat; 44·6% edge.	22·2% running; 77·8% still.	66⅔% power; 33⅓% hand.

No. 2, water in bath changed once. No. 5, water not changed. No. 6, flow of water just sufficient to keep it fresh. No. 7, flow of water slight. No. 8, speed applied, 35 to 40 pounds per minute.

All molds and sand American Society of Civil Engineers' recommended sizes.

In a paper written by me, in discussion of Lewis & Whitfield's paper (*Am. Soc. C. E. Proceedings*) October, 1894, I scrutinized some seventy-four boiling tests that had been made under all the accelerated tests known in Germany, and taking those cements that had boiled and had stood the accelerated tests, I showed conclusively that in every case these cements contained an excess of lime or sulphate of lime, thus proving conclusively the point advanced by the speaker to-night.

Considering only what Mr. Humphrey himself states as to the manufacture of the pats to be subjected to his suggested test of 170°, and considering what he has to say on the things that are to be observed under this test, it would seem that possibly the simple normal test of the Germans would more fully comply with Mr. Humphrey's desired system of testing, viz.: "I am not in favor of any system which depends on cumbersome methods or expensive apparatus. The number of tests should be few, and simple in execution."

This whole question of hot tests, boiling tests, steaming tests, etc., has been gone over and over and over again by cement authorities all over the world. Faija, the pioneer, started it, advocated it and pushed it to the fullest extent, and yet, at the Engineering Congress at the Chicago World's Fair, publicly acknowledged that he was wrong. The Germans, with their careful and painstaking methods, have produced a half dozen various tests or methods of accelerated tests for determining the constancy of volume of Portland cement. For years, to my knowledge, they have been investigating this subject, and the Royal Testing Bureau at Charlottenburg, the various representatives of the various departments in Germany have, in connection with the German Cement Makers' Association, been laboring on this subject. Four years ago a committee was appointed to go over it thoroughly and conclusively. It was vested with full powers to probe the subject to the bottom, and after two years of experimenting they made their first report through Max Gary, Director of Division of Tests of Construction of the Technical Laboratory at

Berlin, and this report, just made in the contributions of the Royal Laboratory at Berlin, was presented at the German Cement Makers' Association meeting in 1899. On the strength of it, an *ad interim* report was made by the committee above stated, as follows:

"*Final Conclusions:* The result of the investigation may, therefore, be summed up as determining that none of the so-called accelerated tests for constancy of volume is adapted to furnish a reliable and quick judgment in all cases concerning the practical applicability of a cement. The investigation has further proved that all ten cements which withstood the pat test in the normal system of testing are practically constant in volume when used in test pieces and cement wares. The increase in strength of the test pieces with water- and air-hardening speaks well for the practical utility of the cements.

"The assertion quoted early in the report that the normal or standard tests were insufficient, particularly when the cement is to be hardened in the air in its practical application, has received no confirmation by the investigation of the commission. Nevertheless, the commission is ready to carry out still further investigations, and requests the sponsors for the accelerated tests for soundness to provide sufficient quantities of such cements as pass the normal tests, do not satisfy the accelerated tests, and prove unsound in practical service. The cements should be sent to the Royal Testing Laboratory until October 1, 1900. Until it has been possible to discover a test for soundness which is reliable and can be carried out in a shorter time than the standard test, the pat test of the normal specifications must be retained as decisive."

Two years have gone by since this report, and at a meeting of the German Cement Makers' Association, held in February of this year, the Committee on Accelerated Tests, made a further report, which says:

"After having made tests up to the length of two years, the commission for deciding as to the constancy of volume and adhesive power of Portland cement came to the conclusion that none of the so-called accelerated tests (boiling tests, etc.) were capable in all cases of affording a quick and reliable judgment in regard to the practical usefulness of a cement.

"Last year those tests ended, after four years, and Mr. Max Gary, of the Royal Testing Laboratory at Charlottenburg, has published the results of those tests as were made by his department, and also by the above-mentioned commission, in the 'Communications from the Royal Testing Station,' the main contents of which are as follows:

"Even the four-year tests have confirmed the results and conclusions of the two-year tests; neither the four years' observation, nor measuring the expansion, nor the firmness of the 10 cakes tested, and that were not considered up to the standard, justified us in declaring those cements useless for practical purposes.

"This is confirmed by pieces of cement-ware made of this same cement, and which have lain these four years in the open air. (They are on a frame 4 meters high and exposed to the influence of the weather. Rain and snow can collect in deep places, while protruding places are dry; the influence on the surface, therefore, is very unfavorable). Yet those objects did not show, during those four years, changes that amount to anything. Small flaws that were observed after the first four months are only on the surface, and appear with even the very best cement in the open air.

"Therefore, the commisson must refute as wrong, based on those tests, that the accelerated tests as to the constancy of volume allow a better judgment as to the usefulness of cement than the normal tests, and the normal tests must therefore be kept as the deciding tests for the constancy of volume of Portland cement.

"Another point has to be touched: In the report two years ago, the commission declared itself willing to make further tests, and publicly invited those persons recommending accelerated tests to produce in sufficient amount such cements as pass the normal test but do not pass the accelerated test, and which expand during practical use. Though the said parties had time until the first of October, 1900, to produce such cement, neither documents nor samples have arrived at the Royal Testing Station nor the commission concerning such cement, so the commission considered its work concerning the constancy of volume ended."

After reading the report and hearing some discussion on the same, the following resolution was presented, viz.:

"*The assertion that the so-called accelerated tests for constancy of volume generally admits of a better judgment about the usefulness of cement than the normal test, must be refuted as wrong.*"

which was finally adopted, with but a single negative vote.

This expression of the German Society of Cement Manufacturers, which numbers among its members and has present at its deliberations the heads of all the important departments of public works in Germany, and the head of the German Testing Laboratory, would seem to be conclusive on this subject of accelerated tests; and it leaves as a test for constancy of volume the ordinary German normal test, which is as follows:

"Small pats of the cement are made, which are allowed to stand twenty-four hours in damp air, and are then placed, some in water and some in the air, and observed until twenty-eight days old. If they do not become distorted or cracked during this time, the cement is considered sound."

To my mind, therefore, it would seem that as we have all recognized the Germans as being in the fore-front of Portland cement manufacture, and representing the highest degree of scientific and technical knowledge, that it would be wrong for us to disregard the conclusion of such a body and the result of work of so serious a character covering a period as long as four years; and therefore, while I may say, as a result of my own experience, that most of our Portland cements boil; yet I do feel that, as a practical fact, following out Mr. Humphrey, that the best test is a *bête noir* to manufacturers, in the sense that it tempts them to adulterate and over-plaster; for the reason, I believe, that it is a test which might be omitted as a definite, positive, conclusive test, but might be used by the manufacturer for his own information in the manufacture of cement, and by the engineer from time to time, as suggested in the Report of the U. S. Army Engineer Corps, for the purpose of informing himself as to the character of the material that is presented to him, but not as a conclusive test for the rejection of the material.

To the ordinary public it might seem that too much time and thought are wasted on this subject of cement testing, but it is one that is so serious in its results and so vital in its objects, in connection with the large buildings and works of all countries, that it cannot be disregarded; and it is gratifying to know that so eminent a society as the Franklin Institute should consider the subject as it has. As a former President of the American Society Civil Engineers, himself one of the most distinguished of bridge engineers in this country, said to me, "We are in the age of cement," and his conclusions are certainly right on this subject. The growth of the Portland Cement Industry in this country, from 1890 to the present year has been from 335,500 barrels to 8,482,020 barrels in 1900, or a growth of 8,146,520 barrels, and to all of us who are familiar with the subject, the growth of the literature has been even out of proportion to the growth of the product. There is no subject that is exciting among engineers, builders, and others so large an interest as concrete and cement in all its forms, and there is every indication that the coming century will see the flower of the cement age and the use of cement most general in all forms of construction.

A MEMBER:—Mr. Humphrey in your paper, among the tests for soundness, you did not mention the "Lamp-Chimney Test." I should like to know whether you regard the test as a valuable one?

MR. HUMPHREY:—The "Lamp Chimney Test" was in general use some years ago, and is still used in some laboratories. I do not regard the test as a rational one.

Practically speaking, all cements expand slightly upon hardening, especially the neat pastes. It is perfectly natural that they should. The formation of crystals in a supersaturated solution is characterized by a gradual increase in the volume of the crystalline mass. The hardening of cement is said to be the result of similar conditions. Ordidarily the common lamp chimney is readily cracked by internal pressure; it is not surprising, therefore, that good cements fail under this test. Besides the commercial chimney varies considerably in thickness from the light thin type to the double thick or extra heavy kind. It is evident, therefore, that the chimney test as usually specified, without any stipulations as to the character or thickness of the chimney is very indefinite and unsatisfactory. Again the swelling of the neat cement by itself, if not excessive and accompanied by checking, cracking or other indications of unsoundness, is not dangerous. Inasmuch as cement is not used pure in actual work, but is mixed with varying proportions of sand, gravel and broken stone, whatever expansion there may be is usually taken up by the voids in these aggregates without producing any noticeable injurious results.

The test is not therefore adapted for practical use first, because it cannot be defined with sufficient accuracy to secure reliable results, and secondly these results, when obtained, furnish no positive information as to the soundness of the cement.

MR. F. L. GARRISON:—I should like to know whether any experiments have been made with the microscope? A great deal has been done with the microscope in the study of iron and steel, and it seems to me that it could be used in a similar manner in the study of cement.

MR. HUMPHREY:—The only microscopical work of importance, of which I have any knowledge, is that by Professor Le Chatelier, and was published in the *Annales des Mines* in 1891. This is a very valuable contribution.

Thus far no data have been obtained upon which to base a test for determining the quality of cement by the aid of the microscope. The difficulties attending the microscopical study of cement, the lack of proper facilities and time, especially the latter, are some of the causes which have prevented research of this character in permanent laboratories, at least in those in this country. This is practically an unexplored field of research.

DR. WAHL:—Did I not understand you to say, Mr. Humphrey, that the carbonization in lime mortars was only superficial. I should think that this carbonization would extend through the mass.

MR. HUMPHREY:—Chemical analysis of the mortars from ancient structures made by various persons, show that the amount of carbonization in these mortars is slight. An examination of the mortar shows that only the outer surface has become hardened, and that in many cases the heart of the mortar is quite soft, indicating that even after thousands of years this carbonization has not extended through the mass. The outer surface of the mortar becomes carbonated very rapidly, but the carbonization of the inner mass proceeds much more slowly.

S. F. PECKHAM (Correspondence):—On reading this exceedingly elaborate and valuable paper I find very little to criticize. There are, however, three points to which I wish to call attention.

(1) The method of chemical analysis, recommended by Mr. Humphrey, requires the fusion of one-half grain of the finely pulverized sample, dried at 100° C., with four or five times its weight of sodium carbonate.

I have elsewhere shown (*Journal of the Society of Chemical Industry*, June 29, 1901, vol. xx, page 539) that this method of analysis by fusion of the whole cement, after pulverizing and drying, gives more or less erroneous results, for the reason that pulverizing and drying destroys the identity of the specimen. If a cement that has been injured by absorbing water is dried before it is analyzed, the sample analyzed is not the sample submitted. If a badly ground sample is finely pulverized before it is analyzed, the sample analzyed is not the sample submitted. In both instances certain differences that may exist in the specimens that are submitted are obliterated in the specimens that are analyzed by pulverizing and drying.

The process of analysis recommended by Mr. Humphrey will commend itself in most respects as very admirable; yet, I believe it is based on a fundamental error. I do not believe that in the analysis of cement an ultimate analysis is to be desired, and if had, is of much value as determining the comparative value of cements. Repeated analyses have shown that the more completely a cement approaches a theoretically perfect cement in its physical and chemical properties, the more completely it is soluble in dilute acids, including not only the mineral acids, but very dilute organic acids as well. A cement that contains grains of sand or overburned oxide of iron or underburned "mix," provided these constituents are found in the proper proportions, will give entirely satisfactory results upon ultimate analysis by fusion with sodium-carbonate. It is only when the cement is dissolved away from the inert impurities that the true character of the sample is revealed by chemical analysis.

(2) Mr. Humphrey recommends that for sieving, the sample should be heated to 130° F. Again, I claim that the sample should be tested as received. A sample of good cement is dry and fine, and sieves easily. If a sample is damp and lumpy, it should not be dried to make the labor and time of sifting less.

(3) Mr. Humphrey claims that if a finely ground cement makes high, neat tests and low sand tests the sand tests are not properly made. Such a conclusion is by no means legitimate. An unevenly burned sample may consist of good quality diluted with unburned "mix" or unburned cement-rock. Such a cement will test fairly high, neat and fail badly when mixed with sand, because, to start with, the cement is diluted with inert matter, and when it receives its proportion of sand it is heavily overloaded. Lime, as carbonate, is not cement, and when from 25 per cent. to 33 per cent. of a cement is calcium carbonate, the sample to begin with is practically from $\frac{1}{4}$ to $\frac{1}{3}$ sand.

This condition is not recorded by any of the tests recommended by Mr. Humphrey. Ultimate analysis by fusion does not touch it. It is found only by determining the loss at a red heat or by direct determination of carbonic acid. In my judgment, the determination of the loss at a red heat should now be omitted.

Mr. Humphrey (Closure): The practice of inspecting and testing cement at the place of manufacture has been in vogue for several years on important engineering works, among which may be mentioned, the Government work under Major F. V. Abbott at Lake Winnibigoshish, Minnesota; the dam and power-house of the Power Company at Sault Ste. Marie, and all constructions (requiring cement) along the lines of the Baltimore and Ohio and the Chesapeake and Ohio Railroads. As Mr. Lesley states, a recent illustration of this method is afforded by the practice of the Rapid Transit Commission of New York.

It is hardly necessary to state that as far as the quality of the cement is concerned, it is immaterial where the inspection and testing is done, provided the cement is not subsequently damaged.

The chief advantage of this method is that it avoids the extra handling of the cement delivered on the work, which would be the case where cement, submitted for inspection on the work, was rejected.

The successful moving of the two walls, as described by Mr. Lesley, should be attributed to efficient engineering rather than to the quality of the cement used, since any high-grade American Portland cement, properly used and allowed sufficient time to harden, would stand this test.

The importance of normal consistency cannot be too strongly emphasized. The percentage of water required to produce this consistency varies with each sample of cement. The percentage of water required to produce a plastic paste of normal consistency of neat cement with the various brands of Portland cement varies from 15 to 30 per cent. With natural cement the range is from 18 to 40 per cent.

The practice, therefore, of using the same percentage of water for all samples of cement of the same class is a vicious one, which cannot be too strongly condemned.

The "milk-shake apparatus" referred to by Mr. Lesley, was, I think, first proposed and used in the Testing Laboratory of the St. Louis Water Works Extension. This machine, shown on Plate VIII, *Fig. 1*, of this paper, was described in the article by Mr. S. Bent Russell, which appeared in the *Engineering News* of January 3, 1891. This machine has the same defect as the other forms of mechanical mixers, viz., the impossibility of producing a paste without the cement "balling up."

Concerning accelerated tests for constancy of volume, I desire to state that I have never been in favor of the "radical boiling test" nor do I believe in its infallibility.

The German cold-water test is reliable, but on account of the length of time required to develop evidences of unsoundness by this method, it is of little practical value as a test of reception where the acceptance of a cement is based on the results of the seven-day tests. Mr. Lesley errs, I think, when he states that Faija, in his paper before the Engineering Congress at Chicago in 1893, abandoned his steam and hot-water test; his position, as regards this test at that time was the same as when he first proposed it.

With all the defects and objections to the test, I think, after mature reflection, it will be admitted by all that the general use of the "boiling" or "hot-water" test in testing-laboratories, both on the work and at the place of manufacture, has done more to improve the quality of the American Portland cements than any other test.

While I am not willing to endorse this test for general use, yet I do think that in the hands of the expert it is of considerable assistance in judging the quality of a cement.

Of the present methods for making the accelerated tests, I believe that the immersion of the pat (after twenty-four hours in moist air) in steam coming from boiling water, contained in a loosely closed vessel, to be the simplest test, and immersion in water maintained constantly at 170° F. to be the most reliable test. The duration of these tests should not exceed twenty-four hours.

The failure of a cement to meet the requirements of this test should not necessarily be cause for its rejection; it should, however, lead to a further and more careful investigation of its quality.

I believe there is urgent need for some form of accelerated test for constancy of volume, and trust that some method will be proposed in the near future which can be used by novice and expert alike with some degree of accuracy.

Concerning Professor Peckham's remarks, I desire to state that the object of the fineness test is to ascertain the thoroughness of the final pulverization of the cement at the mill, by determining the percentages of the grains passing certain standard sieves.

It would be impossible to make this properly with a cement which contained enough moisture to cause it to be "lumpy," unless these lumps were first reduced to a powder by drying.

Besides, since a thoroughly dry cement sieves much more readily, the preliminary drying renders the work of sieving far less tedious.

If a cement has absorbed sufficient moisture to cause it to become "caked," the fact is at once visible and is taken into consideration in the preliminary inspection of the cement. If the cement meets the other requirements this condition should not be sufficient cause for its rejection.

The adulteration of cement with "unburned mix" or "unburned rock," can be detected by determining the carbon dioxide; when the adulteration is to the extent stated by Professor Peckham, it can be detected by means of the specific-gravity test.

The practice of effecting a solution of the sample of cement for analysis by fusion with sodium carbonate of the thoroughly dried and finely pulverized sample is quite common, and is a most reliable way of decomposing the silicates.

When the quantity of clay or insoluble silica is desired it is determined separately.

At the present time it is an ultimate analysis that is required.

While Professor Peckham's theories as to the object of chemical analysis and his proposed methods for the examination of cements may be perfectly correct, their value is yet to be demonstrated.

Mining and Metallurgical Section.

Stated Meeting, held Wednesday, November 13, 1901.

UPON THE CONSTITUTION OF BINARY ALLOYS.

BY JOHN ALEXANDER MATHEWS, PH.D.

Continued from vol. cliii, p. 21.

MOLECULAR DEPRESSION OF THE FREEZING-POINT OF METALS.

In 1889, Prof. Ramsey determined the molecular weight of many metals by the method of measuring the change of vapor-pressure of the solvent, mercury, at various temperatures below and at its boiling point. From these experiments he states, "that it would appear legitimate to infer that in solution, as a rule, the atom of a metal is identical with its molecule, as the physical properties of those metals which have been vaporized would lead us to suppose."

Messrs. Heycock and Neville found that when two metals were melted together, considering M as a solvent and N as the metal dissolved, then,

(1) The freezing-point of M is lowered—the most usual result.

(2) The freezing-point of M is raised, *e.g.*, silver in cadmium and antimony in tin.

(3) The freezing-point is unchanged, *e.g.*, thallium in lead, and I believe, within narrow limits, silver in gold.

These investigators, to whom we are so much indebted for their careful researches upon alloys, went further and showed that none of these cases was at variance with Van't Hoff's theory of solution. They did this by filtering off in each case the part first solidifying. In the first case the more fusible filtrate was found to be richer in dissolved metal than were the crystals. In the second instance the opposite was true, for the crystals were richer in dissolved substance, N; *i.e.*, the solvent was not first to crystallize, but what was probably a definite compound having a melting-point higher than its constituents. The pure metal *M*, and the compound M_xN_y, then, form isomorphous mixtures, whose melting-points are all above that of *M*.

In the third case there was absolutely no separation of the two metals during cooling; *i.e.*, the first crystals and the fluid part were identical. The two metals, in such a case, form isomorphous mixtures.

In these experiments of Messrs. Heycock and Neville, only very dilute solutions were studied, in which, as in the case of dilute salt solutions, it might be expected that the molecules of the dissolved substance would obey the laws of gases. From these experiments it would seem likely that solid alloys exhibit osmotic pressure, and that there is an expansive tendency upon the part of the dissolved substance to escape from the solution.

When these experiments were performed quantitatively, Heycock and Neville found that two of the empirical laws of Coppet and Raoult hold good for alloys, viz.:

(1) For moderate concentration the fall of the freezing-point is proportional to the weight of dissolved substance present in a constant weight of solvent, and,

(2) When the falls produced in the same solvent by different metals are compared, it is found that a molecular weight of a dissolved metal produces the same fall whatever the metal is.

In these experiments tin was the solvent, and it was assumed that the metals are monatomic, or that their molecules are all of one type, R*n*, when *n* is constant and probably = 1.

The third law is probably incorrect, for it assumes that if a constant number of molecules of solvent be employed, the fall is independent of the nature of the solvent. The solvent in the case of metals often tends to chemical combination with the other metal. Thus gold in 100 atoms of sodium gave 4° fall; in tin, 3°; and in potassium, only 1·8°.

FREEZING-POINT CURVES.

Having now made plain, I hope, the nature and meaning of the cooling curves of a metal or alloy, we next direct attention to freezing-point curves. These are constructed by plotting the critical points of a large number of cooling curves, using as before temperature for the vertical axis, but percentage composition, and not time, for the horizontal co-ordinate; that is, in studying such a series of alloys as the copper-aluminum series, we take the cooling curves of, say, twenty-five alloys varying in composition between Al = 0 per cent. and Cu = 0 per cent. *Fig. 13.* In preparing

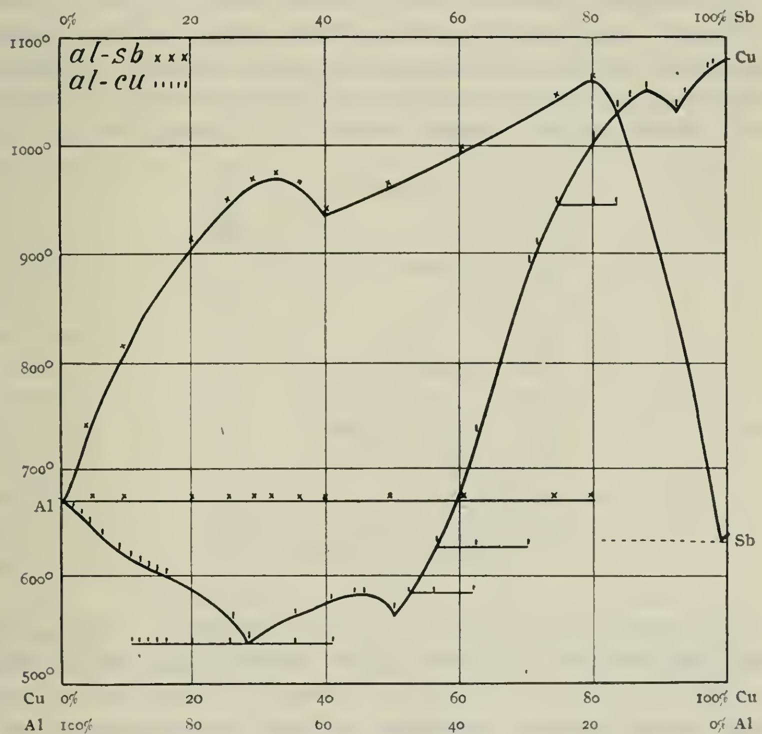

FIG. 13—Cooling curves.

this curve, the cooling curves of alloys corresponding to all the dots along the upper line were obtained. When two or more dots are seen in the same vertical line it indicates that in the cooling curve of the alloy of corresponding composition two or more halts occurred in the movements of the galvanometer. In either end of the curve only one break occurs; the alloys at either end of the series are solid solutions. Extending from Al, 84 per cent., to Al, 55 per cent., the crystallization of a eutectic is indicated by the line of horizontal breaks. The composition of the eutectic is Al, 67; Cu, 33 per cent. The other horizontal lines are not eutectics; their meaning has not yet been discovered. Before discussing such complicated curves as these, let us consider what are the general types of curves which are obtained with different alloys. In *Fig. 14* the curves are not drawn to scale, but are approximately correct in form. Gold and silver alloys form perfectly isomorphous mixtures, and their freezing-points give almost a straight line joining the freezing-points of the pure metals. Copper and silver give the typical curve of two metals which mix

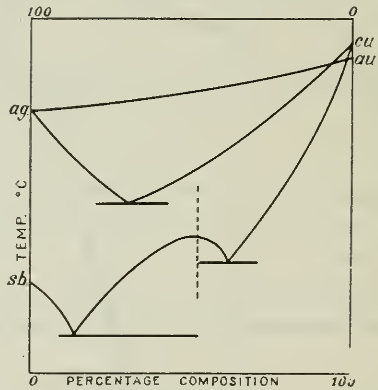

FIG. 14.

in all proportions but do not unite chemically. Antimony and copper unite to give the highly colored compound known as regulus of Venus, Sb_2Cu_5. This is indicated in the cooling curve by the intermediate summit occurring at a percentage composition equivalent to Sb_2Cu_5. In general such summits will be found to occur at points corresponding to simple atomic proportions, and indicate an intermetallic compound. To explain such a curve we have only to consider it as made up of separate sections as indicated by the dotted line in our figure. The compound has a melting-point of its own, quite independent of that of the constituent metals. It may be higher, lower or intermediate as compared with the individual metals which enter into its composition. In the Sb-Cu curve we must consider that one series of alloys is composed of Sb and Sb_2Cu_5, and the other of Sb_2Cu_5 and Cu. We are virtually dealing with two series of alloys, each of which taken separately is of the simple type illustrated by the Ag-Cu curve. In the Sb-Sb_2Cu_5 portion of the curve there can be no free copper, and in the Cu-Sb_2Cu_5 portion there can be no free antimony. This is not strictly true, because intermetallic compounds, as a rule, undergo some dissociation. In each portion of the curve, then, we are dealing with a pure metal and an intermetallic compound. Each component of these pairs has its respective freezing-point depressed by the presence of the other, as shown by the work of Heycock and Neville. The author's Al-Cu curve may be similarly explained; the two intermediate summits correspond to the compounds Cu_2Al_5 and $AlCu_3$, occurring at 48·4 and 87·6 per cent. copper respectively. Le Chatelier thinks he has microscopic evidence of at least four compounds; our curve does not show them, unless, in some way, they are connected with the horizontal lines, whose explanation we have not fathomed. Our own microscopic study of these alloys is not completed. The curve as here presented is simpler than we actually found it to be; we present its main features simply as a type; its complete explanation will be made the subject of another paper.

The Al-Sb curve, also shown in *Fig. 12*, indicates the presence of a compound whose melting-point is more than 400° above that of either constituent. Its formula is SbAl (81.6 per cent. Sb). The components into which this curve may be resolved, then, are Sb-SbAl and SbAl-Al. The compound seems to be almost insoluble in either metal, but on theoretical grounds we should expect to find a slight depression in the freezing-point of each metal by the addition of a small amount of the other to it, but this has not been detected experimentally. In the case of tin-aluminum alloys, however, I detected a fall of 3° in the freeezing-point of tin by the addition of 0.5 per cent. Al, while with increased additions of Al, the freezing-point is raised over 300° at a concentration of 10 per cent. Al, 90 per cent. Sn. The freezing-point curve of the Au-Al alloys is very complicated, but it presents some original features, and we present it with some words of explanation quoted from Mr. Neville, *Fig. 15.* His remarks bear upon the relation of metallography to freezing-point curves, and also to the possibility of the existence of compounds not indicated by a summit. Excluding the branches *AB* and *IJ*, each of the others *may* represent a compound. The point *E* corresponds to the composition Au_2Al. "The microstructure shows that the summit *E* is an almost homogeneous body, and that all solid alloys whose composition lies between that of *D* and

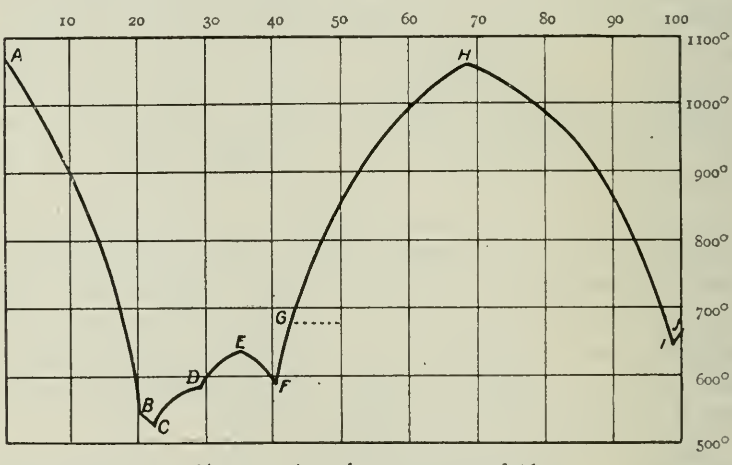

FIG. 15—Atomic percentage of Al.

F contain large crystals of the body *E* immersed in a mother-substance. As we descend the curve from the summit, the large crystals of *E* are found to occupy less and less of the whole alloy until at *D* and *F* they cease to exist. Exactly similar phenomena show themselves on the branch *GHI*. The summit *H* occurs at the formula $AuAl_2$. [The beautiful purple alloy of Roberts-Austen.] . . . These criteria taken together—(1) the occurrence of a summit at a formula percentage; (2) the presence of large crystals of the same kind, decreasing in amount as we decrease the branch on either side are, considered by Mr.

Neville, "an absolute proof of the reality of a compound." In the British Association report (1900) from which I quote, Mr. Neville gives arguments to show that such a branch as *FG* may indicate a compound and concludes: "Thus an alloy may contain a compound, although that compound does not occur as the sole constituent of any particular alloy." This point requires further study, it seems to me, and judgment should be withheld until further evidence is produced.

UPON THE NATURE OF EUTECTICS.

We are indebted to Dr. Guthrie for the name "eutectic," and we shall do well to confine the use of the term to indicate the most fusible alloy of a series—the one which freezes last. Eutectics may differ in certain properties and upon these differences they may admit of classification, but all eutectic alloys possess these common properties:

(1) They are, in any one series of alloys, of uniform composition.

(2) Their freezing-point is constant throughout any one series.

(3) Their freezing-point is the lowest in the series.

(4) They are not chemical compounds.

Their composition may, and occasionally does, correspond to some simple atomic ratio. This coincidence, though striking, does not prove the presence of a compound as the sole constituent of a eutectic. Usually they possess a more or less laminated microstructure. Often it requires the very highest magnification to detect the two constituents. Mr. Stead, in his splendid paper upon "Iron and Phosphorous" (*Jour. Iron and Steel Institute*, 1900) gives some ideas upon the subject of eutectics, and besides recognizing the essential features which we have already mentioned, adds that a eutectic may consist: (1) "Of two or more metals which do not unite chemically; (2) of a metal and a definite compound containing that metal; (3) or possibly of two or more definite compounds. (4) It may consist of a mixture of a solid solution of one metal in another and a free metal. (5) It may contain a solid solution of a definite metallo-metallic salt and that same metallo-metallic salt in the free state. (6) It may possibly consist of two solid solutions." To my mind the last of these statements is the most general of all, even though Mr. Stead qualifies it with the word "possibly." It embraces several of the preceding conditions, and since I am unwilling to admit that any metal or intermetallic compound ever separates *absolutely* pure, those conditions in which Mr. Stead speaks

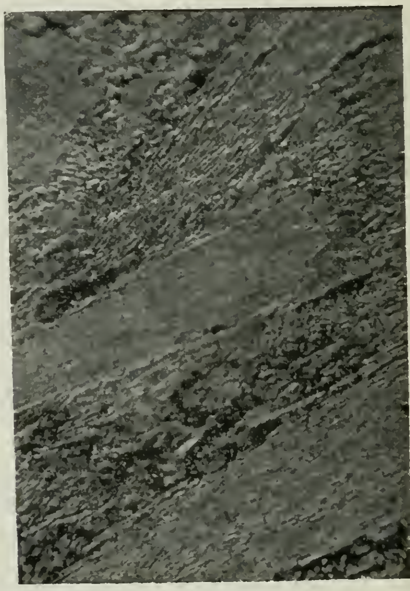

FIG. 16.

of the separation of pure substances seem to me slightly inaccurate. The most that we can say of them is that they are solid solutions of very small concentration. The form of freezing-point curves showing eutectics, together with Professor Roozeboom's explanation of them, renders it improbable that any strictly pure material separates at the point at which the eutectic solidifies. This does not in any way concern the phenomenon of a eutectic, or one of its constituents, splitting up at still lower temperatures into its elemental constituents, *e. g.*, iron carbide, *may* split up into *pure* iron and *pure* carbon.

In some alloys whose freezing-curve lies almost wholly above the melting-point of either or both constituents, but in which there is a slight depression of the freezing-point at either end for small additions of one metal to the other, as in the Sb-Al or Sn-Al series, and where this depression is further indicated in a well-marked eutectic break in the cooling-curves of the various alloys of the series examined,

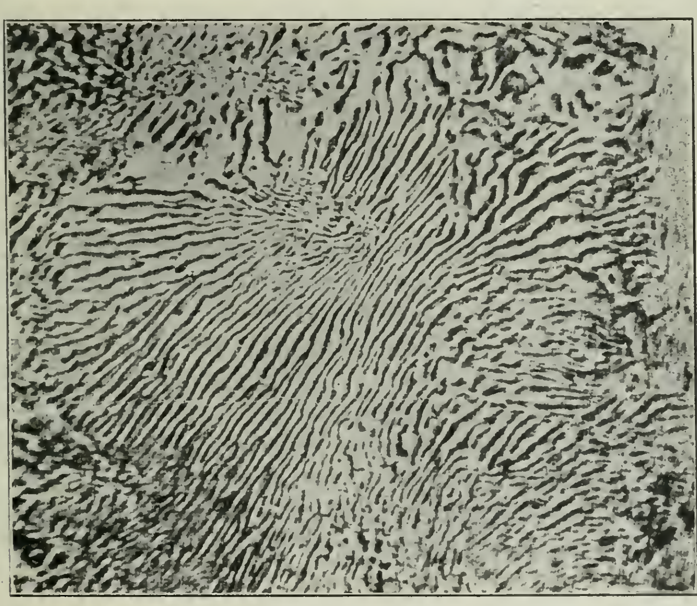

FIG. 17.

it may be possible that the eutectic is a single solid solution. The quantity of dissolved substances in such cases might not be sufficient to seriously change the type of cooling-curve, and probably the highest magnification would not resolve such a eutectic into two components. The fact that it crystallizes isomorphously with the pure metal argues that no two juxtaposited constituents are there. While our own work is not conclusive on this point, we may add that this conception of an eutectic is in no way opposed to the four essentials stated above, or with Dr. Guthrie's definition. The fact that in certain cases, *e. g.*, the 1 per cent. Cu, 99 per cent. Sn alloy, we may have a well-marked characteristically banded structure (*Fig. 16*) argues for rather than against the idea that a single solid solution may be a eutectic; for the absence of this structure in some dilute metallic solutions, and the isomorphism of the ground mass in such cases with pure metal seems to strongly confirm this idea. The typical metallographic structure of eutectics is illustrated by *Figs. 16* and *17*. The latter is pearlite, whose components are ferrite and cementite. This banded structure, however, need not be considered as essential. *Fig. 17* is by Osmond and is taken from Roberts-Austen's "Introduction to Metallurgy."

SOLID SOLUTIONS.

In all that has preceded alloys have been considered as solutions of metals; many points of similarity between alloys and ordinary solutions, (such as gases in liquids, liquids in liquides and solids in liquids) have been mentioned. The latter class of solutions are fully discussed in text-books on physical chemistry, but the extension of our knowledge upon the subject of solutions of solids in solids is of very recent date and, so far as we have advanced in this field of investigation, the results are mostly confined to technical periodical literature of recent date. Professsor Roozeboom and his pupils have conducted very careful researches upon the nature of fused mixtures of metallic salts and their behavior upon cooling (see Roozeboom, Stoffe and Van Eyk; and Reinders, and Hissink—*Zeits. Phys. Chemie.* Vols. xxx and xxxiii, respectively). This work throws considerable light upon the problems of chemical equilibrium in alloys.

Prof. Sir William Roberts-Austen defines a solid solution as "a homogeneous mixture of two or more substances in a solid state. In metals, no one has worked as yet with noncrystalline mixtures and solid solutions of metals when crystalline are solid 'isomorphous mixture' or 'mixed crystals.'" Mr. Stead, from whose paper we quote this definition, classifies solid solutions as follows:

(1) "Those in which one constituent of an alloy in crystallizing retains a portion of the other homogeneously diffused through its whole crystalline mass."

(2) "Those in which during crystallization the central portion of the crystals contains less of the dissolved metal than their external boundaries." (The condition described here corresponds to that of an unstable system. Complete homogeneity could probably be obtained by long annealing.)

(3) "Those in which the metals form a definite compound which is retained in solid solution in the excess of metals." (I should like to add that the "compound" may be the "solvent" in the sense in which that term is here used, and retain one or other of its constituent metals in solid solution. This accounts for the fact that when crystals of an intermetallic compound are isolated and analyzed, their compositions vary when the crystals have been isolated from alloys of different compositions.)

(4) Mr. Stead adds, "those in which the nonmetallic elements form definite compounds with a proportion of the dissolving metal that remain in solid solution." While probably referring to the phosphide and carbide of iron primarily, Mr. Stead's fourth class would include the property of such metals as copper and iron to dissolve a quantity of their respective oxides. Such solutions have not been studied in the light of modern theories.

ALLOYS AND THE PHASE RULE.

The phase rule of Gibbs, which has been found to be of inestimable value in explaining chemical equilibrium in liquid solutions, is of equal applicability to such complex solids as alloys. There may be, however, some difficulty in applying the sound theoretical principles upon which this doctrine is based, because of the fact that, owing to too rapid cooling, equilibrium may not have been attained in the particular alloy which we are studying. This seems only natural when we reflect that perfect equilibrium in a liquid solution is only attained slowly, and we cannot expect in a solidifying mass to find complete equilibrium when the molecular freedom is so much less than in ordinary solutions. The rate of cooling in some cases would have to be extremely slow to insure complete equilibrium and therefore one need not be surprised to find that his microscopic and pyrometric results do not agree with the theoretical constitution demanded by the provisions of the law of phases. If, however, equilibrium has been established then the number of distinct substances in the mass will depend upon the number of constituents which enter into the composition of the mass. Let us recall Professor Trevor's classification of systems as provided for by the phase rule, and later we shall point out how this must be modified when applied to alloys.

(1) When in any system the number of phases exceeds the number of components by two, the system is nonvariant.

(2) When the number of phases exceeds by one the number of components the system is monovariant.

(3) When the number of phases is equal to the number of components the system is divariant.

To understand these statements we must first know what is the strict meaning of the terms, "phase" and "component" as here used. As defined by Professor Bancroft, "A phase is a mass chemically or physically homogeneous, or a mass of uniform concentration; the number of phases in a system is therefore the number of different homogeneous masses, or the number of masses of different concentration." According to the same authority, "The components of a phase or system may be defined as the substances of independently variable concentration in the phase or system under consideration."

If we are going to consider alloys, both liquid and solid, as solutions—and the tendency of present methods of research seems to favor this conception of them—then it will obviously facilitate matters if we adhere as closely as possible to the phraseology, definitions and general usage of terms as employed by the best authorities upon this subject. This fact has been lost sight of by some writers and considerable confusion has arisen. For example, it does not seem to me that the adaptation of the phase rule which Le Chatelier gives (*Comptes Rendus*, January, 1900, and the *Metallographist*, October, 1900) is at first sight at all comparable with Trevor's classification of systems, which is the one with which all English speaking chemists become familiar in their studies of chemical equilibrium. Professor Le Chatelier's formula is correct, but how or why does not appear, nor does his explanation accompanying the formula make clear that any relationship exists between Trevor's generally accepted classification as applied to solutions and his own formula as applied to rocks, alloys, etc. That is, one misses the point entirely that not only do alloys resemble solutions, but *are* solutions; the very fact of the applicability of the phase law to them is strong evidence to that effect.

Let us see what Trevor's classification of systems means. Suppose n equals the number of components, and r the number of phases. Then these three generalizations become

(1) $n + 2 = r$, a nonvariant system.
(2) $n + 1 = r$, a monovariant system.
(3) $n = r$, a divariant system.

Then, in the case of single substances, such as water, $n = 1$, whence in equation (1), $r = 3$,—the maximum number of phases of a single substance which can exist in equilibrium, but only at one temperature and pressure, for the system is nonvariant. Concentration is obviously not a variable in the case of a single substance. If, in such a system as the above, either temperature or pressure varies, one of the phases will disappear. If a system of one component exists in but two phases, it has one degree of freedom, *i. e.*, it is monovariant. In other words, for each pressure there is one temperature, and for each temperature there is one pressure at which the two phases are in equilibrium. A single substance in a single phase constitutes a divariant system, for within certain limits temperature and pressure can vary independently.

When two substances (components) are present, as salt and water, then n being 2, r is 4. That is, a system of two components may exist in four phases in equilibrium, but only at one temperature, one pressure and one concentration. But in a system of two components and three phases (monovariant) when one condition is fixed, either temperature, pressure or concentration, the other two adapt themselves to suitable values and equilibrium is maintained and no phase disappears. The fixing of one of the physical conditions thus defines the system. When the number of phases and components are equal, then fixing one condition no longer defines the system, for the other two conditions are capable of variation; *i.e.*, if temperature is fixed, changes of pressure will give rise to a series of concentrations, or, by changing the concentrations we get a series of pressures; similarly, if the concentration is fixed, then equilibrium is maintained if all changes in pressure are attended by concomitantly opposite changes in temperature.

These are the general principles regarding the application of the law of phases to systems in equilibrium containing either one or two components, when pressure, temperature, and, in the latter case, concentration are the variable conditions. When we apply this law to the explanation of problems of equilibrium in alloys some modification of the numerical expressions defining the systems is necessary. This modification is in the nature of simplification, for with metals and alloys under usual working conditions the pressure is approximately one atmosphere and the vapor pressure is so small that its variations with slight changes in barometric readings from time to time are absolutely inappreciable. Therefore we may properly disregard pressure as a variable and vapor as a phase affecting equilibrium. Consequently, with pure metals we have but one variable physical action, temperature, while in alloys the variables are temperature and concentration. Since the number of possible phases is diminished by one (vapor being neglected) our equations become,

(1) $n + 1 = r$, a nonvariant system.

(2) $n = r$, a monovariant system.

(3) $n - 1 = r$, a divariant system.

In a system of one component, *e. g.*, a pure metal, $n = 1$ and the first equation gives $r = 2$, *i. e.*, metals may exist in two phases in equilibrium, but only at a single temperature, for the system is nonvariant. If heat is added, solid metal becomes liquid; if heat is abstracted liquid becomes solid; or the change may be from one allotropic form to another at a fixed temperature; the equation does not give the number of possible phases, but simply how many of them may be simultaneously existing in equilibrium.

In the second equation, $r = 1$, *i. e.*, within certain limits in a system of one component and one phase the temperature is independently variable. Equation three gives $r = 0$, which is impossible, *i. e.*, one component cannot constitute a divariant system when there is but one variable physical action under consideration.

Passing now to binary alloys, we have two physical variables: temperature and concentration; in our equations, since $n = 2$,

(1) gives $r = 3$, a nonvariant system.

(2) gives $r = 2$, a divariant system.

(3) gives $r = 1$, a divariant system.

In other words, when there are two variables:

two components and three phases = nonvariant system,

two components and two phases = monovariant system and

two components and one phase = divariant system.

In regard to determining the number of components in a system, Professor Bancroft says that the main point to be observed is, that each compound is not necessarily a component, and the fact that two components unite to form a phase in definite proportions does not entitle us to call this resulting compound or combination a component if we are dealing with the equilibrium of this resulting compound with the constituents which go to compose it. Thus, according to Professor Bancroft, in dealing with the equilibrium of $CaCo_3$, with CO_2 and CaO, we have but two components, and the $CaCO_3$ is simply a solid phase containing them, and, he continues: "On the other hand, it is not permissible to take calcium and oxygen as two of the actual components of this system, because they are neither independently variable, nor are they in equilibrium with the system." The accompanying table shows the relationship between the expressions applicable to liquids and solutions on the one hand and metals and alloys on the other, in a form which admits of easy comparison. From this it will be noticed that the expressions indicating the different systems are the same, whether n equals 1 or 2, but differ when the number of physical actions vary. In this table n = components; p = physical actions or variables, and r = phases.

TABLE OF COMPARISON.

	Single liquid, *e. g.*, H_2O.	Single metal, *e. g.*, Au.	
n	= 1.	= 1.	n
p	Temperature, pressure.	Temperature.	p
r	Vapor, liquid, solid (ice).	Liquid, solid.	r
Then (1)	$n + 2 = r_1$ = nonvariant system; *i. e.*, $r = 3$, gives " "	$n + 1 = r_1$ = nonvariant system; *i. e.*, $r = 2$, gives " "	(1)
If r then (2)	= vapor and liquid, $n - 1 = r_1$ = monovariant system; *i. e.*, $r = 2$, gives " "	Liquid *or* solid, $n = r_1$ = monovariant system; *i. e.*, $r = 1$, gives " "	r (2)
If r then (3)	= vapor, $n = r_1$ = divariant system; *i. e.*, $r = 1$, gives " "	There can be no divariant system, for, $n - 1 = r$ in this case gives $r = 0$.	r
	Liquid solution, *e. g.*, H_2O and NaCl.	Binary alloy, *e. g.*, cast-iron.	
n	= 2 — water and salt.	= 2 iron and carbon.	n
p	Temp'ature, concentrat'n, pressure.	Temperature, concentration.	p
r	Salt, ice, vapor, saturated solution.	Liquid cast-iron, solid solution of carbon in iron, graphite.	r
Then (1)	$n + 2 = r_1$ = nonvariant system; *i. e.*, $r = 4$, gives " "	$n + 1 = r_1$ = nonvariant system; *i. e.*, $r = 3$, gives " "	(1)
If r then (2)	= salt, solution, vapor, $n + 1 = r_1$ = monovariant system; *i. e.*, $r = 3$, gives " "	Liquid cast-iron, graphite. $n = r$, monovariant system; *i. e.*, $r = 2$, gives " "	(2)
If r then (3)	Salt and solution, $n = r_1$ = divariant system; *i. e.*, $r = 2$, gives " "	Liquid cast-iron (*i. e.*, carbon solution in iron). $n - 1 = r_1$ = divariant system; *i. e.*, $r = 1$, gives " "	(3)

There remain two points in regard to the application of the phase rule to alloys that may be touched upon briefly. First, the equations given do not indicate the number of phases in which one or two components may exist, but the number of phases that can exist simultaneously in equilibrium. Pure iron may probably exist in three solid and one liquid phase, but never in all at a single temperature. According to Roozeboom the iron-carbon alloys, while consisting of but two components, may exhibit at least seven phases, viz.: carbon, gamma-, beta- and alpha-iron, liquid solution, solid solution of carbon in gamma-iron or martensite, and cementite or iron carbide, FeC_3. Second, in applying the phase rule to pure intermetallic compounds we must consider that we are dealing with a system of one component. The composition of its phases, whether liquid or solid, is fixed by the law of definite combining proportions. When existing alone, therefore, it obviously can exist but in a liquid and solid phase giving a nonvariant system, or in the liquid *or* solid phase as a monvariant system—the temperature within limits being independently variable; concentration is not a factor any more than with a pure metal.

Let us now explain the freezing-point curve of a typical series of alloys considered as an equilibrium curve and in the light of the phase rule. *Fig. 18* shows the freezing-curve of the Cu-Ag series. The curve *a*, *e*, *c*, represents the temperatures at which solidification begins to take place for any given concentration. The curve *a*, *b*, *d*, *c* shows the temperature at which solidification is complete. Except for the eutectic portion of this curve the exact position of it is not known; *i. e.*, in our figure the dotted portions are conjectural. They may be calculated with some degree of accuracy, according to Stansfield, when the latent heat of fusion of the solvent is known and assuming that the dissolved substance is monatomic in the liquid state. The upper line *a*, *e*, *c* is what Roozeboom calls the "liquid" curve; the lower lines, *a*, *b*, *e*, *d*, *c* he calls the "solid" curve. When the liquid and solid curve have a maximum or minimum point, as in the curve before us, they touch, and the liquid whose composition corresponds to this point of meeting will solidify at a single temperature. A similar diagram to this one may be used in explaining such complicated curves as the Al-Cu curve if we remember that the terminii of the liquid and solid curve need not be pure metals, but may be a metal and a compound, or perhaps two compounds. The more or less triangular areas, *a, b, e* and *c, d, e* represent mixtures of liquid and solid phases. The areas *a, b, r, f* and *c, b, d, s* are solid solutions and *b, d, f, g* is the area of solid eutectic. Above the liquid curve we have only liquid phases, while below the solid curve we have only solid phases, and, as has been said, intermediate areas correspond to a mixture of liquid and solid phases.

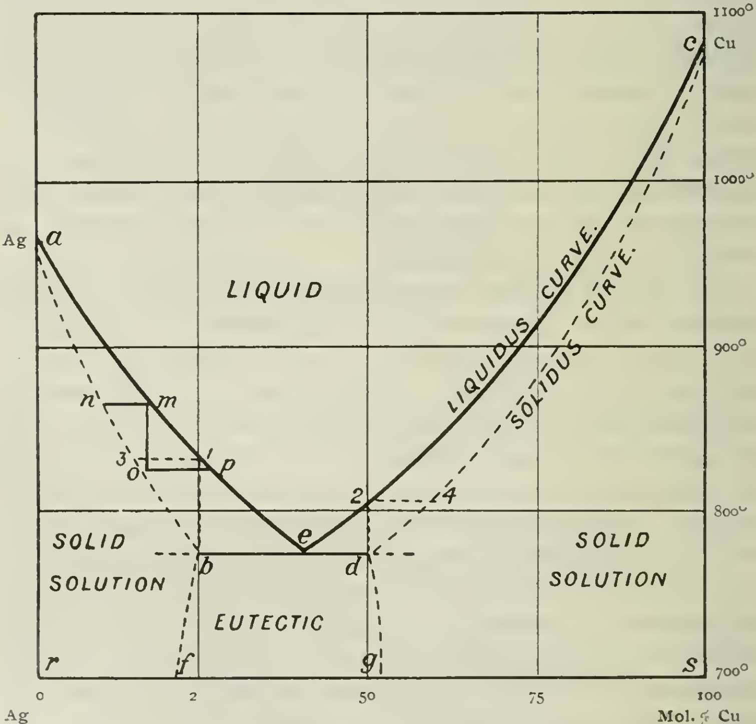

FIG. 18.

Let us consider the phenomena exhibited during the cooling of alloys represented by compositions along the branch of the curve *a, e*. Identical phenomena are displayed by alloys on the other branch *e, c* of the freezing-curve. At the points *a* and *e* we are dealing with pure silver and eutectic respectively; since both the freezing-point and the composition of these is fixed, we have at *a* and *e* nonvariant systems.

All points in the liquid area correspond to a system of two components, Cu and Ag; and one phase, solution; hence divariant. Within the area marked "solid solution" we also have a divariant system; the components are the same—the phase is solid solution. The temperature and concentration of either the liquid or solid solution are free to vary so long as no curve in the diagram is intersected. In the case of the eutectic, since composition is fixed, we have a monovariant system: two components and two phases, viz., solid solution of copper in silver and solid solution of silver in copper. The temperature may vary independently until it rises to the melting-point of the eutectic. The area marked "solid eutectic" is apt to be misleading. Any point in it represents an alloy whose composition is of the average composition indicated by that point when projected upon the composition line, but alloys in this area are not composed of pure eutectic only, as the figure seems to indicate, but of eutectic plus solid solution. The eutectic itself is composed of two solid solutions whose compositions are indicated at the points where a horizontal line through the point in question cuts the lines *b*, *f* and *d*, *g*. These lines indicate the maximum solubility of either metal in the other, and since this probably decreases, even after solidification, they are properly drawn somewhat slanting. The meaning of this area, therefore, is that such alloys as give a eutectic point in their cooling curve consist of a conglomerate of solid phases, while all solid alloys lying outside of this area are homogeneous and consist of but one phase.

According to Professor Roozeboom the phenomenon of solidification takes place in this way. Any alloy represented, *e. g.*, by a composition which we will call *m*, begins to solidify by the separation of crystals of the composition *n*. They therefore contain less copper than liquid alloy *m*. The residual liquid has therefore become enriched in copper and its freezing-point is lowered, and its composition passes from *m* to *p*, at which point final solidification takes place, giving crystals of the composition *o*; that is, while the liquid solution is changing from *m* to *p*, the composition of the mixed crystals has changed from *m* to *o*. Complete solidification has thus taken place through an interval *m*, *o*. There is an alloy, obviously, whose final solidifying point is *b*; at this point it is a liquid of composition *e* which solidifies. Similar changes take place on the branch *c*, *e*, so that all alloys between compositions *1* and *2*, after separating out crystals of solid solution of Cu in Ag or Ag in Cu, whose compositions vary respectively along the lines *3*, *1* and *2*, *4*, become concentrated to the composition *e* where saturated solid solutions represented by the compositions *b* and *d* solidify side by side, constituting the eutectic; that is, any alloy the vertical projection of whose composition upon the solid curve does not intersect the eutectic line consists of a single solid solution. Any alloy whose composition projected vertically intersects the eutectic line consists, when solid, of a conglomerate of eutectic of constant composition *e* (made up of two solid solutions of compositions *b* and *d*) and a solid solution whose composition is represented by the horizontal projection of its composition upon the corresponding solid curve.

At points in *a*, *b*, *e* and *c*, *e*, *d* solid solutions and liquid solutions are in equilibrium; *e.g.*, at the temperature *mn* there exists in equilibrium solid solution of composition *n* and liquid solution *m*. The relative amounts of these depend upon the concentration of the original alloy. Along the line *b*, *d* liquid solution of composition *e* and solid solutions of the compositions *b* and *d* are in equilibrium. The system is nonvariant. At points along the curves *a*, *e* and *a*, *b* monovariant systems exist, the phases in each being liquid and solid solution. By definition eutectic is the last substance to freeze; therefore, solid eutectic can only be in equilibrium with a liquid solution of its own composition *e*, and with solid solution. All points on *b*, *e*, *d* represent nonvariant systems, for here two components may exist in three phases at the temperature represented by the line of the eutectic.

In discussing the application of the law of phases, LeChatelier says that in a mixture of solid bodies at ordinary temperatures, and resulting from such reversible transformations as solidification through cooling or crystallization from solution, it appears that the stable state corresponds to a monovariant system; *i. e.*, the number of phases must be equal to the number of independent constituents (components) which enter into its composition, The result of this applied to alloys obtained by fusion is, that when a nonvariant system is cooled below its point of last solidification, one of these phases disappears. The system becomes monovariant and remains so to the ordinary temperature, provided none of the solid substances undergo any subsequent transformation during cooling. Otherwise it passes through a new invariant point, as in the case of carburretted iron, and always reaches a monovariant system, As indicating the direction of change in equilibrium when the system is altered, Le Chatelier says: "Any change in the factors of equilibrium from outside is followed by a reverse change within the system." "Thus," according to Professor Bancroft, "if heat is added there is an increase in the formation of the component or phase involving an absorption of heat. In other words, the system in equilibrium tends to return to equilibrium by elimination of the disturbing element." To illustrate, we might mention slowly cooled cast-iron. It usually consists of two components—iron and carbon; and three phases—iron, graphite and carbide. The system is nonvariant. It is unstable in the solid condition and on re-heating the carbide of iron phase disappears, breaking up into iron and graphite. There are now two phases and two components—the system is monovariant and, according to Le Chatelier's theorem, is stable.

CONCLUSION.

It may seem to you that in what has gone before I have laid too much stress upon pyrometric methods and the curves which they afford. I have done so purposely, as I considered this the least familiar to Americans of all the methods of studying alloys. I would not presume, either, to go into details of the value of metallography, since I see that such an eminent authority as Mr. Sauveur is to follow me in this course of lectures, and other gentlemen are to present papers to you upon metallographic topics. Again, I have made no mention of the value of electrical conductivity, tensile strength, elasticity and other usual physical and mechanical tests. They are of the utmost practical value, but they may be safely omitted from *one* lecture, for, to tell the truth, they have been too long relied upon to give all the information necessary in regard to alloys. I think it will not be long before both metallography and scientific pyrometry will be considered indispensable in metal work, the latter not only to give evidence in regard to freezing-points and equilibrium curves, but to regulate to a nicety the temperature of annealing, tempering and working processes. In regard to the recent contributions to our knowledge of alloys, I regret to say that too much of it has come from foreign sources. As a nation we have not done our part in this branch of investigation. You may recall that almost without exception the authorities I have quoted have been English or French scientists. Mr. Sauveur is compelled to seek most of the material for his valuable journal, *The Metallographist*, from these same authorities. How long is this to continue? When are American scientists to assume their just share of this work? When will our technical societies give it official recognition as the Institution of Mechanical Engineers and the Société d'Encouragement pour l'Industrie Nationale have done? It will be universally admitted that there are yet many problems in regard to alloys awaiting solution. To solve these problems is to open the way for great industrial advances in every industry that is dependent upon metals and alloys in any way, and what manufacturing industry is not? Research is the one means of bringing about their solution. Pardon me, in conclusion, for making a new and figurative adaptation of the law of phases to this kind of "solution." We are dealing with a system of one component—research; time and toil are the physical actions; metallography, pyrometry and physical-mechanical testing of the ordinary sort are the phases. The phases exceed the components by two; the system is nonvariant. Let us not be discouraged because of the magnitude of the numerical equivalent of the physical conditions. Our knowledge in no department of science is complete.

[*To be concluded.*]

IN MEMORIAM.

JOHN G. BAKER.

John G. Baker, an active and highly esteemed member of the Franklin Institute, died on Sunday, December 8, 1901, after a lingering illness.


JOHN G. BAKER.

Mr. Baker was born near Princeton, N. J., on May 11, 1833. He served his apprenticeship as a carpenter, and shortly thereafter established himself in business in the city of Washington as a model-maker for inventors.

His subsequent career as an inventor of many ingenious implements and machines made him widely known and brought him reputation and a competence.

His first invention was a machine for making glaziers' points, which he later modified into a machine for toothing hand-saws. This invention was adopted by Henry Disston, the Philadelphia saw manufacturer, in whose employ Mr. Baker entered in 1861. While in the Disston works Mr. Baker invented and successfully introduced a number of devices for grinding various kinds of saws and other labor-saving devices.

In 1864 he associated himself with T. Henry Asbury, under the firm name of Asbury & Co., for the manufacture of an indicator to save the reverse belt in screw-cutting lathes.

The firm of Asbury & Co. was dissolved and the Enterprise Manufacturing Company was organized in 1866. In the development of this company's operations Mr. Baker's ingenuity as an inventor proved conspicuously fruitful. No less than forty inventions originated with him in connection with the company's business, many of which have become staple articles in the hardware trade. The most useful of these, perhaps, are a meat-chopper and a self-measuring faucet for drawing and measuring molasses and other heavy liquids, both of which are extensively used throughout the United States.

His most important invention was the well-known "Baker pressure blower," now largely in use in foundries and mines, and for which he was awarded the John Scott Legacy Premium and Medal by the Board of City Trusts of Philadelphia, on the recommendation of the Franklin Institute, in January, 1875.

He retired from active business in 1888, though he continued to retain the position of Vice-President of the Enterprise Manufacturing Company until his death.

Mr. Baker was the typical American mechanic—resourceful, self-reliant, and ingenious—and possessed of the rare faculty of quickly perceiving and applying the simplest, most direct and effective methods of solving a mechanical problem presented to him. Even after his retirement from active business, to enjoy the evening of his useful life in leisure, he still took delight in inventing devices of various kinds. An ingenious stereoscopic camera and improvements in fishing-tackle were the fruits of these leisure hours.

Mr. Baker became a member of the Franklin Institute in 1866, and from his earliest connection with the society manifested a lively interest in its work. His services were always at command for committee work, as the records of the Committee on Science and the Arts bear testimony. His reluctance to figure conspicuously among his associates caused him repeatedly to decline election to the directorship, a position which he was frequently urged to accept. On more than one occasion he contributed substantially to the financial needs of the Institute, but these contributions were invariably coupled with the stipulation that his name should not be made known. His lamented death leaves the writer free to publish that which, in life, his modesty caused him to conceal.

His many sterling traits of character earned for him the sincere respect and esteem of his associates in the Institute.

Mr. Baker is survived by a widow and a married daughter.

W. H. W.

PROTECTIVE COATINGS FOR THE IMMERSED PORTIONS OF IRON VESSELS.

The advent of iron ships has been accompanied with a difficulty which has not yet been met satisfactorily, namely, that of a suitable protective coating of the immersed parts to protect them from corrosion and the attachment of marine plants and animals. The latter is the most troublesome, and especially in tropical waters the rapid accumulation of marine growths of this nature in the course of a few months fouls the bottom of iron ships to such an extent that the speed is reduced to one-half or even more. This, of course, materially reduces the efficiency of a vessel—especially of a naval vessel—and necessitates frequent docking for scraping and cleansing.

The old plan generally employed with wooden vessels was to sheath them with copper sheathing. In the case of iron vessels, however, this cannot be done without first applying wooden sheathing to which the copper can be attached, and this is a somewhat expensive operation, which, on this account, is only occasionally resorted to. Nevertheless, unless some efficient substitute for copper sheathing is devised in the near future, this expensive method will have to be adopted.

Attempts have been made to coat the iron hulls of ships with copper electrically by a process of electric plating on a huge scale, but those appear to be unsuccessful. Meantime recourse has been had to the application of various paints and similar protective coatings, none of which, thus far, have proved entirely satisfactory.

The *Chemiker Zeitung* has an instructive article on this subject, from which the following information is derived : The author enumerates the following indispensable conditions for a protective coating for iron ships to realize the requirements of service : (1) The compositions should protect the ship's hull from corrosion ; (2) they should form a smooth surface, so as to decrease the friction ; and (3) they should dry quickly so that the cleansing of the submarine parts and the application of a double coating can be done in a single day. In the case of new steel vessels, the black scale with plates must first be removed by the use of acid pickle before applying the coating, which otherwise will drop away with the scale and expose the exposed metal to rusting.

The author describes several paint compositions that have been used for this purpose, and gives the preference to that proposed by Rahtjen. This process consists in employing as the vehicle a solution of shellac in spirit, to which is added some iron oxide and a small proportion of linseed oil to give it elasticity. This first or priming coat is intended to serve for insulating the iron hull, and is followed by a second one composed of the same materials with the addition of arsenic and quicksilver. The Rahtjen compositions are highly commended for their permancy, as the salt of the sea water has but little action on the shellac which constitutes the vehicle of the paints. The efficiency of the second coating as a preventive of marine growths is ascribed to the formation of mercuric chloride by the slow action of the seawater on the quicksilver contained in the composition, and which acts as a poison towards the marine organisms. These paints have the further advantage that they dry quickly, so that several coats may be laid on in a single day.

The defects of the Rahtjen paints are, that only a small quantity of mercury can be incorporated in the paint, as otherwise the shellac would be affected, and that the effectiveness of the paint decreases with time because of the slight solubility of the shellac.

Of the mineral poisons incorporated in these compositions, copper and mercury have been found most efficacious. Copper would be preferable on account of the cheapness, but has the objection that when the insulating or priming coat has become defective, its presence in considerable quantities in proximity to the iron hull is apt to cause rapid corrosion of the iron by galvanic action.

The author of the article above referred to gives also some instructive information as to the manner in which the efficiency of such protective compositions is called into action. Thus he explains: the purpose of the poisons in the paint is to kill the germs of the crustaceous animals, which only swim about freely during the first stages of their development, and in seeking a permanent place of growth, attach themselves to the vessel's hull. While the ship is moving through the water the paint layer is being continually affected by friction, so that the seawater can enter into chemical action with the poisons of the paint, whereby there results the production of an antiseptic (germicide) compound on the surface that destroys the organisms which come in contact with it, and so long as the ship's motion is continued, fresh portions of the paint-film are successively exposed with the same result. In time the antiseptic action of the paint-film ceases by the exhaustion of the poisonous material, when its virtue as a preventive of marine growths ceases, though the insulating coating may still protect the hull from corrosion.

Again, when the hull of the vessel is at rest. as when in harbor, the continuous formation on the surface of the paint layer of an antiseptic substance is arrested, being rapidly exhausted by the vast amount of animal life coming in contact with it, and not being renewed by frictional contact with the water as when the ship is in motion. This is the explanation of the fact that a vessel in port (particularly in tropical waters) fouls much more rapidly than when at sea. It is observed, however, that the antiseptic agents which have thus become exhausted after a few weeks' detention of the vessel in port, again become effective after putting to sea, when the exhausted particles of the paint skin are removed by friction, exposing a new poison-saturated surface.

The whole subject of the proper protection of the hulls of iron vessels from rusting and marine growths is in a sort of transition stage, very much like that of finding an efficient substitute for wood on warships, for, notwithstanding the many remedial agents that have been proposed, none of them has fully met all the requirements, and the problem still awaits its solution.

W.

Franklin Institute.

[*Proceedings of the annual meeting held Wednesday, January 15, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 15, 1902.

Vice-President THEO. D. RAND in the chair.

Present, 137 members and visitors.

The annual reports of the Board of Managers and of the various committees and Sections were presented in printed form, and were duly accepted.

The tellers of the annual election reported the election of the following candidates:

For President	(to serve one year)	JOHN BIRKINBINE.
" *Vice-President*	(" three years)	WASHINGTON JONES.
" *Secretary*	(" one year)	WM. H. WAHL.
" *Treasurer*	(" ")	SAMUEL SARTAIN.
" *Auditor*	(" three years)	WM. H. GREENE.

For *Managers* (to serve three years).

BENJ. SMITH LYMAN,
COLEMAN SELLERS,
ISAAC NORRIS, JR.,
STACY REEVES,
THOS. P. CONRAD,
JAMES M. DODGE,
C. LELAND HARRISON,
FRANCIS SCHUMANN.

For *Members of the Committee on Science and the Arts* (to serve three years).

HUGO BILGRAM,
FRANK P. BROWN,
J. J. DE KINDER,
W. C. L. EGLIN,
A. M. GREENE, JR.,
LEWIS M. HAUPT,
FRED. E. IVES,
A. E. KENNELLY,
WILFRED LEWIS,
EDGAR MARBURG,
C. J. REED,
E. ALEX. SCOTT,
COLEMAN SELLERS,
H. W. SPANGLER,
MARTIN I. WILBERT.

Capt. B. W. Dunn, Ordnance Department U. S. A., Frankford Arsenal, presented a detailed description of the Gathmann Gun Tests, illustrating the same by a series of lantern slides showing the results of comparative tests of the Gathmann gun and projectile and the standard service gun and projectile with time fuse, at the Government proving grounds.

Mr. J. O. Nixon, Link-Belt Engineering Company, gave a description of the Renold Silent Chain-Gear, with illustrations, showing the successful application of the gear to a number of diverse machines where in many cases the use of ordinary systems of power transmission would be extremely difficult. The subject was referred to the Committee on Science and the Arts.

Dr. Joseph W. Richards, Lehigh University, gave a brief preliminary account of the Theory of Harmony announced by Professor Goldschmidt of the University of Heidelberg. (The subject will be presented *extenso* at the meeting of the Physical Section on Monday, January 22d.

Dr. Henry Leffmann made some remarks apropos to the proposal now before the National Congress to establish a Forest Reserve in the Southern Appalachian Mountains, and illustrated the subject with the aid of lantern views. It was voted to give the sanction of the Institute to the project by becoming a member of the Appalachian Park Association.

Mr. Edwin A. Heyl exhibited and described an improvement in horns for talking machines, which the inventor termed the "Vocalophone." The advantages claimed for the new horn were demonstrated by a comparison of reproductions of vocal and musical records with the horn commonly used and the improved device.

A memorial of the late Luther R. Faught, life-member of the Institute, prepared by Dr. Edwin J. Houston, was presented by the Secretary and referred to the Committee on Publications.

Adjourned. WM. H. WAHL, *Secretary.*

ANNUAL REPORT OF THE BOARD OF MANAGERS OF THE FRANKLIN INSTITUTE FOR THE YEAR 1901.

[WITH APPENDICES.]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 8, 1902.

To the Members of the Franklin Institute.

The record of 1901, like those of recent years, while satisfactory in a number of features, is not encouraging when the work which the Institute has done, and that which it could and should do, are contrasted. The increase of membership is welcomed as adding to the interest of the Institute, and as aiding somewhat its income. The attendance at meetings, the character of the papers presented, and the work of Sections are all gratifying evidences that the members feel a lively interest in the work of the organization, but there is unfortunately constantly before us the knowledge that local appreciation is wanting of what the Institute has accomplished and of the assistance it could render the public.

That we command the services of noted specialists who deliver lectures or present papers before the Institute or its Sections; that the *Journal* is widely circulated and gladly received in exchange for the most noted publications throughout the world; that membership in the Institute is recognized as an honor, and that the verdict of the Committee on Science and the Arts is sought by inventors, are gratfying evidences of appreciation; but the condition which faces the Institute is, that each year it is necessary to supplement the income from regular sources by voluntary contributions to maintain the work in at least an apparently satisfactory condition.

The Franklin Institute should not for one moment have to consider whether a book could be purchased, but should have the money ready to add any domestic or foreign publications to its library which come within the list of technical or mechanical works. Its Sections and Committees should be more liberally supported, thus providing for advanced usefulness and greater value to the Institute and to the public at large.

It is essential that something should be done to attract attention to the Institute and its functions, and to obtain from the public such assistance as will be serviceable, in fact necessary to carry on its work as it should be done, especially as the reduction of interest rates on investments makes the work of sustaining the Institute, as it should be conducted, more difficult each year. That it has work to perform in the future, as in the past, is admitted; to perform it creditably a large revenue is necessary; to obtain this revenue there are several possible methods.

(1) The roll of members can be augmented and the dues of members could be increased, a policy which many technical societies have been compelled to adopt.

(2) Appropriations from municipality, State or Government could be solicited, and although they have been earned by public service rendered be the Institute in the past, and undoubtedly would be repaid with interest in the future, the acceptance of such assistance might not be without drawbacks. The Institute to-day stands entirely untrammeled, and the reports of its work now go before the world free from the taint of partisan, factional or political significance.

(3) An increase in facilities could be secured by a sufficient endowment, the annual interest from which would yield a sufficient income for the needs of the Institute; or, annual contributions might be solicited for which some direct return could be made to various industries or organizations specially benefitted by the Institute's work.

The third plan seems to be the most helpful and desirable, and it is believed that if the members of the Institute could be brought to co-operate, each doing what he or she could, the result would be most satisfactory.

A sufficient endowment would also indirectly aid the effort to secure better accommodations in a more desirable location, a subject of the deepest interest to the Board of Managers, and which is now in the hands of a special committee. Greatly as we revere the old building, which, for more than three-quarters of a century has been the home of the Institute, the Board realizes that business changes have made the present location undesirable, and that the requirements of the organization demand better accommodations. But the limitations of our treasury and the charter under which the Institute exists prevent taking this most desirable step without financial aid.

The Institute, recognized as a useful organization for more than three-quarters of a century, with a world-wide reputation, commonly is credited with having ample funds. It is unfortunate that this is not the case, and equally unfortunate that the increasing demands of the present time, coupled to the decreasing interest rate, make the maintenance of the work more difficult than in past years.

A united effort on the part of the membership should bring the Institute into a position of financial ease and permit it to enlarge its field of usefulness both for the present and the future.

Reports of the various committees, hereto appended, will indicate that the year's work throughout has been well done, and attention is called to the several instances during the year where the Institute has taken up problems of general interest. At present there are under discussion, in the hands of special committees, the Metric System of Weights and Measures and the Hazards to Life and Property associated with our rapid industrial progress.

The Institute is ready now, as in the past, to carry forward investigations which advance science and the arts, or which in any way benefit the general public. Its large membership of men skilled in science and practice has always responded to the call of the national, State and city government, and has rendered without compensation services which could not otherwise be secured unless at heavy expense. These public services should invite such generous appreciation as will place the Institute in position to carry on its unselfish work with greater effectiveness.

By order of the Board.

JOHN BIRKINBINE,
President.

FINANCIAL STATEMENT FOR THE YEAR 1901.

Balance on hand January 1, 1901	$1,051 86
Receipts in 1901	32,206 23
Payments in 1901	32,208 13
Balance on hand January, 1902	$1,049 96

APPENDICES.

REPORT OF THE COMMITTEE ON ELECTION AND RESIGNATION OF MEMBERS, 1901.

Members at the close of 1901	2,287	
New members elected in 1901, who have qualified	153	
		2,440
Lost by death, resignation and non-payment of dues		97
Total membership at the end of 1901		2,343

ALEXANDER KRUMBHAAR,
Chairman.

ANNUAL REPORT OF THE COMMITTEE ON INSTRUCTION, 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 1, 1902.

To the Board of Managers.

The Committee on Instruction has continued the arrangement with the Central Branch of the Young Men's Christian Association for two courses of popular scientific lectures during the autumn and winter seasons. The services of the lecturers, as heretofore, have been obtained gratuitously, and the Committee recommends that a suitable acknowledgement be made to the gentlemen who have so generously co-operated in the educational work of the Institute.

The statistics of the schools exhibit a substantial increase in the number of pupils, especially, in the Schools of Naval Architecture and Machine

Design. The growth of interest in these comparatively new undertakings appears to have fully justified the Committee in proposing their establishment.

Following are the figures of attendance as compared with the preceding year:

	1900.	1901.
Drawing School	298	299
School of Naval Architecture	61	97
School of Machine Design	61	80
Totals	420	476

The Committee is fully satisfied with the zeal and ability of those in charge of the schools.

Respectfully,

WILLIAM H. WAHL,
Chairman.

ANNUAL REPORT OF THE COMMITTEE ON PUBLICATIONS FOR 1901.

To the Board of Managers.

The Committee on Publications deems it a subject for congratulation that the *Journal* of the Institute has, of late years, become in reality what its name would imply, viz., the medium for the recording and publishing of papers and communications representing the scientific activity of the various branches of the Institute.

For this highly satisfactory condition, the several sections—at present numbering six—deserve the credit; and the Committee feels satisfied, from the evidence afforded by the *Journal*, that at no time in its history has the Institute displayed so much and such varied scientific activity as at the present time.

The Committee has not felt the necessity for a number of years of seeking outside material to fill the pages of the *Journal*, and from present indications of the activity of the Sections there will be no need of this for the future, the amount of material in reserve for publication being at all times more than sufficient for the Committee's requirements.

The society publications and periodicals received in exchange for the *Journal* at the close of 1901 numbered 512, an increase of 15 over the previous year. The value of this mass of literature added annually to the library may be estimated conservatively at $1,600.

THEODORE D. RAND,
Chairman.

ANNUAL REPORT OF THE COMMITTEE ON THE LIBRARY FOR THE YEAR 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 6, 1902.

To the President and Members of the Franklin Institute.

GENTLEMEN:—The Committee on the Library respectfully reports the following summary of the operations of the Library during the year 1901:

ADDITIONS.

	Bd. Vols.	Unbd. Vols.	Pphs.	Chts.	Phts.	Engrs.	Drawings.
By Gift	530	320	844	26	11	6	1
Exchange	25	1	2				
Binding	67						
From Com. on Pub. .	46	17	12				
Purchase	74	50		2			
Lea Fund		3					
	740	391	858	28	11	6	1

Total additions for the year 2,035

A decrease of 1,374 from 1900, and of 802 from 1899.

Total number of volumes, January 1, 1902 53,360

Total number of pamphlets, January, 1902 37,891

The Library also contains 2,855 maps and charts, 659 designs and drawings, 1,225 photographs, 191 newspaper clippings, 30 manuscripts.

BINDING.

Periodicals reported among the additions	67
Old books	29
Lea Fund books	1
Rebound	5
	102

Exchanges.—Five hundred and twelve societies and publications were on the exchange list of the *Journal* during the year 1901, an increase of 15 over 1900.

It is twenty years since the yearly additions to the Library have been so few. All sources of income the past year have temporarily been almost wholly cut off, few books were bought, little binding was done, and the additions were mainly made up of special gifts, even more than is usually the case. Owing to financial circumstances, the Moore Fund, ordinarily yielding about \$750 a year; the Memorial Library Fund, about \$50 a year; and the James T. Morris Fund, about \$100, have the past year supplied no books; and the Lea Fund, about \$150 a year, very few. The prospect is now good for the usual income from all the funds in the coming year.

Some years ago the Librarian found by actual count that the number of visitors to the Library, though variable, according to the weather and season, averaged about 150 daily, two-thirds of them in search of patents. The number still continues apparently undiminished or increasing. The Library, with its more than 95,000 volumes, pamphlets, maps and drawings, and its large daily attendance, is unquestionably the most important feature of the Institute; and clearly every effort should be made to maintain and augment its usefulness.

It is extremely important that the binding be virtually brought up to date as regards the complete sets of current periodicals during the coming year, by binding about 500 volumes, at an expense of about $450. After that, about $300 a year would cover the cost of binding and make it possible to bind all the serial publications as soon as each volume is completed, so that the sets could be kept in better condition and be more useful for reference. In addition, a large number of not yet completed sets of periodicals could, with great advantage, be bound up at once if there were any funds for the purpose. The cost would probably be $250

The Board of Managers has lately granted to the Library the use of the northern room near the front door on the lower floor, and a large portion of the required shelving has just been put in at the cost of nearly $100, yet to be paid. The rest of the shelving can be put in later and will cost about $50. At that time it will be necessary to put in electric lighting also, at a cost of perhaps $25. A small gallery should also be built for the upper shelves, and a little staircase to reach the gallery. The room is perfectly fireproof, excepting the very slight risk from the window and door. Perhaps means may later be found to replace the window with wire-glass and to add an inexpensive fireproof door outside the present one. The additional book-room came not a day too soon; for books already had to be removed to the floor from the shelves of the main book-stack to make space for important acquisitions of patent publications in frequent use. The new room will be used for books that are less frequently consulted and for the nearly 38,000 pamphlets.

This rapidly growing and immensely valuable collection of pamphlets has for many years necessarily been lying in disorder and unusable in the part of the third-story that is not fireproof. They can now be gradually removed to the new room, and at least roughly arranged, as soon as pamphlet cases and a little additional assistance can be provided. About 15,000 of them have already been catalogued. There is very pressing need that they should all be catalogued and thoroughly arranged; but for that purpose it would be necessary to employ permanently at least one more assistant in the library, at an expense of about $10 a week.

That assistant would also make possible more complete attention to a very important part of the library work that has for some years had to be neglected from want of sufficient force; and that is, the constant watchful pursuit of numerous municipal and governmental serial publications of great engineering value, and countless trade catalogues, highly useful when methodically arranged. They are willingly sent without cost to the library, since it is an unexcelled repository for such works. Yet, with the frequent change of officials and clerks, the Library is but too apt by oversight to be dropped from mailing lists, unless the Library keeps a careful lookout and sends a timely reminder when an expected publication is missing.

The Library has a collection of 500 maps of the U. S. Geological Survey. The sheets are valuable, and some are out of print. They are frequently used, and for want of proper room are getting badly torn and seriously damaged. A suitable case should be provided with space for a thousand sheets. If the charts were arranged according to States in shallow drawers, with room enough, it would not be necessary to handle all the maps when only one is needed.

The cost, then, of satisfying the more pressing needs of the Library, beyond the ordinary expense for the coming year, would be:

Binding, mainly completed sets of periodicals	$450
Binding periodicals of sets not yet completed	250
Fitting up the new book-room, about	250
An additional assistant, about	500
Map case, perhaps	100
	$1,550
In addition, certain very much needed books not within the power of the special funds would cost about	1,000
	$2,550

The members of the Library Committee still continue to show great zeal in their work, and co-operate harmoniously for the interest of the Institute.

BENJAMIN SMITH LYMAN,
Chairman.

ANNUAL REPORT OF THE COMMITTEE ON SCIENCE AND THE ARTS FOR THE YEAR 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 1, 1902.

To the President and Members of the Franklin Institute.

The following report of the Committee on Science and the Arts is respectfully submitted:

The number of cases pending on December 31, 1901		48	
The number of new cases proposed by application was	36		
By reference from Institute	3		
By reference from Sections	1		
By vote of the Committee	5		
Total number of new cases in 1901		45	
Total number of cases before Committee in 1901			93
Number of cases acted upon (of which there are pending 5.)	51		
Finally determined		46	
Cases pending December 31, 1901		47	
			93

The forty-six cases completed were determined as follows:

Award of Elliott Cresson Medal	4
Award of John Scott Medal	12
Award of Edward Longstreth Medal	4
Award of Certificate of Merit	1
Reports without Award	5
Reports advisory	2
Cases withdrawn	1
Cases dismissed	17
Total	46

The Committee has been more or less hampered throughout the year by the difficulty in obtaining a sufficient quorum of its members to take final action on the reports at hand, and still more by the insufficiency of its membership to deal expeditiously with the numerous cases before it. The investigation of the subjects proposed for consideration frequently demands of the Sub-Committees the devotion of much time and effort to the proper elucidation of their respective subjects, and as these investigations must necessarily be made by experts and professional men whose time is largely preoccupied, and whose service in this work is given gratuitously, it follows, of course, that the work proceeds less rapidly than is desirable. The summer months being, furthermore, virtually eliminated from the working time of the Committee, its activity is thus practically limited to some nine months of the year.

In view of these conditions it is recommended and proposed that the membership of the Committee be increased from its present number of forty-five (45) to sixty (60). The additional force doubtless would serve to add materially to the Committee's efficiency and to relieve its individual members of too onerous a burden.

Pursuant to the resolutions of the Institute the Committee has proceeded to the preparation of properly engraved diplomas of award to accompany the several medals now within the gift of the Institute. The details of this matter, as embodied in the report of the Sub-Committee having the work in charge, form the subject of a communication from the Committee to the President and Board of Managers of the Institute, under date of the 4th ult., and the proposed action having on the 11th ult. received the approval of the Board, the engraving and printing of the three diplomas in question, namely, the Elliott Cresson, the Edward Longstreth and the John Scott Medals have been ordered to execution by the American Bank Note Company in accordance with proposition and design submitted by that company under date of November 26th ult., and the work will be completed in due course.

LOUIS E. LEVY, *Chairman.*

ANNUAL REPORT OF THE COMMITTEE ON MEETINGS, 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 1, 1902.

To the President and Members of the Franklin Institute.

The Committee, with the Secretary's aid, arranged the program of the ten stated monthly meetings of the past year, as required by the by-laws.

These arrangements involved the presentation of eighteen communications, and of a number of subjects that were presented informally.

The average attendance at the meetings during the year exhibits a substantiated increase as compared with the several years immediately preceding. A number of the topics presented afforded acceptable material for the *Journal.*

The publication of the Monthly Bulletin has been continued as heretofore, and notwithstanding its considerable cost, the Committee believes it would be very unwise to discontinue it.

Several important subjects presented at the meetings have given cause for the appointment of special committees for their consideration, and it is anticipated that the investigations of these committees will prove of substantial value.

WASHINGTON JONES, *Chairman.*

ANNUAL REPORT OF THE CHEMICAL SECTION FOR THE YEAR 1901.

To the Committee on Sectional Arrangements.

During the year 1901 the Chemical Section held seven stated meetings, at which the following subjects were presented and discussed:

Dr. W. J. Williams. "What Constitutes an Explosive?"

Mr. Lyman F. Kebler. "What does the Designation C.P. Mean?" "A New Method for Chromic Acid and the Soluable Chromates;" "A Communication on Walnut Oil."

Prof. F. W. Clarke. "The Chemical Work of the U. S. Geological Survey."

Mr. W. E. Ridenour. "The Chemistry of Deposits in Steam Boilers;" "The Estimation of Caustic in the Presence of Alkali Carbonate."

Dr. Jos. W. Richards. "Some Abridgements of Chemical Calculations."

Dr. Robert H. Bradbury. "Some of the Researches of Professor Spring, of Liége;" "The Goldschmidt Process for the Reduction of Certain Metals and the Production of High Temperatures."

Dr. H. F. Keller. "Moissan's Work on Fluorine and the Fluorine Compounds;" "The Shimer Crucible for Carbon Combustions."

In addition to the above, several of the meetings were devoted to the exhibition of numerous new and interesting chemical lecture experiments (Drs. Keller, Bradbury, Leffmann and Sadtler). Mr. Wm. McDevitt presented some instructive experiments suggested by explosions at fires; and Dr. Keller showed and commented on a fine series of lantern-slides illustrative of historical chemistry.

The membership of the Section has been fully maintained, and the general interest of the members continues unabated.

The program for the first semester of 1902 is of unusual interest.

HARRY F. KELLER,
Chairman of Executive Committee.

ANNUAL REPORT OF THE ELECTRICAL SECTION FOR THE YEAR 1901.

To the Committee on Sectional Arrangements.

The Electrical Section held eight meetings during the year 1901, a number of which were fruitful of valuable papers.

The interest of the membership in the work of the Section and the general activity of the Section's affairs are noted with satisfaction. A number of the papers presented during the year have appeared in the *Journal.*

Following is a list of communications presented :

Mr. Carl Hering. "Three-Phase Electric Traction in Europe."

Dr. Joseph W. Richards. "Secondary Reaction in Electrolysis."

Mr. Charles Wirt. "Theatre Dimmers and Stage Lighting."

Mr. W. J. Hammer. "Recent European Electrical Progress."

Mr. Edwin S. Church. "Minting Machinery and Appliances."

Dr. A. E. Kennelly. "The Edison Storage Battery."

Mr. C. J. Reed. "Polarization in Batteries."

Mr. Paul M. Lincoln. "The Parallel Operation of Alternating Current Generators."

Mr. A. J. Wurts. "Development of the Nernst Lamp in America."

Subjects for discussion were communications by Messrs. Hering, Kennelly, Eglin and others on "The Electrical Arts at the Pan-American Exposition," and a paper by Mr. C. J. Reed on "Electro-Chemical Action."

The prospects of the Section for the immediate future are most satisfactory.

WILBUR M. STINE,
Chairman Executive Committee.

ANNUAL REPORT OF THE MINING AND METALLURGICAL SECTION FOR THE YEAR 1901.

To the Committee on Sectional Arrangements.

The proceedings of the Section have been duly conducted by the following officials :

President, F. Lynwood Garrison.

Vice-Presidents, Arthur Falkenau and Spencer Fullerton.

Secretary, G. H. Clamer.

Conservator, Dr. Wm. H. Wahl.

Membership.—The total membership shows increase of two over 1900.

Meetings.—The Section held nine meetings during the year.

Work of Section.—The papers presented were of unusual interest, as was also a discussion on the subject of "Moulding Machines and Foundry Practice," and a discussion pertaining to products of mining and metallurgical interest, as exhibited at the Pan-American Exposition, to each of which an evening was devoted. All communications were considered of sufficient importance to be published in the *Journal.*

Following is the list of papers presented :

Dr. John A. Mathews, Mr. Wm. Campbell, } "The Constitution of Metals and Binary Alloys."

Mr. Richard L. Humphrey. "The Inspection and Testing of Cements."

Prof. F. L. Garrison. "Effects of Deforestation in Northern China."

Mr. Joseph Richards. "Utilization of Waste from the Use of the White Metals."

Caspar Wistar Haines. "Remarks on the Earthquake, January 16, 1900, in the State of Colima, Mexico."

G. H. CLAMER, *Secretary.*

ANNUAL REPORT OF THE MECHANICAL AND ENGINEERING SECTION FOR 1901.

To the Committee on Sectional Arrangements.

The Section has a membership of 199. It has held eight meetings, four of which have been devoted to the general discussion of topics of importance. The average attendance at the meetings has been 33, which should be greater for a section as large as this. To increase the interest at the meetings it has been proposed to have monthly reviews of progress in various branches in engineering.

A list of the subjects presented and discussed during the year is hereto appended.

DANIEL EPPELSHIMER, JR.,
Secretary.

January.—"The Wear and Tear of Steam Boilers" (being the address of the retiring President), Mr. John F. Rowland, Jr.

February.—"The Jig Habit in America." Mr. Oberlin Smith, Bridgeton, N. J. Illustrated with specimens and models.

March.—"The Automatic Gun and Its Military Aspects." Cecil H. Taylor, Bound Brook, N. J.

April.—"Invention and the Rights of Inventors." S. Lloyd Wiegand.

May.—"Moulding Machines and Modern Foundry Practice." Discussion. (Opened by Wilfred Lewis.) The subject was illustrated by the exhibition of the machine operated by compressed air.

October–November.—"Steam Boiler Design and Inspection, with Special Reference to the *City of Trenton* Disaster." Discussion by Messrs. Samuel M. Vauclain, John M. Hartman, George B. Hartley, Washington Jones, R. D. Kinney and James Christie. The subject was illustrated by specimen test pieces and bolts from the exploded boiler shell, and lantern slides.

December.—"The Renold Silent Chain-Gear." Mr. J. O. Nixon, Link-Belt Engineering Company, Philadelphia. Illustrated by specimen gears, and numerous lantern slides showing the application of the gear to a variety of machines.

REPORT OF THE SECTION OF PHOTOGRAPHY AND MICROSCOPY FOR THE YEAR 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 1, 1892.

To the Committee on Sectional Arrangements.

The meetings of the Sections have been well attended during the entire year, and the communications presented have been of considerable interest and novelty. The principal part of the work refers to photography, and it is hoped that an effort may be made to discuss questions relating to microscopy more fully in future.

The following is a summary of the matters presented during the past year:

January 3d.—Address of Retiring President. Subject: "Application of Photography to Police and Sanitary Administration."

February 7th.—"Effect of Time on the Sensitiveness of Photographic Materials." Dr. C. F. Himes.

"Effect of Light on Chemicals, Especially Mercurous Compounds." Henry Leffmann.

"The Wager Exposure Scale." E. Wager-Smith.

March 7th.—"Use of Lantern-Slides for Class Demonstration." Dr. I. N. Broomell.

"An Exposure Meter." John G. Baker.

"Suggestions for Labeling Lantern-Slides." Henry Leffmann.

April 4th.—"Exhibition of views of a newly discovered cave at Mapleton, Pa." W. N. Jennings.

May 2d.—"Photographic record work." General discussion.

"A New Lantern Polariscope." F. E. Ives.

"Lothian Stereoscope." Dr. C. F. Himes.

October 3d.—"Application of Photography to Legal Records." Dr. Henry Leffmann.

November 7th.—"Use of Sensitive Paper in X-Ray Work." M. I. Wilbert.

December 5th.—"Experiments on Over-Exposure and Reversal in Photography," with demonstrations by U. C. Wanner and M. I. Wilbert.

The Section gave some time to the consideration of a plan for holding a general photographic exhibition, but the question has been laid aside for the present.

Respectfully submitted,

HENRY LEFFMANN,

Chairman of Executive Committee.

REPORT OF THE PHYSICAL SECTION OF THE FRANKLIN INSTITUTE. FEBRUARY, 1901—DECEMBER, 1901.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, January 1, 1902.

To the Committee on Sectional Arrangements:

To those in Philadelphia and its vicinity who are interested in Physics, Astronomy and Geophysics, it should be a cause of satisfaction that during the past year the Physical Section of the Franklin Institute has maintained so vigorous an existence. Nine meetings have been held in the eleven months with an average attendance of about twenty. Eighteen papers have been presented, of which a list is appended. More than this, it has not been difficult to secure the communications above mentioned. It would seem that the Physical Section supplies an opportunity hitherto lacking for the presentation of new work before a sympathetic and appreciative body.

It has proved to be an easier matter to obtain the communications than to secure an audience fitting to their interest and high character. It is, however, to be remembered that the sciences to which the Section is dedicated appeal to a very limited number of persons. Physics and Astronomy connotate the laboratory and observatory and these, save in rare instances, are found in connection with institutions of education only. In this is found an explanation of the fact that both for papers and for regular attendants the Section has had to rely chiefly, though by no means entirely, upon teachers in either colleges or high schools. Of the eighteen papers no less than sixteen were presented by men connected with such institutions. It is to be hoped that some way may be devised of making the work of the Section appeal to a wider audience. In addition to the regular monthly meetings the Section may properly claim the credit for the appearance of Dr. Stratton before the Institute.

We may further congratulate ourselves that the Section has in no case left the arrangement of the meetings and the obtaining of papers to the Secretary of the Institute. This evidence of an independent life augurs well for the future.

GEORGE F. STRADLING,
President.

APPENDIX.

List of papers presented before the Physical Section of the Franklin Institute. February, 1901–January, 1902.

1901.

February—"On Certain Harmonic Curves." Dr. H. C. Richards.

"The Geometric Chuck and Curves Produced by It." Dr. E. A. Partridge.

March—"Antarctica: A History of Antarctic Discovery." Mr. E. S. Balch.

"Thermometer Glass at Higher Temperatures." Mr. Wm. McClellan.

April—"E. H. Lenz as one of the Founders of the Science of Electro-Magnetism." Prof. Wilbur M. Stine.

"Recent Progress in Double-Star Astronomy." Prof. E. Doolittle.

May—"A New System of Making Corrections in Calorimetric Experiments." Dr. Joseph W. Richards.

"Historical Sketch of the Measurement of the Dimensions of the Earth." Mr. H. I. Woods.

"Preliminary Note on Crystallization under Electrostatic Stress." Dr. Paul R. Heyl.

September—"Crystallization under Electrostatic Stress" (Second Communication). Dr. Paul R. Heyl.

Reviews of Recent Progress.

"Light." Dr. H. C. Richards.

"Electricity." Dr. M. G. Lloyd.

October—"Heat." Mr. William McClellan.

November—"The Question of the Divisibility of the Atom." Dr. A. S. Mackenzie.

"Report on Period of Rod Vibrating in a Liquid."

"Report on Absoption Spectrum of Chlorine." Both reports given by Dr. Mackenzie.

"Recent Progress in Sound." Dr. Geo. F. Stradling.

December—"Mechanism and Causation of Hot Waves." Mr. Harvey M. Watts.

SECTIONS.

(*Abstracts of Proceedings.*)

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—Proceedings of the thirteenth *Stated Meeting*, held Thursday, December 5, 1901. Dr. Henry Leffmann in the chair. Present, 31 members.

On motion, it was resolved to ask the Board of Managers for an appropriation of $75 for the expenses of the Section for the year 1902.

The Executive Committee was directed to report at the next stated meeting nominations for officers of the Section to serve for the year 1902.

Mr. Wanner presented a communication on the making of "Reversed Pictures and their Development in Actinic Light." The speaker showed some samples of reversed pictures, and also demonstrated the development of these pictures before the meeting.

Dr. Leffmann called attention to a communication of Professor Nipher, lately read before the St. Louis Academy of Science, a copy of which was laid on the table for inspection by members.

Mr. Wilbert showed some samples of normal and reversed negatives, and called attention to the wide range of exposure that was possible, using the same plate and normal developer.

Mr. Samuel Sartain invited attention to several pictures that he had made accidentally. These were developed in the dark room.

The subject was further discussed by Messrs. Leffmann and Ridpath.

Mr. F. E. Ives followed with a description of a novel stereogram, for which he proposed the name of the Parallax Stereogram, and described the method of taking the pictures. (See the *Journal*, January, 1902).

Dr. Leffmann called attention to the programs arranged for the succeeding meetings. Adjourned.

M. I. WILBERT,
Secretary pro tem.

Proceedings of the fourteenth *Stated Meeting*, Thursday, January 2, 1902. Dr. Charles F. Himes in the chair. Present, 22 members and visitors.

The retiring President presented his annual address, the subject being "Photographic Permanence and the Work of the Amateur Photographic Exchange Club of 1860–64." The paper was fully illustrated with a number of prints made upwards of forty years ago.

The annual report of the Section, prepared by the Executive Committee, was read and approved. The same Committee reported a list of nominations of officers of the Section for the year 1902, which was approved, and the Secretary was instructed to cast the ballot of the meeting for the nominees.

The following gentlemen were thereupon declared elected to the offices set opposite their names:

President—Dr. Henry Leffmann.
Vice-Presidents—F. E. Ives, J. W. Ridpath.
Secretary—Martin I. Wilbert.
Conservator—Dr. Wahl.

The meeting ordered the incorporation in the Section's records of the following tribute to the memory of the late John G. Baker, Vice-President of the Section, viz.:

"The members of the Section of Photography and Microscopy desire to record their sorrow for the death of their fellow-member and Vice-President, John G. Baker. Our memory of him is not only of his geniality and sincerity, but also of his integrity and his zealous work in the interest of the Section from the first hour of its existence.

"Our published records contain numerous contributions from him, supplementary to, and descriptive of, valuable and novel plans in photography. By his death we have lost an active and companionable associate, and photographic art and science has lost an industrious votary. Adjourned.

MARTIN I. WILBERT, *Secretary.*

MINING AND METALLURGICAL SECTION.—Proceedings of the *Stated Meeting* held Wednesday, January 8, 1902, Prof. F. Lynwood Garrison, President, in the chair. No quorum being present, the papers of the evening, viz., the Annual Address of the retiring President and the "Pocono Coal of Berkley and Morgan Counties, W. Va.," by Mr. William Griffith, Mining Engineer, Scranton, Pa., were read by title.

WM. H. WAHL, *Secretary pro tem.*

MECHANICAL AND ENGINEERING SECTION.—Proceedings of the *Stated Meeting* held Thursday, January 9, 1902. Mr. A. M. Greene, Jr., in the chair. Present, 42 members and visitors.

The annual election resulted in the election of the following:

President—A. M. Greene, Jr.

Vice-Presidents—Arthur Falkenau, Charles Day.

Secretary—Daniel Eppelsheimer, Jr.

Conservator—Dr. Wahl.

The principal communication of the evening was read by Prof. Emory R. Johnson, University of Pennsylvania, on "The Engineering and Commercial Features of the Isthmian Canal." The speaker discussed the work of the Isthmian Canal Commission, and gave a summary of the results embodied in its report, and commented on the relative merits from the engineering and commercial standpoint of the several projects for the passage of the Isthmus. (The paper will appear in the *Journal*).

Mr. Francis Head followed with a paper on "The Use of Fuel Economizers in Connection with Steam Plants," which was fully illustrated with the aid of lantern slides.

DANIEL EPPELSHEIMER, JR., *Secretary.*

ELECTRICAL SECTION.—*Stated Meeting* held Wednesday, January 16th, 8 P. M. President Morris E. Leeds in the chair.

A communication on "The Mechanical Efficiency of Illumination" was presented by Mr. Thomas Spencer.

Dr. E. F. Roeber opened the discussion of the subject of "Some General Principles Concerning the Design of Galvanic Cells." Messrs. Henry, Reed, and Stine participated in the discussion.

The election of officers to serve the Section for the year 1902 was postponed until the stated meeting of February.

RICHARD L. BINDER, *Secretary.*

④ JOURNAL

OF THE

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OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

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THE FRANKLIN INSTITUTE.

Annual Meeting, held Wednesday, January 15, 1902.

THE ALASKO-CANADIAN FRONTIER.*

By THOMAS WILLING BALCH.

At the end of May, 1898, the United States and Great Britain agreed to appoint an Anglo-American Joint High Commission to consider and arrange upon a basis more favorable to both sides, such important problems as the regulations of the North Atlantic fisheries, commercial reciprocity and the Behring Sea fishery question. Soon after, "For the first time a statement was presented by the British Government to the Government of the United States on the 1st of August, 1898, developing the fact that a difference of views existed respecting the provisions of the treaty of 1825" between the United States and the English Empire, concerning the meaning of the Alaska frontier as defined in the Anglo-Russian treaty of 1825;[1] and on August 23d the British Government claimed that the eastern boundary of Alaska should run from the extremity of Prince of Wales Island at 54° 40′, along the estuary marked on recent maps as Pearse Canal, up to the top of the Portland Canal, from there straight to the coast and then along the mountains on the mainland nearest to the shore and across all the sinuosities of the sea that advance into the continent up to Mount Saint Elias.[2] (See map, Plate I.)

By the treaty negotiated at Saint Petersburg and signed there on February 16–28, 1825, the Muscovite and the British Empires agreed in Articles III and IV of that treaty upon the following divisional line between their respective North American possessions.[3]

"ARTICLE III.

"The line of demarcation between the possessions of the High Contracting Parties upon the coast of the continent, and the islands of America to the northwest, shall be drawn in the manner following:

"Commencing from the southernmost part of the island called Prince of Wales Island, which point lies in the parallel of 54° 40′ north latitude, and between the 131st and the 133rd degree of west longitude (meridian of Greenwich), the said line shall ascend to the north along the channel called Portland Channel, as far as the point of the continent

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[1] "The Alaskan Boundary," by Hon. John W. Foster: *The National Geographic Magazine*, November, 1899: Washington, p. 453. Mr. Foster, the able author of this article, was Secretary of State, 1892–93, in the Harrison Administration, and has been from the beginning one of the United States members of the Joint High Commission.

In collecting maps on the subject of the Alaskan frontier, I have received kind aid from Mr. P. Lee Phillips, chief of the Map Division of the Library of Congress, and Mr. Tittman and Mr. Andrew Braid, of the United States Coast and Geodetic Survey, at Washington, D. C.

[2] "The Alaskan Boundary," by Hon. John W. Foster: *The National Geographic Magazine*, November, 1899: Washington, p. 455.

[3] "Fur Seal Arbitration." Washington, Government Printing Office 1895; Vol. IV, pp. 42–43.

where it strikes the 56th degree of north latitude; from this last-mentioned point, the line of demarcation shall follow the summit of the mountains situated parallel to the coast, as far as the point of intersection of the 141st degree of west longitude (of the same meridian); and finally, from

Prepared in the Office of the U. S. Coast and Geodetic Survey. Treasury Department.

PLATE I—United States and English boundary claims.

the said point of intersection, the said meridian line of the 141st degree, in its prolongation as far as the Frozen Ocean, shall form the limit between the Russian and British possessions on the continent of America to the northwest.

"ARTICLE IV.

"With reference to the line of demarcation laid down in the preceding Article, it is understood:

"1st. That the island called Prince of Wales Island shall belong wholly to Russia.

"2nd. That wherever the summit of the mountains which extend in a direction parallel to the coast, from the 56th degree of north latitude to the point of intersection of the 141st degree of west longitude, shall prove to be at a distance of more then 10 marine leagues from the ocean, the limit between the British possessions and the line of coast which is to belong to Russia, as above mentioned, shall be formed by a line parallel to the windings [*sinuosités*] of the coast, and which shall never exceed the distance of 10 marine leagues therefrom."[4]

The negotiations that resulted in the treaty of 1825 were originated by an Ukase promulgated in 1821 by Emperor Alexander the First, in which, in addition to claiming exclusive jurisdiction for Russia in the waters of Behring Sea and a large part of the northern part of the Pacific Ocean, he extended also the territorial claims of Russia from the 55°, as claimed by the Ukase of 1799 issued by the Emperor Paul, down to the 51° of north latitude. The United States and Great Britain both protested against the pretensions of sovereignty asserted in the Ukase of 1821. In 1824 the United States and the Russian Governments signed a treaty in which, among other things, they agreed on the parallel of 54° 40′ as the divisional line between their respective territorial claims; all below that line Russia agreed to leave to the United States to contest with Great Britain, and all above it the United States consented to leave to Russia to dispute with England.

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[4] Owing to the importance of the French text, which the British Government in its printed argument in the Bering Sea Seal Fisheries Case (*Fur Seal Arbitration*, Vol. IV, p. 500) recognized as the official version, and the fact that French is the diplomatic language of the world, which was probably much more the case in 1825 than to-day, the French text is given here:

"ARTICLE III.

"La ligne de démarcation entre les possessions des Hautes Parties Contractantes sur la côte du continent et les îles de l'Amérique nord-ouest, sera tracée ainsi qu'il suit:

Meanwhile, the course of negotiations between Russia and England did not progress as smoothly; but finally in February, 1825, nearly a year after the signing of the Russo-American Treaty, the Russian and the English plenipotentiaries signed the treaty containing the two articles above quoted. For more than half a century the British Empire never contested the interpretation openly placed by both the Muscovite and the United States Governments that under those two articles, first Russia and later—after the cession of Russian America or Alaska in 1867 to the American Union—the United States were entitled to a strip of territory (*lisière*) on the mainland from the Portland Channel or Canal in the south up to Mount Saint Elias in the north, so as to cut off absolutely the British possessions from access to the sea above the point of 54° 40′. In August, 1898, for the first time, the British Empire formally claimed at the Quebec Conference that the proper reading of those two articles entitled Canada to the upper part of most or all of the fiords between the Portland Canal and Mount Saint Elias.[5]

"A partir du point le plus méridional de l'île dite Prince of Wales, lequel point se trouve sous le parallèle du 54e degré 40 minutes de latitude nord, et entre le 131e et le 133e degré de longitude ouest (méridien de Greenwich), la dite ligne remontera au nord le long de la passe dite Portland Channel, jusqu'au point de la terre ferme où elle atteint le 56e degré de latitude nord ; de ce dernier point la ligne de démarcation suivra la crête des montagnes situées parallèlement à la côte, jusqu'au point d'intersection du 141e degré de longitude ouest (même méridien) ; et, finalement, du dit point d'intersection, la même ligne méridienne du 141e degré formera, dans son prolongement jusqu'à la Mer Glaciale, la limite entre les possessions Russes et Britanniques sur le continent de l'Amérique nord-ouest.

"ARTICLE IV.

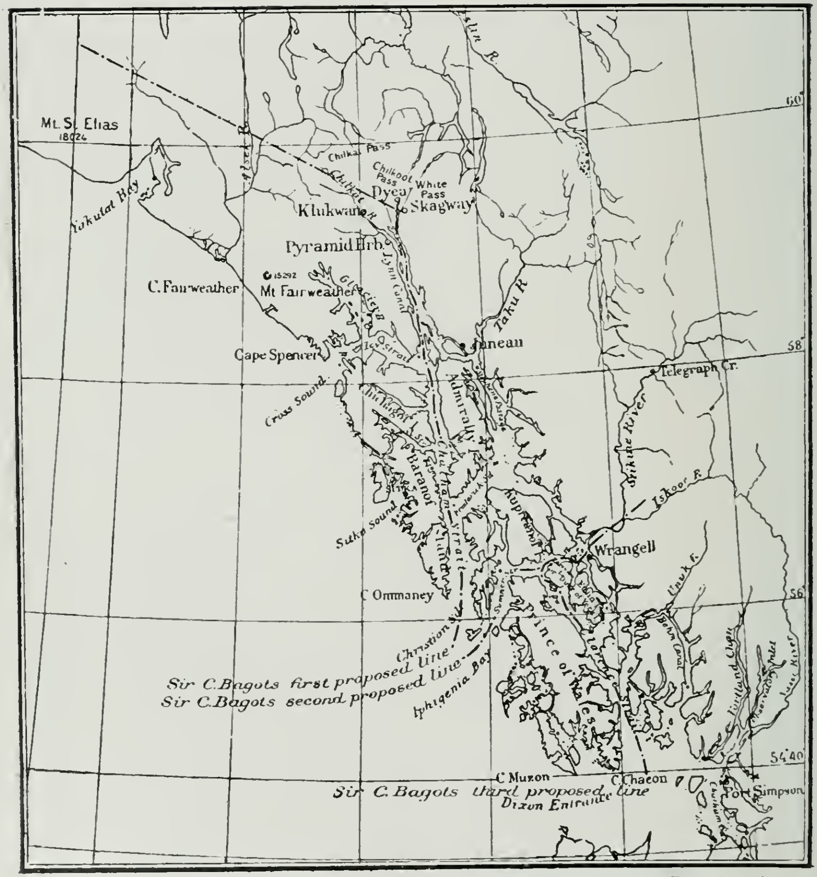
"Il est entendu, par rapport à la ligne de démarcation déterminée dans l'Article précédent :

"1. Que l'île dite Prince of Wales appartiendra toute entière à la Russie.

"2. Que partout où la crête des montagnes qui s'étendent dans une direction parallèle à la côte depuis le 56e degré de latitude nord au point d'intersection du 141e degré de longitude ouest, se trouveroit à la distance de plus de 10 lieues marines de l'océan, la limite entre les possessions Britanniques et la lisière de côte mentionnée ci-dessus comme devant appartenir à la Russie, sera formée par une ligne parallèle aux sinuosités de la côte, et qui ne pourra jamais en être éloignée que de 10 lieues marines."

[5] "The Alaskan Boundary," by Hon. John W. Foster: *The National Geographic Magazine*, November, 1899: Washington, p. 453.

A review of the negotiations during the years 1822, 1823, 1824 and 1825, between Count Nesselrode and M. de Poletica in behalf of Russia, and first of Sir Charles Bagot and afterwards of Mr. Stratford Canning, later Lord Stratford de Redcliffe, for Great Britain, shows clearly that the



Prepared in the Office of the U. S. Coast and Geodetic Survey Treasury Department

PLATE II.—Sir C. Bagot's three proposed boundaries, 1824.

agreement finally reached as embodied in the treaty of 1825 was to exclude the British North American territory from all access to the sea above the point of 54° 40′. From the very inception of the negotiations, the Russians insisted upon the possession for Russia of a strip or *lisière* on the mainland from the Portland Channel up to Mount Saint Elias expressly to shut off England from access to the sea at all points north of the Portland Canal. Sir Charles Bagot, on behalf of England, fought strenuously to keep open a free access to the sea as far north above the line of 54° 40′ as possible (Plate II). First, he proposed that the line of territorial demarcation between the two countries should run "through Chatham Straits to the head of Lynn Canal, thence northwest to the 140th degree of longitude west of Greenwich, and thence along that degree of longitude to the Polar Sea."[6] To this Count Nesselrode and M. de Poletica replied with a *contre-projet* in which they proposed that the frontier line, beginning at the southern end of Prince of Wales Island, should ascend the Portland Canal up to the mountains; that then from that point it should follow the mountains parallel to the sinuosities of the coast up to the 139th degree of longitude west from Greenwich, and then follow that degree of longitude to the north.[7]

At the next conference Sir Charles Bagot gave Count Nesselrode and M. de Poletica a written modification of his first proposition. In this new proposal he first stated that the frontier that they demanded would deprive Great Britain of sovereignty over all the *anses* and small bays that lie between 56° and 54° 40′ of latitude, and that owing to the proximity of these fiords and estuaries to the interior posts of the Hudson's Bay Company, they would be of essential importance to the commerce of that company; while on the other hand, the Russian-American Company had posts neither on the mainland between those degrees of latitude, nor even on the neighboring islands. Sir Charles proposed that the line of separation should pass through "the middle of the canal that separates Prince of Wales Island and Duke of York Island from all the islands situated to the north of the said islands until it [the line] touches the mainland." Then advancing in the same direction to the east for ten marine leagues, the line should then

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[6] "Fur Seal Arbitration," Vol. IV, p. 424.

[7] "Fur Seal Arbitration," Vol. IV, p. 427.

ascend towards the north and northwest, at a distance of ten marine leagues from the shore, following the sinuosities of the coast up to the 140° of longitude west from Greenwich and then up to the north.[8]

At the next conference the Russian plenipotentiaries again insisted upon their original proposal that the frontier line should ascend the Portland Canal and then follow the mountains bordering the coast line.

Sir Charles Bagot then brought forward a third boundary line that, passing up Duke of Clarence Sound and then running from west to east along the straits separating Prince of Wales Island and Duke ot York Island to the north, should then advance to the north and the northwest in the way already proposed.[9]

But again the Russian diplomates insisted on their original proposition. On April 17, 1824, Count Nesselrode addressed to Count Lieven, the Russian Ambassador at London, a long and exhaustive review of the negotiations with Sir Charles Bagot, and instructed Count Lieven to press the Russian views upon the English Cabinet. In that communication, after speaking of Russia's declaration at the beginning of the negotiations that she would not insist upon the claim to the territory down to the 51° put forward in the Ukase of 1821, and that she would be content to maintain the limits assigned to Russian America by the Ukase of 1799, he went on to say "that consequently the line of the 55th degree of north latitude would constitute upon the south the frontier of the States of His Imperial Majesty, that upon the continent and towards the east this frontier could run along the mountains that follow the sinuosities of the coast up to Mount Saint Elias, and that from that point up to the Arctic Ocean we would fix the limits of the respective possessions according to the line of the 140th degree of longitude west from Greenwich.

"In order not to cut Prince of Wales Island, which according to this arrangement should belong to Russia, we proposed to carry the southern frontier of our domains to the

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[8] "Fur Seal Arbitration," Vol. IV, p. 428.

[9] "Fur Seal Arbitration," Vol. IV, p. 430.

54° 40′ of latitude and to make it reach the coast of the continent at the Portland Canal whose mouth opening on the ocean is at the height of Prince of Wales Island, and whose origin is in the lands between the 55° and 56° of latitude."

Russia, by limiting her demands to those set forth in the Ukase of 1799, simply defended claims against which, for over twenty years, neither England nor any other power had ever made a protest. England, on the contrary, sought to establish her right to territory which she had thus passively recognized as Russian, and which lay beyond any of her settlements. Count Nesselrode contrasted the policy of the two states in a pithy sentence: "Thus we wish to retain, and the English Companies wish to acquire."

The negotiators were thus brought face to face with their rival claims. The Russians insisted, on the one hand, that they must have possession of a *lisière* or strip of territory on the mainland in order to support the Russian establishments on the islands and to prevent the Hudson's Bay Company from having access to the sea and forming posts and settlements upon the coast line opposite to the Russian Islands; while Sir Charles Bagot maintained, on the other hand, that Great Britain must have such part of the coast and inlets north of 54° 40′ as would enable the English Companies and the settlements back from the coast to have free access to the fiords and estuaries opening into the ocean.

After a few months, Mr. George Canning, the English Foreign Secretary, instructed Sir Charles Bagot to agree to the Portland Channel as part of the frontier line; but with the reservation, first, that the eastern line of demarcation should be so defined as to guard against any possibility, owing to subsequent geographical discoveries, that it could be drawn at a greater distance from the coast than ten marine leagues, and secondly, that the harbor of Novo-Archangelsk (now Sitka) and the rivers and creeks on the continent should remain open forever to British commerce.

During the course of the new negotiations between Count Nesselrode and M. de Poletica in behalf of Russia, and of Sir Charles Bagot for England, the second of these two points was the main object of discussion. Sir Charles was unable to conclude a treaty with the Russian diplomates, for the latter refused to agree to open forever the port of Novo-Archangelsk to British commerce. Neither were they willing to grant to the subjects of England the right *forever* to navigate and trade along the coast of the *lisière* that it was proposed Russia should have. The British Ambassador, realizing that it was impossible for him to negotiate a treaty in accordance with his instructions, soon thereafter left Saint Petersburg.

In the latter part of the year 1824, Great Britain appointed Mr. Stratford Canning, later Lord Stratford de Redcliffe, one of the ablest of her diplomates, to continue the negotiations left unfinished between Sir Charles Bagot, and Count Nesselrode and M. de Poletica. When Canning took up the negotiations, Great Britain had receded from all contentions except as to the width of the *lisière*. In his instructions he received power to arrange for a line of demarcation that should run along the crest of the mountains, except where the mountains were more than ten marine leagues from the shore, in which case the frontier should follow, at a distance of ten marine leagues inland, the sinuosities of the shore. With these new instructions Stratford Canning was able to conclude a treaty to which Sir Charles Bagot could not have agreed. And on the 16/28 of February, 1825, Stratford Canning on behalf of Great Britain, and Count Nesselrode and M. de Poletica for Russia, signed a treaty definitely dividing Canada and Russian-America.

George Canning, towards the end of his instructions to Stratford Canning, showed what was the chief motive of England in the pending negotiations with Russia. He wrote:

"It remains only in recapitulation to remind you of the origin and principles of this whole negotiation.

"It is *not* on our part essentially a negotiation about limits.

"It is a demand of the repeal of an offensive and unjustifiable arrogation of exclusive jurisdiction over an ocean of unmeasured extent; but a demand qualified and mitigated in its manner, in order that its justice may be acknowledged and satisfied without soreness or humiliation on the part of Russia.

"We negotiate about territory to cover the remonstrance upon principle.

"But any attempt to take undue advantage of this voluntary facility we must oppose."[10]

Thus the chief concern of the English Government was to obtain from that of Russia an official disclaimer of the assertion in the Ukase of 1821 that the waters of Behring Sea and parts of the northern Pacific were exclusively Russian waters. Russia would not assent to formally recognize the right of English ships freely to navigate those seas, unless the boundary question was also arranged and settled so as to insure to Russia an unbroken *lisière* from the Portland Canal up to Mount Saint Elias. And on this last point England, after a long and stubborn resistance, finally yielded.

Much of the trouble that the negotiators of the Anglo-Muscovite treaty of 1825 had in agreeing upon the eastern boundary of the *lisière* was due to a lack of knowledge respecting the mountains along the northwest American coast. According to Vancouver's chart and other available information, a mountain range ran along the coast not far from the sea. When Stratford Canning and Count Nesselrode and M. de Poletica finally agreed upon the mountain divide as the frontier between the two nations, Canning, acting upon instructions from his cousin, George Canning, who was British Secretary of Foreign Affairs, insisted that should the summit of the mountains prove to be at any point more than ten marine leagues from the shore, then the line of demarcation should be drawn parallel to the *sinuosities* of the shore at a distance of ten marine leagues. This ten-league limit to the eastward was inserted on purpose, as George Canning stated in his instructions to Stratford Canning, to guard England against a possibility of having her territory pushed back to the eastward a hundred miles or more from the sea in case the crest of the mountains

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[10] "Fur Seal Arbitration," Vol. IV, p. 448.

was found in reality to lie far back from the coast instead of close to it, as was then supposed.

Thus a review of the negotiations that culminated in the Anglo-Muscovite treaty of 1825 shows clearly that the negotiators of that treaty intended to include within the Russian territory a *lisière* on the mainland, stretching from the Portland Canal in the south up to Mount Saint Elias in the north, and extending between those points far enough inland to exclude the English possessions absolutely from access to the coast line above 54° 40′.

The treaty was drawn in French, and an English copy was also prepared. In the French version, the language of diplomacy,[11] it is said that the inland frontier of the *lisière* shall be a line drawn "parallèle aux *sinuosités* ['windings' in the English version] de la côte." The meaning of the phrase is made absolutely clear by the use of the word *sinuosités*. Littré, who was a member of *l'Académie Française*, defines, in his *Dictionnaire de la Langue Française*, "sinuosités" as meaning: "Qualité de ce qui est sinueux. Cette rivière fait beaucoup de sinuosités. Il allait dans une chaloupe avec deux ingénieurs côtoyer les deux royaumes de Danemark et de Suéde, pour mesurer toutes les sinuosités, Font. Czar Pierre. Les Jeunes Déliens se mêlèrent avec eux (les Anthéniens) pour figurer les sinuosités du labyrinthe de Crète, Barthél. Anach. ch. 76."[12] Webster defines *sinuosity* to mean: "1. The quality of being sinuous, or bending in and out. 2. A series of bends and turns in arches or other irregular figures; a series of windings. 'A line of coast certainly amounting with its *sinuosities* to more than 700 miles.' S. Smith."[13]

Thus the use of the word *sinuosités*, independently of all other evidence, shows that the negotiators of the treaty

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[11] "Fur Seal Arbitration," Vol. IV, p. 500 *et seq.*

"Principes du Droit des Gens," par Alphonse Rivier: Paris, 1896, Vol. II, p. 19.

"Introduction to the Study of International Law," by Theodore D. Woolsey: New York, 1888, fifth edition, p. 270.

[12] Littré, Paris, Hachette et Cie, 1873.

[13] "An American Dictionary of the English Language," revised by Professors Goodrich and Porter of Yale; Springfield, Mass., 1876.

meant to include within the Russian *lisière* the whole of the Lynn Canal and all other fiords above the Portland Canal.

Aside, however, from the manifest intent of the negotiators as thus revealed, the meaning and understanding of both the British and the Russians as to the definite frontier for which they arranged between their respective empires in the treaty of 1825 is conclusively proved; *first*, by the overwhelming multitude of maps of the best cartographers of the various leading powers of the world, including those of England and Canada, in sustaining the boundary always claimed in the beginning by Russia and afterwards by the United States; *secondly*, by the acts of the British and the Canadian authorities until well towards the close of the nineteenth century.

In the year 1825, shortly after the treaty defining the frontier between Russian and British North America became known, A. Brué, one of the leading French cartographers, published at Paris a map entitled: "Carte de l'Amérique Septentrionale; Redigée par A. Brué, Géographe du Roi; Atlas Universel, Pl. 38." On this map Brué drew the boundary of Russian America on the continent from the top of the Portland Canal at the distance of ten marine leagues from tide water round all the sinuosities up to the 141° of longitude, and then along that meridian to the north. Two years later, in 1827, the celebrated Russian Admiral and navigator, A. J. de Krusenstern, published at Saint Petersburg, "par ordre de Sa Majesté Impériale," a "Carte Générale de l'Ocean Pacifique, Hemisphère Boréal." (Plate III). Krusenstern drew on the mainland the frontier of Russian America from the top of the Portland Canal round the sinuosities of the shore at a distance of ten marine leagues from tide water up to the 141° and then northward along that meridian. Along the line of the 141° is inscribed, "Limites des Possessions Russes et Anglaises d'après le Traité de 1825." Two years later, in 1829, there appeared at Saint Petersburg a map of the eastern extremity of Siberia and the northwest coast of America. This map was "No. 58u (b)" of the "Atlas Géographique de l'Empire de Russie," etc., that was prepared by Function-

PLATE III.—Imperial Russian map: "Dressé par M. de Krusenstern, Contre-Amiral * * * publié par ordre de Sa Majesté Impériale. Saint Petersbourg, 1827."

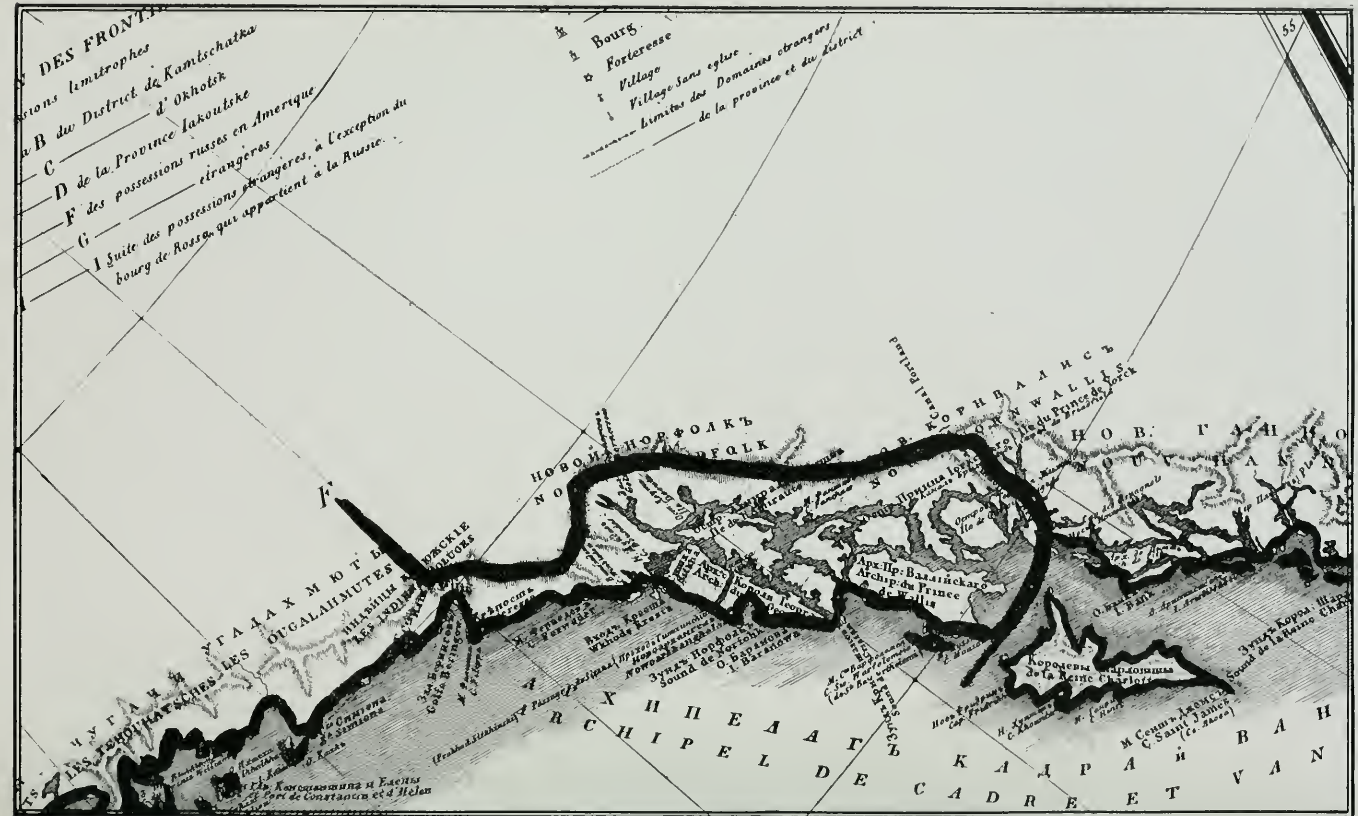

PLATE IV.—"Carte Générale * * * de la côte N. W. de l'Amérique," prepared at St. Petersburg in 1829, by Functionary Piadischeff "au Dépôt Topographique militaire."

ary Piadischeff. On this map, Piadischeff drew the Russo-British frontier from Mount Saint Elias down to the top of the Portland Canal and then along that sinuosity down to the sea at 54° 40′, thereby shutting off Britain from access to the sea above 54° 40′. (Plate IV).[14]

The British Government made no protest against the way Krusenstern and Piadischeff had marked the boundary. On the contrary, a few years later, in 1831, a map was prepared by Joseph Bouchette, Jr., "Deputy Surveyor General of the Province of Lower Canada," and published the same year at London by James Wyld, geographer to the King, and "with His Majesty's most gracious and special permission most humbly and gratefully dedicated * * * to His Most Excellent Majesty King William IVth * * * * * compiled from the latest and most approved astronomical observations, authorities, and recent surveys." (Plate V). It reaffirmed the boundary as given upon Krusenstern's imperial map. Again, in a "Narrative of a Journey Round the World, during the years 1841 and 1842, by Sir George Simpson, Governor-in-chief of the Hudson's Bay Company's Territories in North America," published at London in 1847,[15] a map in Volume I, showing the author's route, gives the line of demarcation between the Russian and the English territories as it was laid down by Krusenstern in his map of 1827. (Plate VI).

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[14] See map, Plate IV. Map "No. 60ᵘ (a)" of the atlas is entitled, "Carte Générale de l'Empire de Russie," etc. This is a map of the whole Russian Empire in 1829, and in the left-hand lower corner the boundary of the Russian-American *lisière* is given as on map "No. 58." Charles Sumner used this general map of the Empire, "No. 60," in preparing his speech in support of the purchase of Alaska in 1867. The copy that he used is now in the Harvard Library. The reproduction of map "No. 58" in this paper was made from a copy of Piadischeff's Atlas, now in possession of the writer, that belonged originally to Prince Alexander of Hesse, a brother of the Empress, Alexander the Second of Russia. The titles and nomenclature of the atlas are given both in Russian and French. The French title is: "Atlas Géographique de l'Empire de Russie, du Royaume de Pologne et du Grand Duché de Finlande * * * * par le Fonctionnaire de la 6ᵉ Classe Piadischeff, employé au Dépot Topographique militaire dans l'Etat-Major de Sa Majesté Impériale: Commencé en 1820 et terminé en 1827, revu et corrigé en 1834.

[15] London: Henry Colburn, 1847; there is a copy of this work in the British Museum.

PLATE V.—Canadian map of 1831: "Compiled * * * by Joseph Bouchette, Jr., Deputy Surveyor General of the Province of Lower Canada."

Ten years later, in 1857, an investigation into the affairs of the Hudson's Bay Company was held by a special committee of the House of Commons. At that investigation, Sir George Simpson, who was examined, presented a map of the territory in question, and, speaking for the Company, said: "There is a margin of coast, marked yellow on the

PLATE VI.—Map in "Narrative of a Journey Round the World," by Sir George Simpson, London, 1847.

map, from 54° 40′ up to Cross Sound which we have rented from the Russian Company." (Plate VII). This map shows that the strip of land on the continent extended far enough inland to include all the sinuosities of the coast so as to exclude, according to the United States claims, the British

PLATE VII.—Map of the Hudson's Bay Company: "Ordered by the House of Commons to be printed 31st July and 11th August, 1857." The Russian territory, which is darker than the Canadian in this reproduction, is colored yellow on the original map.

territory altogether from any outlet upon salt water above 54° 40′.

Again in 1867, about the time of the sale by Russia to the United States of Russian-America—to which William H. Seward gave the name of Alaska[16]—"Black's General Atlas of the World" was published at Edinburgh. In the introduction of this work, the following description of Russian-America is given:

"Russian America comprehends the N. W. portion of the continent, with the adjacent islands, extending from Behring Strait E. to the meridian of Mount St. Elias (about 141° W.), and from that mountain southward along the Maritime chain of hills till it touches the coast about 54° 40′."

Then, on three maps of this atlas, "The World," No. 2, "The World on Mercator's Projection," No. 3, and "North America," No. 39, the Russian territory from Mount Saint Elias down to the end of the Portland Canal at 54° 40′ is marked so as to include within the Muscovite possessions all the fiords and estuaries along the coast and thus cutting off the British territory entirely from all access to tide water above 54° 40′. In addition there is given a small map marked at the top, "Supplementary sketch map, Black's General Atlas, for Plate 41," and at the bottom, "United States after Cession of Russian-America, April 1867, Coloured Blue." On this sketch map the territory purchased by the United States is marked, "Formerly Russian America," and like the rest of the United States it is colored blue. And the boundary of the new territory of Alaska is given as upon the other three maps of this atlas, Nos. 2, 3 and 39, already cited, according to Brué's map of 1825, and Krusenstern's map of 1827, and the Canadian and the English maps already referred to, and in accordance with the territorial claim that Russia and the United States have always maintained and acted upon.

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[16] "Seward at Washington as Senator and Secretary of State," by Frederick W. Seward. New York: 1891, Vol. III, p. 369.

Concerning the sale of Alaska by Russia to the United States, see "Speech of Hon. Charles Sumner, of Massachusetts, on the cession of Russian-America to the United States;" 1867, *passim*, and "The Alabama Arbitration," by Thomas Willing Balch, Philadelphia, 1900, pp. 24–38.

Many other maps can be mentioned, in addition to those above quoted, against Britain's recent claim. For examples, Petermann's map in the *Mittheilungen*, of April, 1869; Thomas Devine's map prepared and printed in 1877 at Toronto by order of the Canadian Government; Alexander Keith Johnston's map of "North America" in his "Handy Royal Atlas of Modern Geography," published at Edinburgh and London, in 1881; E. Andriveau-Goujon's map of "l'Amérique du Nord," published at Paris in 1887; and finally the wall map (1897) of the "United States" by Edward Stanford,[17] an important map-maker of London to-day, give to Alaska the limits always claimed since 1825 by Russia and the United States.

Some maps—for example, "The World," by James Gardner, published in 1825 and dedicated "To His Most Gracious Majesty George the IVth;" "Nord America, Entworfen und gezeichnet von C. F. Weiland," 1826; and a "carte Physique et Politique par A. H. Brué," 1827—bring the Russian boundary on the mainland from Mount Saint Elias down only to a point about half way opposite Prince of Wales Island at about 56°, and then along the estuaries so as to include all of Prince of Wales Island in the Russian Territory, instead of carrying the frontier to the top of the Portland Canal and then down to the sea at about 54° 40′ But for all the territory above the point on the continent about half way opposite Prince of Wales Island up to the 141° west from Greenwich, these maps give the divisional line between the Muscovite and the British territories, far enough inland and around the sinuosities of the coast so as to cut off the British territory from all contact with tide water. Besides, Weiland, in a map of 1843, corrected his error in his map of 1826 in stopping a little short of the Portland Canal in marking the Russo-Canadian boundary; and in Brué's maps of 1833 and 1839 the divisional line is given as it was marked on his map of 1825. Gardner's map is overwhelmed by the multitude of English and Canadian maps—governmental and private—that followed Krusen-

[17] "The United States." London: Published by Edward Stanford, 26 and 27 Cockspur Street, Charing Cross, S. W., July 15, 1897.

stern's delineation of the line of demarcation. And additional proof of how far south the negotiators of the treaty of 1825 intended that the Russian *lisière* should extend when they used the phrase, "la dite ligne remontera au nord le long de la passe dite Portland Channel, jusqu'au point de la terre ferme ou elle atteint le 56[e] degré de latitude nord," is clearly shown by Vancouver's chart, upon which he inscribed the name "Portland Canal."[18]

Probably the most important English map as showing what the best geographers of the British Government thought, until very recently, was the true boundary, is the British "Admiralty Chart No. 787," giving the northwest coast of America from "Cape Corrientes, Mexico to Kadiak Island," prepared in 1876, by F. J. Evans, R. N., published in 1877, and corrected up to April, 1898. On this chart of the British Admiralty, the frontier of the United States descends the 141° of longitude west from Greenwich, and then advancing on the continent, but passing round the sinuosities of the coast so as to give a continuous *lisière* of territory cutting off the Dominion of Canada from all contact with any of the fiords or sinuosities that bulge into the continent between Mount Saint Elias and the Portland Canal, the frontier is drawn to the head of the Portland Canal in about 56°, and then down that sinuosity, striking Dixon's entrance at 54° 40′. *Thus the British Admiralty itself upholds the territorial claims held and maintained by both the Russian and the United States Governments.*[19] (Plate VIII.)

The English and the Canadian Governments, through their official representatives, have again and again recognized the claim of Russia down to 1867, and since then that of the United States, that the area of Russian-America or Alaska comprises an unbroken strip of territory on the con-

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[18] "A Chart shewing part of the Coast of N. W. America with the tracks of His Majesty's Sloop Discovery and Armed Tender Chatham commanded by George Vancouver."

[19] I bought the copy of this chart, from which map Plate VIII is reproduced, at Edward Stanford's, 26 and 27 Cockspur Street, Charing Cross, S. W., London, in September, 1901, showing that up to that date, at least, the British Admiralty agreed with the United States as to the frontier.

PLATE VIII.—British Admiralty Chart, published June 21, 1877, under the superintendence of Captain F. J. Evans, R. N. Hydrographer, and corrected to April, 1898.

tinent, extending from Mount Saint Elias in the north to the Portland Canal in the south; that this strip of land encircles all the sinuosities of the shore; and that by this strip the Dominion of Canada is cut off from all contact with the indentations of the sea along the northwest coast of the continent between the Portland Canal at about 54° 40′ north latitude and Mount Saint Elias. From these numerous official acts, a few are presented here.

In 1857 a "Select Committee" of the House of Commons of the British Parliament was appointed "to consider the state of those British Possessions in North America which are under the Administration of the Hudson's Bay Company, or over which they possess a License to Trade."[20] The committee consisted of nineteen members in all, among whom were Mr. Secretary Labouchere, chairman; Lord John Russell, Lord Stanley, Mr. Edward Ellice, a native of Canada and a director of the Hudson's Bay Company, Mr. Gladstone, Mr. Roebuck, and Sir John Pakington. The committee examined, among others, Sir George Simpson, who for thirty-seven years was the governor of the territories of the Hudson's Bay Company. Part of Sir George Simpson's testimony was as follows:

"1026. Besides your own territory, I think you administer a portion of the territory which belongs to Russia under some arrangement with the Russian Company?—There is a margin of coast marked yellow in the map from 54° 40′ up to Cross Sound, which we have rented from the Russian-American Company for a term of years.

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[20] Parliamentary Papers, 1857

Accounts a — Rep. XV.

Report from the Select Committee on the Hudson's Bay Company together with the proceedings of the Committee, minutes of evidence, Appendix and Index. Ordered, by the House of Commons, to be printed 31 July and 11 August, 1857.

Second Session, 1857.

Veneris, 8° die maii, 1857.

Ordered, That a Select Committee be appointed "to consider the state of those British Possessions in North America which are under the Administration of the Hudson's Bay Company, or over which they possess a License to Trade," (page II.).

"1027. Is that the whole of that strip?—The strip goes to Mount Saint Elias.

"1028. Where does it begin?—Near Fort Simpson, in latitude 54°; it runs up to Mount Saint Elias, which is farther north.

"1029. Is it the whole of that strip which is included between the British territory and the sea?—We have only rented the part between Fort Simpson and Cross Sound.

"1030. What is the date of that arrangement?—That arrangement, I think, was entered into about 1839.

"1031. What are the terms upon which it was made; do you pay a rent for that land?—The British territory runs along inland from the coast about 30 miles; the Russian territory runs along the coast; we have the right of navigation through the rivers to hunt the interior country. A misunderstanding existed upon that point in the first instance; we were about to establish a post upon one of the rivers, which led to very serious difficulties between the Russian-American Company and ourselves; we had a long correspondence, and to guard against the recurrence of these difficulties it was agreed that we should lease this margin of coast, and pay them a rent; the rent, in the first instance, in otters; I think we gave 2,000 otters a year; it is now converted into money; we give, I think, 1,500£ a year."

It will be observed from the foregoing questions and the replies of Sir George Simpson, that the Hudson's Bay Company in 1839, recognized by an official act, to wit, a lease of Russian territory, that Russia had a *lisière* on the continent from Mount Saint Elias almost down to Fort Simpson, and that owing to this strip of land the British territory was pushed back about thirty miles "inland from the coast." In addition it will be noted that Sir George Simpson in describing the positions and extent of the land rented by his company from the Russian Company, referred to a map that he showed the committee, and upon which the *lisière* belonging to Russia was marked so as to include the sinuosities of the coast, the Lynn Canal and all the others fiords above 54° 40′, *entirely*, and so cutting off the

English territory absolutely from all contact with tide water.

Subsequently, in the course of Sir George Simpson's examination, the question of the lease in 1839 by the Hudson's Bay Company of the Russian *lisière* again came up, and the following questions and answers were asked and given:

"1732. Chairman. I think you made an arrangement with the Russian Company by which you hold under a lease a portion of their territory?—Yes.

"1733. I believe that arrangement is that you hold that strip of country which intervenes between your territory and the sea, and that you give them 1500£ a year for it?—Yes.

"1734. What were your objects in making that arrangement?—To prevent difficulties existing between the Russians and ourselves; as a peace offering.

"1735. What was the nature of those difficulties?—We were desirous of passing through their territory, which is inland from the coast about 30 miles. There is a margin of 30 miles of coast belonging to the Russians. We had the right of navigating the rivers falling into the ocean, and of settling the interior country. Difficulties arose between us in regard to the trade of the country, and to remove all those difficulties we agreed to give them an annual allowance. I think, in the first instance, 2,000 otter skins, and afterwards 1500£ a year.

* * * * * * *

"1738. During the late war [the Crimean] which existed between Russia and England, I believe that some arrangement was made between you and the Russians by which you agreed not to molest one another?—Yes, such an arrangement was made.

"1739. By the two companies?—Yes; and Government confirmed the arrangement.

"1740. You agreed that on neither side should there be any molestation or interference with the trade of the different parties?—Yes.

"1741. And I believe that that was strictly observed during the whole war?—Yes.

"1742. Mr. Bell. Which Government confirmed the arrangement, the Russian or the English, or both?—Both Governments."

This additional information shows that the English Company rented the *lisière* from the Russian Company because the *lisière* shut off the English Company from access to the fiords of the sea that advanced into the continent. And further, these questions and replies prove that the English Government—by confirming the agreement of the English Company with the Russian not to interfere with each other while their respective Governments were busy waging war in other parts of the world during the years 1854, 1855 and 1856—recognized and sanctioned the claim of Russia that she had an unbroken *lisière* on the mainland extending far enough inland so as to envelop within her own domains the Lynn Canal and all the fiords that penetrate into the continent above the Portland Canal.

Some twenty years after the investigation by the House of Commons into the affairs of the Hudson's Bay Company, the Canadian Government, through the intermediary of the British Foreign Office, formally recognized that the *lisière* of Alaska shut off Canadian territory from access to the sea.

It was in 1876, while taking a prisoner named Peter Martin, who was condemned in the Cassiar district of British Columbia for some act committed in Canadian territory, from the place where he was convicted to the place where he was to be imprisoned, that Canadian constables crossed with the prisoner the United States territory lying along the Stickine River. They encamped with Martin at a point some thirteen miles up the river from its mouth. There Martin attempted unsuccessfully to escape, and made an assault on an officer. Upon his arrival at Victoria, the capital of British Columbia, he was tried and convicted for his attempted escape and attack upon the constable, and the court sentenced him. The Secretary of State, Hamilton Fish, protested against this infringement of the territorial sovereignty of the United States in the Territory of Alaska. In a letter to Sir Edward Thornton, the English

Minister at Washington, he said: "I have the honor, therefore, to ask again you attention to the subject and to remark that if, as appears admittedly to be the fact, the colonial officers in transporting Martin from the place at which he was convicted to his place of imprisonment, *via* the Stickine River, did conduct him within and through what is the unquestioned territory of the United States, a violation of the sovereignty of the United States has been committed, and the recapture and removal of the prisoner from the jurisdiction of the United States to British soil is an illegal act, violent and forcible act, which cannot justify the subsequent proceedings whereby he has been, is or may be restricted of his liberty."

The transit of the constables with their prisoner, Martin, through American territory was not due to a mistake on their part as to the extent of Canadian territory, for J. B. Lovell, a Canadian Justice of the Peace in the Cassiar district of British Columbia, wrote to Captain Jocelyn in command at Fort Wrangel, saying: "The absence of any jail here (Glenora, Cassiar), or secure place of imprisonment, necessitates sending him through as soon as possible, and I hope you will excuse the liberty we take in forwarding him through United States territory without special permission." After an investigation into the facts of the case, the Dominion Government acknowledged the justness of Secretary Fish's protest by "setting Peter Martin at liberty without further delay," and thus recognized that the Canadian constables who had Martin in their charge when they encamped on the Stickine thirteen miles up from the mouth of the river, were on United States soil, and so that Canada's jurisdiction in that region did not extend to tide water.[21]

Another recognition by the British Empire that the *lisière* restricted Canadian sovereignty from contact with the sea, occurred shortly after the case of Peter Martin.

Owing to a clash between the United States and the

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[21] "Papers relating to the Foreign Relations of the United States." Washington: Government Printing Office, 1877, pp. 266, 267, 271.

Canadian customs officials as to the extent of their respective jurisdiction on the Stickine River, their two governments agreed in 1878 upon a provisional boundary line across that river. The Canadian Government had sent in March, 1877, one of its engineer officers, Joseph Hunter, "to execute," in the language of Sir Edward Thornton to Secretary Evarts, "a survey of a portion of the Stickine River, for the purpose of defining the boundary line where it crosses that river between the Dominion of Canada and the Territory of Alaska." This Canadian engineer, Hunter, after measuring from Rothsay Point, at the mouth of the Stickine River, a distance ten marine leagues inland, determined—in the light of Articles III. and IV. of the Anglo-Russian Treaty of February 16–28, 1825, which two articles he was instructed expressly "by direction of the minister of the interior" to consider in locating the boundary—that the frontier crossed the Stickine at a point about twenty-five miles up the river and almost twenty miles in a straight line from the coast. Without considering whether, owing to the break in the water-shed caused by the passage of the Stickine through the mountains, the United States territory extends inland to the full extent of thirty miles, Hunter decided that the line should cross the river at a point twenty miles back from the coast, but still far enough back from the mouth of the river to shut off Canadian territory from contact in that district with the sea. He came to this decision, because he found that at that point a range of mountains parallel to the coast crossed the Stickine River, and, as he stated expressly in his report to his chief, he acted upon the theory that this mountain range followed the shore line within the meaning of the treaty of 1825 as he understood it. In his report to his government he said: "Having identified Rothsay Point on the coast at the delta of the Stickine River, a monument was erected thereon, from which the survey of the river was commenced, and from which *was* estimated the ten marine leagues referred to in the convention." The Canadian Government sent a copy of this report, together with a map explaining it, through the British Foreign Office to Sir

Edward Thornton at Washington, who communicated it to Secretary William M. Evarts, with the purpose of obtaining his acceptance of this boundary. Mr. Evarts agreed to accept it as a provisional line, but with the reservation that it should not in any way prejudice the rights of the two governments whenever a joint survey was made to determine the frontier. By this voluntary proposal of a provisional boundary across the Stickine River, the British and the Canadian Governments showed that in 1877 and 1878 they considered that Canadian territory above the point of 54° 40′ was restricted by the meaning of Articles III and IV of the Anglo-Muscovite Treaty of 1825 from access to the sea.[22]

The foregoing review of the negotiations that resulted in the treaty of 1825, and the subsequent acts of the nations concerned in the Alasko-Canadian frontier, show clearly that, from the very inception of the negotiations, Russia insisted upon the absolute possession of a continuous, unbroken *lisière* on the continent down to the Portland Channel for the openly expressed purpose of shutting out England from access to the sea above 54° 40′; and that England finally yielded the point.

During Polk's administration (1845–49), when the United States and Great Britain advanced conflicting claims to the territory lying between the Rocky Mountains and the Pacific Ocean, now known as British Columbia, and the supporters of Polk took up the cry of "Fifty-four forty or fight," Russia offered her American possessions to the United States if they would maintain their claim to the territory west of the Rockies up to 54° 40′, the most southern point of Russian-America, thereby shutting out Britain entirely from access to the Pacific Ocean.[23] But owing to the jealousy of the slave power, our government yielded all the country west of the Rockies and above the 49° of north latitude, and

[22] "Papers relating to the Foreign Relations of the United States." Washington: Government Printing Office, 1878, p. 339.

[23] "Papers relating to Foreign Affairs, accompanying the annual message of the President to the second session of the Fortieth Congress," 1867, Part I., Washington: Government Printing Office, 1868, p. 390.

thus permitted the British Empire to obtain an outlet on the Pacific. Not content with this successful territorial extension, the British Empire, after having allowed without a protest for almost three quarters of a century, the inclusion by the Muscovite and the United States Government within their sovereignty—as is shown both by the maps and other official acts of these two nations—of all the sinuosities or fiords along the coast of the mainland above 54° 40′, the British Empire now lays claim, since the discovery of gold in the Klondike, to a large and to us most important part of our Alaskan domain. The American and the British contentions to-day are well expressed by the pithy sentence in which Count Nesselrode over seventy-five years ago contrasted the efforts of Russia and England when they were seeking to agree upon a frontier between their American possessions: "Ainsi nous voulons conserver, et les Compagnies Angloises veulent acquerir." (Thus we wish to retain, and the English Companies wish to acquire.)

Canada wishes, and she has the support of England, to have her claim—that she is entitled to many outlets upon tide water above 54° 40′—submitted to the arbitration of third parties.[24] The United States should never consent to any such arrangement. If such a plan were adopted, and a decision were given altogether against Canada, she would be no worse off than she has been from 1825 to the present day, while anything decided in her favor would be a clear gain to her. This country, on the contrary, cannot by any

[24] A letter by the writer, entitled, "Canada and Alaska," briefly touching on the boundary question, was printed in the New York *Nation*, January 2d, 1902, and the New York *Evening Post*, January 4th. Another letter, also under the same title, written by a gentleman at Ottawa, appeared in the same papers, January 16th and 18th, respectively. Still another letter under the title of "Facts about the Alaskan Boundary" was published in the *Nation* of January 23d and the *Evening Post* of January 27th. This last communication was written by a gentleman in California, evidently a Canadian. The Hon. William H. Dall, of Washington, D. C., followed with a strong letter, "The Alaskan Boundary," in the *Nation*, January 30th, and the *Evening Post*, February 1st. Then another communication by the writer, "Canada and Alaska," was given a place in the *Nation*, February 6th, and the *Evening Post*, February 7th.

possibility obtain more than she now has, viz., that which she purchased from Russia in 1867, and to all of whose rights she succeeded; at the same time the United States can lose heavily. For the inclusion in Canadian territory of only one port, like Pyramid Harbor or Dyea on the Lynn Canal, would greatly lessen for the United States the present and future value of the Alaskan *lisière*. The evidence in the case is overwhelmingly on the side of the United States and shows that they are entitled, by long, uninterrupted occupancy and other rights, to an unbroken strip of land on the continent from Mount Saint Elias down to the Portland Channel. There is no more reason for the United States to allow their right to the possession of this unbroken Alaskan *lisière* to be referred to the decision of foreign judges, than would be the case if the British Empire advanced a claim to sovereignty over the coast of Georgia or the port of Baltimore and proposed that this demand should be referred to the judgment of subjects of third Powers. For if the claim of Canada to Alaskan territory is referred to foreigners for settlement, the United States can gain nothing, while they will incur the risk of losing territory over which the right of sovereignty of Russia and then of the United States runs back unchallenged for more than half a century. If France advanced a claim to the Isle of Wight and then asked England to refer her title to the island to the arbitration of foreigners, would Great Britain consent? And for the English Empire to advance a demand to many outlets upon tide water on the northwest coast of America above 54° 40′ and then ask the United States to submit this claim to the arbitration of the citizens of third Powers, is a similar case. Whether the frontier should pass over a certain mountain top or through a given gorge is a proper subject for settlement by a mutual survey. But by no possibility has Canada any right to territory touching tide water above 54° 40′. The United States should never consent to refer such a proposition to arbitration.

THE FRANKLIN INSTITUTE.

Stated Meeting, held Wednesday, December 18, 1901.

THE EVOLUTION OF FIRE-ARMS AND ORDNANCE AND THEIR RELATION TO ADVANCING CIVILIZATION.

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During the ages prior to the invention of printing and the making of books, the world seemed almost to stand still. It is true that great minds have existed in all ages as they do to-day. They were manifested by superb excellence in music, painting, architecture, and in literature, both poetry and prose; but when one mind was so fortunate as to make its impress upon the world, millions of equally great intellects lived and died without adding to the intelligence, happiness and well-being of mankind.

Great Thinkers in all Ages.—Evidence is abundant that inchoate thoughts and marvelous inventions were evolved and partly, if not wholly, developed during every century in the far-back, almost hidden ages of the past, most of them dying with the mind that conceived, and others recorded only so imperfectly that the greatest ideas were soon neglected and forgotten.

Each new thinker commenced at the foot of the ladder, and, though he reached a high eminence, he died and left no better starting point than he enjoyed when he commenced his work of investigation.

It is true that accidental observation puts thoughts in people's minds which we call inventions. Newton saw an apple fall to the ground, and the thought of gravity was impressed upon him. Another saw the lid of a teapot forced up by the confined steam, and the power thus manifested taught him the principle of the steam-engine.

Gunpowder: How Invented.—It is said that gunpowder was first invented in China and India many centuries before the Christian era. In many localities in those countries the soil is impregnated with nitre. All cooking at that time was by wood fires, and the people lived in tents and huts with earth for their floors. Countless fires made of wood upon ground strongly impregnated with nitre must have existed every day, and when such fires were extinguished, a portion of the wood must have been converted into charcoal, some of which would, of necessity, become mixed with the nitre in the soil. By this means two of the most active ingredients of powder were brought together, and it is very natural that when another fire was kindled on the same spot a flash might follow. This would lead to investigation, and then the manufacture of gunpowder was conceived. Whether this be true or not, we have abundant evidence that the origin of gunpowder and artillery goes far back in the dim ages of the past.

Fire-Arms before Christian Era.—The Hindoo code, compiled long before the Christian era, prohibited the making of war with cannon and guns or any kind of fire-arms. Quintus Curtius informs us that Alexander the Great met with fire weapons in Asia, and Philostratus says Alexander's conquests were arrested by the use of gunpowder.

It is also written that those wise men who lived in cities on the Ganges "overthrew their enemies with tempests and thunderbolts shot from the walls." Julius Africanus mentions shooting powder in the year 275. It was used in the siege of Constantinople in 668; by the Arabs in 690; at Thessalonica, in 904; at the siege of Belgrade, 1073; by the Greeks in naval battles in 1098; by the Arabs against the Iberians in 1147; and at Toulouse, in 1218. It appears to have been generally known throughout civilized Europe as early as 1300, and soon thereafter it made its way into England, where it was manufactured during the reign of Elizabeth, and we learn that a few arms were possessed by the English in 1310, and that they were used at Crecy in 1346.

Guns Used against English, 1327.—Greener says (page 5): "Barbour, in his life of Bruce, informs us that guns were first employed by the English at the battle of Werewater, which was fought in 1327, about forty years after the death of Friar Bacon, and there is no doubt that four guns were used at the battle of Crecy, fought in 1346, when they were supposed to have been quite unknown to the French, and aided in obtaining the victory for British arms. Froissart gives an excellent representation of a cannon and cannoneers in 1390."

Formidable Weapons in Pre-Historic Age.—We have much evidence to prove that far back in the pre-historic age weapons existed of a most formidable character. That they were effective at a long distance is well established, and that the mastodon, the cave bear, and other powerful animals were killed by them is evidence of their deadly nature.

The boomerang and the throwing stick found in the hands of the Australian savage could hardly be improved upon or even equalled at the present time, and it is doubtful if any material improvement was made in weapons of war during the many centuries from the earliest historic age down to the time when fire-arms became generally used as instruments of warfare.

Long-Bow and Cross-Bow were Destructive.—The long-bow and cross-bow were very formidable and destructive weapons. The battles of Poictiers and Agincourt were won almost entirely by soldiers armed with the bow, its length being about the height of a man, and the greater part of the victorious army at Crecy were armed with the same weapon. Marvelous stories come to us as to their range, but probably 600 yards is the longest authentic flight of an arrow from bows of that character, and 400 yards was regarded as an exhibition of great skill.

The cross-bow is of very ancient origin. It is mentioned in the Bible and by Josephus. Pope Innocent III forbade the use of so cruel and barbarous a weapon as the cross-bow against Christians, but sanctioned its use against infidels. Contrary to the orders of the Pope, King Richard I introduced the cross-bow in England, but its use was discouraged by Henry VII, Elizabeth, James, and Charles I, although in 1572 cross-bowmen were sought to engage in the service of Charles IX. The first guns used by soldiers seem to have been called hand-cannon.

Hand-Cannon and Matchlock.—The "hand-cannon," as first used by the French, Italians, and Netherlanders, consisted of a small bombarde affixed to a straight piece of wood, and fired from the shoulder by means of a match.

The first account of the use of hand-cannon in Germany was in 1381, and at the close of the fifteenth century hand-cannon or small fire-arms were in use throughout Europe as military weapons.

The matchlock was in use in 1460, but the opposition to the use of guns, especially hand-guns, was very determined. Armored knights clamored loudly against the use of firearms. Thick armor would generally repel arrows which so often glanced, and knights were seldom killed; but the advent of guns and bullets made the armor of little value as a protection. Gunshot wounds were regarded as certainly mortal, and much opposition to the use of guns was manifested. Small cannon were used at Crecy, their first certain employment on the field of battle.

An old French writer says:

"No use has yet been made in France, in 1547, of that terrible weapon against men. The French used it with good effect against some castles in 1338, but they would blush to employ it against their fellow-creatures. The English, less humane, without doubt outstripped us, and made use of some at the celebrated battle of Crecy, which took place against the troops of King Edward III, of England, who was so spiteful and treacherous that he plagued Philip de Valois and his troops to the last; and the greater part of the terror and confusion was occasioned by the cannon, which the English used for the first time, and had placed upon a knoll near the village of Crecy, and to which the French assign their defeat."

Skill of English Archers.—The skill of the archers of England with the long-bow and the lack of skill with the gun caused many to prefer the bow to the gun as a weapon of warfare.

In the reign of Queen Elizabeth, Sir John Smith, a general of much experience, stated that the bow was the superior of the hand-gun, and although he was taken up sharply by Mr. W. Barwick, Gent., he stuck to his contention. "I will never doubt to adventure my life," he writes, "or as many lives (if I had them) amongst 8,000 archers complete, well chosen and appointed, and therewithal provided and furnished with a great store of cheaves of arrows, as also a good overplus of bows and bow strings, against 20,000 of the best harquebusiers and musketeers there are in Christendom."

Several competitive trials between the gun and the bow are on record, the results generally showing military advantages to the latter. A reliable match decided at Pacton Green, Cumberland, in August, 1792, resulted in a grand victory for the bow. The distance was 100 yards, the bow placing sixteen arrows out of twenty into the target, and the ordinary musket twelve balls only.

Perceiving such results as these so late as the eighteenth century, it is not surprising that in its earlier days the gun proved an inferior weapon to the bow when in the hands of a good archer.

This bad shooting with the gun and its unpopularity was due to the recoil. A few shots so bruised the soldier's arm that his thought was on his shoulder more than his aim, and in spite of himself he would cringe or wince at the moment of firing, and thus deflect the gun.

There is no record of the muskets used at the trials above quoted, but in all probability the "Brown Bess" would be the one chosen, it being the standard military arm at that period.

Opposition to Improved Methods in Military Service.—What is politely called conservatism, in other words, an adherence to ideas or anything else which is old and established, is not as deeply rooted in the minds of people to-day as it was years ago.

Learned men ridiculed the idea of the practical use of steamboats and railroads, and Fernando Wood was ridiculed off the stump when beaten for re-election to Congress, being charged with voting to appropriate $25,000 of the people's money to be used by a crank who claimed that he could hold communication between Washington and Balti more by means of wires stretched between the two places. Officers of the army and navy of all countries have been even more conservative than the mass of the people.

Many writers have dwelt upon the fact that among military men there has always seemed to be an intangible something which caused them to oppose improved methods or improved weapons, no matter how superior they may have been shown to be.

Napoleon III says, in his valuable treatise on the "Past and Future of Artillery:"

"Inventions which are before their age remain useless until the stock of general knowledge comes up to the level." The Emperor also speaks of "being enamored of old ways and of presenting for ages practices that are most stupid. * * * And not only does *routine* scrupulously observe, like some sacred deposit, the errors of antiquity, but it actually opposes, might and main, the most legitimate and the most evident improvements."

An English writer says: "On looking now at the specimens of early small arms and artillery preserved in museums and arsenals, it is surprising how little change has been undergone, either by ordnance or musketry, during that long period of comparative inaction. Except in the superior composition of the metal, cannon cast in the reigns of the Georges exhibited little alteration or improvement beyond their condition in the time of Elizabeth. The muskets borne by our soldiers in the Peninsula and at Waterloo differed in no essential particular from those with which their ancestors fought at Blenheim, August 17, 1704, and Ramilies; and the substitution of the percussion cap for a flint-lock took place at a still later period."

Even Napoleon and his Marshals were Unprogressive in Armament.—Nor was this display of apathy confined to England alone. Almost every other nation in Europe concurred in manifesting the same contented indifference. Marmont, to the close of his life, upheld the old musket as the most formidable and effective of all possible weapons; and Napoleon withdrew the rifle from the imperial troops, to whom it had been partially issued during the wars of the Republic;

nor was it restored to the French armies till after the invasion of Algeria, in 1830, when it was adopted for the equipment of the Chasseurs d'Orleans.

The Duke of Wellington was often heard to say that, "looking to the amount of mechanical skill in England, and the numerical weakness of the English army as compared with those of the great Continental Powers, British troops ought to be the best-armed soldiers in Europe."

While duly aware of the imperfections both of small arms and ordnance, the Duke was equally competent to form a correct estimate of the difficulties which beset their removal; but amongst these he did not for a moment admit the force of military *routine*, which, next to rash innovation, Napoleon III has pronounced to be the most redoubtable enemy to all improvement, regarding errors as sacred only because they are old.

The Duke, so far from being a partisan of "Brown Bess," gave his sanction to its supersession by the Minie-rifle in 1851. But before authorizing the adoption of the rifle, he justified the measure in his own mind, not only by the example of the French and Belgian experiments, but by personal inspection of the ease with which the gun could be managed by the man, and the facility afforded by it for loading. It gave place a few years after to the Enfield; and slight as was the superiority which the Minie, when first introduced, presented over the antiquated musket of the previous century, its partial introduction by the Duke of Wellington was the only improvement that up to that time had been made in the construction of small arms.

An English author in reference to the period (about 1830) said: "Prior to that time, however, some perception of the superiority of the rifle had begun to be felt in Great Britain, after its efficiency had been witnessed in the hands of the Americans, whose marksmen had been indebted to its skilful use for their advantages over ourselves, as well as for subsequent successes in their expeditions against the Mexicans."

While the Duke of Wellington was quite inclined to the conservative idea, he was less extreme in his views than many distinguished officers of that period.

Excellent breech-loading rifles were made in England, and in use when he was a child.

Both Wellington and Napoleon were too well informed not to have known that neither breech-loaders nor revolvers are inventions of modern date. Both were known in Germany as early as the close of the sixteenth century. There are in the Musée d'Artillerie at Paris wheel-lock harquebuses of the sixteenth century which are breech-loaders; and there is, in the Tower Armory, a revolver with the old matchlock, the date of which is about 1550. A German harquebus of the sixteenth century, in the museum of Sigmaringen, is a revolver of seven barrels. Nor is rifling a new thing in fire-arms, for there was a rifled variety of the old harquebus of the sixteenth century, in which the balls were driven home by a mallet, and a patent was taken out in England for rifling in 1635.

Breech-Loaders and Percussion Locks Opposed.—There is now a breech-loading rifle in the War Department library which was invented in 1775 and was used by the British troops at the battle of Brandywine on September 11, 1777, and excellent breech-loaders are now at Fort Monroe which were made by the Chinese possibly a thousand years ago.

The percussion-lock was invented in 1800, and was patented in England in 1807, and yet the man who is held up in England as the superior of Napoleon fought all his wars with the muzzle-loading flint-lock.

Now, all this was bad enough; but in the face of abundant information that breech-loading arms had been before the public throughout the world for centuries, and despite the fact that the percussion-lock was publicly advocated in 1807, and in general use in Europe from 1838, and I might say almost universal use in 1846, and manufactured in our arsenals in 1844 and 1845, 1846 and 1847, our army fought the Mexican War with flint-lock arms, most of them the old musket.

I read very recently the following: "In 1842 percussion-locks were adopted, and when the Mexican War broke out there were enough to have armed the troops, but General Scott preferred the flintlock musket."

I think the writer was in error as to the quantities of guns with percussion-locks in May, 1846, but with energy every soldier of General Scott's army could have been given a gun of that character.

Percussion muskets were manufactured at the Springfield Armory in 1844. The records of the War Department show that during that and the next four years the percussion muskets manufactured by the government were as follows:

1844	2,956
1845	14,332
1846	26,468
1847	26,300
1848	26,017

Infantry in Civil War Armed with Muzzle-Loaders.—But worse than all that, the War of the States came on in 1861, and this great government, with its ports open and unlimited resources, and with inventors of excellent breach-loading arms begging the government to purchase, the old muzzle-loader was kept in use.

These efforts were so persistent that Floyd, a civilian Secretary of War, in his report written during the summer of 1859 in referring to breach-loading arms, said:

"Under the appropriations heretofore made by Congress to encourage experiments in breech-loading arms, very important results have been arrived at. The ingenuity and invention displayed upon the subject are truly surprising, and it is risking little to say that the arm has been nearly, if not entirely, perfected by several of these plans. These arms commend themselves very strongly for their great range and accuracy of fire at long distance; for the rapidity with which they can be fired, and their exemption from injury by exposure to long-continued rains. With the best breech-loading arm, one skilful man would be equal to two, probably three, armed with the ordinary muzzle-loading gun. True policy requires that steps should be taken to introduce these arms gradually into our service, and to this end preparations ought to be made for their manufacture in the public arsenals."—*Report of the Secretary of War, 1859.*

After the War of 1861 began, the government was induced to supply a portion of the cavalry with breech-loaders, and the following is a list of guns of that character purchased and manufactured during the five years ending June 30, 1866, a great number, probably the most of them, being supplied during the last half of the period mentioned, and many even issued after the war had closed. The list is as follows:

1,509 Ballard,	20,002 Maynard,
1,002 Ball,	1,001 Palmer,
55,567 Burnside,	20,000 Remington,
9,342 Cosmopolitan,	80,512 Sharps,
22,728 Gallagher,	30,062 Smith,
1,052 Gibbs,	94,156 Spencer,
3,520 Halls,	25,602 Starr,
11,261 Joslyn,	4,001 Warner,
892 Lindner,	151 Wesson,
14,495 Merrill.	

General Dyer, Chief of Ordinance, in his report for 1864, said: "The use of breech-loading arms in our service has, with few exceptions, been confined to mounted troops."

We see here that the infantry received substantially no breech-loading arms, and it was not until the war was over that the question was "to receive early attention."

Indescribable Opposition to Progress.—Captain Kimball, of the Navy, in an article in *Scribner*, in 1889, in referring to this indescribable something, sarcastically said: "The English bowmen made a gallant stand against the ignominy consequent upon the use of the brutal musket; the French bravely rejected the breech-loader in the Napoleonic wars; the Americans did nobly in refusing to use percussion-lock guns in Mexico, and in greatly preferring wonderfully bad muzzle-loaders to comparatively effective arms with which to kill each other during the Rebellion; but all these heroic attempts at stopping military progress fade into insignificance when one contemplates the glorious resistance to the utilizing of magazine mechanisms upon the rifle."

It would seem difficult to account for these repeated anomalies, but when we reflect that armaments are selected by experienced soldiers, and that they have been indelibly impressed with the excellence of weapons which they have used, and that they realize that any change would require a new course of instruction, and at the same time involve great expense in the change of armament, some idea is conveyed as to the reasons why they desire to adhere to old systems and weapons.

Even in late years, when a hammerless gun was offered to the government, officers of distinction urged as a serious objection that it would be impossible for the soldier to come to support arms. It seemed not to have occurred to them that the manual of arms could be changed and that there was no necessity for ever holding a gun in such a position.

It is said that when Sir Isaac Newton was sitting at his desk near a fire-place, deeply absorbed in an intricate problem and the fire had become so hot as to really give him pain, he cried to his servant to move the fire away. The servant saw that his master's mind was entirely on his work, and as he could not move the fire-place he asked Sir Isaac if it would not do as well for him to move his chair. Sir Isaac's mind reverted for a moment to things terrestrial and said: "I had not thought of that."

Naval Battles at Manila and Santiago Fought with Black Powder.—Smokeless powder was invented prior to 1886 and was in general use in Europe soon after that date.

It was adopted by us, together with the Krag-Jorgensen, in 1892, and yet in June, 1898, every infantry volunteer regiment went to Cuba as an army of invasion armed with the old Springfield rifle with black powder cartridges. The Spanish had smokeless powder and the Mauser-rifle, which many experienced officers regard as better than the Krag-Jorgensen. Even in the far-off Philippines, the insurgents were armed with the Mauser and Remington.

But what is still stronger evidence of this too conservative spirit, notwithstanding that smokeless powder has been used for heavy guns for ten years, black powder, with its clouds of smoke, was the only kind furnished to Dewey at Manila, or to Sampson, Schley and Clark at Santiago, and the "Brooklyn" was so enveloped in a mountain of smoke that officers on adjacent ships only knew that the "Brooklyn" had not gone to the bottom by hearing the thunder of her guns as this noble cruiser hurled tons of iron missiles into the Spanish ships.

Militia Laws, Antiquated and Obsolete.—But should we criticise army officers for this adherence to things that are old, when we are confronted with the fact that the Congress of this progressive country has been even more derelict in this respect than its servants, the officers of the army. During my substantially twenty years' service in Congress, I made several attempts to secure legislation which would modernize the laws regarding that great arm of defence, the "militia of the United States," but owing to the inflexible rules which govern the House of Representatives, all such efforts proved to be unavailing, and to-day the law regarding our militia, which was enacted the 8th day of May, 1792, and amended the 2d day of March, 1803, with all its utter inapplicability to present conditions, is nevertheless now the law of the United States.

The following is, *verbatim*, the present law with regard to arming the militia, as it is solemnly reproduced in the latest copy of the revised statutes of the United States (page 285):

"THE MILITIA."

"SEC. 1625. Every able-bodied male citizen of the respective states, resident therein, who is the age of eighteen years, and under the age of forty-five years, shall be enrolled in the militia.

"SEC. 1626. It shall be the duty of every captain or commanding officer of a company to enroll every such citizen residing within the bounds of his company, and all those who may, from time to time, arrive at the age of eighteen years, or who, being of the age of eighteen years and under the age of forty-five years, come to reside within his bounds.

"SEC. 1627. Each captain or commanding officer shall, without delay, notify every such citizen of his enrollment, by a proper non-commissioned officer of his company, who may prove the notice. And any notice or warning to a citizen enrolled, to attend a company, battalion or regimental muster, which is according to the laws of the state in which it is given for that purpose, shall be deemed a legal notice of his enrollment.

"SEC. 1628. Every citizen shall, after notice of his enrollment, be constantly provided with a good musket or firelock of a bore sufficient for balls of the eighteenth part of a pound, a sufficient bayonet and belt, *two spare flints*, and a knapsack, a pouch with a box therein to contain not less than twenty-four cartridges, suited to the bore of his musket or *firelock*, each cartridge to contain a proper quatity of powder and ball; or with a good rifle, knapsack, shot-pouch and *powder-horn*, twenty balls suited to the bore of his rifle, *and a quarter of a pound of powder;* and shall appear so armed, accoutered and provided when called out to exercise or into service, except that when called out on company days to exercise only, he may appear without a knapsack. And all arms, ammunition and accouterments so provided and required shall be held exempted from all suits, distresses, executions, or sales for debt or for the payment of taxes. Each commissioned officer shall be armed with a sword or hanger and *spontoon*."

It will thus be seen that every member of the militia or state organizations, and as to that matter, practically all citizens of the United States, who are not exempted by law from militia duty, are violators of the law who have not supplied themselves with a musket or firelock, bayonet and belt, two spare flints, a knapsack, cartridges or a good rifle with shot pouch, powder horn, balls, a quarter of a pound of powder and various other things too numerous to mention, and at this time very difficult, if not impossible to obtain, and absolutely worthless for battle, if unfortunately they were obtainable.

As but few of us have ever heard of a "spontoon," I will explain that it means a pike or a halberd or a long-handled weapon.

American Officers Now Show Progressive and Commendable Spirit.—I am glad to say that American officers of this period are showing a most commendable spirit, and this struggle for improvement is growing stronger every day; but while in many things we lead the world, we have up to this time kept a little behind in weapons of war.

That this condition will no longer exist there is abundant evidence. The "Bureau of Ordnance" and the "Board of Ordnance and Fortifications," both of which are composed of very able and distinguished officers, are giving most intelligent investigation to new inventions, and every possible encouragement to inventors who have anything of real merit to present.

What is true of the army is equally true of the navy. Both services seek to do full justice to inventions by civilians, and this wise and intelligent action on their part will prove of great benefit to the army and navy.

Non-Recoil and Automatic Guns.—For several years inventors have devoted much effort towards contriving methods by which recoils of guns could be lessened, and, if possible, entirely neutralized. The advantage of this would be quite apparent to sportsmen as well as to soldiers.

After several shots with ordinary guns the recoil so bruises the arm that the soldier's mind is largely distracted from his duty by the anticipated shock, and almost unconsciously cringes as he pulls the trigger, thus very materially disturbing the aim.

A gun without any recoil, and which could be fired several times without being taken from the shoulder would, therefore, have an advantage over the old arm which, it is no exaggeration to say, would double the efficiency of the soldier.

The effect of the operation of such a gun would be somewhat like the column of water from a hose-pipe. The eye of the fireman is constantly upon the point he wishes to strike, and by the slight movement of the nozzle he changes the direction until the flow of water is upon the desired point. If the fireman were compelled to look down upon the pipe every other moment, he would be constantly losing his bearing, and would be obliged to regain it every time he looked up, and would be constantly commencing anew.

So with this new fire-arm, the soldier's eye being constantly upon the position of the enemy, and very often by the cutting of twigs, or throwing up dust, he can tell how far he has shot to the right or left, or if too low, and like the fireman with the hose, by the slightest change of his aim, he will be better able to hit in the succeeding shots, while the chance would be much less if the gun was taken from the shoulder and the eye taken off the enemy after each shot.

To Neutralize Recoil of Cannon Very Advantageous.—Also, a device which would neutralize the recoil in cannon would be even a greater advantage than in small arms. Especially would this be true in the navy, where the recoil of heavy guns proves very injurious to a ship. Thus, the injury to the "Iowa," "Texas" and "Brooklyn" in firing during the Santiago campaign was such that much time and money had to be spent to repair the damage.

The devices heretofore existing to neutralize the recoil of heavy guns have involved the use of weights, many of them as heavy as fifteen tons and some exceeding thirty tons. It will therefore readily be understood that an invention by which the recoil if neutralized by the energy of the expansion of the same gas as is used to throw the projectile should be highly appreciated.

Greener says, page 337: "The recoil of an ordinary 12-bore gun, loaded at the muzzle, varies from 40 to 48 pounds, seldom exceeding the latter; that of a breech-loader varies from 68 to 76."

Experiments with the 45 caliber Springfield rifle showed a free recoil of 19·84 foot-pounds energy for black or ordinary gunpowder, while experiments with smokeless powder give a recoil of 17·06 foot-pounds. In this experiment a 500-grain bullet was used and the muzzle velocity was 1,259 feet per second.

Other experiments have shown that the ratio of recoil energies of black and smokeless powders have been found for general practical purposes as 1·18 to 1.

The recoil of a gun measured in pounds means that the gun in recoiling compresses a spring by the same amount that it would have been compressed if a dead load of the same number of pounds had been placed on the spring in a vertical position.

Captain Dunn, of the Ordnance Department, in discussing this subject, uses these words: "It has been a generally accepted opinion that the further development of power in the rifle must be attended by increased recoil and that the latter was already equal to the average soldier's endurance."

Importance of Skilful Firing.—There is one and but one purpose in engaging in battle, and that is to gain the victory over the enemy.

There is one and only one purpose in placing arms in the hands of the soldiers composing the army, and that is to enable him to shoot, and either kill or disable soldiers of the opposing army, and the construction and use of fire-arms should all be of a character to best attain that purpose, while at the same time the soldier is as far as possible screened from the enemy's fire.

The tendency of soldiers has been to fire so high that fully 95 per cent. of their charges were ineffective.

During the battles of the Civil War, which occurred in a wooded country, the tops of the highest trees were cut to pieces, and in all engagements at night or even in twilight, it was very plain to see that most of the guns were fired at too great an elevation. This was due in a great measure to the fact that from three to five times as much time was occupied in loading as in firing, and the attention of the soldier was in that way largely diverted from the one essential purpose for which he had been put in action.

All this was true when the main line of the army was engaged, but its importance became much intensified in the case of soldiers firing upon the skirmish line. During the Civil War, on both sides much attention was paid to teaching the skirmish line to never fire until an enemy was visible against whom they could direct a shot.

Now, substantially all battles are fought in open order, and all things being equal, the successful army will be the one whose soldiers are so armed and drilled that their entire attention and thought can be directed upon the one main object of their presence on the field.

This cannot be done with any gun which requires the soldier after firing to take his gun from his shoulder, take his eyes off the enemy, throw out the shell, place a cartridge in position and then return the gun to the shoulder ready for firing. The very fact of his doing all this while under fire tends to create a nervousness which would not exist if his eyes and attention had been constantly upon the enemy.

The tendency in the battles of the present day is to fight at a very long range, and especially is this true when armies are forming and making preparations for an attack. The skirmishers are generally lying down, and with the shelter of grass or a small bush or a log or stone can very generally be so concealed that it is difficult for the enemy to distinguish them from something inanimate.

Officers who have served on the skirmish line have often found soldiers insisting that certain objects before them were the enemy's skirmishers, when careful observation would show that the object referred to was a log or stump or something of that character.

Again, they have often seen the soldier on the enemy's skirmish line remain so quiet that it took careful observation to discern a movement by which his real character could be determined, and this movement was very generally the movement of the arm necessary to prepare the gun for firing.

Non-Recoil Semi-Automatic Gun is Essential.—It will be easily understood what a great advantage it would be for a soldier to have a gun which could be fired repeatedly without, or substantially without, any movements of the arms or hands, thus enabling him to keep his eyes constantly upon the object against which he was directing his fire.

During campaigns it is often the case that both armies place their advance lines in entrenchments, and each side is constantly watching an opportunity to fire when the opposing soldiers show their heads or shoulders above the breastworks.

In such case it would be a great advantage to have a gun which could be fired with accuracy several times without taking the eye from the enemy and without removing the gun from the shoulder. He would have his eye constantly upon the enemy's parapet, and every exposure above the parapet would enable him to make an effective shot.

Officers have often heard the idea advanced that in order to enable a soldier to fire deliberately it is necessary for him to take his gun from his shoulder after firing, and go through the several motions which are necessary in most modern guns to throw out the shell and place another cartridge in position. I think this is a great mistake. There is now no question but that if a soldier is saved this interruption he can, with proper training, be made to fire with more deadly deliberation and with much more accuracy.

Marksmanship little Considered up to Thirty Years Ago.—Until within the last thirty years very little attention was paid to marksmanship.

Lord Roberts said that when he first entered the army, with the exception of a few scattered skirmishers, opposing lines had to approach to within some eighty yards of each other before they could open fire with any chance of hitting one another. There was then much to be urged in support of the theory that marksmanship was but a small matter in comparison with drill. Lord Roberts also remarked that it frequently happened in our conservative army (the English army) that theory long outlived the conditions which had given it birth, and as lately as twenty years ago there were many commanding officers who looked upon target practice as an unnecessary nuisance forced upon them by the authorities to the detriment of drill, and therefore of service efficiency.

Range of Old Musket and Bad Marksmanship.—The range of the old musket was very uncertain, and many writers of that period contended, that although officially said to be effective at a range of 200 yards, it was the working rule of the soldier to reserve his shot *till he saw the whites of his enemy's eyes*, and even then it was said that before he could bring down his man he must fire a full weight of his body in lead.

Less than forty years ago, a well-trained marksman, provided with an old regulation musket, was placed to fire at a target 18 feet square, from a distance of 300 yards, and found that he could not even put into that spacious area *one bullet out of twenty*. At 200 yards his success was no greater, and yet, the fire-arm thus tested, was the regular weapon of the British soldier, so late as the year 1852.

In 1838, a series of experiments were undertaken by the officers of the Royal Engineers, at Chatham. The target first employed was 3 feet wide and 11 feet 6 inches high, and was struck by about three-fourths of the balls at 150 yards, fired with full charges—with reduced charges only about one-half hit. About this distance the difficulty of hitting was so great that the width of the target had to be increased to 6 feet; and, at 250 yards, of ten shots fired with full charges, not one hit the target; at 300 yards, shot after shot was fired without one hitting the object aimed at or its whereabouts being ascertained.

The amount of ammunition fired from large guns without effect has been very great.

In siege operations and in naval warfare the ammunition wasted is far in excess of the injury done to the enemy; so much so, that after the fall of Sebastopol, the surface of the plateau in the vicinity of the captured citadel was strewn with balls as though with the products of a mine of ready-wrought iron.

An officer who saw this, wrote: "I hardly exaggerate in saying that over some dozen acres a smart fellow might have crossed every yard of ground upon iron without laying a foot on the earth. The cannon balls were *thick* in some places, and scattered in others, but the coup d'oeil was indescribable."

Americans the Best Marksmen.—On the other hand, the accuracy of fire by American riflemen has often attracted attention, instances being given in which ten consecutive shots at 220 yards were planted within a space no larger than a small playing card—a feat which up to that period was considered unequaled.

It is, I think, generally admitted that the victory of Jackson, at New Orleans, was largely due to the accurate fire of what were called "backwoods riflemen." Jackson's entire army, including himself, were contemptuously alluded to by the British as men of that character as distinguished from trained soldiers. In fact, Packenham himself, while dying, expressed chagrin that he should lose his life in battle with men so different from the trained soldiers he commanded. The same soldiers that fought in New Orleans, on January 8, 1815, fought under Wellington five months and ten days later at Waterloo. In that battle the French and English armies were at first nearly 1,200 yards apart, but as musketry, as well as grape shot, were at that time unavailing at such a range, the army under Napoleon approached within 200 or 300 yards of the English lines without suffering serious loss. In battles fought before that time, it was the practice of opposing forces to array themselves at distances varying from 500 to 1,000 and sometimes 1,500 yards.

"Brown Bess," the British gun from 1734 to 1839, weighed 11½ pounds and was 75 caliber. It was then changed to percussion lock, but was not much improved, as a report of British Engineers in 1846 uses these words:

"It appears from their experiments that as a general rule musketry fire should never be opened beyond 150 yards, and certainly not beyond 200 yards; at this distance half the number of shots missed the target, 11 x 6 feet.

This is all changed by the long range and accuracy of fire at the present time.

At the battle of Las Guasunas the American line was formed about 750 yards from the Spanish entrenchment. This approximation was made necessary on account of the formation of the ground. For the same reason the American line was formed at San Juan from 500 to 700 yards from the enemy's entrenchments, which were on the San Juan bridge, at which place the Spaniards were well entrenched. It may generally be assumed that where the country is fairly open and bare of trees lines will be formed a little beyond the range of fire-arms.

The Gun of the Future.—The future gun will be automatic or semi-automatic, worked by the force generated by the expansion of the same gas which causes the flight of the projectile, and the same gas will apply and exercise a force pushing the gun forward just sufficient to neutralize the recoil. Such a gun, beside possessing the advantages before mentioned, can be made very light, and that means much, especially for artillery.

A gun embodying this important feature has been devised by Mr. Samuel N. McClean, and signalizes an advance in arms more important and of greater value than anything which has been invented for a third of a century.

The old rifle, as is well known, was made very heavy in order to lessen the recoil, and for the same reason much metal was put in cannon that with the McClean invention, can be eliminated.

A navy 12-inch gun weighs about fifty-three tons, and its carriage about twenty-seven tons.

An army 12-inch disappearing carriage weighs about 500,000 pounds, the counterweights weighing 150,000 pounds.

If the McClean system could be applied to attain the purpose that is now effected by the disappearing carriage an immense amount of ponderous weights could be eliminated.

PROGRESS OF IMPROVEMENTS IN WEAPONS.

Weapons of warfare and for the chase have been of two distinct types.

First: swords, javelins and battle-axes to be held in the hand; and

Second, weapons by or with which missiles are hurled or projected.

The various stages in the latter type of weapons may be stated as follows:

(1) The primitive use of stones and sticks thrown by the hand as a means of attack or defense.

(2) The sling by which stones were hurled with accuracy for long distances.

(3) The spear thrown with skill and force.

(4) The long-bow.

(5) The cross-bow, the principle of which was applied to large guns called artillery as well as to weapons held in the hand. During the ages when these various improvements were being made, certain deviations from those most commonly used, such as the boomerang, were very skilfully made and used by the people of savage nations, and, in some localities, especially in Assyria, India and China, inventions were developed far in advance of the period when they became generally known to the world.

(6) The most important change was early in the fourteenth century, viz.: The use of elastic gases to drive a projectile from a long tube which was called a gun, the elastic gases being created by the explosion of gunpowder. At first the guns were fired by igniting the powder with a torch held in the hand.

(7) The matchlock.
(8) The wheel-lock.
(9) The more modern flint-lock.
(10) The percussion lock.
(11) The rifle-bore.
(12) The breech-loader.
(13) The magazine gun.
(14) The percussion tape.
(15) The hammerless gun and firing-pin.
(16) The use of a contrivance by which, with a movement of the hand after each shot, the empty shell was thrown out and the new cartridge placed in position. Each of the above were deemed to be marked advances upon preceding methods.

(17) The McClean invention, by the use of which, after the elastic gas has performed its work in driving the projectile, a portion is captured and used to convert the ordinary rifle or field-gun or siege-piece into a machine gun, in which a section of the barrel is made to perform the function of the steam-chest of the steam-engine, and the bullet is made to become both the valve and the cut-off, and the explosive force of the gas is made to press the gun forward the moment the projectile leaves the muzzle, just sufficient to counteract the recoil, while at the same time it operates a wonderfully simple mechanism, throws out the empty shell, instantly reloads the piece, and if desired, repeats the operation in an inconceivably short time until the magazine of the piece is emptied.

The McClean mechanism and inventions apply with equal utility to all classes of fire-arms and ordnance from the smallest pistol, military rifle and sporting arm, to field-guns, siege-guns and to naval guns of largest caliber.

In speaking of the McClean inventions, an eminent military engineer, of almost fifty years' devotion to his profession, said:

"The work of resisting the recoil and reloading the piece, especially in the case of small arms, becomes a serious tax upon the strength of the 'man behind the gun,' and its performance by the surplus energy of the gas is a most important aid, whether employed by soldier or sportsman. The effect of all this upon the general accuracy of fire can hardly fail to be beneficial. The use of this device in artillery would tend to reduce the weight necessary for carriages, and thus add greatly to the facility and rapidity of transportation upon the field."

When we consider the simplicity of mechanism, the great durability of the parts and the few parts involved, I do not think it is going too far to say that the McClean inventions mark an advance in fire-arms more useful and valuable than anything that has ever preceded them.

In all engagements in which I have ever been a participant, the armament of the soldiers associated with me, unfortunately, has been inferior to that of the opposing forces.

In Cuba and in the Philippines our troops came nearest to enjoying an equality in that respect. There we had the Krag-Jorgensen and the Springfield rifles. We were opposed in Cuba by the Mauser rifle, and while there is a difference of opinion among officers, I am inclined to regard the Mauser as possessing certain special advantages.

In the Philippines, most of the insurgent fighting force were also armed with the Mauser, though many were armed with the Remington and a few with inferior guns of various description.

These experiences very forcibly impressed me with the paramount importance of our government, by the most searching and thorough investigations and experiments, seeing to it that beyond question of doubt our superb soldiers be furnished with the best possible weapons. This was the main incentive which caused me to become interested in the McClean inventions. My study of this subject convinces me that warfare in the future will be conducted upon scientific principles. It will become an exact science, and we must have scientifically constructed weapons.

Naval Marksmanship.—A great deal has been said in commending the action of our naval officials in what is called lavish expenditure of money for target practice on our naval ships, and the victories at Manila and Santiago were attributed in a great measure to the vastly superior skill in marksmanship of our cannoneers over those of the Spanish navy.

While this is all true, the facts regarding our marksmanship on those occasions show that with improved guns much better results could be obtained. The report of Admiral O'Neil, Chief of the Bureau of Ordnance of the Navy, dated October 1, 1901 (page 35) says:

"If the lessons of the naval battle of Santiago de Cuba, the 'Bellisle' experiment off the Isle of Wight, the 'Scorpion' experiment at Bermuda, and the recent record of target practice of the North Atlantic fleet teach anything, it is that very few hits are made with large caliber guns, even under the most favorable conditions. At Santiago de Cuba, out of eighty-six shots fired from 12- and 13-inch guns, at distances not exceeding 3,500 yards, but three hits were recorded. In the 'Bellisle' experiments but five hits out of fifteen shots fired from 12-inch guns were recorded, the distance being from 1,300 to 1,700 yards.

"In the North Atlantic squadron, out of twenty-six 13-inch shots fired, none hit the target, which was of large dimensions, the distances being from 2,000 to 4,000 yards.

"At the battle of Santiago de Cuba, 319 8-inch projectiles were fired by the United States vessels, the number of guns engaged being presumably eighteen, the number of hits recorded being thirteen, or less than one per gun, the average number of shots per gun being eighteen."

A more detailed statement regarding the ammunition expended and hits in these naval actions is in the record of the Navy Department, and was kindly furnished me by Admiral O'Neil.

His letter is as follows:

"December 12, 1901.

"*My dear General Wheeler:*

"I enclose memorandum of ammunition expended by the United States vessels at Santiago de Cuba and at Manila, with memorandum showing the number of hits recorded by a board which examined the wrecks of the Spanish ships after the action at Santiago.

"As the vessels were sunk at Manila, no record of hits could be made at that place. Yours truly,

"CHARLES O'NEIL."

HITS AT SANTIAGO.

Oquendo:

1	12 or 13-inch.	
5	8-inch.	
11	4, 5 or 6-inch.	 61
44	6-pounders.	

Viscaya:

5	8-inch.	
10	4, 5 or 6-inch.	 28
13	6-pounders.	

Maria Teresa:

2	12 or 13-inch.	
3	8-inch.	
5	4, 5 or 6-inch.	 29
17	6-pounders.	
2	1-pounders.	

Colon:

3	4, 5 or 6-inch.	 6
3	6-pounders.	

Total 124

AMMUNITION EXPENDED IN THE NAVAL ACTIONS OFF SANTIAGO AND MANILA.

Santiago.	13-inch.	12-inch.	8-inch.	6-inch.	5-inch.	4-inch.	6-pdr.	3-pdr.	1-pdr.
								47 mm.	37 mm.
Indiana	13		61	33			1,744		25
Oregon	34		123	41			1,561		141
Texas		8		97			400		330
Brooklyn			100		473		1,200		200
Iowa		31	35			251	1,056		100
Gloucester							589	780	
Vixen							27		18
	47	39	319	171	473	251	6,580	780	814
Manila.									
Olympia			36		281		1,000		360
Concord				182			220	120	60
Petrel				116				313	
Raleigh				53	341			137	100
Boston			48	162			220	256	420
Baltimore			73	122			547		692
			157	635	622		1,978	826	1,632

At Santiago, 1,300 large caliber and 8,174 small caliber, in all 9,474 shots, out of which 124 hits were recorded, less than one hit out of seventy-six shots. This result was applauded by naval officers as well as the people at large, both in Europe and America. Now, if one hit out of seventy-six shots is applauded as good marksmanship, one can conceive of the fearful waste of ammunition which has heretofore been tolerated.

About One Out of 2,000 Shots Effective with Recoil Gun.—To persons who criticise this ratio as bad marksmanship, I would recall that military writers have asserted that since the use of fire-arms in battle it has taken, upon an average, enough metal in the form of shot to equal the weight of a man to secure one effective shot. This would mean about one effective shot out of 2,000.

Such criminal waste of ammunition seems almost incredible, and this very unbusinesslike condition is due largely to the kind of gun which has been used. The recoil has had much to do with the inaccuracy; and the necessity for taking the gun from the shoulder, the eye off the object, and the mind off the purpose, is responsible for much of the balance.

In the future, war must be conducted upon business principles. The use of a non-recoil, semi-automatic rifle, and thorough drill and instruction of the soldier, will entirely change this very unsatisfactory condition. The effort should be to so train an army that the missing of a shot would be the exception and not the rule, as it has been with the old recoil gun. We may not reach such a perfect degree of excellence, but we can and certainly will approach it.

I have seen the McClean one-pounder cannon fired fifteen times into a bank without re-aiming, and every shot went into the same hole. It was mounted upon light wheels without lock, rested loosely on the trunions, and there was not a particle of recoil. It was this which secured such accuracy.

Science and Skill to Win Future Victories.—Science and scientific skill now enter into every civil vocation. What has heretofore been accomplished by the exercise of manual strength is now done with a thousand or more times the ease with which it was formerly done by human effort. To-day, one man by the control of steam or electric power does work which a few years ago required the combined strength and exertion of a thousand or more human beings.

The nations which will win victories in the future will be those who use the most skill in the application of scientific methods and scientifically made arms, ordnance and other machines of war.

The lessons taught by the campaigns of Alexander, Hannibal, Cæsar and Napoleon will not be discarded or minimized, but the victorious generals of the future will still study and adhere to the essence of the principles taught by the great commanders of all ages, intelligently applying them to modern conditions, modern methods, modern weapons and modern science.

Celerity of movement, and strategy and tactics, by which the plan of campaign and plan of battle will be nearly accomplished before the enemy can discern the real purpose of the assailing enemy; the concentration of force at the point of battle, the protection of flanks and lines of communication, and assailing those of the enemy, and much else that was important to victory in the past, will be equally important in the future.

In the days of Greece and Rome much was attained by individual strength and individual prowess, and throwing masses upon the enemy; and it was much the same in the knighthood days of the Middle Ages; and Napoleon owed many a victory to his skilful concentration of masses of soldiery. Now it will be concentration and accuracy of fire. The training and drill of the soldier will be carried to a high standard of perfection, and he will be taught that every shot must be well directed and count for a purpose. With such soldiers, armed with the best guns and with skilful generals, victory will be certain.

The A, B and C of warfare is to know all about your own army, all about that of the enemy, and all about the country, and also to know when it is best to use cavalry, when best to use artillery, and when best to rely upon infantry, and when to use two or even three of these arms at the same time.

Government Should Give Soldier the Best Arms.—Every government should seek to provide those whom they send forward to battle with the best possible weapons for defense or for attack. To fail in this is a crime of the first magnitude. Especially is it a crime in a country like ours, where the best blood of the land always hastens to the country's standard when its prestige, safety or honor is imperiled. All good citizens desire peace with all the world; but there is no truer saying than that to preserve peace is to be always prepared for war.

We do not need and should not have a large army, but the army and navy we have should be the best armed and equipped and instructed of any on earth.

Every invention of war weapons should be thoroughly tested by skilful officers, and Congress should appropriate liberally for this purpose, and we should see to it that no nation has an armament in any respect superior to our own.

ELECTROLYTIC SEPARATION OF TIN.

A curious parallelism is presented between the well-known Hoepfner process for the extraction of copper from its ores and compounds, and a method just patented to Paul Bergsoe, of Copenhagen, for the electrolytic recovery of tin from scrap and waste alloys. The Hoepfner process, it will be recalled, depends upon the varying valence of copper, and consists in bringing a salt of copper in its higher state of oxidation into contact with the ore, whereupon copper passes into solution and the solvent is reduced from the cupric to the cuprous condition; this solution is then electrolyzed with insoluble anodes to deposit one-half of its metal, restoring the remainder to its original valence and reconstituting the solvent. Bergsoe reacts upon tin-bearing materials with stannic chloride, and subjects the stannous salts formed to electrolytic treatment as above, restoring its valence and solvent power, and recovering an amount of metal equivalent to that dissolved. Both methods are simple, and indeed identical in theory. The Hoepfner process has encountered in practice the very serious obstacle of a low-reaction velocity—a solvent action so slow as to render its application to the most commonly occurring ores of copper, the sulphides, of doubtful practicability. From this defect the new process is free, for the stannic salts are energetic solvents. The successful treatment of tin scrap, however, has proven in the past a difficult problem, not only on account of its very low tin content, but because of the tendency of the iron to pass with the tin into solution. As applied to this purpose, therefore, the industrial value of the new process is yet to be demonstrated.—*Electrical World.*

Mining and Metallurgical Section.

Stated Meeting, held Wednesday, November 13, 1901.

UPON THE CONSTITUTION OF BINARY ALLOYS.

By John Alexander Mathews, Ph.D.

Continued from vol. cliii, p. 140.

DISCUSSION.

Mr. A. E. Outerbridge, Jr—I have been much interested in Dr. Mathew's paper, and I wish to ask whether the subject of "segregation" in binary alloys, such as silver and copper, has been studied by these new methods of scientific research. I am led to make this inquiry, having noticed a striking difference in the cooling curves shown in the diagrams of two alloys, one only of which segregates; in these diagrams the cooling curve of an alloy of silver and copper and the cooling curve of an alloy of silver and gold are shown in juxtaposition. The silver-copper curve is very pronounced, and terminates sharply at the point where the eutectic alloy separates; the silver-gold alloy, on the other hand, shows a nearly straight line, and no eutectic. It is well known that alloys of silver and copper segregate while alloys of silver and gold are stable.

More than twenty years ago, long before the modern science of micro-metallurgy had been developed, I had occasion to make an investigation of the rather obscure phenomenon known as segregation of the silver-copper alloys used in coinage, and, incidentally, of the alloys of silver and gold, and copper and gold, in which segregation is almost *nil.*

The standard alloy used for the silver coin of the United States is composed of 900 parts of silver and 100 parts of copper, while the standard for the gold coins is 900 parts of gold and 100 parts of copper.* The gold-copper alloy, when

* A trace of silver is permitted in this alloy.

once formed by melting and thorough mixing, remains practically homogeneous, so that when the metal is cast into ingots and rolled into strips, from which the planchets or blanks are stamped out, all of the pieces are found to be of nearly uniform fineness. Not so, however, in the case of the silver-copper alloy. No matter how carefully the metallic mixture may have been prepared, or how thoroughly the molten metal may have been stirred, it will be found that segregation will always occur, even to such an extent that it is impossible to obtain a fair sample for assay by cutting off a little piece from an ingot, as is commonly done with the gold-copper alloy. The silver assay sample is, therefore, dipped out from the molten silver-copper alloy and poured in a fine stream into cold water, thus cooling the metal instantly in the form of small shot or "granules"; in this way segregation in the sample is avoided, but it has never been overcome in the ingot. It is for this reason that the coinage laws permit a much greater limit of "tolerance" or variation from true standard, in the case of silver coins, than in the case of gold coins; nevertheless, I believe it has actually happened that silver dollar coins struck from metal which originally assayed of the proper fineness have been condemned because the variation in fineness due to segregation in the ingot exceeded the limits allowed by law.

The true cause of segregation has never been satisfactorily explained so far as I know. It does not appear to be associated in any way with the specific gravity of the different metals composing an alloy; thus, for example, with silver-copper alloys it has been found that in certain proportions of silver and copper the silver segregates towards the center of the bar, and in other proportions the richer alloy is found near the surface, while still another curious fact has been proven in this connection, namely, that when silver and copper are alloyed in the proportion of about three-fourths silver and one-fourth copper little or no segregation occurs. In the case of silver-gold alloys there is, I believe, very little if any segregation, and it seemed to me that there may be some connection between these phenomena and those revealed by the diagrams on the screen to which I have referred, showing a pronounced eutectic alloy in the silver-copper metal and the absence of the eutectic in the silver-gold alloy. I therefore have thought that it might be an interesting as well as appropriate question to ask Dr. Mathews whether the light of microscopic investigation, in connection with the study of the cooling of metals from a molten state, has been cast upon this important unsolved problem in metallurgy?

I may say, in conclusion, that some of the observations on alloys of the precious metals noted herein were made by me as long ago as 1873, in the course of a somewhat prolonged investigation of a method of quantitative spectroscopic analysis then recently proposed by Sir Norman Lockyer and the assayer of the Royal Mint, now Sir Roberts-Austen.* The fact that silver-copper alloys segregate was known long before this time, but was regarded as an unsolvable metallurgical riddle or paradox. I am inclined to believe that a careful and thorough investigation of the micro-structure of different portions of an ingot of silver-copper alloy, in which marked segregation has been shown to exist by the ordinary but very accurate methods of humid assay of silver, would be likely to increase our knowledge of the underlying cause of this somewhat mysterious phenomenon known as segregation.

MR. PAUL KREUZPOINTNER:—The very instructive paper read this evening by Professor Mathews raises a good many questions in my mind concerning the results of scientific research of the structure of metals as to the relation of these results to applied, that is, practical metallurgy. However, before asking questions, I beg to express great pleasure and satisfaction with the progress made in the use of the microscope as applied to metallurgy. Any one who knows anything of the struggles, disappointments and criticisms the pioneers of metallography were subject to sixteen and twenty years ago will appreciate the progress made in this line of scientific work during the last ten years, since when

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* "Electrical Spectra of Metals." *Proceedings American Philosophical Society*, May, 1874, Vol. XIV, p. 162.

the microscope has become an indispensable instrument in the work of investigating the properties of metals. But while I would be the last one to deduct even an iota from the value of the work done in this line by Sorbey, Martens, Heyn, Osmond and others, I beg to remind you that members of the Mining and Metallurgical Section of the Franklin Institute deserve credit and recognition for the pioneer work done in this country, and your present president, Mr. Garrison, who took up the work of metallography twenty years ago, preparing the soil for those who were to reap the fruit of his planting, knows what difficulties a pioneer in any line of work has to contend with.

As to some of the points raised by the lecturer it would be interesting to know why a metal, for instance steel, when in a physical state where the eutectic is already more or less clearly formed, still appears to possess tolerably fair physical qualities although a low elasticity. If I understand aright the formation of the eutectic means a more or less complete segregation of the surplus of elements over and above that percentage of elements which eventually form the eutectic, and where the elements composing the metal and which are then still in a fluid state will freeze simultaneously. If segregation means a dissociation of elements; and dissociation contributes to the loosening or lessening of the cohesion by which the crystals are held together and which is an essential in contributing to the strength of a metal, then why is it that metals of such a large segregated structure like illustration No. 3 (I believe, K), where the eutectic has formed or is forming, still show a high degree of cohesive force in daily practice? Is there a mastics formed during segregation which acts as a binder, so to speak, or is the strength produced merely by the mechanical interlocking of the crystals? By this I do not mean that such metal is equal in quality to one of normal conditions, that is, a metal of fine structure free from the disturbing influences of segregation as much as is possible. There is a fertile field yet for the scientific investigator of the structure of a metal and the relation of that structure to the maximum usefulness and serviceability of that metal in daily practice in determining the effects of heat on the structure in an ascending ratio instead of in a descending ratio, as is the case in determining the cooling curve. What I mean by this is, what is the nature of the various structures of a given metal at various points of heat when we heat that metal from the cold state to the point of fusion? To Brinnel we owe much information in that line, but unless I am mistaken, no such systematic, thorough researches have been made of the structures of metals when heated up to the point of fusion as when cooled down from the point of fusion and the corresponding physical quality at these different structures. I am well aware of the many detached and sometimes practically valuable investigations on the lines I refer to, but what the practical metallurgist, the expert, who has neither the time nor facilities to make continuous investigations needs, is a series of illustrations of the microstructure of the most frequently used structural metals at different degrees of heating that metal and letting it cool and the corresponding physical strength at that heat. Immense quantities of good metal have been spoilt in mill and shop on account of improper heat treatment, and while we know a little better to-day there is still a great deal to be learned on the subject, and our technical universities, who are doing already such good work in physical metallurgy, can be of great benefit to the engineer and our industries by thus contributing their mite to practical metallurgy. In answer to the question by the last speaker whether crystals are necessarily always formed by heat and are not likewise formed by shock and vibrations, I would say that experience seems to indicate that metals, that is iron and steel, do not crystallize under the influence of shock and vibrations as was commonly supposed formerly.

But while metals do not crystallize due to shocks and vibrations they undoubtedly become fatigued, and when thus fatigued, iron and steel break more easily, and because they break more easily when fatigued, they break more quickly under undue strains, and because breaking quickly the metal has no time to flow, its plasticity does not come into play, and if we succeed in breaking a metal before flow sets in, we get a transverse section of the individual fibers which will give the fractured surface a crystalline or granular appearance, which is then assumed to be a crystallization due to shock and vibrations.

Thus, cause and effect are confounded; the crystalline fracture is assumed to be a cause, while the cause is the fatigue of the metal, which breaks more easily and quickly because it is fatigued, and because it breaks more quickly the plastic property of the metal does not come into play, flow will not take place, and because there is no flow the metal will break crystalline, and what we take for crystallization due to shock is nothing but the transverse section of the individual crystals or fibers of the metal thus broken. Under favorable conditions we can break the finest staybolt iron, so that it will show a so-called crystalline or, more properly expressed, a granular fracture. The condition is, that the vibrations set up by the blow or blows in the portion receiving the blow will not be transmitted to that portion from which the first portion is to be broken off, or, in other words, that the portion held in some manner while one end or portion is being broken off, is held so rigidly that the vibrations produced in the piece to be separated are not transmissible to the piece held. The aim and object must be to prevent flow of the metal, not to allow plasticity to come into play before fracture takes place. If shock and vibrations produce crystallization, then iron car-axles ought to crystallize sooner than any other structure; but I have come across iron car-axles which were sixteen, eighteen, twenty and one was twenty-one years in service, and they showed no sign of crystallization.

On the other hand, I have tested and examined 2-inch rods from old canal locks which never had been shocked by any manner of means, and yet they were largely crystalline, the reason for this being, that the iron was granular or cold-short when it was new, as is often the case, and when such a granular portion or nest of crystals happens to come in a location in the structure which has to carry the load, that structure is liable to break under a lower stress than was calculated for, because the structure is weakened by the granular spot of iron imbedded in the fibrous mass, and thus we have a contributary cause to produce fatigue in the metal sooner than it ought to become fatigued and, as said previously, the structure will break more quickly because weakened by a streak or nest of originally crystalline-iron, and because breaking quickly it will break without flowing.

First-class metal, breaking slowly by fatigue or, as we call it, breaking in detail, does not show any granular fracture at all, and I have examined such detail fractures with the microscope a number of times in iron and steel and never was able to detect any change in the structure of the metal more than an eighth of an inch at the point of fracture, and then the changed metal was of an amorphous structure and not crystalline, as it ought to be if there is anything at all in the crystallization theory by shock and vibrations.

Steel being crystalline to begin with, we cannot reasonably even speak of the crystallization of steel. In order for a metal to crystallize it must be a fluid solution like a salt-solution, for instance, or at least in a pasty state, and therefore the science of crystallization is opposed to the theory that crystals can form in a cold piece of metal of such a form and consistency as are formed in a solution according to the laws of crystallization. Crystals are the product of the solidification of chemical elements which are attracted toward each other while being held in a mobile state in the solution, according to the law of their affinities, whatever that law may be, and, moreover, the forces which tend to destroy a structure, or part of a structure, of whatever kind, are directly opposed to the formation of crystals, because strains and stress act upon a metal in the nature of work, and therefore these destructive forces tend to break up the structure, to make it smaller if large, or amorphous, silky, or velvety, if the structure was small from the beginning.

MR. ROBERT JOB: The question of the last speaker, whether iron becomes crystalline as a result of stresses in service, recalls a case which came under my observation a number of years ago. The end of a truss-rod upon a bridge had broken, and the rod, which was of $1\frac{1}{4}$ inches rod iron, about ten feet long, had been thrown upon the ground, and had broken into three pieces, showing a coarsely granular fracture. The rod had been in position thirty years, and the theory was advanced that granulation and brittleness had resulted owing to continuous vibrations in service. Thinking that the conditions of the other rods might disprove this statement, we had several removed from the bridge from positions corresponding exactly with that occupied by the defective rod and subject to exactly the same stresses for the same length of time. Upon fracture, we found a long fibrous structure in each case, with tensile strength of about 48,000 pounds per square inch, and an elongation of 35 per cent. in a 2-inch section. In other words, the vibrations of thirty years' service had not caused any change from the original fibrous structure. Analysis next showed that the composition of these different bars was closely alike, as were the etched sections, and it seemed evident that the rods had been rolled from the same stock, and probably at the same time. Thus it was clearly shown that the coarse granulation had not resulted from the stresses of service, but that it had been present in the iron at the start, this condition being the result of defective heat-treatment in the manufacture.

The whole question of heat-treatment of metals is to-day receiving great attention, and as a practical result, great improvement in the quality of many products has been attained. As a case in point, we may cite the effect of heat-treatment upon the quality of steel rails, as shown by an investigation which has been in progress with us for some time. It was found that a rail of a given composition when finished at a yellow-heat was exceedingly brittle, and broke like glass under a single blow in the drop test. A rail of identically the same composition, but finished at a red color, withstood without fracture eight blows of a 2,000 pound weight falling a distance of 20 feet, the supports being 4 feet apart.

Sections under the microscope showed in the former case a very coarse granular structure, and in the latter a much finer interlocking appearance, which amply accounted for the increased strength observed under the drop test. In service, similar differences would be observed as to wear, since it has been repeatedly proven by different observers that rapidity of wear varies—other things being equal—with the size of the grains of the metal. Thus, we have found that this mere difference in finishing, temperature has transformed a fragile untrustworthy rail into one of vastly increased toughness and ability to resist wear.

DR. JOSEPH W. RICHARDS (Lehigh University) begged leave to call attention to the information which might be gotten respecting the constitution of alloys by a consideration of the variation in other physical properties than the melting points. The specific gravity of alloys compared with their specific gravity calculated from that of their constituents, showed usually a contraction taking place during an alloying, sometimes an expansion. Dr. Mathews had shown that gold-silver alloys were peculiar in that their melting points varied regularly and uniformly from that of silver to that of gold. These very alloys are also peculiar in their specific gravities, which vary regularly from that of silver to that of gold without any expansion or contraction taking place at any point. The speaker had verified this fact many times by finding the specific gravities of gold-silver alloys exactly the same as that calculated from their composition. This absence of expansion or contraction in alloying seems to be, therefore, characteristic of alloys which are simply homogeneous mixtures and which form no eutectic alloy. Dr. Mathews had shown that 0·5 per cent. of aluminium in tin depressed the melting point of the latter, but over this amount the melting point rose, 1 per cent. having the same melting point as pure tin. While the speaker had not measured the specific gravities of these alloys, he felt confident in predicting that similar phenomena would be found in this respect, that 0·5 per cent. of aluminium in tin would probably be found to increase the specific gravity of the latter, instead of decreasing it; that above this amount would lose its effect, and that the alloy with 1 per cent. would probably be found to be of the same specific gravity as pure tin. It is known that one-half per cent. of aluminium in iron *increases* its specific gravity. One per cent. of antimony in lead makes an alloy *heavier* than lead; 2 per cent. is of the same specific gravity as lead, and higher percentages are lighter. It is very likely that these alloys will show an analogous behavior in their melting points. Alloys are best studied by making a complete correlation of as many physical properties as can be observed, and such study is not only of high theoretical interest but of immediate practical value in the arts.

MONOCHROMATIC COATINGS ON GERMAN SILVER AND PLATINUM.

BY DR. R. BOETTGER.

After innumerable experiments success has been achieved in producing the finest monochromatic shades on platinum and German silver by electrolytic decomposition of organic manganous salts, the metal to be treated being connected with the positive electrode. It has formerly been supposed that it was principally the form of the negative pole that influenced the nature of the Nobili figures. Although this cannot be denied altogether—for with a sharply pointed platinum wire only ring-shaped figures can be obtained—yet the rule is subject to modification with regard to certain manganous salts. With solutions of manganous hippurate, acetate or succinate, even when a very thin platinum wire serves as the negative pole, colored rings are never formed, but a monochromatic coating, on the positive electrode. This has only been observed with these special salts. It further appears that metals which form higher oxides, such as lead and manganese, are the best in solution for producing Nobili figures. In using the special manganese salts for obtaining the monochromatic coating, the shape of the negative pole appears to be immaterial; but it is advisable to secure uniformity of the coating on the positive element, that the negative element should take the form of a disk of platinum. Nothing decided can be said about the strength of the manganese solutions, as the best strength depends upon the power of the current, and must be determined by each experimenter for his own special case. The weaker the current the stronger the manganese solution must be. The color produced changes greatly and quickly, so that it is essential instantly to break the current when the desired color has been produced. Golden yellow, green, and purple are obtained with particular brilliancy. The metal should be removed from the bath as soon as the current is stopped, rinsed with distilled water, and carefully dried with soft blotting paper. If manganous chloride or lead acetate is used, the colors appear in rings instead of as a uniform layer and of all the colors of the rainbow, in the softest shades, the predominant hues being green, golden yellow and blue, each whole system of rings surrounded by a yellow zone.—*Der Metallarbeiter.*

Section of Photography and Microscopy.

Stated Meeting, held Thursday, December 5, 1901.

On the REVERSAL of the PHOTOGRAPHIC IMAGE and its SUBSEQUENT DEVELOPMENT in ACTINIC LIGHT.

By M. I. Wilbert,
Member of the Institute.

Prof. Francis E. Nipher, during the past year, presented several communications to the Academy of Science of St. Louis that have renewed the general interest in the subject of sensitive silver salts and their characteristic behavior to the action of actinic light. The peculiar fact that these silver salts have the property of assuming a certain physical condition that makes them particularly susceptible to the action of reducing agents, and also that an extended exposure to actinic light brings about a reversal of this peculiar physical condition, were facts that had been observed long before Professor Nipher made his interesting communications. The feature of his experiments that was original, and that was all that Professor Nipher claimed to be original, is the posibility of developing these reversed photographic images in actinic light.

The statement of this fact, when first made, appeared to be so different to what we were accustomed, in ordinary photography, that it created widespread attention, and caused numerous experiments to be made along the same lines.

Following up a suggestion that was made before the Photographic Section of the Franklin Institute some two months ago, I made several experiments along these same lines. These experiments appeared to me to be interesting, and to some extent also instructive. Professor Nipher, in one of his early communications, states that a photographic plate, even after it had been exposed for weeks to diffused daylight, would still give an image if exposed to the direct spark discharge of an induction coil or a static machine, and subsequently developed, either in the dark room or in bright daylight,

To test this statement I allowed several pieces of damaged photographic plates to lay exposed to bright light for ten days or two weeks, and then subjected them to a spark discharge; the resulting electrographs were then developed in bright light, and, in each case, the resulting picture showed the action of the electric spark clearly. In one case, shown here, a coin was placed in the center of photographic plate and connected with one terminal of the induction coil. In the resulting picture we see plainly the radiations of the spark in all directions. The most interesting part of this particular experiment, however, is the fact that the area immediately under the coin and a very narrow zone around it has been completely reversed, while the spark radiations, with few exceptions, show dark against the fogged background of the plate itself.

Following up Professor Nipher's own experiments, I exposed a very sensitive photographic plate under a positive, in this case a dense lantern slide, for five minutes and then developed the same at a window with northern exposure, using an old metol-hydroquinone developer without any further restrainer. The resulting picture makes a presentable lantern slide, and represents fairly the condition of the original positive.

To get an idea of the length of time necessary to obtain the best results under similar conditions, I interposed between the plate and positive a piece of heavy black paper. By withdrawing this paper gradually, I was able to make on the same plate exposures of one, two, four and eight minutes. As will be seen by an inspection of this picture the portion of the plate that has not had any exposure under the positive is entirely opaque; that portion that was exposed for one minute is very dense, but the portions of the picture representing the deepest shadows in the original picture are not entirely reversed. The portion of the plate exposed for two minutes is next in density, and here we have complete reversal of all portions of the picture. The parts that were exposed four and eight minutes respectively, are not nearly as dense and differ but little in their general appearance. This is a feature that should especially be noted, as it appears to me to be evidence of a protective influence of the upper layers of changed silver salts, similar to what we would naturally expect to have had in the plates that had been exposed to diffused daylight for weeks. This experiment was gone over on several occasions, with uniform results. Even in cases where the highest exposures were as long as an hour, or the equivalent of many hours' exposure in a camera, the high lights came out quite clear, barring the general vagueness that we see in all reversed pictures.

A series of exposures made with a camera were, however, of even greater interest to me showing, as they do, the very great range of exposure that is possible with rapid photographic plates under practically the same conditions of illumination and development.

The subject was Girard College in bright sunlight at or near mid-day. The first picture is a negative made with an exposure of approximately half a second, using a Beck wide angle lens having a 128 stop. The resulting picture developed with fresh normal metol-hydroquinone developer, came up to its present density in a little over four minutes after being placed in the developing solution and is, if anything, a little overexposed.

The next picture had exactly the same amount of exposure with a sixteen stop, giving it approximately eight times the amount of light of the first; the plate developed very much more rapidly, but is still a very fair negative.

The next one of the series was exposed for five seconds under exactly the same conditions as the previous one, using the sixteen stop, and the result is still a negative, though rather thin; but when we consider that it has had eighty times the light that the first negative had, we will appreciate that the possible range of exposure is really remarkable.

The next plate had an exposure of fifty seconds, and shows traces of general fog; you can see, however, that there are distinct traces of reversal; the high lights are completely reversed and almost clear. This plate would probably come somewhere near the so-called zero condition, where the positive and negative conditions of the plate would nearly balance each other, and give as result of this a general fog.

Again increasing our exposure, this time to ten minutes, and developing the picture in the dark room, we obtained a completely reversed picture that is rather dense in the shadows on account of the activity of the developer; under these conditions, this plate did not require more than three minutes in the developing solution, and appeared to flash up very rapidly.

The next picture was given the same exposure, but developed at an unprotected window having a northern exposure —in fact, the same window from which the plate had been exposed in the camera. This required eight or nine minutes to develop, and the conditions of the development were quite different. When the plate was first taken from the camera there was plainly seen a distinct outline of the picture as a negative. On placing the plate in the developing solution this negative image gradually faded until the plate was perfectly blank, then the edges of the plate that had been protected by the holder began to darken, and following this the positive image gradually but slowly developed, the process requiring at least three times the time that was necessary in the dark room.

Some further experiments were made with a view of testing the action of various reducing agents; the exposure in these cases was twenty-five minutes, and more or less satisfactory results were obtained by using pyrogallic acid, amidol, eikonogen, metol, metol-hydroquinone, and hydroquinone as the reducing agents. The first four did not give very satisfactory results, as their action appears to be too rapid and rather irregular; the fifth was the developer used in the experiments alluded to above; while the sixth, hydroquinone, was used with and without an alkali, with interesting results. The first picture, which I show you, had the normal amount of alkali, as advised in the formula given by the manufacturer of the plates that were used. This plate shows up quite clearly and is quite black.

The next plate was developed with a developer having but half the amount of alkali, and, as you can see, it has a distinctly brown cast of color; it was also slower in development, although not markedly so.

The third plate was developed without the addition of any alkali, and presents a reddish-brown appearance in color, though sharp and distinct in all details of the picture. Speculation as to the principles involved in these processes are perhaps out of order, but it appears to me as though the action of light on sensitive silver salts was in the first stage a purely physical one, disarranging the equilibrium of the molecules of the silver salt and making them susceptible to chemical reducing agents; prolonged action brings about a chemical change that produces opaque metallic silver or a silver oxid, and this in turn protects the silver bromide molecules immediately below it from further action of light; the physical change is going on in other portions of the plate, and these in turn are susceptible of reducing by proper chemical agents, while the portion that has been acted on chemically is not. The change brought about by reducing agents is much more intense and opaque than is that produced by light, thus giving us a dark picture against a gray or hazy background. As evidence of this I would say that so far I have not seen any reversed pictures in which the highlights were represented by perfectly clear glass.

More evidence that the ultimate action of light is chemical is found in the fact that we may expose a plate under a negative for a sufficient length of time to produce the outlines of the picture distinctly, then fix the resulting plate in hyposulphite solution and still retain the faint outline of the positive, and this in turn may be intensified so as to be distinctly visible.

Under proper conditions we would no doubt be able to make photographic negatives in this way without the use of a dark room; that is, by first dissolving out the unchanged silver salts in a fixing bath and subsequently intensifying the very weak but distinct image remaining on the resulting plate.

BOOK NOTICES.

Water and Water Supplies. By John C. Thresh, Sc.D., etc. Third edition, revised and enlarged. Philadelphia: P. Blakiston's Son & Co., 1901. 8vo, pp. XV + 527.

This work treats of water, its composition and properties, rain and rain-water, surface water, subsoil water, natural spring waters, deep-well waters, river water, quality of drinking waters, impure water and its effects upon health, the interpretation of water analyses, the pollution of drinking water, the self-purification of rivers, the purification of water on the large scale, domestic purification, the softening of hard water, quantity of water required for domestic and other purposes, selection of sources of water-supply and amount available from different sources, the protection of underground water supplies and the protection of surface-water supplies, wells and their construction, pumps and pumping machinery, the storage of water, the distribution of water, the law relating to water-supplies, rural and village water-supplies, and water charges.

The work has an elaborate general index and an index of proper names. It is well illustrated.

Furnace Draft: Its production by mechanical methods. Wm. Wallace Christie, Consulting Engineer, etc. Published by the author. Paterson, N. J., 1901. 12mo, pp. 44. (Price, 50 cents.) D. Van Nostrand & Co., New York.

The author devotes this momograph to the consideration of the several methods of operating furnaces by mechanical means, by blowers, steam jets, etc.

Linear Drawing and Lettering. For beginners. By J. C. L. Fish, Assoc. M. Am. Soc. C. E., Associate Professor of Civil Engineering in the Leland Stanford Junior University. Published by the author. Palo Alto, Cal. 7 x 10½, oblong. V + pp. 65, 46 figures. (Limp cloth, $1.00.)

Chapter I treats of instruments and materials. Chapters II and III give the details of the course of "Linear Drawing and Lettering" in Stanford University; and Chapter IV treats of the details of draughting as pursued in the course of instruction followed in the University.

Franklin Institute.

[*Proceedings of the Stated Meeting held Wednesday, February 19, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, February 19, 1902.

President JOHN BIRKINBINE in the chair.

Present, 124 members and visitors.

Additions to membership since last report 12.

Dr. Joseph W. Richards, Bethlehem, Pa., presented an interesting informal communication on the "Electro-Chemical Industries at Niagara Falls," which he was requested to prepare for publication in the *Journal*. The remainder of the evening was devoted to the consideration of the report of the special committee appointed at the stated meeting of November 20, 1901, to consider the adoption of the metric system of weights and measures in the United States.

The report (with appendix) as submitted is as follows:

REPORT OF THE SPECIAL COMMITTEE APPOINTED BY THE FRANKLIN INSTITUTE TO CONSIDER THE FEASIBILITY AND ADVISABILITY OF THE ADOPTION OF THE METRIC SYSTEM IN THE UNITED STATES.

WHEREAS, It is desirable to obtain an international standard of weights and measures, also to simplify and regulate some of our existing standards; and

WHEREAS, The metric system is commendable, not only as a suitable international standard, but also for facility of computation, convenience in memorizing and simplicity of enumeration;

Resolved, That the Franklin Institute approves of any movement which will promote the universal introduction of the metric system with the least confusion and expense.

Resolved, That the national Government should enact such laws as will ensure the adoption of the metric system of weights and measures as the sole standard in its various departments as rapidly as may be consistent with the public service.

JAMES CHRISTIE,
American Bridge Company, Pencoyd, Pa.

A. E. KENNELLY,
Houston & Kennelly, Electrical Engineers.

F. E. IVES,
Photographer and Engraver.

WILFRED LEWIS,
President Tabor Machine Co.

S. M. VAUCLAIN,
Superintendent Baldwin Locomotive Works.

JESSE PAWLING, JR.,
Instructor in Physics, Central High School.

GEORGE F. STRADLING,
Instructor in Physics, N. E. Manual Training School.

HARRY F. KELLER,
Professor of Chemistry, Central High School.

A. FALKENAU,
Engineer and Machinist.

L. F. RONDINELLA,
Instructor in Engineering, Central Manual Training School.

January 22, 1902.

APPENDIX.

Questions discussed at meeting of Sub-Committee, January 17, 1902.

QUESTIONS.	Answers Agreed upon.
(1) Assuming the desirability of an international standard, could we expect nations using the metric system to abandon that and adopt our system?	No.

(2) Can we not concede the advantages of the metric system for purposes of computation. and also as being readily memorized and the relations between weights and measures borne in mind without much effort?	Yes.
(3) Have any valid objections against the metric system been effectively urged, excepting that the numeration cannot be continuously subdivided by two?	No.
(4) Is not this similar objection to our decimal currency overcome by the advantages of the system?	Yes.
(5) For convenient minimum units of hand rules, is not the mm. better than either $\frac{1}{16}''$ or $\frac{1}{32}''$, the latter being rather a fine subdivision for ordinary rough measurements?	The mm. is equally as convenient.
(6) Assuming that the change in our system could be effected, without serious expense or confusion, could we recommend this change as desirable?	Yes.
(7) Could not such a change be fairly initiated if the national Government would adopt the system in all its departments where no serious confusion would occur from an early change, gradually extending the system to other departments, when people became accustomed to its use, and tools were accumulated which conformed to the new standard?	Yes.
(8) In the workshops, could not a large proportion of existing tools and gauges be retained until they were gradually superseded, merely designating their nominal dimensions in the nearest convenient metric units?	We anticipate no prolonged serious confusion.
(9) If in the course of a term of years the system came into universal use in the service of the government, is it probable that its adoption would follow elsewhere within a reasonable time?	Yes.
(10) Would it appear to be practicable to inaugurate the adoption of the metric standards for weights or for liquid measures in advance of linear measures, as the former would not involve the abandonment of such numerous and costly tools as would the latter?	No.

The Secretary presented a large amount of correspondence from prominent manufacturers and professional men throughout the country whose opinions on the report had been solicited.

The discussion of this subject was then opened by Mr. George M. Bond, representing the Pratt & Whitney Company, of Hartford, Conn., who spoke in opposition to the committee's views.

Messrs. Vauclain, Christie and Pawling, of the committee, Dr. E. J. Houston, Mr. Spencer Fullerton and a number of others spoke in favor of the report.

After a protracted discussion, the report was adopted by a decisive majority.

(The discussion and important correspondence on the subject will shortly be published in the *Journal.*)

Adjourned. WM. H. WAHL, *Secretary*.

SECTIONS.

(*Abstracts of Proceedings.*)

PHYSICAL SECTION.—*Stated Meeting* held Wednesday, January 22, 1902. Dr. Stradling in the chair.

The annual report of the Section's operations during the past year was thereupon presented and, after reading, was adopted and ordered to be transmitted to the Board of Managers.

The following were elected officers of the Section to serve for the ensuing year, viz.:

President—Dr. A. Stanley Mackenzie.

Vice-Presidents—Dr. Martin C. Lloyd, Dr. Geo. F. Stradling.

Secretary—Mr. Jesse Pawling, Jr.

Conservator—Dr. Wahl.

The Section tendered a vote of thanks to Dr. Stradling, its retiring President, for the admirable manner in which he had conducted the affairs of the Section during the past year.

The meeting thereupon proceeded to the consideration of a report of a Special Committee of the Institute on "The Feasibility and Advisability of the Adoption of the Metric System in the United States." The report was discussed by Messrs. E. Goldschmidt, G. A. Hoadley, A. S. Mackenzie, Geo. F. Stradling, and Jesse Pawling, Jr. On Dr. Stradling's motion the report was unanimously endorsed.

Dr. Jos. W. Richards, of Lehigh University, followed with the paper of the evening on the "Goldschmidt Theory of Harmony," after some discussion of which the meeting was adjourned.

JESSE PAWLING, JR.,
Secretary.

CHEMICAL SECTION.—*Stated Meeting* held Thursday, January 23, 1902. Dr. Williams in the chair. Present, 34 members and visitors.

The following were elected officers of the Section for 1902, viz.:

President—Lyman F. Kebler.

Vice-Presidents—Mr. Joseph Richards, Dr. R. H. Bradbury.

Secretary—W. E. Ridenour.

Conservator—Dr. Wahl.

The meeting passed a unanimous vote of thanks to the retiring President, Dr. Williams, for his services during the previous year.

Mr. Kebler then assumed the chair.

Dr. H. F. Keller presented for consideration the report of a Special Committee of the Institute on "The Feasibility and Advisability of the Adoption of the Metric System of Weights and Measures in the United States." On motion of Dr. H. W. Jayne, numerously seconded, the Section endorsed the report.

Dr. W. F. Hillebrand, U. S. Geological Survey, read the paper of the evening, on "Modern Methods of Analysis as Applied to Minerals and

Rocks," which was discussed by Mr. Kebler, Dr. H. F. Keller, Prof. L. F. Garrison, and the author.

The meeting passed a vote of thanks to the author and adjourned.

WM. H. WAHL,
Secretary pro tem.

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—Proceedings of the fifth-teenth *Stated Meeting*, held Thursday, February 6th. Dr. Henry Leffmann in the chair.

The annual election at the previous meeting resulted as follows:

President -Dr. Henry Leffmann.

Vice-Presidents—F. E. Ives, J. W. Ridpath

Secretary—Martin I. Wilbert.

Conservator—Dr. Wahl.

The report of the Special Committee appointed by the Institute at the November meeting to consider "The Feasibility and Advisability of the Adoption of the Metric System of Weights and Measures in the United States" was brought before the Section and unanimously approved.

The paper of the evening, on "Orthochromatic Photography," was presented by Mr. John Carbutt. The speaker illustrated the subject by photographs made on orthochromatic plates. The effects produced by color screens were also illustrated.

Mr. Ives expressed his satisfaction in finding that American manufacturers had taken up the manufacture of red sensitive plates.

The question of a safe light for a dark room was brought up, and Mr. Carbutt stated that a screen allowing but few of the extremes of red rays was the only one that was possible.

Dr. Leffmann referred to the very great use that was being developed for aniline dyes, not alone for dyeing purposes, but also in connection with scientific investigations, and latterly with color photography.

M. I. WILBERT,
Secretary.

MECHANICAL AND ENGINEERING SECTION.—*Stated Meeting* held Thursday, February 13, 1902. Mr. A. M. Greene, Jr., in the chair. Present, 48 members and visitors.

Mr. James Christie, chairman of the Special Committee of the Institute to report on "The Feasibility and Advisability of the Adoption of the Metric System of Weights and Measures of the United States," presented the committee's report and advocated its endorsement by the Section. (The report will be offered for approval by the Institute at the stated meeting of February 19th). The report was discussed by Messrs. Spencer Fullerton, J. C. Trautwine, Jr., Jesse Pawling, Jr., H. T. Colvin and others. The report was approved and the action of the Section was ordered to be transmitted to the Institute.

Mr. Walter Ferris presented a paper on "Water Meters," fully illustrated by lantern-slides.

Mr. Charles Day followed with a communication on "The Application of Motor Drives to Machine Tools," illustrated by numerous examples exhibiting the advantages derived from this mode of power transmission. Both papers were freely discussed.

D. EPPELSHEIMER, JR.,
Secretary.

JOURNAL

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VOL. CLIII, No. 4. 77TH YEAR. APRIL, 1902

ELECTRICAL SECTION.

Stated Meeting, held Thursday, December 19, 1901.

PARALLEL OPERATION OF ALTERNATORS.

BY PAUL M. LINCOLN,
Electrical Engineer, Niagara Falls, N. Y.

Some time during the month of November, 1900, the writer conceived an idea for a device which would indicate synchronism between two alternators about to be thrown in parallel. The idea seemed to promise far better results than anything used before, and a model which was constructed and put in operation about January 1, 1901, bore out this anticipation. The attention which the instrument attracted from the electrical engineering profession was thought to be sufficient to justify submitting it to the Franklin Institute for examination. Correspondence with the commitee appointed by the Institute on the question to whether or not it was desirable for me to come to Philadelphia to explain its workings in detail, finally culminated in an invitation to read a paper covering the general subject of the operation of alternators in parallel.

I am well aware that much has been written on this subject, and that it is not likely that my discussion will bring out anything new. My treatment, however, may enable some of you to look at this subject from a new viewpoint.

It is fortunate that we possess a mechanical analogy that, in many respects, gives an exact representation of alternators in parallel. In place of a bank of alternators taking mechanical power from their prime movers and supplying electrical power to a common bus bar, substitute a bank of spur gears also taking power from prime movers and supplying it through these gears to a common shaft. This line shaft is the mechanical analogue of the bus bars, and the spur gears of the alternators. At the bus bars is available in shape of electrical energy transmitted through dynamos all the power of the various prime movers. Similarly, at the line shaft, is available in the shape of mechanical energy transmitted through spur gears all the power of the various prime movers.

Successful operation of alternators in parallel consists:

(1) of proper division of load; and

(2) of freedom from hunting.

The first of these headings may be again subdivided into:

(*a*) Alternators driven by a common prime mover; and

(*b*) Alternators driven by separate prime movers.

The only case of alternators driven by a common prime mover that deserves consideration from its practical use is that of alternators' belt connected to a common line shaft. In this case division of load is a matter of pulley ratio and of tightness of belt. This problem is exactly the same as that of division of load among a number of belts that are taking mechanical power from one line shaft and delivering it to another. In both cases the division of power depends upon pulley ratio and relative condition of belts.

The real problem of proper load division, however, comes under the second heading. In this case the analogy between alternators and gears is perfect. In the case of the gears, relative load taken by prime movers is, of course, dependent entirely on conditions outside the gears. Just so the division of load between prime movers driving alternators, and, consequently, between the alternators is entirely dependent on conditions outside the alternators. That is, there is no adjustment of field or other conditions of an alternator which will make a change in the relative amount of load supplied through that alternator—unless the alternator is forced out of step. This condition is comparable in our analogue to the stripping of the gear, and is a condition which occurs under abnormal circumstances. To those whose experience has been entirely with the parallel operation of direct-current dynamos, the idea of field adjustment having no effect on relative load may at first seem somewhat startling. The mechanical analogue of direct-current machines in parallel is the connection of the various prime movers to the line shaft by belts, or other friction connection, where an addition of load produces a slip. Stiffening the field is like tightening the belt. The tendency of either stiffening the field of one of a bank of parallel connected direct-current dynamos, or of tightening the belt of one of a bank of belt-connected prime movers driving a common counter-shaft is to slow down that prime mover. The governor in correcting this tendency causes the prime mover to assume a larger share of the load. With the alternator, however, the case is different. Prime movers, driving a bank of parallel connected alternators, are practically geared together, and any condition which effects the speed of one effects the speed of all similarly and simultaneously. It will be at once recognized, therefore, that the problem of successfully operating alternators in parallel is a problem of governors, and not a problem of alternators.

Governors must control speed with respect to load, in one of three ways:

(1) Speed may rise with increasing load.

(2) Speed may be independent of load, and

(3) Speed may fall with increasing load.

Proper division of load occurs between parallel connected alternators only when the prime movers are provided with governors of the third type. A moment's consideration will show that equilibrium between the loads of parallel connected alternators cannot be maintained with either of the first two types; for if two or more prime movers, each provided with a governor of the first type, are connected by gear or alternator to a common load, the prime mover, whose governor first responds to an increase in load—it being impossible to so adjust the governors that they will all respond simultaneously—takes the entire load increase. The increase of load being thus taken care of, the governor of the prime mover, which has assumed the increase, demands for a condition of equilibrium a speed higher than the original; that prime mover will, therefore, assume another increment of load and tend to raise the speed to a higher level. This tendency to a higher speed will, of course, bring into action in a contrary direction some of the remaining governors. The net result will be that some alternators would be delivering the maximum power of their prime movers; others would be taking power from the bus bars to turn their prime movers, and one would be somewhere between these two limiting values.

In the case of a governor which regulates speed entirely independent of load—the case practically of the isochronous governor—there would exist neither the tendency to entire absence of equilibrium, as under the first-mentioned type of governor, nor the tendency toward perfect equilibrium as under the third-mentioned type. It is impossible, however, to adjust all governors to the same degree of sensitiveness. In a bank of parallel connected alternators, therefore, there is nothing to prevent that alternator and prime mover with the most sensitive governor from continuing to assume all load changes until the limiting load of its prime mover is reached.

It is, therefore, only with the third type of governor—where speed falls as load increases—that proper division of load between alternators connected in parallel can be attained. It is with this type of governor only that the maximum loaded prime mover tends to assume all decrements of load, and the minimum loaded prime mover tends to assume all increments of load.

In regard to the per cent. drop in speed between no load and full load required to bring about proper load division, I can only quote my own experience with the turbines and dynamos of the Niagara Falls Power Company. In this case the speed drops about 2 per cent. between no load and full load, and the difference between the loads taken by various turbines rarely amount to more than 10 per cent. of a single unit's output.

There are other methods of insuring a proper load division, but they are open to such objections that they are not generally employed. One method is to actuate all governing mechanisms from a single master governor. This necessitates some connection, either mechanical or otherwise, between all the governors in use. Another method is to set in a fixed position all governors but one, allowing that one to take all load changes. In this method no attempt is made to equalize loads, and it might lead to disastrous results in case the entire load is suddenly removed.

Proportional division of load, while desirable, is not an absolute essential to the successful operation of many plants. Freedom from hunting, on the other hand, is an essential; and where hunting exists to any pronounced extent, successful parallel operation is impossible. Further, hunting is a fault which may arise not only with alternators in parallel, but in any electric circuit containing two or more synchronous machines, be they alternators, synchronous motors or rotary transformers.

Before discussing the causes of hunting, let me say a few words in regard to the reactions which occur between the field and armature of an alternator. Each time a given point on an alternating armature passes a pair of poles, the armature voltage passes through a complete cycle. That is, the voltage has passed from a maximum in one direction to a maximum in the other, and has returned to the point from which it started. Let this point of the armature be so selected, that when there is no load it passes the center of the pole-pieces at the instant the armature voltage is passing through zero. If, now, the position of this point relative to the center of the pole-pieces be examined when the alternator is under load, it will be found in the case of a dynamo ahead of and in the case of a synchronous motor back of the center of the pole-piece at the instant voltage is passing through zero. The amount of this movement depends, first, upon the amount and character of the load, and second, upon the regulation of the alternator. That is, the load-current in the armature combines with the field-current, and their resultant field takes up a new position which, in a generator, is in the direction of armature motion, and in a motor in a contrary direction. In other words, a mechanical strain on an alternator armature produces a mechanical displacement. Our mechanical analogue of the gears fits this aspect of the alternator if we replace the inflexible rigid gear tooth with an elastic flexible one, as, for instance, one of soft rubber. The greater the strain on this rubber toothed gear the greater will be the distortion of the teeth in contact. In the alternators the pressure between teeth is a current in the armature, and distortion of the gear teeth is a distortion of the alternator field. In either case we have both the prime conditions necessary for an oscillating system—a mass free to move and an elastic force which tends to return that mass to its original position in case of displacement. Hunting in synchronous machines is simply the oscillation of the armature masses accelerated and retarded above and below normal average speed. The problem of preventing such hunting is, therefore, the problem of causing a pendulum to cease swinging. A pendulum to continue swinging must have:

(1) An active force, synchronous with the pendulum swings; and

(2) Freedom to move without undue friction.

Hunting of parallel connected alternators, like the swinging of a pendulum, will die out if not kept alive by some synchronous force. Also hunting of alternators in parallel like the swinging of a pendulum may be kept alive and even made violent by an exceedingly small synchronous force, provided movement can take place without undue friction. It is evident, then, that the two lines of attack to prevent hunting are:

(1) The removal of the synchronous force tending to keep alive the oscillatory motion, and

(2) The provision of electric friction tending to destroy such motion if once started.

Probably the most potent factor in keeping alive hunting will be found in the steam-engine governor as usually installed. The slight acceleration and retardation that hunting causes in the speed of the prime mover is sufficient to throw the governor into action, particularly if the governor is of a type in which inertia plays a prominent part. The delay in time between speed change and the corrective force applied by the governor is enough to cause a part, at least, of this corrective force to alternate synchronously with the speed changes. That is, hunting induces or tends to induce in certain governors a synchronous force to keep itself alive. The remedy to this action is to make the time between a movement of the governor balls and its application of the corrective force greater than the time of a half-cycle in the period of oscillation. That is, to prevent hunting, the period of oscillation should be decreased and the time of response to motion of governor balls increased. The period of oscillation is decreased by reducing the mass of alternator armatures and by increasing their synchronizing force or, what is the same thing, stiffening their regulation. For a plant already in operation, however, these quantities are fixed, and in any event there is a limit to the time-period of oscillation even with the most favorable design, which may bring it within time of response to motion of governor balls. A remedy then involves the prolongation of the time of response to motion of governor balls. This object may be accomplished by the aid of properly designed dash-pots. Mr. W. L. R. Emmett, of the General Electric Company, in a very able paper read by him before the American Institute of Electrical Engineers, on October 25. 1901, described an ingenious dash-pot which he has designed and repeatedly used with success in connection with engine governors for the suppression of hunting. In that paper Mr. Emmett states that his dash-pot has suppressed hunting in every case where it has been applied, even though the hunting was so severe before its application that parallel operation was impossible.

This action of the governor, however, is not the only force which may give the synchronous impulses necessary to keep parallel connected alternators hunting. The uneven torque at different points in an engine-cycle may give rise to hunting if the natural period of oscillation is synchronous with the engine impulses. One remedy for this difficulty is to synchronize engines as well as alternators, so that at any instant of time each engine is in a similar part of its cycle. Another remedy lies in the addition of fly-wheel capacity, which will tend to absorb and smooth out the mechanical fluctuations which must otherwise appear as fluctuations in load between alternators. There are other forces which may, under certain circumstances, become sufficient to account for hunting, and which will be discussed later.

The second preventive for hunting lies in so designing the alternating machines that as soon as hunting occurs a resisting force of a frictional nature is set up tending to destroy the oscillation. This remedy is like floating the pendulum bob in oil or water. This electric friction is introduced by surrounding the field poles, especially near the tips, with a band of copper. The change in field flux, caused by the distortion of hunting, produces eddy currents in this copper band. Still better is a copper plate which projects under the pole tips. The shifting of magnetic flux to and fro across the pole face while hunting is more pronounced than the total variations in that flux, and this flux change is a maximum at the pole tip. Consequently, the copper pieces under the pole tips, or in fact any conductors so placed that the shifting field produces eddy currents therein, are better adapted to produce the desired damping effect than is the short circuiting band. With a solid pole-piece the metal of the pole-piece itself forms a path for the eddy currents, and it has always been noted that the performance of solid pole alternators is better, so far as freedom from hunting is concerned, than laminated pole alternators where the poles are without the protection described above. It is evident, however, that a complete remedy cannot be obtained by the damping method alone. So long as there is a force tending to produce oscillations, hunting will exist. In any case the amplitude of the oscillations will be such that the energy absorbed by them is equal to the energy producing them. Damping, therefore, means simply a reduction of the amplitude and not a complete suppression of the oscillation. To complete the cure not only must the damping or friction element be introduced, but the tendency to produce oscillation must be removed.

My experience has included a few cases of hunting on circuits in which the prime movers were turbines and the governors were of a relay type where response to motion of governor balls required several seconds—much longer than the time of a half-period of the hunting. In these cases the hunting can be ascribed neither to the irregularity of torque in the prime mover, nor to the action of the governor. In all of these cases which have come under my observation, some part of the electric system—not necessarily, however, that in which hunting was most noticeable—contained synchronous machines with laminated poles and no damping pieces. That is, these machines were capable of maintaining hunting oscillations with an exceedingly small synchronous force. As an example of the minute power to keep alive an oscillation where there is no damping, witness the clock in which a few foot-pounds of energy in a coiled spring keeps the pendulum oscillating for days and even for weeks. The small amount of power required to account for the hunting above mentioned is, undoubtedly, due to the fact that the power in an alternator is delivered as a series of impulses—not as a steady push. During one-half of a complete hunting oscillation these impulses tend to accentuate such oscillation, and during the next succeeding half-oscillation, to suppress the oscillation. If the time of a complete oscillation is exactly equal to the time of any odd number of impulses, the sum of impulses for accentuation may be one impulse greater than the sum of impulses for suppression of the oscillation. It may thus occur that where resistance to oscillation is slight, hunting is kept alive by the fact that the power of the prime mover is delivered as a series of impulses, and not as a steady push. It may be objected that in polyphase alternators the instantaneous amount of power is always a constant. This is true for the summation of power from all phases, but is not true for the individual phases which go to make up that summation. That this explanation is the correct one is indicated by the fact that the hunting referred to above was invariably suppressed by changing the field strength. Changing the field strength by changing somewhat the elastic force operating upon the armature causes in turn sufficient variation in time period of oscillation to throw it out of step with its exciting force.

A paper on the parallel operation of alternators would hardly be complete without some discussion of the methods and apparatus used for throwing alternators in parallel. Particularly is this true, in the present case, where the writer's device for synchronizing alternators is directly responsible for the presentation of this paper.

Both spur gears and alternators must be properly synchronized before throwing them into service. In the case of spur gears, before throwing them into mesh they must not only be brought to practically the same speed, but the instant for throwing into mesh must be so chosen that a tooth of one is exactly opposite a space between teeth of the other. If either of these requisites be neglected there is considerable danger of stripping a gear. The alternator is very similar. Before paralleling alternators, not only must the frequencies be practically the same, but the instant for paralleling must be so chosen that the E.M.F. waves are in practically the same phase relation. That is, the two E.M.F. waves must pass through zero in a certain direction at practically the same instant of time. If either requisite be neglected there is considerable danger that the alternator will fail to fall into step.

The standard methods of synchronizing alternators are:
(1) The use of synchronizing lamp.
(2) The use of synchronizing voltmeters, and
(3) Woodbridge's method.

All three of these methods depend upon the fact that a change of phase relations between two E.M.F.'s causes a change in the resultant voltage obtained by combining the two E.M.F.'s. If the angle between any two radii of a circle represents the phase angle between two E.M.F.'s, the chord joining the outer ends of the radii represents the resultant of the two combined E.M.F.'s. A resultant voltage of zero, therefore, indicates that the two E.M.F.'s are in phase. A lamp, or voltmeter, which will indicate when this zero voltage occurs, will indicate also that the difference in phase between the two E.M.F.'s is zero. The principal difficulty with this method is that no alternating voltmeter responds to less than 20 per cent. of its normal scale, and an incandescent lamp will not begin to glow until about 40 per cent. of its normal voltage is reached. That is, the limits of sensitiveness of synchronism by voltmeter is about 25° each side of zero phase difference and by lamps nearly 50.° Inside these limits the operator has no indication of phase difference except what he can judge by interpolating between equal lamp brightness or equal voltmeter readings. With unsteady and constantly changing speeds such interpolation becomes both difficult and dangerous. Still further inaccuracies arise from the sluggishness with which a lamp filament follows a change in voltage, and in the case of some voltmeters from the fact that they are far from dead heat.

It is very often the custom to reverse one of the voltage components so that zero phase difference is indicated with a maximum resultant and not with a zero resultant; or, in other words, the two E.M.F. components have a phase difference of 180° instead of 0° when synchronism exists between the two armatures. The gain in sensitiveness, however, is slight, for the reason that at and near the point of maximum resultant voltage it requires a large phase change to cause a noticeable resultant voltage change. A decrease of one per cent. for instance, from the maximum resultant voltage occurs only when the two components have departed from their in phase position by nearly 20.°

A very ingenious method has been devised by Mr. J. E.

Woodbridge, which overcomes the lack in sensitiveness inherent in the methods described. Woodbridge's method is applicable only to polyphase alternators, and particularly to three phasers. The method consists of combining two E.M.F.'s which have a phase difference other than 0° or 180° when the alternator armatures are in synchronism. Thus in a three-phase system two E.M.F.'s are combined whose phase angle is 60° at the instant the alternators are in synchronism. The resultant voltage is equal to each of the components when these components are equal, and to nearly an average when they differ slightly. This modification not only brings the voltmeter reading at synchronism within the sensitive range of the voltmeter but also causes synchronism to occur at a period in the cycle of phase change where variations in the phase angle are proportioned to variations in voltage. The objection to the method is that there are two points in the cycle of phase change where a voltmeter thus connected will indicate apparent synchronism. One of these points is true synchronism and the other 120° from synchronism. It becomes necessary, therefore, to provide an additional indicator that will tell which of these two points is the correct one.

All the foregoing methods employ a device sensitive to change in voltage for the real purpose of measuring a phase relation. The methods are indirect, in that they indicate resultant voltage which is dependent upon phase relations and do not attempt to indicate the phase relation itself. My device attacks the problem directly, being a true phase indicator and is independent of relative values of the two E.M.F.'s, whose phase relation it indicates. Outwardly this synchronizer consists of a hand which revolves around a dial, just as the hand of a clock revolves around its dial. *Fig. 1* gives a view of the instrument. When in operation the angle between the hand and its printed image is always equal to the phase angle between the E.M.F. of the bus bars and that of the incoming alternator. If the incoming alternator is running too fast the hand rotates clockwise, and if too slow counter-clockwise. One revolution of the hand clockwise, therefore, means a gain of one cycle by the incoming alternator over the bus bars; while one revolution counter-clockwise indicates a loss of one cycle. In other words, a single revolution clockwise or counter-clockwise indicates that the incoming alternator is relatively one pair of poles ahead of or behind the alternators with which it is to be paralleled. The following description of the instrument is an extract from a paper read by me before

FIG. 1.

the American Institute of Electrical Engineers, August 21, 1901:

"Suppose a stationary coil F has suspended within it a coil A, free to move about an axis in the planes of both coils and including a diameter of each. If an alternating current be passed through both coils, A will take up a position with its plane parallel to F. If now the currents in A and F be reversed with respect to each other, coil A will take up a position 180° from its former position. Reversal of the relative directions of currents in A and F is equivalent to changing their phase relations by 180°, and, therefore, this change of 180° in phase relations is followed by a corresponding change of 180° in their mechanical relations. Suppose, now, that instead of reversing the relative direction of currents in A and F, the change in phase relations between them be made gradually and without disturbing the current strength in either coil. It is evident that when the phase difference between A and F reaches 90° the force between A and F will become reduced to zero and a movable system, of which A may be made a part, is in condition to take up any position demanded by any other force. Let a second member of this movable system consist of coil B, which may be fastened rigidly to coil A, with its plane 90° from that of coil A, and the axis of A passing through a diameter of B. Further, suppose a current to circulate through B whose difference in phase relative to that in A is always 90°. It is evident under these conditions that when the difference in phase between A and F is 90°, the movable system will take up a position such that B is a parallel to F, because the force between A and F is zero, and the force between B and F is a maximum; similarly when the difference in phase between B and F is 90°, A will be parallel to F. That is, beginning with a phase difference between A and F of O, a phase change of 90° will be followed by a mechanical change in the movable system of 90° and each successive change of 90° in phase will be followed by a corresponding mechanical change of 90°. To investigate the mechanical relations corresponding to intermediate phase relations, suppose the phase difference between A and F to be O. The force between A and F will then be a maximum, and that between B and F, O. Now, suppose a small change in phase between A and F to take place. The force between A and F becomes slightly less than the maximum, and that between B and F becomes other than O. The movable system will take up the position of equilibrium between forces on A and B, which will be somewhere intermediate between A parallel to F, and B parallel to F. It is further evident that the direction of motion of the movable system depends upon the sign of the differential change, so that such a device is not only responsive to a change of phase between A and F, but is also responsive to the direction of that change.

"It may be interesting to note that quadrature between A and B is not absolutely necessary either in the mechanical angle between A and B, or in the phase angle between their currents. So long as there is a displacement between A and B, both mechanical and electrical, the device is responsive to phase changes between A and F.

"The above is a brief description of the principles upon which rests this new synchronism indicator. Connect coil F to a set of bus bars, and coils A and B to a dynamo that is to be parallel to those bus bars, and in the motion of movable coils there is a constant index of the phase condition of the dynamo with respect to the bus bars. . . .

"In the commercial construction of the device a laminated iron magnetic circuit is used and the phase difference between A and B is secured by using a non-inductive resistance in series with one coil, and an inductance in series with the other. It is, of course, impossible to secure 90° phase difference in this manner, but sufficient difference can be obtained to answer all practical purposes. Current is introduced into the armature through three slip rings, one of the rings being made to do duty for one end of both armature windings.

The ideal synchronizing device should perform three functions.

(1) It should tell the amount of difference in frequency.

(2) It should tell whether the machine being synchronized is running too fast or too slow.

(3) It should tell the exact point of synchronism.

Comparing the above described device with devices now ordinarily in use for the purpose of synchronizing, in regard to these criteria, we find :

(1) In regard to the first point, no difference obtains.

(2) In regard to the second point, the new device gives complete information, while ordinary devices give none.

(3) In regard to the third and most vital point, usual devices give only partial information, while that given by the new device is complete not only at the point of snychronism, but at every other point in the cycle."

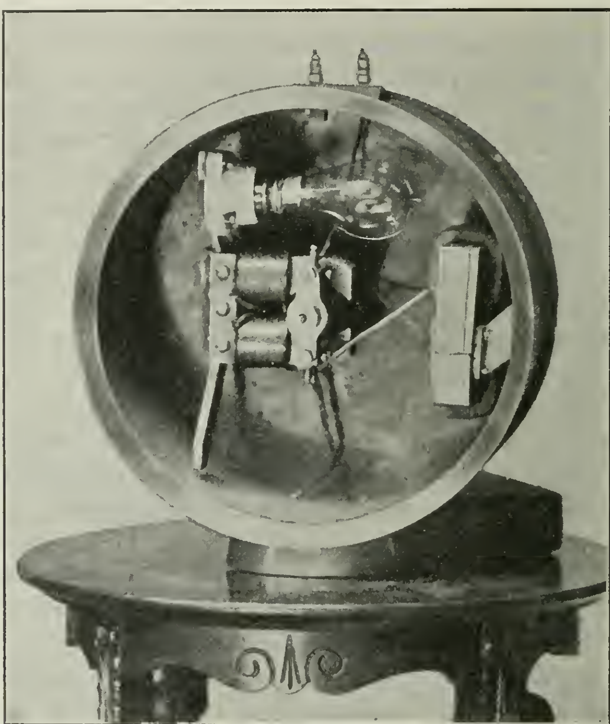

Fig. 2.

Fig. 2 shows a view of the synchronizer with dial removed. The lamp serves the double purpose of illuminating the translucent dial and of acting as the dead resistance in series with one of the armature elements.. The inductive resistance in series with the other armature element is shown at the right. The field of the instrument, corresponding to coil *F* in the preceding description, is shown in the center. The armature is swung between its pole-pieces.

The experience with this device in connection with the alternators of the Niagara Falls Power Company has been most gratifying. The record performance has been the paralleling of five 5,000-horse-power dynamos in three minutes—a feat that under former conditions would have been quickly done in fifteen or twenty minutes.

The present tendency toward large unit and stiff regulation demands that the indication of synchronism for paralleling shall be done by something other than a makeshift, as has been largely the case in the past. Whether it be the device described above, or some other, the time has come when makeshifts should be discarded and a device better adapted to the purpose used for synchronism indication.

THE ENERGY OF THE UNIVERSE.

In a paper on the energy of the universe, in the *Revue Scientifique*, M. L. Skvortzow discusses the influence of electrical phenomena in cosmogony. He considers that in the past history of the earth, and of other celestial bodies, electrical and chemical energy have originally played the most important part, and that heat energy has become more and more important in proportion as the earth has assumed a more material form, so that the more its energy has passed from the dynamic to the static form the greater has been the absorption of dynamical energy in overcoming resistances. The heat of the earth M. Skvortzow attributes to electric currents circulating mostly near the surface; the interior of the earth, on the other hand, he thinks, may be as cold as the greatest depths of the ocean. Changes in the aspect of the earth, as well as meteorological phenomena, are attributed to electric currents induced by solar influence. The temperatures of different planets are considered to depend less on their distance from the sun than on their reserve of energy and on the currents which the sun induces in them in virtue of their axial and orbital motions. Will this theory of the electro-magnetic origin of the earth's heat reconcile the two opposing views on the age of the earth?

W.

INVENTION OF WIRELESS TELEGRAPHY.

According to a press cablegram, Marconi's claims to the invention of wireless telegraphy were severely criticised at a recent meeting of the French Academy of Sciences. According to documentary evidence adduced credit for the invention is due, first, to Feddersen and Maxwell, both Englishmen; then to Hertz, of Germany; but principally to Professor Branly, a Frenchman, who conceived and constructed in 1890 the receiver for electric waves which is used by Marconi; and next to Professsor Lodge, an Englishman, who read a paper before the Electrical Congress of 1894 pointing out the possibility of transmitting telegraphic signals with the Hertz apparatus and receiving them with the Branly tube of metallic fillings, which possibility was actually put into practice by the Russian scientist, Popoff, in 1896. It was further asserted that most of Marconi's patents are worthless, owing to the foregoing facts. Finally, it was pointed out that neither the French nor the German Army nor the English Navy is using the Marconi system, though results are obtained equal to those obtained with his apparatus, except transmission across the ocean, which the scientists unanimously decided is yet unproved.—*Elec. World*.

NEW FRENCH PATENT REGULATIONS.

Hilary S. Brunot, Consul at St. Etienne, reports the following regarding the new patent regulation in France, which took effect early this year :

In order that a patent may be obtained in France, the three following conditions are necessary: That the invention be absolutely new ; that it possesses an industrial character ; that it be not contrary to public order or security, good morals, or the laws of the country. An invention is not considered new when, previous to the date of filing of the application, it has received sufficient publicity in France or abroad to render it easy of imitation.

The patents are not guaranteed by the Government, which delivers them at the risk and peril of the applicant ; there may be noticed on thousands of patented inventions the letters "S. G. D. G.," meaning "without the guaranty of the Government."

The cost of a French patent is 500 francs ($96.50) for five years ; 1,000 francs ($193) for ten years ; and 1,500 francs ($289.50) for fifteen years. This tax is paid in annuities of 100 francs ($19.30) in advance.

To obtain a patent every inventor must file at the prefecture of his department.

"*a*. An application to the Minister of Commerce and Industry in which is mentioned the subject of his invention as well as the desired duration of the patent.

"*b*. A complete description of the invention in duplicate.

"*c*. Drawings or specimens necessary to the understanding of the description, also in duplicate.

"*d*. A list of the papers thus filed."

The decree above alluded to concerns exclusively the drawings and descriptions of the inventions. The first article prescribes that the descriptions must be written with the pen in a clear hand, or printed on paper 33 centimeters long by 21 broad ($12\frac{3}{4}$ x 8 inches), leaving a margin of 4 centimeters ($1\frac{1}{2}$ inches). One side of the paper only must be used, and no design is allowed in the text. The second article refers to the drawings.

For the first six months of 1902 the decree will not be applied in an absolute manner.

The courts of Lyons have recently rendered a decision interesting to patentees. By the law of 1844 the person who has obtained a patent must work his invention in France within two years after the date of patent or he will lose his rights.

The courts have decided that the working must be real, and that publicity due to the cession of the patent to another party is not sufficient. W.

ELECTRICAL SECTION.

Stated Meeting, held Thursday, March 2, 1902.

ELECTROCHEMICAL POLARIZATION.

By C. J. Reed.

If the conceptions of different individuals are indicated by the definitions they give of a phenomenon, it must be admitted that there is not at the present time any very definite conception of electrochemical polarization.

Before discussing the phenomenon of polarization itself, it will be interesting to consider a few of the definitions given by various authorities.

Wetham defines it as "A peculiar condition of platinum plates." According to this definition it would not be difficult to avoid polarization in a galvanic battery, as it would only be necessary to avoid platinum plates.

Neuman defines it as "Increased resistance of the cell and consequent falling off of the current." According to this view, polarization could be avoided whether the plates be of platinum or not, by merely increasing the surface of the plates and reducing the distance between them.

McMillan says, "Polarization is hydrogen films on the surface of the negative strip."

Park Benjamin says, "Polarization is a condition due to the formation of a body, most commonly hydrogen, by electrochemical decomposition upon the negative electrode, whereby a current in opposite direction to the normal current of the cell is produced, and through which the normal current may be greatly weakened. To remove this body by any suitable means, chemical or mechanical, is termed 'depolarizing;' and if a chemical agent is employed for the purpose, it is called a 'depolarizer.'"

Neuberger says, "By polarization is meant any phenomenon, or the sum of all phenomena, which require an increase of voltage during electrolysis. These phenomena are (*a*) deposition of gases at the electrodes whereby the gases cause an opposite current through the electrolyte; (*b*) the resistance of very dense layers of gas between poles and electrolyte; (*c*) changes of concentration in the electrolyte; (*d*) changes of temperature."

Grawinkel and Strecker define it as follows: "If the condition of the electrolyte or of the electrode is changed, an electromotive force is produced in the electrolytic cell at the electrodes, counteracting the external electromotive force and, therefore, diminishing the current. This phenomenon is called polarization, and the electromotive force produced in the cell is called the electromotive force of polarization or the counter-electromotive force.

The following is given by Prof. Houston: "The collection of a gas, generally hydrogen, on the surface of the negative element of a voltaic cell." "The collection of a positive substance like hydrogen on the negative element or plate of a voltaic cell sets up a counter-electromotive force, which tends to produce a current in the opposite direction to that produced by the cell.

"Polarization causes a decrease in the normal current of a cell:

"(1) On account of the increased resistance of the cell from the bubbles of gas which form part of its circuit.

"(2) On account of the counter-electromotive force produced by polarization.

"There are three ways in which the ill-effects of polarization of voltaic cell can be avoided.

"(1) *Mechanical.*—The negative plate is furnished with a roughened surface, which enables the bubbles of gas to escape from the points on such surface; or a stream of gas or air is blown through the liquid against the plate and thus mechanically brushes the bubbles off.

"(2) *Chemical.*—The surface of the negative is surrounded by some powerful oxidizing substance such as H_2CrO_4 or HNO_3, which is capable of oxidizing the hydrogen and thus thoroughly removing it from the plate.

"(3) *Electro-Chemical.*—The negative element is immersed in a solution of a salt of the same metal as that forming the negative plate. Thus a copper plate in copper sulphate cannot be polarized, since metallic copper is deposited on its surface by the action of the hydrogen which tends to be liberated."

We see from these definitions that polarization is a condition, a substance, a resistance, an increase of resistance, an electromotive force, a counter-electromotive force, an obstructing body, a film of hydrogen, a gas, a collection of positive substances, bubbles of gas, the resistance of a dense layer of gas, a change of concentration, a change of temperature, a current opposing a normal current, the counteracting or diminishing of an external current, a decrease in a normal current, or the sum of all these things or phenomena.

Most authors discuss polarization without attempting to define it. This is fortunate. Otherwise we should probably be obliged to select our definition from a much larger assortment of material and immaterial than that given above.

It is evident that different persons have had different things or phenomena in view when they defined polarization. Some, for example, evidently have had the ammeter in view and noticed a diminution of electric current; others have had a voltmeter in view and noted a change in potential difference; others have had the negative electrode in view and saw bubbles of gas rising from it; while some have had a thermometer or hydrometer in view and noted a change of temperature or concentration. Some have evidently had in view a galvanic battery evolving electrical energy, while others had in view an electrochemical cell absorbing energy.

It is quite apparent that at least a part of the confusion manifested in the above definitions is due to mistaking coincidence for cause and to the fact that theoretical assumption has been more convenient than the ascertaining of facts.

It is plain, from the definitions quoted above, that polarization is a phenomenon and not a substance. It is also evidently a phenomenon accompanying some kind of change in the condition of an electrochemical cell. An examination of the facts shows that this change of condition is invariably the result of a single cause. This cause is the passage of an electric current through the cell and not the formation of hydrogen or of any other substance. Polarization never occurs, except as a result of the passage of current, and there is no electrochemical cell in which the passage of a current does not tend to produce this changed condition, with which so many have struggled in attempts to frame a definition. The action of the current itself is always the cause of the changed condition, and the result of the passage of a current is always a change in the potential difference at the terminals of the electrochemical system during the passage of the current. This change in the potential difference is frequently in the direction opposed to the current flowing, but not always. The evolution of hydrogen and other substances at the cathode sometimes occurs simultaneously with polarization, but not always. Whether these substances are liberated or not depends altogether on the nature of the electrochemical reagents used, the speed of electrochemical reaction, the presence of reactions not electrochemical, the temperature and other conditions.

Faraday established the fact, and it has been verified many times since, that the passage of an electric current through an electrochemical cell is accompanied by electrochemical changes in the cell which are proportional to the quantity of electricity that passes. The inevitable effect of a current must be, therefore, to exhaust or destroy the chemical condition or composition of the cell as it was originally composed, and to produce a cell of different composition having different electromotive forces, resistances or concentrations. An apparent exception to this statement is found in the case of a cell in which the substances destroyed by electrochemical action at one electrode are produced at the other, and in which these substances are rapidly transported by some means from one electrode to the other. And it is only in such cases that there is apparently no polarization.

There is an intimate connection between the progressive electrochemical changes in a cell and its progressive polarization, which demonstratively points to the conclusion that polarization is most correctly defined as *a progressive change in the composition and electromotive force of an electrochemical system necessitated by the progressive exhaustion of one or more of the electrochemical reagents.*

All of the so-called phenomena of polarization are merely phenomena due to electrochemical exhaustion; that is, phenomena due to the change or substitution of a new electrochemical system for a preceding one.

To make this proposition clear by an illustration, certain electrochemical systems in action evolve hydrogen gas at the cathode; others do not. If the passage of an electric current through a system in which hydrogen is not evolved causes chemical changes which progressively convert the system into one evolving hydrogen, the ultimate result must be the evolution of hydrogen gas at the cathode.

In such a case the hydrogen should not be assumed to be the cause of polarization. It is the progressive polarization or exhaustion of the original electrochemical system which converts it into a new system, and one of the products of electrochemical change in this new system is hydrogen. In other words, it is polarization that causes the hydrogen, instead of hydrogen causing the polarization or being the polarization.

The evidence in support of these conclusions becomes manifest when we examine carefully the intimate relation existing between the energy evolved or absorbed in electrochemical reactions and the phenomena of polarization.

Electrochemical cells may be conveniently divided with reference to the energy of their electrochemical reactions into two classes—symmetrical and asymmetrical.

Symmetrical cells are those in which, when a current passes in either direction, the sum of the electrochemical reactions is zero and there is neither chemical nor electrical energy generated at the terminals of the cell by electrochemical action. Or, we may say, the quantity of electrical energy converted into chemical energy at one electrode is equal to the quantity of chemical energy converted into electrical energy at the other electrode.

An aqueous solution, *Fig. 1*, of copper sulphate (of which the density is maintained in a uniform condition by rapid stirring) containing two copper electrodes, constitutes such a cell. The passage of a current in either direction causes copper to dissolve at the anode and an equal amount to be deposited at the cathode; the total amount of copper sulphate is the same in the final as in the initial state, or the sum of the electrochemical reactions is zero. The chemical energy of the copper dissolved at the anode is added to the energy of the circuit as electrical energy, and an equal amount of electrical energy is taken from the circuit at the cathode and converted into chemical energy by the deposition of the metallic copper. For cells of this class I have elsewhere

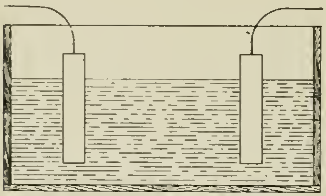

FIG. 1.

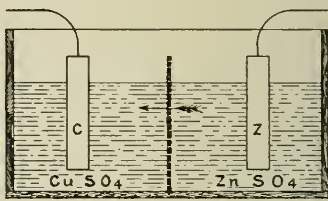

FIG. 2.

suggested the name of *agenic*, because neither electrical nor chemical energy is generated by conversion of one into the other between the terminals of the cell. Symmetry of structure or appearance does not constitute electrochemical symmetry. For instance, a solution of sulphuric acid containing two exactly similar platinum or carbon electrodes would not constitute a symmetrical cell. The passage of a current through such a cell, instead of producing at one electrode the substance consumed at the other, produces oxygen at one electrode and hydrogen at the other, and causes a continuous transformation of electrical into chemcal energy. A symmetrical electrochemical system must contain at the beginning a substance which is produced at one electrode and destroyed at the other by the electrochemical action.

An asymmetrical electrochemical system is one in which the passage of a current in one direction causes electrical energy to be converted into chemical energy, and the passage of a current in the opposite direction converts chemical energy into electrical.

A simple asymmetrical cell is shown in *Fig. 2*, in which *c* represents a copper electrode surrounded by copper sulphate, and *z*, a zinc electrode, surrounded by zinc sulphate, the two sulphates being separated by a porous partition. A current passing from *z* to *c* through the electrolyte dissolves the zinc at *z* and deposits copper at *c*. The dissolving of the zinc converts its chemical energy into electrical energy at *z*, and the deposition of copper converts electrical energy into chemical at *c*. But the formation energy of the zinc sulphate is greater than that of the copper sulphate. Hence, the electrical energy added to the circuit is greater than the chemical energy abstracted from it, and the cell generates electrical energy or is electrogenic. But as this action proceeds, the quantity of the copper sulphate is continually diminishing and will finally disappear altogether, while the quantity of zinc sulphate is continually increasing, and will eventually entirely replace the copper sulphate. As the current continues, the original asymmetrical system is progressively converted into a symmetrical system, and eventually zinc will be dissolved at *z* and deposited at *c*.

During the change in the composition of the cell; that is, during the exhaustion of copper sulphate, there is a simultaneous progressive change in the potential difference at the terminals of the cell. At the beginning, when the copper sulphate is abundant, there is an electromotive force of 1·09 volts and a potential difference of about 1 volt with a moderate current. But if the current be maintained constant, the potential difference will fall as the copper sulphate diminishes, and will become zero before copper sulphate is entirely exhausted. During this process the zinc sulphate gradually makes its way through the porous partition by diffusion, convection and molecular interchange and reaches *c*, replacing the copper sulphate.

It is this gradual reduction in the potential difference at the terminals that constitutes the phenomena of polarization in this case, and it is evidently the necessary result of the change in chemical composition described. The substitution of the zinc sulphate for the copper sulphate is not, however, the only substitution that takes place. Water being always present in such a cell, and being more easily decomposed than zinc sulphate, begins to transmit a part of the current as soon as the copper sulphate is insufficient to transmit the current, and by its decomposition deposits hydrogen instead of copper. This continues to some extent even when there is sufficient zinc sulphate to transmit the total current.

If, however, a current be passed through the cell shown in *Fig. 2* in the opposite direction, viz., from c to z through the electrolytes, the copper will be dissolved at c and its chemical energy converted into electrical, at that point, while zinc will be deposited from solution at z and its chemical energy restored at the expense of electrical energy taken from the circuit at that point. As the formation energy of the zinc sulphate is greater than that of the copper sulphate, more electrical energy will be taken from the circuit at z in depositing the zinc than that which is added to the circuit at c by the dissolving of the copper. Hence, there will be a counter-electromotive force between the terminals of the cell opposed to the current. If a constant current be maintained through the cell in this direction, this counter-electromotive force will be 1·09 volts and the potential difference between the terminals will exceed this value so long as zinc sulphate is the only substance decomposed at z. But when some of the copper sulphate by diffusion, convection, molecular exchange or otherwise, arrives at z, it will be decomposed in preference to the zinc sulphate, as this requires less energy than the zinc sulphate for its decomposition. Only a small quantity of copper sulphate reaches z at first, which is sufficient to transmit only a small fraction of the total current, the balance still finding its way out of the electrolyte through the decomposition of the zinc sulphate. Consequently, the counter-electromotive force and the potential difference are resultants of that due to the copper and that due to the zinc. This resultant potential difference and electromotive force must be less than that due to the zinc and greater than that due to the copper, its value varying with the relative quantities of current transmitted by the copper sulphate and by the zinc sulphate. Hence, when copper sulphate first reaches *z*, that is, when the system first begins to change from an asymmetrical to a symmetrical system, polarization sets in and the counter-electromotive force and potential difference begin to fall. As the copper sulphate increases and the zinc diminishes at *z* until the copper sulphate is in sufficient quantity to transmit the total current, the counter-electromotive force diminishes *pari passu* to zero and the potential difference nearly to zero. The cell has now been entirely changed from an asymmetrical to a symmetrical system by the substitution of the copper sulphate for the zinc sulphate, or by the total exhaustion of the zinc sulphate as the electrolytic medium of transmission for the current. The fact that some zinc sulphate still remains as an inert body in solution does not alter the case, but merely proves or illustrates the fact that when the zinc sulphate and the copper sulphate are equally accessible to the cathode in quantities sufficient to transmit the total current, the compound requiring the least absorption of electrical energy will be decomposed to the total exclusion of the one requiring greater energy.

This selective tendency of a current to be transmitted by a compound requiring the minimum absorption or maximum evolution of electrical energy in preference to other equally accessible compounds, has already been described before this Institute as the *law of maximum electrogenesis.** The recognition of this law makes it evident in a convincing manner that in every case the phenomena of polarization are merely phenomena of electrochemical exhaustion as described above.

Returning to the cell shown in *Fig. 2*, considered in its initial state, when the current passes with the arrow from

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* *Jour. Frank. Inst.*, May, 1901.

z to c, the system is evolving electrical and exhausting chemical energy. It is a galvanic battery in the process o discharging and furnishing electrical energy. Its polarization; that is, the exhaustion of the copper sulphate, causes it to become less and less efficient as a battery. Its electromotive force falls in time nearly to zero. But if the current be maintained by an external source after the copper sulphate has been exhausted, water will then be decomposed and hydrogen will be deposited instead of copper, and zinc oxide will be formed at z instead of zinc sulphate. This will commence when the potential difference falls to .34 volt.

Finally zinc and hydrogen are both deposited and the cell ceases to act as a battery, both electrodes being now of zinc. But if the original current is from c to z instead of from z to c, the system absorbs electrical and accumulates chemical energy. It is an accumulator in process of charging and continues to act as such until the copper sulphate reaches z. It then becomes less and less efficient as an accumulator, until its polarization, that is the exhaustion of its zinc sulphate, changes it completely to a symmetrical system, dissolving copper at one electrode and depositing it at the other. It now ceases altogether to act as an accumulator, ceases to convert electrical into chemical energy. In other words, it becomes an agenic cell, identical with that shown in Fig. 1, when the concentrations are the same.

In the brief time allotted we have been able to consider in detail only a single case, but the same process of investigation may be applied to all electrochemical systems without exception, and in every case the conclusion is the same, viz., that polarization is merely the progressive change described above.

——A CURIOUS ACCIDENT occurred in Boston a few days ago, resulting, it is stated, in considerable damage to the electrical equipment of the Boston Elevated Railway. A woman, evidently on a shopping tour, was carrying home two brass curtain rods, one of which dropped on the track at the Sullivan Square Terminal. It fell across the third rail and one of the traction rails, and short circuited the system. Considerable insulation was destroyed and traffic was interrupted for some time.

PHYSICAL SECTION.

Stated Meeting, held February 27, 1901.

ON THE HARMONIC CURVES KNOWN AS LISSAJOUS FIGURES.

BY HORACE C. RICHARDS.

Of all types of finite motion the simplest, as well as the most interesting, is that known as simple harmonic motion; the projection of uniform circular motion on a line in its plane. It is simpler than circular motion, for it is a component of that motion, and is confined to a straight line. It is simpler even than uniform rectilinear motion, for its reversal of direction involves no discontinuity of velocity. And to its simplicity is added an æsthetic charm. Alike in observing the reciprocating motions of an engine, the oscillations of a pendulum, and the majestic sweep of the tides, we feel a distinct sense of pleasure in the appreciation of this graceful rhythm.

It is, moreover, a type of motion most important from the physical point of view. Its chief property may be stated in the law that the acceleration is always directed toward the center of the motion and is proportional to the distance from that center. But this is the law of the motion of bodies under the action of an elastic force; and consequently we have to deal with this form of motion not only in those slow oscillations of elastic bodies which may be followed by the eye, but also in the more rapid vibrations met with in acoustical phenomena. The electrical waves emitted by an alternating dynamo and the vastly more minute electromagnetic waves which constitute light furnish other physical examples of the harmonic law.

It is on account of this importance of harmonic motion in physics that so much significance is attached to the law known as Fourier's theorem: that any periodic motion may be resolved into harmonic constituents. From the purely

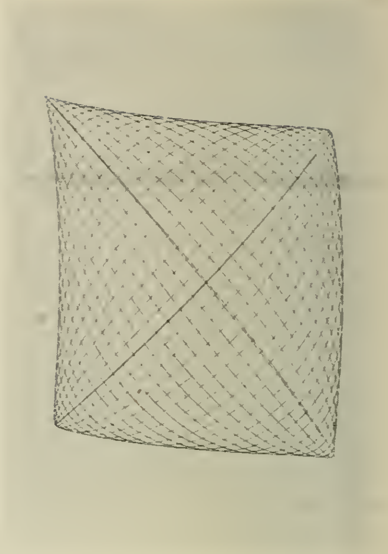
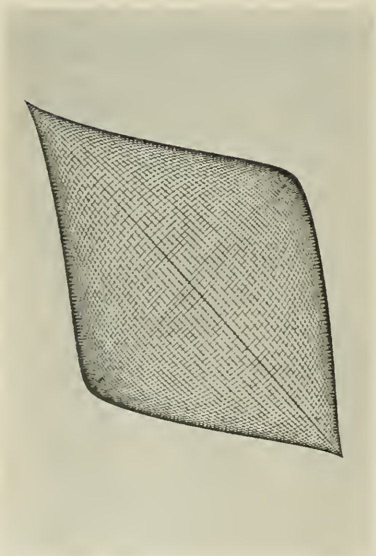


FIG. 1. FIG. 2.



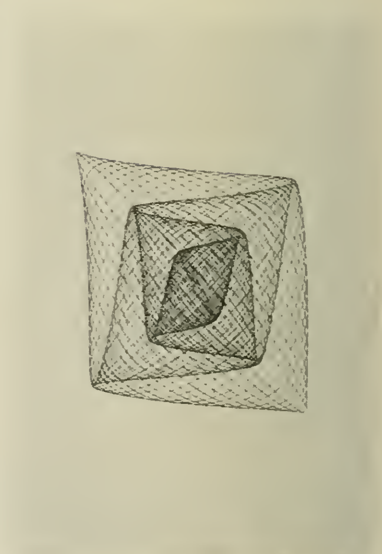


FIG. 3. FIG. 4.

PLATE I.

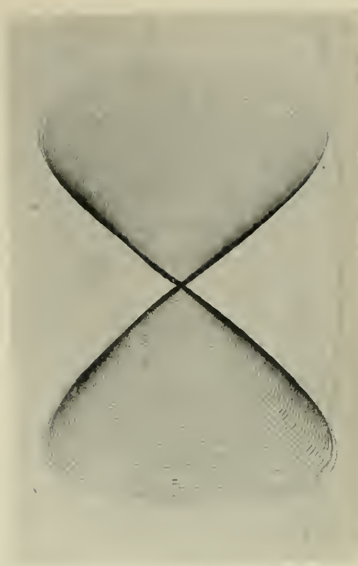


Fig. 5.

Fig. 6.

Fig. 7.

Fig. 8.

PLATE II.

mathematical point of view, other resolutions are possible; but they do not correspond to physically independent motions.

The converse of Fourier's problem—the combination of a number of harmonic motions in the same line—has been experimentally exhibited in the tide-predicting machine of Lord Kelvin, and much more completely in the harmonograph constructed by Michelson. The latter instrument, a description of which may be found in the *American Journal of Science* for 1898,* combines as many as eighty harmonic constituents; the resultant motion being exhibited graphically by combining it with a uniform motion at right angles to its own direction.

The curves to be considered in this paper, however, are of a different character. They are formed by the composition of two harmonic motions inclined to each other. These curves were first discussed by Bowditch in 1815 †, but as they were first brought into prominence forty years later by the beautiful acoustical experiments of Lissajous, they are generally known as Lissajous figures. They may be produced by any device capable of compounding two harmonic motions, such as a pendulum swinging in two planes with different periods (Dean, Blackburn), a beam of light reflected successively from two tuning-forks (Lissajous), a rod of rectangular or elliptic section vibrating laterally (Wheatstone), or a tracing-point governed by two pendulums which oscillate in planes inclined to each other (Tisley). It is by the last process that the curves illustrating this paper were drawn.

The character of the curve depends mainly on the relation of the periods of the components. Some general properties may however be deduced. Representing by a and b the amplitudes of the component motions, T_1 and T_2 their periods, and writing

$$\omega_1 = \frac{2\pi}{T_1}, \ \omega_2 = \frac{2\pi}{T_2},$$

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* *Am. Jour. Sci.* (4) **5** p. 1 (1898). Also *Phil. Mag.* (5) **45** p. 85 (1898).
† Mem. Acad. Arts and Sci. [1] **3**, pp. 241, 413 (1815).

the curve in general may be represented by

$$\text{I.} \qquad \begin{cases} x = a \cos \omega_1 t, \\ y = b \cos (\omega_2 t - e), \end{cases}$$

e being the possible difference of phase. It is evident that the curve is always contained in the parallelogram $x = \pm a$, $y = \pm b$. If, as is usual, we take rectangular axes, the inclosing parallelogram becomes a rectangle.

If T_1 and T_2 are incommensurable, x and y will never simultaneously return to their original phases, so the curve will have no period. Otherwise, if the ratio of the periods, reduced to its lowest terms, be $n : m$, the initial phases will first recur together after a time

$$T = m\ T_1 = n\ T_2.$$

The curve will therefore have a period which is the least common multiple of the component periods. In this case, which is the most interesting, we may write equations I

$$\text{II.} \qquad \begin{cases} x = a \cos m\ \omega\ t, \\ y = b \cos (n\ \omega\ t - e), \end{cases}$$

where

$$\omega = \frac{2\ \pi}{T}.$$

From these equations we see that in each period x and y take their maximum values m and n times respectively. In other words, the curve will have m loops along the sides $x = \pm a$ and n loops along the sides $y = \pm b$ of the parallelogram. This property furnishes a ready means of determining the ratio of the components from an inspection of the curve. It must be noticed, however, that in certain special cases, where the curve retraces itself backward in the second half of its period, each loop must be counted twice and each free end as a single loop. Examples of this may be noted in *Figs. 5, 9, 13*, etc.

The curves possess some general properties of symmetry which may be readily deduced. Since the ratio $n : m$ is in its lowest terms, we may without loss of generality take m always odd. Consider the position of the tracing point one half period after the time t. We find this by substituting for t in equations II

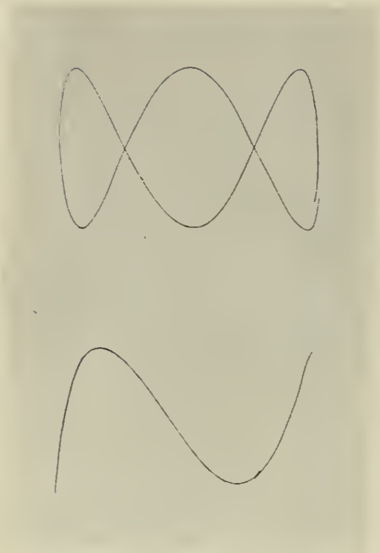

FIG. 9.

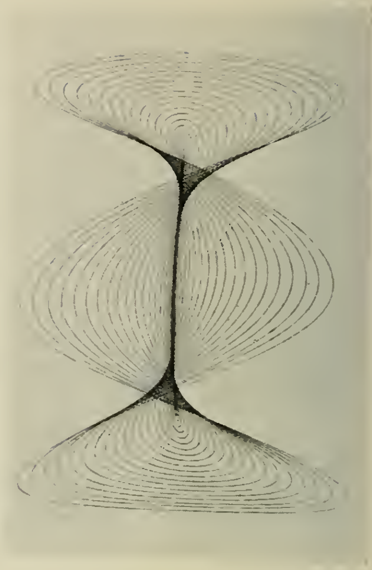

FIG. 10.

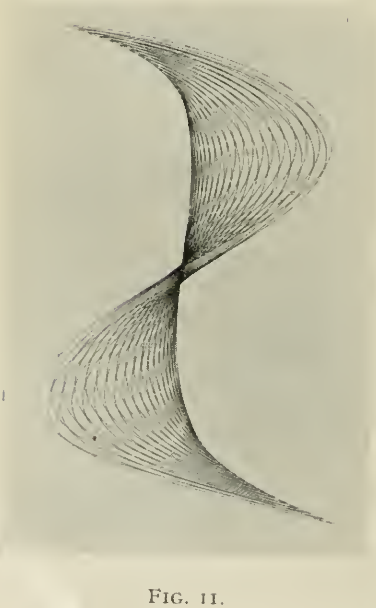

FIG. 11.

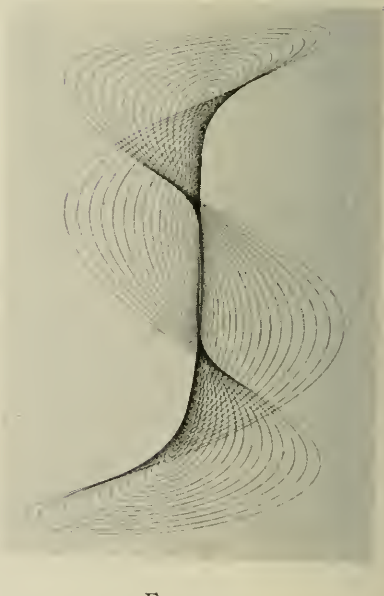

FIG. 12.

PLATE III.

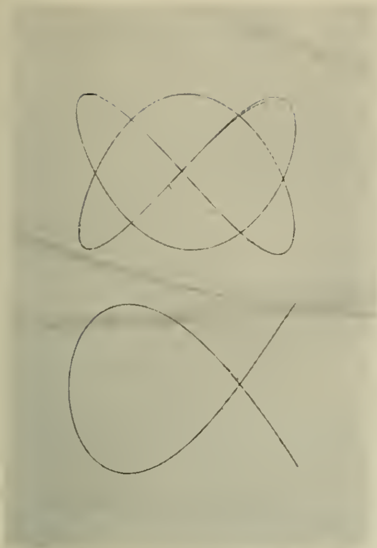

Fig. 13.

Fig. 14.

Fig. 15.

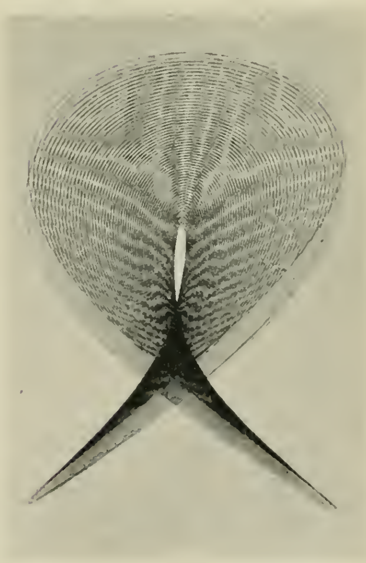

Fig. 16.

PLATE IV.

$$t' = t + \frac{1}{2} T = t + \frac{\pi}{\omega}$$

which gives

$$x' = a \cos (m \omega t + m \pi) = a \cos m \omega t \cos m \pi,$$

$$y' = b \cos (n \omega t - e + n \pi) = b \cos (n \omega t - e) \cos n\pi.$$

Now as m is odd, x' always equals $-x$; while y' is equal to $+y$ or $-y$, according as n is even or odd. We may therefore distinguish two classes of curves: (1) If n is even, corresponding to every point (x, y), there is a point $(-x, y)$, traced half a period later; in other words, the curve is symmetrical about the axis of y (the *even* axis). (2) If n is odd, corresponding to each point (x, y), there is a point $(-x, -y)$, or the curve is symmetrical about its center, but not in general about either axis.

Cases of especial interest occur when e takes the values 0 and $\frac{\pi}{2}$. These cases are known as the end phase and middle phase of the curve. (If e equals π or $\frac{3\pi}{2}$ we have merely a reversal of these curves.)

(1) *End Phase:* $e = 0$. Equations II become

III. $$\begin{cases} x = a \cos m \omega t, \\ y = b \cos n \omega t. \end{cases}$$

Consider now the position of the tracing point at a time t before the end of the period. This we find by substituting for t in equations III,

$$t'' = T - t = \frac{2\pi}{\omega} - t;$$

which gives

$$x'' = a \cos (2 \pi m - m \omega t) = a \cos m \omega t = x.$$

$$y'' = b \cos (2 \pi n - n \omega t) = b \cos n \omega t = y.$$

It follows that in the second half of the period the curve retraces backward the path described in the first half. It is no longer closed but has free ends, which are at adjacent or opposite corners of the rectangle according as n is even or odd. It has no higher symmetry than in the general case.

(2) *Middle Phase:* $e = \frac{\pi}{2}.$ Equations II become

$$\text{IV.} \qquad \begin{cases} x = a \cos m\, \omega\, t, \\ y = b \sin n\, \omega\, t. \end{cases}$$

Making the same substitution for t of $T - t$, we obtain

$$x'' = a \cos (2\,\pi\, m - m\, \omega\, t) = a \cos m\, \omega\, t = x.$$

$$y'' = b \sin (\,2\,\pi\, n - n\, \omega\, t) = - b \sin n\, \omega\, t = - y.$$

The curve is symmetrical about the axis of x. But we have seen that in general, if n is even the curve is symmetrical about the axis of y, while if n is odd it is symmetrical about the center. In all cases, therefore, the curve in its middle phase is symmetrical about both axes.

The curves corresponding to the two simplest ratios may easily be investigated more closely, If $m = 1$ and $n = 1$ we have the figure corresponding to unison. Equations II become

$$\text{V.} \qquad \begin{cases} x = a \cos\, \omega\, t, \\ y = b \cos (\omega\, t - e). \end{cases}$$

Eliminating $\omega\, t$ we obtain the equation of the path

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} - 2\frac{x\,y}{a\,b}\cos e = \sin^2 e,$$

which represents an ellipse. At the end phase it reduces to a straight line

$$\frac{x}{a} = \frac{y}{b},$$

while at the middle phase it becomes the symmetrical ellipse

$$\frac{x^2}{a^2} + \frac{y^2}{b^2} = 1.$$

The next ratio, that of the octave ($m = 1$, $n = 2$), is expressed by the equations

$$\text{VI.} \qquad \begin{cases} x = a \cos \omega\, t, \\ y = b \cos (2\, \omega\, t - e), \end{cases}$$

and eliminating $\omega\, t$ as before, we obtain the equation of the path

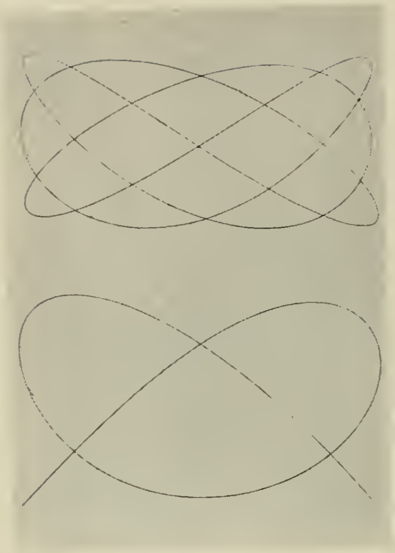

FIG. 17.

FIG. 18.

FIG. 19.

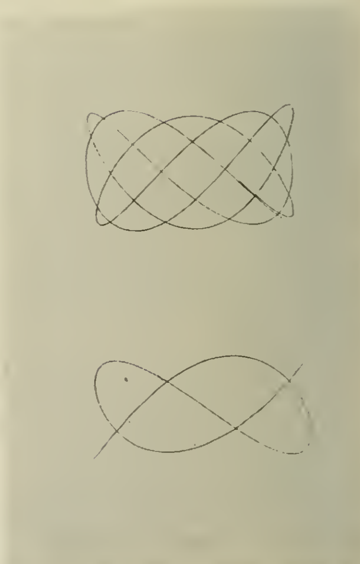

FIG. 20.

PLATE V.

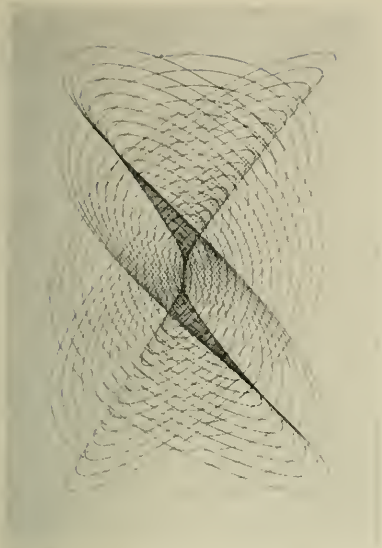

FIG. 21.

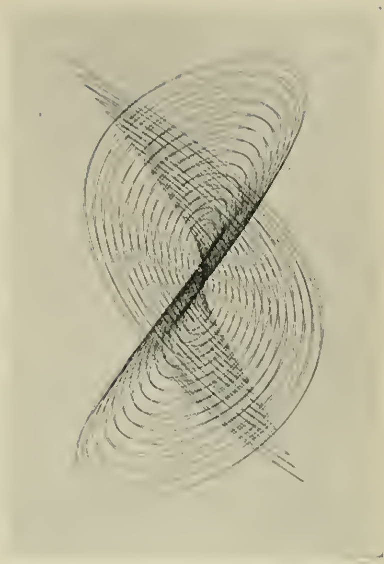

FIG. 22.

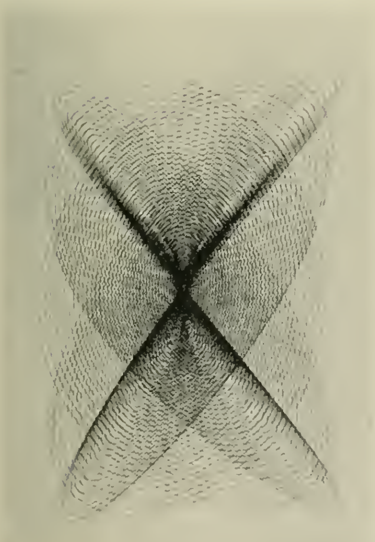

FIG. 23.

FIG. 24.

PLATE VI.

$$\frac{y}{b} = \left(2\frac{x^2}{a^2} - 1\right)\cos e + \left(2\frac{x}{a}\sqrt{1 - \frac{x^2}{a^2}}\right)\sin e$$

The general curve is of the fourth degree At the end phase it reduces to the parabola

$$\frac{y}{b} = 2\frac{x^2}{a^2} - 1,$$

and at the middle phase it becomes

$$\frac{y^2}{b^2} = 4\frac{x^2}{a^2}\left(1 - \frac{x^2}{a^2}\right),$$

a curve similar to the lemniscate and known as the hippopede. (See *Fig. 5*, Plate II). Curves corresponding to less simple ratios are much more complex and do not lend themselves readily to detailed analysis.

So far we have considered commensurable ratios. When the ratio is incommensurable, the curve, as before stated, is not periodic.

A case of interest occurs when the ratio of the periods is nearly that of two small integers. Writing this ratio $(n + a) : m$, where m and n are integers and a is a small fraction, and retaining the relation $\omega_1 = m\,\omega$, equations I may be written

$$\text{VII.} \qquad \begin{cases} x = a \cos m\,\omega\,t, \\ y = b \cos (n\,\omega\,t + a\,\omega\,t - e). \end{cases}$$

These correspond to equations II, except that the phase e is replaced by a phase varying with the time,

$$e' = e - a\,\omega\,t.$$

If a is so small that there is no appreciable change in this phase during the time $2\pi \div \omega$, the curve traced in that time will correspond to the ratio $n : m$; but as it is repeated the phase slowly changes, so that the various forms of the $n : m$ curve are successively traced. It may, therefore, be described briefly as an $n : m$ curve of variable phase. This type of curve is beautifully illustrated in Lissajous' experiment with two tuning-forks when the interval is not exact.

The curves illustrating this paper were drawn with the instrument known as the Tisley compound pendulum or harmonograph. It consists essentially of two heavy pendulums of adjustable period mounted so as to oscillate in perpendicular planes. A horizontal arm attached to the upper part of each pendulum and extending in the direction of vibration communicates its motion to a tracing point hinged at the intersection of these arms. When both pendulums are swinging, the point will receive a harmonic displacement from each pendulum, and the resultant curve will be of the character we have discussed. As the component motions are not perfectly independent nor strictly harmonic, there is a slight distortion in the curve, which is especially noticeable when the amplitude is large. The curve may be traced on paper by using a glass tube drawn to a capillary tip and filled with ink, or it may be traced on smoked glass with a needle-point.

If the tracing is allowed to continue after the end of a period, the curve will of course be repeated, but, on account of the loss of amplitude due to friction, the successive curves will be traced within one another, forming the graceful figures characteristic of the Tisley pendulum. Unless great care is taken to have the periods of the pendulums accurately commensurable, the gradual change of phase will alter the form of the curve, and often diminish the symmetry and consequent beauty of the resulting figure. If the components are "in tune," however, a slight deviation of the phase from zero will cause the lines to cross at a small angle and so produce the watered effect which enhances the beauty of the curves. A very slight change of phase will entirely alter the appearance of the watering. Examples of this may be seen in Plates II and IV.

The original curves, of which those on Plates I to VI are reversed copies,* were traced on smoked glass. The glass was first wiped with an oiled cloth so that the needle-point should sweep smoothly over the surface without chattering.

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* In the reproduction some of the fine detail is lost. The originals, of course, consist of fine sharp lines. In a few of the reproductions there is an additional effect of watering not on the originals, due to the interference between the lines of the figures and those of the process screens. This is most marked in *Fig. 1*, which in the original shows no trace of watering, being simply a long spiral.

It was then coated as evenly as possible with a film of soot from a small flame of burning camphor, the film being just thick enough to be opaque. It is important that the soot be of fine texture and not too coherent, otherwise it will be detached from the glass in flakes. Camphor smoke was found to be much more satisfactory than the smoke from gas or oil. The needle used was stout, so as not to bend, and had to be frequently sharpened to give the finest lines. The periods of the pendulums were adjusted first with heavy fixed weights, then with a smaller sliding weight on one, till the desired ratio was accurately secured.

Plate I illustrates the ratio 1 : 1 or *unison*. *Fig. 1* gives the middle phase, the tuning being practically perfect. The resulting curve is therefore a long spiral. *Fig. 2* illustrates a slight deviation from unison, the sliding weight being purposely displaced. The curve starts in the end phase (a diagonal line) but changes to a variable ellipse which opens out and then closes up to the opposite diagonal corresponding to $e = \pi$. In this curve the ratio is about 39 : 40. In *Fig. 3* the deviation from unison is less, and the change of phase is therefore slower. In these two curves the tracing point was lifted after half a cycle. *Fig. 4* exhibits the result of allowing it to continue through two cycles. These figures belong to the second class, as they are not in general symmetrical about either axis.

Plate II illustrates the *octave*, 2 : 1. These belong to the first class and are always symmetrical about one axis. *Fig. 5* gives the typical curves of the principal phases—the hippopede and the parabola. *Fig. 6* shows the middle phase, and *Figs 7* and *8* the end phase. These last exhibit the effect of watering, the phase not being exactly zero.

Plate III illustrates the *twelfth*, 3 : 1. These belong to the second class. *Fig. 9* gives the typical curves, *Figs. 10* and *11* the principal phases, and *Fig. 12* an intermediate phase.

Plate IV illustrates the *fifth*, 3 : 2. *Figs. 15* and *16* exhibit varieties of watering. In *Fig. 15* the phase is very nearly zero, so the watering is slight, while in *Fig. 16* there is a distinct difference of phase, and the watering is very marked.

Plates V and VI illustrate the ratios 4 : 3, 5 : 3 and 5 : 4, or the intervals of the *fourth*, *sixth* and *major third*. These show the same general characteristics. As the ratio becomes expressible by larger numbers the curves begin to lose their individuality; for example, the middle phase of 4 : 3 and 5 : 4 (*Figs. 18* and *23*) resemble each other closely.

The series of figures of the end phases also are related. For instance, comparing *Fig. 7* with *Figs. 15*, *19*, *22*, *24*, we may regard the latter figures as produced from the first by bending it more and more as the ratio becomes more complex. Of course, we may consider *Fig. 7* itself as produced by the bending into parabolas of the diagonal line forming the end phase of the ratio 1 : 1, repeatedly traced with diminishing amplitude.

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A NEW IRON PIGMENT.

A new pigment is thus described by the *Electrical World and Engineer:* Dr. Alexander S. Ramage, of Cleveland, O., discloses a method of producing a remarkable iron pigment, with an old formula and new properties—the hydrated basic ferric oxide $Fe_2O_3Fe_2(OH)_6$. It presents one of the first commercial applications of those physico-chemical theories which have been developed largely by the study of dilute electrolytes. In outline the process is as follows : The waste ferrous liquor derived from iron pickling—generally ferrous sulphate or chloride with some free acid—is neutralized and then oxidized by the joint action of air and steam. As the oxidation proceeds an alkali, as sodium carbonate in solution, is added in quantity sufficient to maintain substantial neutrality, and simultaneously therewith a large volume of water, which, as will appear, is the true precipitating agent.

The effect is that known as hydrolysis, or the decomposition by water of a salt composed of a base and acid between which there is a great disparity of strength, in the present instance a compound of a weak base with a strong acid. By the oxidation of ferrous sulphate, basic ferric sulphate is formed, and this salt in the presence of the large volume of water is hydrolyzed, yielding sulphuric acid and basic ferric hydrate—the pigment in question. A reaction of this character would, of course, soon reach an equilibrium and the yield of pigment would be but small, wherefore the gradual addition of the alkali to combine with the acid as it is liberated and to insure the continuance of the reaction to complete precipitation of the iron. The pigment is light yellow in color, but is readily converted by heat into the several iron oxide reds, and affords also a suitable base for mixed pigments. Its absorptive capacity for oil greatly exceeds that of the standard pigments, being two and one-half times that of standard French ocher, and seven and one-half times that of white lead. Its covering power, as compared with these pigments, is almost proportionately high.

TEXAS FUEL OIL TESTS.

A Texas oil company has printed a report of Prof. J. E. Denton, of Stevens Institute, upon tests at Beaumont, Texas, of crude petroleum as fuel, made for the company. The tests were made in a horizontal return tubular boiler 6 feet in diameter, 18 feet long, and having a grate surface of 45 5 square feet. In the tests, data for comparative purposes were obtained from the same boiler when burning buckwheat anthracite coal. With Beaumont crude oil the equivalent evaporation from and at 212° was 15·49 pounds of water per pound of oil. The corresponding figure when using buckwheat coal was 11·79 pounds of water per pound of combustible, and 8·94 per pound of coal. The efficiency of the boiler determined by the oil tests was 78·5 per cent. and by the coal tests 77·6 per cent. A quantity of steam varying from 3·1 to 4·8 per cent. was consumed in the oil blast. In the two tests with coal the horse-power developed by the boiler was 92·6 and 119·1 In the five tests with oil the horse-power varied from 112·7 to 220·1 horse-power, the evaporation per pound of oil from and at 212° in the first mentioned case being 15·05 and in the second 14·75. These figures are considerably less than those that have been obtained with Eastern crude petroleum, which range above 18 pounds of water per pound of oil.—*Electrical World.*

SUBTERRANEAN TEMPERATURE.

The driving of the great Simplon tunnel through the Alps is affording some interesting data on the subject of subterranean temperature. The work has progressed in the north heading over 21,000 feet, and in the south heading over 17,000 feet. Temperature observations have been taken both of the rock and the atmosphere. The temperatures of the rock show a steady increase with the depth of penetration in both headings. Thus at 1,640 feet from the portal of the north heading the rock showed a temperature of 54·3° F., while in the south heading at the same distance from the portal the temperature was 56·2°. At 6,560 feet the temperature in the north heading was 63·6°, and in the south heading 69·7°. At 12,920 feet from the north heading the temperature was 76·3° ; at 15,090 feet it had risen to 86·3°, and at 16,400 feet penetration the temperature of the rock was 89·1°. The highest temperature recorded previous to September last was 92·2° F. Early in October a heavy stream of water was encountered, which temporarily drove the gangs of workmen from the heading and necessitated temporary suspension of work in the main tunnel. The heaviest flow of water recorded at that time was about 200 gallons per second ; and while it has been productive of considerable inconvenience in the tunnel work, it brought with it the advantage that it produced a very marked decrease in the temperature of the rock. The temperatures of the air are not given, for the reason that they vary with the amount of ventilation. During the summer it was found necessary to deliver to the northern end of the tunnel 39,000 cubic feet of air, and 66,000 cubic feet to the south end.—*Scientific American.*

PHYSICAL SECTION

Stated Meeting, held Tuesday, December 17, 1901.

THE MECHANISM AND CAUSATION OF HOT WAVES.

By Harvey M. Watts.

In looking into the question of their causation it must be clearly understood, to begin with, that hot waves in the United States, Europe and Australia are due exactly to the same general conditions. The hot wave is the resultant of a certain definite trend in the general circulation of the atmosphere, and is not "local" in origin in any sense, localities, according to their situation and topography, merely modifying or intensifying the general conditions favorable to the production and continuation of high temperatures.

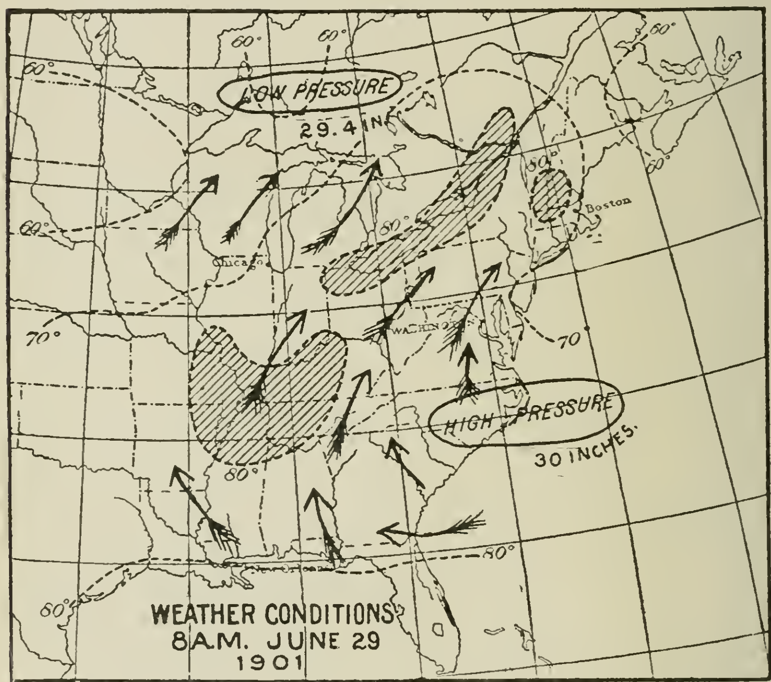
While, therefore, the hot waves in the United States are not caused by the "deforestation due to advancing civilization," as has been suggested in a British medical periodical, it is true that the lay of the land in certain parts of the United States does favor the intensification of hot waves after they have been set up by general causes, cosmical in origin and terrestrial in sweep and extent. In brief, the meteorological mechanism of hot waves, which is very simple, is as follows: The initial cause is a certain stress in the shape of a high pressure (barometric) area in the general circulation which sets up a movement of air from the south, from the tropics northward; secondly, this stress, which is far-reaching and reacts all around the globe, slows up the normal eastward drift of the ærial eddies in the North Temperate Zone, and produces a stagnation in the lower atmospheric levels; thirdly, the local circulation thus set up causes certain changes in the upper levels of the lowermost stratum of the atmosphere in the matter of moisture content, so that radiation at night is checked and the air retains the heat gained during the day in abnormal measures; and, lastly, what with the south-to-north circulation, clear, pitiless skies during the day and the checking of radiation at night, vast areas of land favorably situated become overheated until an enormous extent of territory is covered with a blanket of superheated air, which extends to a considerable height, and, indeed, carries the temperatures up to the higher cloud level. The duration or extent of the hot waves thus established is unaffected by the local conditions, however, and the hot wave can only give way through a change in the drift of the air, a reversal of the south-to-north system of circulation, This reversal can only come through an about-face in the mechanism of the general circulation, and is neither affected by bombardment of dynamite nor by prayers, any more than the law of gravitation would be changed by similar human efforts.

The hot wave, therefore, plays its part in connection with what are known as the fixed or permanent characteristics of the general circulation of the globe. These fixed relations take the form of a belt of high pressures near the tropics and a belt of low pressures near or at the poles. In between these two permanent systems move, in a great eastward swirl, the enormous aerial eddies that condition weather and climate in the North Temperate Zone. These eddies are only two, the high-pressure, anticyclonic, down-draught eddy (called "highs" for short) and the low-pressure, cyclonic, up-draught eddy (called "lows" for short). The highs are the center of clear weather phenomena, the lows the center of stormy, cloudy weather. While they ever move eastward, their rate of motion and the location of their paths are determined by the general movements of the atmosphere, and it is these variations that produce the marked seasonal vagaries, cold or mild winters, moderate or tropical summers, as the case may be. Whenever, for instance, the general circulation brings it about that the subtropical high pressures tend to increase in area and intensity, this means a summer of hot waves, for this has the effect of retarding the eastward drift and of holding up the migratory areas of high pressure, so that the air circulates sluggishly from the South, from the tropics poleward, and, being relatively and at times dynamically warm, what with the effect of 100 per cent. of sunshine daily, makes possible the terribly heated periods with their maxima of over 100°.

As specifically revealed on the United States Weather Bureau maps, our hot waves set in when there is a high over Texas, the Gulf, the Southern States or over the Bay of Charleston, extending out toward Bermuda, with a low over the extreme Northwest, the upper Missouri or Mississippi Valley, or over the Lakes. According to the law of atmospheric circulation, since the winds move down and out from the areas of high pressure into and about and up the areas of low pressure, so long as these highs hug the southern circuit, and the lows keep along the northern circuit, the air movement, if from south to north spirally (southeast to northwest and southwest to northeast) and the peculiar dynamic factors that mark this kind of circulation, give the sun the fullest opportunity to intensify the heat over the areas involved. As long as this circulation keeps up, so long will the hot wave continue; and, with the slow shift of the areas of high pressure and the areas of low pressure eastward across the country, tends to repeat itself, until, by a relaxation in the subtropical high and an increasing of pressure over the northern circuit, the system is broken up, and instead, we get a high pressure in the north and a low pressure to the south. Then with the reversal in the wind movement, a north-to-south drift taking the place of the south-to-north, the grasp of the hot wave is relaxed.

All our famous hot waves reveal the familiar and invariable mechanism described. The great hot wave of August 3-18, 1896, which wrought such a terrible loss of life in New York and other large cities, and broke all records of deaths until surpassed by the record of the hot wave of June 26-July 7, 1901, was due to a persistent area of high pressure over the Southern States, which discharged its dynamically heated tropical air into the areas of low barometer, which, while slowly moving eastward, kept persistently in the North.

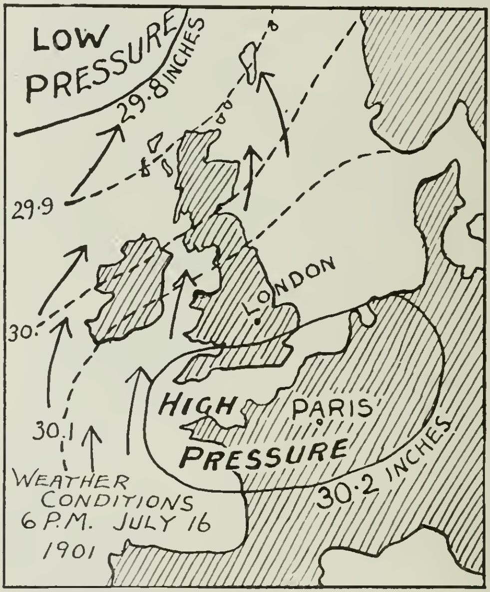
Coming to the extended hot wave of June-July-August, 1901, the daily weather maps reveal the same circulation--high pressure over the Gulf States and lows over the west, northwest and Lake Region. So long as this kept up the rigors of the heat were unabated, but the break that came to the Middle Coastal States, the upper Lake Region, New England and certain portions of the Central States during the week of July 8th to 15th was due to a deepening of pressure over the Lakes and New England and the appearance of a



Hot-weather conditions over the eastern part of the United States. State of things existing at the beginning of the great heats of the first week of July, 1901.

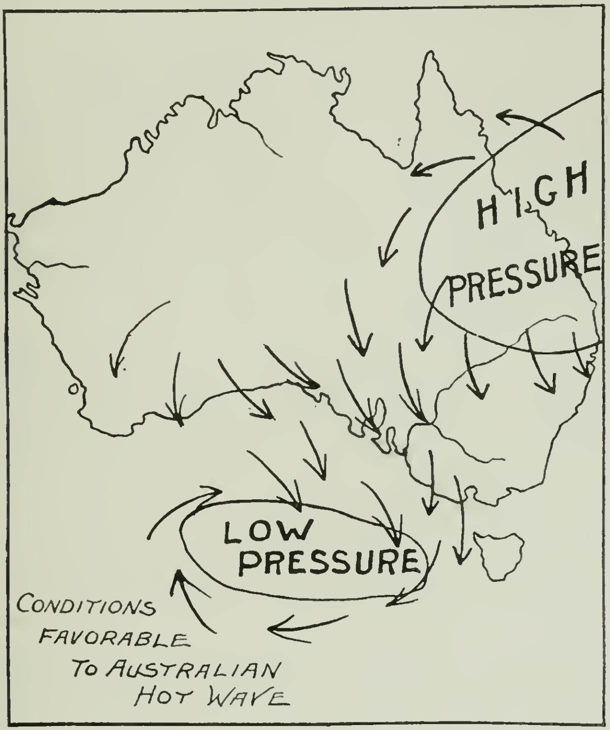
low barometric area in the ocean off Cape Hatteras. This gave a north-to-south circulation, with the formation on July 11th, 12th, 13th, of a decided cyclonic storm center out of the area of low barometer, so that what with the tropical showers, cool northeast winds on shore reaching to the Alleghenies and the dry and cool breezes that moved from out of the Lakes to New England high, a very large section of the United States experienced marked relief. At the same time while this reversal of the hot-wave mechanism gave relief in the East, the continuance of a circulation from a high pressure over the Gulf into a low pressure over the upper Mississippi Valley kept up the hot wave over the Western States with disastrous results to the crops and with serious effects on humanity. No greater contrast can be afforded, however, in the matter of American weather than that shown on the maps of July 12–13th, or of July 25–26th, which reveal the cool wave circulation at work on the coast giving minimum temperatures of 60, and the hot-wave mechanism still at work in the interior with Kansas, Nebraška, Iowa and Missouri maxima of 104 to 108°.

In studying the mechanism of hot waves here, in Europe, or in Australia, it must be remembered that any one of the determining factors may become the controlling element in the heat-making. At times over the interior valleys of the United States—and this was marked during the hot wave of July 12–26th over the middle Mississippi and Missouri valleys, the corn-belt region—the heat seems to be due to the sheer insolation, the sheer unintermittent sunshine accompanying a circulation that is almost monsoonal in its steady, sluggish movement from the Gulf to the northwest and in a spiral toward the Arizona-Utah "low;" and though there may be no marked "high" over the Gulf, there will be invariably a decided "low" over the west or northwest into which the monsoonal circulation discharges itself, but without rain or any of the normal phenomena connected with "low" areas. Again, the hot wave will seem more particularly to be due to the decided circulation, dynamically heated, from out a "high" which may overlie the Ohio Valley, while at other times, with the circulation not so definitely of the hot-wave type, apparently the checking of radiation is one of the chief factors in a local visitation. Of course, when all three factors combine, then the hot wave is the most intense and of the longest duration. So far as the hot-wave mechanism of Europe goes, insolation under the clear skies of a high barometer is the common type. A hot wave over the British Isles and Scandinavia is caused by a "high" pressure lying over Northern France or a little further north, while France is baked if the "high" lies squarely over it, or a little to the south. In Australia the hot waves which affect the eastern and southern coastal colonies are the result of a circulation from out the heated interior into a "low" lying over the ocean. This north-to-south circulation is, of course, owing to the reversal of things south of the equator, a motion from the heated tropics



Hot-weather conditions for the British Isles and Northwestern Europe. The map represents the primary stage of the hot wave which lasted from the 16th to the 20th of July, 1901, with maximum of 90° in London, and from 93° to 95° on the Continent.

poleward, and is equivalent to our own south-to-north movements, but owing to the character of the Australian continent, the air arriving from the heated interior of Australia is hotter than any of our own sirocco blasts east of the Rockies, so that during the terrible heats of 1896 Australian cities experienced shade temperatures of 122°. Western Europe rarely experiences anything like the heat intensities familiar to Australia and common to the valleys of the United States, and this is owing to the mitigations due to the aerial circulation from off the ocean. However, owing to the fact that each region has its own critical temperatures for sunstroke and heat exhaustion, temperatures of from 80° to 85° to 90° are fatal in England and Scandinavia and on the Continent. These would be without effect in



In the Southern Hemisphere the movement of the winds out of a "high" and into a "low" is the reverse of that in the Northern Hemisphere. The hot waves of Australia are due to "highs" that overlie the interior and send out scorching winds over the coastal regions, which are the seat of Australian civilization.

the United States where climate tolerance has raised the limit of endurable maxima above 90° for the greater part of the country.

In running over a score and more of lantern slides which were in the form of reproductions of synoptic weather charts, showing hot-wave conditions in the United States, Europe and Australia, Mr. Watts drove home the lesson as to the essential simplicity of the hot-wave mechanism, its anticyclonic character, and in conclusion pointed out that the map made it clear that local conditions were entirely secondary in the matter of causation. Though the land masses and water masses bore exactly the same relation to each other year by year, there was no such similarity of relation in the aerial pressures and atmospheric movements that lay over them. On the contrary, these varied from year to year, from one period to another, swinging to a maximum in the way of a great drought or a warm winter, and then on back to the opposite effect. For the real cause of such changes one must look to the variations in the general circulation; the result of its own reactions, owing to its mobility, while the cause of the definite shift in the atmospheric stresses must be found in the sun. As all these changes must be due to law, the speaker hoped that the time was coming when by a study of cosmical causes meteorologists in time may be able to forecast seasonal conditions, to predict hot or cool summers, while with better sounding of the ocean of air by means of kites, with recording instruments attached, all the factors that make for the persistency of hot waves may be ascertained and a high degree of accuracy in the daily forecasts secured. We are, perhaps, not so far away from this period as most imagine. The kite work of the United States Weather Bureau, and more particularly the splendid work done at the Blue Hill Meteorological Observatory at Blue Hill, Mass. (ten miles south of Boston), by H. Helm Clayton, under the direction of Mr. A. Lawrence Rotch, where the kites with their meteorographs have sounded the air three miles high, has shown what can be ascertained as to upper air conditions, while Meldrum and Eliot, on the part of meteorologists, and the Lockyers, for astronomers, are making considerable headway in working out the connection between solar conditions and seasonal variations in the weather of the Indian Ocean and adjacent regions. But as no section of the globe is independent in the matter of circulation, since the highs and the lows, permanent and migratory, interlock the earth around, it is clear that if it becomes possible to forecast the seasons in the tropical and subtropical regions we shall have made a decided step toward discovering a rythm in the seemingly capricious weather of the temperate zones.

SOLIDIFYING METALLIC POWDERS BY PRESSURE.

The "flow of metals" such as lead, and even iron and steel, when subjected to great pressure, is a phenomenon with which engineers have long been familiar and in certain industries, such as the manufacture of lead pipe, hot and cold-punched nuts, etc., it is taken advantage of. The fact, however, that metallic powders, shavings, etc., can be compacted into solid masses by extreme pressures is not generally known.

Some interesting experiments of this kind have recently been conducted in the Workman Engineering Building at McGill University, Montreal, by Professors Anderson and Nicholson. They consist of taking filings or turnings of brass, iron, copper, tin and other metals, and, by means of pressure, securing therefrom solid bars of metal, differing very little in appearance and strength from those to be seen, as a rule, in hardware stores and machine shops. The pressure brought to bear upon the filings or shavings is about 78,000 pounds to the square inch, and this has the effect of welding them into a solid whole or bar of metal. The filings are first encased in a jacket made of steel, which is conical in shape. Pressure is continued for about ten minutes, at the end of which time the tube is removed from the cylinder and opened, when a solid cylindrical bar of brass, lead, tin, iron, or copper, as the case may be, is found. These bars differ very little in appearance from those which have been made either by welding or casting, and appear to be equally strong as the latter. Some idea of the success of the experiment may be gathered from the fact that it has been found quite easy to solidify the filings of Pittsburgh brass, probably the hardest brass manufactured at the present day. This metal is found to yield quite readily to pressure. W.

A NEW WHITE LEAD PROCESS.

It is announced by the *Iron Age* that an extensive new plant principally intended for the manufacture of white lead by a new process is about to be put in operation in Brooklyn. The Brooklyn plant will have a corroding capacity of 12,000 tons annually and a drying and grinding capacity of 15,000 tons. In addition to this the plant will produce also lead oxides, lead pipe, sheet lead and lead traps.

The new process is not in any wise a departure from the principle of the "Old Dutch Process." It consists simply of a method of preparing the lead so as to hasten corrosion. Instead of casting the lead into buckles to be placed in the corroding pots it is transformed into fine filaments. It is known as the Bailey process, and is said to effect perfect corrosion in three days.

In the Bailey process the melted lead is forced by its own gravity through a perforated plate of thin steel, which forms it into threads or hairs about $\frac{1}{100}$ inch in diameter. The lead solidifies and cools almost as soon as it passes the plate, and is transferred to long shallow trays with slat bottoms. Each of these trays as it receives its charge runs back automatically into place in a suitably constructed bin or rack. When all the trays in this rack have been charged with the fibrous lead the process of corrosion begins. Mingled vapors of acetic acid, moisture, air and purified carbonic acid gas are blown in through suitable openings at the bottom and sides of the rack, and after circulating freely through the mass finally escape. The temperature meanwhile is maintained automatically at the degree most favorable to satisfactory corrosion.

The process is in every respect under absolute control, and any tray with its contents can be withdrawn for examination at any time. Corrosion is completed within three days, and uniformity is more nearly absolute than by any other known process.

The corroded fibres are thrown into water, where they immediately fall into impalpable powder. The cream-like liquid from the disintegrating tank is then run, without the necessity of grinding, into a separating cylinder formed of wire gauze with a mesh of 120 strands to the linear inch. This cylinder is partly submerged in water, and as it revolves the fine particles of corroded lead pass through the meshes, while the uncorroded particles, which in this process never rise above 8 per cent., pass out through the end of the separator and, being in a very fine state of subdivision, are in the best possible condition for conversion into lead acetate and other lead salts. There is therefore no rehandling and remelting of the tailings, as in the ordinary process.

The separated lead is finally washed by flotation, and when dried is ready for sale as dry lead. When intended for sale as lead in oil it requires, of course, a run through the mill; but, owing to the fineness of the product, grinding is necessary only to insure perfect assimilation of the oil.

PHOSPHORESCENT COMPOUNDS FOR LUMINOUS PAINTS, ETC.

Moureto, who has made a careful study of the best conditions for producing phosphorescent compounds, finds that the sulphide of strontium gives pronounced luminosity. His experiments indicate that to produce any phosphorescence it is necessary to have at least 2½ per cent. of active material present in the mixture, and that the phosphorescence increases with the percentage of active substance. The intensity of the phosphorescence may be notably increased by introducing into the mixture used for the production of the compound certain foreign additions. He recommends the use of a mixture composed of barium (or strontium) sulphate, sulphur and carbonates of the alkaline earths, to which, before heating, there should be added small proportions of sodium carbonate, common salt, or bismuth subnitrate. The mixture should be maintained for some time at a cherry-red heat.

W.

SINGLE CURVED vs. DOUBLE STRAIGHT JETTIES.

AN APPLICATION OF NATURAL LAWS TO THE REMOVAL OF BARS.

By Lewis M. Haupt.
Member of the Institute.

Vibration is fundamental to the transmission of energy. Heat, light, sound and electricity are propagated by waves of different intensities.

The vibrating pendulum or hairspring marks time; the oscillating piston turns the crank; the vibrating diaphragm transmits the voice. Molecular oscillation within variable limits is a well-recognized law of Nature, even in the transmission of stresses through solids. In fluids it is more discernible in the particles themselves. Hail, snow, dust, sand or rain, driven by wind, seldom pursue a straight course. The path of a storm-center is generally a curved line, while the motion of its elements is in the nature of a cycloid. A river never takes the shortest line to the sea, but oscillates wherever possible, from bank to bank, carving its bed of pools and shoals in such manner as to check and regulate its otherwise destructive energy.

No stream pursues a straight course if it can possibly avoid it, but swings gracefully from side to side, cutting first on the right then on the left, but always on the concave bank, and depositing on the elbows and crossings or points of inversion. Neither do the filaments of the stream pursue parallel paths. The maximum velocity is found near the mid-area of the section, but if these particular particles were to continue uninterruptedly along this course of maximum velocity they would run away from the rest of the water and leave a void. This tendency is checked by a constant interchange of position of the particles which restores the equilibrium by swirls and eddies, cross, and even back currents, in certain sections. It not infrequently happens that water runs up hill, as in the case of a flood when the crest of the wave gives a reverse slope up stream. Such are a few of the phenomena of flowing bodies, which are well recognized and understood by navigators or others who are familiar with streams flowing between banks; and precisely the same conditions characterize streams in the ocean or other large bodies of water.

Numerous illustrations of this law of running water are within reach of every one.

Every map of the rivers of any country, and especially in alluvial plains; every chart of ocean currents or of tidal estuaries, shows this law to be universal.

The huntsman, fisherman, navigator or traveler on waterways will recall apt illustrations of this law of running water and knows that the deepest water is on the concave side of the bend and not in the cut-off. So true is this, that a pilot navigating a river of which he has no chart invariably hugs the concave banks, swinging from side to side, and is only in doubt as to the channel in the straight reaches between bends.

The drainage rivulets to oceans, lakes or ponds, as they flow across the strand, pursue a serpentine course and not a straight line. The same features are found in the delta mouths of rivers and their passes, which are not straight but which oscillate with variable radii to the sea.

These phenomena have been recognized by some of the most experienced hydraulic engineers of the continent in the treatment of their interior waterways with excellent results.

The following extracts may serve to emphasize the necessity for a close study of this subject on the part of those interested in the development of our commercial highways.

Wheeler, in his "Tidal Rivers," says: "In carrying out works for the improvement of channels in sandy estuaries, it will be found much less costly and more effective to direct and develop the one main low-water channel than to attempt to make a straight channel in a new direction by training regardless of existing circumstances." * * * "By attempting to drive a channel through sands in a direction which nature has not selected will be found costly to carry out and difficult to maintain."

* * * "Straight reaches are not found iu practice to preserve the same uniformity of depth as curved channels under the varying conditions of flow to which they are subject."

"In a curved channel the greater velocity and scouring action of the stream is concentrated on one side, preventing deposit altogether, or causing it to accumulate on the convex side only, leaving the greater part of the bed of the river at its full navigable depth.

"A concave bank sets up a scouring action in the current by diverting the particles of the water from their straight course, causing that rotary motion and boring action which occurs in all bends, and which operates in deepening the channel along the concave side.

"From observations made by Mr. H. C. Ripley it was found that in a natural river the water was 58 per cent. deeper in the curved portion of the channel than in the straight reaches.

"Mr. Stevenson says: As a purely abstract question, he, however, was of opinion that it might safely be affirmed that a stream is most likely to follow a permanent course when directed by a concave bank; that the centrifugal force in curved channels has a tendency to draw the greater portion of the water to the concave side, and that the greatest scouring power will be found on that side; whereas in a channel directed by straight walls, the current, having no such bias for either side, is more easily thrown across from side to side.

"Mr. Scott-Russel, in pointing out the evils which had arisen from the abrupt interpolation of portions of straight cuts into gently winding rivers, expressed the opinion that to make part of a natural river straight was a dangerous undertaking; that where the curves were gentle the natural bends should not be interfered with; that a river has an oscillating motion, and there is a similar process going on in it to that which goes on in a pendulous body. A pendulum set in motion continues to oscillate isochronously without the expenditure of any new force, and in like manner, if once a curve or bend was established in a river with considerable current, the mere fact of the commencement of curvature would give it a tendency to continue that curvature, and the stream would go on oscillating regularly to the sea in curves of opposite curvature. Continuity of a system of oscillation should therefore be maintained.

"Captain Culver, speaking of the improvement of rivers, says that straight reaches are strictly to be avoided, more particularly where there is an established business upon the banks of the river to be trained. With a straight reach the deep-water track is acted upon by the most trifling causes, ranging from side to side at will, and it follows that under these circumstances there is no security whatever for the permanency of the deep water, either in a fixed channel or at the shipping berths.

"The commission of engineers employed by the French government to report as to the improvement of the navigation of the estuary of the Seine, in recommending the extension of the training walls through the estuary down to Honfleur, advised that these should assume a sinuous form, having a concave bend leading to the entrance to Honfleur harbor, in order that deep water should be maintained at the entrance jetties.

"M. Fontain, in designing work for the rectification of the Rhine, avoided straight cuts and adopted curves. In the determination of the radii of curvature, he was guided by the inclination and force of the current. General Eads also adopted a curved form for the jetties for the South pass of the mouth of the Mississippi. In the Weser no attempt has been made to straighten the channel, the existing curves being eased and improved. By cutting off abrupt bends, contracting the width where too great, fixing the course through sands, and deepening where required, a channel may be brought into a uniform condition with a series of gentle curves and rendered serviceable for navigation at far less cost and with better results than by making an entirely new cut. The course may not be so direct in the former as in the latter case, but the want of directness will probably be fully balanced by the more permanent maintenance of a uniform depth throughout, and the greater scouring power of the ebb and flood water."

"The effect derived from training takes time to develop, and the full benefit of the deepening is seldom felt until a considerable period after the work is completed. The benefit to navigation is more quickly realized when the deepening is also assisted by dredging, not only by the actual material removed by the dredger, but also by the disturbance of the material by the buckets assisting its transport by the water.

"Tidal currents will, without any aid from training, keep to the same channel if sufficiently deep. The tendency of all flowing water is to run along the line of the deepest channel, especially if that line is in the direction most favorable for the run of the flood and ebb."

When it comes to the application of these principles, they are so frequently violated with us as to cause a doubt as to whether they are really recognized as fundamental.

To violate a physical law is to cause an injury which can be repaired only at considerable loss by the application of a counter irritant. Nature will constantly seek to restore normal conditions. It is surprising to find mathematical principles so frequently misapplied to physical problems. Because "a straight line measures the shortest distance between two points" it does not follow that two parallel straight lines with currents between them will be better as training walls for a navigable channel than a single curved jetty which aids those currents to carve out and maintain their own channel. Yet it is the almost invariable practice to build two such parallel *straight* jetties or to dredge *straight* cuts in rivers which soon refill and require constant expense for maintenance. At least three times has the cut through the "Cherry Island Flats" in the Delaware river, two miles long, been dredged to 26 feet, mean low water, and just as frequently has it filled almost immediately after, to its ruling depth of 19 feet.

The concave bank practically controls and directs the stream—it is the point of attack for the currents and the point where scour is most rapid. The convex bank is the reciprocal of that where there is virtually no current and where deposit takes place. The continuous change of direction caused by the resistance or reaction of the concave bank, and the consequent differences of velocity of the fluid-particles before and after impart, cause a latteral bottom movement which carries the silt across and on to the convex bank, thus maintaining a deep channel under the concave bend; the depth increasing with the sharpness of the turn.

By maintaining these oscillations by the erection of artificial banks or reaction-jetties across ocean bars the channel may be quickly and cheaply created by natural processes; but great care must be taken in adjusting their curvature and location to the requirements of the particular site, for if placed on the wrong side of the channel with respect to the littoral drift at inlets, they will result in failure.

Thus, a single structure may be made to produce its counterpart, or convex training-bank, which will be automatically adjusted to the regimen of the currents, whether tidal or fluvial, at less than half the cost of the parallel straight jetties, and give a far better and more permanent result, requiring a much smaller expenditure for maintenance.

Such a result has actually been secured at Aransas Pass, Tex., a purely tidal inlet, by an incomplete jetty detached from the shore and placed on the windward side of the channel, as designed by the writer, and the same principles may economically be adapted to the opening of the delta mouths of silt-bearing streams by single concave reaction jetties to better advantage than by two parallel or convergent jetties. Such designs would appear to conform to the laws of flowing bodies, and would not do violence to nature, but, on the contrary, assist her operations.

ELECTRICAL RECOVERY OF PURE TIN FROM WASTE TIN CUTTINGS.

Gelstharp.—An illustrated description in detail of a process which is said to have been successful in a practical trial. The tin is recovered by electrolysis, and the clean iron is either sold as best scrap or converted into green copperas, which may be further converted into red oxide and Nordhausen sulphuric acid, the profits derived therefrom being exceptionally large. The electrolyte in the stripping tanks is a 1¼ per cent. solution of commercial hydrochloric acid, to which is added a small quantity of oil of vitriol.—*Elec. Chem. and Met.*

TESTS OF ALUMINUM BRONZES.

Tetmayer gives an account of tests made in the Zurich Polytechnic School at the request of the Neuhausen Aluminum Company, which provided the samples. The specific gravity first rises and then falls, as the percentage of aluminum increases. The maximum strength for soft alloys was obtained with 3·4 per cent. Al, and for hard alloys with 1·4 per cent. Al. For brass, the elasticity decreases with the increase of Al, and is extremely low with 2 per cent. of the latter. Silicon increases the specific gravity and reduces the elasticity. Iron, in the proportion in which it was present in the alloys, was not observed to sensibly influence the physical characteristics. Aluminum bronze, containing 10 per cent. Al with 1·5 per cent. Si and Fe, is too brittle to be of any practical value. As regards the abrasion by friction, the hard alloys, with less than 89·6 per cent. Cu, lose little in weight, while the soft alloys, with less than 6 per cent. Al, heat and wear away rapidly.—*Rev. de l'Elec.*

AGE OF THE STONEHENGE MONUMENTS.

The work of raising the Great Monolith at Stonehenge, England, has enabled archæologists to form a more reliable estimate regarding the epoch in which these druidical monuments were erected. There has hitherto been much controversy on this point, certain authorities clinging to the assertion that it was built in Roman times, while others contend that it was erected during the bronze period. While making excavations round the monolith for the concrete bedding, a large number of neolithic stone implements were unearthed that show every sign of having been used to cut and to square the stones. They all bore marks of hard working, and when of no further use for cutting, the stones had been apparently thrown aside and afterward used to make a bedding to support the uprights. Experts therefore now entertain little doubt that Stonehenge was built in the neolithic age, for had it been built in the bronze or iron age, bronze or iron tools would have been used. Although leading authorities do not quite agree as to the actual date of the introduction of bronze into Great Britain, it is generally conceded to have been about 1500 B. C. It is consequently apparent that Stonehenge must have been constructed at some period considerably previous to that date.—*Scientific American.*

IN MEMORIAM.

LUTHER R. FAUGHT.

Among the many members of the Franklin Institute who have won wide distinction and honor for meritorious inventions in science and the mechanic arts, Luther R. Faught holds a high rank. His inventions in the mechanical field have been both numerous and valuable. It is fitting, therefore, that a brief record of his life-work should be read before the Institute and published in the *Journal* for permanent record.

Luther R. Faught was born in Sidney, Me., on the 22d of February, 1828. He was of German extraction, his ancestors settling in America in 1751.

LUTHER R. FAUGHT.

His early life was spent on his father's farm. But it was the blacksmith's shop and wagon factory, necessary adjuncts to all large farms in those early days, that possessed the chief charm for the growing lad. Here, during the evenings, for the daylight was almost wholly spent in farm work, he first evidenced that mechanical genius which so characterized his after-life. Here, among other rude triumphs, achieved under these circumstances, he produced a model of a reaping machine, said to embody principles similar to those of the McCormack reaper. This model is still in existence.

But the farm and its environs at last grew too circumscribed for the young inventor, so we find him at seventeen years a runaway from home. A working capital of ten cents, added to a muscular activity, which served him so well in later years, carried him to Augusta, Me., where he found, in the machine shop of Rufus Wolcott, employment more suited to his tastes. Here he remained for three years until he learned his trade as machinist. He then went to Dover, N. H., where he joined himself with the firm of Hanson & Ogden. On the dissolution of the partnership of this firm he went to Macon, Ga., where he was engaged by Robert Finley as Superintendent of his machine shop. It was while engaged in this work that he took out his first patent on November 1, 1853. This invention was for an automatic steam governor, in which the first attempt was made to employ the frictional resistance which the governor has to overcome as the means by which its movements are controlled. Though not a complete solution of the difficulty, yet it was in a direction that led to the perfection of the class of governors to which it belongs.

In 1855, Mr. Faught went to Atlanta, Ga., where he became the General Superintendent for the Winship Bros. Here he remained until the War of the Rebellion, in 1861. In 1865, he was a refugee to the North, and settled in Philadelphia.

By far the greater part of his working life was spent as General Superintendent of A. Whitney & Son in Philadelphia, car-wheel manufacturers. During the early part of his work here he was closely connected with improvements in horse-powers, for which he took out numerous patents. His work in this direction fairly revolutionized the former methods of cotton ginning in the South.

It will be seen, from an inspection of the list of patents taken out by Mr. Faught, appended to this notice, that between 1866 and 1884, some of the more important are for a hold-fast for lathes; a boring bar, which markedly increased the output of a machine for boring the hub of a car wheel; and a round woven pulley belt.

After leaving the Whitneys, he was employed by Bement, Miles & Co., Philadelphia, where he remained from October 24, 1892, to March 2, 1894. Here he acted as general utility man in the mechanical department. His especial duty was to improve foundry methods, especially the mixing of irons to suit the various requirements of the work. In this work he was eminently successful.

His invention for improvements in mining car wheels was for a device which enabled a wheel for mining and other purposes to be fitted to run loose on an axle without the necessity and expense ot collars on the axle. This improvement prevented the access of dust and grit to the running parts, while at the same time it permitted the bearings to be oiled with a minimum of lubricant. The wheel hub, closed on its outer end, was provided with both a recess, into which a key block is dropped, and with an oil chamber. Each end of the axle was turned with a groove, into which the key block falls when the wheels are in position, and in which they are secured by the plugs which close the oil chambers. Wheels, built in accordance with these improvements, were exhibited by A. W. Whitney & Son, at the Novelties Exhibition of the Franklin Institute, in Philadelphia, in 1885. At the same exhibition another invention of Mr. Faught was shown. This was an improved pedestal to secure the axle to the body of the car, and so constructed as to exclude dust from the inside face of the hub.

Perhaps the most valuable invention of Luther R. Faught was that of the radial expansion chill made sometime late in 1875, or early in 1876. This important invention was not patented, either by reason of some mistaken idea that through his employment by the Whitneys he was not entitled to patents for inventions of his own on car wheels, or, possibly, for reasons best known to himself, he deemed it undesirable to thus assert his rights. There is, however, no doubt as to his invention of the contracting chill. The Whitneys themselves acknowledge his right to the invention, as will be seen from the following, taken from the issue of the *Locomotive Engineer* for November, 1888:

"*Editor The Locomotive Engineer :*

"Referring to the article entitled 'Hard Tests of Chilled Wheels' in your issue for October, we beg leave to say that the honor of having invented the first contracting chill of which we have any knowledge is due to Mr. L. R. Faught, Superintendent of our mechanical department. It was made in 1876.

"The Whitney contracting chill was invented by the present senior member of this firm in 1885.

"A. WHITNEY & SONS.

"PHILADELPHIA, PA."

This Faught contracting chill was in all respects a pioneer invention; and provided an entirely new means for ensuring the desired result. As in the case of nearly all such inventions it had rival claimants; but, without entering again into a controversy that has long since been fairly settled, the honor of the invention has, by the concensus of authoritative opinion, properly been awarded to Luther R. Faught. In the manufacture of car wheels the essential requirements are long and smooth running so as to increase, as far as possible, the duty of the wheel in satisfactory car-miles. To insure this, the wheel, when manufactured, must not only possess accurate roundness, but must have its tread and flange uniformly hard so as not only to resist rapid wear but also to ensure uniform wearing under prolonged use. To ensure hardness to the wearing surfaces of the wheel it is necessary to provide the mold of the wheel with a chill, that shall effect a uniform and rapid chilling or cooling of the molten iron when poured into the mold.

Before the pioneer invention of Faught's radial contracting chill, it was customary to provide a fairly heavy cast iron ring, whose inner surface was turned out so as to conform to the shape of the tread and flange of the wheel to be cast. The ring was provided at suitable points with handles or lugs for convenience in handling. Though this primitive form of chill ensured a hardened or chilled surface, due to the sudden chilling of the iron on coming in contact with the cool surface of the ring, yet it was unsatisfactory, because, either on account of differences in thickness of the ring, where the lugs or handles were located, or by reason of other peculiarities in construction, the hardening was far from uniform throughout. Moreover, the chill did not expand uniformly throughout, so that parts of the chilling surface remained in contact with the cast wheel longer than others, thus not only producing unequal chilling, but also resulting in a warping of the surface of the mold and a consequent change in the shape of the wheel. Moreover, the unequal stresses caused by unequal and excessive expansion of the chilling surface, resulted in its breaking up and disintegrating, thus injuring the smoothness of the chill surface and necessitating frequent renewals of the chill.

In order to overcome the tendency of the solid-ring chill to increase its diameter unevenly and thus produce unequal chilling and irregularities of shape of the car-wheel, Mr. Faught invented the radial contracting chill by providing segmental chilling blocks united to a continuous outer ring, cast in a single solid piece by expansion-bars forming a radial web. There was thus provided an inner and an outer ring connected by radial arms, the inner ring being divided between the arms, each radial spoke or arm supporting a segment of the inner ring. When heated, the expansion of these radial spokes carried the inner ring towards the center, thus ensuring efficient chilling by keeping the chill in contact with the tread of the wheel, both for a longer time and more uniformly than in the old form of chill.

Working along these original lines the inventor produced a chill in which the effect of expansion, instead of withdrawing the chill surface from the face of the wheel, through the radial expansion of the bars, caused this surface to contract, thus continuing its chilling effect on the tread of the wheel. Generally, therefore, Mr. Faught's great invention in chills for car wheels consisted in means whereby the expansion of the metal caused the chill to move towards the wheel, instead of, as in all earlier forms, away from it. On this fundamental and radically new departure, all subsequent operative improvements in chills for car wheels have been founded. A wheel cast in a chill of this form was exhibited by A. W. Whitney & Son's, at the Centennial Exhibition, in Philadelphia, in 1876.

Although this early form of contracting chill constituted an immense improvement over anything that preceded it, yet it manifested a tendency to lose its true shape, not only from the rough handling of the foundry, but especially because the inner-ring section, being firmly clamped between the cope and drag, lost its free movement. At a later date, 1886, Mr. Faught remedied this defect by tying the arms together alternately at top and bottom. A subsequent improvement, made in 1887, consisted in what is called "the suppression of the mold," in which the outer ring was clamped between the cope and drag, enclosing the space between the arms in the mold, thus leaving the inner ring of sections quite free to move inwards, since they were devoid of any clamping.

In a letter to the *National Car and Locomotive Builder*, of June, 1889, in discussing the cause of uneven chilling of car wheels, and the value of the contracting chill for preventing the same, Mr. Faught thus contrasts the old and improved forms of chills:

"*Editor of the National Car and Locomotive Builder:*

"In the last (May) issue of your journal you refer, under the title 'The Cause of Uneven Chilling,' to the value of contracting chills as a preventative. As the original inventor of contracting chills, permit me to add a few lines relative to the subject.

"The *construction* of the solid-ring chill determines that it shall increase its circumference, and hence its diameter, under the influence of heat; the *inequality of clamping* the chill in combination with the cope and drag, determines that this increase shall be in a varying degree around the circumference, causing both uneven chilling and loss of superior roundness in the wheel. To overcome these defects I invented the chill now known as the contracting chill, composed of segmental chilling blocks united to a continuous outer ring, by a radial web of expansion bars; and cast as its predecessor, the solid ring, in a single piece. The common mode of clamping the contracting chill with the cope and drag resting upon the inner section, I found did not allow the segmental blocks sufficient freedom of action, and therefore as the difficulty was not removed as fully as I desired, I invented a cope and drag with a method of clamping which did away with all restriction of movement to the inner section . . ."

In a later invention Mr. Faught greatly increased the efficiency of his contracting chill by a device which practically increased the length of the radial bars, without proportionately increasing the size of the chill. It is evident that the longer these bars are, the greater is the contraction that is obtained before the outer ring becoming heated expands and tends to draw the chill away from the radius of the wheel. The inventor effected this lengthening of the radial bars, or arms, by what he calls *secant-bars*, or straight bars inclined to the segments of the inner ring; instead of at right angles thereto, as in the original form. The secant-bars were interlocked like the girders of a bridge. Chills constructed on this principle are more efficient in action than those of the earlier type.

The *Locomotive Engineer* for January, 1889, referring to the invention of the contracting chill, very correctly says of Mr. Luther R. Faught:

"His invention goes back to 1876, and his patents nearly as far. Mr. Faught is to the contracting chill what Howe was to the sewing machine, Whitney to the cotton gin, Ericsson to the screw propeller, and Jethrow Wood to the common plow—the pioneer."

From 1896 to 1901, Mr. Faught was employed by the Lobdell Car Wheel Company, of Wilmington, Del. This engagement began July 27, 1896, and continued until his death on November 23, 1901, for a period of over five years. His duties with the Lobdell Car Wheel Company were confined principally to matters connected with his patent self-oiling wheel and the contracting chill.

Marked inventive ability in the mechanical line was the characteristic of Luther R. Faught. His numerous patents were for inventions that were eminently practical and useful, and many of them have come into extended use.

EDWIN J. HOUSTON.

PHILADELPHIA, DEC. 23, 1901.

LIST OF PATENTS FROM 1853 TO 1901.

No. 10,185, Improved Regulators of Steam Engines. Patented November 1, 1853.

No. 25,497, Improvement in Horse-power Machines. Patent dated September 20, 1859.

No. 60,163, Turning Lathe, December 4, 1866.

Reissue No. 4,390, Turning Lathe. (Patent No. 60,163, dated December 4, 1866). May 23, 1871.

No. 115,290, Hold-Fast for Lathe Spindles, May 30, 1871.

No. 115,291, Die Stock for Cutting Screws, May 30, 1871.

No. 119,681, Cog Gearing, October 3, 1871.

No. 120,048, Horse-power, November, 21, 1871.

No. 123,774, Horse-power, February 20, 1872.

No. 132,205, Pulley Belt, October 15, 1872.

No. 143,974, Horse-power, October 28, 1873.

No. 151,213, Horse-power, May 26, 1874.

No. 152,222, Horse-power, June 23, 1874.

No. 169,975, Car Axle Box, November 16, 1875.

No. 220,441 (Sproull and Faught) Car Axle, October 7, 1879.

No. 241,481, Boring Bar, May 17, 1881.

No. 241,482, Boring Bar, May 17, 1881.

No. 241,483, Machine for Boring Metal, May 17, 1881.

No. 243,234, Worm and Worm Wheel Gearing, June 21, 1881.

No. 253,523, Boring Bar, February 14, 1882.

No. 255,846, Boring Bar, April 4, 1882.

No. 257,157, Clamp for Stationary Spindles, May 2, 1882.

No. 260,467, Shaft Coupling, July 4, 1882.

No. 260,468, Shaft Coupling, July 4, 1882.

No. 262,212, Device for Lubricating Loose Wheels and Pulleys, August 8, 1882.

No. 289,522, Boring Bar, December 4, 1883.

No. 302,831, Boring Tool, July 29, 1884.

English Patent No. 11,577, Continuous Cutter, September 29, 1885.

English Patent No. 11,578, Car Wheel, September 29, 1885.

Canada Patent No. ——, 1890. Car Wheel Chill.

No. 311,652, Boring Bar, February 3, 1885.

No. 321,590, Car Wheel, July 7, 1885.

No. 327,148, Metal Boring Bit, September 29, 1885.

No. 327,149, Device for Holding Metal Cutters, September 29, 1885.

No. 327,150, Car Wheel, September 29, 1885.

No. 327,151, Device for Holding Metal Cutters, September 29, 1885.

No. 341,326, Chill for Car Wheels, June 28, 1887.

No. 365,590, Chill for Car Wheels, June 28, 1887.

No. 365,914, Chill for Car Wheels, July 5, 1887.

No. 402,664, Chill for Car Wheels, May 7, 1889.

No. 477,524, Chilled Cast Iron Car Wheel, June 21, 1892.

No. 588,325, Mine Car Running Gear, August 17, 1897.

No. 588,326, Mine Car Running Gear, August 17, 1897.

APPLICATIONS NOW PENDING.

(*a*) Car Wheel, Serial No. 76,449, filed September 29, 1901.

(*b*) Car Wheel, Serial No. 76,450, filed September 25, 1901.

Cores for Casting Car Wheels, Serial No. 77,230, filed October 2, 1901.

(*a*) Issued as Patent No. 687,453, November 26, 1901.

(*b*) Issued as Patent No. 687,454, November 26, 1901.

NOTES AND COMMENTS.

MARBLE IN ALASKA.

J. E. Cronin, who has recently returned from Marble Creek, on Prince of Wales Island, and W. K. Sheldon, who has just joined him from San Francisco, have some wonderful stories to tell of the fine character of the marble to be found at Shakan, on Prince Edward Island. The marble is said to be equal to the best Italian marble. Mr. Sheldon said: "When I received samples of this marble last February I saw at once that it was unlike any marble now being produced in the United States, but identical with the marble produced in the world-famous quarries of Carrara, Italy. While there are a number of profitable quarries in the United States and a large amount of American marble is used, yet the Italian product has always occupied a field which no American marble could fill, as evidenced by the fact that during the fiscal year ended June 30, 1900, nearly 40,000 tons of Italian marble were imported into the United States. This marble from Alaska, in texture, color and chemical analysis, is identical with the Carrara deposit, and there is no reason why it should not supplant the foreign product entirely. I went to Alaska in March and found a mountain of high-grade marble. While I was fully convinced as to the quality of the material, I did not wish to make a final report on the property as a commercial proposition until large blocks had been quarried, taken to the market and sawed. We, therefore, quarried a number of blocks, and I took them to San Francisco and had several of them sawed. These blocks, of course, were taken from the surface of the ground, and, having been subjected to climatic influences for several centuries, were soft for a depth of several inches from the surface ; but the trial demonstrated the fact that the deposit is a remarkably sound one, and I have every reason to believe that the percentage of high-grade sound marble will be much larger than in any of the deposits now being worked in the United States. In fact, when you consider that in developing marble quarries it is customary to take out and throw away from 10 to 20 feet of the surface material, the results obtained are unprecedented. The amount of the matter is that on an island in southeastern Alaska, within a few feet of deep water, there is a larger area of high-grade marbles than the combined area of all the quarries now being worked in the United States "—*Seattle Post-Intelligencer.*

THE GOLDSCHMIDT WELDING PROCESS.

The application of the Goldschmidt welding process for welding rails has lately undergone a simplification, according to a report in the *Zeitschrift des Vereins Deutscher Ingenieure*, Vol. XLV., p. 1545. The curcible in which the required quantity of thermite is to be melted is made in the form of a funnel, about 16 inches in height and 12 inches in diameter at the top. The bottom end is provided with an opening about ¾ to 1 inch in diameter, which serves as a pouring hole. An iron plug is used to close the hole during the reaction, and the hole is freed preferably by knocking this up into the liquid mass, when it is at once fused. In this way the precise moment for discharging the contents is easier to regulate than by allowing the plug to remain in position until melted out. The top of the plug should be covered with a little fine dry sand to prevent it burning out too soon. After filling the crucible, the open upper end is covered with a metal hood, provided with a hole, through which the contents are ignited. This arrangement has the advantage that the metal flows into the mold before the slag, whereas in ordinary crucible the slag always runs off first. The loss of heat by radiation is much reduced, and the lining is not exposed for so long to the heat of the thermite, since the time it remains in the crucible is no more than what is just required for the reaction—that is, a few seconds only. In welding objects, the surface on which the thermite is poured is first cleaned with a file, but the abutting ends of the rails or bars need not be touched at all.

Further applications of the Goldschmidt method for the production of high temperatures are described by H. Goldschmidt in *Stahl and Eisen.* Its use in welding is especially referred to, and it is pointed out that through its aid chromium and maganese are now being manufactured on a commercial scale. The manufacture is effected in a large crucible-like vessels in which some hundredweights of the metal are made at each operation. The reaction is so rapid that this operation requires only about half an hour for its completion. While the carbon-free chromium made is utilized in particular for steel, the pure manganese is used for the manufacture of copper manganese alloys. Up to now the only form in which chromium has been available to the steel manufacturer was as ferrochrome, containing from about 40 to a maximum of 65 per cent. of chromium. This product contains usually from 10 to 12 per cent. of carbon, calculated on the chromium contents. The chromium is, therefore, present in ferrochrome, not as the free metal, but as a chromium carbide. All steels, therefore, that are produced with the aid of ferrochrome are, strictly speaking, not chrome steels, but chrome-carbide steels. With the pure chromium other alloys can therefore be made which have other properties. Thus, for instance, they are less hard. It is possible, too, by the use of metallic chromium, to make chrome steels with a much higher chromium contents than has been hitherto possible, owing to the simultaneously high carbon contents which would otherwise have to be present. In the open-hearth process the chromium is added toward the end of the operation ; at a time, that is, when the introduction of impurities is most objectionable. The loss by oxidation when high percentage ferrochrome is used amounts to from about 20 to 25 per cent., but is considerably less when pure chromium is employed. If it is a question of producing chrome steele of, say 2.5 per cent. chromium, and with 0.2 or even 0.1 per cent. of carbon pure chromium must be employed. Otherwise the carbon contents could not be kept below 0.30 or 0.35 per cent. In the manufacture of crucible cast-steel also, when it is desired to produce a metal with only from 1 to 2 per cent. of chromium, the metallic chromium is especially valuable. Further, in the case of the manufacture of high chromium tool steels, with 6 to 10 per cent. of chromium, metallic chromium is utilized. One works is indeed endeavoring to increase still further the chromium contents in metal to be used for tool steel.

Metallic manganese has not found the same application to steel manufacture as has been the case with chromium. In the first place, manganese steels have been less in evidence than chrome steels, and then, too, the relatively high cost of the metallic manganese has stood in the way of its utilization. Thus its cost, as compared with the ferromanganese commonly employed, is about ten times as great. It has, however, been largely utilized of late in the manufacture of alloys with copper. Pure manganese oxidizes very slowly in the air, and forms alloys with considerable facility.

The Goldschmidt method is used also to manufacture pure alloys of iron and manganese with titanium. It is possible to produce in this way alloys of iron and manganese containing 40 per cent. of titanium, but these only melt at so high a temperature that it has not been found desirable to make the commercial product contain more than 20 to 25 per cent. of titanium. The addition of this alloy to the iron must be effected at a high temperature. Such small additions of titanium as a few tenths per cent. are stated to give iron and steel a very tough, fibrous structure. It has recently been found that titanium will alloy much more freely with manganese than with iron, and instead of the ferrotitanium a manganese titanium with from 30 to 35 per cent. of titanium has been made. If this alloy is used, twice as much manganese is introduced into the metal under treatment. As, however, usually only 0.1 to 0.2 per cent. of titanium is required, the additional manganese contents does not matter. This manganese-titanium is more readily dissolved by ingot iron than the iron alloy. A better method of introducing the titanium consists, however, in its addition in a nascent state, by the addition of so-called manganese-titanium thermite to the ladle containing the molten iron. The titanium takes up dissolved nitrogen, the resulting red crystals having been subsequently found by micrographic investigation.—*The Iron Age.*

BOOK NOTICES.

A Manual of Volumetric Analysis. Treating on the subjects of Indicators, Test-Papers, Alkalimetry, Acidimetry, Analysis by Oxidation and Reduction, Iodometry, Assay Processes for Drugs with the Titrimetric Estimation of Alkaloids, Estimation of Phenol, Sugar; Tables of Atomic and Molecular Weights. By Virgil Coblentz, Ph.D., Phar.D., F.C.S., Professor of Chemistry in the New York College of Pharmacy. Illustrated. Published by P. Blakiston's Son & Co., 1012 Walnut Street, Philadelphia, 1901. Octavo, 180 pages. (Price, $1.25, net.)

Chapter I is devoted to definitions, apparatus, and standard solutions. Chapter II treats of Analysis by Saturation, and describes indicators, test-papers, the preparation of standard acid and alkali solutions, alkalimetry, acidimetry, direct percentage estimators, empirical solutions in titrating, volumetric estimations of alkaloids. Chapter III is devoted to Analysis by Oxidation and Reduction, and treats specifically of estimations with potassium permanganate, direct and indirect methods of estimation with potassium dichromate, determinations involving iodine and sodium thiosulphate V. S., estimation of free iodine and iodometry. Chapter IV is devoted to Analysis by Precipitation, treating of the estimation of combined halogens, their acids and solutions of silver nitrate and sodium chloride V. S., Volhard's or thiocyanide method. Chapter V treats of the estimation of phenol, and the volumetric estimation of the sugars. An Appendix gives some useful tabular data. W.

Practical Calculation of Dynamo-Electric Machines. A manual for electrical and mechanical engineers, and a text-book for students of electrical engineering. By Alfred E. Wiener, E.E., M.E. Second edition revised and enlarged. 8vo, cloth, pp. 750, 390 illustrations and 125 tables. New York: Electrical World and Engineer, 1902. (Price, $3.00.)

Part I. Physical Principles of Dynamo Electric Machines, two chapters. Part II. Calculation of Armatures, eight chapters. Part III. Calculation of Magnetic Flux, three chapters. Part IV. Dimensions of Field-Magnet Frames, three chapters. Part V. Calculation of Magnetizing Force, two chapters. Part VI. Calculation of Magnet Windings, four chapters Part VII. Efficiency; Calculation of Dynamos, Motors, and Motor Generators, six chapters. Part VIII. Practical Examples of Dynamo Calculation, two chapters. Appendices: I. Table of Dimensions of Modern Dynamos. II. Wire Tables and Winding Data. III. Dynamo Specifications. IV. Remedies for Sparking Due to Various Causes. W.

Water-Tube Boilers. Based on a short course of lectures delivered at University College, London. By Leslie S. Robertson. M. Inst. C.E., etc. (With upwards of 170 illustrations.) 8vo, pp. XV + 213. New York: D. Van Nostrand & Co., 1901. (Price, $3.00.)

Chapter I of this work defines the water-tube or tubulous boiler and presents some historical data. Chapter II treats of circulation in water-tube boilers, the rate of heat transmissions, ratio of heating surface to greater surface efficiency of heating surface, and forced draught. Chapter III is given up to the description of individual types of large tube boilers of this class. Chapter IV describes similarly the small tube boilers; and Chapter V consists of boiler accessories, such as reducing valves, steam separators, feed-water heaters, etc. W.

Electro-Magnets: their design and construction. By A. N. Mansfield, Sc.B. (Van Nostrand's Science Series, No. 64.) New York: D. Van Nostrand Company, 1901. (Price, 50 cents.)

The above-named work is a completely revised edition of No. 64 of "Van Nostrand's Science Series," originally entitled, "Electro-magnets, the Determination of the Elements of their Construction," by T. H. du Moncel.

Encyclopédié Scientifique des Aide-Mémoire. Librairie Gauthier-Villars, Paris, France.

Since our last notice of this work, the following volumes have been issued:

Vigneron (Eug.), Ingénieur, ancien Professeur à l'École supérieure d'électricité.—Mesures électriques. *Essais industriels.*

Equevilley (R. d'), Ingénieur civil des constructions navales, ancien Ingénieur aux Forges et Chantiers de la Méditerranée.—Les bateaux sous-marins et les submersibles.

Minet (Ad.), Ingénieur chimiste, Directeur du Journal *l'Électrochimie.*—Galvanoplastie et Galvanostégie.

Astruc (*H.*), Ingénieur agricole, Préparateur à la Station œnologique de l'Aude.—Le vin.

La Convention du Mètre et le Bureau International des Poids et Mesures. Par Ch-Éd. Guillaume. Paris: Gauthier-Villars, 1902. 4to, pp. 238.

Following closely upon the appearance of Bigourdan's valuable historical work, "*Le Système métrique des Poids et Mesures,*" this beautiful volume forms an admirable supplement to it.

After a brief review of the origin of the metric system and the progress made in metrology from that time to the establishment of the International Bureau in 1870, the author proceeds to describe the laboratories of the Bureau at Sèvres, near Paris, and their elaborate instrumental equipment.

The following chapters are devoted to a detailed account of the researches and other activities of the Bureau during the quarter of a century which has elapsed since these laboratories were inaugurated.

In the main part of the book Dr. Guillaume describes and discusses in a most interesting manner the development of methods and instruments used in the construction standards of length and mass, as well as the auxiliary measurements of temperature and barometric pressure.

In much of this work, as is well known, he has been conspicuous as one of the most active and successful members of the staff of the Bureau, but he accords the fullest measure of credit to his associates, irrespective of their nationality. It is pleasing to note in this connection his appreciation and extended account of the work of our fellow-countryman, Professor Michelson, of determining the wave-lengths of certain radiations (of the cadmium spectrum in particular) in order to fix the length of the meter in terms of a constant of nature, an investigation which resulted in determinations of "a certainty and precision hitherto unknown."

The remaining chapters deal with some special investigations conducted by the Bureau, and the decisions of the International Committee. There are also some appendices relating to various matters of interest in connection with the use of the metric system.

The book may be recommended not only as an admirable *résumé* of the labors of the International Bureau of Weights and Measures, but also as a lucid and scientific account of the state of metrology at the beginning of the twentieth century.

Its value is greatly enhanced by numerous excellent illustrations.

H. F. K.

Franklin Institute.

[*Proceedings of the Stated Meeting held Wednesday, March 19, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, March 19, 1902.

President JOHN BIRKINBINE in the chair.

Present, 118 members and visitors.

Additions to membership since last report 12.

The Actuary reported, by direction of the Board of Managers, the loss of one of its members, Mr. Stacy Reeves. The Actuary likewise presented a report of a Special Committee of the Board to devise means of increasing the revenues of the Institute. This report, which received the endorsement of the Board, recommended an increase of the annual dues of the various classes of members.

By direction of the Committee on Science and the Arts, the Secretary reported a proposed amendment to the By-Laws, increasing the number of the Committees' members from forty-five to sixty, of whom twenty shall be chosen each year.

Prof. Lewis M. Haupt presented a communication on "The Curvilinear Currents," which was freely illustrated by lantern slides.

Mr. F. E. Ives followed with a description of "A New Photo-Micrographic

Process." Mr. Ives illustrated his apparatus and methods. Referred to the Committee on Science and the Arts.

Mr. Morris Earle, for Messrs. Williams, Brown & Earle, described a new form of light adapted for the stereopticon and also for a variety of photographic work.

Mr. Earle projected satisfactorily a number of lantern slides with this light in a single, and also in a dissolving, magic lantern. He also exhibited a convenient form of bromide enlarging apparatus, using the same light, by means of which either the professional or the amateur photographer is enabled to make bromide enlargements of any size, and in a shorter time than is ordinarily required. By a simple change in this apparatus, and the use of an ordinary hand-camera, it can also be employed for the making of lantern slides from negatives of any size.

The new light is also arranged on a pedestal stand with a parabolic illuminator, by means of which photographic copying and photo-engraving work can be done rapidly and cheaply.

The "Bright White Light" apparatus consists of a steel tank, in which is placed a few quarts of kerosene oil; air is pumped into the tank by means of a small bicycle pump to a pressure of 60 pounds. This pressure forces the oil through a slender tube to the lamp, whence it escapes through a minute aperture into the vapor tube. The vapor tube is first heated by igniting a spoonful of alcohol beneath it; afterward the heat is maintained by a Bunsen flame in the lamp itself. In this hot vapor tube the oil is instantly converted into vapor, mixed with the air and driven by pressure into the burner, where it produces a brilliant light.

Three quarts of kerosene oil, it is claimed, will run the light for twenty-four hours. It is recommended by the makers because of its portability (the entire weight of the apparatus being only ten pounds), its cheapness and ease of operation.

Mr. W. N. Jennings followed with an exhibition of a series of lantern slides which he had made, representing some characteristic local scenes during the recent storms and floods.

Adjourned. WM. H. WAHL, *Secretary*.

COMMITTEE ON SCIENCE AND THE ARTS.

[*Abstract of proceedings of the stated meeting held Wednesday, February 5, 1902.*]

MR. LOUIS E. LEVY in the chair.

The following reports were adopted:

(No. 2149.) *Molding Machines.*—Harris Tabor, Edgar H. Munford and Wilfred Lewis, Philadelphia.

ABSTRACT.—These machines are the subject of a number of patents granted to Tabor in 1895, to Tabor and Munford in 1897, and more recently to Lewis, reference to which is made in the report.

The various devices have been carefully examined, and one of them has been observed in daily work for about a year in a large foundry in this city where several other forms of power molding-machines are used.

One man, an unskilled operator, is now turning out at this foundry from seventy-five to one hundred and ten molds per day in snap-flasks, the output depending somewhat on the character of the patterns. A skilled operator in the same foundry will put up about one-half of this number of flasks from the same pattern per day. In making 2½-inch and 3-inch oil dishes, which are exceedingly thin castings (about ⅛-inch thick, 1¼ inches long, 2½ inches deep), the same operator will set up about eighty flasks per day as compared with twenty similar molds made by hand.

The Tabor Vibrator Machine is of two distinct types, viz , the "Split Pattern Vibrator Machine" and the "Vibrator Frame Machine," where solid patterns are attached without change to protecting carriers to a frame to which is attached the vibrator, by which means the patterns are handled and drawn from the finished mold. Both of these types are built for either hand or power ramming.

The patterns may be changed on either the "Split Pattern" or the "Vibrator Frame" machine very quickly—in some cases in less than three minutes—and a large day's work has been made with even changes of pat. terns during the day. In one foundry, where a number of these machines are installed, it is not unusual to send patterns to be fitted to the Tabor Machine to make a single mold. In these machines "stripping-plates" have been practically abandoned, for stripping-plates, when properly used, are expensive.

The advantage of the Tabor Machine of the "Split Pattern" type is shown by a statement that, given a set of split patterns, these may be fastened directly to the pattern-plates or machine and used without change, whereas, with other forms of machines embodying stripping-plates, these patterns must be fitted to what are known as building-down pieces, plane top and bottom, and the stripping-plate must be brought accurately to the joint-surface and cut exactly to the outline of the pattern at this surface.

When new and special patterns are made for the Tabor Machines, they are of much cheaper form than the costly appliances which are required on all other machines which combine good work with pattern-drawing mechanism.

In conclusion, the investigators report that the Tabor Vibrating Molding Machines, using either split or undivided patterns without stripping-plates, mark a distinct and valuable improvement in the art of molding. The Scott award is recommended. [*Sub-Committee.*—A. E. Outerbridge, Jr., Chairman ; Wm. C. Henderson, Robert Job.]

(No. 2152.) "*Star*" *Ventilator.*—Henry O. Herman. (Referred by the Judges of the National Export Exposition.)

ABSTRACT.—The apparatus is intended to be attached at the top of buildings or flues, and to utilize any current of air that may be blowing, for the purpose of creating an upward current through the ventilator. (The details unintelligible without an illustration.)

The committee named to make the investigation made some experiments with a 10-inch ventilator of this construction to determine its suction-power under various conditions, and found that the quantity of air discharged was sufficient to satisfactorily ventilate a room to which it might be applied.

The report recommends the award of the Scott Legacy premium and medal. [*Sub-Committee.*—H. W. Spangler, Chairman; T. Carpenter Smith, J. M. Emanuel, Wm. McDevitt.]

(No. 2194.) *Method of Making Large Objects from Aventurine Glass.*—Edward M. and Sydney B. Walsh, Janvier, N. J.

ABSTRACT.—The process is covered by U. S. patent, No. 629,973, dated August 1, 1899, granted to applicants and involving the production of aventurine glass in masses of such size as to be suitable for the manufacture of large objects, such as slabs, tiles, plaques, etc., which, hitherto, has not been accomplished.

The new process consists substantially in melting together in a muffle furnace in a suitable mold a quantity of selected fragment of the glass, with exclusion of the air, the mold corresponding approximately to the size of the sheet desired. When melted, the fragments form a compact mass, which is previously cooled and molded, and finally cut and polished in the usual way.

The inventors submitted specimens of their product in the form of slabs of considerable size.

In view of the novelty of the process, and the new and valuable properties of the product, the Scott award is recommended. [*Sub-Committee.*—Joseph W. Richards, Chairman; Henry F. Keller, Samuel Sartain.]

(No. 2196.) *Hammer's Experiments in Long-Distance Phonographic and Telephonic Transmission.*—William J. Hammer, New York.

The experiments here referred to were made by Mr. Hammer in connection with a lecture delivered before the Franklin Institute, on February 4, 1889, and consisted in transmitting the sounds of the human voice, both talking and singing, and of instrumental music, between New York City and the lecture hall of the Institute, through 104 miles of Long-Distance Telephone Company's metallic circuit, 98 miles of which was overhead or underground, and the remaining 6 miles under water.

The experiments involved several new and ingenious combinations of telephonic and phonographic transmission.

The investigators to whom the subject was referred find, with other things, that the author at an earlier date above named, had successfully demonstrated the possibility of employing the phonograph and the land-operating telephone for use as a relay and repeater in the telephonic transmission of sound over long-distances. The Scott award is recommended to the author for this early demonstration. [*Sub-Committee.*—L. F. Rondenella, Chairman; Carl Hering, Arthur J. Rowland, Thos. Spencer.]

(No. 2197.) *Governor System.*—Francis M. Rites.

ABSTRACT.—This system is covered by a number of patents (1886–1900) granted to applicant. The system investigated involves a method of designing the governors so as to realize the advantage to be gained in utilizing inertia to assist centrifugal force in shaft-governors and incidentally make proper provision for such disturbing influences as friction in the joints, the lack of gravity balance, and the friction and inertia of reciprocating parts.

Generally speaking, the patented inventions in this field have aimed at accomplishing one or the other of these functions, frequently at the expense of simplicity. The Rites Governor System has its value more in the method adopted in designing the governors so as to condense all these functions in a single governing mechanism.

The report makes special reference to one of Rites' patents (No. 534,579, February 19, 1895), which contains as its essential elements the combination in a single-weight shaft-governor of an inertia weight and centrifugal weight integrally formed.

The body of the report bears upon the system of calculation or design developed by Mr. Rites by which he can clearly predetermine the performance of the governor as to regulation, rapidity of action and stability.

For the details the reader is referred to the report. The award of the John Scott Legacy Premium and Medal is recommended. [*Sub-Committee.*—A. Falkenau, Chairman ; H. W. Spangler, Hugo Belgram.]

(No. 2198.) *Trolley Wheel.*—C. H. Bierbaum, Buffalo, N. Y.

ABSTRACT.—This device (patented) consists of two stamped annular steel flanges and a central ring of copper forming the tread, held together by suitably casting within the central opening a hub with projecting flanges, which on cooling grips the parts, firmly holding them together. The chief advantages are cheapness of manufacture, on account of the small amount of copper used, and lightness.

The objectionable features in the use of steel flanges are the injurious effects of the hard steel on the trolley wire, which will be aggravated by the arcing which inevitably occurs, and the liability to accidental injury from striking an obstruction.

The novelty of the invention appears to consist in the method of holding the parts together by the cast hub. No award. [*Sub-Committee.*—Lucien E. Picolet, Chairman ; Wm. E. Bradley, Charles C. Heyl.]

No. 2199.) *Storage Battery.*—Wm. Bell.

(An advisory report.)

No. 2204.) *Improvements in Screw-Jacks.*—D. Glenn, Del Rio, Texas.

ABSTRACT.—This invention is the subject of letters patent of the U. S., Nos. 610,044–5, Nov. 30, 1901, to applicant.

(An advisory report.)

(No. 2208.) *Automatic Revolving Time-Table.*—Johnston.

(An advisory report.)

No. 2217.) *Safety-Brake for Trolley Lines on Steep-Grade Roads.*—H. C. Moyer, W. M. Blooming, Glen, Pa.

(An advisory report.)

The following reports were read and held over for final action at the next meeting, viz.:

(No. 2181.) *Enclosed Arc-Lamp.*—C. J. Toerring, Philadelphia.

(No. 2183.) *Stencil Machine.*—Andrew J. Bradley, St. Louis, Mo.

(No. 2185.) *Process of Treating Tool Steel.*—Fred'k U. Taylor, Maunsel White, Bethlehem, Pa.

(No. 2195.) *Single-phase Electric Motor.*—Wagner Electric Company, St. Louis, Mo.

(No. 2205.) *Automatic Electric Brake Motor.*—Crocker-Wheeler Electric Company, Ampère, N. J.

(No. 2206.) *Turret Lathe.*—Julius Wilhelm Pittler, Leipzig-Gohlis, D. R.

The annual election of the Committee resulted in the choice of Mr. Thomas P. Conard for Chairman, for the coming year.

The Secretary reported that the Committee's proposal for the issue of a diploma to accompany the award of the Edward Longstreth Medal of Merit had been approved by the Board of Managers of the Institute; and that for a similar diploma to accompany the Scott award, the Board of Directors of City Trusts had given its sanction. W.

SECTIONS.

(*Abstracts of Proceedings.*)

ELECTRICAL SECTION.—*Stated Meeting*. Thursday, February 20th, 8 P. M. President Morris E. Leeds in the chair. Present, 32 members.

The following officers were elected to serve for the current year, viz.: President, Thomas Spencer; Vice-Presidents, Joseph Richards, Geo. T. Eyanson; Secretary, Richard T. Binder; Conservator, Dr. Wahl.

The meeting was devoted principally to a communication from Prof. Arthur J. Rowland, who presented some notes of his observations of the behavior and efficiency in service of the American type of Nernst Lamp. The speaker's remarks were quite informal, and indicated that while the lamps gave a fine light, there was still room for improvement in certain details of construction before they could be said to be completely adapted for commercial service.

Mr. J. D. Pierce exhibited and described a new type of miniature receptacle for incandescent lamps, which embodies some novel features of construction.

RICHARD L. BINDER,
Secretary.

PHYSICAL SECTION.—*Stated Meeting* held Wednesday, February 26th. Dr. A. S. Mackenzie in the chair.

Dr. Wm. S. Franklin, of Lehigh University, Bethleham, Pa., presented a communication on "Poynting's Theorem," which was illustrated with the aid of lantern slides.

The subject was discussed by Drs. A. T. Mackenzie, Geo. F. Stradling, H. M. Lloyd, Prof. Geo. A. Hoadley and Mr. Jesse Pawling, Jr.

The thanks of the meeting were voted to Dr. Franklin Adjourned.

JESSE PAWLING, JR.,
Secretary.

CHEMICAL SECTION.—*Stated Meeting* held Thursday, February 27th. Mr. Lyman F. Kebler, President, in the chair. Present, 15 members.

Mr. Wm. H. King, of the Chemical Division of the U. S. Dep't. of Agriculture, read the paper of the evening on "The Estimation of the Carbohydrates with especial Reference to Food Products." The paper was discussed by Messrs. L. F. Kebler, M. I. Wilbert, C. A. Hexamer, Dr. H. F. Keller and the author. (The paper will be published in the *Journal*).

The meeting passed an unanimous vote of thanks to Mr. King for his admirable communication, and adjourned.

WM. H. WAHL,
Secretary pro tem.

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Stated Meeting*, held Thursday, March 6th. President Dr. Henry Leffmann in the chair.

It was announced that the paper originally intended for this meeting, *i.e.*, "The Microscope in the Determination of Geological Age," by Mr. Lewis Woodman, had, for unavoidable cause, been postponed.

Dr. Leffman presented a communication entitled "Recent Applications of the Microscope to Sanitary Questions." The subject was illustrated with photo-micrographs projected on the screen, and by means of the table microscope.

Mr. Fred. E. Ives read a communication on "Photography as a Means of Recording Scientific Phenomena," and presented an ingeniously contrived camera-box, adapted for use with the microscope. In demonstrating the possibilities of this device, Mr. Ives also showed a very complete and compact folding microscope of his devising.

Mr. Ives proceeded to illustrate the subject by projecting some photomicrographs, showing the high-class work that could be done with very little effort. Adjourned,

MARTIN I. WILBERT, *Secretary.*

ELECTRICAL SECTION.—*Stated Meeting* held Thursday, March 20th. President, Thomas Spencer, in the chair.

Mr. Caryl D. Haskins, of the General Electric Company, read the paper of the evening, on "Electrical Measuring Instruments."

The speaker dealt with such electrical measuring instruments as are used in commercial practice—both instruments of position (as switchboard instruments), and portable instruments.

The paper constituted a practical treatise on the various limiting conditions which surround the use of instruments of this class in various branches of the art, for varying purposes, and dealt extensively with the principles which have been resorted to, to achieve the desired end under these varying conditions.

Initial accuracy and permanence of same, dead-beat qualities, character and distribution of scale, influences due to external causes, etc., were dealt with and analyzed in relation to the form of physical structure used in the coersive mechanism.

Mr. Haskins' paper was illustrated with lantern slides, embodying both photographs of instruments and numerous diagrams. The subject was discussed by Richard L. Binder, Prof. Arthur J. Rowland, Carl Hering and the author.

At the close of the discussion the meeting passed a vote of thanks to Mr. Haskins for his interesting and comprehensive treatment of the subject.

RICHARD L. BINDER, *Secretary.*

JOURNAL

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FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CLIII, No. 5. 77TH YEAR. MAY, 1902

Mechanical and Engineering Section.

Stated meeting held Thursday, October 16, 1901.

MR. JAMES CHRISTIE in the chair.

THE CONSTRUCTION AND INSPECTION OF STEAM BOILERS; WITH ESPECIAL REFERENCE TO THE "CITY OF TRENTON" DISASTER.

DISCUSSION.

MR. JAMES CHRISTIE:—Gentlemen, in the absence of the president of the Section, I have been requested to take the chair at the meeting; and as there is no other business of importance, we will proceed at once without any formality to the subject chosen for general discussion this evening.

The subject, as announced on the bulletin, is "The Construction and Inspection of Steam Boilers; with especial reference to the 'City of Trenton' disaster." The Committee on Sectional Arrangements has requested Mr. Samuel M. Vauclain, Superintendent of the Baldwin Locomotive Works, to open the discussion.

[Before proceeding with the discussion, Mr. Vauclain read a letter which he had addressed to the Neafie & Levy Ship

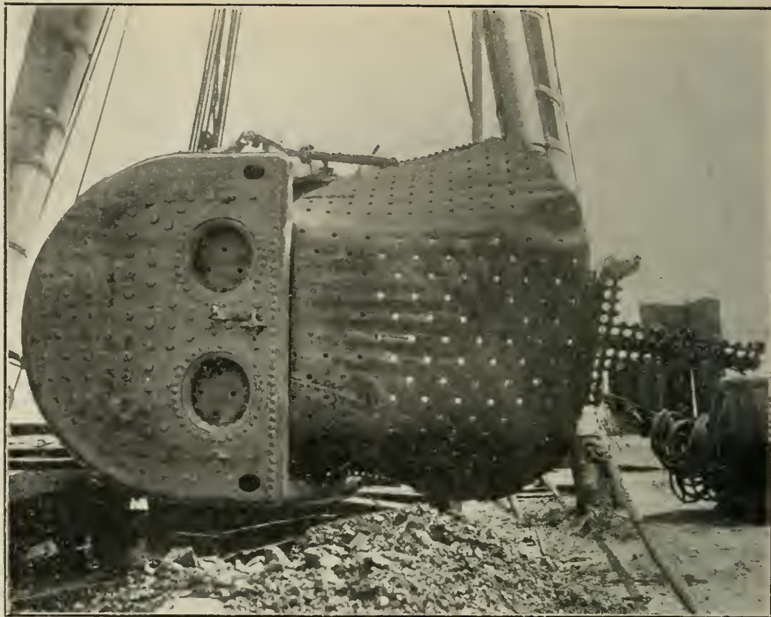

PLATE I.—Front end of exploded boiler of the "City of Trenton," showing crown-sheet with part of tube-sheet attached.

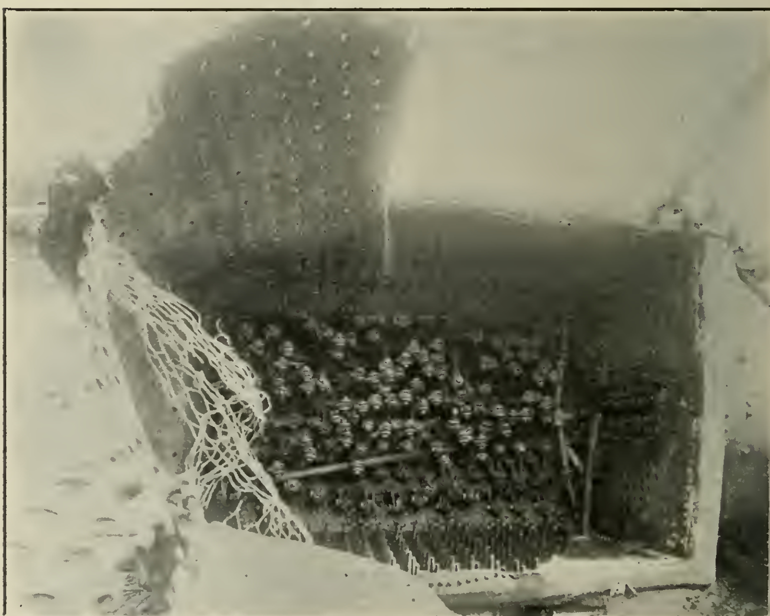

PLATE II.—Exploded boiler lying on one side with tube-sheet to left and side of fire-box seen above.

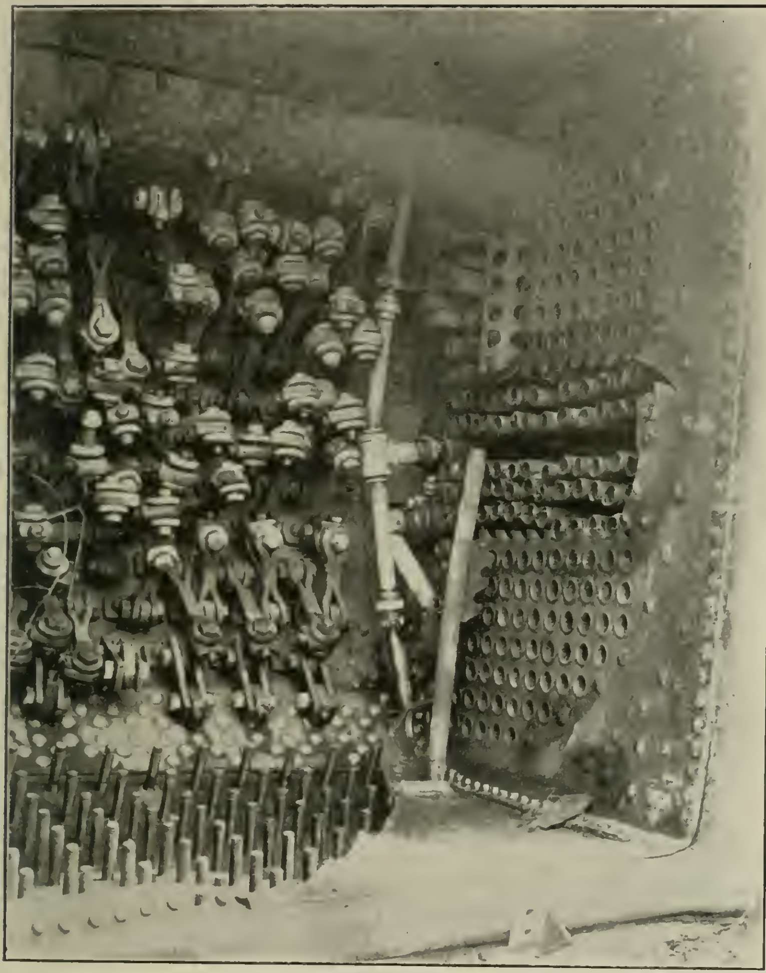

Plate III.—Enlarged section of Plate II showing ruptured tube-sheet and end of tubes.

Engine Building Company, the builders of the boilers of the "City of Trenton," asking for data referring to certain details of construction for a section of plate taken from the exploded boiler, for the purpose of making tests of same, and for information concerning the location of the exploded boiler on the boat, and the steam and water connections thereto.

To these inquiries an answer was received, the Counsel for the Company, J. Warren Coulston, Esq., saying in substance that, in his opinion, the present was an inopportune time to investigate the subject.

The chairman, therefore, stated that, in his judgment, the proposed discussion at this meeting would simply amount to an exchange of individual opinion among the members of the Section on the subject in its general bearings for the purpose of eliciting information respecting the construction and inspection of steam boilers, which would not represent the opinion of the Franklin Institute, and asked the meeting whether there was any objection to proceeding. No objection being offered, the chairman requested Mr. Vauclain to open the discussion.—THE SECRETARY.]

MR. VAUCLAIN:—Shortly after the explosion of the boat, through the courtesy of Mr. J. Shields Wilson and Mr. Sommers N. Smith (Mr. Wilson, I believe, operated the boat and Mr. Smith built the boat and also the boiler), I was permitted to view the exploded boiler on the wharf. This was a few days after the boiler had been taken out of the river.

It required but a glance at the condition of the crown-sheet to explain why the crown-sheet had blown down and the boiler had caused the damage that happened on that eventful day. The holes in the crown-sheet through which the crown-stays had been screwed were elongated, and were also enlarged in their upper diameter; or, in other words, in the diameter not next the fire but next the water —which indicated that as the sheet being hot, these holes had gradually enlarged in the upper diameter until the rivet-head of the stay could no longer hold it in position.

I have with me three photographs which were taken of the boiler as it rested on the wharf. A glance at the point there of these two would, I think, enable any one who has ever had any experience with locomotive boilers at once to understand—I find the Secretary has slides of these three and will show them on the screen.

The crown-sheet is in this section and was the part that first became overheated; and the condition of the holes are as you will notice: there is one which shows very plainly (the direction in which the light comes through the holes rather spoils the picture); but in this one you will notice how the sheet is drawn, drawing the holes in an oblong shape. There were a number of these holes that were so drawn, and all in this vicinity.

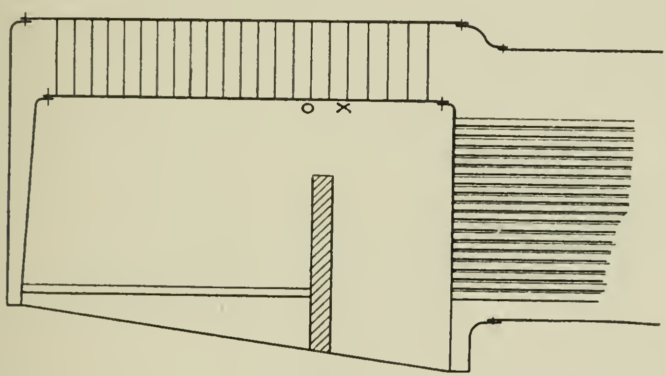

Fig. 1.

The picture now shown is a view showing the interior looking into the roof of the boiler, showing where the flue-sheet tore away and passed out with the crown-sheet. The crown-sheet is here, and shows very plainly the distortion, the holes being oblong and the sheet showing a corrugated appearance—part of it between these crown-stay holes.

This one gives you, perhaps, a little better idea of it. The light strikes it in a somewhat different manner. The light shadow on this print does not indicate the hole. Of course, a portion of the hole is shown in the very dark shadow. You will notice that the direction in which the photograph is taken shows the edge of the plate in the hole, as well as what light will pass through the hole; but it brings out very clearly the distortion of the hole found in the sheet.

The starboard boiler, however, was in position in the boat, and afforded an opportunity for an expert to learn exactly how the boiler was operated whilst it was in service; and this is what was found (*Fig. 1*): The boiler, having the ordinary fire-box of a locomotive boiler, and the crown-sheet stayed and held in a similar manner to locomotive boilers, with the flues extending from the flue-sheet in the fire-box to the flue-sheet in the smoke-box. The length of the fire-box was about 9 feet 6 inches, but was shortened so as to give a grate-bar about 6 feet in length, as near as I could judge. Not having any drawings or accurate dimensions it is impossible for me to say what that length was; but at the end of these grate-bars a bridge-wall was built about 17 inches off the crown-sheet; consequently, all the products of combustion (the wall being there) had to pass over this bridge-wall on the road to the flues, and consequently the flames were concentrated on this portion of the crown-sheet. It is therefore evident that when the water in the boiler got to too low a limit or down to the crown-sheet level, that the violence of the heat at this point overheated the crown-sheet; even if there had been a very little water over it, it would have driven that water away; and as the stays were constructed in this manner (being merely an eye-screw screwed through the crown-sheet and riveted there, *Fig. 2*), this portion of the metal would be the first to become heated, because on the upper side the water could not reach the plate. But I attach very little importance to this, as the time that would be required to heat this red-hot would be a little longer than what would be required to heat the plate surfaces, so very thin, after they were once exposed. The action when the plate commenced to get hot was with the stud being screwed in that way, and another one over here; there is a pocket in that manner (*Fig. 3*); and of course, when the plate would pocket in this manner on the other side of

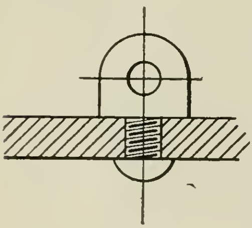

Fig. 2.

this stud the upper diameter would increase, and, consequently, the thread would slip—the thread of the stud—and the entire strain or load would be pushed or cast upon the rivet head of this stay. This in turn, being red-hot, would afford very little, if any, resistance; and if some of the old studs in the boiler are examined it will be found that just such a thing took place. The threads for at least one-half, and on some of them for two-thirds, are very good—are sloughed off somewhat, owing to their being hot; but the rivet end is pulled off, and some of them were sloughed off, as in the case of the one that you see before you: you can see the burr still adhering to the bolt. Those which were in a more remote part of the fire-box will show that a little more of the stud had been torn off with the sheet as it went down.

It is reasonable to suppose that when once three or four or a half-dozen of these stays became disengaged from the

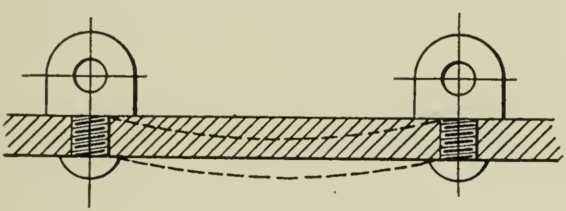

FIG. 3.

crown-sheet, nothing remained to happen but an entire rupture of the boiler; and as you noticed, from the pictures shown on the screen, that is just what happened: the sheet turned practically inside out—turned right down and extended slightly back, into the back head of the boiler, closing up the fire-box door-holes.

It is difficult to say very much about anything that requires little to be said of it; and this is one of those cases. It is difficult to theorize to any very great extent when we have evidence so plain before us as to what occurred, or as to what existed at the time this boiler exploded; the evidences in the sheet and in the end of the studs. Just how it was caused is perhaps a different matter, but certainly the primary cause of the explosion of this boiler was due to a scarcity of water in it at the time that the crown-sheet let go. Mr. Chairman, I suppose that a great many present would like to have something to say in regard to this matter and I, for the present, will close my remarks. I do not wish, however, to have it understood that I have said all that I can say in regard to this matter, and would be pleased to answer any questions which any one may wish to ask me. [Applause].

THE CHAIRMAN:—There are quite a number of gentlemen present who I have no doubt will be very glad to say something on this subject; and we hope that they will not neccessarily confine their remarks to this particular case, but to the subject of weaknesses of steam boilers, and boiler inspection, and boiler disasters in general. I would be glad to hear from Mr. John M. Hartman.

MR. JOHN M. HARTMAN:—Mr. LeVan was on the ground right after the boiler was taken out. He made a thorough examination of it, and would be in a better position to talk about it than I would be; and I would ask that you call on Mr. LeVan to present his observations.

MR. W. BARNET LE VAN:—On the ninth day of September I went inside of the exploded boiler. The lantern slides are misleading, from the fact that the background seems to show that the sheets bulged. The sheet was not bulged; neither was it bagged between the stay-bolts. Any one looking at the picture would think the sheet sagged down like the belly of a cow; but no such thing occurred. I examined the boiler and could find no sagging between the stay-bolts; I could find no evidence of heat whatever; I could see the line of the water-mark, some four inches above the crown-sheet, distinctly marked. The fact is, in the first place, that the mistake was made of having too coarse a thread on the stay-bolts; in the second place, they had too short a hold and an insufficient overhanging or rivet to hold it in place. The elongation of those holes no doubt occurred by the force of the explosion; the fact that the threads are torn off as though turned off in a lathe shows that they had not sufficient hold, due to the coarseness of the thread, which allowed them to strip off, as one would tear apart a sheet of postage stamps. The faces of the sheet as it was driven against the front head indicated there were no cracks or bags whatever in the crown-sheet.

MR. VAUCLAIN :—Mr. President, as this is a flat contradiction of my statement, I desire the opportunity of answering Mr. Le Van. In the first place, gentlemen, my eyesight is good, and I can tell when a sheet is bagged as well as any one. It is not necessary when a crown-sheet is hot and is driven out of the fire-box and lapped around the back head of the boiler for it to bag down like the belly of a cow; but this sheet had decided bagging like that between the stay-bolt holes, or between the crown-stay holes; and the crown-stay holes were in that shape [indicating.] It would be very difficult to tap a hole exactly in that shape and screw

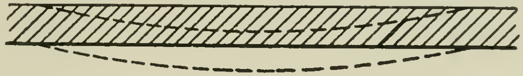

FIG. 4.

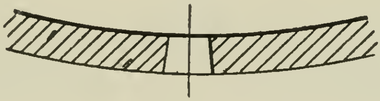

FIG. 5.

a straight stud in it. This diameter, the larger diameter, was on the upper side. Now, Mr. Le Van says that the reason why this crown-sheet came down was because there were not enough threads screwed through the crown-sheet to hold it there, and the riveting underneath was insufficient. I have personally superintended the construction of about 16,000 locomotive boilers, and in the majority of those locomotive boilers the crown-sheet was held with a stay which is screwed through and simply riveted over, and screwed through the roof-sheet as well. Any gentleman here present—if he will take the trouble to come to the Baldwin Locomotive Works—can see that construction every day in the year, and we have no trouble with them, and they hold. Furthermore, I have built hundreds of locomotive boilers to specifications in which this was the crown-sheet [indicating] and these were the crown-bars; this was a washer between the crown-bar and the crown-sheet some 2½ inches in diameter. The bolt was the T-headed bolt ⅞ inch diameter; whereas this stud was 1⅜ inch; the hole in the sheet was a plain hole, without a single thread in it, and the bolt was simply passed through and headed on the under side. Now, if no threads in the sheet would hold the crown-sheet with the rivet head turned underneath, it is fair to presume that two threads, or three threads, or four threads—but as I counted, in this sheet, I think, six threads, five of which were fully engaged, I think they were sufficient to sustain the load that was placed upon them.

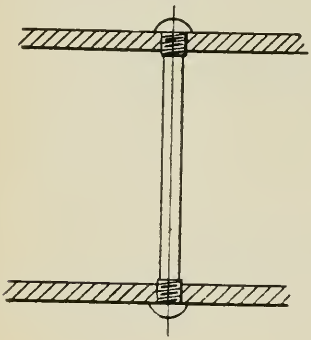

FIG. 6.

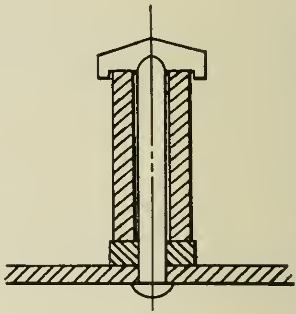

FIG. 7.

These stays carried each the equivalent of about thirty-nine square inches of surface, which at 175 pounds per square inch would make very little over 6,000 pounds (between six and seven thousand pounds) total load. The amount required to pull this stud out of this sheet with a very indifferently riveted head is a little over 40,000 pounds, which would be a factor of safety of 6. The sheets were ½-inch thick; and under the Government rules the space between the studs is within the limit. Or, in other words, that the sheet exposed to the pressure in its normal state would not bulge or pocket, but before it would bulge or pocket would have to be warm, and that was the condition in which I found this sheet.

MR. LE VAN:—How far apart do you make your stay-bolts? Do you make them $8\frac{3}{4}$ inches one way and $10\frac{3}{4}$ inches the other?

MR. VAUCLAIN:—We do not make our stay-bolts $8\frac{3}{4}$ inches one way and $10\frac{3}{4}$ inches the other way; and to the best of my knowledge nobody does.

MR. LE VAN:—Didn't this boiler have such?

MR. VAUCLAIN:—No! The entire area suspended between each bolt was 39 inches. (*Fig. 8.*)

THE CHAIRMAN:—The subject is now open for discussion; and I wish to remind the gentlemen present that in order to get at the facts of the matter, and in order that the audience may all understand, I think it may be well that the

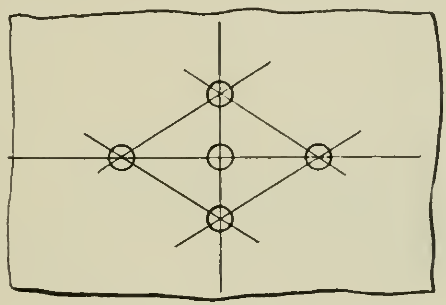

FIG. 8.

gentlemen present here will relieve the Chair and avoid, if possible, anything in the way of personal colloquy.

MR. JOHN M. HARTMAN:—Mr. Chairman, gentlemen, I wish to begin by saying that Mr. Wilson, President of the Steamboat Company—an old friend of mine—came to me with the request that I should investigate the cause of this explosion and get at the bottom facts. We will take first these lugs (*Fig. 9*), 10 threads to the inch. These shanks run about $\frac{13}{16}$-inch long from the shoulder to the head of the rivet. The crown sheet is $\frac{1}{2}$-inch thick. Here at the shoulder is a groove $\frac{1}{8}$-inch wide, cut down into the screw, leaving only $\frac{3}{8}$-inch of good thread. This $\frac{3}{8}$-inch is further reduced by riveting over at the edge of the thread, which harms it. Practically we have three good threads. Then we have the rivet, riveted over the edge of the hole about $\frac{3}{16}$-inch and drawn out quite thin. With that small amount of thread and the thin rivet head, we have not sufficient strength for holding up the crown. Here is a lug (*Fig. 10*), screwed carefully into the plate and is cut in two, to give a better idea of the section. To show the strength of these stay-bolts and the crown, a cast-iron frame was made to which to bolt a $\frac{1}{2}$-inch plate of the same quality as the crown-sheet. Four $1\frac{1}{4}$-inch bolts passed through the frame at the same distance as the stay-bolts are from center to center, to hold the plate to the frame. The central hole was tapped and a lug (*Fig. 10*), screwed in and riveted over, making all the conditions as nearly like the original as possible. On the testing machine, at 28,000 pounds, the rivet head cracked and

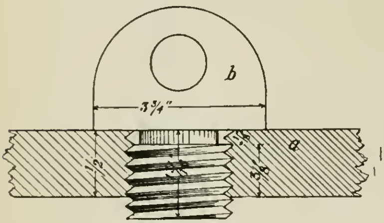

FIG. 9.

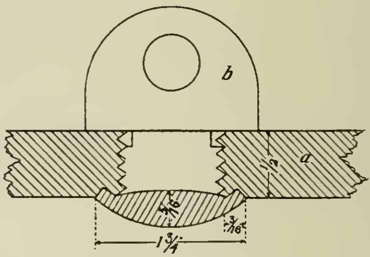

FIG. 10.

spalled around the edges, the lug kept yielding and the plate buckling for twenty-five minutes at 28,000 pounds, when 32,150 pounds was applied, which ruptured the lug and plate. The total buckling was $\frac{11}{16}$-inch. The thread in the plate stretched and little damage was done to thread in the plate and the thread on the lug. The angle or incline of the thread stretched the soft plate sideways and allowed the lug screw to pull through it. In soft plate the pulling side of the thread should be at right angles to the center of the screw on the stay or lug. The strain on each stay-bolt at 175 pounds per inch (the Government allowance) of steam pressure is 7,122 pounds. At the Government test-pressure it was 10,684 pounds. The Government uses a factor of safety of 6 or the safe load must be $\frac{1}{6}$ of the breaking strain. This would call for a breaking strain of 64,104 pounds, which would give a factor of safety of 3 or about one-half what the Government allows. The working drawing of this boiler shows the details of the lug with the shank 2 inches long and threaded the whole length. For what reason was that 2-inch shank put on? The working drawing shows the stay-bolt lugs (or eye-bolts as shown) screwed into two thicknesses of $\frac{1}{2}$-inch plate, whereas in reality it only screws into one thickness. Had a washer been put under the lug to keep the full depth of the thread in the plate and a deep rivet head turned over whose diameter would have been $\frac{21}{16}$-inch as used by Mr. Hartley, this crown-sheet would not have come down.

In regard to the crown-sheet the following is my opinion based on what I saw.

The crown-sheet gave way at the stay-bolt marked x, in exhibit (*Fig. 1*), near the middle of the sheet. The strain was then transmitted to the next two bolts, which gave way, followed by all the others. The crown-sheet buckled in coming down and tore off some of the stay-bolts bulging out at the sides and end of the boiler; the crown-sheet finally tore off about one-third of the tube-sheet and passed on down into the fire-box with part of the tube-sheet attached. The other end held on to the end-sheet, tearing part off and doubled over above the fire doors. The boiler went straight up in the air. The crown is intact, the tearing being below it. The flange near the top of the tube-sheet shows defective riveting. There was no explosion, simply a big puff when the boiler left the hull.

If the crown had parted over the bridge-wall, or at the hottest point, one part of it would have gone one way and part of it the other way. This, to my mind, removes all doubt about there being low water. The roll scale or surface, put on the plate of the crown in rolling it in the rolling mill, was still on the fire side of the sheet. I picked it off with my knife. This always disappears when a plate is overheated. The scale was in lamination and polished.

Mr. Vauclain:—Through the kindness of the Coroner, I think that I can explain why the stud was left 2 inches long. I have with me a blue-print of the boiler—supposed to be the working drawing of this boiler; and as it was not given to me by the manufacturers, I felt that it would be a breach of etiquette for me to show it; but you will see before you the details of these studs. This stud is drawn in detail, as Mr. Hartman has shown: it is 2 inches long; it has a rather flat face there. That is, perhaps, what led the boilermaker astray slightly; although I think the error was a very unimportant one. The working drawing, in the other hand, shows that (*Fig. 11*) there is the crown-sheet; this is the back head; it shows an I-bolt that is rounded and then comes down like that, with a thread on, and comes on through and is headed over, and another headed over.

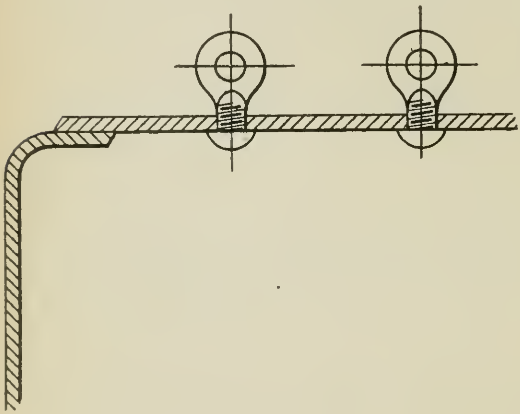

FIG. 11.

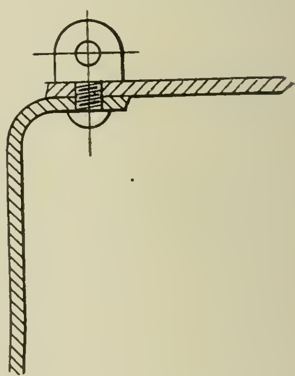

FIG. 12.

The intention evidently was on the part of the draughtsman, and the scale would indicate here that the distance from here down to what would be rivited over should be about 2 inches. His idea, no doubt, was to not have the thick metal come down on the crown-sheet any more than he could help it, but allow the water to get a little closer to this thread. The detail of the drawing being made in this shape, with a 2-inch prolongation, perhaps, led the boilermaker to believe that that was to be screwed down to the sheet, and then what he did not need on this end to be cut off and the rest was to be riveted over. In putting in crown-sheets in ordinary locomotive boilers, we always leave the stud a little longer than necessary, so that the men may screw them through about the right amount to rivet over and then cut the other end off and rivet them over, also. I failed to find on this print, except in the rear view, which shows the stud carried down as Mr. Hartman indicated: there is the crown-sheet, which comes around in this way; and then there is an inner line showing the flange of the flue-sheet; or the back head, as you may please, and the stud has come on through both of those (*Fig. 12*) and that is clearly an error in the drawing-room, and frequently happens, to which the mechanic pays no attention, but takes the drawing to the drawing-room in a well-regulated establishment (if he is going to build a lot more of them) and has it corrected—and has it made that way. That deviation here is a matter for the boilermaker—the foreman of the boilermakers—to explain.

THE CHAIRMAN:—The subject is open for general discussion. Any gentlemen present, we would be glad to hear from them.

MR. GEO. B. HARTLEY:—I examined the boiler in the "Quaker City." It was similar in every respect to the boiler that exploded. I found the conditions that Mr. Hartman speaks of, and would say he rather under- than overstated them.

The head of every screw-stay within the fire-box had been caulked all around, leaving a well-defined groove, thereby weakening its hold.

It suggests itself to me, that after the stay was screwed to place, if enough material had been allowed to form an adequate head, after being riveted over by a light maul, that there would have been no leakage and no necessity for the objectionable after-caulking. The screw stays on the rounded part of the sheet did not get the full benefit of their threads—due to the sheet rounding away from them.

The stays inside of the boiler were formed by what may be called connecting links, some being fastened to that part screwed into the crown-sheet by pins while others were held in place by an ordinary bolt and nut. Many of these stays were loose and could be easily rattled; and while it is true that when the pressure was applied to the boilers these stays would take a strain, it is a question that before doing so the stays already tight would be subjected to too great a tension, possibly to the point of giving way.

MR. ROBT. D. KINNEY stated, that so far as he had noticed in the matter of the boiler explosion on board the boat "City of Trenton," no attention seemed to have been given to the possible listing of the boat to starboard and its effect upon the water in the boilers, especially if the water was at the time very shallow over the crown-sheets, adding, that much list might practically have based the crown-sheet sufficiently to have caused it to have become overheated, which seemed to be the primary cause of the explosion.

FIG. 14.

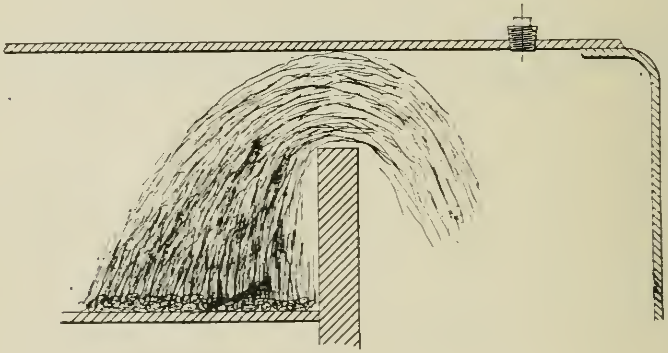

FIG. 13.

He remarked further that the crowding of the passengers to starboard through something attracting their attention generally to that side of the boat would be no unusual occurrence on an excursion boat loaded with passengers, as this boat was at that time of the disaster.

MR. VAUCLAIN:—I am the only one who can answer that. Here was the fire (*Fig. 13*); there was the bridge-wall, and I believe there was the fusible plug. The fire in being drawn from the flues was no doubt drawn in this direction, and secondly, deflected in that direction; so that this part of the sheet was much hotter than that part of the sheet (*Fig. 14*). I think any one who examined the exploded boiler could find very little, if any, distortion in this seam through overheating, which gave sufficient evidence to me to think that the water was not as low as was testified to by the Government inspectors, or 10 inches below the crown-sheet. The water had probably barely reached the crown-sheet, or at the level of the crown-sheet, and this heat would drive the water away; but the proof is that the metal in the soft plug was about half run out, that is, that the metal had got soft enough in the soft plug to push down and have very little of it left in the plug; and you will notice that this plug is screwed in from the inside of the crown-sheet, whereas in my experience we have always screwed them up from the under part of the crown-sheet and tried to locate them in the hottest place, so that the plug—the head of the plug would receive some of the flame and a projection passed up into the crown-sheet here—as soon as the water got below that the heat would be transmitted through this metal sufficiently fast to allow this to blow out before the crown-sheet was actually bare. This plug was probably about ½-inch to ¾-inch above the crown-sheet. From the appearance of the thread it was barely through the lower side or just about flush with the sheet; consequently, in a well-protected position, up in that position, a large portion of the metal had passed out by the overheating, and you will remember reading the evidence as given at the Coroner's jury: it was like pulling teeth to get any admission from the Government Boiler Inspectors as to the exact location of this plug, or as to which would be the hottest portion of the sheet with the fire and bridge-wall in this position. One of the inspectors even went so far as to say, after finding he could not escape giving the desired information, that he thought perhaps the flue-sheet itself had blown out first and carried the crown-sheet down with it; but this is why the soft plug was only half melted out, instead of wholly melted out. With the soft plug down that way there, or there, the metal in the soft plug would have probably been entirely gone; although I do not think if the metal in the soft plug had gone out entirely that this explosion would have been prevented. If these studs had had nuts on the under side like that [indicating *Fig. 15*] the time might have been prolonged a little; it might have taken two or three minutes longer before they would have had trouble; but the engineer testified to this (if I remember correctly—any one present has the privilege of correcting me) that he looked at his gages in the engine-room and felt that his water was all right; and that he went back in the engine-room and tried the water there and it was all right; but that he did not have as much water in that boiler as he had in the other boiler, and for that reason, and for that reason only, he immediately went to the feed-valve and opened the feed-valve wide on this boiler—wide open—and the feed-valve on the starboard boiler was only a half-turn open, which was $\frac{1}{8}$ full according to his evidence. It

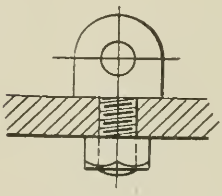

FIG. 15.

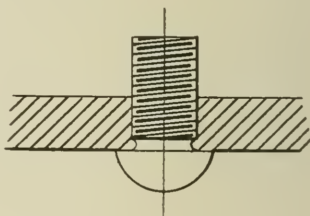

FIG. 16.

is an unusual thing for any engineer—locomotive or stationary—to open his feed-valve wide if there is only a variation in the boiler of about an inch between the water level as it was and as what he would like to have it. There certainly must have been in that man's mind a conviction that the water in that boiler was lower than it should be, and *much lower* than it should be, or he would not have immediately opened the feed-valve wide open.

MR. LEVAN:—How does Mr. Vauclain's idea accord with the Franklin Institute test, that at 550° the metal is as strong as at 32°? Why should not the fusible plug have melted when it got to 701° when the pressure was 55,000 pounds to the square inch according to those measurements? That fusible plug was in a position to receive an intense heat: how could it be otherwise? The flame had to pass right about it. If that sheet had been hot the plug would have melted, and not before. I can't conceive how Mr. Vauclain can assert that the sheet was hot when the fusible plug didn't melt.

MR. VAUCLAIN:—Mr. Chairman, I think that I just finished explaining, that from my point of view the fusible plug was not in the hottest place in the furnace, and that the sheet immediately over the bridge-wall was much hotter than the sheet was at this point, or at the point removed a slight distance from it.

The fact that the soft plug was not over the bridge-wall is conclusive that it was not in the hottest place; and any distance to which the soft plug was moved—placed from this bridge-wall—would remove it from the hottest place on that sheet. I have not the figures nor drawings to show exactly where that plug was; but the plug was approximately some 6 or 8 inches from the flue-sheet—so testified to, I believe, to the best of their judgment.

Now, a bridge-wall in a locomotive boiler is not an uncommon thing. There are many locomotive boilers built with bridge-walls. This boiler had forced blast.* What has a locomotive? The most violent forced blast that can be had. I have seen as high as 10—at one time 16—inches of water on a locomotive fire-box—locomotive fire. The heat is very intense; and quite frequently the crown-bolts, and especially where we use a headed crown-bolt with a button head screwed in, and those in turn are made like this [indicating *Fig. 16*], there is a groove underneath the head. Now the crown-sheet is $\frac{3}{8}$-inch thick and we cut two or three threads, and we get these two or three threads on the upper side, where in this case they are on the lower side of the crown-sheet; and the lower side—one-

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* The following statement is added on the authority of Mr. Le Van.—[ED.]

The statement made, that forced draft was used on this boiler, upon investigation, proves not to be the fact.

Consequently, the heat being concentrated at the point *O X* in *Figs. 1* and *20* as stated is wrong, as there was no means by which forced draft could be produced. The heat was consequently equalized all over the under side of the crown-sheet.

third of the sheet—there is no hold in this sheet at all. Of course, we have the head as a safeguard, but nuts are used by some people. There are various ideas on this subject as to how to hold a crown-sheet up when it commences to get hot; and all locomotive crown-sheets don't go down when they do get hot, because they are caught in time; and when a sheet is treated in that manner by a careless engineer or fireman we call it a scorched crown—simply scorched: if it had been a minute or two longer it would have been a *hot* crown and it would have been down. It is no uncommon thing to have a crown-sheet hot enough for the sheet to stretch and let the bolt head pass up through it.

Referring back to the soft plug, I believe I said that the metal in the soft plug was half melted out; and as the Government required pure tin in this plug (which would melt somewhere near 450°) the inspectors testified that they did not put tin in this soft plug because it would not stand well enough—it would melt out too quick; and another alloy was used; and further, I believe, that lead was too soft. It takes a very great deal more to melt lead than it does tin; but the composition that was put in was supposed to melt, I believe, at 438°; where, by analysis of the metal that was in that soft plug we found that it didn't become limpid until it was between 500° and 600°, and did not melt, I believe, until about 650°.

[*To be concluded.*]

ELECTROLYTIC COPPER TUBES.

London *Nature* informs us that the Dumoulin process for the electro-deposition of copper in the form of tubes does not appear to have been very successful at Widnes, where a works for operation of this process was built in 1896–1897. According to the fifth annual report of the Electrical Copper Company it has been decided to close the works permanently and to sell the plant, since at no period of its operation has a profit been earned. The Dumoulin process depends upon the electro-deposition of copper upon revolving mandrils, specially treated strips of skin being used to supply the friction necessary for obtaining smooth and dense deposits. The process differs from the well-known Elmore process chiefly in this substitution of skin for agate burnishers; and it is noteworthy that in neither case has the financial success realized the early expectations of the promoters of the companies operating these processes.

THE FRANKLIN INSTITUTE.

Proceedings of the Annual Meeting, held Wednesday, January 15, 1902.

THE RENOLD SILENT CHAIN GEAR.

By J. O. Nixon.

The Renold Silent Chain Gear is one which may be run at high speeds, with no noise, for the transmission of any amount of power.

With this brief definition as a preface, we will proceed to a more detailed consideration.

Chain gearing, as a means of transmitting power between parallel shafts (with a positive velocity ratio), is one of those fundamental mechanical devices which each of us unconsciously reinvents at some time or another. It is theoretically obviously the correct method because of the following reasons:

(1) It provides a positive velocity ratio.

(2) It may be used on long or short centers.

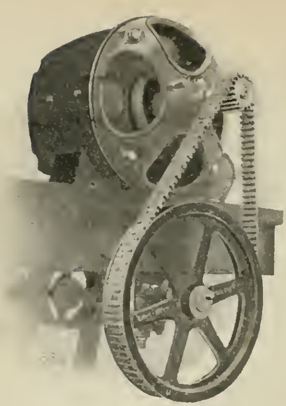
(3) There is no loss from journal friction due to tension in the slack side of the chain, because such tension does not exist.

(4) It is not affected by heat or dampness.

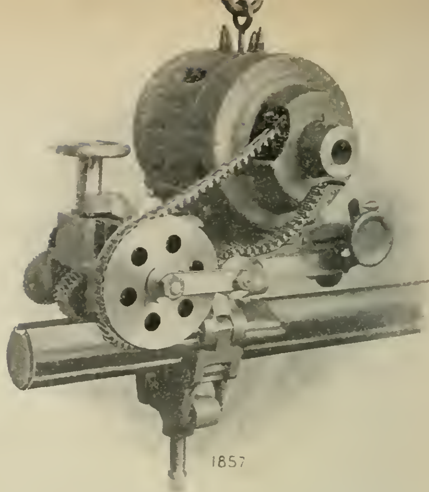
While these advantages have long been recognized, it has still happened that in the majority of cases it has not been possible to employ chain gearing because of the large amount of noise connected with its use, and because of the low speeds at which it was absolutely necessary to run it.

It is by no means to be inferred, however, that only a small amount of drive chain is in use to-day. The amount made and sold every year aggregates many million feet, so that the actual number of chain drives is very large, but the proportion that they bear to the total number of power transmissions is small.

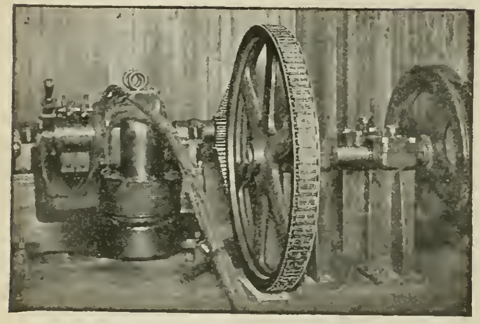
As stated above, the reasons for this limitation are the noise and the speed limits.



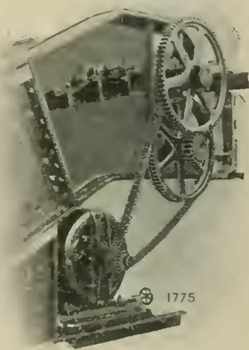
5-horsepower motor drive, 1,200 revolutions per minute. Drives counter-shaft.



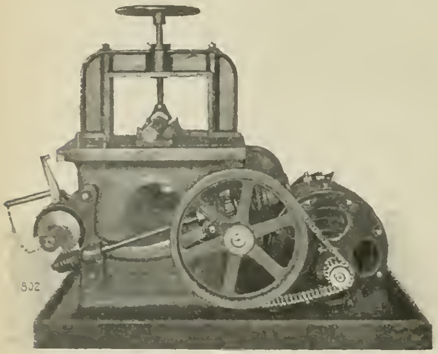
Motor-driven portable keyseater



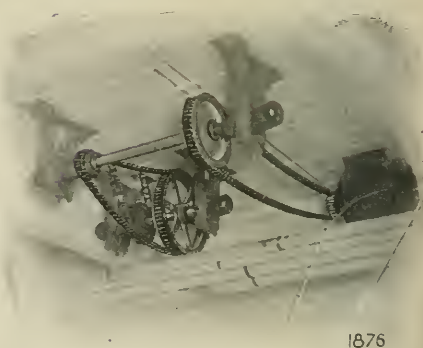
7½-horsepower motor drive. 875 revolutions per minute. Drives merry-go-round.



9 horsepower motor drive, 870 revolutions per minute. Drives stair lift.



3-horsepower motor drive, 1,100 revolutions per minute. Drives Higley cold saw.



1-horsepower motor drive, 690 revolutions per minute. Drives packing table.

RENOLD SILENT CHAIN GEAR, SHOWING VARIETY OF SERVICE.

We will now discuss the action of the ordinary chain gear and show the inherent causes of the above defects.

Fig. 1 will possibly make this clearer. The figure shows a driven sprocket wheel and a chain of the ordinary type. This chain, it is assumed, was made to fit the sprockets. However, as soon as the gear was started up, the pitch lengthened so that this is no longer the case. This length-

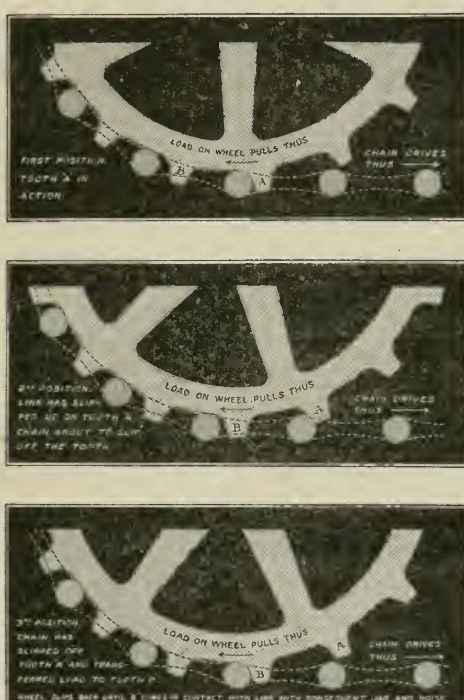


FIG. 1.—The action of an ordinary chain.

ening of the pitch, or stretch, is due to the following causes: The pins bed in their bearings, the stress on the chain stretches the material, which is, of course, elastic, and wear of the pins and of their bearings begins at once, and is a constantly increasing factor. Add to this the decrease in root diameter of the sprocket due to wear, and we have the conditions shown in the figure of a wheel running with a chain that is too big for it. This means that one tooth alone is doing all the work at any given time.

In the first section of *Fig. 1* we have tooth *A* in action; in the second section the wheel has revolved and the chain is about to slip off tooth *A*, and in the third section the chain has slipped off, and the wheel has slipped back under the influence of the load, until tooth *B* comes in contact with the chain. This slipping back of the wheel makes a noise and causes a shock to both chain and wheel. These shocks occur every time a link passes out of mesh and, therefore, at even very moderate speeds, the number per minute is very large. It has been proved by experiment and by practice that this jarring action is very wasteful of power, and that the amount of power consumed by it increases more rapidly than the speed increases, so that the allowable useful working stress becomes smaller and smaller with increasing speed. This limits the speed at which a chain may be run. Of course, this limiting speed varies greatly for various styles of chain, and is much higher for a steel roller chain of proper design than for a malleable chain. What has been said above with reference to a driven sprocket applies with equal force to the driver.

From the foregoing it will immediately be inferred that the solution of the problem of producing a high speed and a silent chain gear lies in the production of a wheel and chain which shall always remain a perfect fit each with the other entirely independent of the stretch of the chain. Such a chain gear has been developed by Mr. Hans Renold, of Manchester, England, and has been in wide and successful use in Europe for some five years past. This chain gear consists of a chain composed of links of a peculiar form stamped from the sheet or cut from a drawn bar fastened together by shouldered rivets into a chain of any desired width, running over cut sprocket wheels with teeth of a shape varying with the size of the wheel. It is absolutely silent and may be run at high speeds. It is capable of transmitting any amount of power, from the smallest to the greatest.

How the Renold chain gear accomplishes its results may be best seen by reference to *Fig 2.* It will be noted at once that the chain has contact with the wheel on the faces of the teeth only, and not on the root circle at any time. The flat bearing surfaces of chain and wheel at corresponding angles cause the chain to take the form of a perfect circle at all times, with a pitch diameter corresponding to the pitch of the chain, and not to the pitch of the wheel, as is the case where the bearing is on the root circle. Because

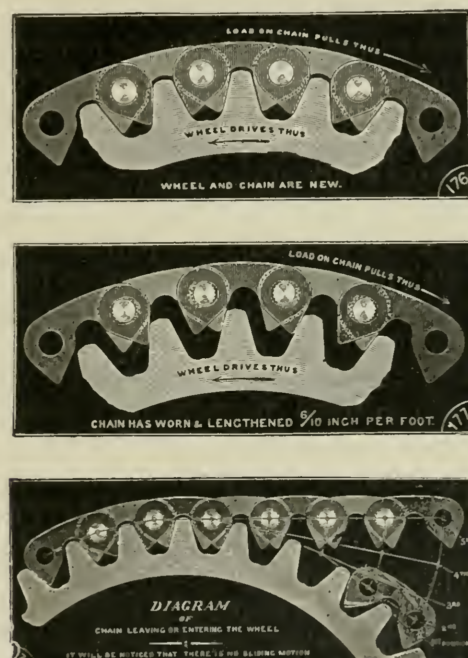


Fig. 2.—The action of the silent chain.

of the above, every tooth in mesh is in equal contact with the chain, and remains so, whatever the stretch. As any given tooth goes out of contact with the chain there is no slipping back of the chain, for the next, and every other tooth in mesh, is in perfect working contact with it. Thus there is no noise connected with the operation of the chain, and the cause which limits the speed of the ordinary chain gear does not exist. The first section of *Fig. 2* shows a new chain on the sprocket; the second section shows the same chain after having stretched, and illustrates how the chain automatically adjusts itself to the sprocket, remaining always a perfect fit for it; the third section shows the rolling action of the chain as it comes into mesh. There is seen to be no sliding of the chain on the sprocket tooth, which means, of course, minimum of wear and a maximum of efficiency. The Renold Silent Chain Gear, as Mr. Renold has named this development, is therefore noiseless; it can be run at high speeds; and it retains the originally perfect action until worn out. Another valuable property, which is a corollary of the self-adjusting feature of the chain, is the possibility of running two or more chains side by side on the same wheels. In this way, when large powers are to be transmitted and the width of the chain necessarily becomes too great for convenience in manufacture or in handling, several chains may be used with the perfect assurance that each will bear its proper share of the load. This is in great contrast to the known impossibility of getting two ordinary chains to stretch evenly and so distribute the load between them. As the number of chains becomes greater the difficulty by the old method is more than proportionately increased.

The life of a chain is the length of time which it will take to stretch it so much that it ceases to have any bearing whatever on the teeth of the sprocket wheels. A very small bearing will suffice, because the load is divided between all the teeth in mesh. Hence, to prolong the life of the chain, the steel used in its manufacture must be of the very highest grade obtainable, and it must be worked with the utmost accuracy. A steel for the links of high tensile strength allows the use of pins or rivets of large diameter while preserving the tensile strength of the chain as a whole. The large bearing surface so obtained is rendered yet more valuable by the use of hardened pins of high-grade material. The washers on the ends of the rivets claim no little attention. They must be small in diameter and not too thick. This necessitates a steel of high tensile strength and elasticity, so that the washer shall grip the rivet when it is forced over it and still, by its small size, not add to the bulk nor detract from the appearance of the chain.

From the question of material one naturally passes to the allowable limits of error in workmanship. In a general way it may be said that the chain should be as accurate as it is possible to make it. The pitch must be accurate, and the rivets must be neither so short as to bind the links nor so long as to give excessive play.

The point to be especially observed and emphasized in the above is the fact that the chain remains a perfect fit for the wheel independent of the stretch. This means that an old chain works as well as a new one.

It also means that the engineer can now adopt chain gearing wherever it is indicated or desirable, and is not forced to adopt a substitute or a makeshift because of objectionable noise or because the desired speed is too light for chains.

Before giving a history of the development of the chain it will perhaps be well to indicate roughly the principal fields into which the Silent Chain has found its way.

First and most important comes motor driving. It is no exaggeration to say that the Silent Chain has made the use of motors possible in very many cases, and in countless other cases it has made their use much more convenient.

This is because it provides a flexible means of connecting the power and the work, and one which is silent, positive and efficient. It may be run at high speeds, often permitting the use of a light high-speed motor instead of the more expensive and heavier low-speed machine of the same power.

It makes less noise than spur gearing, does not require short centers, nor need the centers be absolutely fixed.

It may, on the other hand, be used on shorter centers than are possible with belting; and a wasteful and troublesome idler does not have to be provided for. Above all, the speed ratio is positive, as slip is impossible.

Related to motor driving is dynamo driving. It often happens that while space for an engine and generator is limited, the appropriation is also too limited to permit the installation of a direct connected set. In this case it is possible to install a chain-driven dynamo in but little more space than that occupied by the direct connected set, and at much less cost.

Governor Drives.—The importance of a positively driven governor cannot be overestimated. The difficulty of suddenly accelerating heavy governor balls is emphasized by the wide belts in use on well-known engines. By the use of chain, an absolutely positive, non-slipping drive is obtained and one that is noiseless.

Machine Tools.—The Silent Chain has found large employment, both for use within the tools themselves and for driving them. A little later I will present some slides showing both applications.

A positive drive for feeds is most important, if we wish to get the maximum of work out of the tool. This is being recognized more and more every day, as is evidenced by the advertising colums of the trade papers.

The Silent Chain has been largely used for this purpose both in this country and abroad.

For driving the spindles of milling machines, etc., especially where the feeds are driven independently, the importance of a positive drive cannot be overestimated.

If the cutter is belt-driven, when it strikes a hard spot in the metal, it slows down or stops (because the belt slips), but the feed goes right on with disastrous results to cutter and work. This is not theory but a sad fact, with which many of us are acquainted.

Hardly a machine shop is being put up nowadays that is not electrically driven, and many old shops are being altered from belt-driven to motor-driven.

Here the convenience of the Silent Chain is most manifest, as it permits the easy attachments of motors to tools where their use would be inconvenient if not impossible without it.

Automobiles.—The features of the Silent Chain which particularly recommend it for automobile work are its efficiency and its automatic compensation for stretch.

It one case, there was a gain in effective power on the rear axle of over ¼-horsepower, due to the substitution of roller chain by Silent Chain, the motor being a 7-horsepower gasoline engine.

The automatic compensation for wear also prevents the chain "riding the sprockets." Silent chain is now in use on hundreds of motor-cars of all types and sizes—both in England and on the continent.

The Silent Chain was by no means a happy thought, but —like almost all other important inventions—is the result of a great deal of hard work and hard thinking by men who have made chain-gearing a life-study.

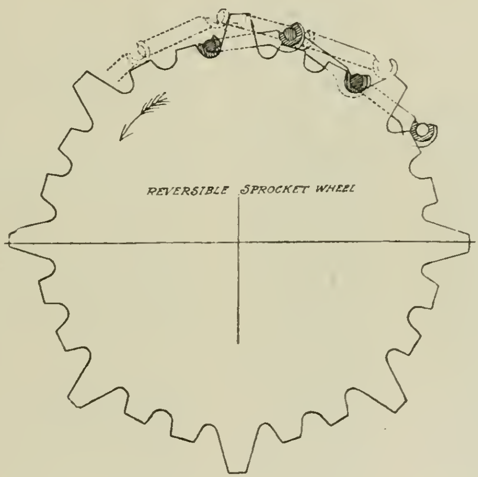


Fig. 3.

As one interesting point in this development, *Fig. 3* illustrates the construction of a sprocket designed some years ago for use with Ewart Link-Belt by Mr. James Mapes Dodge.

The chain on this sprocket compensates for the elongation in the same manner that the Silent Chain does by taking a position farther out on the wheel teeth. This is but another illustration of the often noted circumstance of two men working entirely independently, arriving at the same result at about the same time. It is also interesting to note that this link-belt star sprocket is also a practical success.

The Silent Chain Gear was then the result of a thorough appreciation of the problem to be solved. It was preceded by what Mr. Renold called his "High-speed Block Chain," which was a chain made up of blocks and side-bars with the blocks of peculiar shape. The idea was to get a block shape and a tooth shape such that the block would roll to its seat instead of sliding on the tooth. This was accomplished in great measure, but the difficulties due to chain elongation and wheel wear were, of course, unprovided for.

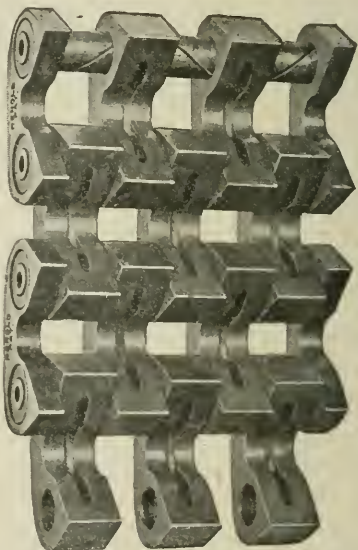


FIG. 4. FIG. 5.—"Block" type. FIG. 6.—"Multiple Block" type.

Last came the Silent Chain. In that form shown on the patent drawings it was impracticable for large powers, and required a complicated sprocket with staggered teeth. This form, however, was rapidly left behind and the present shape of link evolved, which may be used for all powers and which does not require a complicated sprocket.

The chain was originally made from links stamped from the sheet as shown in *Fig. 4*. The rivets were shouldered and the chains were made in the same pitches and widths as now.

The steel was comparatively soft, and it was necessary to keep the rivet diameter small to preserve the tensile strength of the chain, with a consequent limitation in bearing area on the pin. One obvious remedy was to harden the link. This, however, was not commercially possible, because the burrs left by the punching, which knocked down and did no harm ordinarily, became knife-edges, which cut the sprockets directly the links were hardened. This made it necessary to grind the working faces of the links, which was too expensive an operation.

After several attempts Mr. Renold finally succeeded in getting a bar drawn of the desired cross-section and commenced the manufacture of the Block Silent Chain (*Fig. 5*) and the multiple block (*Fig. 6*). These had hardened blocks, larger rivets and oil grooves.

This type was the highest development up to a short time ago, when it was found possible to further improve the stamped silent chain by the use of harder link stock of higher tensile strength. This permitted the use of rivets larger than those in the block chain, and by a process for smoothing the working surfaces of the links the stamped type again comes to the front (*Fig. 7.*)

Steel fully as good, if not superior to any Mr. Renold has obtained in Europe, is obtainable in this country, so that the chains now made here, of which samples are shown, are of as high a grade as possible in every way.

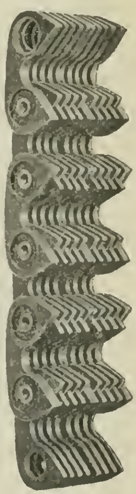


FIG. 7. New flush-side stamped silent.

The sprockets used with the Silent Chain form, of course, an indispensable part of the gear. They must be accurately cut with special cutters and may be of any material. The teeth have straight sides to give a full bearing with the working surfaces of the chain. The angle of the tooth is different for every diameter of sprocket; or, to put it in another way, the angle between the sides of the tooth becomes greater as the number of teeth increases. The limits to the number of teeth that may be employed are practically fixed at 18 and 120. The former limit is set by the fact that in a wheel with this number of teeth the sides of the teeth are parallel. Conversely, when a wheel has 120 teeth, the tooth becomes so blunted as to make slipping a possibility, so that this number should be exceeded only where the load is absolutely uniform.

The fact that the load on the sprocket teeth is distributed over all the teeth in mesh, obviates the necessity of using a metal of high-tensile strength for the sprocket wheels. The fact that there is no sliding of the chain on

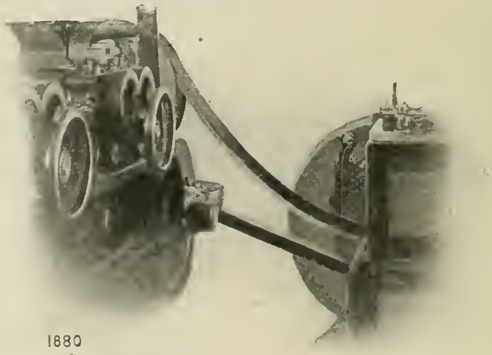


FIG. 8.—40-horsepower motor drive, 720 revolutions per minute. Drives shredding machine. Chain shown in motion. Note smooth action.

the sprocket teeth obviates the necessity of using a very hard metal to minimize wear. It is therefore possible to make a strong and durable sprocket wheel of cast-iron. Steel, however, has been used for the small wheel on automobiles, and in other cases where the service was particularly severe. The flanges are put on after the teeth are cut, and are either shrunk on, or riveted to the wheel.

With regard to the practical use of the chain the following points may prove of interest: It is obviously necessary that one wheel of the pair be flanged to prevent the chain running off; it has been found that better action is obtained where the driven wheel is flanged; the chains may be run with the sprockets so close together as to barely clear, or the drive may be of any length up to ten to fourteen feet without supporting idlers and, if such support be used, may be of any length found economical and desirable.

The only factor so far found which serves to limit the speed is the difficulty of keeping the lubricant on the chain at very high speeds. At speeds exceeding 1,300 feet per minute, the oil is thrown off by centrifugal force, but speeds as high as 2,300 feet have been employed successfully by enclosing chain and wheels and running them in oil. In this connection, however, it may be well to

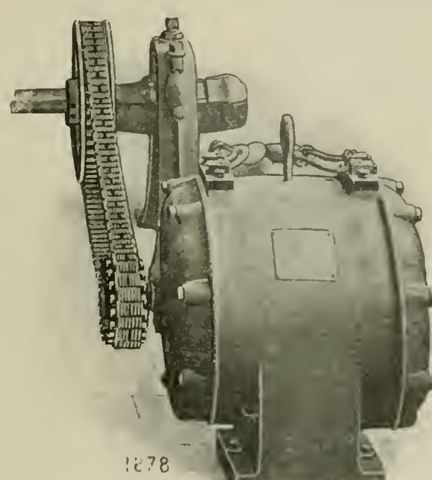


FIG. 9.—1-horsepower motor drive. 690 revolutions per minute. Drives tumbler dumper.

call attention to the fact that the chain speeds being lower than the speeds necessary for belting, allow sprockets of correspondingly smaller diameters for the same angular velocities. The chain thus effects a marked economy of space, not only in the diameter of the wheels, but because of the comparatively long centers absolutely essential with belting. The line of centers may be horizontal, inclined or vertical, provided that the shafts are parallel; but there are two limitations on vertical drives. The small wheel should not be the upper one, because the weight of the chain crowds it into the sprocket and gives bad action. Some form of tightening device should be provided, either by adjusting the centers or by an idle roller on the slack side of the chain, so as to prevent the chain, when it stretches, falling away from the lower sprocket. Both of these troubles may be obviated by inclining the line of centers.

Wherever the chain gear is exposed to dust or grit of any sort, it should be enclosed in a dust-tight casing. In any case, a light metal guard should be provided to prevent anything falling into the gear.

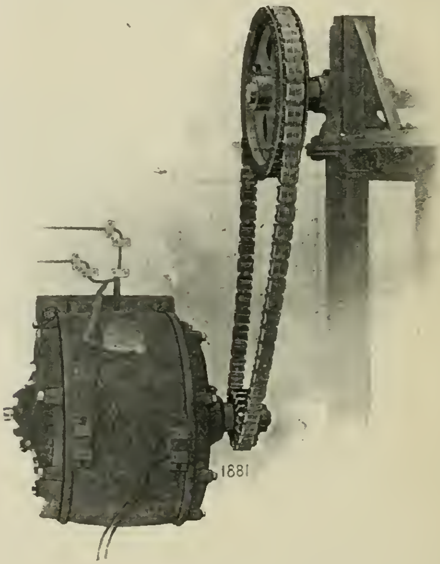


FIG. 10.—7½-horsepower motor drive, 710 revolutions per minute. Drive curing machines.

To sum up.

The Renold Silent Chain Gear possesses, in common with all chain gears, these advantages:

(1) A positive speed ratio (no slip).

(2) No tension in the slack side of the chain and, therefore, a minimized loss in journal friction.

(3) Adaptability to short centers or to long centers.

(4) Adaptability to hot or damp situations.

In addition to these it possesses the following unique advantages:

(1) It is silent.

(2) It may be run at high speeds.

(3) The initially perfect action is preserved throughout the life of the chain.

(4) The load is distributed over all the teeth in mesh.

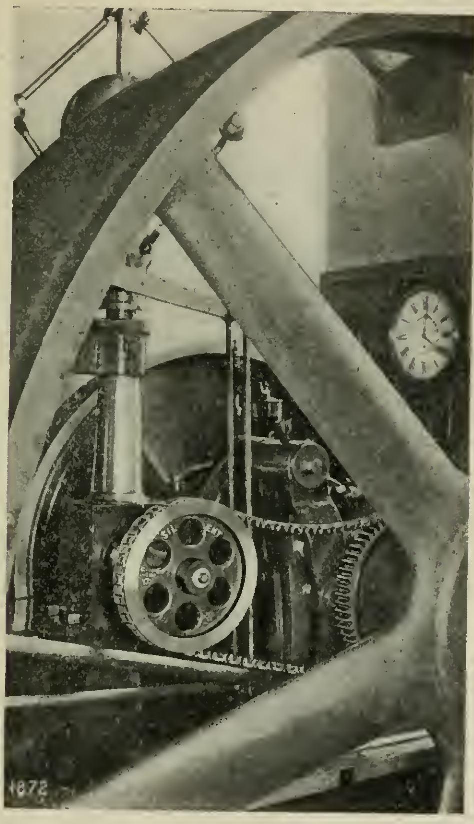


FIG. 11.—Governor drive on 14½ x 24 inches Porter-Allen engine.

It is superior to leather or rubber belts because:

(1) It provides a positive speed ratio.

(2) There is a minimum loss in journal friction.

(3) It can be used in hot or damp situations.

(4) It can be used on short centers without a troublesome and wasteful idler.

It is superior to spur gearing because

(1) It is noiseless.

(2) It does not require fixed centers.

(3) It does not require short centers.

(4) There is no sliding friction on the teeth.

(5) It is smoother in action and generally more durable.

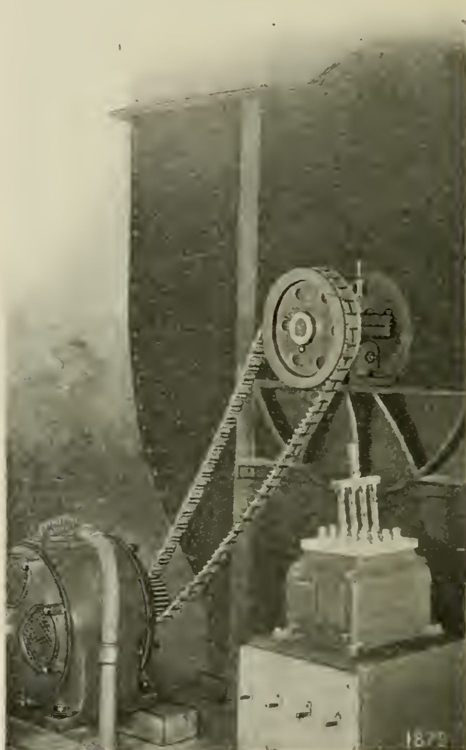


FIG. 12.—10-horsepower motor drive, 710 revolutions per minute. Drives Sturtevant fan.

We will now take up the illustrations of the practical application of the chain.

While the Silent Chain was invented and developed in England, it may fairly be said to be making rapid progress in this country since its comparatively recent introduction.

The largest single installation is that in the new factory of the Natural Food Company, makers of Shredded Wheat Biscuit, Niagara Falls, N. Y. Here there are nearly forty drives, which vary in size from 1 to 40-horsepowers.

Fig. 8 shows a 40-horsepower drive, motor runs 720 revolutions per minute, and the chain about 1,100 feet per minute. This drives a shredding machine and is one of four similar drives.

The conditions throughout these works are severe because induction motors are employed which start suddenly. No clutches are used, and the sudden stress would be disastrous to spur gearing where the load is concentrated on one tooth. These conditions are easily met by the Silent Chain, however, because the load is distributed between all the teeth in mesh.

As a contrast, *Fig. 9* shows one of two 1-horsepower drives for tumbler dumpers. The absence of special base plates necessary for spur gearing is noticeable.

Fig. 10 illustrates the convenience and adaptability of the chain. The camera was pointed upward, the motor being on the ceiling. It is one of the 14 drives for the curing machines. Motor 7½-horsepowers, 710 revolutions per minute.

The remaining cuts illustrate very well all the various points brought out in the body of the paper.

They are interesting as showing the variety of size and service to which the chain is adapted.

POLLUTION OF STREAMS BY MINING COMPANIES.

The following editorial comments are taken from the *Engineering and Mining Journal* of recent date:

A decision of considerable importance to the coal mining companies of Pennsylvania, especially in the anthracite region, has just been rendered by the Supreme Court of that State. The case has been before the courts for several years, originating in Schuylkill County where the Court of Common Pleas decided against the mining companies, and two appeals were taken, which finally brought the case to the court of last resort.

The suit was brought in the first place against the Lehigh Coal and Navigation Company to recover damages for polluting Panther Creek by flooding the stream with coal dirt and refuse from the mines. The plaintiff in the case owned a grist mill on the stream which he claimed had been injured by the dirt and silt carried into his dam and race, and the lower court awarded $4,900 to cover the injury done. The amount at issue in this case was not particularly large, but as it is a test case which will cover a number of other suits which have been brought or are waiting for this decision, its importance to the companies is very much greater than the number of dollars absolutely at issue.

From an early date it was the practice of the collieries in the anthracite region to run the mine water, carrying with it a very large amount of coal dust and waste, into the nearest convenient stream. Even when the extent of mining business was comparatively small there was more or less controversy over this, and when the collieries became more numerous several of the companies built retaining dams and other works at considerable expense in order to provide places where the coal and dust tailings could be dumped. This was not universally done, however, and much of the refuse continued to find its way into the streams until the waters of the Lehigh, Nescopec, Lackawanna and other rivers throughout the region became thoroughly polluted.

Of late years there has been some improvement in this respect, which was rendered absolutely necessary by the growth of the towns and cities through the anthracite country and the difficulty of obtaining a proper supply of water for them as long as the pollution of the streams continued. In some cases, however, the expenses of the necessary work were paid for by the cities and towns and not by the companies, while at some collieries refuse was dumped on the banks of the streams only to be carried away by the spring floods. The condition, in short, has been unsatisfactory, and the right of the people to the use of the water unpolluted by refuse has not by any means been recognized by the companies.

In this decision the language of the Supreme Court is very clear, and not only supports the decision of the Court of Common Pleas but even goes considerably beyond it. Thus the opinion of the court below held that the injury to the plaintiffs was not permanent and that the dam and race could be cleared of coal dirt at a cost very much less than the water power; but while this point was correct, the very nature of the operation showed that, as it had been conducted, there was practically a continuous interference with the proper use of the water by means of the continued loading of the stream with dirt. It was correct, therefore, not only to assess the damages actually sustained by the plaintiffs, but also to restrain the defendant company by an injunction from polluting the waters of the creek in the manner described in the complaint.

In this case, as in other similar actions, the injunction of the court is the appropriate resort for the prevention of trespasses and nuisances which are persisted in and which threaten to become permanent in their nature. Ample authority may be found for this both in the Common Law and in the statutes of Pennsylvania, and the Supreme Court therefore sustained and confirmed the perpetual injunction against the Lehigh Coal and Navigation Company granted by the lower court. It becomes apparent that if this case is followed up by other suits—as it will be in all probability—during the next few years some of the anthracite companies will be put to considerable expense in providing retaining dams and dumping grounds. In some cases there may be serious difficulties involved in the construction of such works, but that they will be required in the future there is no doubt.

Section of Photography and Microscopy.

Annual Meeting held Thursday, January 2, 1902.

PHOTOGRAPHIC PERMANENCE AND THE AMATEUR PHOTOGRAPHIC EXCHANGE CLUB—1860–64.

BY PROF. CHARLES F. HIMES, PH.D., LL.D.,
Honorary Member of the Institute.

(Being the Address of the retiring President.)

With the very first experiment in photography the question as to permanence of results arose. The process of Wedgewood and Davy failed in practical results, mainly because the pictures were permanent only when examined by artificial light, and even then deteriorated. The question, "How long will it keep?" has been running all through the literature of photography to the present time, and to-day, owing to the wide range in the applications of photography, it is continually recurring in regard to the preparations used, but more especially, in view of the growing application for record purposes, in regard to the final product, the finished print. The question as to the keeping qualities of any of the preparations used in any of the stages of the photographic practice toward the finished print is a very secondary one, except in so far as it may affect the permanence of the print. Whether developers will keep, matters, upon the whole, very little. Their very efficiency depends, in fact, upon their chemical instability. It is only a question of a little more or less trouble, and perhaps a little more expense. So, in regard to fixing solutions, where instability may have graver consequences to the finished print, there need be no room for question, because they can be placed above suspicion by fresh preparation. But how long negative plates or films will keep in good condition before exposure in the camera, and under what conditions, is not simply a question of convenience or expense but of applicability of photography where considerable time must necessarily intervene between their preparation and use, as upon scientific and other expeditions. Closely related to this is the other question, as to the length of time allowable after exposure before development. These questions are, however, of importance, practically only between comparatively narrow limits. An answer to the question as to the years that dry plates will keep in good photographic condition, or whether they will keep indefinitely, has, however, some interest as affording an *a fortiori* argument for belief in and demand for plates that may keep in good condition for any particular purpose. But it is when we come to the print, as has been said, the question assumes its highest importance, a dominant character. All consideration of the photomechanical print may be omitted in this connection. It has a utility and permanence all its own, but, as it is necessarily not strictly photographic, but one remove from the photograph, if it lacks some of the record qualities, especially of minute microscopic rendering of details, it fails, it might be said, in original entry quality of the photograph.

It is not proposed this evening to traverse this question of photographic permanence exhaustively, or to discuss it by means of facts independently and scientifically established, but rather as a question to be determined empirically by means of data of the highest character for authenticity, furnished by one of the earliest photographic organizations —the Amateur Photographic Exchange Club, from 1860 to 1864, composed of men of careful habits of systematic work and observation, many of high scientific attainments.

As preliminary, it may be well to fix definitely what is meant by time-effect in this connection. Time is recognized as so necessary a factor, that it has almost come to be regarded as capable *per se* of producing changes, or that some things will change, perhaps all things deteriorate, simply with lapse of time. Now a time effect *per se* is inconceivable, and we are justified in any case of, what for convenience may be called photographic change, in looking for some cause as fully as we are justified in mechanics in assuming a cause where a body changes its state of rest, or of uniform motion in a straight line. These causes of change in sensitive films, and other photographic preparations, may be internal or external, or a combination of both. They may be minute, feeble, operating slowly but with cumulative effect through a very long time. There may be in some cases molecular movements in a sensitive film that will gradually change its photographic character. There are many analogies, physical and chemical, to suggest it. In a supersaturated solution of a salt, crystallization may take place rapidly, but gradually enough to be watched, and just as such crystallization can be started by an external mechanical impulse, so, it is conceivable, external causes, changes of temperature, vibrations of all kinds, even jarring of floors and buildings, may assist, even if they do not originate, molecular changes that may affect the whole photographic character of a film.

Perhaps a more analogous case is that of the slow crystallization of axles, wheels, bolts, cannon, and so forth by jarring. We are familiar, too, with what is called the "continuating action" of light, and it is a question whether sufficient allowance has been made for this fact in accounting for deterioration of films, and papers exposed even to a very feeble light during preparation in drying. Experiments made upon papers, though not as decisive as could be wished, seemed to substantiate this view. Again, well-defined chemical reactions, as experience in the laboratory shows, require time, and in many cases prolonged time, for perceptible result, and such may be taking place slowly in a sensitive film, and eventually affect decidedly its character. When, further, all that is covered by the term atmospheric conditions, normal and abnormal, is taken into consideration we have a legion of possible agents operating to change photographic preparations and products. Now, the fact to be recognized is, that all these conditions are matter of observation and investigation, and in many cases may be eliminated or neutralized, as the great advance in certainty and keeping qualities of plates and papers in the past few years shows.

In investigation of such conditions a natural tendency to give, perhaps, undue weight to established chemical reactions may cause minute subtle conditions to be overlooked, accompanied by a conscious or unconscious assumption that highly sensitive photographic compounds are in their very nature sensitive to other physical and chemical agencies than light; that they are in short in their nature wanting in permanence, in spite of the fact that silver chloride, bromide and iodide are amongst the most stable chemical compounds, so far as other agencies than light are concerned. An over-caution resulting from this want of faith in photographic preparations often manifests itself in *à priori* conclusions, and positive, scarcely tested statements, and a time-limit has been fixed entirely unwarranted, and once fixed is apt to maintain its place. As a little illustration, it was stated by some one that blue-print sensitizing solution would not keep; it became permanently incorporated in photographic literature, and there it remains in spite of published statements, again and again, that it will keep unimpaired for years, and many may be deterred from its use by this positive charge of instability. Time has in very many cases, then, doubtless, been made the scapegoat for many avoidable causes of photographic deterioration, for imperfect knowledge or oversight of conditions, for unscientific or careless work, for improper treatment, and storage, etc. The survival of one specimen in excellent condition is sufficient to establish permanence of any method, and direct investigation to the causes of deterioration with certainty of success in discovering and combating.

Considerations such as these suggested that the objective presentation this evening simply of some well-authenticated facts in this connection might not be without general interest, and at the same time recall some of the pioneer workers of your city. As to the keeping qualities of gelatinc-bromide plates, it may be well to recall a partial report made at a previous meeting of this section. Reports upon this point have been exclusively made upon plates that have remained unused by accident, and not upon those carefully tested and deliberately placed aside for this purpose. One or two failures with plates kept longer than usual, ascribed to their age, served to fix a time-limit beyond which others were not permitted to go before use, and unused ones would not be likely to receive much care. Unopened dozen boxes out of gross boxes of plates, by Cramer, Inglis, and Eastman, in my possession since 1884, were furnished to this Section and to the Philadelphia Photographic Society to be tested. Plates from the same lot had been tested at different intervals since that date by myself and others. The results were generally as satisfactory as with the plates originally. The plates at that time had not the rapidity of plates subsequently prepared, and it is not possible to say positively whether there was any loss of sensitiveness with age, but there did not seem to be.

According to report made by Mr. M. I. Wilbert to this Section, with the accompanying negatives upon the Cramer plates exhibited here, they seem to have been in as good condition as when first received fourteen years before. By a singular coincidence, illustrative of what has been stated, whilst these old plates were establishing the permanent excellence of the Cramer plates, that maker was advertising the innovation of affixing to packages of all plates sold the time-limit within which they would be guaranteed, and there was expectation on the part of some that this lead would be followed by other makers. But the advertisement has disappeared. An interesting report upon plates submitted to the Philadelphia Photographic Society by its technical committee is published in its proceedings,* with full discussion, including the effect of mode of packing the plates. Whilst the lot of plates at their disposal seemed to manifest a a greater tendency to fog, this may have been due in part to shorter exposure and energetic development, as the plates, although the most rapid of that date, were not as sensitive as those of to-day. These plates at the time they were submitted were, with reason, believed to be the oldest authenticated non-exposed plates, and are, probably, of gelatino-bromide plates; but in the course of the evening, in connection with other matters pertaining to the club

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* *Journal of Photographic Society of Philadelphia*, Vol. V, No. 6.

already alluded to, prints will be laid before you from negatives made upon plates so much older before exposure that the importance of the preceding ones will be reduced.

A word in regard to the origin and character of the club will serve to show the basis of reliability for data furnished by it. Its history, carefully written by Coleman Sellers, now Doctor, was published several years ago,* and it will not be necessary, therefore, to go into details as fully as might be otherwise desirable. It originated in a desire on the part of some amateurs to obtain copies of a peculiarly good stereograph of a "cattle picture," taken by Robert Shriver, of Cumberland, Md., without actually begging the pictures. They proposed, therefore, to exchange pictures of their own for them. At the suggestion of H. T. Anthony, not then a member of the firm, but an enthusiastic amateur, a club was formed, limited to twenty, without formal constitution or regularly elected officers, or form of election of members, all matters being regulated by correspondence and common consent, with most perfect harmony. Among the rules were the following:

"(1) None but amateurs in the art shall be recognized as members, and the number shall not exceed twenty.

"(2) No member shall forward for exchange any work not his own.

"(3) Every member shall forward each other member on or before the 15th of January, March, May, July, September and November, at least one stereoscopic print, a copy of which has not been sent before, or its equivalent mounted and finished.

"(4) Should any members desire to exchange with any others oftener than bi-monthly, they can do so by agreement.

"(5) Any one failing to send one print bi-monthly shall be struck off of the book of the party he so fails to send to, unless satisfactory reason is given for his default.

"(6) All photographs must be properly labeled with a descriptive name, the name of the artist and the date of the

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* *Anthony's Photographic Bulletin*, 1888.

printing, and they must be guaranteed not to fade for two years; and if toned by experimental process, must be marked 'Experiment.'

"(7) Two unmounted prints shall be equivalent to one mounted and finished of the same size. Two-card or quarter-plate shall be equivalent to one stereoscopic, and two stereoscopic to one whole plate."

Among the members were H. T. Anthony, Lewis M. Rutherfurd, the astronomical photographer; F. F. Thompson, of Wall Street, Augustus Wetmore, C. Wager Hull, John M. Masterton; Prof. O. N. Rood, the eminent physicist, Professor Emerson, of Troy; in Philadelphia, Constant Guillou, Coleman Sellers, F. T. Fassitt, S. Fisher Corlies, Prof. Fairman Rogers, E. Borda, Dickerson Sargent; in in Washington, Tition R. Peale, and Rev. Dr. Charles Hall; in Pittsburg, Capt. T. J. Brereton; in Cumberland, Md., Robert Shriver; in Boston, Dr. John Dean, and in Baltimore, George B. Coale, and others perhaps in the later years, as the rule in regard to numbers readily relaxed to admit a desirable additional member. The exchange prints in possession of each of the members of that club constitute unique collections in the character and variety of the subjects and workers, and are perhaps the best authenticated prints of that date, with reliable data, on many desired points. The specimens presented for your inspection were taken, almost at random, from my own collection. They will be found generally in most excellent condition, showing no more the effect of time than a collection of engravings might do. They have been kept, not with very special care, in pasteboard boxes, and have been handled in use as stereographs, perhaps more freely than ordinary photographs.

Prof. O. N. Rood, for many years of Columbia University, New York, writes: "A short time ago I examined all my old photographic prints and found them in perfect order; I also examined the prints received from members of our club: they were as good as new. But I have photographic portraits of Dr. John Torry, the chemist, and of Dr. B. A. Gould and others, that have nearly faded out, though not nearly so old as ours."

Robert Shriver, President of the First National Bank, Cumberland, Md., reports his, so far as yet in his possession, in good condition, exceptions due to bad washings and hypo. Dr. Coleman Sellers states "a great many of these are in an excellent state of preservation, running uniformly through the work of the various members of the club, and they have not been given any special care other than that they have been kept in separate cases and labeled, the cases being open at one end."

F. T. Fassitt reports that prints on albumen paper afford greatest claim for permanency. Prints made of the Sanitary Fair, 1864, hung exposed to changes of light and weather in his library since then, have retained all their brilliancy without the least sign of deterioration.

But there was a great deal done by members of the club, outside of the rules and simple exchange of prints, in way of exchange of ideas and experience, with permanent results in some cases upon photographic practice. Thus alkaline development of dry plates was first suggested by Borda in place of the universally practised acid development. The great problem of that day was the production of a dry-plate process that would equal in rapidity and results the collodion wet-plate process, and require the minimum of trouble in practice. The virtues of beer, solid milk, albumen, tannin and many other substances were tested and reported upon. The tannin process seemed to be the survival of the fittest. In this connection Borda also suggested warm development. Many of the prints exhibited are from tannin negatives, and the negatives and stereographs on glass by that process show its possibilities. It is exceedingly simple and certain, and, according to a method contributed by myself, plates can be prepared up to the last stage, of flowing with a solution of tannin—fifteen grains to ounce of water—in broad daylight. But a far more interesting fact, just put in my possession, bearing on photographic permanency, are the prints, here exhibited, from tannin negatives, on plates prepared in 1863 and exposed in 1898. The plates made by him—Mr. Shriver writes—"had been hid away in an outer room, where they were subjected to extremes of heat and cold, but were kept perfectly dry. They come up in development as good as I could have expected if the exposure had been made in 1863, instead of thirty-five years later." The prints exhibited will be found to compare favorably with those from the best gelatine dry-plates of to-day. He adds very pertinently: "Here, then, was an answer to the query, 'How long will dry plates keep?' Good for thirty-five years, and perhaps indefinitely, if made by the 'tannin' process." Apropos at this point is an interesting experience of Dr. Coleman Sellers in regard to the permanence of the impression in the camera before development. A dozen tannin plates furnished by him to a friend were returned in three weeks as exposed. After several had given no results upon development, he re-exposed one of the plates and obtained an excellent negative. From a series of experiments, subsequently made, he concluded that between the time of exposure and development every day that intervened diminished the chance of success. He regards the effect of exposure "as a strain put upon the parts of the film according to the intensity of the light that fell upon each particular moment of surface, but this strain would gradually be relaxed if not fixed by an immediate development, and there was no chemical action on the plate, but mere physical action that was temporary in its character." To return to the club prints—and among these are included some by Joseph M. Wilson, of the same period—they will be found to be exclusively on albumen paper, and, except when marked experimental, as a rule toned in alkaline gold bath, and fixed in separate bath. The paper in many, perhaps most, cases was albumenized by the members themselves, as commercial albumen paper in early days was not as uniform in quality as at present, and poor prints were apt to be attributed to the paper, and often with right. Many of the members of the club had the advantage, several years before its publication, of one of the greatest improvements in albumen printing imparted to them by H. T. Anthony, namely, the fuming of the well-dried sensitized paper with ammonia. It eliminated many sources of failure. Considerable variety of tone and of surface is observable in the prints: some are somewhat discolored, but few more so than a collection of engravings under the same circumstances would exhibit, whilst very few show symptoms of fading, and in slight degree. The print of the full-moon, by Rutherfurd, it will be noticed, shows no discoloration or change of any kind, whilst the white mount has very perceptibly discolored.

It has hung, framed, in a well-lighted room for years. In some prints little specks of mold appear upon the face, which can be rubbed off without apparent injury to the print. This seems to originate in the paste, as the labels on the back in some cases show the same effect, even when the prints are free from it. The mold does not necessarily involve the albumen of the print. The portrait shown, which hung upon a damp wall, was half of it loosened from the mount by a mass of mold, and yet, at present, scarcely shows the outline of it, after being soaked off from the mount and thoroughly washed. In the selection, readily made, from a collection of commercial prints of the same date as those of the club, many will be seen to have almost faded out of sight, among them some by makers justly of highest reputation, as Langenheim, of Philadelphia. These might be explained as toned with the old combined bath before its bad character had been established. Of the foreign views the French have deteriorated most; and the Italian, of which least was expected at the time, show less change; and the English and German are as a rule well preserved. The collection shows that albumenized paper can be made to yield results not only unsurpassed in beauty of tone and rendering of the best qualities of the best negatives, but, with proper care in the production, unchanged after forty years, and promise of remaining so for forty years to come. A first-class albumen print may then, indeed, at present be regarded as the standard of permanence, not on *à priori* ground, but as an experimental fact. The departures from permanence exhibited may be explained in many cases by the defects and uncertainties of the process at that early period, now eliminated, and in other cases by the want of care at all stages, especially in toning and washing. The uniformly high permanence of the club prints may be attributed to the character of the workers, their extreme care in manipulation and in the processes employed, and the limited number of prints made at a time, which made greater care possible. There is no argument in this for a return to albumen paper but surely a suggestion, that for record purposes albumen prints might well be included with those of platinum and carbon, which are so generally exclusively recommended or demanded. The practical lesson of the prints is that permanence is perhaps not so much a matter of process as of conscientious care in carrying out the conditions necessary for permanence with any process. Even the blue-print process, in the specimens here exhibited, that have been exposed to light and many conditions unfavorable to permanence, compares favorably with the best processes in this respect, whilst the platinum prints, with pure whites, when made about twenty years ago, show discoloration, due most likely to the paper. This simply shows that it is unsafe to argue too far on *à priori* grounds from the permanence of platinum as a metal to the permanence of prints based upon it, where so many complex conditions are involved, conditions still matter of discussion by Chapman, Jones, Haddon, Jacoby and others, in explanation of admitted deterioration of platinum prints in some cases. The duplication of prints for record purposes by several of the most approved processes, made by persons of established character for conscientious care in printing, with a record of the essential details of the process employed in each case, might contribute something to the character of the record.

The albumen process, as the prints show, has no superior for photographic expression, rendering all the good qualities of a negative. It is a perfected process: simple, easy, not more troublesome than any printing-out paper, and with little practice, certain in its results. Its fine surface is not easily abraded, and it permits warm solutions and warm water at all stages without the least risk of injury. The paper ready for sensitizing is a commercial article of uniformly excellent quality. It is without offensive gloss. Matt and glassy albumen paper, ready sensitized and of good keeping qualities, have been recently advertised by a German firm, and have been commented on favorably for certainty and excellence of results, said not to be distinguishable from those of platinum. The demand for albumen paper in this country, according to a reliable estimate, is only about five per cent. of that of ten years ago. Its use is confined to those who still demand it, and to cheap commercial work on a large scale, in this; especially stereographs.

The loss of popularity is due in part to the requirement of negatives of the highest class, and in a measure adapted to it. Negatives that might yield very valuable prints with some of the more recent papers would be absolutely worthless with albumen paper. The developing papers, too, permit practice of photography by many who would otherwise not find time to indulge in it, and economize time for the professional. It is in connection with record work, however, that albumen paper has some claim to be considered.

ELECTRO-PLATING BATHS.

M. Jules Meurant, of Liége, Belgium, in a recent patent describes a series of baths for the electro-deposition of zinc, tin, nickel, copper and silver, the characteristic feature in all cases being the addition to a neutral or alkaline solution of the metal, of an organic non-electrolyte, generally a carbohydrate, and in the majority of cases a polyatomic alcohol or derivative. The chief interest of the patent is in the bath described for the deposition of adherent coatings of zinc upon steel. This solution is formed by precipitating zinc from its chloride solution as carbonate, redissolving in ammonium chloride, and adding for each kilogram of zinc chloride taken, 300 grams of gum arabic or equivalent amounts of sugar or glucose. The patentee states that steel tubes coated with zinc from this bath were successfully submitted to the following tests preparatory to their acceptance by the Dutch Navy: (1) Crushing to completely flatten the tube; (2) folding the flattened tube upon itself; (3) crushing a ferrule cut from the tube in the direction of the axis of the cylinder; (4) elongation test of 27 per cent.; and that the protecting layer of zinc showed no alteration either by scaling or breaking. Zinc baths containing carbohydrates are well known in the art, having been patented in France by Blas and Miest, in Germany by Kaselowsky and Basse, and in this country by Frauenfelder and Meyer; the claims of the present patent are limited, therefore, to the specific procedure in preparing the electrolyte.—*Electrical World.*

Section of Photography and Microscopy.

Stated Meeting, held Thursday, March 6, 1902.

A PHOTO-MICROGRAPHIC DEVICE.

By Frederic E. Ives,
Member of the Institute.

Photography, as a means of recording scientific phenomena, is more used and appreciated every day, and would no doubt be more used than it is if all investigators were fully acquainted with its capabilities and equipped to obtain satisfactory results without unnecessary expenditure of time and labor.

Take, for example, the production of photo-micrographs. Probably most people who use a microscope would like occasionally to fix the image which they see; yet comparatively few do so, because it is generally believed that successful photo-micrography involves the exercise of exceptional skill and experience, special and expensive camera devices, rearrangement of microscope and illumination, in positions comparatively difficult to accommodate one's self to, and much time lost. Undoubtedly there is much truth in this, as photo-micrographs are generally made. It is true that a very small and light camera can be attached to the tube of the microscope, over the eye-piece; but under these conditions the amplification in the photographs is much less than it appears in the microscope, and the optical conditions are not of the best.

After making a series of photo-micrographs in the usual way, with the microscope on a special stand and the tube disposed horizontally, I recently set myself the problem of providing for the reproduction of the image observed in the ordinary use of the microscope, without any readjustment whatever, without touching the microscope or even re-focusing. I made the conditions that nothing should interfere in the slightest degree with the comfortable use of the microscope in the usual way; that it might be used at any desired inclination or tube-length, and with any convenient source of light; that the adaptation and removal of the camera should occupy very little time, and that the amplification in the photograph should always correspond to that in the microscope.

After some study I set to work and managed, with a box-lid, two small shelf-brackets, the bed and rack of an old 4 x 5 camera, a box originally made to carry some 2½ x 3 plate-holders, a 10-inch focus lens, and some minor odds and ends, to produce the device which I now bring to your notice, and which fulfils all of the conditions I have named.

To commence with, we have the box-lid, a clamped piece of ½-inch mahogany, about 10 inches wide and 12 inches long. The microscope is used on this as a base, on which it is held in place by stops against which it is pushed. This base is necessary to provide a fixed support for the camera attachment without touching the microscope itself. Next we have one of the small shelf-brackets, securely attached to the baseboard on each side of the microscope, in such manner that one of the screw-holes in the end comes exactly opposite the center of the joint of the microscope. They are separated just sufficiently to clear all the working parts of the microscope. The screw-holes in the brackets are the points of attachment for the camera device, and must occupy such a position in order that the camera may swing from the same center as the microscope body, and thus be adjustable by a single movement for any desired inclination, from horizontal to vertical. On a double pillar microscope, and even on the Swift portable which I am using, the camera attachment could be adapted to swing from the centers on the microscope itself, and the special base and brackets thus dispensed with; but with the usual horseshoe base "continental" stands the bracket supports are necessary. The total height of the bracket supports for a Bausch & Lomb "BB" stand is not over 3½ inches, and they are not in the least in the way of anything one ever wants to do with a microscope. A detachable double upright can, however, be used, if preferred, and fastened to the microscope base at a moment's notice.

The camera is a simple box, with a lens at one end and fitting for a plate-holder at the other—the lens of 10-inch focus, and the distance from the lens to the sensitive plate, 10 inches. This length of camera insures an image having the same amplification that would be calculated for that in the microscope, provided that its lens occupies the normal position of the eye above the eye-piece. The 10-inch camera and lens are used on the assumption that the position of the objective and eye-piece cannot be altered without altering the character of the image; that the rays forming each pencil of light emerging from the eye-piece are parallel to each other when the microscope is focussed, and that the camera must, therefore, focus parallel rays in order to make a sharp image of the microscope projection at the focal plane without re-focusing the microscope. This assumption is correct under certain conditions, which I shall specify later.

I did not have a 10-inch focus lens, but had 12-inch and 60-inch plano-convex lenses. A pair of these cemented together made a crossed lens of about 10-inch focus, and I mounted it in an old view-lens tube, so that the flattest side comes towards the eye-piece, but an inch back from the eye-point, when the mounts almost touch each other. I should not have used this lens mount, because, although it does not affect the focus upon the ground-glass to move the lens back from the eye-point, it does alter the size of the image by bending the outer pencils of the cone of light in towards the middle. One should either use a double-convex lens at the eye-point, or a lens of longer focus above the eye-point, in combination with a correspondingly longer camera, in order to keep the amplification strictly correct.

On general principles, I would not select a non-achromatic lens for this purpose, and do not recommend it; but it happens that with some achromatic objectives and huygenian eye-pieces a non-achromatic lens gives the best projection. It does not make much difference whether it be achromatic or not if monochromatic light is used for photographing, which is what I recommend with this method of working. The fact that objective and eye-piece combinations do vary somewhat in this respect makes it a safe rule to photograph with approximately monochromatic light of the brightest part of the spectrum, thereby insuring definition equal to that seen in the microscope.

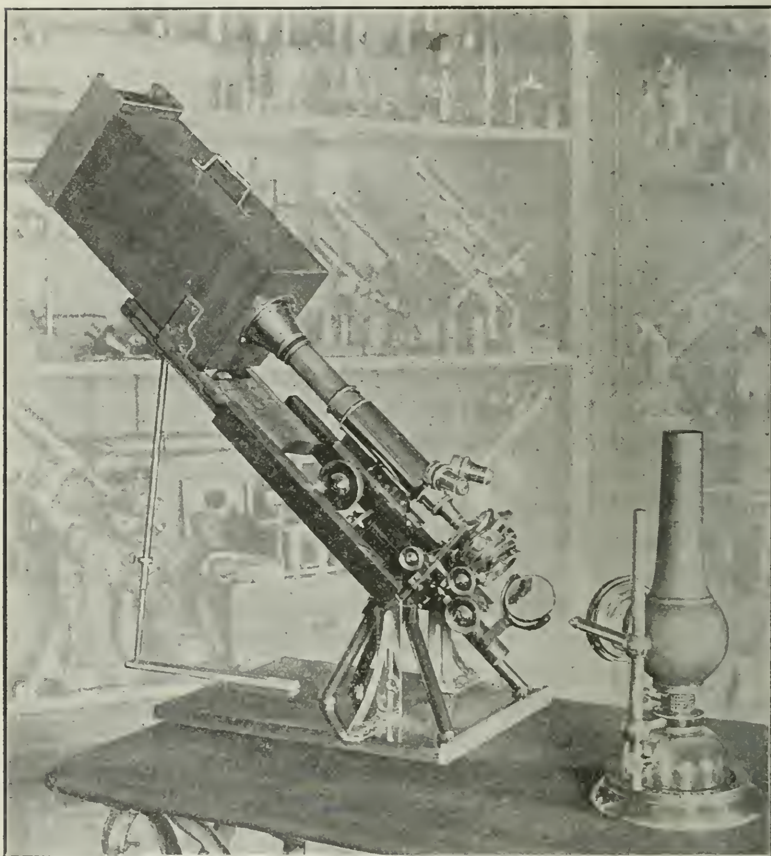

Photo-micrographic camera adapted to a Swift Folding microscope, used on a special base with brackets.

The camera has a rack and pinion movement on a base-board having two rigidly attached arms extending forward, and carrying pins to engage in the screw-holes of the brackets. I have cut slots into the screw-holes, so that the pins drop into place, and provided an automatic lock which prevents the pins from being lifted out except when the camera is swung below the horizontal plane. An adjustable telescopic strut and detachable extension to the baseboard supports the camera and fixes it at any desired inclination. It is in alignment with microscope tube when the camera lens-mount and the eye-piece are concentric with

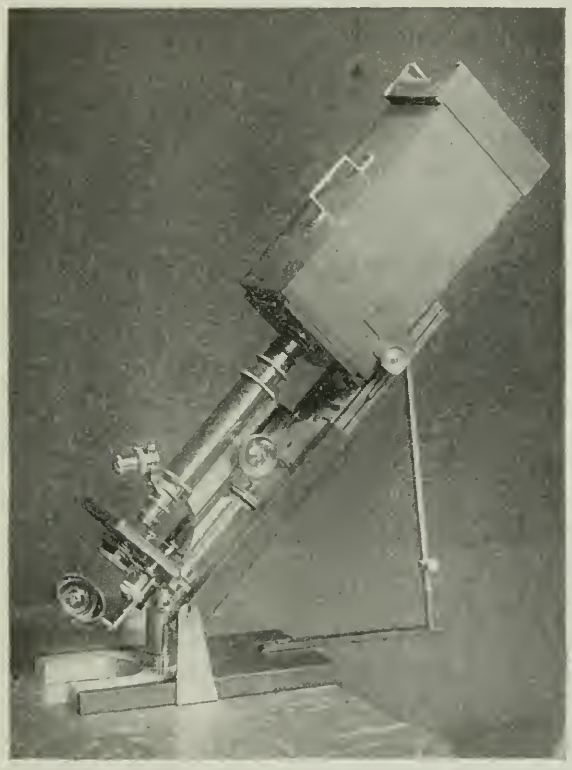

Photo-micrographic camera adapted to a Bausch & Lomb "BB" microscope, with detachable bracket support.

each other when brought together by the rackwork. For optical reasons, strict alignment is far less important than it would be with a camera containing no lens.

It takes me from twenty to thirty seconds to attach the camera and adjust ready for an exposure. A color screen is then [usually] placed between the source of light and the microscope mirror, admitting approximately monochromatic light of such color as the objectives are best corrected for. Plate-holder is then inserted, and exposure made, using an isochromatic plate.

Photo-micrographs made in this way, ranging from comparatively low amplifications up to 1,500 diameters, cannot be distinguished in character or quality from similar ones which were made after most careful preparation with far more elaborate, expensive and troublesome photo-micrographic apparatus.

Heretofore, I have done this work on a specially heavy table, legs resting upon rubber buffers, heavy marble top also resting on rubber buffers, an inch-board resting on coils of rubber tubing. Without these precautions, I had trouble with vibration when the microscope was used in a horizontal position. I have been using the new device on an ordinary table, and have been surprised to find no difficulty whatever with vibration, so far. The microscope appears to be far less sensitive when inclined at approximately 45° angle than when either horizontal or vertical. I exposed two hours on an amphipleura with oblique light from a Welsbach burner, and the lines came out sharp. Most subjects, with central light, require only one to five minutes' exposure, using rapid isochromatic plates.

I have stated that one of my objects was to avoid the necessity of re-focusing the image, which is objectionable, both because of the time it takes and because it alters, however slightly, the character of the image. The conditions under which no re-focusing is necessary are, that the camera lens shall focus parallel rays at the focal plane of the camera, and that the microscope shall have been focussed with an eye that focuses pefectly parallel rays upon its retina. Under these conditions, the rays forming each pencil of light emerging from the eye-piece are parallel to each other, and will therefore come to a perfect focus at the focal plane of the camera. If the microscope were focussed by a short-sighted or an abnormally far-sighted eye, the rays forming the emerging pencils would not be parallel to each other, and therefore would not come to a perfect focus at the focal plane of the camera.

Huygenian eye-pieces, and some others, are calculated for an eye that focuses parallel rays, and cannot give the best results to either a short-sighted or an abnormally far-sighted eye. A short-sighted person sees an image which is formed above the correct plane in the eye-piece, and an abnormally far-sighted person sees an image which is formed below the correct plane. A person having theoretically normal sight will focus the microscope correctly, not only for the best image obtainable, but for the focal plans of my photo-micrographic camera. Any one whose eye is not theoretically normal will have to re-focus on the ground-glass of the camera when using low powers; but when he has done so, the image will be exactly what it should be. My own eyes are short-sighted, and in pursuit of my object I have as nearly as possible overcome this difficulty by providing myself with a compensating lens in a mount which fits loosely over the eye-pieces of the microscope, and makes my vision, for the time being, theoretically normal. As this lens belongs to my eye, and not to the eye-piece, I remove it after focusing. This procedure is equivalent to wearing spectacles adjusted for clearest vision at very great distances, which any one may do; but the eyes of very young people, with great powers of accommodation, may not even then be trusted implicitly when working with very low powers. With high-power objectives and eye-pieces, hypermetropes, emmetropes and moderate myopes focus practically all alike.

This new outfit is not calculated to supersede some of the very elaborate and expensive photo-micrographic apparatus and devices which have been produced by Zeiss and others for some special purposes. It will not produce photographs of large size, such as can be made with projection eye-pieces and very long cameras, nor include wide angles of view in low powers, such as may be obtained with Zeiss planar lenses without eye-pieces; but it should be far more generally useful, because it can be relied upon to reproduce clearly whatever detail the eye sees in the microscope, and with the least possible cost, either in time, labor or expenditure for equipment.

It will be evident that the same camera and procedure as to focusing is adapted to making records with telescopes, spectroscopes and some other optical instruments.

ELECTRICITY DIRECT FROM COAL.

The latest of the many indefatigable attempts made to obtain electricity direct from coal is that of Hugo Johe, of Chicago. He has obtained the patent on a new battery. In his invention a retort is employed, which is surrounded by a cylindrical case containing a battery of four cells. The furnace gases from the retort circulate against the inner side of the cell and case.

Partitions of porous coal divided each cell into three sections, containing respectively nitric acid, sulphuric acid and ferric chloride solution. In the nitric acid an electrode is immersed, and in the narrow chloride solution a lead electrode.

The operations are as follows: A suitable quantity of sulphate of lead is placed in the retort with a quautity of coal nearly sufficient to reduce the sulphate to sulphide, and the mixture is then heated until all of the coal is oxidized. The sulphide is freed from impurities which may have been brought into it by the coal, and is then mixed with sulphate of lead in sufficient quantity to yield metallic lead and sulphur dioxide, which reduction is effected by again applying fuel heat to the retort. The sulphur dioxide passes through a pipe into the larger section of the several cells, reducing the ferric chloride therein to ferrous chloride. Previous to this the generation of the electric current is started by putting the lead electrodes into the larger section and suitably connecting them with the carbon electrodes. The current may be considered as consisting of two currents, one generated by the action of the ferric chloride of the lead electrode, and the other by the action of the nitric acid through the interposed porous walls, and sulphuric acid by means of molecular exchange of ferrous chloride. The flow of sulphur dioxide is so regulated that the sulphuric acid formed is not more than sufficient to decompose the chloride of lead formed in the battery reaction. The lead in the retort is allowed to flow into a pan, where it is suitably shaped or solidified for an electrode. The sulphate of lead deposited by the battery is allowed to accumulate, and at intervals is drawn off by means of siphons, and the deposit of sulphate of lead electrode removed. The temperature of the battery is regulated so that the nitric acid which enters into the sulphuric section is evaporated, the vapors being passed through a condenser and there condensed again to nitric acid, flowing back into the nitric acid sections through a pipe. This process of distillation, oxidation and condensation is kept up by regulating the temperature of the battery and supplying sufficient cooling water to the condenser. Thus oxygen is supplied to the nitric acid, while the generation of electric energy with consumption of oxygen goes on. The E.M.F. of the cell at 100° C. is said to be about 1·75 volts.—*Scientific American Supplement.*

Mining and Metallurgical Section.

Annual Meeting, held Wednesday, January 8, 1902.

A REVIEW OF RECENT PROGRESS IN THE MINING INDUSTRIES OF THE UNITED STATES.

BY F. LYNWOOD GARRISON.

Being the address of the retiring President.

Since it is the custom of the retiring President of our Section to present to you an address of this meeting, I have chosen for my theme a short review of the progress and notable changes that have taken place in the mining industries of the United States, during the past one or two years.

It is obviously impossible for me to treat so large a subject exhaustingly in a paper of this character, consequently many interesting matters must be passed unnoticed.

Of all the mining and metallurgical industries, the most notable advance evidently has been in those relating to coal and iron, which to-day exhibit a degree of prosperity that would have been thought impossible ten years ago.

The present highly active condition of all our manufacturing industries have resulted in an unprecedented demand for fuel, taxing to the utmost the railway facilities for delivering it to the consumer, though there is apparently no difficulty in mining all that is required. This statement, however, needs some qualifications in its application to the anthracite coal production. In 1900, the output of this class of fuel was considerably less than in 1899, and when the figures for 1901 are available a further falling off is likely to be observed.* A result of this has been a continu-

* Statistics to hand since writing the above shows this is not the case, the production being for

	Tons.
1899	47,665,203
1900	45,107,484
1901	53,386,271

It is unlikely, however, this increase will continue, or the production for the past year hold its own, unless the present exceedingly prosperous and stimulated condition of business continues.—F. L. G.

ous displacement of anthracite by bituminous coal, with a consequent appreciable increase in the smokiness of the atmosphere of our large eastern cities.

Whether or not the increased price of anthracite is justifiable, in view of the economies and improvements in up-to-date mining, is a question I cannot undertake to discuss. We must consider, of course, that as the deposits are worked out deeper mining is necessary with its consequent greater cost and difficulties in haulage, ventilation and drainage. I do not believe the best of modern machinery or methods can on the whole overcome these difficulties sufficiently to permit the mining of anthracite coal as cheaply as it was produced ten years ago. There is no doubt that in Philadelphia, for instance, we are paying more for anthracite than is just and fair. This is due chiefly to the freight charges, and such extortion benefits the railway rather than the miner. If such conditions check production, they are not an unmixed evil, for, we must remember, the Pennsylvania anthracite fields are relatively small and are being rapidly exhausted.

A notable feature of the bituminous coal industry is the disposition to consolidate the interests involved in its production. This is especially true in what is known as the "Coking Coal Fields," and it is evident that the iron and steel producers are looking far into the future for the purpose of making sure of their fuel supplies for many years to come. While the Connellsville deposits are not yet exhausted, it appears from the best available information that they cannot last much longer, hence other districts must be, and are now being, rapidly developed to meet the ever-increasing demand for coke.

A much more serious difficulty facing the steel worker is the steadily increasing difficult of obtaining iron-ore. The demand has grown so enormously, that it would seem likely, at least in Pennsylvania, that we will soon be as badly off in this respect as the British steel manufacturer, and will have to bring our ores from over the sea. The Lake Superior iron-ore deposits are justly considered the largest in the world; but it seems likely that their superficial limits have been about determined. Persistent explorations north of the Canadian boundary line have thus far failed to realize expectations, and the hopes of finding an extension of the Minnesota deposits into Canadian territory are by no means bright. In the case of manganese ores a condition has been reached wherein we are largely, if not chiefly, dependent upon foreign sources of supply.

When we consider that the steel industry in this country is not yet fifty years old, and the leaps and bounds with which it is growing, the question forces itself irresistibly, can this pace be maintained? It is true, just as in the case of the basic process, that new inventions will doubtless render ores available which under the present conditions cannot be used, as, for instance, those containing appreciable quantities of titanium and the iron-sands of certain localities. It is now possible to concentrate by magnetic methods, lean and silicious ores heretofore considered unfit for use. Such methods, however, necessitate the comminution of the ore, a condition possessing serious objections when such concentrates are used in a blast-furnace. But these supplies would be a very small proportion of the enormous quantities required, and with our present knowledge of the country's mineral resources it is difficult to see where they are to be obtained. One hundred years is but a short space in a nation's life. Our once seemingly inexhaustible timber supply is rapidly nearing the end, and the nation is but 125 years old. Ores, unlike trees, cannot be cultivated and reproduced; deposits once exhausted are done for all time. We will be told to go deeper down into the earth; but as every experienced mining engineer knows, this is fallacious. Mineral deposits do not extend down into the earth indefinitely, and with the rapidly increasing improvements in machinery these limitations in depth are not likely to be beyond reach; in fact, in the great majority of cases they are so even now.

Since the demonitization of silver in the United States the production of gold all over the world has steadily increased. In this country many deposits previously regarded as silver mines are now denominated gold producers; not because the silver has been replaced by gold, but simply owing to the fact that with improved methods and economics it has been found that the contents of gold in the ores has been ample to overcome the decreased value of the silver. It is doubtful if, as a general thing, those silver-producing sections of the country that were the most ardent upholders of the bimetallic monetary doctrines are to-day any less well off than five or six years ago before the great decline in the value of silver. It is, of course, perfectly true that there are some sections where inaccessibility and cost of transportation have made it impossible to operate many silver-lead deposits previously profitable. Such ores, lacking the redeeming gold element, must await more favorable conditions, which are certain to come with the development and settlement of the country.

The use of electricity generated by water power is becoming very general in sections where fuel is expensive. The great improvements made in tangential jet water wheels have resulted in economics hitherto not supposed possible, with the result that many low-grade mines can now be operated that were previously unprofitable to work.

The introduction of the cyanide process has been a great boon to the gold-mining industry. We have been slow to adopt it in this country, and its merits are even yet by no means fully realized, nor its limitations reached and understood.

On the whole, conditions have never been so favorable to the economic extraction of gold from its ores, and the exploitation of many of our low-grade gold-deposits constitute to-day some of the best opportunities for legitimate mining that has ever existed in the world. Investments in enterprises of this character are probably safer than those in the great majority of commercial and manufacturing schemes. The product has an unalterable value and constant demand, not affected by trust, statute or panic, which is more than can be said for any other mineral or agricultural product in the world.

It seems rather strange that the United States has thus far produced little or no tin. Small deposits exist in various parts of our wide domain, but the repeated attempts to work them have, without exception, resulted in failure. Large tin-ore bodies may yet be discovered, possibly in the great granitic areas of Idaho, which are as yet practically unknown, even in a geographical sense. Our present supply of tin comes altogether from the Malay Peninsula and the adjacent islands. How long these deposits can meet the world's great demand upon them there is no means of ascertaining. From all I can learn, much of the tin-bearing area of these regions has been exhausted. It is probable, however, that new deposits will be developed in Siam, Borneo and Sumatra.

The production of copper in the United States has increased from 295,812,076 pounds in 1891 to 606,117,166 pounds in 1900; that is, 310,305,090 pounds or about 51½ per cent.—a greater ratio of increase than that shown by any of the other metals. This condition has no doubt been caused by the enormous demand for this metal in the electrical arts, with a consequent high price, and also improvements in methods of extraction from its ores. The result has been that vast quantities of low-grade ores can now be operated at a profit, whereas a few years ago it was impossible for them to compete with the rich and unique deposits of Lake Superior. New deposits of lean-ores are being discovered througout the Western mining states and territories, notably in Arizona; but as yet no notably rich stores of this metal have been reported. It is therefore unlikely that the demand will be oversupplied and the present price naturally reduced unless some other metal or substance can be found to replace copper in electrical constructions, which is not at all likely. In fact, the returns for 1901 show a reduction, the total production being 595,000,000 pounds for the year, a falling off of about 18 per cent. The pneumatic or Bessemerizing process for the treatment of mattes and electrolytic refining has been of inestimable value to the industry, and there seems to be a constant and steady improvement in all other directions; hence, the outlook for the future of copper production is most encouraging.

The lead industry has not been marked by any great advance during the past decade, although a steady, moderate increase has been manifested from year to year. Southeastern Missouri is still the center of the purely lead-mining business. Here we have peculiar deposits known as "disseminated" lead-ore, which show no signs of exhaustion, but, on the contrary, the developments from year to year tend to the conviction that these great sheets of galena cover a larger area than has heretofore been suspected. It is most probable, therefore, that Missouri is likely to maintain her prominence as a lead-producer for many years to come.

The production of zinc has, likewise, not been largely increased, notwithstanding the phenomenal activity in the Joplin Region in 1899 and 1900. The zinc-ores of this district are by far the best in the world, but the deposits are relatively small, pockety, uncertain, and of slight depth. The area covered by them, however, is great, and the district as a whole is as yet but slightly developed. These ores, however, must now compete with the cheaper ores from the silver-lead districts of the Rocky Mountains. This, together with the relatively slight increase in the consumption of zinc in the arts, has kept prices low. The Joplin miner has, therefore, been confronted with a falling market for his ore and a steady rise in the cost of supplies, fuel and labor; for which reasons his lot for the past year or two has not been an altogether happy one; and most of the many enterprises floated during the Joplin boom of 1899 have come to an unfortunate end.

In the metallurgy of zinc there have been no radical improvements. Gas-firing of the retorts is now adopted whenever it is found possible, and when natural gas is not available, producer-gas is used. Some advance has been made in the system of roasting, a process which has hitherto been crudely carried on.

The production of quicksilver in the United States shows a decided decline, notwithstanding the steadily increasing demand for it. The greater part of the production comes from California; but these mines are beginning to show symptoms of exhaustion. Cinnabar has recently been found in quantity in Texas and in British Columbia, and within the past four months is reported from the Thunder Mountain district of Idaho.

As is the case with tin, it will soon be a serious matter to obtain a sufficient supply of quicksilver to meet the world's demand. The older deposits have about reached their limit of production, and no new supplies are reported save those in Kwei Chau province in China; but how soon these apparently great deposits will be available is very uncertain. From what little personal knowledge I have of the geology of Central Idaho and Eastern Oregon, I would not be surprised if cinnabar in workable quantities would eventually be discovered in these as yet little-known (in a geological sense) regions.

One of the remarkable and surprising incidents of the mineral development of this country during the past decade has been the discovery of the vast extent of the regions containing petroleum. These deposits appear to exist in rocks ranging all the way from the older Devonian of Pennsylvania to the comparatively recent Tertiary of Texas and California. The newly discovered Texas deposits appear to be of a richness equaled only by the famous deposits of the Caspian Sea Region. The oil of the younger rocks differs somewhat from that of Pennsylvania, which, all things considered, is probably the best in the world. Good flowing wells have recently been struck in Utah and Wyoming, and it is not at all improbable that the oil-yielding territory may be still further extended.

TECHNICAL NOTES FROM VARIOUS SOURCES.

——Rubber in Venezuela is the subject of a most interesting report in the Consular Bulletins of the State Department from Consul Goldschmidt, of La Guayra. It embodies a description of the caoutchouc of the Upper Orinoco by Dr. Lucien Morisse, who says that 50,000 emigrants could easily be employed in the forests gathering the gum, and cutting the trees, without any injury to the permanence of the supply.

——An artesian well in Grenelle, France, took ten years of continuous work before water was struck, at a depth of 1,780 feet. At 1,259 feet over 200 feet of the boring rod broke and fell into the well, and it was fifteen months before it was recovered. A flow of 900,000 gallons per day is obtained from it, the bore being 8 inches. At Passy, France, there is another artesian well 1,913 feet in depth, and 27½ inches diameter, which discharges an uninterrupted supply of 5,500,000 gallons per day; it cost $200,000. An artesian well at Butte-aux-Cailles, France, is 2,900 feet in depth, and 47 inches diameter. These are all surpassed by an artesian well in Australia, which is 5,000 feet in depth.

—*The Electrician* states that a proposal has been submitted to the municipal authorities at Rouen, by the chief of the fire department, for *the utilization of tramway trolley wires in connection with the extinguishing of fires.* All the principal thoroughfares of the town are provided with electric tramways, and the proposal is that pumps capable of being electrically driven should be installed in a number of suitable positions on the tramway route, to be switched on to the trolley wires, so that the pumps may be used as occasion necessitates. The proposal is said to have been favorably received, and is now under the consideration of the authorities.

—IN a recent lecture in Philadelphia, Sir Robert Ball is reported to have utterly discredited any idea of "*talking with Mars.*" Sir Robert estimated that to signal Mars so as to be seen by a Martian astronomer who happened to be looking this way, using the wigwag system, we should need a flag 300 miles long and 200 miles wide, waved from a staff 500 miles long. Flashlight signaling he considers equally impracticable. If Lake Superior could be filled with petroleum and set on fire, the resulting blaze might be discerned as a speck of light, but not sufficiently prominent to suggest that anything unusual was in progress. He does not appear to have considered electrical means worthy of discussion.

—IN order to combat *the smoke nuisance*, the Prussian government called together a committee for the trial of all smoke-consuming apparatus, says *The Engineer.* This committee has finished its work, and common measures are about to be taken to remove the evil. It was proposed to institute schools where stokers could be specially trained for the handling of steam-boiler plants. The Steam Boiler Revision Association consulted most of the branch associations, and the result was that most of them were against the idea of the schools; but it was proposed to send properly qualified men to instruct the stokers employed in the various steam plants.

—THE AURORA, ELGIN AND CHICAGO RAILWAY, now being built, will employ a third rail weighing 100 pounds per yard. This road is to give a high-speed service between Chicago, Aurora, and Elgin by way of Wheaton. The track is being laid with a view of maintaining a maximum speed of 70 miles an hour on the level, and the motor equipments which have been ordered are designed for that speed. The General Electric train control system is to be used on the cars, and there are to be four 125-horsepower motors on each car.

IN MEMORIAM.

JOSEPH RICHARDS.

Joseph Richards was born in Stourbridge, near Birmingham, England, in 1840, and in his youth was a machinist in Chance Brothers' chemical works, where his father was a departmental superintendent. He studied chemistry at the Midland Institute, Birmingham, where he attended Hofmann's lectures. One of his classmates was Joseph Chamberlain, now Great Britain's Colonial Secretary. Later he was in partnership with his father in the business of manufacturing chemists, making copperas, blue-vitriol, and ammonia salts from gas-liquors. In 1871 he came to America, intending to take charge of a plant for working the gas-tar liquors of the Chicago gas-works, but the day before he was to start West, the great Chicago fire wiped out the whole plant. Finding himself in Philadelphia, he started to re-work the waste dross from galvanizing pots, and soon built up a large business, refining as much as 3,000 tons a year. His process was patented, and depended upon the treatment of the dross with sulphur vapor at a red-heat, whereby most of the iron was sulphurized, the remainder collecting at the bottom as an iron-zinc alloy. Later, the Delaware Metal Refinery was incorporated to work this process, and also to reduce waste oxides of lead and tin, and to manufacture solders.

JOSEPH RICHARDS.

A specific gravity balance, to test the grade of lead-tin solders, was devised by him, and is now in almost universal use in the solder trade. It is described in this *Journal* (May, 1899), and its value was recognized by the Franklin Institute by the award of the John Scott medal.

Later, the firm was one of the first to commence the manufacture of tin and terne plates, and Mr. Richards devised an ingenious balance for determining the amount of coating on specimens of such sheets. Since 1885 he was deeply interested in the production and possible uses of aluminum. He found many uses for the metal at a time when its cost ($8 per pound) made its application impracticable, but which have since come into general use. He patented the use of aluminum in deoxidizing and refining zinc and brass; also its use in small quantities in galvanizing pots. He devised and patented the most successful solder for aluminum which has yet appeared; a solder which has been in use ten years here and in Europe. This invention also was awarded the John Scott Medal on the recommendation of the Franklin Institute. He had large experience in remelting, handling and working aluminum; his hard, light alloys of aluminum with zinc being now in general use, and are acknowledged to be the cheapest light aluminum alloys of high quality to be had. Much of his experience in handling and treating white metals and their wastes is embodied in two communications to the *Journal of the Franklin Institute* (June and July, 1901). He was an active member of the Institute, having been succcessively President of its Metallurgical, Chemical, and Electrical Sections.

Besides being well known in the zinc, lead, tin and aluminum industries, he had a wide circle of most intimate and cherished friends, for to know him was to admire his equable and genuine character and to become attached to his attractive personality. From scientific acquaintances of the highest position to his humblest workman, he was beloved as a "best friend." He died in Philadelphia, March 22d, aged 62, leaving a widow and three children; his only son is Prof. Joseph W. Richards, of Lehigh University, the well-known writer on "Aluminium," and President of the American Electrochemical Society. His youngest daughter Florence H. Richards, is a practising physician in Philadelphia.

W. H. W.

STACY REEVES.

Stacy Reeves, a prominent master builder, and for a number of years a member of the Board of Managers of the Franklin Institute, departed this life March 8, 1902, in his seventy-fourth year.

Mr. Reeves was born on his father's farm, near Mt. Holly, Burlington County, N. J., on June 16, 1828. Here the early years of his childhood were spent. After the death of his parents—both of whom he lost during his childhood—he was placed in the home of a distant maternal relative, where he remained until fifteen years of age, attending the primitive country schools of the neighborhood during the winter and assisting about the farm in the summer months.

STACY REEVES.

After spending one year at the Friends' School at Westtown, he was apprenticed to Mark Balderston, a leading master carpenter of Philadelphia, with whom he learned his trade. He continued in the employ of Mr. Balderston some two years after reaching his majority. In 1851 he established himself in the business in which he continued with conspicuous success until the time of his death.

In 1862-63 he served in the Civil War as a member of the Pennsylvania Militia.

In 1869 he joined the Carpenters' Company, the oldest association of its kind in America (having been established in 1724). Of this association he served successively as Secretary (1889 to 1891); Vice-President (1892), and President (1895).

He was one of the charter members of the Master Builders' Exchange, of Philadelphia; was for a number of years one of its directors, and became its third President.

For a number of years prior to his death he was one of the directors of the Penn National Bank.

Mr. Reeves became a member of the Franklin Institute in 1889. He was chosen as manager in 1890 and served continuously until his death. For several years he also served as one of the curators, in which capacity his skill and experience as a builder proved extremely useful in connection with the extensive alterations to the library and reading room of the Institute, which were made about four years ago. In many other affairs of importance his colleagues in the board were wont to place the utmost dependence in his sound judgment.

The business that he founded in 1851 did not assume large proportions until after the Centennial Exhibition, in 1876, but in later years it grew very rapidly into prominence. The firm enjoyed the reputation of being one of the most conservative and reliable in the city. His son, Albert A., was taken into partnership in 1877, and a younger son, Henry, in 1885, and the firm name was changed to that of Stacy Reeves & Sons.

The firm founded by Mr. Reeves constructed many prominent buildings in Philadelphia and its vicinity. Among them are: The Wood Building, at Fourth and Chestnut Streets; Drexel Building, at Fifth and Chestnut Streets; Forrest Building, on the east side of Fourth Street, south of Chestnut; Hotel Lafayette, on the west side of Broad Street, south of Chestnut; the Lehigh Valley Railroad Buildings, at Mauch Chunk, Pa.; Lehigh University, at Bethlehem, and the Industrial School, built by the Misses Drexel, at Eddington, Bucks county. The restoration of the historic Independence Hall, ordered by the City Councils several years ago, was also undertaken and carried out by the firm.

Personally, Mr. Reeves was one of the most modest and unassuming of men. His sound business judgment was held in high esteem by his colleagues, and his advice and co-operation were never withheld when asked for. He will long be remembered for his sterling traits of character by his associates in the Institute, who sincerely deplore his loss.

W. H. W.

ANNUAL REPORTS OF THE DIRECTORS OF THE SCHOOLS OF DRAWING, MACHINE DESIGN, AND NAVAL ARCHITECTURE FOR THE SESSIONS 1901-1902.

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, April 25, 1902.

THE DRAWING SCHOOL:—It gives me great pleasure to announce that the classes have been larger this season than for many years, and that the attendance has kept up to the end better than ever before. This is not only complimentary to the school, but also shows that the students are here for business, and that they come here because they know that the time will be profitably employed and the results will be practical and useful to them in their various careers.

The industrial interests of the country are becoming so immense and the requirements in the way of knowledge and technic so great, that it behooves the young men of the land to take advantage of every opportunity to learn everything bearing upon, or of use in, their various trades and professions.

They will find that mechanical drawing is the very best study to start with. It trains their imaginations to conceive the location and relation of points, lines aud surfaces, the combination of these into simple solids, and so on up to complicated forms and their movements. It awakens a desire to understand the geometry of the forms and the kinematics of the movements, and thus leads up to the mathematics of the subject.

The work is interesting and gratifying, and no one who follows it up with ordinary application can fail to be greatly improved and to have his prospects and position bettered by it. That these facts are becoming better known and understood, the growth of the school and the work of the students amply testify.

My assistants this season have been: Mr. Clement Remington, Mr. John Rae, Mr. Edward V. Hindle, Mr. A. N. McConnell, and Mr. W. W. Twining.

Appended hereto are some data relating to the Branch School at Germantown Junction, which is in a flourishing condition.

WM. H. THORNE, *Director*.

THE BRANCH SCHOOL is located at No. 2906 N. Sixteenth Street, and it is readily accessible by the trains of the Pennsylvania and Reading Railroads and by the trolley cars. It is under the immediate direction of Mr. Haakon E. Norbom. The same text-books are employed and the same methods of instruction are followed as in the Central School. Pupils enjoy the same privileges also as to attendance upon the lectures. The Branch School is devoted exclusively to the teaching of Mechanical Drawing—the classes being divided similarly into Junior, Intermediate and Senior Classes. The beginning and ending of the winter and spring terms and the times of holding the classes and terms of instruction are the same as those of the Central School.

It is very gratifying to see the steady increase in the attendance at these night sessions. Last year it was found necessary to increase the desk room by building an annex which would accommodate two-thirds more than the original number of scholars. The year just ended (the eighth) shows an attendance for the two terms of 128, which tests the increased facilities of the school to the utmost.

Tickets for the Branch School can be obtained by addressing F. Macomb Cresson, at the office of George V. Cresson Company, Allegheny Avenue, west of Seventeenth Street.

THE FOLLOWING STUDENTS ARE ENTITLED TO HONORABLE MENTION:

In the Senior Mechanical Class.

Charles F. Miller, Frederick Thomas Uezzell,
Bernard G. Smith, Elmer S. Hicken,
L. Hastings Alexander, James Wilmer Shaffer,
Herman Ruch, Jr., Frederick Schoen,
Elmer B. Severs, Charles H. North.

In the Intermediate Mechanical Class.

George Blair, Herman Kilburger,
Howard W. Howitz, Henry O. Schmitt,
Christ. Bockius, Otto A. Guenther,
Henry S. Cowell.

In the Junior Mechanical Class.

Richard H. Marshall, Benjamin F. Norman,
Chester Dall, Clarence Vent,
C. M. Markoe.

In the Architectural Class.

Harry Stull, John Armour,
Eugene Fisher, Powel Dilworth,
Joseph Hettel, D. M. Donjian.

In the Free-Hand Class.

Edward Rush, Alfred Thornton,
Benjamin Feldman.

THE FOLLOWING STUDENTS ARE AWARDED SCHOLARSHIPS FROM THE B. H. BARTOL FUND, ENTITLING THEM TO TICKETS FOR THE NEXT TERM:

Leonard Hoerle, James F. Bowen,
Clarence Helmbold Wilson, J. B. Minick,
J. Warren Smith.

THE FOLLOWING STUDENTS HAVING ATTENDED A FULL COURSE OF FOUR TERMS, WITH SATISFACTORY RESULTS, ARE AWARDED CERTIFICATES:

L. Hastings Alexander, John Leschinsky,
John Bardsley, Charles F. Miller,
Daniel W. Dallin, James E. Murphy,
John F. Dyson, William J. McFarland,
Ernest Erhardt, Herman Ruch, Jr.,
John Folz, Edgar C. Sattler,
James E. Guenther, James Wilmer Shaffer,
Harry Fred. Hawkins, George W. Simons,
Elmer S. Hicken, Bernard G. Smith,
John Franklin Horner, Merritt Sticker,
Franklin S. Hodge, August Suess,
Eugene Fisher.

AND FROM THE BRANCH SCHOOL:

John Francis Kiely,	Ario Pardee Housman,
Charles F. Sturm,	Ernest R. Strenger,
Edward E. Fenstermaker,	David B. Mealing,
George W. Vache.	

SCHOOL OF MACHINE DESIGN:—While our expectation of a large increase in attendance has not been fully realized, the increase has been substantial, and in this respect the school is experiencing a healthy growth.

The earnestness and enthusiasm of the students in prosecuting their task is very gratifying. The excellent grade of work done is attested by the drawings presented herewith, which form a small portion of the problems assigned.

Instruction has been carried on by a judicious mingling of lectures and individual instruction following some standard text-book as a basis and by supplying hectograph copies of notes on the more difficult parts of the subjects.

Great care has been taken in presenting the details of the various subjects, and no theorems within the range of the students' mathematical knowledge have been allowed to pass unproved. This is particularly true of the course on the strength of materials, in the text-book of which much is taken for granted. Though the task of supplying proofs adapted to the conditions has involved considerable labor, the results attained have justified the expenditure of energy.

For the very satisfactory manner in which the mathematical work has been conducted I am indebted to my friend and colleague, Mr. E. H. Waldo.

LUCIEN E. PICOLET,
Director.

THE FOLLOWING STUDENTS HAVING COMPLETED THE COURSE ARE AWARDED CERTIFICATES:

James Anderson,	H. C. Gibson,
H. N. Becker,	Nathan L. Jones,
G. H. Benzon,	Harry J. Stephan,
Edw. J. Beuter,	Wm. Williams.

THE SCHOOL OF NAVAL ARCHITECTURE:—I have the honor to report that the school of Naval Architecture has more than doubled its enrollment of last year, and that the average attendance for the senior division during the winter term was 90 per cent., and for the spring term 90·9 per cent., and for the junior division 90·3 per cent. for the winter term and 90·5 per cent. for the spring term. Nine of the senior class have attended the full term of two years and will be graduated this term.

The senior class has studied and examined all kinds of general arrangements and the details of fittings for same, and completed the necessary calculations that are required for the proper and efficient design of any vessel. They have made themselves proficient in the use of the integrator, estimating costs, calculating weights of material and centers of gravity, strength of structure of the hull under different conditions, as well as the powering of ships. Their home-work has shown wonderful energy, considering that the drawings, etc., are made without the help of the usual adjuncts of a drawing office. In fact, the graduating class has exhibited more than ordinary intelligence, and its members are neat and careful draughtsmen, zealous and painstaking.

The junior students have progressed rapidly in both theoretical and practical naval architecture, and have also shown marked zeal in their work both in class and at home, numerous drawings and tracings beyond the average in neatness and accuracy having been made from blue-prints and drawings lent for that purpose.

By the kind permission of Mr. Morse, the president of the New York Shipbuilding Company, Camden, N. J., the class was allowed to inspect that company's plant as well as to witness the successful launch of a large cargo steamer. The class is also indebted to the following gentlemen for donating very valuable prizes for attendance, homework and examinations:

Mr. C. H. Cramp, president of the Wm. Cramp & Sons, ship builders and engineers, Philadelphia, Pa.; Mr. H. W. Morse, President of the New York Shipbuilding Company, Camden, N. J.; Mr. Lewis Nixon, proprietor and manager of the Crescent Shipyard, Elizabethport, N. J.; Mr. Clement A. Griscom, president of the International Navigation Company, Philadelphia, Pa. Mr. C. H. Cramp has also offered a special prize to the apprentices employed by his firm who are students of this school, and who show marked zeal and efficiency.

ALEX. J. MACLEAN,
Director.

THE FOLLOWING STUDENTS ARE AWARDED CERTIFICATES:

Wm. Binder,	O. B. Evans,
M. M. Borden,	C. H. Harden,
N. H. Brown,	H. B. Shields,
F. M. Evans,	J. L. Warner,

H. Yeager.

BOOK NOTICES.

Lessons in Applied Electricity, principles, experiments, and arithmetical problems. An elementary text-book. By C. Walton Swoope, Assoc. Am. I. E. E., etc. 8vo, pp. XIII + 462. New York: D. Van Nostrand & Co., 1901. (Price, $2.00.)

This work was originally prepared for use by the pupils of the evening classes in practical electricity, at the Spring Garden Institute, in Philadelphia, which were in charge of the author.

The various lessons embrace the following subjects: magnetism, magnetization, magnetic fields, theory of magnetism, magnetic induction, magnetic circuits, earth's magnetism, voltaic electricity, batteries, electrolysis, measurement of current strength, resistance, Ohm's law and battery connections, measurement of resistance, electrical development of heat, electro-dynamics, electro-magnetic induction, the induction coil, dynamo-electric machines, armatures, direct-current dynamos and motors, electric lighting.

To each chapter is appended a series of questions to test the progress of the pupil. The book is profusely illustrated. W.

The Engineering Index: Five years, 1896–1900. Edited by Henry Harrison Suplee, B.Sc., etc., assisted by J. H. Cuntz, C.E., M.E. 4to, pp. 1230. New York and London: *The Engineering Magazine*, 1901.

A feature of the *Engineering Magazine* that has contributed greatly to its usefulness, is the index of current engineering literature published with each impression. The present volume has been re-edited from the *Magazine* and embraces "The Engineering Index" of that journal, from 1896 to 1900, inclusive. It constitutes the third of these volumes, the previous issues covering respectively the years 1884–1891 and 1892–1895. The arrangement is by subject-matter, with elaborate cross-references. W.

A Laboratory Guide to the Study of Qualitative Analysis. By E. H. S. Bailey, Ph.D., Professor of Chemistry, and Hamilton P. Cady, A.B., Assistant Professor of Chemistry, in the University of Kansas. Fourth edition. 12mo, pp. 235. Published by P. Blakiston's Son & Co., 1991. (Price, in cloth, $1.25, net.)

The authors explain in their preface that they have sought in this work to teach not only the facts and the mechanical methods of carrying out the various operations of analysis, but also to render them more intelligible and interesting to the student by a proper application of the theory of Electrolytic Dissociation and of the Phase Law. A new method is presented for the separation of arsenic, antimony, and tin, and also for the separation and identification of the acids. W.

Franklin Institute.

[*Proceedings of the Stated Meeting held Wednesday, April 16, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, April 16, 1902.

President JOHN BIRKINBINE in the chair.

Present, 70 members and 18 visitors.

Additions to membership since last report, 7.

The order of business was suspended in order to permit of final action on the amendments to the By-laws presented at the stated meeting of March 19th.

After a prolonged discussion of the several proposed amendments, which occupied the major portion of the meeting, they were severally adopted by more than the required two-thirds vote of the members present.

In substance, the amendments as adopted increase the annual dues of contributing members from $8 to $15, and for fractional parts of the year proportionately; the dues of non-resident members from $2 to $5; the dues of resident life members from $100 to $200, and of non-resident life members from $40 to $75; and the annual tax on second-class stock from $6 to $12, and for fractional parts of a year proportionately.

The number of members of the Committee on Science and the Arts was increased from forty-five to sixty, of whom it is provided that twenty shall be retired each year.

The following members were elected to the committee in conformity with the amendment just referred to, viz.:

Robt. D. Kinney, Frank Roselle, D. A. Partridge, Caspar Wistar Haines, Ernest M. White, Sam'l P. Sadtler, W. N. Jennings, Henry Leffmann, Jesse Pawling, Jr., E. Goldsmith, Robt. H. Bradbury, Werner Kaufman, Clayton W. Pike, Chas. Murset, Rich'd L. Humphrey. Also, Mr. John Price Wetherill was elected to the Board of Managers to fill the vacancy caused by the death of Mr. Stacy Reeves.

Adjourned. WM. H. WAHL, *Secretary.*

COMMITTEE ON SCIENCE AND THE ARTS.

[*Abstract of proceedings of the stated meeting held Wednesday, April 2d—Adjourned meeting Friday, April 11th, 1902.*]

The following reports were adopted :

(No. 2132.) *American Roller Bearing Company's Roller Bearing.*—Myron F. Hill, Cambridge, Mass.

This subject is one of those recommended for investigation by the Jury of Awards of the National Export Exposition.

ABSTRACT.—The invention is the subject of letters-patent to Mr. Hill granted in several foreign countries.

The sub-committee made tests of the bearing as applied to vehicles, and finds as follows :

" The most noteworthy feature of these bearings is their excellent workmanship, the closeness of the gauging and the hardening of the rollers and racers. Although the principles used in this bearing are not new, the excellent workmanship and the production of a successful bearing are worthy of recognition. A Certificate of Merit is accordingly granted to Myron F. Hill, the inventor." [*Sub-Committee.*—Arthur M. Greene, Jr., Chairman.]

(No. 2181.) *Enclosed Arc Light.*—C. J. Toerring, Philadelphia.

ABSTRACT.—The lamp is of the enclosed arc type, and is intended to burn on direct-current, constant potential circuits of 110 and 220 volts.

The inventor does not disclose anything fundamentally new, but evidences careful attention to details.

Thus, the inventor uses a large inner globe, which embraces the lower carbon holder, and which, being farther away from the arc, is less liable to be broken ; also, it allows the introduction of the hand in cleaning. The design and material of the steadying resistance is a feature. This is so arranged that in case the carbons become stuck together, it will carry the current without injury.

The lamp was tested by the sub-committee, and the claim of the manufacturers of a life of over two hundred hours, with ordinary half by twelve-inch "Electra" carbons, was verified, as well as the lasting power of the resistance for over twenty-four hours. The award of a Certificate of Merit is made to the inventor. [*Sub-Committee.*—Francis Head, Chairman ; Richard L. Binder.]

(No. 2183.) *Stencil Machine.*—Andrew J. Bradley, St. Louis, Mo.

ABSTRACT.—This subject relates to a machine for cutting paper stencils used in marking addresses, shipping directions, etc., on cases, boxes, etc., and is the subject of several letters-patent to applicant. The details of the machine cannot be made intelligible without the aid of illustrations, and reference must therefore be made to the specifications filed with the report.

The committee finds that the Bradley machine is the only one on the market for doing this special class of work, and that it answers its purpose very satisfactorily, the brand being clear and easily read, avoiding the mistakes in shipments which frequently occur when hand-marking is used because of its illegibility and the difficulty usually met with by any one but an expert in forming letters legibly with a brush.

The cutting of the stencil with this machine is as quickly done as the writing of an address on an envelope with the typewriter, and the marking is done by the slight passing of a brush over the stencil on the surface to be marked. As many as 1,000 impressions have been made from the same stencil without noticeable wear or defacement. The question of time lost in hunting up old stencils is answered by the fact that it is quicker to make a new one.

The award of the John Scott Legacy Premium and Medal is recommended. [*Sub-Committee.*—J. Logan Fitts, Chairman; Samuel Sartain, Henry F. Colvin.]

(No. 2195.) *Single-phase Motor.*—Wagner Electric Company, St. Louis, Mo.

The invention is covered by letters-patent No. 389,352, September 11, 1888, to Messrs. Anthony, Jackson & Ryan, and relates to the general scheme of using a closed external circuit in connection with the armature, using a commutator on the same, curreut only being supplied to the field.

Another patent granted to Arnold, No. 543,836, August 6, 1895, introduces the idea of using the closed circuit on the commutator brushes and also a governor on the shaft, operating a short-circuiting ring on the commutator.

Other and more recent patents—Pillsbury & Schwedtmann, No. 620,609, March 20, 1899, and No. 603,709, May 10, 1898—to Schwedtmann relate respectively to the special form of governor, brush-holder and commutator arrangement and the arrangement of the field winding; the use of a special form of frame and the method of holding the stator in the frame.

The sub-committee's conclusions are as follows: The Wagner Electric Company has developed a practical single-phase motor which has given satisfactory service in commercial use.

In view of the fact that this is a distinct departure from other types of self-starting single phase motors, which either start as series motors or make use of phase-splitting device to obtain their starting torque, the Edward Longstreth Medal of Merit is granted to the manufacturers. [*Sub-Committee.*—Arthur J. Rowland, Chairman; Thomas Spencer, Carl Hering, L. F. Rondinella.]

(No. 2185.) *Process of Treating Tool Steel.*—Fred. W. Taylor and Maunsel White, Bethlehem, Pa.

This report is reserved for publication in full. The committee's conclusion is as follows: In conclusion, the sub-committee takes pleasure in recommending the award of the Elliott Cresson Medal to the inventors, for their discovery and development of a method of treating a certain composition of tool steel, which has made it possible to largely increase the output of machines doing roughing work. [*Sub-Committee.*—Charles Day, Chairman; James Christie, Wilfred Lewis, A. Falkenau.]

(No. 2205.) *Automatic Electric Brake Motor.*—Gano S. Dunn, East Orange, N. J.

ABSTRACT.—The invention is the subject of U. S. letters-patent, No. 515,-755, March 6, 1894, and No. 654,142, July 24, 1900, to applicant.

The motor is specially designed for use on bi-polar machines. The brake mechanism is of the band type, and is so arranged as to be operated in one or both directions. The motion of applying the brake-band is imparted by means of a spiral spring, which is allowed to go into action as soon as the current is thrown off the field magnet.

When the machine is at rest and the field not energized, a heavy coil spring pushes this movable pole piece away from the armature, and tightens a leather-lined band which grips a brake-wheel attached to the spider of the armature. When the current is thrown on, the magnetism of the field draws the movable pole piece inward, overcoming the force of the coil spring, and causing the brake-band to release the brake-wheel, thus leaving the armature free to rotate. Thus, it will appear that the brake is automatic in action; that the field resumes its normal position when the motor is in operation, and that no extraneous coil or other mechanism is required, the brake spring being overcome and the armature released by the static attraction of the field. The spring and brake-band are adjustable, so that the armature may be allowed a few revolutions in which to slow down before stopping, or be stopped and held firmly the instant the current is shut off; and in all cases the friction of the brake is greater than the driving power of the motor, so that a brake motor is able to hold up any load which it is able to hoist.

The report proceeds then to describe machines of the reversible type embodying the same general principles of construction, and gives the results of the committee's examination of a number of motors equipped with this brake, which were at the Pencoyd Iron Works.

The report concludes that the device is ingenious, that the details have been carefully worked out, and that this motor fills satisfactorily a certain field of operation. A Certificate of Merit is awarded to the inventor. [*Sub-Committee.*—Messrs. C. L. Eglin, Chairman; Clayton W. Pike, Arthur J. Rowland, C. J. Reed.]

(No. 2209.) *Synchronism Indicator.* Paul M. Lincoln, Niagara Falls, N. Y.

ABSTRACT.—Letters-patent covering this device are granted to applicant, No. 685,155, October 22, 1901. The investigating committee had the opportunity of examining its operation at the power-house of the Philadelphia Electric Company.

The purpose for which this instrument is used is to synchronize two or more alternating current generators which it is desired to connect in parallel. It indicates the phase and frequency-relation of two separate sources of electro-motive forces. It also indicates which of the generators is operating at the higher frequency and what the difference is.

[For a full description of the instrument, with illustrations, the reader is referred to the paper of Mr. Lincoln, read before the Electrical Section of the Institute, and published in the impression of the JOURNAL for April, 1902.]

The report continues that the only instruments available for the purpose prior to the invention of this device were either lamps or a voltmeter. Lamps were so arranged as to either be extinguished or be up to full candle-power when the two machines were in synchronism. The possibility of error is very great with the use of incandescent lamps, as there is a wide range of voltage from the point of unity until the filament commences to glow, which gives no indication to the operator of the difference of voltage or of the phase-relation of the two machines about to be connected. With the use of the voltmeter this difficulty is overcome, although it does not indicate which of the machines is running faster.

The committee finds that the instrument fully meets all the requirements of actual practice and greatly facilitates the connecting of alternating current machines in parallel. Also, that the instrument has been carefully designed and arranged in a practical form, and that it is a meritorious and ingenious invention. The report recommends the award of the John Scott Legacy Premium and Medal to the inventor. [*Sub-Committee.*—Messrs. C. L. Eglin, Chairman; Coleman Sellers, Thomas Spencer.]

(No. 2213.) *Nutrium.* A New Food Preparation. National Nutrient Co.

The product referred to is a dry, white powder, consisting of the solids of separated skimmed milk, obtained, according to the statements furnished to the committee, by evaporating the watery portion at a low temperature.

The committee awards a Certificate of Merit in the following terms:

"For the preparation of separated skimmed milk in a dry and finely pulverulent state so as to obtain all the solid ingredients in a permanent form without the use of preservatives." [*Sub-Committee.*—Henry Leffmann, Chairman; Bayard H. Morrison, Sarah Tyson Rorer, Frederick A. Genth, Jr.]

(No. 2215.) *Developing Tray for Photographic Negatives.* A. B. Sheppard, Burgettstown, Pa.

(An advisory report.)

(No. 2200.) *Method of Telephoning Over Electric Light and Power Circuits.* Frederick Bedell, Ithaca, N. Y.

(An advisory report.)

WM. H. WAHL,
Secretary.

SECTIONS.

(Abstracts of Proceedings.)

PHYSICAL SECTION.—Proceedings of the meeting, Wednesday, March 26. Dr. Mackenzie in the chair.

The paper of the evening was read by Dr. Albert P. Wills, on "Recent Work in Magneto-Striction." The paper was illustrated by lantern slides. Discussed by Dr. Lloyd, Dr. Goldsmith, Dr. Mackenzie and Mr. Pawling.

This was followed by a paper on "The Hearings of the Committee on Coinage, Weights and Measures on the Metric System," by Jesse Pawling, Jr. The following motion, offered by Mr. Pawling and duly seconded, was adopted:

To appoint a committee of the Physical Section to formally call the attention of other scientific, technical and manufacturing bodies to the action of the Franklin Institute, and to request that the subject be taken up for consideration, and to obtain opinions from individuals prominently identified with educational, scientific, engineering, technical, manufacturing and commercial work, as to the desirability of such legislation; and to report to the Physical Section of the Franklin Institute at its next meeting what other steps might be taken with propriety to further the proposed metrological reform. Discussed by Dr. Lloyd, Dr. Goldsmith and Dr. Mackenzie.

The President appointed on the above-named committee Jesse Pawling, Jr., Chairman; Dr. Geo. F. Stradling and Dr. A. E. Kennelly.

The thanks of the Section were tendered to Dr. Wills for his paper.

Adjourned.

JESSE PAWLING, JR., *Secretary.*

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Stated Meeting*, held Thursday Evening, April 3, 1902. President, Dr. Henry Leffmann in the chair. Present, 17 members and visitors.

Mr. Lewis Woolman read the paper of the evening, entitled "Microscopic Organisms in the Determination of Geologic Age." Mr. Woolman exhibited a large number of lantern slides, illustrating the subject, and also a number of mounted specimens which were projected on the screen with the lantern microscope. These were also shown under the table microscope. The speaker also presented a number of photographic illustrations of some features of the subject.

Mr. F. E. Ives exhibited a number of photo-micrographs that he had made of diatomes and other test objects.

The chairman presented to Mr. Woolman the thanks of the meeting for his interesting communication. Adjourned.

MARTIN I. WILBERT,
Secretary.

MINING AND METALLURGICAL SECTION, ELECTRICAL SECTION.—*Joint Meeting*, held Thursday Evening, April 10th. Mr. A. M. Greene, Jr., in the chair. Present, 104 members and visitors.

Mr. F. W. Taylor, late of Bethlehem Iron Works, presented a paper on "Shop Management," in which were outlined the piece-work, premium and bonus systems of paying for labor, and the methods necessary to obtain "fair pay for a fair day's work," with satisfaction to the workingman and a just profit to the employer. The speaker emphasized the fact that, with proper management, high wages to first-class men means increased production and decreased cost.

The speaker gave some pointed illustrations of these facts by figures obtained from current practice at the Bethlehem Iron Works.

The paper was discussed by Messrs. Cyrus Chambers, Jr., Arthur Falkenau, Wilfred Lewis, Spencer Fullerton, A. M. Greene, Jr., and others. Adjourned.

D. EPPELSHEIMER, JR.,
Secretary.

JOURNAL

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OF THE STATE OF PENNSYLVANIA,

FOR THE PROMOTION OF THE MECHANIC ARTS.

VOL. CLIII, No. 6. 77TH YEAR. JUNE, 1902

THE FRANKLIN INSTITUTE.

Stated Meeting, held Wednesday, February 19, 1902.

President JOHN BIRKINBINE in the chair.

THE METRIC SYSTEM OF WEIGHTS AND MEASURES.

DISCUSSION.

THE CHAIRMAN:—There is now in order a discussion of the Report of the Special Committee on the Metric System. That is not a new subject before the Franklin Institute, nor wanting in interest to a body devoted to the mechanical arts. It is a subject which the Institute has taken cognizance of a number of times, and of which we must now take cognizance, remembering that any statement we make, as the Franklin Institute, goes forth with whatever influence we have behind it, for that purpose. When the matter was presented at a meeting several months ago, it was referred to a committee selected with care and with the view of having all the Sections represented. The committee has worked well and made a report which you have all seen and which is here for your approval or disapproval.

Supplementing the work of the committee, the Secretary addressed a number of letters to parties—members of the Institute and others, whose opinions on the subject it was thought desirable to obtain. Whether the Institute is prepared to vote on the matter to-night is a matter for you to decide, but it is certainly desirable to discuss the subject as thoroughly as we can. The report will now be read by the Secretary.

REPORT OF THE SPECIAL COMMITTEE APPOINTED AT THE STATED MEETING OF NOVEMBER 20, 1901, TO REPORT ON THE FEASIBILITY AND ADVISABILITY OF THE ADOPTION OF THE METRIC SYSTEM OF WEIGHTS AND MEASURES IN THE UNITED STATES.

WHEREAS, It is desirable to obtain an international standard of weights and measures, also to simplify and regulate some of our existing standards; and

WHEREAS, The metric system is commendable not only as a suitable international standard, but also for facility of computation, convenience in memorizing and simplicity of enumeration;

Resolved, That the Franklin Institute approves of any movement which will promote the universal introduction of the metric system with the least confusion and expense.

Resolved, That the National Government should enact such laws as will ensure the adoption of the metric system of weights and measures as the sole standard in its various Departments as rapidly as may be consistent with the public service.

JAMES CHRISTIE,
American Bridge Company, Pencoyd, Pa.

A. E. KENNELLY,
Houston & Kennelly, Electrical Engineers.

F. E. IVES,
Photographer and Engraver.

WILFRED LEWIS,
President Tabor Machine Company.

S. M. VAUCLAIN,
Superintendent Baldwin Locomotive Works.

JESSE PAWLING, JR.,
Instructor in Physics, Central High School.

GEORGE F. STRADLING,
Instructor in Physics, N. E. Manual Training School.

HARRY F. KELLER,
Professor of Chemistry, Central High School.

A. FALKENAU,
Mechanical Engineer and Machinist.

L. F. RONDINELLA,
Instructor in Engineering, Central Manual Training School.

APPENDIX.

Questions discussed at meeting of Sub-Committee, January 17, 1902.

Questions.	Answers Agreed upon.
(1) Assuming the desirability of an international standard, could we expect nations using the metric system to abandon that and adopt our system?	No.
(2) Can we not concede the advantages of the metric system for purposes of computation, and also as being readily memorized and the relations between weights and measures borne in mind without much effort.	Yes.
(3) Have any valid objections against the metric system been effectively urged, excepting that the numeration cannot be continuously subdivided by two?	No.
(4) Is not this similar objection to our decimal currency overcome by the advantages of the system?	Yes.
(5) For convenient minimum units of hand rules, is not the mm. better than either ^{1} ″ or $\frac{1}{32}$″; the latter being rather a fine subdivision for ordinary rough measurements?	The mm. is equally as convenient.
(6) Assuming that the change in our system could be effected, without serious expense or confusion, could we recommend this change as desirable?	Yes.
(7) Could not such a change be fairly initiated if the National Government would adopt the system in all its Departments, where no serious confusion would occur from an early change, gradually extending the system to other Departments, when people became accustomed to its use, and tools were accumulated which conformed to the new standard?	Yes.
(8) In the workshops, could not a large proportion of existing tools and gauges be retained until they were gradually superseded, merely designating their nominal dimensions in the nearest convenient metric units?	We anticipate no prolonged serious confusion.
(9) If in the course of a term of years, the system came into universal use in the service of the Government, is it probable that its adoption would follow elsewhere within a reasonable time?	Yes.
(10) Would it appear to be practicable to inaugurate the adoption of the metric standards for weights or for liquid measures, in advance of linear measures, as the former would not involve the abandonment of such numerous and costly tools as would the latter?	No.

The Chairman:—I shall ask Mr. George M. Bond, of the Pratt & Whitney Company, of Hartford, Conn., to open the discussion.

Mr. Bond:—It is with a good deal of diffidence that I undertake to make the remarks that are called for, as I had expected to hear the discussion in advance, so that I could have something to refer to that would be, perhaps, to the point; but I would say, in a preliminary way, that it was only lately that I attended a hearing before the Committee on Coinage, Weights and Measures, in Washington. I was somewhat surprised to find how few were present at the meeting, which I presumed was to afford the representative manufacturers of the country the opportunity of expressing their views on the suitableness or the practicability of introducing compulsory measures looking toward the adoption of the metric system. I stated to the Committee at the time that while I was fully aware of the advantages of the metric system in its application for computation and scientific purposes, I was not aware of its great convenience in ordinary workshop practice. I have had some experience in linear work, not in weights or in liquid or dry measures; so that I am not prepared to say anything in regard to either of the latter at this time; but I told the Committee that I considered the English units, such as are adopted and have been used for years—the inch, and yard, and foot—as tending more to convenience and to economy than would be the case with the subdivisions or the units as represented by the millimeter or the meter. The millimeter is too small for the purposes of the workshop, in that it has to be multiplied many times if decimals are to be avoided and if all the other subdivisions of the meter are to be avoided. If the meter is the unit, then, of course, the decimal point would have to be used.

It is a serious matter to all concerned that the meter was adopted of the length that it was, in terms of the quadrant of the earth's surface, which we all know was a mismeasurement; but if it had been correctly determined, or better still, if it had been made commensurate with the inch, so as to be 40 inches long, then this change which is now advocated would be a comparatively simple matter where the translation has to be carried from one to the other. We all know that it is impossible for certain kinds of work, without carrying out the relation to a number of decimals, to make this interchange or translation within the limits of necessary accuracy.

It has also occurred to me at this time, and at this meeting, to ask if it had been considered what changes are going to be required in the Army and Navy Departments. That clause in the House of Representatives' bill covers the almost immediate adoption of the metric system by the Government in all its Departments. The Navy Department has, as we all know, adopted the Franklin Institute or Sellers thread for bolts and nuts. That system enters into all construction by the Navy Department and has been used for a great many years. It was on the recommendation of the Franklin Institute that this thread was adopted by the Department, and the change to the metric system for the Navy Department in that one item alone would be a serious matter. It would be almost impossible to make the change in even units so as to make the work as interchangeable as it is now required. Another thing, the railroads of the country have adopted the United States standard thread, and it is now in use on every railroad of the country; and it is really the officially recognized standard of all railroads by the action of the Master Car Builders' and the Railway Master Mechanics' Associations.

These standards are all in English measure, and the pitches are English; so that there would have to be a translation, which means a good deal of trouble and expense. Not only drawings must be made in the metric system, but also all specifications. It is not possible to make these changes in the units without changing standards that have been in service a great many years. I understand that at the next hearing of the Committee at Washington representatives of the Army and Navy Department will be called in, and perhaps a number of manufacturers representing the largest industries of the country. It will be of a good deal of interest to know what will be their views on the subject. I can only speak for our own company, of course; and I know that this change is deprecated, and we would hesitate a long time before we would undertake to make it. Of course, if made compulsory by law it would be a necessary measure. The law as already passed by Congress legalizes the use of the metric system, and that, of course, is fair to those whose business requires the use of it and who find it more convenient; it is perfectly right that they should use it; and I have long held and expressed the opinion that the metric system has its advantages and I appreciate them; that they are specially adapted for computation, for calculation; but I do not think, from the experience that we have had and that I have had, especially in certain lines of work, that it would be of the great advantage that is claimed for it.

I have not seen the report or any account of the testimony of representatives before the Committee on Coinage, Weights and Measures, nor do I know the manufacturers who have appeared. I understood at the time of my attendance that it was to be a meeting called for manufacturers, and I was surprised to find myself the only one, except one gentleman who represented a very important industry—that of scientific and optical instruments of the very highest order; and I appreciate and I fully understand that the metric system in his work is absolutely necessary for the reason that his work is entirely with the scientific public, and, of course, scientific persons find the use of the metric system of advantage. That goes without saying; we do not argue that case at all; but to have the law framed as the bill certainly reads at this time (there is no other interpretation of it, so far as I can judge), means the compulsory adoption of the metric system after a certain date. It is very specifically stated so for all the Departments of the Government. That is the point I want to bear upon more particularly to-night, for the reason that most large manufacturing concerns (our own included) have a great deal of business with the Government, and we must know whether it will be necessary that our contracts be specified in the metric system—all machines, drawings, and all of the specifications made in the metric system after a certain date. That, of course, will be a ruling which will be necessary if the bill is passed, and if this had to be done it would mean considerable expense and trouble.

As far as concerns the clause relating to compulsory use otherwise than in the Government Departments, that is not so clearly stated; but it implies compulsory adoption in about two and a half years. That is the way it is interpreted by persons who are quite familiar with the text as represented by the bill, and it certainly will not be thus accepted and allowed to pass without a protest which will be entered by many manufacturers. It may not be by a majority, but there will be some who will demand to be heard in the matter.

THE CHAIRMAN:—The Chair wishes to apologize to the Chairman of the Committee, Mr. Christie, for not calling first upon him to speak for the report, but under the circumstances as he has been informed that Mr. Bond had to leave by an early train, he felt that the Institute would excuse that omission, and he trusts also that Mr. Christie will do so. Before asking Mr. Christie to continue the discussion, I shall ask Dr. Wahl merely to read the names of those who have contributed to the discussion by correspondence, because it may be that you may wish to call for the reading of some of these letters. After the names have been read I shall give Mr. Christie the floor.

THE SECRETARY:—This correspondence has accumulated very rapidly, and a large portion of it came to my hand only yesterday and to-day, and I have had opportunity only to arrange it into two classes: one favoring the report, and the other either non-committal or unfavorable. [Reads names of correspondents.]

MR. JAMES CHRISTIE:—In presenting for your consideration the report of the Committee on the Metric System, it is perhaps unnecessary for me to assure you that the results of the deliberations of the committee were not derived hastily, or without due thought and consideration being given to the subject.

It is a serious matter to alter our old and well-established systems of weights and measures, and such a change could not be justified unless the advantages to be gained were of such profound importance as to warrant it. We are required to consider the subject in all its bearings and its manifold relations to the needs of every element in the community. The fact that our present systems, especially as they apply to weights and measures of capacity, are inconsistent and incongruous, is not alone sufficient reason to entirely abandon them. If this alone was the only objection, we could readily remove the imperfections, whilst retaining the fundamental units, but we are required to consider not only internal economy and convenience, but also international relations with the rest of the world.

The metric system of weights and measures has outlived the century, and a review of its extension and growth indicates that it is gradually but surely superseding the ancient systems throughout the civilized world. The English-speaking people so far have not accepted it in their commercial methods. Nevertheless, we find the system is intrenching itself both here and in Great Britain, and whilst it has not yet had any popular recognition here, it is probably only a question of time before we will realize that we have in existence a dual system of weights and measures.

It will be observed that hitherto the most earnest advocates for the adoption of this system have been found amongst professional or scientific men, or those who are required to make extensive or laborious computations. On the other hand, manufacturers and merchants have either been indifferent or been opposed to a change. The reasons for this are self-evident. The chief advantages of the metric system lie in the correlation existing between its fundamental units. This harmony of relations tends to facilitate computations and largely reduces the strain on the memory in arithmetical calculations. Evidence obtained from those who are constantly required to make extensive computations by both methods, indicates that a large amount of time would be saved for computers if the computations were based upon the metric system instead of our own existing system. It is probably no exaggeration to state that one-half the time of the average computer would be saved in technical and commercial calculations, if made according to metric units. The saving in the time of the computer is not the only economy, but we should consider also the facility with which one can retain in memory the fundamental elements of the system. It is probable that the large majority never attempt to memorize entirely our own system of weights and measures, but keep hand-books convenient for reference. On the other hand, as a personal experience, although I rarely have to use the metric system, when I do apply it I find I have a clearer comprehension of it in my mind than I have with many of our own tables of weights and measures. It is probable that a very considerable proportion of the strain on the school child would be relieved if it dealt with metric units in the study of arithmetic and its applications.

If we had to consider the subject solely from the workshop point of view, for its convenience in linear measurements, etc., I would then say we need no change at all. I can see no reason why we cannot use our inch or foot just as well as we could use any subdivision of the meter. A disadvantage of any decimal system arises from the fact that it cannot be continuously subdivided in halves; but this is only one point of view, and in all such cases we have to balance advantages against disadvantages. It is frequently urged that the popular mind does not instantly or readily grasp a decimal subdivision. This is largely a habit of thought, and our experience with our decimal currency tends to disprove it. I am sure that the conceptions of 25, 50 or 75 cents are not more readily grasped by the ordinary mind if we designate these sums as ¼, ½ or ¾ of a dollar.

The objections sometimes urged against the metric system that its units are inconvenient for practical application, that the meter is too large and the millimeter too small a unit, are not conceded by those who habitually use this system only. On the contrary, it is claimed that the millimeter is a more convenient minimum unit for the ordinary everyday service than either our one-sixteenth or one-thirty-second of an inch; the former being too large and the latter too small for convenience on hand scales. Any decimal multiple of the minimum unit, varying from the millimeter to the myriameter, can be used as the base unit, according to the magnitude of the measurement involved, and these different units are readily interconvertible by a simple mental process or by the transposition of a decimal point, in marked contrast to the inconvenience found in converting our linear units of inches, feet, yards, fathoms, rods, chains, furlongs, miles, and several other units of length, which are all more or less used in special applications.

Of late years we find many strenuous advocates of the metric system in the ranks of our manufacturers, mechanics and merchants. I think this spread of opinion arises from two causes: first, the great increase in late years of our interchange of products with foreign nations that are now using the metric system, and hence the growing realization of the necessity for an international system of weights and measures. Secondly, the large amount of computation connected with all business enterprises in modern times. The amount of computation that is now made in connection with manufacturing enterprise is much greater in proportion to the product than it was at any former time in the world's history. This is owing to all business being conducted on a more scientific basis than heretofore, and the amount of specialization is vastly increased. Take our steel works, for example; men still in active life recall the time when no chemists were employed in connection with our steel industries, and but few, if any, technically trained engineers. Results were arrived at by shrewd, skilful men, trained to habits of close observation, and the work that was accomplished was wonderful, considering the limited precedents that guided them. These men, like the architects of old, "builded better than they knew." Now we find our workshops and offices filled with men who have been thoroughly trained in the applied sciences. The work they perform is the result of laborious calculations, from which the elements of chance are eliminated as far as possible. To these men any system of metrology is a great desideratum, which results in a saving of time or needless labor ; and in the ranks of these you will find the strongest advocates for the adoption of the metric system.

The strongest objection that is usually raised against such an innovation as herewith proposed, is the confusion and large expense involved in the change. It is true we will have to endure some temporary confusion, but the amount of expense necessarily connected with the change is probably much over-estimated. It is usually assumed that if we are called upon to apply the metric system of measures in our manufacturing establishments, the immediate result would be the replacement of large quantities of tools, especially of gauges and measuring-devices that are made to conform to these gauges. The fact of the matter is, to the workman the unit of measurement, so far as fine subdivisions are concerned, is a nominal distinction. He works from gauges the dimensions of which originate in the tool-room. So far as rough measurements are concerned and where the hand-rule is used, it can make but little difference to the workman what unit of measurement is adopted. He can soon readjust his thoughts and acts to suit any system. If called upon to adopt the metric system in any establishment under my control, I would make no immediate change in any of the tools, but use them as they are, merely denominating them in the nearest convenient metric unit. For example, call an inch 25 millimeters, and so on with any multiple or subdivision of the inch. It would be urged against this that there is no correspondence between the absolute and nominal dimensions of the tool. This, although very desirable, is not absolutely essential ; in fact, in many of our tools, the dimensions are only nominal and not exactly real. It would be desirable, however, to begin to make new gauges and tools to replace the old ones as they were worn out. These could be made of actual dimensions to correspond closely with the nominal. The consequence would be that workshops would have for a period two standards, the old and the new ; the old applying to all existing products and designs, and the new tools to be applied to new designs and products. The former would gradually disappear and the latter would in time entirely take their place. The manufacturer simply would maintain a double standard—all named in metric units—the one gradually disappearing to be replaced eventually and entirely by the new standard. Many shops at present have two or more standards of dimensions in service, between which they have to preserve distinction, and it would simply require some extra care to prevent confusion or error.

The subject of screw-threads is also frequently quoted and the difficulty and confusion that would result if we were to attempt to change our present system. I may ask, in reply, why change it at all? If it is good, retain it as it is indefinitely. The pitches of screw-threads are entirely arbitrary and the difference could be denominated by the letters of the alphabet or by any other distinction that is desired. The workmen produce these screw-threads by the change gears of their lathes, or by special tools that are furnished them for the purpose, and it is a matter of indifference to them from what unit of measure they are derived. If in the course of time a revision of the system of screw-threads should be desirable, they might then, as a matter of consistency, be based upon metric units; but until such a contingency occurs, there can be no object in making any change.

The opponents of the metric system frequently attempt to stifle it by ridicule. When the Stone Bill was before Congress, a few years ago, a rather humorous speech was made by one of the members that caused considerable amusement, and it was claimed that it had some effect in bringing votes to the opposition. A system, however, that has endured for a century and received as wide acceptance as this, cannot be silenced by an epigram or disposed of with a sneer.

The foreign sound to the names is objected to. If the meter or the kilogram or the liter are not considered euphonious or acceptable, I am sure it is easy to translate them into desirable terms. "A rose by any other name would smell as sweet;" remembering, however, that terms or expressions that are uniform in all languages would facilitate very much international understanding and convenience.

An article published by a noted mechanical engineer of New York attempts a similar form of argument by imagining a child sent to the grocery to purchase a kilogram and 850 grams of cheese, and questioning the effect of such a demand upon the parents and the grocer. That was simply trifling with the subject. If we stop for a moment and reflect that a gram is a very fine subdivision of weight, more strictly comparable to the grain than to the ounce, wouldn't it be equally absurd for a child so instructed to ask the grocer for a pound, 10 ounces, 6 drachms and 20 grains of cheese; or if in search of some commodity the druggist dealt in, to ask the latter for a pound, 6 ounces, 5 drachms, 2 scruples and 6 grains of his commodity? Would not the merchant be at least as much astonished by the latter requests as the former in the metric measures? Arguments of that kind are very funny, but not logical. If the metric system were in vogue for the ordinary purposes of life, we would do just as the French and Germans do. We would speak of the halves and quarters of the meter or the kilogram or the liter, just as we do with our decimal coinage, without any inconsistency.

The gentleman who preceded me criticised the millimeter as being too small a unit for popular convenience on hand rules. This doubtless depends upon the point of view. To many others the millimeter appears to be a very convenient minimum unit. Our ordinary rules are usually divided into sixteenths of an inch, which is too large a minimum unit, and the half of this, or the thirty-second, is rather too fine a subdivision for ordinary rules. From a broader point of view we might state that if the matter of the units of subdivisions on rules, or facility of measurements, or convenience of workshops, were the only questions involved, we do not need to make any change, our present system is good enough. But we must look further; we must consider the army of men who make our computations, burdened as they are with our incongruous systems. Also the convenience and economy of those who are engaged in the world's commerce. We desire to facilitate international trade relations and remove all the barriers that we can with consistency, to permit international exchanges. If an international system is to be sought we surely cannot expect those who have discarded their ancient systems and adopted the metric standard to drop this and take up our heterogeneous methods. By reading the report of the committee you will observe that no compulsory measures are proposed or suggested. It is not probable that any system could be forced upon the community, against the popular desire, by any legislative enactment, and yet the very nature of the case implies that to make it effective it must be adopted by all; there can be no half-way measures in connection with any system of units, in which the members of the community have to deal with each other, any more than we could have several languages co-existent as a national tongue. It is quite possible, however, for the National Government, which is the largest consumer of the country, to put it in force in its own Departments; it exists already in some of these Departments. All that is necessary is to enforce it in others as rapidly as possible, without serious expense or confusion to the interests involved. If the National Government would do this, the great army of manufacturers who are supplying the national wants would necessarily adopt the system in their relations with the Government. This would surely lead to its adoption in our relations with each other. The movement already has a strong following in Great Britain, and it is not improbable that the British may precede us in its adoption, and when the system comes into popular use, the only wonder will be that we so long delayed in accepting so convenient and time-saving a system. (Applause.)

MR. SPENCER FULLERTON:—In the case of the adoption of these measures, would the sizes of articles in general use be changed? I mean by that, for instance, in your business, Mr. Christie—you make I-beams and channel irons—would the dimensions of those products be changed to the nearest equivalent in the metric system? For instance, a 4-inch channel might actually be altered to measure $3\frac{8}{10}$, or whatever the fraction was, to agree with the decimal and points of the metric measure; and would the whole range of sizes of our rod and bar irons be altered?

MR. CHRISTIE:—I do not see any reason why there should be any change at all. I think we should simply take them as they are and give them the nearest metrical measurement. I think in time we would adopt new sections. That would come in time; but it would cost something to do it. We are making new sections and new tools all the time.

Why should we change our screw-threads because of adopting the metric system? There is not a nation in the world but what uses the United States standard screw-thread. A system of metric screw-threads has been proposed and may be adopted; but the reason it has not been adopted is because there is no necessity for it. We can govern that and maintain our own, and keep to the present standard. We do not need to change the present standard of screw-threads.

MR. JESSE PAWLING, JR.:—I beg leave to make a statement. I was present with Mr. Bond at the meeting of the Committee on Coinage, Weights and Measures, and I can affirm positively, from remarks made by Mr. Southard * and Mr. Shafroth † after the hearing, and also from conversation with Dr. Stratton, the Director of the National Bureau of Standards, that it is not the intention of the framers of the Bill to force the metric system upon the country; it is not their intention to make this system compulsory; and, whether this bill is passed or not, the English system can be used in any machine shop as long as the owners see fit. And I beg leave to state another point: At that meeting of the Committee on Coinage, Weights and Measures a strong effort was made to obtain from those who were interested in machine tools an answer to the question: where would the expense be incurred in the change? After considerable questioning, it was brought out that such tools as taps, dies, reamers, etc., would need to be changed. This question was asked by Dr. Stratton, who is familiar with machine tools, as well as being a scientific man: If the metric system were made compulsory, what change would be required to be made in a shaper, for example? It was answered that no change need be made in floor tools.

THE CHAIRMAN:—The Institute would be glad if Dr. Houston would participate in the discussion.

DR. EDWIN J. HOUSTON:—Mr. President and Gentlemen, I came rather to listen than to talk. This proposition appeals to me as a scientific man, and from that viewpoint there can be no question as to how I would vote if I had to vote; that would be for the adoption of the

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* Hon. J. H. Southard, Chairman of the Committee on Coinage, Weights and Measures, of the House of Representatives.

† Hon. J. F. Shafroth, who introduced the bill, H. R., 123.

metric system. We have passed the time, I think, in this country when we can gravely continue to announce to the world that our unit of length is based on the length or the space occupied by three barleycorns placed side by side. There might have been a time in the world's history when three average barleycorns grown in a limited region could be got that together would measure an inch with approximate exactness, or thirty-six of those barleycorns would equal 1 foot; but that time has long since passed. Of course, as a scientific man, I cannot but feel that we ought to adopt the metric system. As an electrician, with the practical units of electricity being metric units, there can be no doubt as to the advantage that would accrue by the adoption of the metric system. When we change our lineal dimensions to areas and volumes, or to squares and cubes, then the disadvantages of our system become more and more apparent, and I should hope to see some movement taken by the Institute to urge upon Congress the adoption of the metric system.

I can readily understand, however, the position which Mr. Bond, the representative of the Pratt & Whitney Company, took that there would be a difficulty in carrying out this change if it were made too suddenly, and I can even appreciate the difficulty that he and other men might experience in endeavoring to carry out practically those large decimals of small units of length. Still, the advantages of the metric system are so great that I do not think that any one or two classes in the community should expect the great body of the community—the working scientific men, the practical scientific men of the world and the public at large, let alone the coming generations—to continue indefinitely in the use of the old system. The chairman of the committee has pathetically alluded to the additional difficulty to the work of the schoolboys—he even included the girls—and I quite agree with him that we ought to have a less barbarous system of units of length, weight and measure generally; therefore, I am heartily in favor of the proposed change; but I think that any change that is to be made should not be made instantly obligatory on the entire community, but that a reasonable time should be allowed for its adoption, so that great shops could gradually change their patterns, pitch of screws, standards, etc. On the whole, I am decidedly in favor of the change recommended by the committee.

THE CHAIRMAN:—The Chair was not without guile in selecting this committee, because he felt that, just as Dr. Houston says, the technical men would favor the metrical system. On the other hand, he had reason to believe that if there was a weak point in that system, it would be found out by the practical men who have learned their trade at the bench and have handled the tools and who had to deal with them; and if you noted the composition of the committee, you will observe that it is very evenly divided between the two classes. Mr. Christie happens to represent a company that has not only built bridges for this country, but we have all been glad and proud of the fact that they have gone clear over into Egypt and done notable work there. We might go even further, because the Baldwin Locomotive Works have sent their products all over the world, and I think the members will agree with me that Mr. Vauclain, superintendent of those works, is perfectly competent to speak of the merits and demerits of the metrical system.

MR. S. M. VAUCLAIN:—I was unfortunate enough to be selected as a member of this committee. The committee found, when they assembled together, that they were practically all of one mind, although they took several ways of expressing themselves. You will notice in the appendix to the report of the committee—No. 9—we say, if in the course of a term of years the system came into universal use in the service of the Government, it is probable that its adoption would follow elsewhere within a reasonable time. This paragraph made it possible for all the members of the committee to unite in signing the report, and the fourth paragraph of the report, the second resolution, "That the National Government should enact such laws as will ensure the adoption of the metric system of weights and measures as the sole standard in its various departments as rapidly as may be consistent with the public service," will make it apparent to you that this committee was a unit against any compulsory measure being enacted by Congress to enforce the metric system upon the manufacturers of this country.

So far as the metric system is concerned from a manufacturer's standpoint, it certainly should have no terrors. Where—in what workshop—can you find a dozen men who will measure the same piece of work and find the same result with the ordinary 2-foot rule, or such scales as are ordinarily provided for their use? Could any manufacturer in America to-day rely upon the accuracy of the measurement of its employés in its product? Instead of having first-class fits and interchangeability he would have first-class misfits and ruination of his trade.

We have a moderately large workshop, employing some 11,500 men; our work is very much in detail; the first-class locomotive of to-day requires 13,000 separate pieces, all accurately made, before that locomotive can be turned out on the track as a finished product; and if we stop to consider that five finished locomotives of this kind are turned out every working day in the year, it can readily be understood how poorly these locomotives would be fitted together if we relied upon each and every one of these 11,500 men to do the measuring necessary to fit these parts together with the drawings furnished by the draftsmen in their hands.

What is the natural procedure, then, in a workshop of this kind? You receive the drawings from the drawing-room; they are all made to, we will say, the English measures—12 inches to the foot, 3 feet to the yard, or whatever you please—no matter how you may see fit to speak of it; but really and truly these drawings are not made to the ordinary English measure; they are made to a scale which is adopted, and which represents 12 inches to the foot, or 3 feet to the yard, or so many sixteenths inches to an inch. The scale that we have adopted in our drafting-room is a scale of 2 inches to the foot, and in comparing everything that we look at, we do not consider the foot at all; but if it is 2 inches long it is a foot long.

Therefore, is it not apparent to any one here that if these designs are made according to the metric system, it would be just as easy for us to judge of the size of an article by the dimension on the metric drawing as it would be upon the drawing that is made to conform to our present system of measurement? Granting that that is the case, and the drawing is placed in the shop, the holes are marked the nominal diameter; we will have an inch hole marked upon the drawing, one-inch-and-an-eighth hole, or an inch-and-a-quarter hole. Now, do we put inch-and-a-quarter holes or inch-and-an-eighth holes in this work? Not at all; the holes are almost any size. These sizes are governed by a set of standards which is adopted by the workshop for the bolts that are to fit in those holes; and in order to get a tight-fitting bolt we are not foolish enough to undertake to fit a straight bolt in a straight hole where several straight holes come one above another in several pieces and expect to get a good fit all the way through; we adopt a taper bolt the nominal diameter of which is one inch, or one and an eighth, or one and a quarter; and when the hole is drilled it is drilled the nearest size to the smallest part of the reamer which is to pass through that particular piece; and then the pieces are reamed to a set of standard gauges and never measured at all. I doubt, if you would go through a workshop containing three or four hundred men, whether you could find a dozen first-class two-foot rules in the establishment, and a steel scale has grown to be a curiosity. Of course, a steel scale is necessary; but it should be in the standardizing department. Where these scales are compared and seen to be of the proper size, and where all the calipering-gauges for turning work to proper diameters are made and corrected, the use of a measuring stick (and a two-foot rule is nothing more than an ordinary measuring stick) is merely to approximate the diameter. The pair of calipers will be set probably within a sixteenth of an inch for the rough cut; but the finishing cut is always calipered or measured with a positive gauge that is furnished by the measuring department or those who we suppose know how to measure.

Some few years ago I was not inclined to favor the adoption of the metric system, and I cannot say to-day that I am in favor of it. We can get along with the ordinary foot. It doesn't make any difference, particularly, how long a foot is, so that everybody uses a foot that long.

We could just as well get along with the meter. It makes no difference whether the meter has a positive length; that can be determined accurately by scientific measurement. The meter might just as well be 40 inches long, or it might be 36 inches long; it don't make any difference so that that meter will be divided by a decimal system. We think that it would probably be much more convenient than the system we have now of dividing up the foot, which we now use; and, feeling that way about it and the adaptability of any system to the workshop, I really have no objection to the adoption of the meter.

It resolves itself into this: that in the scientific world of to-day—a world that we did not enjoy thirty or forty years ago—those of us who have lived more than fifty years will remember when we were boys and going to school we did not have and we were not compelled to learn those things which the average boy is compelled to learn now before he can start out into the world to undertake to earn a living; we had very little electricity—very little, indeed; we were just commencing to know something about it; and when you come to talk about telephones, about the economical consumption of steam, and a dozen other very important matters that are absorbing the public to-day, you will wonder what we did have to study when we were boys.

Furthermore, it seems to me that we can look upon this matter somewhat in this light: if we were to go to Germany—any one of us—to start a workshop, we would not undertake to start it on an English-speaking basis; we would learn the German language, and we would do our business in German. Now, if we are going to adopt the metric system, all we will have to do will be to learn to think in the metric system. You can go in this city to-day to a school of languages and in a few weeks learn to speak almost any foreign language sufficiently well to enable you to get along. I have known men who have been able to master enough of the German language in three weeks to get along splendidly in Germany. How is this done? This is done simply by the professor or teacher compelling the party or person who desires to acquire a knowledge of the language, to stop thinking in his own language and to think in the language that he is going to adopt, or temporarily adopt. When he is compelled to think in that language, it is a very easy matter to teach him to speak in that language; and therefore, instead of making our articles 4 or 3 feet long, we would make them a meter long, or a meter and a half long; everything that we would make we would think about in accordance with metric sizes and metric dimensions.

The gentleman from the Pratt & Whitney Company did not enlighten us fully or sufficiently well to give us an opportunity to give proper weight to his remarks. He failed to tell us how expensive the change would be. He said that it would be expensive; he said that it would be troublesome, but did not tell us how expensive or how troublesome. When a change of this kind would commence in any manufacturing establishment, it would first commence in the drawing-room (because unless the drawings were made in accordance with the metric system, the men in the shop could never work to it), and there would be very few gauges in use in the shop that would ever have to be changed, because the gauges do not depend upon the figured dimensions on the drawings ; the drawings would all be figured for the gauges. A certain gauge would be called for instead of a certain dimension. In our works to-day there is not a single hole drilled in a connecting rod where the straps are fitted on the stub ends of the rods, that is drilled to a dimension ; the drawings do not refer to any dimensions ; we have no use for dimensions, but we have for gauges. They are marked to be drilled with a certain gauge and a certain bushing piece. You could not use an inch and a quarter drill in an inch and an eighth bushing. Whatever bushing you use determines the size of drill that you are going to use ; and whatever gauge you use determines the distance apart the holes may be and the number of them, and the distance they are from the end to the stub. The workman goes ahead and drills regardless of consequences, in accordance with the gauge that is ordered on the drawing ; and the result is that these parts are perfectly interchangeable, and hundreds and thousands of these parts are duplicated from time to time and shipped to almost every country on the face of the earth, and that without a single dimension, either metric or English, on the card—simply the gauge number calling for that part. This may be met with the remark that those people who do not do their work with gauges would not find it so easy to change ; but that is easily confronted by stating that no first-class shop, or any shop, no matter how small it might be, that desired to enter into competition with the world, would ever do its work in any other way and expect to succeed ; it would die a natural death sooner from the mere fact that it failed to use gauges or jigs for the output of its work—even though it had only one of a kind to make—much sooner than it would if it undertook to use the metric system. (Applause.)

THE CHAIRMAN :—The Chair has a list of gentlemen here whom he would be glad to call on, but he is reminded that the hour is getting late. You have the resolution before you, which is entirely in your hands as to what disposition you shall make of it. The matter can be disposed of to-night; this discussion can be continued; it is for the Institute to decide.

A. E. KENNELLY :—I move that the meeting adopt the report and take such steps in connection therewith as the resolution and report call for. (Motion seconded.)

THE CHAIRMAN :—The motion is that the meeting adopt the report of this committee and take such steps as the resolution and report call for. The question has been called for: are you ready for the question? Those in favor of the motion will say aye, those opposed, no. The ayes appear to have it. The Chair decides that the ayes have it. (Applause.)

This resolution will now go forth with the endorsement of the Franklin Institute, and the Secretary will see that the voice of the Institute as expressed to-night is properly presented.

THE SECRETARY :—I should like to have the views of the members regarding the publication of this mass of correspondence, some of which is exceedingly interesting—both that in favor, that against, and that non-committal. I assume that it should be edited in some way—possibly all of it—for publication.

DR. HOUSTON :—I move that it be referred to the Committee on Publications, with power to act. (Seconded.)

THE CHAIRMAN :—The motion is before you that these contributions be referred to the Committee on Publications, with power to act. (Carried.)

[*To be concluded.*]

FUEL OIL ON SOUTHERN PACIFIC RAILROAD.

It is reported that the Southern Pacific Railroad is making elaborate preparations for the use of oil as fuel through that system. The company intends to establish seventy-two steel tanks of 50,000 barrels average capacity along its lines, and has let the contract for this work. These tanks will have a capacity of 3,600,000 barrels, and these, together with the thirteen already constructed by the company, will give a total capacity of 4,425,000 barrels. Julius Kruttschnitt, first assistant to the President of the Southern Pacific Company, is authority for the statement that the company intends to use oil for generating power on the locomotives from one end of the line to the other, and eventually to substitute oil for coal fuel on engines, ferryboats and steamboats. Already the company has converted 210 locomotives into oil burners and has on hand material at the Houston shops for converting 120 more.—*Iron Age.*

PHYSICAL AND CHEMICAL PROPERTIES OF GELATIN.

At the meeting of the Academy of Science of St. Louis, on the evening of January 20th, Dr. George Richter delivered an address on the "Physical and Chemical Properties of Gelatin," which he described as a spongy substance differing materially from other solids. The manner of manufacture of gelatin and its chemical and physical characters was described in detail, and considerable attention was given to the rate of absorption and evaporation of water by gelatin, and the phenomenon of its apparent solution in water. A new hygrometer was exhibited and described, the action of which was based upon the water absorption of gelatin. W.

THE FRANKLIN INSTITUTE.

A lecture delivered at Association Hall, February 8, 1902.

THE GASES OF THE ATMOSPHERE.*

BY DR. H. F. KELLER,
Member of the Institute.

That distinguished chemist, who has added more than any other living man to our knowledge of the earth's atmosphere—Prof. William Ramsay, of London—has said that to write the history of the development of this subject would be to write a history of chemistry and physics.

While this remark must, of course, not be taken literally, it is certainly true that the early investigations on the nature of air coincide largely with the beginnings of physical science, and also that the progress in the study of the atmosphere has steadily kept pace with the advance of the two great branches of this science. No other subject has received greater attention at the hands of physicists and chemists during the past three centuries, and yet this field of research is far from being exhausted, for it continues to yield rich harvests to those who are tilling it in our own days.

It is on account of the remarkable discoveries that have been recently made, that I have chosen the subject for this lecture, in which I purpose to tell in popular language the story of the rise and development of men's ideas concerning the composition of air, and to illustrate this story with a number of experiments and lantern slides.

Much of what I shall say will doubtless have a familiar sound to a large part of this audience, and I must ask their indulgence to enable me to give a connected account for the benefit of those who are not acquainted with the evolution of atmospheric chemistry.

* In the preparation of this lecture free use has been made of the standard works on the history of chemistry, and especially the writings of Professor Ramsay on this subject.

According to a view which originated with the famous philosopher, Aristotle, the preceptor of Alexander, all material things in the universe were made up of four elementary principles or elements, namely, earth, air, fire and water. Each of these elements was supposed to be endowed with and characterized by, certain fundamental qualities which it imparted to those bodies into the composition of which it entered. Thus earth was cold and dry; air warm and moist; fire, on the other hand, warm and dry; and water cold and wet.

This doctrine was universally accepted as a fundamental principle in philosophy, and survived, in variously modified forms, until the middle of the seventeenth century, and some remnants of it can indeed be traced to a much later period.

As an illustration of how it was applied to explain natural phenomena, let me quote a passage from a "Manual of Astronomy," written in Anglo-Saxon a thousand years ago: "There is no corporeal thing which has not in it the four elements, that is, air and fire, earth and water. . . . Take a stick and rub it on something, it becomes hot directly with the fire that lurks in it; burn one end, then goeth the moisture out at the other end with the smoke." And I may add that the ascending smoke, and the ashes remaining after the wood is burned up, were regarded as evidence of the presence of air and earth in the stick.

Strange as the doctrine of the four elements may seem to us at the present day, it endured longer than any other philosophical teaching; its overthrow we owe to the great experimental philosopher, Robert Boyle, who, in his book, "The Sceptical Chymist," published in 1661, advanced convincing arguments to prove that Aristotle's elements (and those which had been added by the alchemists) cannot be regarded as "certain primitive and simple bodies which, not being made of any other bodies, or of one another, are the ingredients of which all those called perfectly mixt bodies are immediately compounded, and into which they are ultimately resolved"—a definition which agrees perfectly with our modern conception of the elements as substances out of which two or more different substances have not been obtained.

It is to Boyle also that we are indebted for one of the most remarkable early attempts to ascertain the nature of the atmosphere. In his "Memoirs for a General History of Air," he sets forth his reasons for considering air to be a mixture of "at least three kinds of corpuscles," attempts to explain its action upon burning substances, and proposes numerous experiments by which our knowledge of air might be advanced.

But while he made many most important contributions to the *physical* side of the subject, and clearly recognized air as a material substance, he did not succeed in discovering its composition, or in ascertaining its relation to combustion or respiration.

The first steps along these lines were made by his contemporary, John Mayow, an English physician. In a now famous treatise, which he wrote in Latin and published in 1674, this acute thinker endeavors to prove that air consists of two kinds of particles. By close observation and shrewd reasoning he is led to believe that only one of these constituents, the *nitro-aerial particles*, is necessary for supporting life and the burning of inflammable substances, and that the other, which remains after this active constituent is removed, is incapable of supporting either respiration or combustion. He asserts that the "fire air" enters the blood during respiration, and is the mainspring of motion in animals and plants. By well-conceived experiments he shows that saltpetre and various acids contain the "nitro-aerial particles;" that it is this constituent which is absorbed when a burning candle is placed in air confined over water, and that the residue left after the fire air has been consumed is lighter than fire air. He argues that a combustible cannot burn in a vacuum, that is, in the absence of air, unless it contains its own supply of fire air, as gunpowder, for example.

Mayow communicated the results of his studies to the Royal Society, but they received scarcely any recognition from his contemporaries, and were quite forgotten when, a century later, a greater mind, equipped with a fuller knowledge, compelled the scientific world to accept views which are almost identical with those advocated by Mayow.

At the time of Mayow's death, which occurred in 1679, the only facts concerning the atmosphere that had been definitely established, were:

(1) That it consists of matter, and, therefore, possesses weight and exerts pressure upon the surface of the earth.

(2) That it is a fluid which, however, differs from other fluids, such as water, in that it has "a spring or elastical power," that is, will yield to external pressures, but regain its original volume when the pressure is removed.

(3) That it is not an element, but composed of at least two kinds of particles; and

(4) That it is in some way connected with the processes of combustion and respiration.

More than half a century elapsed before any further progress was recorded. In 1727 an English clergyman, named Stephen Hales, published some essays on "Vegetable Staticks," in which he gives "a specimen of an attempt to analyze air by a great variety of chymiostatical experiments," and describes at great length his observations of the influence of air on the growth and development of plants. From our point of view this book is of interest, as it shows that Hales was a skilful experimenter, who devised new methods and apparatus to produce and study various gases. We still follow his example in employing separate vessels for generating and collecting gases, and there is no doubt that Hales prepared, in a more or less pure condition, a considerable number of different gases, but he failed to perceive wherein these gases differed from common air. His prejudiced mind was satisfied to regard them as modified atmospheric air, and he concluded that the latter is a "chaos, consisting not only of elastick, but also of unelastick particles, which in plenty float in it."

We miss in Hales' researches the guiding hand of theory, and nowhere, perhaps, in the history of science is the value of theory better illustrated than by the subsequent developments in our subject.

About the time when Hales' book appeared, a very remarkable though erroneous doctrine, intended to explain the nature of combustion and allied phenomena, had come to be generally accepted. The *theory of phlogiston*, as it is called, originated with the German philosopher, John Joachim Becher, but was further developed and definitely formulated by another German, George Ernest Stahl. In this doctrine it was assumed that all combustibles are compound bodies containing a certain subtle principle, the phlogiston, which makes its escape when the substance burns, while the other constituent is left behind either in the form of an acid or as an earthy powder or calx.

Experiment.—Thus, when sulphur or phosphorus is burnt, the phlogiston was supposed to leave these substances and pass into the air, sulphuric or phosphoric acid remaining; and metals, such as tin or lead, when ignited, would yield up their phlogiston and be converted into calces, a transformation which to this day is spoken of as *calcination.*

Non-combustible substances—lime, for instance—on the other hand, were supposed to be calces; and it was believed that if the phlogiston were restored to them, they would again become combustible. As to what this mysterious principle really was, the ideas of chemists seem to have been very vague, and to have changed from time to time.

A sound theory must include all the known facts concerning what it professes to explain, but the theory of phlogiston had a weak spot in its failure to account for the gain in weight of metals when they are calcined, a fact which had long been demonstrated.

The improved facilities in experimentation created by Hales, and the phlogistic theory, combined to stimulate chemists to renewed activity in the study of gases and the atmosphere. During the second half of the eighteenth century the leading investigators directed their energies mainly along this line. Important discoveries followed one another in rapid succession: different "kinds of air," or gases, we would say, were obtained and carefully examined; atmospheric air was recognized as a mixture of such gases, and the relative amounts of these were accurately determined;

and, finally, the chemical changes in which the atmosphere takes part—the phenomena of combustion, calcination and respiration—were correctly explained.

This period in chemistry has, therefore, properly been called the *pneumatic period.* The great discoveries which were then made form the foundation upon which has been reared the great edifice of chemical science.

I shall not attempt here more than a very brief sketch of the great achievements made during this period, for I desire to dwell more particularly upon some of the recent discoveries.

Five great names stand out prominently among those of the chemists of the pneumatic period. They are Joseph Black, Joseph Priestley, Carl Wilhelm Scheele, Henry Cavendish and Antoine Laurent Lavoisier. Of these the four first named may be said to have contributed the principal discoveries of facts, while to Lavoisier, who was scarcely inferior to them as an experimentalist, belongs the glory of having given the true interpretation of their results.

Before the middle of the eighteenth century no aeriform fluid or gas had been clearly distinguished from common air. The gases which Hales and others had obtained were regarded as *modified* air, differing from pure air much in the same way as natural water differs from distilled.

The discovery by Black, in 1751, of "fixed air," or carbon dioxide, was the first instance of a gaseous substance to be recognized as different from air, and, at the same time, as a constituent of the latter. Black obtained it from magnesium carbonate by means of Hales' apparatus; he heated the substance in a bent gun barrel, and collected the gas given off, as I am doing now, over water. In 1755 he published his celebrated thesis entitled, "Experiments on Magnesia Alba, Quicklime, and other Alcaline Substances," a model of inductive reasoning and experimental inquiry, in which he establishes the nature of carbon dioxide as a distinct substance, and proves that small amounts of it exist in air.

Experiments.—A few experiments will help us see the difference between "fixed air" and ordinary air.

(1) A burning taper or candle is extinguished at once when brought into carbon dioxide.

(2) This gas is much heavier than air, for you see that a soap-bubble will float upon it.

(3) And we can always recognize its presence by this simple test, that it turns clear lime water turbid or milky.

In 1766 an important paper was published by Cavendish under the title of "Factitious Airs." It contains, among other things, the first account of "inflammable air," afterwards called hydrogen. This gas was made by the action of acids upon metals, and its discoverer, being like most of his contemporaries, an adherent of the phlogistic theory, expressed the opinion that it either was or contained phlogiston, and that its source was in the metals. While this turned out to be erroneous, Cavendish describes very accurately the principal properties of the new gas, and characterizes it as an individual substance. Many years later it was recognized as an element, and quite recently the French chemist, Gautier, has proved that the atmosphere contains an exceedingly small amount of it.

Experiments.—(1) To illustrate the properties of hydrogen, I prepare it by pouring sulphuric acid on pieces of zinc contained in this generator.

(2) The gas being very much lighter than air, a soap-bubble filled with it will quickly rise to the ceiling; and

(3) We can easily identify this gas by igniting it; it burns with a non-luminous flame.

In 1772 Cavendish obtained another important gas—"mephitic air" or nitrogen; but the same discovery was simultaneously made by a pupil of Black—Daniel Rutherford—whose dissertation on the subject appeared in the same year, and Cavendish, quite indifferent as to the credit due him, made no claim for priority.

Both chemists made this gas in the same way: they passed ordinary air over glowing charcoal, and then removed the carbon dioxide so produced by absorbing it with lime or potash. They noticed that the remaining gas extinguished flames, was not combustible, and had no effect upon lime water.

Experiments.—(1) I have here some jars filled with nitrogen; you see how the flame of this candle is smothered in the gas, and that the latter is not inflammable.

(2) Clear lime water poured into the other jar does not become cloudy.

Rutherford considered nitrogen as a kind of phlogisticated air, although it is incapable of burning.

The discovery of the most important constituent of air, that of oxygen, followed next. It was made independently by Scheele in Sweden, and by Priestley in England. It furnished the clue to the true nature of the atmosphere, and led to a complete revolution in the views of chemists. Without exaggeration it may be said that it marks the beginning of scientific chemistry.

It is interesting to compare the ways by which Priestley and Scheele arrived at the same result. The former was a clergyman, who made experiments as a relaxation rather than for any serious purpose, and while he did make a great number of brilliant discoveries, he lacked in thoroughness and scientific ability to draw the proper conclusions from what he observed. Scheele, on the other hand, was a born investigator, who conducted his experiments with definite ends in view, and whose acute intellect was able to deduce the true meaning from their results.

These differences are clearly reflected in the writings of the two chemists.

Here is Priestley's account of his discovery of oxygen: "Having procured a lens, I proceeded with great alacrity to examine, by the help of it, what kind of air a great variety of substances would yield, putting them into vessels filled with quicksilver, and kept inverted in a basin of the same. I endeavored to extract air from *mercurius calcinatus per se;* I presently found that, by means of this lens, air was expelled from it very readily. Having got about three or four times as much as the bulk of my materials, I admitted water to it, and found it was not imbibed by it. But what surprised me more than I can well express, was that a candle burned in this air with a remarkably vigorous flame. I was utterly at a loss how to account for it."

In contrast with this hap-hazard manner of experimenting, Scheele is led to the discovery of oxygen in the course of a research in which he seeks to establish a theory of the nature of fire and to find an explanation of the part played by air in the phenomena of combustion. In his famous "Treatise on Air and Fire" he shows that air consists of at least two kinds of elastic fluids, one of which, the *vitiated air* or nitrogen, remains when the other, which he calls *fire air*, has been removed by combustion, respiration, or chemicals. He describes how this fire air or oxygen can be obtained by heating various substances, such as nitre and oxide of mercury, and then goes on "to prove that ordinary air, consisting of two kinds of elastic fluids, can be compounded again, after these have been separated from one another." He shows conclusively that oxygen made from calces is identical with that in air, and determines the principal properties of the gas.

He also estimates, though only roughly, the relative amounts of oxygen and nitrogen contained in air, and describes many interesting observations on the respiration of animals.

Experiments.—(1) We can readily obtain some oxygen by heating chlorate of potash in a glass tube.

(2) The distinguishing characteristic of this gas is that it supports combustion better than ordinary air; you notice that this glowing chip bursts into flame in it.

(3) A burning candle emits more brilliant light; and

(4) A piece of phosphorus is consumed with dazzling splendor.

I have pointed out before that the great activity in chemical research during the pneumatic period must be largely credited to the phlogistic theory. Its sway continued unbroken while Scheele and his English contemporaries made the great achievements of which I have told you.

Not only did this (to us) absurd hypothesis completely dominate the mode of thinking, it determined the very form of expression. It would be quite impossible for us to read intelligently the chemical writings of those times without having a knowledge of its conceptions and terms, just as no one can hope to make much headway in the chemical literature of to-day without being conversant with the atomic theory, which is the basis of our chemical philosophy.

It is to the genius of Lavoisier that we owe the overthrow of the reign of phlogiston, and its replacement by the sound reasoning which has lifted chemistry to the rank of a true science.

Even prior to the discovery of oxygen, and before the exact composition of air and water had been ascertained, this master mind was able to renounce the phlogistic doctrine, and to declare it at variance with the fact that the substances produced when bodies burn weigh *more* than the combustibles themselves. The discovery of oxygen (of which he learned in 1774, the year when Priestley made it), and experiments of his own on the calcination of metals, enabled him to formulate a new theory of combustion, according to which only one of the constituents of air—oxygen—is capable of absorption by the burning body, while the other—nitrogen—is incapable of supporting combustion. By argument as well as experiment Lavoisier met his opponents, and finally succeeded in converting most of them to his views.

The *New Chemistry*, as Lavoisier's system came to be called, undertook to explain, and with complete success, all the facts of a chemical nature known in those days, and its essential features have stood the test of experience to the present day.

In his "Elementary Treatise on Chemistry," published in 1789, Lavoisier produced a book which, in many respects, has served as a model to all who have since written upon the subject; his views are made the basis of a new arrangement of the facts, which he states with marvelous clearness in a new language from which the terms of the discarded doctrines are rigidly excluded.

Lavoisier also showed that respiration is analogous to combustion, that it is, in fact, a process of slow combustion in which oxygen is consumed, and carbonic acid exhaled with the unconsumed portion of the oxygen and the nitrogen.

I have yet to speak of one of the most important researches on the composition of the air which was in progress while Lavoisier's ideas were taking shape, and which largely contributed to make these ideas more precise. I refer to the classical "Experiments on Air," which Cavendish began in 1777, and published in the years 1784 and 1785. In these researches by one of the greatest experimenters the world has ever known, the atmosphere was proved to have a constant composition, the relative amounts in it of oxygen and nitrogen being determined with marvelous exactness. The process employed by Cavendish for this purpose had been previously used by other chemists, but with results very different from those which he obtained. For, while Priestley and the others had come to the conclusion that air collected in various kinds of weather, and in different localities, varies in its composition, *he* found as the result of a large number of analyses that, after removing the small amounts of carbonic acid it contains, air is made up of

Nitrogen (phlogisticated air)	79·16 per cent.
Oxygen (de-phlogisticated air)	20·84 "

results which differ but very slightly from those of the best modern determinations.

In the course of these investigations Cavendish made other discoveries of far-reaching consequence; he showed that water is not an element but a compound, consisting of hydrogen and oxygen, established its exact quantitative composition and also that of nitric acid, and, as we shall presently see, obtained argon in an impure state, without, however, recognizing it as a peculiar gas.

With the labors of Cavendish and Lavoisier the investigation of the atmosphere was, for some years at least, regarded as practically completed. The mysterious element of Aristotle, and "chaos" of Hales, had been recognized as a mixture in definite proportions of oxygen and nitrogen gases, containing also very small amounts of watery vapor, carbonic acid and ammonia. The chemical changes that take place when combustible substances burn in air, when metals are calcined in it, and when animals breathe, were clearly understood; it is the active constituent of air, the oxygen, which combines with the combustible or the animal matter, while the nitrogen simply restrains this activity by diluting the oxygen.

The nature of air was thus established in its main features, but an enormous amount of work was to be done in the following century to make our knowledge of the atmosphere more exact and complete.

Before proceeding with my story, I wish to exhibit on the screen a number of pictures to illustrate the part I have told.*

Returning now to where we left the thread of our narrative, we enter once more a period of great activity in the experimentation with gases. In 1804 Gay Lussac and Humboldt made a large number of very careful analyses of specimens of air collected in many parts of the world, at different altitudes and in all kinds of weather. They employed a new process of analysis which they believed was more accurate than that of Cavendish, but the results were nearly identical with the English chemist's; the ratio of oxygen and nitrogen was always the same, 100 volumes of air being found to consist of 21 of oxygen and 79 of nitrogen.

Constancy of composition having been shown to be essential to chemical compounds, was it not likely that the constituents of air were chemically combined rather than simply mixed?

Further experiments by Gay Lussac revealed the fact that whenever two gases combine to form a compound, they do so in a simple proportion, such as 1 volume of one gas to 1 volume of the other, or 1 : 2, or say 1 : 5; but no such simple proportion existed between the volumes of the oxygen and nitrogen in air.

[*To be concluded.*]

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* The slides shown included portraits of nearly all the chemists who had been named, as well as pictures illustrating their lives and labors.

Mechanical and Engineering Section.

Stated meeting held Thursday, October 16, 1901.

MR. JAMES CHRISTIE in the chair.

THE CONSTRUCTION AND INSPECTION OF STEAM BOILERS; WITH ESPECIAL REFERENCE TO THE "CITY OF TRENTON" DISASTER.

(*Concluded from p. 340.*)

DISCUSSION.

MR. WASHINGTON JONES:—How would you, Mr. Vauclain, determine that that boiler had no water in it, and whether the water was below the crown-sheet?

MR. VAUCLAIN:—It would be a very difficult matter to determine just how much water was in this boiler, or as to how far it was below the crown-sheet. The Government boiler inspector testified that he thought the water was 10 inches below the crown-sheet; but it is my honest opinion that the Government boiler inspector did not know anything about it; and if you would like to have my opinion, my opinion is that the water, from what I have seen—I have no evidence—even though one man testified under oath that the oiler told him that they had low water, and the oiler testified under oath that that was not true—my idea is, gentlemen, that the water had either reached, or not quite reached, the top of the crown-sheet, and that the intensity of the flame in this section treated this very little water—and the water acted on the sheet—just as water would on a hot stove-plate; consequently, this portion of the sheet got hot (*Fig. 17*), and if there had been that small amount of water it would have helped to keep those portions cool which were protected, as this would be where the flames would pass around. Any one who has noticed the flames in an ordinary heating furnace in a blacksmith shop would know just exactly how that flame would travel, and that the after-portion—where the fire very little reached—

the water would bank up on there; consequently this water would be driven out and driven away. Will it cause the trouble? It would, because a very slight heating in that plate would interfere very seriously with the holding power of one of those studs; not so much on account of the number of studs intact, but on account of the sagging of the plate—the slight sagging of the plate—which would loosen the tightness of the fit, if it was tight on those studs; and when we criticise the production of any well-regulated manufacturing establishment, it is only fair and just for us,

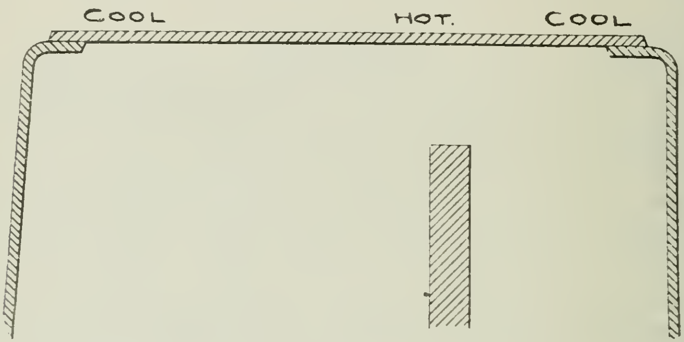

FIG. 17.

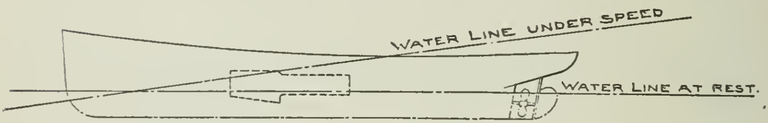

FIG. 18.

without other evidence at hand, to feel that the work was done properly.

Also, inasmuch as this work has been tested by inspectors appointed by law and, as I understand, was supervised by others who were watching and who were interested in the manufacture of this particular boiler and the boat itself; therefore, if the boiler had passed through the scrutiny of these various persons who were entrusted with the character of the work that was being done on this boiler, and tested and passed and fired and put in service, certainly the boat was not turned over with any knowledge on the part of any one—either employed by the Government or by the manufacturer who made it—that there was anything in this boiler that would interfere with it giving good service; and it is my honest opinion—whilst I have not built a boiler just like this (we do not build boilers just like this for locomotive work), I would not be afraid to use a boiler constructed like that in the stationary service or ordinary service where I would always be able to have sufficient water over the crown-sheet.

Mr. Chairman, there is another matter in connection with this that has been lost sight of. I am not a shipbuilder; I do not know whether I can draw a ship (*Fig. 18*).

FIG. 19.

Now, this boiler was placed that way in this boat, and the propeller came out at the stern like that, and the engines, I believe, were in this region, immediately aft of the boilers. Now, the crown-sheet was level and the boat, according to the testimony which I listened to, was making a speed of somewhere between fifteen and eighteen miles an hour. If it had been figured in accordance with the elapsed time, as the testimony gave, it would be eighteen miles per hour. The builder claims that he doesn't think the boat can make eighteen miles an hour; but anywhere from fifteen to eighteen miles an hour. If the boat—I won't give you the nautical term, because I don't know it; but if the boat was lying level—that is, if the boiler was level with the water, when it was standing still—when the boat would be driven at speed and the water line would change into that (exaggerating it), the bow coming out of the water and the stern being pulled down by the screws, the water line in the boiler would appear like that (*Fig. 19*), wherever it might come; and consequently the tendency would be very much greater to have a scarcity of water on the crown-sheet when the boat would be running at maximum speed if the engineer was watching the gage in the engine-room than it would had the boiler been turned around the other way in the boat and operated. Then the faster the speed the greater the depth of water would be over the crown-sheet.

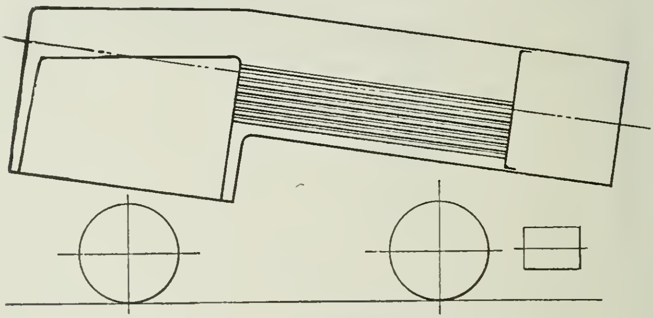

Fig. 20.

In locomotive practice it is customary to lower the crown-sheet at the back three inches (*Fig. 20*) so that if you do get in an abnormal position you won't drop one portion of the crown-sheet, but when you get at that angle the water will be practically level all over the crown; and when we build for abnormal grades running from 10 per cent. to 25 per cent., the boiler, in addition to having a very much angled crown-sheet, is also placed on the locomotive in this fashion; so that when it gets on grade the boiler is comparatively level and in proper working condition.*

* It may be of interest to add that, since the meeting, Mr. Hartman has reported to the editor that in a trial made on a similar boat, no difference in the position of the water-line could be detected when standing still and under full speed.—[Ed.]

MR. HARTMAN :—The remark made that "Life is too short to get up intricate stay-bolts," brings up the question whether it is not better to make intricate stay-bolts and save life.

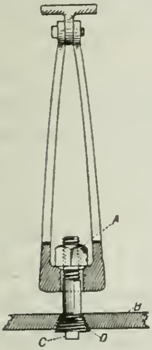

FIG. 21.

The stay-bolt, *Fig. 21*, shown in drawing, is one that can be tightened up without calking, is flexible sideways and can be adjusted to exact length. It has the advantage of the thread and a shoulder in the crown-sheet and the strain is brought on the thread and taper at the same time so that one cannot give way before the other.

Why does not the Government fix on a diameter of stay-bolt proportioned to thickness of the sheet and limit the spacing to increase the number of stay-bolts, as there is more safety in a number of small stay-bolts than in a few large ones? The diagonal spacing gives a long distance between two stay-bolts and a short one between the other two. Is this right? No one has studied this question and obtained better results than the Baldwin Locomotive Works. Their spacing is at equal distances, straight lines and more stay-bolts of a smaller size. Why should not the Government adopt what practical experience has taught these builders? Too many stay-bolts are an evil to be avoided on account of cleaning the boiler.

There appears to be a dread of putting plenty of manholes and handholes in a boiler for cleaning. When they are made amply strong and properly fitted up, there is no trouble with them. To all this comes the reply, you are making your boiler cost too much. But the fact is, that, comparing the cost of the explosions that occur from defective construction all through our land and the additional cost of building boilers properly, it will be found that the proper construction is by far the cheapest in the end. The use of limestone water should be prohibited unless the lime is precipitated before entering the boiler, which can now be done. Why boil lime mud, waste coal and ruin the boiler?

In marine boilers the pure distilled water from surface condensing is used, which keeps the boilers clean, and no risk is run of burning from heavy deposits of scale. Why not use surface-condensers exclusively with land and marine boilers and get the highest efficiency?

An ideal boiler would have a cylindrical fire-box lined with fire-brick, kept hot by the firing, and in which all gas shall be burned to carbonic acid, and the products of perfect combustion then passed through a great number of small tubes using condensed water.

Having no direct radiation from the fire to the boiler or tubes, the heating surface must be increased to a certain extent, for we have a larger volume of heat but at a lower temperature, which requires the increase. There is no risk of burning the boiler. On the other hand we have a boiler of the highest type of efficiency with a long life. The bricks lining the fire-box are prevented from melting by the surrounding water, while the intense heat insures perfect combustion before the gas reaches the tubes. Once unconsumed gas reaches the tubes, the heat is absorbed by the tubes and no further combustion takes place, the escaping gas goes up the chimney in great black clouds, causing a loss and poisoning the atmosphere.

Many years ago, there was built at the works of I. P. Morris & Co., a boiler with the inside and outside shell bolted together by stout flanges at top and bottom. The outside shell could be removed, leaving the interior free for the workman to get at for repairs and cleaning.

MR. GEORGE B. HARTLEY:—I would ask Mr. Vauclain if any testimony was given regarding the working of the feed-pump or the feed-pipe. I was not in the city at the time and do not know. Now I assume in this argument that the pump was working as it ordinarily did; that the valves on both boilers were opened—one perhaps a little more than the other; because one boiler will take more water than the other when both are feeding from one line; and in this instance I understand that the engineer opened this one valve wide. Was there any evidence offered as to why that was necessary, or why the boiler did not feed as it had been right along? Or was anything said about the condition of feed-pumps or feed-pipes; whether they were found to be free from obstructions?

MR. VAUCLAIN:—There was nothing in evidence regarding the condition of the feed-pumps. They were supposed to have been working all right—at least, there was no evidence that they were not; and the reason, I believe, that the engineer assigned for opening this feed-valve wide was there was a little less water in this boiler than in the starboard boiler, and for that reason he opened the feed-valve wide.

MR. COLVIN:—I understood that this discussion was to have been on boilers generally, and not particularly on the exploded structure. I have had some little experience in the boiler business. I would like to ask if any gentleman here ever knew a crown-sheet—had positive proof of one—coming down, unless it was for want of water? I have known them to carry 200 pounds of steam on the engine. I never knew one to go down unless it was for the want of water, unless the sheet would become heated so that it got soft in other places. I have had thirty-five to forty years of experience, and I have seen a good many crown-sheets down, and I have never seen one that gave out unless it was, evidently, as plain as day, from want of water. Just to demonstrate that: The crown-sheet bolts are generally larger; more precaution is taken with them than the side stays in the same locomotive; generally they are $\frac{3}{4}$ inch when the crown is only $\frac{7}{8}$ inch. You do not hear of the stay-bolts pulling out the side sheets at all—that is out of the question; it is always on top and in the place indicated by Mr. Vauclain.

MR. LE VAN:—I would like Mr. Vauclain to tell me why these threads were stripped off the bolts at the back end of the boiler and also at the sheet. If that crown sheet was hot it would not have stripped off. The ends of those stay-bolts were just as though they were turned off in the lathe.

MR. VAUCLAIN:—In answering Mr. Le Van's question I simply want to remind the gentlemen present that I have already taken the trouble to state that I believe that that was the hottest portion of the crown-sheet; and as the flame passed over this portion of the crown-sheet it became heated, and that this portion and this portion particularly were not affected by the heat to the same extent. I also stated that my opinion was that the water had just about reached the top of the crown-sheet, or not quite, and that this heat had driven what water might have been there away, and it was barely possible that these sheets were protected to a certain extent at their extreme ends; and the condition of the studs and the condition of the steam, both at the front and back, and the condition of the plate also, indicated that such probably had been the case; therefore, the tendency to pull these threads and shear them off—not as though they were taken off in a lathe, but as though pulled off the grain of the iron—was a natural consequence; these in front, not so much so; and a careful examination of the boiler will show that that was the case; whereas, in the center or immediately forward thereof, the studs were more or less sloughed off, as though the metal had been quite hot. One of these studs taken out about this point and cut and etched and a portion placed under the microscope would indicate that the bolt had been hot on the end, or that it had been heated on that particular end hotter than it had on the portion of the bolt that was up here. This end that had protruded through and which had sloughed off indicated that the temperature had been quite high, so far as we were able to determine.

MR. LE VAN:—I would like to state that at the point which you stated was the hottest the threads were not stripped; it was next to the flue sheet where they stripped, and I cannot understand how you can make any distinction between the temperature at the flue-sheet and at the front of the box. How do you arrive at that?

MR. VAUCLAIN:—I think that it would be difficult to explain this matter more clearly. Without entering into personalities, it is difficult to make a man see who will not see; but if a bridge-wall is placed in any vessel the portion immediately over the fire, or where the fire starts over the bridge-wall, if the blaze is severe, is much hotter than the part over here, or than the part that is back there; and if there is any disposition to doubt that, let us take an ordinary cook-stove (*Fig. 22*). Here is the fire and here is the bridge-wall, and the flames are passing over. In this case the flame is drawn straight, goes under this plate (the flame is not drawn down immediately and passed through the tubes here), so that the action here on the back plate will be much more severe than it would in the case of this boiler; but any man here who has ever been a boy and has watched his mother's cook-stove, will remember that when the plates commenced to get hot this one got hot first, and usually the plate began to redden on this portion and not on the front portion. Nobody ever saw the front of a stove burn out quickly; it will last quite a long while; while the bearing

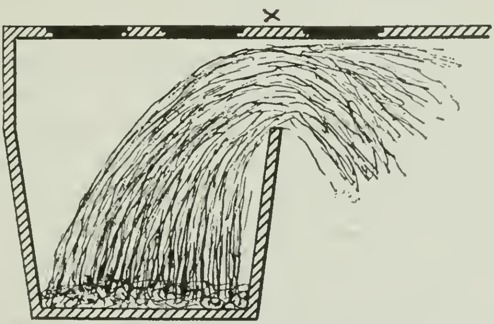

FIG. 22.

plate in the center was always the first to go; that the bearing plate in the center which carried the plates (and as you will remember it carried across like that, and the two plates were hung to it); this plate would sink in the center and in some were supported. So, therefore, if we go back to our childhood days and remember what we saw then, we can easily understand why this crown-sheet was hotter immediately over the bridge-wall, or back of it, than it was at the back end or at the front end.

MR. GEO. B. HARTLEY:—I would say if there is a marine-boiler manufacturer here he can testify that crown-sheets of marine boilers frequently come down, and not necessarily because of low water. They have come down so that in a 36-inch or a 40-inch corrugated furnace you can scarcely get a 5-inch space between the down part of the crown and the grate-bars—remained, in fact, almost flat down on the grate-bars. Possibly that is due to oil; and I have been waiting for some one to suggest the possibility of oil in this boiler. When examining the "Quaker City" boilers, the engineer climbed in side first, and before I got in I asked him where the oil came from that was on his hands (it was thick). He replied, "While shaking the braces," showing that there was considerable oil in the boiler. I said, "I think you had better boil this boiler out with soda before you get under way again." He said, "We have done it;" showing that there must have been an excessive amount of oil in the beginning, or that the oil was going in there all the time from some source.

MR. VAUCLAIN:—Mr. Hartley's point is well taken. I noticed in this exploded boiler quite an amount of oil had been in the boiler; but I think that the oil would probably make the water very foamy, and the indications were, in the boiler I examined, that the oil had been floating on the top of the water and had gotten very firmly attached to the upper portion of some of the stays. Now, of course, when the boiler was blown out, there was a film of oil on top of the water; that oil would settle down on the crown-sheet and around the stays and make them appear very badly; but it is a fact (it is not a practice that I approve of) that a great many people are feeding oil into their boilers in order to keep them from making scale, and having trouble from that cause.

MR. GEO. B. HARTLEY:—I would like to show another condition that existed in the "Quaker City" boilers. Into the back head of the boiler, on a line about four inches above the crown-sheet, was a gage cock, four inches above this was a second gage cock. These cocks, when tried, would show when the necessary amount of water was in the boiler. In addition to these, and connected from the side of the boiler at each end, was a water column. On to these columns were attached in the regular way three gage cocks and a water glass gage. The columns were connected to the boiler by the usual top and bottom pipes (top for steam, bottom for water.)

This water column I believe to be imperfect, the column proper being of so small a diameter that the trying of a gage cock would lift or siphon the water out, even though the water would be several inches below it, thus misleading the one who tried the gage as to the true water level. The glass gage would show the true level, providing the connections to it were clear, but in the "Quaker City" I found the glass broken and the connections to it closed. A water column should have a sectional area of at least nine square inches with 1¼-inch pipe connections thereto.

THE CHAIRMAN:—I have no doubt that our friends who build the water-tube boilers would say, "build the boilers so that the vital parts will not be exposed to such extreme danger in case the water should get a little low."

MR. FALKENAU:—I would like to ask Mr. Vauclain whether in the construction of his boiler he has any suggestion to make as to what could have been done to avoid to some extent the danger of structural weakness in the "City of Trenton" boiler. Would not a greater amount of riveting have been advisable? From all appearances and the calculations which I have roughly made of the stay-bolts, the bolts were in themselves strong enough. The strain, I believe, amounted to about six thousand, was it not?

MR. VAUCLAIN:—Between six and seven thousand.

MR. FALKENAU:—Do you think there would have been any decided advantage in having had much larger heads, or having put nuts on the ends of the stays?

MR. VAUCLAIN:—Replying to the question, I beg to state that there is quite a difference of opinion on that point. So far as the stays in a crown-sheet are concerned—in a side sheet which is practically the same as the crown-sheet, excepting that the water very seldom if ever gets away from the side sheet, no one ever dreams of having any further protection than merely riveting the stay-bolt end over; and the stay-bolt end is riveted over very slightly; it is considered that the less height the rivet head has, the less trouble they will have with that stay-bolt leaking in the fire-box itself. As the sheet is there, and if the stay-bolt simply projects ⅛-inch and is hammered over, it is considered better than if the stay-bolt had a head that was $\frac{1}{4}$ inch or $\frac{5}{16}$ inch high; because in the latter the head would become hot and expand the thread or pull in the sheet, and then when the bolt would cool off and the fire would be drawn, the belt would contract and the water would ooze out, which is a condition that prevails in a great many locomotive boilers—especially in bad-water districts.

I was in Indiana quite recently in the shops of the Vandalia–Terre Haute Railroad, and I was informed that it was a daily practice to get in with the hammer and poen up these stay-bolts; that the water was so bad that the tendency of these bolts to heat and expand and then contract and get loose was very annoying indeed. In a crown-sheet we simply reverse the condition of affairs; but what the average railroad man fears is that some expert of a locomotive engineer will let the crown sheet get hot—will not carry enough water; consequently he resorts to some precaution, and that precaution is generally a device of his own. One man will tell you that he doesn't want anything under his crown-stays—that when a crown gets hot he wants it to let go; but he builds his crown tapering; that is, he lowers it at the back so that when it gets low in water it will commence to be exposed in this manner, and these bolts will let go and that will fulfil the office of the fusible plugs; and he will get in with the jack and reset his sheet. Others suggest the putting on of a nut, and others object to it. Others put in a button head. Others don't like the button head, because it pulls off too easily if it gets hot (I never saw one pull off yet) and they put a hex head on. I might tell you of a number of other devices that are used in order to keep this crown-sheet from coming off when it gets hot; but invariably it *comes* off if the plate has been left to get hot enough.

Answering fully the question that was asked me, we are viewing this boiler not as we would have viewed it if we had been called upon to examine it—if we had been called in before there was any accident; but we now have our hindsight, and we can think of a whole lot of things that would have kept up that crown a little longer; and among the number of things we might have put on a nut; we might have put more stays in, and put them closer together; we might have done a great many things. If I had been the designer, and would have had the knowledge that I have now of locomotive boilers, I would not have used any of this rigging at all; I would have used an entirely different method of supporting the crown-sheet—perhaps a more slender method. There are some two tons of material, I think, in this boiler that might have been saved; but that is neither here nor there; I think that it would be very ungentlemanly in me, if not a complete violation of professional ethics, to criticise the design of this boiler; because I would be criticising these things after the accident had happened and not when the boiler was designed and when it was put in service. The Government furnishes men paid salaries for this particular thing; and this boiler was designed by the engineer, I suppose, at Neafie & Levy's; and the testimony was that it was submitted to the Government—their inspectors—and the "Board sat upon it." I don't know what that means, but suppose they looked at it and they approved it; they made their calculations, probably, and they approved it; and it was built and accepted and put in service; and I feel perfectly safe in saying that if the crown-sheet had not been overheated at that point it would have been running yet.

THE CHAIRMAN:—We are now ready for further discussion.

MR. FULLERTON:—Mr. Vauclain says, if he should design that boiler now, with his present knowledge, he would use a lighter construction there. I think he would make it interesting to us if he would kindly suggest how he would have supported that crown-sheet.

MR. VAUCLAIN:—In the first place, the gentleman did not state the matter quite right. I said that if I had designed this boiler with the knowledge that I have of locomotive boiler construction I would not have designed it in this way at all, not because this accident happened and with the result that has been chronicled. If I had been designing this boiler—I will only treat the fire-box (as that is the important part) and the holding of the crown-sheet which let go. Now this boiler had a flat crown-sheet, if I remember correctly, and was so shaped (*Fig. 23*); it had crow-feet in the top here, and studs in the bottom; and a link with a hole in there and a hole in there, suspended on the crown-sheet. Any one who has worked at the business at all knows the difficulty of getting every one of those a tight fit. I suppose it can be done; but life is too precious to waste time in doing that; therefore, if you will permit me, I will draw my half on this side. I would have made

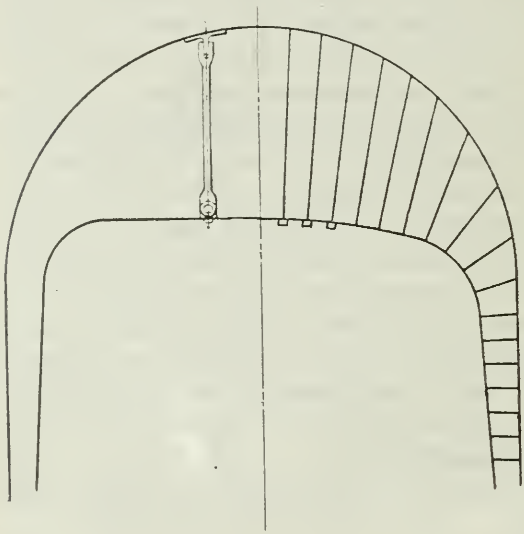

FIG. 23.

FIG. 24.

my crown-sheet like that (indicating), and in place of this system of links I would have simply started radial with this crown-sheet and put in a system of ordinary screw-stays which I will draw in full for you (draws), making these 1 inch in diameter and reducing these to $\frac{7}{8}$ inch or a little less between the threads, where they enter the sheet; and the side-stays from there down, I would have made them $\frac{7}{8}$ inch in diameter at this point and about $\frac{11}{16}$ inch in the center. They would have been shorter stays. These stay-bolts would have come through there; there would have been a slight angle to this crown-sheet, about 3 inches, so that from the tipping of the boat, if the boat went down behind, the crown-sheet might probably have been level. The sheet in this case was $\frac{1}{2}$ inch thick all round. I would not have made it over $\frac{3}{8}$ inch; and if the crown-sheet had been made separate from the side sheets, so that they could have been removed easily in case of blistering or mud-burning on the side, I would have made the crown $\frac{3}{8}$ inch and the side only $\frac{5}{16}$ and screwed the screw-stays into this and have rested my case there. The center rows in the crown-sheet here, if I had known that I was going to use a bridge-wall, would have had nuts or head bolts as a further safeguard to prolong the time that would be required to pull the hot sheet over the end of that bolt. [Applause.]

MR. FALKENAU:—I would like to ask Mr. Vauclain for what object he would round the crown-sheet in his form? That is, as I recall it, in the Bellepaire construction you have a flat crown-sheet, practically, stayed vertically; whereas, in this of that you have a sort of radial staying. What advantage you deem that there?

MR. VAUCLAIN:—In a Bellepaire the top is usually flat and we have a square corner which couples onto the round of the boiler here; the stays come vertically through the sheet, and we get a good thread through each sheet. Then we start with the side sheet, and then we have transverse stays to stay each portion of the sheet; and those are usually heavy on account of getting in a sufficient number. In this case the top of the boiler is rounding, and in order to get a good thread in the thin crown-sheet, that is, to have the thread at right angles to the crown-sheet, it is necessary to have these stays radiate, as near as possible, from the center from which this curve is struck; and as this curve changes where it gets smaller and smaller, of course the pitch on the outside is much larger. If you would take the other side of the boiler and have it just as it is now, in order to get threads through there at right angles, we would have to put a stay that way, a stay that way; and when we come here we would have our threads—we would not have a full thread in the sheet. Here, we always have a couple of full threads in the sheet. In this case we would not, and we would have a large surface there exposed to the pressure;

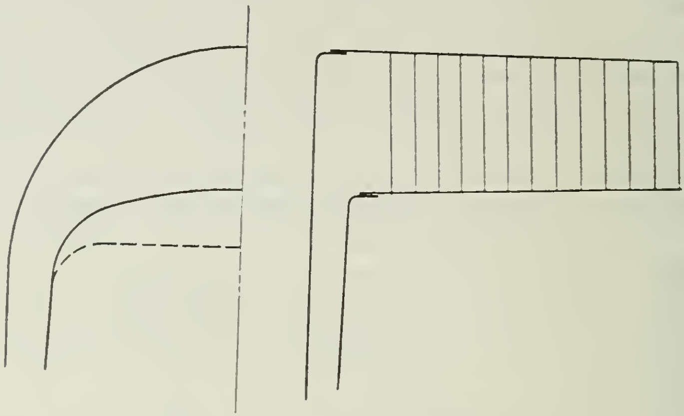

FIG. 25.

secondly, it would necessitate some more stays across in this direction in order to stay this surface. With this construction, however, that is avoided and those stays are not necessary, for the reason that in any crown-sheet, whether it be round or flat, there is just as much tendency for this sheet to go up as there is for that sheet to go down; there is an internal pressure tending to pull these two sheets apart; and if the bolt gets loose, the sheet gets soft and the bolt loses its holding power, the plate bulges down and comes down.

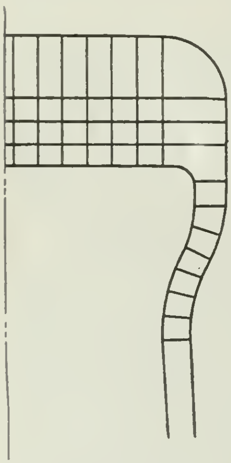

FIG. 26.

Now, it is further desirable to reduce the number of stays as much as possible; because the less stays we have in the boiler the less weight we have and the more room we have for water. There has not been a single mention made of the scarcity of water in this boiler. As designed, the space over the crown-sheet had a great deal of iron in it; but no one ever asked how much water there was in it, and the more iron you have in over your crown-sheet the more difficult it is for an engineer to feed his engine, so that he has a uniform depth of water over his crown-sheet; or, in other words, if a space over your crown-sheet is taken up with iron very largely and there is a limited amount of room for water, your boiler will make or lose water much more rapidly than if held with a system of staying, such as this; and that is what has made this a preferable staying over any other style, and we have them all.

MR. HARTMAN:—In regard to boiler inspection, I ask: Is that (*Fig. 10*) the proper thing to put in a crown-sheet? You never know where the thread is going to begin on the crown-sheet or when it is going to begin on the screw of the lug. On screwing it in the crown to fetch up fore and aft, you will find it will often fetch up on the shoulder athwartship. What is the boilermaker going to do? He simply sticks a spud in the hole and yanks her around fore and aft. There is then a strain on the thread anywhere from 300 to 1,300 pounds. These lugs $1\frac{1}{4}$ x $3\frac{1}{2}$ inches cover too much of the crown and keep the water from protecting the threads on the lug. I have seen boilermakers kink a stay-bolt if it was a little long, to shorten the distance between the ends; but on applying pressure the stay bolt straightens out, leaving some other part take the strain and do double duty. In other cases have seen boilermakers poen the stay-bolts cold to make them longer, which weakens the bolt. Since soft steel has come into use, in recent years, it has proved so serviceable that it is with us to stay. Should not the Government in its formula for spacing take into account what is the stiffness of the plate? I think that has been lost sight of.

Should we not have adjustable stay-bolts that can be made just the right length to suit circumstances? Then heating your boiler as hot as you can at atmospheric pressure, the internal strains from riveting will adjust themselves; you can adjust the stay-bolts to get a more uniform tension of all parts.

Should not the Government fix the diameter and depth of all cold rivet heads on any part of the boiler?

Should not the inspector after supplying his 50 per cent. excess of pressure in testing go into the boiler to see if any stay-bolts have been strained or started? I have no doubt but that it was this excess that started some of the rivet heads of the "Trenton's" boiler.

THE CHAIRMAN:—The subject is still open for discussion.

MR. KINNEY:—When Mr. Hartman was giving the account of the experiment he made there, he made no mention of having made a comparison of the two kinds of metals: the metal he used in his experiment and the metal out of which those stays were made—whether both of the same kind, possessing the same ductility and the same tensile strength. There is quite a difference in his statement as to its resistance value and that of Mr. Vauclain, who stated 60,000 pounds; and Mr. Hartman gave it as the result of a practical, actual pull, as only 32,150 pounds. Now there is quite a difference.

MR. HARTMAN:—That is, steel as near the composition of the steel in that crown-sheet as you can get. The lugs were made of iron, as near as we could judge, and of the same quality that those old ones were. We split one of them in two by sawing it down part way and broke it open to examine the grade; and we tried to put the thing as nearly under the same conditions as possible. I tried to find who furnished that plate, to get a piece from them; but the steelmakers wouldn't tell me; so we had to fall back then on a piece of steel that was as near the same quality as possible, but on riveting our sample found our iron riveted better than the original stay-bolt lugs.

———:—Why can't the gentleman make one test, or a series of tests, of those stay-bolts?

THE CHAIRMAN:—He asks, if there was one or more tests—one test, as I understand Mr. Hartman?

MR. HARTMAN:—We made one test that was pulled slowly, giving it about 25 minutes; the other test we pulled quick. Both of them commenced to spall the rivet at the same strain, but pulling the other plate so much quicker it ran higher; but giving 25 minutes, the rivet was badly broken up at 28,000 pounds, and final rupture was at 32,150 pounds.

THE CHAIRMAN:—I merely wish to make one observation in few words in reply to my friend Hartman, more especially in relation to the subject of the steel plate, namely: he speaks about the question whether the Government should not use another formula for the resistance of flat surfaces to buckling, or to giving way under pressure, when we deal with soft steel. I think there is probably some misunderstanding on this, from the fact that in speaking of very low tension steel, which we know is very ductile—we can bend it over inside or outside without any show of breaking—I was going to say here that the very exhaustive tests, not only one, but the results of hundreds and many hundreds of tests, I think very clearly demonstrate that the stiffness of the metal, that is, its resistance to pressure or to yielding under pressure by buckling, shows that it follows very closely its tensile strength; in other words, if that steel was, say, as low as 50,000 pounds tensile strength and you were comparing it with iron of 50,000 pounds, that the resistance would be very similar. If 60,000 pounds, it would be about one-fifth more.

MR. HARTMAN:—In reference to the heat in the fire-box it strikes me that there are two phases to that, from what experience I have had in other boilers. Calling that the bridge-wall (*Fig. 17*), when you first throw your coal on the fire your carbonic oxide is generated so rapidly that it is not all burned near the front—this is comparatively cool; but when it gets further back, then the air that strikes it here from back of the bridge-wall you will find gives the greatest heat there; that is, when you put fresh coal on. Now when the coal has burned and got up good and bright, then you get your most intense heat over the bridge-wall. Why not use a fusible plug at each place?

For some three or four months—during the time that the work on the "Trenton" was being completed—Mr. Wilson was confined at home with a serious illness; but at the time it was being finished he got down to the works on three occasions with the help of his nurse to look around at the work, so that personally he could not give it the inspection that he would have done if he had been well.

THE CHAIRMAN:—We will adjourn for this evening, thanking you all for your attendance.

ELECTROLYTIC LEAD AND OXIDES.

At the recent meeting in Philadelphia of the recently organized American Electrochemical Society, Mr. Pedro G. Salom read a paper describing a process of his invention for electrolytic lead reduction which is in operation at Niagara Falls. The principle lies in the use of lead sulphide as cathode in an acid solution, the hydrogen cation forming with the sulphur gaseous hydrogen sulphide, and thus reducing the lead sulphide to lead in spongy form. The form and construction of the cells used were described and the difficulties discussed which had been encountered. The principal difficulty was that the reduction was not complete, and the degree to which the ores are reduced varies without apparent reasons. It was stated that at present 92 to 95 per cent. of the sulphide is reduced. Another difficulty is that the workmen's eyes are affected in the operation of the process. Two pounds of lead are produced per horse-power hour, 48 cells in series being electrolyzed at 130 volts. Specimens were shown of spongy lead, compressed sponge, litharge and other materials made by Mr. Salom's company. Owing to the spongy form, the material is very readily transformed into litharge. The company intends to take up later the manufacture of storage battery plates.

W.

SAPPHIRES IN NORTH CAROLINA.

One of the most valuable of the recent contributions to the literature of the sapphire is that of Dr. Joseph Hyde Pratt, mineralogist of North Carolina, published as Bulletin 180, Series 9, of the United States Geological Survey. On page 9 of the paper he discusses two or three localities where it occurs in gneisses and schists. In Cowee township, Macon County, it is found in seams or layers in a hornblende (amphibole) gneiss, which is itself derived from the alteration of an igneous rock (gabbro), the augite having been changed into hornblende, and the rock much sheared by pressure. The corundum shows plainly that it was an original constituent and not an alteration product. The other occurrence is even more interesting, as it shows the presence of corundum in gneisses and quartz-schists derived from sedimentary beds. These rocks extend along the crest of the Blue Ridge from Rabun County, Ga., to Clay County, N. C., and carry bands or zones of corundiferous schist, conforming to and belonging with the rest. All these schists, Dr. Pratt regards as very ancient sandstones and shales, greatly elevated, eroded and metamorphosed; in the course of which changes the aluminous shales yielded first bauxite, and then the excess of alumina crystallized as corundum.—*Engineering and Mining Journal.*

PHYSICAL SECTION.

Stated Meeting, held Wednesday, November 27, 1901.

THE QUESTION OF THE DIVISIBILITY OF THE ATOM.

BY A. STANLEY MACKENZIE, PH.D ,
Member of the Institute.

There has never been a time since the atomic theory of matter first took its place in the world of science as a tried and tested hypothesis that its reasonableness or sufficiency has not been questioned. It is true that the objection of many people to the theory has been based on *a priori* reasoning, and not on the capability or incapability of the theory to co-ordinate experimental evidence. Indeed, the fact is too frequently lost sight of that this is one of the prime objects of a theory, if not the only logical object. The fundamental meaning of a theory is an imagined mechanical model that, with its supposed motions, characteristic properties, degrees of freedom, etc., will simulate and reproduce for us, when subjected to the laws of dynamics as we know them for ordinary matter, the behaviors of unknown agencies. The atomic theory has always met with much objection, because it forces us, we are told, to believe in the existence of absolutely hard, round, perfectly elastic small bodies, or else of bodiless centers of force, or of ghostly pieces of electricity getting their apparent mass in an equally ghostly way, or else of infinitesimal cyclones of ether differentiated from the omnipresent ether only in that they are in vortical motion, that is, that they are in suspension in a continuous fluid of their own material, and only on account of their motion are singled out for our comprehension. We are told that the mind cannot admit the existence of such things as realities of the same nature as the bodies we can lay our hands on. In this kind of objection we are confounding two quite different conceptions. In the first and most important instance the atomic theory is a mechanical model—a mathematical model, if you like; in the second place, it is an effort to specify the ultimate physical constituents of matter. We do believe that in so far as our researches have carried us we have in matter a coarse-grained structure; but we must admit that we may have misnamed the inherent or essential nature of the atom; we may have called it a little, hard, round piece of matter (with most of the properties we associate with molar bodies) when it was a piece of ether; or we may have called it ether when it was electricity, etc.; but in every case the same analytical treatment would apply, that is, we should retain practically the same theory with an altered nomenclature. The test of a good theory is not its beauty or symmetry, nor its *a priori* likelihood, but its accuracy as a model and its suggestiveness. By this test the atomic theory is a good one, and all advance in our knowledge concerning the behavior of matter as such has been made under its guidance. But the special claim it has to goodness is that nothing has been advanced thus far which will take its place.

It has, however, been mainly on other grounds that criticism, not captious, has been directed against the atomic theory. It is that it has not gone far enough; we are left with seventy-odd kinds of atoms, with no suggestion for an explanation of their difference, with no link to establish their connection; and the thought has been almost an universal one during the whole past century that this connection must exist, and that the number of different kinds of atoms can be reduced and some relationships between them established. Even Dalton makes the following admission: "We do not know that any one of the bodies denominated elementary is absolutely indecomposable." As early as 1816, while the atomic theory was still in the hypothesis stage, Prout made his famous deduction from the rather inaccurate numbers then known for the atomic weights of the elements, that these are exact multiples of that of hydrogen, and that consequently all the other atoms are compounds of the hydrogen atom. It is safe to say that the result of the work done in relative atomic weight determination since the time of Prout has been to make these numbers on the whole to approximate more and more to whole numbers. It is true that the number for chlorine is almost exactly 35½; but notwithstanding that, the closeness of many of the numbers to multiples of that of hydrogen is remarkable. The Hon. R. J. Strutt has determined recently the chance that the relative atomic weights should approximate so closely as they do to whole numbers, supposing their values to have been determined in some haphazard manner. He found it to be as small as 1 in 1,000; so that, as he remarks, we have stronger reason for believing in the truth of some modification of Prout's law than in that of many historical events which are universally accepted as unquestionable.

If there is one thing more than another that makes us look with hopefulness for a proof of some such relationship as Prout's, it is the aid it should give toward the introduction of a theory of gravitation. There is no natural phenomenon more universal and less understood than the non-selective action of gravitation, and it is one to which attention is seldom called in text-books on physics; they are apt to leave beginners with the impression that gravity *should* act on all bodies alike, but magnetism differently. This fact early suggested to Graham that all the different elements may have the same atom or ultimate particle, but existing in different combinations of movement. Lavoisier, again, was careful to state that he called certain things elements only because with the means at his disposal they were indivisible. Their chemical investigations had led such men as Kopp, Dumas, Berthelot and others to consider seriously the possibility of the compositeness of the atom as the only explanation of certain anomalous phenomena which they had noted.

In 1887 Lockyer published a book entitled "The Chemistry of the Sun," in which he summed up the results of a long series of his investigations in the domain of the spectrum analysis of heavenly bodies. Certain variations in the intensity, characteristics and circumstances surrounding the appearance of spectral lines led him to the belief that the so-called elements could be reduced to simpler forms, and that these simpler forms actually existed under the enormously high temperatures of the sun and some of the stars. This had been suggested by Brodie and Hunt, years before, from a study of the simplification which matter underwent from elevation of temperature, even of those temperatures we can control and produce on the earth.

No one of these various conjectures took definite form, or led to a specific formulation of an extension of the atomic theory. Such a step has lately come, however, and it arose from the unearthing of a set of phenomena which have been observed for only a scant dozen or so of years, and the important results from which have come upon us with startling suddenness and taken us completely by surprise. They promise not only to reveal to us a relationship of the elements to each other, but to reduce all the elements to compounds of a common stuff; indeed, to tell us the actual weight and properties of this *ur*-atom, this fundamental piece of matter out of which all the known different kinds of matter are made. This hypothesis goes far beyond Prout's wild guess: it not only reduces the number of elements, it cuts the atom—the "uncuttable." It is a magnificent generalization, and most fertile in the possibilities it holds forth, if it can be put on an established footing.

In order to understand the genesis of this new hypothesis, it is necessary to give a rapid summary of the experimental evidence accumulated along these lines during the last few years.

In 1859 Plücker noticed the phosphorescence on the walls of a vacuous glass tube opposite the cathode when an electric discharge is passing through the tube. It seemed as if something emanated from the cathode and traveled away in straight lines, and Plücker found that these cathode "rays" are deflected by a magnet. Goldstein, in 1876, found that these rays started off normally from the cathode, and he concluded that they were waves in the ether. During the years 1879 to 1885 Crookes made a profound study of these rays and considered that we had here to deal with the motion of negatively electrified particles of matter projected with enormous velocities normally from the electrode, causing fluorescence, phosphorescence and heat by their impact on the walls of the vessel and objects placed in the tube, and mechanical motion by impinging on lightly mounted vanes, etc. He called this indeed a fourth state of matter. The two views, (1) that the rays are disturbances of the ether, and (2) that they are electrified particles of matter, have each had its school of supporters. The fact that the rays are deflected in a magnetic field points at once to the latter explanation and has presented an unanswerable argument to the ether-theory believers. J. J. Thomson and others have calculated from the amount of this magnetic deflection the velocity of the motion of the rays; it is about 3×10^9 centimeters per second. Thomson has found also that the radiation is not homogeneous; that a part of it is not deflected at all, and produces no phosphorescence. He has, moreover, found that for the same potential difference between the electrodes the magnetic deflection is the same in all gases. It was found also that when the cathode rays fall upon an obstacle or constriction in the tube the rays are diffusely reflected, and that the far side of the obstacle acts itself as a cathode. When cathode rays, therefore, fall upon a surface it becomes a cathode to all intents and purposes.

Closely connected with these rays are the so-called "canal rays," studied by Goldstein in 1898. They come from the yellow layer next the cathode and can be observed best when they stream out backward through a perforated cathode. They travel in straight lines, but produce no luminescence and are not deviated in a magnetic field. They have a velocity of about $3{\cdot}6 \times 10^7$ centimeters per second, and the ratio of the mass of a particle, m, to the charge it carries, e, is about $3{\cdot}3 \times 10^{-3}$, which is in accordance (as we shall see later) with the supposition that it is ordinary atoms which are projected and at work in this phenomenon. It seems probable that they are part of the return stream of particles to the cathode.

The early experimenters with cathode rays found that even very thin bodies cast black shadows; but in 1892

Hertz discovered that gold and aluminium foils transmitted some of the rays, and concluded from this that the latter could not be particles of matter, and that they must be ether waves, perhaps of the nature of ultraviolet light.

In 1887 Hertz had found that when ultraviolet light fell on a body the rays would discharge it if negatively charged, but would have no effect (or according to Branly a very feeble effect) on a positive charge. If the body is uncharged, the illumination by ultraviolet light gives it a charge which is almost always positive. The air between two condenser plates becomes a conductor when ultraviolet light falls on one or both of them, and there is a leakage current across the dielectric. It is evident that the gas plays a large part in the discharge, but this discharge is also dependent upon the metal of which the plates are formed. Since that time a corresponding effect has been found with X-rays, but it is there entirely produced by the gas and is quite independent of the material of which the plates are made.

At about the same time it was observed that gases could be electrified and made conducting by being brought near hot metals, and that electrified bodies in the gas would be discharged. The gases in all these cases behave as if they had become ionized; and this supposition has been most fruitful as an explanation of the phenomena involved.

Important advances now came with great rapidity. In 1894 Lenard made the cathode rays pass out of the vacuous vessel in which they were produced, by putting a very thin aluminium window in the vessel opposite the cathode. He found a diffuse light spreading out from the window to a short distance, 5 centimeters, into the outer air. These rays seem to possess all the properties of the cathode rays inside, but to distinguish them they are called "Lenard rays." They cause bodies to phosphoresce; they affect photographic plates; they discharge positively or negatively charged bodies; they make the air conducting; etc. By admitting the rays that pass through the window, not to the outer air, but to another enclosed space, the pressure and kind of gas in which could be regulated, Lenard found that the absorption of the rays was constant, no matter what was the nature or pressure of the gas, provided the density was always the same. This is a matter of prime importance from a theoretical standpoint, for it makes the absorptive power depend only on the density of the gas and not on its chemical composition or its physical state. Lenard observed, also, that these Lenard rays are deflected by a magnet in exactly the same way as are the cathode rays, and that they have a velocity about one-third that of light. The deflection is independent of the nature and the pressure of the gas through which they pass. Lenard still inclined towards the ether theory ot these radiations. Most of the upholders of this theory have held that the vibrations of the ether were transversal, but Jaumann suggested that they might be longitudinal. The chief support of an ether theory is the fact that the rays can pass through substances and even out of the tube into the open air, and the chief difficulty the theory has met with is in accounting for the action of a magnet. The electrified particle theory accounts for this action at once. In 1895 Perrin performed some experiments which make for the particle theory. He caught the cathode rays in a sort of Faraday ice-pail and found that they charged the vessel upon which they struck with negative electricity, and J. J. Thomson has proved that the rays carry their charges with them when deflected by a magnet. These experiments brought out another most important fact, namely, that the rays make the gas through which they pass, a conductor for the time being. Thomson suggested that they break the gas up into some kind of ions, and this explained to him why he had failed to find a deflection of the rays in an electrostatic field, as he had expected if they were electrified particles; for, since they move in a conducting field, they would be no more deflected than a current in a wire. Later, Thomson was able to get rid of these ions by using higher vacua, and he then found that the rays were deflected in an electrostatic field. The deflection under these circumstances, combined with the magnetic deflection, leaves us practically with no alternative but to believe that the rays are electrified matter in some form.

In 1900 Lenard showed that the impact of ultraviolet light on a conductor placed in a vacuum caused the latter to give out rays charged negatively and deviable by a magnet, like the cathode rays.

In 1895, the year after the appearance of Lenard's work on the Lenard rays, came another important discovery, that of Röntgen of the rays called after him, or simply "X-rays." He found that when the electric discharge is passing through a vacuous tube covered with black paper, which is opaque to visible, ultraviolet, cathode and Lenard rays, a radiation is emitted by the tube through the black paper, which acts on photographic plates or a fluorescent screen at a great distance. These rays are accordingly much more penetrative than cathode rays; in fact, all bodies are more or less transparent to this radiation, and roughly in the inverse ratio of their densities. They are absorbed to a certain extent by air and other gases. They discharge both positively and negatively electrified bodies when they fall upon them, whether the bodies be conductors or insulators. They ionize the gas through which they pass and make it conducting. They are not deflected by a magnet, and hence are not cathode rays. They come most strongly from the phosphorescent patches on the wall of the tube made by the cathode rays. They do not possess the properties of ultraviolet rays of being regularly reflected, refracted and polarized. When they fall obliquely on a sheet of metal the other side of the metal gives off radiations which affect a photographic plate, but which have not exactly the properties of the original rays. Whenever the X-rays strike a metal surface there is emitted by the surface secondary radiations consisting of two parts: one not deviable in a magnetic field and in many ways similar to the primary radiation, and another part deviable, and consisting of rays agreeing in their properties with cathode rays. These secondary rays have notably a less penetrative power than the primary rays, but they still ionize a gas and discharge electrified bodies. Röntgen at first supposed the X-rays to be longitudinal vibrations of the ether. These radiations are not homogeneous; that is, they are not all equally absorbed by a given substance. Though they ionize the gas through which they pass, as do ultraviolet light and cathode rays, yet they differ from the ultraviolet rays in that the latter ionize a gas only after being reflected from a fluorescent substance or a metal plate, etc. Air loses its power to conduct after passing through water or cotton wool; this fact and the small value of the velocity of the ions of a conducting gas make it seem probable that the ion is the center of an aggregation of a considerable number of molecules.

In 1897 Stokes suggested a very plausible explanation of the X-rays. Assuming that the cathode rays are moving negatively electrified particles, surrounded, in consequence, by a magnetic field of force, when the rays strike suddenly on an obstacle there will be electromagnetic induction, due to the stoppage of the quasi-current, and a thin pulse of magnetic and electric force will be propagated through the medium; it is this pulse in the ether which constitutes the X-rays. J. J. Thomson and Lehmann have independently, at about the same time, given the same explanation. It can be shown that such pulses would be of the same nature as ordinary light in many ways, but, being pulses, would differ from such regular trains of waves as constitute light in having less absorption by matter, and would have no regular period and no regular reflection and refraction and no diffraction. This simple explanation is now very generally accepted, and we have the X-rays put in their proper place as ether waves, along with infra-red, visible and ultraviolet rays, and consequently in an entirely different category from the cathode and Lenard rays, which are streams of negatively electrified particles.

In 1896 Becquerel discovered that uranium and its salts emit (even after being kept in the dark for years) radiations which have a great many properties in common with ultraviolet rays, with cathode rays and with X-rays, and yet the uranium rays cannot be identified as belonging to any one of these three groups. They travel in straight lines; they affect photographic plates and fluorescent bodies; they discharge electrified bodies when they fall upon them. In these respects they behave exactly as do all the three groups of radiations referred to above. Like the cathode and X-rays, they ionize a gas through which they pass and make it conducting, and Rutherford has shown that the velocity of the ions is the same for a gas made conducting by uranium radiations as for one made so by X-rays. The cathode, Röntgen and uranium rays all act as nuclei for water condensation in a dust-free atmosphere. The uranium radiations do not show any regular reflection, refraction or polarization, at least not the most active rays, and in this respect are again like cathode and Röntgen rays, and unlike ultraviolet radiation. Metallic uranium will not hold a charge unless in a vacuum, thus acting as if ultraviolet light fell upon it. The uranium radiation is not homogeneous, but is composed of rays of different penetrability. Rutherford has made a detailed study of these properties, and divides the uranium radiations into two groups: the one, α, more intense and very easily absorbed; the other, β, more feeble and very penetrating.

Soon it was found that thorium and its compounds have the same general radio-active properties as uranium, but the radiations are more penetrating. In addition to the "rays," Rutherford has shown that the thorium compounds give off an "emanation" or gas, which is probably made up of fine material particles, themselves radio-active, and which has the power of making bodies on which it falls radio-active. M. and Mme. Curie have isolated from pitch-blende two new substances, polonium (seemingly allied to bismuth) and radium (allied to barium), with a radio-activity 100,000 times as great as that of uranium; the polonium radiations are at the same time very little penetrative. From the same kind of material Debierne has made actinium (allied to thorium). Still more active substances of this kind are now prepared by De Haën, of Hanover. The radiations from all such substances are grouped under the generic title of "Becquerel rays." The radiation of radium is composed of three parts: (1) rays not deviated in a magnetic or electrostatic field and not very penetrative; (2) a small amount of non-deviable rays which are very penetrative, and (3) deviable rays which are very penetrative. The latter give the same value for $\frac{m}{e}$ as do cathode rays, and indeed have all the properties of cathode rays, including the carrying of a charge of negative electricity, and are indistinguishable from them except in the method of production. The same is generally true of all Becquerel rays; they are, therefore, very like the secondary X-rays. Any substance placed near a radio-active body acquires itself a radio-activity, the so-called "induced radio-activity," which may persist even for days at a time. At a very high temperature Rutherford has shown that the radio-active properties of these substances is destroyed. MacLennan has lately imparted radio-active properties to many substances by letting cathode rays impinge on them for half an hour, and by letting the light from a spark-discharge fall on them, and then gently heating them; but similar experiments made with ultraviolet rays and X-rays have failed.

Zeeman, in 1896, noticed that the bright lines in the spectrum of a source of light placed between the poles of an electromagnet were widened when the field was turned on. The same effect is produced in the absorption line spectrum of a vapor when the latter is placed in a strong magnetic field. An explanation of this phenomenon was furnished by the electromagnetic theory of Lorentz, which assumes that in all bodies small electrified particles, or ions, or "elektrons," are present with a definite mass and charge, and that all light phenomena are due to motions of these ions. These ions when moving in a magnetic field experience mechanical forces which influence their periods. Lorentz's theory suggested at once that the edges of the widened lines should be polarized, and on looking into the matter Zeeman found this to be so. Lorentz's theory explained further that when viewed along the lines of magnetic force a single line was broadened because it has become really a doublet with its two components circularly polarized in opposite directions, and when viewed at right angles to the magnetic force the broadening was due to the fact that the single line has become a triplet with the middle ray plane polarized parallel to the lines of magnetic force and the extreme rays plane polarized perpendicular to those lines. The theory also required that the distance between the extreme rays in either case should be dependent upon the ratio of the mass of an ion to its charge, and that $\frac{m}{e}$ should be equal to

$$\frac{H(\lambda_1 - \lambda)}{4\pi V},$$

where H is the intensity of the magnetic field, V the velocity of light, and λ_1 and λ the wave-lengths of the extreme rays. Experiment has verified these predictions of theory. It further appeared that it was the effect of the magnetic force on the negative ions which produced the phenomena.

Further work by Zeeman, Cornu, Preston, Larmor, Michelson and others has shown that the phenomenon is much more complicated than indicated above, and that a line may be broken up into more components than three, but the elektron theory still accounts for these additional peculiarities when it is viewed in greater detail; but the main features for our purpose are those already mentioned.

Such is the enormous amount of experimental material gathered together by investigators in this small department of science in a short time; the bare recital of their results has taken considerable time. We are now in a position to see how the evidence presented by this great mass of experiments has been able to shed light on the nature of the ultimate structure of matter.

When dealing with the cathode rays and allied phenomena we saw that there were two theories to correlate and account for the observed facts—an ether theory and a corpuscular theory. Either one will account for the rays starting normally to the cathode and traveling in straight lines, for the heating effects and for phosphorescence, and for the purely mechanical effects. The deflection of the rays by magnetic and by electrostatic forces seems explicable on the corpuscular theory only; but their passage through a thin-walled tube is so readily explained on the ether theory that it has given great support to such a theory and for a time caused considerable trouble to the adherents of the corpuscular theory. J. J. Thomson has given some attention to this point. He points out first that the rays may not really pass through the wall, but only start afresh on the other side. He shows that the motion of the particles shot off from the outside of the window in the Lenard experiment would be of the nature of an impulse, and the momentum gained would be proportional to this impulse; and as the deflection of the rays by a magnet is proportional to their momentum, we should expect to find that deflection independent of the nature and pressure of the gas outside the tube; and this is exactly what Lenard noticed. But inside the tube also the magnetic deflection is independent of the nature and pressure of the gas, and yet the motion there is not impulsive and the masses of the molecules of the different gases vary greatly. This led Thomson to the question, what are the corpuscles or particles which carry the charges? The first light thrown on this matter is the observation by Lenard that the rays outside the tube in air at a pressure of half an atmosphere travel only 5 millimeters before their intensity is halved. In the light of the kinetic theory of gases we can deduce from this that the mass of a corpuscle cannot be that of a molecule of air. Moreover, it cannot be that of an aggregate of molecules, because Lenard observed that the distance traversed by the rays is inversely proportional to the density of the gas. This leaves only the possibility that the corpuscles are of smaller mass than ordinary molecules and atoms. Thomson was satisfied that this suggestion would account for the observed phenomena; for if an atom is made up of a great number of holes and corpuscles, the holes being predominant, the mean free path of a corpuscle would be inversely proportional to the number of corpuscles it would meet, that is, to the number of corpuscles in unit of volume, that is, to the mass in unit of volume, or the density. This is exactly what Lenard found, if we assume that the penetrating distance of the corpuscles is their mean free path. Moreover, if all atoms are composed of the same corpuscles, whatever the gas, the magnetic deflections would be the same for all substances, as was found by Lenard to be the case.

This was conjecture. What was now wanted was experiments which would determine numerically the mass of the carriers of the negative electricity. One experiment which we have already referred to gave some evidence in this direction. The amount of the widening of a line in the Zeeman phenomenon gave a measure of $\frac{m}{e}$, and Zeeman found it to be about 10^{-7} C. G. S., E. M. units. The values of $\frac{m}{e}$ for ordinary electrolytes are well known; that for hydrogen being about 10^{-4} units. J. J. Thomson and others now directed their energies to finding the value of $\frac{m}{e}$ and of e, for ionized gases; the product of the two values would give m. Thomson's first method was to let cathode rays fall on a thermopile for a given time and thus measure their energy, W; after they pass the thermopile they are caught in a vessel connected with a capacity and an electrometer and the total charge, Q, on them measured; at the same time the radius of curvature, ρ, of the bending of the rays in a magnetic field of strength, H, is measured. Then

$$\frac{m}{e} = \frac{1}{2}\,\frac{QH^2\rho^2}{W}.$$

The value of $\frac{m}{e}$ thus got was $1{\cdot}5 \times 10^{-7}$, and was independent of the gas and of the nature of the electrodes.

In a second experiment he measured the deflections produced by a magnetic field, H, and by an electrostatic field, F, when each acted over a distance, l, and so arranged the forces that the deflections, θ, were equal. Then

$$\frac{m}{e} = \frac{H^2 l}{F\theta}.$$

The value for $\frac{m}{e}$ thus found was $1{\cdot}3 \times 10^{-7}$. Similar measurements by Lenard and Kaufmann gave similar results.

In his third method Thomson investigated the phenomenon noticed by Elster and Geitel of a decrease in the conduction between two plates illuminated by ultraviolet light when subjected to a magnetic force normal to the lines of flow between the plates. If the strength of magnetic field, H, reduces the flow to a minimum when the distance of the plates apart is d, then

$$\frac{m}{e} = \frac{H^2 d^2}{2V},$$

where V is the velocity of light. The result for $\frac{m}{e}$ was 1.4×10^{-7}.

Here are four entirely different ways of getting at the value of $\frac{m}{e}$, and they all give the same result, namely, that $\frac{m}{e}$ is about 10^{-7}, and that it is independent of the kind of matter employed. The same value has been found by Becquerel in the case of the deviable radium radiation.

We have thus the values of $\frac{m}{e}$ for ordinary electrolytes and for ionized gases, and the latter is about $\frac{1}{1000}$ of the former for the case of hydrogen. It remains to find the relative values of e for hydrogen and for the ion of an ionized gas, and we can deduce the relative values of m for the two cases. C. T. R. Wilson discovered that the ions produced by ultraviolet light act like those produced by X-rays in providing nuclei in dust-free air around which water vapor can condense in small drops. If then we could note the total charge carried by a collection of these drops when they fall, and at the same time count the number of them, we would have the charge on each. Moreover, if we can find the size of the drops we can find their number. The size of the drops can be determined from their rate of fall as a cloud. In this fashion Thomson found the value of e to be 6.5×10^{-10} C. G. S., E. S. units. From a knowledge of the mass of hydrogen required to carry a unit of electricity, combined with a use of the kinetic theory of gases, it is proved that the value of e for hydrogen is of the same order of magnitude as that which was found for the ion. Townsend has proved this fact by an entirely different method, namely, by the rate at which ions diffuse into gases. Since, therefore, the *e*'s are the same for the two cases, and the $\frac{m}{e}$ for the corpuscle is $\frac{1}{1000}$ of the $\frac{m}{e}$ for hydrogen in a liquid electrolyte, it follows that the mass of a corpuscle is about $\frac{1}{1000}$ of that of a hydrogen atom, and that it is the same for all gases. The mass of this negatively charged corpuscle in a gas at low pressure is about 3×10^{-26} of a gramme. It is found, however, not only in low-pressure vacuum tubes, but it is given off by incandescent metals, by metals illuminated by ultraviolet light, and by radio-active substances; and in every case it has the same mass and the same charge, and a velocity which is a large fraction of that of light, the radium corpuscles having the greatest velocity, namely, two-thirds that of light. Thomson states that with the appliances of ordinary magnitude the quantity of such matter produced is infinitesimally small; he calculated that his machine going night and day for a year would produce only $\frac{1}{3000000}$ of a gramme. This at once explains why the chemistry of matter in this state is not studied.

The carriers of the positive electricity behave quite differently from those of the negative, for it is found that the same quantity, *e*, of electricity, instead of being associated with a constant mass equal to $\frac{1}{1000}$ of the mass of an ordinary hydrogen atom, is connected with a variable mass, namely, one of the order of magnitude of that of a molecule of the substance under consideration, thus varying with the nature of the gas in which the electrification is found.

We have then in following the negative charge been led to a something of an invariable mass, of very small amount as compared with the masses of the molecules of ordinary matter, and independent of the kind of matter with which we found it associated—a veritable *urstoff*. What is this corpuscle? Is it a piece broken off from the ordinary atom and at the same time splitting up the neutralized electricity on that atom and taking the negative charge with it and leaving the positive on the remainder of the atom, and are all the elements made up of the same corpuscles, but differing from one another in number and arrangement? And have we the right to say that we have accordingly found the connecting link between the elements? This is one view, and it seems a very plausible one. Or was it electricity only that was split off from the atom? But it has mass, and how are we to conceive of electricity as endowed with mass and apart from matter with our present views? It is true that if we have a corpuscle of matter moving with a charge on it, it has an apparent added mass due to the magnetic field it bears with it; but is it reasonable to dissolve away the carrier of the electricity and say that the latter still has mass? These questions are still to be answered. J. J. Thomson has tried some experiments to test whether the mass is to be associated with electricity alone or whether part of it is due to a carrier, but his investigations have not led him to any definite conclusion.

The results of the experiments briefly described above and the new conceptions based on them have been of value in other fields; they can be used to account for electrical and thermal conduction in metals; they have even led to a very plausible explanation of the Mendelejeff groups and the aurora borealis. But we must confine ourselves here to their bearing on the structure of matter. The difficulty to be overcome in applying them to this problem lies in the question of the seat of the mass, and whether we have in the ion, or elektron, or corpuscle, a thing of the nature of an atom, only smaller, or something else. The matter seems to be mainly a question of words. We have found a thing with a mass $\frac{1}{1000}$ of that of a hydrogen atom, using throughout our calculations the term mass in the only way we can mechanically use it. Now we know matter only as a thing having mass, and we only use the idea of mass in mechanics as a property of matter; hence we know atoms and molecules only as they possess mass; so if mass is our criterion, we have in the elektron a sub-atom, and since it is the same for all elements, we have by inference the ordinary 70-odd atoms as aggregations of this sub-atom. If it should turn out to be true that the energy is purely electrical, it would not materially alter our atomic theory, it would give us simply an electrical "explanation" of mass—a concept for which at present we have no explanation.

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NOTES AND COMMENTS.

"PECULIARITIES OF THE WRITING OF HEALTHY PERSONS."*

BY AUGUST DIEHL,
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p. 4 xx "It appears desirable after *the initial step into the domain of the scientific study of writing*,† first to establish a firm basis for the comprehension of the writing of those sound in health in order to avoid the danger of errors and misunderstandings in the realm of the diseased."

p. 4. The persons whose writing was studied were four male and four female attendants at the University clinic (Ruprecht-Karl.)

p. 5. The questions to which answers were sought were (1) whether and what regular relations exist between the various peculiarities of handwriting. (2) How the chirography is influenced by slow and fast writing. (3) What changes it exhibits when the task is more difficult. (4) How far the individualities of handwriting may be regarded as personal peculiarities and indicate sex.

Ingenious devices together with the kymographion were used to measure the length of the path pursued by the pen, and the time consumed.

The hospital attendants employed as subjects were of the educated peasant class.

The experiments were undertaken in two series of five days each. The writings were performed with a Kohinoor H.B. pencil kept properly sharpened, and the substance written on was well-sized cards. The time chosen was 9.30 A.M.

The tasks assigned were as follows:

(1) Please write slowly and carefully the numbers 1 to 10 on the card.

After this had been done:

(2) The same thing once more.

After a pause of two minutes.

(3) Now, please, write the numbers 1 to 10 as fast as you can.

When this was finished:

(4) Once more, please, as fast as possible.

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* Translated Extracts from Doktor Dissertation.

† This initial step was taken in 1894 by the translator in his book "Bibliotics."

The last task was :

(5) Please write the row of numbers reversed from 10 to 1 just as it is most convenient for you.

The time was called . . . . Sd.
The length of path of the pen . . . . Sw.
The time in hundredths of a second for a millimeter of path . . Mz.

Then $Mz = \frac{Sd}{Sw}$ if Sw be expressed in mm. and Sw and Sd in σ (= 0·01 seconds.) The greatest variations between the means of the writers was about $\frac{1}{100}$ second for a single letter. The average time taken was nearly $\frac{1}{20}$ second to a mm.

The conclusions are: (1) The pen-path is shortened by increased and lengthened by decreased difficulty of the task. (2) The rapidity is lessened by writing backwards and increased by force of will and excitement. (3) The pen-pressure increases with voluntary effort, and decreases by diminished difficulty of the task. (4) The pauses in writing are influenced even in a higher degree by the alteration of the task set than by the rapidity. (5) The rapidity of writing single figures depends most of all on the pen-path, then on the intermediate pauses. But besides this there exists with the effort to write with uniform rapidity a certain tendency to approximate to each other the periods of writing different figures—long figures fast, short figures slowly. Finally the placing of a figure in the row may change the rapidity under the changing influences of the will, of practice, of habit, and of fatigue. (6) Practice appears to make the chirography smaller without increasing its rapidity; the pressure usually diminishes through its influence. (7) With unfavorable mood we find retardation of writing together with increase of pressure, also sometimes diminution of the written characters. (8) Pen-path, rapidity, and pen-pressure are in a high degree characteristics of a single individual; they appear to alter in nearly the same relation by change of the experimental conditions in different men. The same cannot be said of the rapidity. (9) The capacities of different persons for writing are only comparable when undertaken under identically the same conditions. (10) Women write larger, more quickly, and with less pressure than men; the writing movement is made by women with greater ease and less effort of will. (11) On increasing the difficulty of a writing task men respond preferably by raising the effort of the will, women by a diminution of size of the written characters.

P. F.

CAUSE OF AUTUMNAL HAZE.

The Chief of the United States Weather Bureau offers the following explanation of the phenomenon :—The dry haze is undoubtedly due to fine particles of dust. The finest dust is composed of one or all of the following substances, namely, fine particles of soil or the dead leaves of plants, smoke or ashes from wood fires, salt from ocean spray, the shells or scales of microscopic silicious diatoms, germs of fungi, spores of ferns, pollen of flowers, etc. In the still air of the damp nights these dust particles settle slowly down, and the morning air is comparatively clear. During the daylight the sun warms the soil, which heats the adjacent air, and the rising air currents carry up the dust as high as they go. Under certain conditions, which are named in the letter, the layer of dust reaches higher and higher every successive day. During long, dry summers in India it reaches to 7,000 feet with a well-defined upper surface that is higher in the day-time than at night-time. This is a general explanation of dry haze weather and applies to Indian summer also. The reason why we have more of such weather in the autumn is because there is then less horizontal wind and less rising air. W.

BOOK NOTICES.

American Standard Specifications for Steel. By Albert Ladd Colby, Member of Committee No. 1, of the American Section of the International Association for Testing Materials. Second edition. Easton, Pa.: The Chemical Publishing Co., 1902. (Price, $1.10.)

Mr. Colby gives a review of the work of Committee No. 1 of the American Section of the International Association for Testing Materials, showing how their proposed specifications were arrived at from the specifications in use. He refers to the general discussion, both at home and abroad, of these proposed specifications during the past two years, and their final adoption by the American Section last summer.

A general review is then given of the requirements of the specifications, with the reasons which governed the Committee in its decisions, and takes up each of the following headings:

(1) Process of Manufacture.
(2) Chemical Properties.
(3) Physical Properties.
(4) Test Pieces and Methods of Testing.
(5) Finish and Variation in Weight.
(6) Branding.
(7) Inspection.

After this there is a review of each of the nine specifications. These reviews are excellent and give a clear, concise statement of the conditions governing the work. The specifications are becoming the recognized standards of this country; the Rail Specifications, with some modifications, having recently been adopted by the American Railway Engineers and Maintenance of Way Association as their standard.

The book is a good addition to our engineering literature on the subjects treated, and its price is only moderate. W. R. W.

Annuaire pour l'an 1902, publiè par le Bureau des Longitudes. Avec les notices scientifiques. Paris: Gauthier-Villars, 1901. (Price, 1 fr. 50c.)

The publishers of this annual call special attention to the contribution of M. A. Cornu on Polyphase Currents, and that of M. H. Poincaré, on Wireless Telegraphy. The book gives all the statistical data that have made the previous volumes so valuable for reference. W.

Theory of Steel Concrete Arches and of Vaulted Structures. By William Cain, Professor of Mathematics, University of North Carolina. Second edition, thoroughly revised. 16mo, boards, pp. 161. New York: D. Van Nostrand Co., 1902. (Price, 50 cents.)

This volume is a revised edition of the author's previous volume on "Voussoir Arches, etc.," which appeared as No. 42 in the *Van Nostrand Science Series.*

The author states that he has undertaken in this to give a complete solution of the elastic arch of variable section, and a detailed consideration of the arch of steel and concrete combined to illustrate the general graphical treatment.

About one-half of the present volume is stated to be new, and, taken as a whole, while it is independent of the author's other works on arches, practically supplements them.

The work treats, in chapter I, of arches of variable section under vertical loads; in chapter II, of culverts and tunnel-arches; in chapter III, of grooved and cloistered arches; and in chapter IV, of masonry domes. W.

Jahrbuch für das Eisenhüttenwesen. Ergänzung zu "Stahl und Eisen." Ein Bericht über die Fortschritte auf allen Gebieten des Eisenhüttenwesens im Jahre 1900. Im Auftrage des "Vereins deutscher Eisenhüttenleute" bearbeitet von Otto Vogel. I. Jahrgang. (Preis elegant gebunden in Ganzleinen, 10 M.) Verlag von August Bagel, Düsseldorf.

This new Yearbook of Iron Manufacture is intended to supplement the well-known journal, "*Stahl und Eisen*" and the "*Gemeinfasslichen Darstellung des Eisenhüttenwesens,*" issued by the "*Verein deutscher Eisenhüttenleute.*" It is intended that the present publication shall record in systematic order the numerous contributions to the literature of the metallurgy of iron at home and abroad, by the publication of abstracts of the most important of those contributions by which they will be made more readily accessible. The present first volume of the Yearbook forms a large quarto of 450 pages, and contains about 1,800 references and abstracts, comprising the useful literature in this field to be found in 110 technical journals and other publications, and in eight languages. The whole is thoroughly indexed by authors and subject-matter. W.

Franklin Institute.

[*Proceedings of the Stated Meeting held Wednesday, May 21, 1902.*]

HALL OF THE FRANKLIN INSTITUTE,
PHILADELPHIA, May 21, 1902.

President JOHN BIRKINBINE in the chair.

Present, 68 members and visitors.

Elections to membership since last report, 11.

The Actuary reported that the Board of Managers had passed the following resolution at its stated meeting of Wednesday, May 14th, viz.:

"*Resolved*, That a special committee be appointed to act in conjunction with a similar committee to be appointed by the Institute to prepare a memorial of Dr. Henry Morton, deceased, for publication in the *Journal*."

It was thereupon voted to appoint a special committee for the purpose.

The Chairman named the following members: Dr. Coleman Sellers, Chairman; Washington Jones, Samuel Sartain (from the Board), A. E. Outerbridge, Jr., H. R. Heyl and Cyrus Chambers, Jr. (from the Institute).

Dr. Joseph W. Richards, of Lehigh University, presented the conclusion of his remarks on "The Electro-Chemical Industries at Niagara Falls." His present remarks bore specially on the Acker electrolytic sodium process; Hall's electrothermic method of purifying beauxite; the Ampere Electro-Chemical Company's method of producing electrothermic barium hydrate, cyanides and nitric acid, and Acheson's procedure for the electrothermic production of graphite.

Mr. C. H. Ott, Assistant Engineer, Bureau of Surveys, Philadelphia, presented a communication on "The Improvement of the Channels of the Delaware and Schuylkill Rivers by the City of Philadelphia." (Discussed by Prof. Lewis M. Haupt.) The paper was fully illustrated with lantern views.

Under the title of "A Floating Palace," Mr. W. N. Jennings exhibited and commented on a series of lantern views of the yacht "Margarita," owned by Mr. A. J. Drexel.

Mr. Chas. E. Ronaldson was elected to the Committee on Science and the Arts, to take the place of Mr. R. D. Kinney, withdrawn.

Amendments to the By-laws were offered by Prof. F. L. Garrison, providing for dropping from the list the names of delinquent members; and by Prof. L. F. Rondinella, providing for a letter-ballot at the annual election. These amendments are to be voted on at the stated meeting of June 18th. The Secretary was directed to give members due notice of the amendments by publication.

Mr. Jesse Pawling, Jr., Secretary of the Physical Section, reported the action of the Section at its stated meeting in April, appointing a special committee consisting of Messrs. Jesse Pawling, Jr., Chairman; Dr. A. E. Kennelly and Dr. George F. Stradling, to bring to the attention of other societies and of individuals the action of the Institute in reference to the Metric System of Weights and Measures, and to ascertain from them their views on the report adopted by the Institute.

Mr. Pawling called attention to the fact that the three members of this special committee were also members of the Committee on the Metric System (appointed by the President at the stated meeting of November 20, 1901), and suggested that it would be more appropriate for the Institute to confirm this action of the Physical Section, and thus authorize the proposed correspondence to be carried on by a committee of the Institute. This suggestion was approved. Adjourned.

WM. H. WAHL,
Secretary.

COMMITTEE ON SCIENCE AND THE ARTS.

(*Abstract of proceedings of the stated meeting held Wednesday, May 7, 1902.*)

MR. JAMES CHRISTIE, *Chairman pro tem.*

The following reports were adopted:

(No. 2209.) *Synchronism Indicator.*—Paul M. Lincoln, Niagara Falls, N. Y.

ABSTRACT.—This instrument was devised for the purpose of synchronizing two or more alternating current generators which it is desired to connect in parallel, and indicates the phase and frequency relation of two separate sources of electromotive forces. The instrument also indicates which of the generators is operating at the higher frequency and what the difference is.

The invention is protected by U. S. letters-patent No. 685,155, October 22, 1901, granted to applicant.

A full description of the device may be consulted in the JOURNAL (vide Lincoln, Parallel Operation of Alternators, Vol. cliii, 241, April, 1902), to which reference is made.

The report finds that the instrument fully meets all of the requirements of practice, and greatly facilitates the connecting of alternating current machines in parallel. It is pronounced to be a meritorious and ingenious invention. The award of the Scott Legacy Premium and Medal is recommended to the inventor. [*Sub-Committee.*—Wm. C. L. Eglin, Chairman; Coleman Sellers, Thos. Spencer, W. M. Stine.]

(No. 2225.) *Improved Kerosene Oil Incandescent Light.*—Williams, Brown & Earle, Philadelphia.

ABSTRACT.—The subject matter of this application is an adaptation of the kerosene oil incandescent light for use in the magic lantern and for various uses in photography.

The device is based on the system generally known as the Kitson light, but is specially modified in respect of portability and compactness, so as to adapt it for the above-named applications.

In respect of intensity of illumination this light is intermediate between that produced by the best multiple-wick oil lamps and a good oxy-hydrogen light. The illumination is superior to that of any oil flame, being far more even and white. These qualities adapt it well for various kinds of photographic work.

The Edward Longstreth Medal of Merit is awarded to the manufacturers. [*Sub-Committee.*—F. E. Ives, Chairman; L. E. Levy, W. N. Jennings.]

(No. 2184.) *Machine for Cutting and Otherwise Preparing Rattan.*—Julian Pomeroy and Henry W. Larsson, Springfield, Mass.

(An advisory report.)

Under-mentioned reports passed first reading:

(No. 2212.) *Rheostat.* Charles Wirt, Philadelphia.

(No. 2228.) *Device for Photo-micrography.*—Fred'k Eugene Ives, Philadelphia.

Protest was filed by Messrs. Francis Head, A. M. Greene, Jr., and Arthur J. Rowland against report on the Toerring Enclosed Arc Lamp, adopted at the adjourned meeting of April 11, 1902.

WM. H. WAHL, *Secretary.*

SECTIONS.

(*Abstracts of Proceedings.*)

PHYSICAL SECTION.—*Stated Meeting*, held Wednesday, April 23d. Dr Stradling in the chair.

The first paper of the evening was read by Mr. J. Frank Meyer, of the Lower Merion High School, Ardmore, Pa., on "Recent Work in Electric Convection." The reading of the paper was followed by some appropriate remarks by Dr. Stradling.

The second paper was read by Prof. Richard Zeckwer, on "An Investigation of Piano Touch." The paper was discussed by Dr. Stradling, Mr. Meyer, Dr. Partridge and Dr. Goldsmith. A number of interesting lantern slides were shown in connection with the paper.

The committee appointed at the last meeting to bring before the various scientific, technical and manufacturing bodies the action of the Institute on the metric system, reported that no action had as yet taken place, owing to the change in the constitution of the Institute, which was contemplated at the time the committee was appointed.

The meeting then adjourned.

JESSE PAWLING, JR., *Secretary.*

CHEMICAL SECTION.—*Stated Meeting*, held Thursday, April 24th. President L. F. Kebler in the chair. Present, 15 members and visitors.

Dr. J. Merritt Mathews presented an informal communication on "Mercerizing Textiles," which was replete with interesting historical and technical data. A number of specimens of mercerized fabrics were exhibited. Discussed by Dr. H. F. Keller, the chairman, and the author.

Mr. Kebler followed with a communication entitled "An Examination of Physical and Chemical Tests for Strychnia." The author called attention to, and demonstrated the possibility of, color reactions being misleading. Discussed by Dr. Robert H. Bradbury and Dr. Keller. A paper, which was to have been read by Dr. E. A. Partridge, on account of the lateness of the hour, was postponed.

M. I. WILBERT,
Secretary pro tem.

Stated Meeting, held Thursday, May 22d. Dr. W. J. Williams in the chair. Present, 27 members and visitors.

Dr. Edward A. Partridge made an informal communication "On Temperature."

The speaker discussed the several current definitions of temperature that have been proposed by Clark-Maxwell, Kelvin, and others, and indicated in what respects they were unsatisfactory and open to criticism. He discussed the principles of thermodynamics, illustrating their application in the Carnot Cycle, and demonstrated how by this method the absolute zero is established. Discussed by Messrs. Pawling, Reed, Bradbury, Dodge, Whittaker and the author.

Dr. Robert H. Bradbury followed with a "Review of some of the Recent Literature of the Subject of the Periodic Law." Discussed by Mr. Reed and the author. The speakers received a vote of thanks for their interesting communications.

WM. H. WAHL,
Secretary pro tem.

SECTION OF PHOTOGRAPHY AND MICROSCOPY.—*Eighteenth Stated Meeting*, held Thursday, May 1, 8 P.M. Present, 15 members and visitors. Dr. Leffmann in the chair.

Mr. Fred. E. Ives presented a communication describing an ingenious method, of his devising, for "Measuring Objects under the Microscope." The speaker illustrated the adaptation of his device by projecting a photomicrograph on the screen, upon which the dimensions were indicated on a scale.

Dr. Leffmann presented a communication on the use of "Light as a Therapeutic Agent."

Mr. Samuel Sartain made some remarks on a new modification of a pigment printing.

Mr. M. I. Wilbert described another modification now being introduced under the trade name "Ozotype."

An exhibition followed of a series of lantern slides and prints of miscellaneous subjects, contributed respectively by Messrs. Ives, Leffmann, Ridpath and Wilbert. Worthy of special notice among these were several prints of the remains of an ancient aqueduct near Cairo, Egypt.

MARTIN I. WILBERT,
Secretary.

MECHANICAL AND ENGINEERING SECTION, ELECTRICAL SECTION.—*Joint Meeting*, held Thursday, May 8th, 8 P.M. Mr. A. M. Greene, Jr., in the chair. Present, 38 members.

The paper of the evening was presented by Mr. W. A. Heywood, Assistant Superintendent Pennsylvania Iron Works Co., on "Modern Corliss Engines for Railway and Lighting Service."

Mr. Heywood illustrated his paper with the aid of a series of diagrams. The discussion which followed was participated in by Messrs. Francis Head, Thomas Spencer, H. F. Colvin and the author. The paper is reserved for publication.

Following this came a discussion on the "Relative Merits of the Binary and the Decimal Systems of Measurement for the Shop and Drawing Room," which was participated in by Messrs. Jesse Pawling, Jr., Wilfred Lewis, Geo. Schumann, L. F. Rondinella, John Haug, H. F. Colvin.

DANIEL EPPELSHEIMER, JR.,
Secretary.

MINING AND METALLURGICAL SECTION.—*Joint Meeting*, held Wednesday, May 14th, 8 .M., with the American Institute of Mining Engineers, at the Manufacturers' Club.

Present, about 300 members and visitors.

President F. Lynwood Garrison in the chair.

The following program was presented:

"The Development of the Bessemer Process for Small Charges." Mr. Bradley Stoughton, Columbia University, New York.

The author traced the history of the Bessemer process for small charges from the original stationary converter of Bessemer in 1856. Next came the Clapp-Griffith Converter of 1881; Walrand, 1884, and, more recently, the converters of Robert and Tropinas.

The main difference in these converters resides in the method of blowing. Walrand and Legeneisel, in 1896, patented a method covering an afterblow with subsequent addition of ferro-silicon.

The "Baby Bessemer Process," as it is called, is now apparently in successful operation here and abroad. The loss in iron is from 17 per cent. to 30 per cent., and it is claimed that a hotter steel is produced than by the open-hearth method, thus making it particularly useful for the manufacture of small castings.

The paper was discussed by Mr. Campbell, who took exception to the statement that the steel is hotter than that produced in the open-hearth method.

Mr. Wilcox, in connection therewith, gave some reminiscences regarding the Clapp-Griffith converter.

This was followed by a paper on the "Relation between Structure and Durability of Steel Rails," by Mr. Robert Job, Chemist to the Philadelphia & Reading Railway Company.

Mr. Job's paper covered a careful investigation of a large number of rails which had failed in service. The samples were submitted to physical, chemical and microscopical examinations. The conclusions were reached that failures are more frequently due to physical defects than to chemical composition, and the rate of wear on rails of identical chemical compositions is slower with a fine-grain than with a coarse-grain structure.

The paper was illustrated by photo-micrographs and rail sections.

Discussed by Messrs. York, Wm. Kent, Martin and Dr. C. B. Dudley.

The following papers were read by title:

"The Metallurgy of Titanium," Augusti Rossi, New York; "The Growth of Pig Iron Production during the Past Thirty Years," Mr. John Birkinbine, Philadelphia; "The Effect of Re-heating upon the Coarse Structure of Overheated Steel," Karl Göranssen, Sweden; "The Oil Fields of Texas," Mr. Robert T. Hill, U. S. Geological Survey.

G. H. CLAMER,
Secretary.